THURSDAY, NOVEMBER 17, 1898.

THE ANATOMY OF BIRDS.

The Structure and Classification of Birds. By F. E. Beddard. Pp. xx + 548, illustrated. (London: Longmans, Green, and Co., 1898.)

READERS of the *Zoological Society's Proceedings* may not improbably have been struck by the comparatively small number of papers communicated to the last few issues by the prosector. The present handsomely got-up volume shows, however, that Mr. Beddard has been fully occupied in anatomical investigations, and he is to be congratulated in presenting the results of his labours in such an attractive and well arranged form.

As stated in the preface, the work is not solely based on his own researches, since his two immediate predecessors in office devoted a large amount of time and labour to the study of the anatomy and affinities of birds. Portions of such work have been from time to time published in separate papers; but a large amount of MS. was left by Prof. Garrod, of which Mr. Beddard has availed himself. It is thus highly satisfactory to have in a combined and handy form the leading results of the united work of three such distinguished bird anatomists as the author and his two predecessors.

Although works on the external characters and classification of birds abound, treatises in English on avian anatomy and morphology are rare; and the present work therefore fills a distinct gap—and fills it well. Commencing with a purposely brief sketch of the general principles of bird structure, Mr. Beddard devotes the greater part of his volume to the characteristic structural characters of the different groups of birds. To a considerable extent this ground has indeed been covered by Dr. Gadow, but the author has treated the subject in greater detail, and has recorded a number of new facts, many of which are highly important.

The general anatomy of the class is treated in a manner which, while ministering to the wants of the advanced student, can scarcely fail to interest those lovers of birds who desire to know something more than the mere arrangement and colours of the feathers. And especial attention may be directed to the section devoted to the gradually increasing complexity of the folds of the intestine as we pass to the more specialised forms. Here, as elsewhere, an admirable series of illustrations display in the clearest form the various types of structure discussed.

In the osteological section of general anatomy the paragraph devoted to the hyoid (p. 136), if not absolutely incorrect, is certainly very far from clear, and, moreover, does not agree with the explanatory figure on the opposite page. Again, on p. 140 we meet, in a quotation from Dr. Coues, with the word "fadge," which, if correctly printed, certainly stands in need of explanation to ordinary readers. And, although there is no difficulty about their meaning, we must venture to protest against the use of such terms as "schizorhiny" and "holorhiny," in place of schizorhinism and holorhinism.

Turning to the classificatory portion of the subject, the table of contents looks as though the author was starting a missing word competition—without offering a prize to the solver! At the commencement of the table (p. x.) we find the term *Ornithurae* printed in the same vertical line as a series of other names apparently intended to indicate groups of inferior rank; and at the end, also in the same line, the term *Saururae*, evidently the equivalent in rank to the first. Such a small error in alignment might well be passed over without notice. But immediately below *Ornithurae* occurs the term *Anomologonatae*, followed in the same vertical line by *Passeres* and *Pici*. And on looking down the table we fail to find any group corresponding in rank to this *Anomologonatae*, so that there is not the faintest clue to the number of orders it is intended to embrace. On turning to the page (167) where the *Anomologonatae* are described we likewise fail to discover its antithesis. Neither does the index help us, since terms of higher rank than genera are excluded therefrom; this being, in our opinion, a decidedly objectionable plan. Almost by chance, we at length succeeded in stumbling on the missing word—to wit, *Homalogonatae*—on p. 165; but even when thus found, the reader is apparently left totally in the dark as to the number of ordinal groups thus brigaded together. Another glaring error in the "Contents" cannot be overlooked. By the position and large type in which the item "Reproduction [tive] and Renal Organs" is printed, it is made to include such subjects as myology, osteology, &c.!

In printing the names of ordinal and higher groups in block type, the author is perhaps well advised. But personally we decidedly object to the names of writers being similarly distinguished; the important point to which attention should be directed being the fact recorded, and not the more or less distinguished individual by whom it was discovered. And here it may be mentioned that authors' names are not always correctly spelt, a superfluous *e* being generally, although not always, intercalated in that of one well-known avian osteologist. Moreover, if we mistake not, full justice has not been done to the labours of the same writer, who contributed the last paper bearing on one of the subjects on which the late Mr. Wray is alone quoted as the latest authority.

Neither is the work quite free from errors of expression. For example, in treating of the *Limicolae* (pp. 326 and 327), we meet with the following passage:—"The type family is that of the *Charadriidae*, which contains the largest number of genera; the remaining families are not separated from it by very numerous points of difference, and the group as a whole is very near to the gulls, which I only divide as a family." That is to say, a group is allied to a portion of itself!

As regards the serial arrangement of the various orders (of which a large number are adopted), comparatively few remarks will suffice. Some surprise will be experienced in finding the *Accipitres* placed near the end of the series between the extinct *Ichthyornithes* and the modern tinamous, to neither of which they have any affinity. And when we find it stated (p. 469) that the *Ichthyornithes* themselves are probably allied to the stork and plover tribe, it seems strange to find them located between the *Anseres* and the *Accipitres*. Again, when the author himself admits that the nearest relations of the tinamous are the *Struthiones* (*Ratitae*) on the one hand, and the game birds on the other, it seems decidedly strange that the latter group is not placed in juxtaposition.

The mention of *Struthiones* reminds us that the student perusing the table of contents would gain the idea that the group includes only the two extinct families *Aepyornithidae* and *Dinornithidae*, since these two are alone mentioned. On turning to the corresponding text, it will be found that only these two families are referred to by name, from which we draw the inference that Mr. Beddard has allowed his table of contents to be compiled for him. Of course either all or none of the families should have been given.

Reverting to the orders, it may be noted that Mr. Beddard includes the flamingoes (omitted from the table of contents !) in the *Herodiones*, refusing to admit that they have any relationship with the *Anseres*. In this he is fully supported by osteology. In placing the extinct *Hesporornis* next the divers, we are glad to see that he rejects the recently revived heresy of the Ratite affinities of the former. But whether the penguins are well placed between the *Hesperornithes* and the *Steganopodes* may perhaps be open to question. On the other hand, the location of the owls next the parrots, in association with various groups of the old "Picariae," will probably meet with general approbation.

As regards the ancestry of birds, the author, while refusing to reject a dinosaurian affinity, is inclined to admit some kind of relationship with pterodactyles. It is, however, somewhat difficult to understand such a double consanguinity.

In conclusion, attention may be drawn to the remarkable difference displayed by the caeca of different genera of tinamous, as exemplified by the figures on p. 488. The mere record of such differences is, it is true, an addition to knowledge ; but, as has been remarked by Prof. Newton, what we really want to know is the physiological reason for such variations. And until this is ascertained, we are merely wandering aimlessly in the dark.

As an excellent compendium of the present state of our knowledge of bird anatomy, Mr. Beddard's work may be heartily commended. The blemishes by which the present issue is disfigured may, we hope, be removed in a second edition, which ought to be called for at no very distant date.

R. L.

EGYPTIAN MUMMIES IN THE BRITISH MUSEUM.

British Museum. A Guide to the First and Second Egyptian Rooms. Mummies, Mummy-Cases, and other Objects connected with the Funeral Rites of the Ancient Egyptians. By E. A. Wallis Budge, M.A., Litt.D., D.Lit., F.S.A., Keeper of the Egyptian and Assyrian Antiquities. Pp. viii + 92, with 25 plates. (Printed by order of the Trustees, 1898.)

THE national collection of Egyptian antiquities in the British Museum is, speaking generally, the most complete in Europe. Other collections may perhaps excel it in certain classes of antiquities as, for instance, the Louvre in its unique series of Apis stelae discovered by Mariette, or the Berlin Museum in its specimens of sculpture from tombs of the Early Empire. But the collection in the British Museum is the finest representative collection, comprising as it does typical examples of antiquities of most classes and periods. This is nowhere truer than in the two galleries which are set apart for objects connected with the funeral rites of the ancient Egyptians. During the last twenty-five years, and more particularly during the last ten years, the Trustees of the Museum have been steadily increasing their already fine collection of mummies and mummy-cases, so that they are now in possession of a remarkable series ranging over most of the historical period of Egyptian history, from about B.C. 3600 to A.D. 400. Moreover, the work of arranging and cataloguing the collection has kept pace with that of acquisition. During the past eighteen months any visitor to the Egyptian Department might have noticed a small army of workmen setting in place new wall-cases and standard-cases, remounting the mummies and coffins, and, under the direction of the Keeper, arranging them in chronological order. This work has now been brought to an end with the issue of the "Guide to the First and Second Egyptian Rooms" that has just been published by the Trustees.

To give some idea of the scope of the Guide, it may here be stated that the collection exhibited in these two rooms consists of forty-four mummies and eighty coffins and cartonnage-cases, including typical examples of all periods ; wooden figures of *Ptaḥ-Seker-Ȧusȧr*, the triune Egyptian god of the resurrection ; shabti figures of stone, wood, and glazed porcelain, which were placed in the tombs to do the work of the deceased in the nether world ; and sets of Canopic jars, in which the principal intestines of the deceased were placed, after being removed from the body before the process of embalming. The Guide describes the contents of the galleries, case by case, and as these are arranged in chronological order we can examine and compare at leisure the changing fashions and methods of embalming which were practised by the Egyptians during the long course of their history.

In his Introduction, for the benefit of students who are not Egyptologists, Dr. Budge gives a sketch of the principal features of the Egyptian religion, emphasising their belief in a supreme being, *neter*, apart from the *neteru*, the personifications of special powers or natural phenomena. He then sketches the principal views held by the Egyptians with regard to the nature of the gods and the origin of the universe, and this is followed by a descriptive list of the gods mentioned in the "Book of the Dead," and whose names are frequently met with in the main body of the Guide. Passing to the Egyptian idea of a future life, Dr. Budge enumerates the nine parts which were believed to form a man's personality ; the ritual and ceremonies are next referred to which accompanied the deposit of the dead body in the tomb, and which were gradually grouped together by the Egyptian priests into a number of chapters now generally known as the "Book of the Dead." Finally an account of the different methods of mummifying is given, and the tomb is described with its funereal furniture.

Among the mummies belonging to the early period are two of peculiar interest, which we believe are recent additions to the collection. These are the skeletons of two Egyptian officials, Khati and Ḥeni, which are exhibited in the First Egyptian Room in cases E and F above the rectangular wooden coffins in which they were found. These two skeletons date from about 2600 B.C., and represent a peculiar method of burial, unlike the usual Egyptian custom of mummifying the body. In the case of Khati and Ḥeni the flesh was removed from the bones before burial by means of muriate of soda or natron, and the bones were then treated with bitumen, which has tinted them a light yellow; they were then wrapped in linen, a layer of which may be seen in case E under Khati's skeleton. Khati's skull is peculiarly interesting on account of the two indentations in the parietal bones; these, Dr. Budge remarks, "must have been made artificially in early childhood because the surface of the bones is not broken." Ḥeni's bones have been articulated, and the skeleton is about 5 feet 6 inches long; it is a very fine specimen of this method of mummifying as carried on under the eleventh dynasty. This method of mummifying the dead, by treating the bones with bitumen after removing the flesh, goes back many thousands of years, and was probably the earliest method of preserving their dead employed by the inhabitants of Egypt; for many of the skeletons from prehistoric sites that have been recently found by M. de Morgan have been treated with bitumen in a similar manner.

Turning to the later portion of the collection, among the most noteworthy exhibits are three painted cartonnage-cases of a Graeco-Roman official (Plate xxiii.) and his two wives, one of whom is figured on Plate xxiv. These we fancy are also recent additions to the collection, and are probably the best examples of their kind in Europe. The modelling is good, so that the cases are remarkably life-like and give a good idea of the dress worn at the period, about 200 A.D.

We have not done more than give a passing reference to three or four out of this unique series of mummies and mummy-cases. Beginning in the First Egyptian Room with the mummy-case of Mycerinus, the builder of the fourth pyramid at Gizeh in the fourth millenium B.C., and ending with the wooden coffin of the Greek or Roman lady, who lies with her three children at the end of the Second Egyptian Room, we can trace upon the mummies and their cases the religious beliefs of the ancient Egyptians as they developed through a period of some four thousand years. To the student of religions a comparative study of this nature presents considerable attractions, and he will welcome Dr. Budge's guide, which supplies him with concise though detailed information on every exhibit in the two galleries. The Trustees, with a view to enhancing the value of the guide for educational purposes, have issued it in two forms, *i.e.* with and without plates; the former is published at the ridiculously low price of one shilling, and the latter at sixpence. Paper and printing leave nothing to be desired.

OUR BOOK SHELF.

An Introduction to Practical Physics for Use in Schools. By D. Rintoul, M.A. Pp. xx + 166. (London: Macmillan and Co., Ltd., 1898.)

SO many volumes containing courses of work in practical physics have lately appeared, that it would hardly seem necessary to increase their number. But a critical examination of the present volume is sufficient to afford justification for adding the book to those previously available. The author succeeded Prof. Worthington at Clifton College, and has carried on the work commenced there of making practical physics a subject practicable for junior students. The experiments described are thus not of the kind invented by the arm-chair philosopher—now happily becoming extinct—but those which have stood the test of experience, and have proved to be suitable for the class of students expected to perform them.

The book does not provide a complete course of practical work in physics, but only on some branches of physical measurement. Experiments on mensuration and hydrostatics occupy fifty-four pages, heat is dealt with in fifty-seven pages, and the third part on dynamics fills fifty-three pages. Light, sound, electricity, and magnetism are not touched upon, but presumably they will form the subject of a second volume. There can, however, be no doubt that the subjects included in the present volume are fundamental for students of physics, and form the best basis for future work.

The plan adopted by Mr. Rintoul, and proved by him to be suited to the mental capacity of boys of thirteen or fourteen, is a compromise between the Socratic and didactic methods of teaching. Sufficient explanation is given to enable the young experimenter to proceed with his work intelligently, and to grasp the significance of the results. He is then in a position to understand the effects produced by different conditions; and Mr. Rintoul provides him with many questions upon which he can usefully exercise his mind.

The book is especially suitable for the modern sides of public schools. As a physical laboratory manual for use in schools of this character it can be highly com mended.

A Text-book of Special Pathological Anatomy. By Prof. Ernst Ziegler. Translated and edited from the eighth German edition by Donald Macalister, M.A., M.D., and Henry W. Cattell, M.A., M.D. Sections i.-viii., and ix.-xv., in two vols. (London: Macmillan and Co., Ltd., 1897. New York: The Macmillan Company.)

THE first English edition of Ziegler's "Pathological Anatomy" was published in 1884, and at once achieved the success in this country which the original work had already attained in Germany. It is without doubt the best work in pathological anatomy in English. The present edition, translated and edited from the eighth German edition, brings the ever-increasing subject of pathological anatomy up to date, and it may be said at once that the editors have done their work in an excellent and lucid manner. The two volumes under review deal with the pathological changes occurring in particular parts of the body; and with an aspect of the subject which is of great importance to the practitioner, and of great value to the professed pathologist. The latter will find in the work copious references to the literature of each special part of the subject, arranged in a very useful manner. The ordinary medical student will perhaps find the present work (which will be followed by a third volume) too large for his purpose, but for the student for a university degree, and for the working pathologist, Ziegler's "Pathological Anatomy" is a necessity.

Eclipses of the Moon in India. By Robert Sewell, late of her Majesty's Indian Civil Service, Member of the Royal Asiatic Society, &c. Pp. 13; tables lx. (London: Swan Sonnenschein and Co., Ltd., 1898.)

THIS work is in fact a continuation and completion of Mr. Sewell's "Indian Calendar," which was noticed in NATURE for July 9, 1896 (vol. liv. p. 219). The principal matter (besides some notes and additions to the Calendar), is a table of the times, durations, and magnitudes of all eclipses of the moon (whether visible or not in India) for the period of sixteen hundred years, from A.D. 300 to A.D. 1900. The times are reduced to the Hindu prime meridian, that of Lairka (Ujjain), the longitude of which is 75° 46′ east of Greenwich, and are reckoned from mean sunrise (taken as 6h. a.m.) at that place. The calculations are founded on Oppolzer's "Canon der Finsternisse"; but another table gives the figures reduced from the *Nautical Almanac* from its commencement in 1767 (or rather 1768, as no eclipse of the moon occurred in the former year), though the figures in the "Canon" are probably more accurate than those in the Almanac before the year 1819 (not 1821), when Burckhardt's lunar tables were first brought into use in the latter. Mr. Sewell has not thought it necessary to mark the magnitude of an eclipse as greater than total, simply affixing to all such the letter "t." He acknowledges the help in the calculations afforded by Saukara Balkrishna Dikshit, formerly Pandit of the Training College, Poona, whose co-operation was so valuable in his work on the "Indian Calendar," and whose death took place early in the present year; and also expresses his thanks for kind advice and assistance given by Prof. Turner (of Oxford) and Mr. Crommelin (of the Royal Observatory, Greenwich). The precautions taken have probably secured that accuracy which is so particularly essential in matters of this kind; here we will merely point out two errors in p. 4 of the Introduction, where "fixtures" is printed instead of "figures," and Burckhardt's name is spelt without a "k," though Mr. Sewell is liberal of that letter in retaining the obsolete method of spelling "Almanac" with one.

W. T. LYNN.

Famous Problems of Elementary Geometry. By Felix Klein. Translated by W. W. Beman and D. E. Smith. Pp. ix + 80. (London: Ginn and Co., 1897.)

OUR mathematical readers who do not read German will be glad to know that they have now before them a translation of a discussion of three famous geometric problems of antiquity, namely, the duplication of the cube, the trisection of an angle, and the quadrature of the circle as seen through modern eyes. This discussion took place at Göttingen at a meeting of the German Association for the Advancement of the Teaching of Mathematics and the Natural Sciences, and was presented by the great German mathematician, Prof. Felix Klein, with the purpose of bringing the study of mathematics in the university and gymnasium into closer connection. Such an important work as this will doubtless be read very widely, and the joint translators have done good service in making this discussion more available by the excellent translation we have before us.

The Evolution of the Aryan. By R. von Ihering. Translated by A. Drucker. Pp. xviii + 412. (London: Sonnenschein and Co., Ltd., 1897.)

MR. A. DRUCKER has given us a translation of an unfinished work by the late Prof. von Ihering. Much of the argument of the book depends on theories which the leaders of linguistic science have now abandoned. Philologers now confess that community of language does not necessarily imply community of race, and Orientalists and other linguists are hopelessly at variance regarding the "Urheimat" of our race; the book, though ignoring all this, contains much wide reading and keen observation. This is apparent in matters relating to Greek, and especially to Roman, civilisation, the author's special province. In some cases a more intimate knowledge of things Indian would have improved his argument. Thus the Pali Bāveru-jātaka, known to students of folk-lore, is a very important and early witness from the Indian side to commerce between India and Babylon. The "corrective stake" (pp. 54, 55) is also illustrated by the punitive heated pillar (*sūrmī*), mentioned by Manu and earlier authorities.

Mr. Drucker's English is free and lucid; one may quite forget that one is reading a German work of science. In the first sentence of his preface, is not "latest Sanskrit and earliest Babylonian" a slip for the reverse expression?

C. B.

First Lessons in Modern Geology. By the late A. H. Green, M.A., F.R.S. Edited by J. F. Blake, M.A. Pp. viii + 208. (Oxford: The Clarendon Press, 1898.)

THE manuscript of this book was left by the late Prof. Green in a somewhat unfinished condition, and the editor was asked to prepare it for the press. The book is described in the preface as being practically a primer, yet in the third lesson, dealing, among other matters, with the constitution of quartz, after the barest statement of the proportion by weight in which silicon and oxygen combine chemically, and the introduction, with no explanation, of the term "atomic weights," we read: "All this the chemist would express shortly by writing for silica SiO_2; Si standing for twenty-eight parts by weight of silicon, O for sixteen parts by weight of oxygen, and the 2 under the O showing that in silica the oxygen is in the proportion of *twice* sixteen. SiO_2 is called the chemical formula for silica." Is this the kind of information to place before a beginner receiving his third lesson in geology? Later on in the same lesson the chemical composition of orthoclase is dealt with in a similar manner. If the beginner himself were consulted, we imagine his third lesson in geology would be his last. Had the editor omitted these little digressions, which cannot be understood by mere reading, the educational value and the interest of the book would have been much enhanced.

First Stage Inorganic Chemistry (Practical). By Frederick Beddow, D.Sc., Ph.D. Pp. viii + 165. (London: W. B. Clive.)

THE course of practical work contained in this volume follows the elementary syllabus of the Science and Art Department's examination in inorganic practical chemistry. The syllabus gives the outlines of a reasonable course of laboratory work; and therefore the present volume, like others constructed upon the same lines, has several good features. After a few introductory experiments in manipulation, and exemplifying characteristic properties of some common substances, the preparations and properties of a number of common elements and compounds are described. Following this are experiments on the action of heat, water, and acids on some familiar substances, simple quantitative experiments, and exercises in systematic analysis. The volume thus provides elementary students with an instructive course of work in practical chemistry.

Marvels of Ant Life. By W. F. Kirby, F.L.S., F.E.S. Pp. viii + 174. (London: S. W. Partridge and Co., 1898.)

ANTS and their habits form a subject of perennial interest to general readers, so Mr. Kirby's popular account of the more remarkable phases of ant life should be successful. The text is lightly written, for the benefit of general readers who are entirely unfamiliar with insect life in its scientific aspects; but there is also much in it to interest attentive students of natural history.

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 Spectrum of Krypton.

PROF. RUNGE, in your last issue, points out that the wave-length of the bright green line of krypton is 5570·4 (Rowland's scale), while that of the auroral line is about 5571. In the paper presented to the Royal Society by Dr. Travers and myself on June 3, the wave-length is given as 5567·7. The wave-length was re-measured by Mr. Baly on June 7, and its value was found to be 5570·0. That of the feebler green was 5561·8, and of D_4, the yellow line, 5870.0. These values, read by means of a grating, are very close to those given by Prof. Runge. We hope to publish photographic measurements of the other lines shortly. WILLIAM RAMSAY.

University College, London, November 15.

Stereochemistry and Vitalism.

ALTHOUGH Prof. Japp has already replied to the criticisms which have appeared in NATURE on his address to the Chemical Section of the British Association, we should be glad, in view of the interest taken in the matter, and also because we have been investigating externally compensated and optically active compounds for some years past, if you could find space in which we may continue the discussion and bring forward a few facts in support of our views.

In the first place, we will briefly summarise the main points on which there seems to be general agreement, as follows:—Compounds, optically inactive by external compensation, can be prepared under symmetric conditions—such compounds, under suitable and symmetric conditions may separate from solution in enantiomorphously related crystals; under symmetric conditions crystals of the two enantiomorphs are deposited in equal numbers, and the crystalline deposit as a whole, as well as the mother liquor, if separated, are both optically inactive.

Now Prof. Japp contended that, in order to obtain an optically active deposit or mother liquor from such an externally compensated compound (without the aid of some pre-existing asymmetric influence), an intelligently selective or vital force must be called into action, and consequently that vitalism determined the existence of optically active compounds in nature; in his reply he modifies his original contention by introducing the word "constantly"; but this modification makes very little, if any, difference to the arguments which we adduce. His critics, on the other hand, have attempted to show, with what success we do not venture to express an opinion, that chance alone, or other causes apart from vitalism, may have brought about the present occurrence of enantiomorphous compounds in organised nature.

Having frequently had occasion to study the spontaneous crystallisation of externally compensated substances, we had in mind various observations, especially some made during recent work, which led us, from the first, to doubt the validity of Prof. Japp's views. Let us consider, in the first instance, the case of sodium chlorate, a substance which separates from solution in enantiomorphously related crystals. On allowing saturated solutions of this salt to crystallise spontaneously, we found that in only two experiments out of forty-six were equal numbers of dextro-and laevo-rotatory crystals deposited (*Trans. Chem. Soc.*, 1898, 606), the percentage of dextro-rotatory crystals in the 46 crops varying from 24·14 to 77·36; nevertheless, the weighted mean percentage of dextro-rotatory crystals obtained was 50·08 ± 0·11. It is obvious, therefore, that on crystallising this substance under symmetric conditions equal numbers of dextro-and laevo-rotatory crystals are finally obtained, and yet in a single deposit one or other form may be present in large excess.

On the publication of Prof. Japp's address, we hastened to obtain further experimental evidence bearing on this point. For this purpose a saturated solution of sodium chlorate was allowed to evaporate spontaneously, three unselected crystals of the salt of unknown rotation being introduced as nuclei; after a week's time each of the three crystals had grown to a large size, and was very well developed. One of these, weighing 47 grams, was removed from the solution and broken into small pieces which were seeded into saturated solutions of the chlorate; the latter were then placed aside to crystallise. After a week's time these solutions were full of well-developed crystals, which were removed and examined; the crystals were 269 in number, and were all dextro-rotatory.

This experiment shows that an enantiomorphous system may originate from a non-enantiomorphous one without the introduction of any intelligently directive or enantiomorphous influence, and under conditions which might well arise in an inorganic non-enantiomorphous universe. This being so, it is illogical to assume in the present rudimentary state of our knowledge of the subject that enantiomorphism of this kind could not cause enantiomorphism in a system containing a substance such as racemic acid or some other externally compensated compound

Now, although a solution of sodium chlorate deposits on the average equal numbers of dextro-and laevo-rotatory crystals, this does not preclude the possibility of other similar compounds which separate from solution in enantiomorphously related crystals giving a preponderance of one or other form, owing possibly to enantiomorphous influences exerted by surrounding conditions, such, for example, as the earth's enantiomorphism; in fact, as we have already pointed out (*Trans. Chem. Soc.*, 1898, 611), observations in support of this view are not lacking, although they require, and are now receiving, further examination.

We have also shown (*Proc. Chem. Soc.*, 1898, 113) that, apparently, a close analogy exists between the behaviour of sodium chlorate and of an externally compensated mixture of dextro- and laevo-rotatory sodium ammonium tartrates; this parallel we are at present investigating. The first step in our examination of sodium chlorate was to ascertain the ratio between the numbers of dextro- and laevo-rotatory crystals deposited from solution, with the results briefly summarised above; obviously a similar step is necessary in the case of the mixed tartrates. The results which have been obtained in these experiments contrast remarkably with those recorded in the case of sodium chlorate, and are absolutely at variance with the views expressed by Prof. Japp. Ten such solutions have so far been examined, and in every case a strongly dextro-rotatory deposit was obtained, the mother liquor, of course, being strongly laevo-rotatory. We put these experimental results forward with considerable reservation as the work is not complete and we may discover some disturbing influence; but, inasmuch as attention has of late been concentrated on this fascinating subject by Prof. Japp's equally fascinating presentment of it, we feel compelled to make this short statement of the results up to the present obtained in developing the work which we have in hand.

Much more might be written with a view to suggesting the possible sequence of events which might have led to the present conditions of organised nature, but it is of little use attempting such a task until a great deal more experimental evidence is before us.

F. STANLEY KIPPING.
WILLIAM J. POPE.

THE following seems to me a perfectly possible, although purely hypothetical, way in which any amount of an optically active compound could be formed by chance chemical processes.

Let an admixture of chemical compounds be such that only one molecule of an optically active compound is formed. Or, if you like, let a volcanic explosion scatter a collection of equal quantities of dextro and laevo molecules in such a way that one molecule only falls in a certain pool of water containing inactive bodies in solution.

This single molecule must be either dextro or laevo. Let us suppose it is a dextro molecule.

Furthermore, let it be of such a nature that it can react catalytically with surrounding molecules in such a way as to produce in these an asymmetric atom:

e.g.

$$(1)\quad \begin{matrix} \mathrm{C}d_2 \\ \Vert \\ \mathrm{C}ab \end{matrix} + \mathrm{RH_2} = \begin{matrix} \mathrm{C}d_2\mathrm{H} \\ | \\ \underline{\underline{\mathrm{C}}}ab\mathrm{RH} \end{matrix}.$$

$$(2)\quad \begin{matrix} \mathrm{C}d_2\mathrm{H} \\ | \\ \underline{\underline{\mathrm{C}}}ab\mathrm{RH} \end{matrix} + \mathrm{H_2} = \begin{matrix} \mathrm{C}d_2\mathrm{H} \\ | \\ \underline{\underline{\mathrm{C}}}ab\mathrm{H} \end{matrix} + \mathrm{RH},$$

where *abcd* are any monovalent radical not containing an asymmetric carbon atom, while R is a dextro-rotatory bivalent radical. The first molecule formed, according to equation (1), will have the underlined C dextro or laevo, being determined by R being dextro.

When this decomposes according to equation (2) the $\underline{\underline{C}}$ will still be active, and one active molecule of $\begin{array}{l} \mathrm{C}d_2\mathrm{H} \\ | \\ \underline{\underline{\mathrm{C}}}ab\mathrm{H} \end{array}$ will have been produced.

But RH_2 is reproduced, and is consequently capable of repeating this action indefinitely, and of making an indefinite number of $\begin{array}{l} \mathrm{C}d_2\mathrm{H} \\ | \\ \underline{\underline{\mathrm{C}}}ab\mathrm{H} \end{array}$ molecules with activity of the same sign.

Furthermore, this action is scarcely to be distinguished in principle from the reactions brought about by unorganised ferments or enzymes. W. M. STRONG.

Helstonleigh, Champion Park, Denmark Hill, S.E.,
November 1.

IT appears to me that Prof. Kipping and Mr. Pope unintentionally attribute to me opinions which I have never expressed, and which I do not hold. I never for a moment imagined that *in each separate crystallisation*—either of molecularly symmetric substances which, like sodium chlorate, may form either right-handed or left-handed crystals, or of the externally compensated mixture of dextro- and laevo-rotatory sodium ammonium tartrates, in which the asymmetry is molecular—equal amounts of the two kinds of crystals would necessarily be deposited. I never thought of this equality as holding good, except as the mean of a great number of experiments. In my address, when referring to Messrs. Kipping and Pope's results obtained with sodium chlorate, I therefore used the expression "on the average." Besides, I was acquainted with Landolt's experiments on the subject, which prove the same thing. In the case of the dextro- and laevo-rotatory sodium ammonium tartrates, the Pasteur-Gernez method of separating these by starting the crystallisation with a crystal of one of the two kinds, and Jungfleisch's experiments, to which I will refer more fully later on, were sufficient to make me aware of the influence of initial bias on crystallisation, and to prevent me from expecting equality, except as a mean result.

In fact, in these crystallisations everything depends on this initial bias; and in this respect, Landolt's experiments are especially instructive. His method (*Ber. d. deutsch. chem. Ges.*, 1896, p. 2410) consists in precipitating aqueous solutions of sodium chlorate by addition of alcohol. A magma of minute crystals is thus obtained, each of which must be either right-handed or left-handed. The crystals are then separated and ground to a fine powder; and this powder, suspended in a mixture of alcohol and carbon disulphide, is examined in the polarimeter. If both enantiomorphous forms are present in equal quantity, there will be no rotatory effect; whereas a preponderance of either form will be indicated by a rotation in the corresponding sense.

Landolt found that if the precipitation was rapidly effected by the sudden addition of alcohol, the powder was either inactive, or had a very feeble rotation. On the other hand, if the alcohol was gradually added, so as to produce slow precipitation, the powder displayed a marked rotation, which was in some cases right-handed, in others left-handed. The reason is obvious. In the former case, the crystallisation starts simultaneously from a vast number of independent centres; the chances are equal in favour of each centre being right-handed or left-handed; each centre will propagate its own kind; and thus the ratio of right-handed to left-handed forms will not differ very appreciably from unity. In the latter case, as Landolt points out, the crystallisation starts from only a few centres; in these, therefore, either the right-handed or the left-handed form may predominate, and, as sufficient time is given, the dominant form will influence the course of the crystallisation.

In Messrs. Kipping and Pope's slow crystallisations, every opportunity is afforded for any initial asymmetric bias to exercise its influence; and I should have been greatly surprised if the authors had obtained a result other than that which they describe.

There is, therefore, a marked difference between the formation of enantiomorphous crystals and of enantiomorphous molecules respectively, under symmetric conditions: namely, that the crystals propagate their own asymmetry, whilst the molecules, as I pointed out in my reply to Prof. Karl Pearson, do not—at least, so far as experiment informs us

In the crystallisation of the externally compensated mixture of dextro- and laevo-rotatory sodium ammonium tartrates, one would expect a similar state of things to prevail, modified however by the circumstance that in this case, the two opposite asymmetries of the molecules themselves are pre-existent in the solution and must influence the result. One would expect variations, within limits, of the relative quantities of dextro- and laevo-salts deposited; but according to our present views, *the mean variations should occur equally in opposite directions.*

It is therefore with great surprise that I learn that in the ten experiments which Messrs. Kipping and Pope have hitherto made on this point they obtained in every case a strongly dextro-rotatory deposit. It is true that Jungfleisch (compare *Chem. News*, vol. 40, p. 231) published, in 1884, similar results. Jungfleisch's procedure was somewhat different from that of Messrs. Kipping and Pope. Instead of allowing the solution to crystallise spontaneously, he introduced simultaneously into the supersaturated solution, at opposite sides of the crystallising dish, a crystal of the dextro-rotatory and one of the laevo-rotatory salt. In all his experiments the former crystal increased in weight, and the mother liquor contained a corresponding excess of the laevo-salt. I confess that I have always attributed Jungfleisch's results to mere coincidence. It will doubtless be within Prof. Kipping's recollection that in April last I wrote to him calling his attention to these results, in connection with his joint work with Mr. Pope. Prof. Kipping replied (I quote from memory), that Jungfleisch's results were opposed to all that was known of the behaviour of enantiomorphs; that he (Prof. Kipping) did not regard them as conclusive; and that he proposed to repeat them. This he and Mr. Pope have now done; but from what I have just said, it is evident that the outcome of this repetition must have been as great a surprise to him as it is to me. I am glad to find, however, that the authors put forward their results with reserve, and that they contemplate the possibility of discovering some disturbing influence. If no such influence is at work, it is not merely the small matter of my particular application of the principles of molecular asymmetry that will have to go: it is these principles themselves. But I do not anticipate any such catastrophe. The vast accumulation of verified prediction of which the science of stereochemistry can boast, does not point to premises so unsound.

I will wait, therefore, for further light on Messrs. Kipping and Pope's later experiments before drawing any conclusion from them. As regards the experiments with sodium chlorate, I am at a loss to understand in what way they militate against my views. I no more expect equal numbers of right and left crystals to separate in any given crystallisation of sodium chlorate than I expect an unvarying alternation of heads and tails in tossing a coin. Nor do I perceive the application of the experiment in which they obtained none but right-handed crystals of the chlorate; inasmuch as, if they go on repeating it, they will obtain, just as often, none but left-handed crystals. The process will not *constantly* yield the same form.

I must also repeat that I do not regard the spontaneous formation of "optically active mother liquors," or other partial separations of enantiomorphs, especially when the separation may occur in either direction, as a solution of the problem. The separation of enantiomorphs in the living world is, in the overwhelming majority of cases, as I have pointed out in a previous reply, not partial but *total;* and it occurs *constantly* in one direction—only one form survives. This is another case where the word "constantly" applies—a word quoted by the authors, but otherwise ignored by them, except where they are discussing experiments which they themselves regard as requiring confirmation.

I think, moreover, that my critics might in justice take into account the disadvantage under which I laboured in having to compress into the limits of a brief address an account of a subject so vast and intricate. Many of my statements of important points were necessarily somewhat summary and inadequate; and most of the misunderstandings into which my various critics have fallen are, I am aware, due to this circumstance.

The University, Aberdeen, November 10. F. R. JAPP.

MR. STRONG'S suggestion is very ingenious, and I must admit that, *granting his premises*, the chance production of an unlimited quantity of a single asymmetric compound is conceivable. I had not thought of the possibility of one asymmetric molecule acting as a catalytic agent in the way he suggests. It would, however, have been perhaps simpler and more in accordance with the behaviour of enzymes, with which class of ferments he compares this supposed catalytic agent, if he had represented the second stage of the process as a hydrolysis; in which case, of course, the asymmetric group of the resulting compound would have contained hydroxyl in place of hydrogen.

Mr. Strong admits that the process is "purely hypothetical." I think I should go further than this, and say that, considered as an actual process occurring under chance conditions, it is grotesquely improbable.

The "volcanic explosion" carrying "one molecule" of an asymmetric compound into "a certain pool of water," seems to be a reproduction (on a reduced scale) of Prof. Errera's "vortex" which whirls "one simply asymmetric particle" into a particular "planet" (see NATURE, vol. lviii. p. 616, col. 2). F. R. JAPP.

The University, Aberdeen, November 5.

Mental Calculations of a High Order.

THERE are probably among your readers some who are interested, by curiosity or for scientific purposes, in freaks of memory. I am not sure that what my memory has done is remarkable, although it is quite novel to me.

For many years I have been in the habit of using some useless exercise in mental gymnastics to divert my mind from the occupations of the day, and so get quickly asleep. Sometimes it would be extracting the letters of the alphabet successively from some passage in prose or poetry, keeping the number of each letter in mind, and finally counting all the letters in the passage, to make sure that I had allowed no letter to pass by me unnoticed. Again, I would try to think of all the famous poets, or generals, or sovereigns, or statesmen of all time, whom I could recall, in alphabetical order. Whatever might be the task I undertook I resumed it night after night, beginning as nearly as possible where I left off, and continue until I had completed it.

About a year ago the fancy took me to see how far I could go in raising, by mental process only, the number 3 to its high powers. At the beginning I would not have believed it possible to remember fifteen figures in their order. To my surprise I succeeded in raising 3 to the 44th power, making, if I remember rightly, a number of 22 figures. I did it by successive multiplications by 3, and without shortening the process in any way. I did not put a figure on paper until I had reached the 24th power; but always proved every result as far as I could by the nine test—a safeguard against substantially every error save those that might arise from transposition of figures. At the point mentioned I set the product down, and performed on paper this short process:

$$3^2 = 9;\ 3^4 = 81;\ 3^8 = 6561;\ 3^{24} = 6561 \times 6561 \times 6561.$$

As my mental result was wrong in four or five figures in the middle, so to speak, I knew that I must have transposed two figures somewhere between power 15 and power 20, so I went back and began over again. At the 24th power I was right, and so I was when I reached the 44th power.

That seemed to be as far as it was worth while to go, and I then began a more difficult exercise: to ascertain, as in permutations, the product of the numbers from one upward as far as I could go. I have carried the process up to, and including the number 37. The product is a number of 44 figures, whereof the last eight are o's, which do not add to the effort of memory to retain them. To remember 36 digits in their correct order may not be a wonderful feat; it is so easy to me that I do not suppose it is unusual. But I can now remember, and have to-day written down and then repeated to my stenographer, successively, the product of the numbers to 35, to 36, and to 37, having respectively 33, 34 and 36 figures, beside the eight o's with which each number ends, or 103 figures in all. How much further I could carry the process I do not know; I do not purpose attempting to ascertain.

I will add some facts that may be interesting.

(1) I verify my result after each multiplication: first, by proving that the sum of all the digits is a multiple of 9; secondly, by dividing it by 7, 11 and 13, not attempting to remember the quotient, but only the successive remainders, to be sure that the number divides evenly.

(2) Almost every product has some peculiar combination of numbers. For example, in the 35 result there are four 6's together; in the 36 the figures 6789 occur; the first six figures of the 37 product are 137,637; and so on.

(3) The work is done in groups of three figures, and almost every new factor in the multiplication gives some short process of multiplying. Before I begun with 37 it seemed impossible to multiply 34 figures by such a number, odd, large, and a prime number. But the fact that $37 \times 3 = 111$ soon suggested the way to make the process easy. The last nine figures (omitting o's) of the 36 product are 481,508,352. Now $352 = 360 (3 \times 12 \times 10) - 8$. Then we have $111 \times 12 = 1332 \times 10 = 13{,}320$. $37 \times 8 = 296$. $13{,}320 - 296 = 13{,}024$. The multiplication of the next group is easy. $37 \times 500 = 18500$; add 296 $(37 \times 8) + 13$ ("carried") $= 18{,}809$. The next group, 481, is taken as $= 500 - 20 + 1$, and the number with the 18 "carried" from the last multiplication becomes 17,815,809,024.

(4) Does this exercise put me to sleep? O, yes, very quickly!

Boston, U.S.A., October 24. E. S.

The Leonids in 1868.

I WITNESSED the magnificent shower of Leonid meteors on the night of November 13–14, 1866. But I do not recollect seeing any published account of such a display in England in 1868. It occurs to me that the following observations may be worth publishing.

On November 5 in that year I was in Venice. Returning through Milan, I crossed the Alps in a sledge by the St. Gothard in a terrible snow-storm on the 7th, and reached Calais at midnight on the 13th. Neither sun, moon, nor stars had been visible since I left Venice. The Calais boat started for Dover about 1h. 30m. on the morning of the 14th. As we were leaving the port the clouds suddenly cleared off, and a splendid display of Leonids was visible. I judged the shower to be in every respect equal to that of 1866. Some of them were as bright as Jupiter, and left long trails in the sky which took two or three minutes to dissolve. The display kept up until we were within a couple of miles of Dover, when the clouds suddenly came on again and the sky was completely obscured.

Coventry, November 12. WM. ANDREWS.

The Smell of Earth.

"SEE, the smell of my son is as the smell of a field which the Lord hath blessed." Thus poetically spoke the Patriarch Isaac. The man of modern science tells us, prosaically, that the odour of moist earth is due to a bacterium, named *Cladothrix odorifera*. I write to ask if any one has yet accounted for the well-known and peculiar odour, yielded by clay and clayey rocks when breathed upon. This odour can scarcely be due to bacteria, for it is manifested by cabinet specimens more than twenty years old. Pure alumina appears to be odourless.

Leeds, November 12. C. T. WHITMELL.

Breath-Figure of Spider's Web.

A FEW mornings ago I noticed in my bath-room a spider's web spun right across one pane of the window, but not in actual contact with the glass, there being room for a house-fly to buzz up and down the pane without touching the meshes. My morning ablutions giving rise to some considerable quantity of vapour, I observed a very distinct breath-figure of the spider's web upon the glass. I accordingly removed the web and the spider. Next morning, in the absence of the web, on the renewal of the vapour conditions the breath-figure reappeared. I then wiped one half of the window dry with a towel. Now, after five mornings, the breath-figure is quite distinct upon the half which was not touched, and can be faintly seen on the wiped portion. OSWALD H. LATTER.

Charterhouse, Godalming, November 13.

A Second Crop of Apples.

I THINK perhaps it may be worth noting that apple-blossom was gathered in the neighbourhood of Exeter last week. Still more remarkable is the fact that a second crop of apples has made fair progress, as some at the farm of Grâs Lawn, close to the city, some "Red Quaranders" have been gathered, nearly the size of walnuts. Two of these, now somewhat shrivelled, are enclosed. JAMES DALLAS.

Exeter, October 14.

CONTINUITY OF WAVE THEORIES.[1]

CONSIDER the following three analogous cases :—I. mechanical, II. electrical, III. electromagnetic.

I. Imagine an ideally rigid globe of solid platinum of 12 centim. diameter, hung inside an ideal rigid massless spherical shell of 13 centim. internal diameter, and of any convenient thickness. Let this shell be hung in air or under water by a very long cord, or let it be embedded in a great block of glass, or rock, or other elastic solid, electrically conductive or non-conductive, transparent or non-transparent for light.

I. (1) By proper application of force between the shell and the nucleus cause the shell and nucleus to vibrate in opposite directions with simple harmonic motion through a relative total range of 10^{-3} of a centimetre. We shall first suppose the shell to be in air. In this case, because of the small density of air compared with that of platinum, the relative total range will be practically that of the shell, and the nucleus may be considered as almost absolutely fixed. If the period is $\frac{1}{32}$ of a second, frequency 32 according to Lord Rayleigh's designation, a humming sound will be heard, certainly not excessively loud, but probably amply audible to an ear within a metre or half a metre of the shell. Increase the frequency to 256, and a very loud sound of the well-known musical character (C_{256}) will be heard.[2]

Increase the frequency now to 32 times this, that is to 8192 periods per second, and an exceedingly loud note 5 octaves higher will be heard. It may be too loud a shriek to be tolerable ; if so, diminish the range till the sound is not too loud. Increase the frequency now successively according to the ratios of the diatonic scale, and the well-known musical notes will be each clearly and perfectly perceived through the whole of this octave. To some or all ears the musical notes will still be clear up to the G (24756 periods per second) of the octave above, but we do not know from experience what kind of sound the ear would perceive for higher frequencies than 25000. We can scarcely believe that it would hear nothing, if the amplitude of the motion is suitable.

To produce such relative motions of shell and nucleus as we have been considering, whether the shell is embedded in air, or water, or glass, or rock, or metal, a certain amount of work, not extravagantly great, must be done to supply the energy for the waves (both condensational and rarefactional), which are caused to proceed outwards in all directions. Suppose now, for example, we find how much work per second is required to maintain vibration with a frequency of 1000 periods per second, through total relative motion of 10^{-3} of a centimetre. Keeping to the same rate of doing work, raise the frequency to 10^4, 10^5, 10^6, 10^9, 10^{12}, 500×10^{12}. We now hear nothing ; and we see nothing from any point of view in the line of the vibration of the centre of the shell which I shall call the axial line. But from all points of view, not in this line, we see a luminous point of homogeneous polarised yellow light, as it were in the centre of the shell, with increasing brilliance as we pass from any point of the axial line to the equatorial plane, keeping at equal distances from the centre. The line of vibration is everywhere in the meridional plane, and perpendicular to the line drawn to the centre.

[1] "Continuity in undulatory theory of condensational-rarefactional waves in gases, liquids, and solids, of distortional waves in solids, of electric waves in all substances capable of transmitting them, and of radiant heat, visible light, ultra-violet light." Communicated by Lord Kelvin, G.C.V.O., being the substance of a communication to Section A of the British Association at its recent meeting in Bristol.

[2] Lord Rayleigh has found that with frequency 256, periodic condensation and rarefaction of the marvellously small amount 6×10^{-9} of an atmosphere, or "addition and subtraction of densities far less than those to be found in our highest vacua," gives a perfectly audible sound. The amplitude of the aerial vibration, on each side of zero, corresponding to this is $1 \cdot 27 \times 10^{-7}$ of a centimetre.—"Sound," vol. ii. p. 439 (second edition).

When the vibrating shell is surrounded by air, or water, or other fluid, and when the vibrations are of moderate frequency, or of anything less than a few hundred thousand periods per second, the waves proceeding outwards are condensational-rarefactional, with zero of alternate condensation and rarefaction at every point of the equatorial plane and maximum in the axial line. When the vibrating shell is embedded in an elastic solid extending to vast distances in all directions from it, two sets of waves, distortional and condensational-rarefactional, according respectively to the two descriptions which have been before us, proceed outwards with different velocities, that of the former essentially less than that of the latter in all known elastic solids.[1] Each of these propagational velocities is certainly independent of the frequency up to 10^4, 10^5, or 10^6, and probably up to any frequency not so high but that the wave-length is a large multiple of the distance from molecule to molecule of the solid. When we rise to frequencies of 4×10^{12}, 400×10^{12}, 800×10^{12}, and 3000×10^{12}, corresponding to the already known range of long-period invisible radiant heat, of visible light, and of ultra-violet light, what becomes of the condensational-rarefactional waves which we have been considering? How and about what range do we pass from the propagational velocities of 3 kilometres per second for distortional waves in glass, or 5 kilometres per second for the condensational waves in glass, to the 200,000 kilometres per second for light in glass, and, perhaps, no condensational wave? Of one thing we may be quite sure ; the transition is continuous. Is it probable (if aether is absolutely incompressible, it is certainly possible) that the condensational-rarefactional wave becomes less and less with frequencies of from 10^6 to 4×10^{12}, and that there is absolutely none of it for periodic disturbances of frequencies of from 4×10^{12} to 3000×10^{12}? There is nothing unnatural or fruitlessly ideal in our ideal shell, and in giving it so high a frequency as the 500×10^{12} of yellow light. It is absolutely certain that there is a definite dynamical theory for waves of light, to be enriched, not abolished, by electromagnetic theory ; and it is interesting to find one certain line of transition from our distortional waves in glass, or metal, or rock, to our still better known waves of light.

I. (2) Here is another still simpler transition from the distortional waves in an elastic solid to waves of light. Still think of our massless rigid spherical shell, 13 centim. internal diameter, with our solid globe of platinum, 12 centim. diameter, hung in its interior. Instead of as formerly applying simple forces to produce to-and-fro rectilinear vibrations of shell and nucleus, apply now a proper mutual forcive between shell and nucleus to give them oscillatory rotations in contrary directions. If the shell is hung in air or water, we should have a propagation outwards of disturbance due to viscosity, very interesting in itself ; but we should have no motion that we know of appropriate to our present subject until we rise to frequencies of 10^9, 10×10^{12}, 400×12^{12}, 800×10^{12}, or 3000×10^{12}, when we should have radiant heat, or visible light, or ultra-violet light proceeding from the outer surface of the shell, as it were from a point-source of light at the centre, with a character of polarisation which we shall thoroughly consider a little later. But now let our massless shell be embedded far in the interior of a vast mass of glass, or metal, or rock, or of any homogeneous elastic solid, firmly attached to it all round, so that neither splitting away nor tangential slip shall be possible. Purely distortional waves will spread out in all directions except the axial. Suppose, to fix our ideas, we begin with vibrations of one-second period, and let the elastic solid be either glass or iron. At distances of hundreds of kilometres (that is to say, distances great in comparison with the wave-length and

[1] "Math. and Phys. Papers," vol. iii., art. civ. p. 522.

great in comparison with the radius of the shell), the wave-length will be approximately 3 kilometres.[1] Increase the frequency now to 1000 periods per second: at distances of hundreds of metres the wave-length will be about 3 metres. Increase now the frequency to 10^6 periods per second; the wave-length will be 3 millim., and this not only at distances of several times the radius of the shell, but throughout the elastic medium from close to the outer surface of the shell; because the wave-length now is a small fraction of the radius of the shell. Increase the frequency further to 1000×10^6 periods per second; the wave-length will be 3×10^{-3} of a millim., or 3 mikroms,[2] if, as in all probability is the case, the distance between the centres of contiguous molecules in glass and in iron is less than a five-hundredth of a mikrom. But it is probable that the distance between centres of contiguous molecules in glass and in iron is greater than 10^{-5} of a mikrom, and therefore it is probable that neither of these solids can transmit waves of distortional motion of their own ponderable matter, of so short a wave-length as 10^{-5} of a mikrom. Hence it is probable that if we increase the frequency of the rotational vibrations of our shell to one hundred thousand times 1000×10^6, that is to say, to 100×10^{12}, no distortional wave of motion of the ponderable matter can be transmitted outwards; but it seems quite certain that distortional waves of radiant heat in aether will be produced close to the boundary of the vibrating shell, although it is also probable that if the surrounding solid is either glass or iron, these waves will not be transmitted far outwards, but will be absorbed, that is to say, converted into non-undulatory thermal motions, within a few mikroms of their origin.

Lastly, suppose the elastic solid around our oscillating shell to be a concentric spherical shell of homogeneous glass of a few centimetres, or a few metres, thickness and of refractive index 1·5 for D light. Let the frequency of the oscillations be increased to $5{\cdot}092 \times 10^{14}$ periods per second, or its period reduced to ·589212 of a michron: homogeneous yellow light of period equal to the mean of the periods of the two sodium lines will be propagated outwards through the glass with wave-length of about $\frac{2}{3} \times {\cdot}589212$ of a mikrom, and out from the glass into air with wave-length of ·589212 of a mikrom. The light will be of maximum intensity in the equatorial plane and zero in either direction along the axis, and its plane of polarisation will be everywhere the meridional plane. It is interesting to remark that the axis of rotation of the aether for this case coincides everywhere with the line of vibration of the aether in the case first considered; that is to say, in the case in which the shell vibrated to and fro in a straight line, instead of, as in the second case, rotating through an infinitesimal angle round the same line.

A full mathematical investigation of the motion of the elastic medium at all distances from the originating shell, for each of the cases of I. (1) and I. (2), will be found in a volume containing my Baltimore Lectures on "Molecular Dynamics and the Wave-Theory of Light," soon, I hope, to be published.

II. An electrical analogy for I. (1) is presented by substituting for our massless shell an ideally rigid, infinitely massive shell of glass or other non-conductor of electricity, and for our massive platinum nucleus a massless non-conducting globe electrified with a given quantity of electricity. For simplicity we shall suppose our apparatus to be surrounded by air or aether. Vibrations to and fro in a straight line are to be maintained by force between shell and nucleus as in I. (1). Or, consider simply a fixed solid non-conducting globe coated with two circular caps of metal, leaving an equatorial non-conducting zone between them, and let thin wires from a distant alternate-current dynamo, or electrostatic inductor, give periodically varying opposite electrifications to the two caps. For moderate frequencies we have a periodic variation of electrostatic force in the air or aether surrounding the apparatus, which we can readily follow in imagination, and can measure by proper electrostatic measuring apparatus. Its phase, with moderate frequencies, is very exactly the same as that of the electric vibrator. Now suppose the frequency of the vibrator to be raised to several hundred million million periods per second. We shall have polarised light proceeding as if from an ideal point-source at the centre of the vibrator and answering fully to the description of I. (1). Does the phase of variation of the electrostatic force in the axial line outside the apparatus remain exactly the same as that of the vibrator? An affirmative answer to this question would mean that the velocity of propagation of electrostatic force is infinite. A negative answer would mean that there is a finite velocity of propagation for electrostatic force. This velocity, according to views regarding conceivable qualities of aether described in my article "On the Reflection and Refraction of Light" (*Phil. Mag.*, vol. xxvi. 1888) might be greater than, equal to, or less than the velocity of light.

III. The shell and interior electrified non-conducting massless globe being the same as in II., let now a forcive be applied between shell and nucleus to produce rotational oscillations as in I. (2). When the frequency of the oscillations is moderate, there will be no alteration of the electrostatic force and no perceptible magnetic force in the air or aether around our apparatus. Let now the frequency be raised to several hundred million million periods per second; we shall have visible polarised light proceeding as if from an ideal point-source at the centre and answering fully to the description of the light of I. (2) The same result would be obtained by taking simply a fixed solid non-conducting globe and laying on wire on its surface approximately along the circumferences of equidistant circles of latitude, and, by the use of a distant source (as in II.) sending an alternate current through this wire. In this case, while there is no manifestation of electrostatic force, there is strong alternating magnetic force, which in the space outside the globe is as if from an ideal infinitesimal magnet with alternating magnetisation, placed at the centre of the globe and with its magnetic axis in our axial line.

[1] "Math. and Phys. Papers," vol. iii. art. civ. p. 522.

[2] For a small unit of length Langley, fourteen years ago, used with great advantage and convenience the word "mikron" to denote the millionth of a metre. The letter *n* has no place in the metrical system, and I venture to suggest a change of spelling to "mikrom" for the millionth of a metre, after the analogy of the English usage for millionths (mikrohm, mikro-ampere, mikrovolt). For a conveniently small corresponding unit of time I further venture to suggest "michron" to denote the period of vibration of light whose wave-length in aether is 1 mikrom. Thus, the velocity of light in aether being 3×10^8 metres per second, the michron is $\frac{1}{3} \times 10^{-14}$ of a second, and the velocity of light is 1 mikrom of space per michron of time. Thus the frequency of the highest ultra-violet light investigated by Schumann (·1 of a mikrom wave-length, *Sitzungsber. d. k. Gesellsch. d. Wissensch. zu Wien*, cii. pp. 415 and 625, 1893) is 10 periods per michron of time. The period of sodium light (mean of lines D) is ·589212 of a michron; the periods of the "Reststrahlen" of rocksalt and sylvin found by Rubens and Aschkinass (*Wied. Ann.*, lxv. (1898, p. 241) are 51·2 and 61·1 michrons respectively.

No practical inconvenience can ever arise from any possible confusion, or momentary forgetfulness, in respect to the similarity of sound between michrons of time and mikroms of space.—K.

THE SERUM TREATMENT OF RINDERPEST.

THE Report of the Colonial Veterinary Surgeon and the Assistant Veterinary Surgeons of the Cape of Good Hope is at the present time a document of very considerable interest, as it is possible from the facts there set forth to form some definite opinion as to the efficacy of the various methods of protection and treatment against rinderpest that have been recommended in the Colony. It appears that there is now a possibility of completely eradicating rinderpest from South Africa, not, however, by means of the bile method—which was originally introduced by Koch, and appears to induce only a temporary though "active" immunity against the disease—but by means of the use of a "serum."

Drs. Turner and Kollé have, as already reported in these columns, found that it is possible to treat rinderpest successfully with a serum taken from animals which have already suffered from the disease produced either artificially or naturally and reinforced artificially. As a result of their investigations they are now in a position, with the aid of this serum, to control the course of the disease when artificially produced. As animals which have suffered from the disease are immune for a very considerable period, and as an animal that passes through an attack controlled by the serum is protected or "salted," as it is termed, this method has been adopted very extensively for the purpose of protecting animals in affected and suspected districts, and it is probable that in time it will oust the bile method completely; especially as it seems to be attended by fewer disadvantages than accompany the latter method, and can be carried out at less cost and with less loss of life amongst the cattle.

In working with the serum method in rinderpest it is necessary to know as much about the serum used as it is in the case of the treatment of diphtheria with anti-diphtheric serum; but one would imagine that at the Cape, where such enormous financial interests are concerned, there should be no difficulty in obtaining pure serum, the value of which has been carefully estimated, for the treatment of cattle suffering from the earlier symptoms of rinderpest. The relative cost would be slight even were it necessary to have a series of stations from which serum might be obtained, and taking into consideration the fact that this serum might have to be thrown away periodically; and one can scarcely imagine that a method which is described as being nearly perfect, should be allowed to remain unused because there is some slight difficulty as regards the supply of money and laboratory accommodation. In connection with this part of the question, it may be mentioned that it has been proved by experiment that the serum remains quite active for at least seven months. How much longer, still remains to be proved.

It will be remembered that on a former occasion it was pointed out that "it is useless to attempt to infect whole herds which have been previously inoculated with bile, because it does not seem possible to give all such inoculated animals the disease simultaneously. Most of the cattle will become sick after injection of virulent blood, but many may remain unaffected, as the immunity conferred by the bile is still effective and protects them, and they will in consequence not become salted. In these herds the most satisfactory course to pursue is for the owner to obtain a sufficient supply of serum, and inject the cattle as soon as they are observed to be sick, or the thermometer indicates a rise of temperature. Cattle already sick require a larger dose of serum than those that are healthy, and in severe cases a second may be necessary." This, however, as pointed out by the Veterinary Surgeon, is only an indication that the serum must be injected at a still earlier stage than has hitherto been done, so that it is necessary to apply the serum treatment to a herd of cattle which has previously been inoculated with bile soon after the injection of the virulent blood. Cattle that would have reacted to the blood take the disease in a modified form, and become salted; whilst those that would not react are no worse off for the addition of the serum, and probably have a slight addition to their passive immunity. The principal Veterinary Surgeon points out that "the fact that the immunity conferred by bile is now wearing off, makes it necessary for the farmers to take prompt action when the disease appears in their herds, and the fact that the bile immunity has ceased, gives greater prospects of success attending the application of the serum as above indicated." He also points out that infection is not nearly so frequently carried from artificially infected herds as from those which take the disease naturally.

Bearing on this question, there is an interesting article in the August number of the *Agricultural Journal and Mining Record*, published in Natal. The injection of bile, unless very carefully selected and preserved, appears in many cases to have set up chronic abscesses, from the description of which one would have said that they were actinomycotic or discomycotic in character, though this is a question which appears to deserve further investigation. From the annual report of the Commissioner of Agriculture for the year 1897, which appears in the same journal, it is evident that the farmers are now thoroughly alive to the importance of using bile where serum cannot be obtained, but of having recourse to serum inoculation along with the injection of blood from diseased animals, when this method can be properly carried out. The only real drawback, and this is a very slight one indeed, appears to be that in a few cases the injected blood appears to set up some form of "red water," whilst in a few instances a malarial parasite may be transmitted along with the blood.

An idea of the expenditure on rinderpest for the year 1897 may be gathered from the fact that the outlay for fences, &c., was nearly 170,000*l*. A very small proportion of this, added to the laboratory and veterinary outlay, would enable the department to cope with this disease in future, and to save the Colony an enormous sum, both directly and indirectly. Mr. Watkins Pitchford, in his report, states that where the departmental instructions for the use of the serum have been adhered to, good "and often brilliant results have repeatedly been achieved," and that he is right in his conjecture is very evident from the fact that when the serum is given in definite quantity on one side and rinderpest blood on the other, but simultaneously, a reaction is invariably set up in an unsalted animal; and even when this reaction is not very severe, the nature of the disease is so well defined and the results so successful that the animal is perfectly salted for some time against natural infection, and frequently against the injection of rinderpest blood: the severest test to which the method can be put. He believes thoroughly in the permanent immunisation of the herds as a means of stamping out rinderpest; but immunisation with bile, he believes, would fail, although it may be useful as a temporary measure where serum cannot be obtained. It must be remembered, however, that the immunisation of the first animal must as a rule be through the use of bile. He is strongly in favour of maintaining a trustworthy supply of serum for the cases as they may appear, and for the immunisation of any stock in herds which have already passed through the disease, but in which the immunity is still not quite sufficient. Those who are most intimately acquainted with the history of rinderpest, and of the recent work on serum-therapy in connection with this disease, are now satisfied that the extinction of rinderpest amongst Cape cattle is already practically accomplished.

NOTES.

ON Friday last, November 11, the Chemical Society gave a banquet in honour of six of its Past-Presidents who had been for over half a century Fellows of the Society. The occasion was the more noteworthy that these eminent chemists—Gilbert, Frankland, Odling, Abel, Williamson, and Gladstone—are in many cases ranked among those whose teaching has most deeply influenced chemical science. As M. Friedel said: "They form the finest phalanx of the fathers of chemistry which exists in any country." The season of the year prevented many foreign chemists from attending, but a great number of letters and telegrams were received. Amongst the men of science present were Prof. Armstrong, Sir J. Wolfe Barry, Sir G. Birdwood, Sir J. Crichton Browne, Dr. Böttinger, Major-General Sir Owen Tudor Burne, Dr. T. Lauder Brunton, the Vice-Chancellor of Cambridge University, Sir Ernest Clarke, Sir W. Crookes, Major-General Sir J. Donnelly, Prof. Wyndham Dunstan, Dr. Dupré, Dr. Dyer, Mr. T. H. Elliott, Mr. H. M. Elder, Sir John Evans, Major-General Festing, Prof. Michael Foster, Prof. P. Frankland, Sir Archibald Geikie, Mr. W. Gowland, Mr. C. E. Groves, Mr. W. Hills, Sir W. Huggins, Prof. Judd, Dr. W. Kellner, Lord Lister, Major P. A. MacMahon, the President of Magdalen, Prof. Herbert McLeod, Prof. Raphael Meldola, Dr. Rudolph Messel, Prof. H. A. Miers, Prof. G. M. Minchin, Dr. Ludwig Mond, Mr. J. Fletcher Moulton, Q.C., M.P., Dr. Hugo Müller, Sir A. Noble, Prof. W. Ostwald, Dr. W. H. Perkin and Prof. W. H. Perkin, the Master of Peterhouse, Dr. Pye-Smith, Mr. Boverton Redwood, Prof. Ramsay, Lord Rayleigh, Lord Reay, Prof. Roberts-Austen, Prof. Rücker, Dr. Russell, Prof. Smithells, Prof. Sprengel, Sir G. Gabriel Stokes, Mr. J. W. Swan, Prof. J. M. Thomson, Dr. Thorpe, Prof. Tilden, Mr Tyrer, and Sir H. Trueman Wood. We hope to give in another issue a full report of the speeches made on the occasion of this highly successful gathering.

AT the opening meeting of the new session of the Royal Geographical Society on Monday, the President, Sir Clements Markham, referred to the exploration of the Antarctic regions as the most important geographical work of our time. He remarked that the Antarctic agitation had spread over Europe, and was no longer confined to Great Britain. The Germans would certainly despatch an Antarctic expedition in 1900. Dr. Neumayer, the great authority on terrestrial magnetism, had been working with this end in view for years. A steam-vessel, specially designed and equipped for Antarctic service, would be built at Bremerhaven. The German Government would help with funds and in other ways, and would lend officers, even although they might have to go some distance from the end of a telegraph wire. Dr. Erich von Drygalski, the distinguished Greenland explorer, would lead the scientific staff. Dr. Neumayer looked to us for co-operation. They would understand now why they could not wait for Admiralty changes of policy. It ought to be a Government expedition under naval discipline. But if our navy was to be deprived of her right, the next best thing must be done; they must appeal to the country. The Council of the Society had resolved to head the list of subscriptions, even if its name should stand alone. There were numerous calls upon their funds, but they had resolved to strain them to the uttermost for that great national work. The Council would head the list with the sum of 5000*l*.

PROF. MICHAEL FOSTER and Prof. Rücker, Secretaries of the Royal Society, were present at the opening meeting of the Royal Geographical Society, and expressed the sympathy of the Royal Society in the movement for Antarctic exploration referred to by the President. Prof. Foster remarked that the Government granted the sum of 4000*l*. a year for the cultivation of all branches of science, and it had been determined to make an application to the Committee which administered that 4000*l*. for a sum which should indicate how that Committee, representing not only the Royal Society, but all the scientific societies of this country, held in scientific esteem the proposed expedition. It was not so much the sum of money that would be obtained as a clear indication that that Antarctic expedition was an expedition of undoubted and great scientific value. He did trust, speaking on behalf of the Society, that the appeal which had been made by the President of the Royal Geographical Society might be responded to liberally. Prof. Rücker said he did not think too much stress could be laid on the fact that any connection with Germany was in this case of the very greatest importance; that observations which were taken simultaneously in different parts of the Antarctic regions would be worth far more than two sets of observations taken several years apart. It was a matter of the greatest importance that the two expeditions should go out at the same time and co-operate with one another in the sense that their observations should be carried out with a common object. Sir Joseph Hooker, Sir Erasmus Ommanney, and Sir Leopold McClintock also spoke in favour of the proposed expedition. Admiral Sir W. Wharton thought such an expedition would be best carried out under the auspices of the Admiralty, because it would be an expedition of great difficulty and would need strict discipline. But the Government had for the time being declined to afford their aid, and he was very much of opinion that a small expedition, which, he hoped, might be sent out by private aid, would be able to act as the pioneer of a larger expedition afterwards.

THE International Congress of Mathematicians will meet in Paris on August 6–12, 1900. The Mathematical Society of France has appointed committees of organisation, M. Poincaré being president of that concerned with scientific papers, and M. Darboux of that concerned with the other arrangements.

THE fiftieth anniversary of the death of Berzelius was celebrated at Stockholm, on October 7, by a memorial service, at which the King was present. In commemoration of this event a small volume, containing twenty letters exchanged between Berzelius and C. F. Schönbein in the years 1836–1847, edited by Prof. G. W. A. Kahlbaum, has been published by Benno Schwabe, Basel.

DR. CALMETTE, director of the Pasteur Institute at Lille, has been made an Officer of the Legion of Honour.

THE death is announced, at Rome, of Count Michele Stefano de Rossi, distinguished for his seismological work.

LETTERS recently received from Prof. A. C. Haddon, who it will be remembered left England in March last with an exploring party, to continue his researches in the Torres Straits, report favourable progress of the work. The islanders are said to have been everywhere delighted with the phonograph; but misfortune early befell the party, in the miscarriage of a case containing their cinematograph films and Joly's colour-photography apparatus. We regret to add that Dr. Haddon has been the victim of two or three very mild attacks of fever, and that one of his companions has been more severely affected. The latest intelligence reports an improved state of health, and that they will be by now leaving for Bornea, with the exception of Dr. Rivers and Mr. Wilkin, who are expected to return to England before Christmas.

WE regret to see the announcement of the death, at the age of sixty-five, of the French electrical engineer, M. de Meritens, under distressing circumstances. The best-known invention made by M. de Meritens (remarks the *Engineer*) is his dynamo, and though one of the very first, if not the first dynamo of any practical value, it is still used at the present time, though only—to any extent, at least—for electric lighthouses. The dynamo is an alternating-current machine, and its field consists of permanent magnets. In efficiency it may fall short of machines of later type, but it has sterling merit, as is evidenced by the number of years it has been at work continuously, night after night, in the electrically-lit lighthouses of Great Britain, France, and other countries. The last in this country to be so fitted was that at St. Catherine's Point, in the Isle of Wight. This was in 1888. M. de Meritens' inventions did not by any means end here. In the early days of storage batteries he was to the front with an invention by which he sought to increase the surface of the plates immersed in the liquid by means of a plate of lead, so bent that pockets were formed in it, in which lead shot were placed. He also suggested many other forms of battery. M. de Meritens also invented a motor, and at one time turned his attention to electric welding, being the first to suggest using carbon for one pole and the metal to be welded for the other pole. He also attempted to improve on the original "candle" of Jablochkoff by utilising a third rod of carbon between the two outside rods, instead of the usual plaster of Paris or kaolin.

THE annual meeting of the Yorkshire Naturalists' Union has been postponed from Saturday, November 19, to December 17.

THE officers of the Botanical Society of America for the ensuing year are as follows:—President, Prof. L. M. Underwood; vice-president, Prof. B. L. Robinson; secretary, Prof. G. F. Atkinson.

DR. G. KLEBS has been appointed professor of botany and director of the Botanic Garden at Halle, and Dr. W. Schimper succeeds Klebs as professor of botany at the University of Basel. M. C. Sauvageau has been appointed professor of botany to the Faculty of Sciences at Dijon.

WE learn from the *American Naturalist* that the University of California has received from the Alaskan Commercial Company of San Francisco a large and valuable gift, consisting of the collections which the company has been accumulating for many years. The ethnological portion of the collection is especially rich, and is probably one of the best in existence. The collection also includes fossil remains of mammoth, and many skins and mounted specimens of mammals, birds, and invertebrates of the Alaskan region.

THE State of Hamburg has, according to the *Botanical Gazette*, just established, at Freihafen, a station for plant protection under the direction of Dr. C. Brick. The station will look after the introduction of injurious insects with the shipments of living plants from abroad, and its duties will include also the combating of plant diseases, the oversight of the schools of viticulture, and the inspection of vineyards and orchards in the Hamburg region, together with such questions as may arise in the prosecution of the work.

THE winter lecture season at the Imperial Institute opened on Monday with an illustrated lecture on "Trinidad, with some account of the recent hurricane in the West Indies," by Mr. Henry Caracciolo. The lectures are open to the public without payment, seats being reserved for Fellows of the Imperial Institute and their friends. The following are among the subjects of lectures before Christmas:—"The stalactite caves of New South Wales," by Mr. F. Lambert; "Gold-mining in Victoria," by Mr. E. Lidgey; "A national photographic record," by Sir J. Benjamin Stone, M.P.

A FEW particulars of Mr. Nikola Tesla's new method of electric power transmission are given in the current number of the *Electrical Review*. From the article it appears that the invention consists in transmitting electrical power without the employment of metallic line conductors, by taking advantage of the conductivity of the rarefied air existing in the upper regions of the earth's atmosphere. In order to make this practicable, special apparatus has been devised for the production and conversion of excessively high electrical pressure. Heretofore, it has been possible, by means of the apparatus at command, to produce only moderate electrical pressures, and even these, with considerable risks and difficulties. Mr. Tesla, however, claims that he has devised means whereby he is enabled to generate, with safety and ease, electrical pressures measured by hundreds of thousands, and even by millions of volts. He has also, during his investigations with such apparatus, discovered certain highly important and useful facts, which are said to render practicable his new system of transmitting electrical energy. Among these are the following: first, that with electrical pressures of the magnitude and character which he has been able to produce, the ordinary atmosphere becomes, in a measure, capable of serving as a true conductor for the transmission of the current; second, that the conductivity of the air increases so materially with the increase of electrical pressure and the degree of exhaustion, that it becomes possible to transmit, through even moderately rarefied strata of the atmosphere, electrical energy up to practically any amount and to any distance.

DR. C. LE NEVE FOSTER's general report (Part iii.) and statistics relating to the output and value of the minerals raised in the United Kingdom, the amount and value of the metals produced, and the exports and imports of minerals, in 1897, has been published as a Blue Book. The following interesting facts are recorded in it. The output of coal last year was 202,129,931 tons, the highest hitherto recorded. The quantity of coal imported was no less than 37 million tons, and is likewise the highest on record. The output of iron ore reached 13¾ million tons last year. Aluminium and sodium appear in the report for the first time. It is pointed out that the production of alumina from alum clay or bauxite, and the extraction of the metal in the electric furnace, form a new branch of industry which was only started in the United Kingdom by the British Aluminium Company a short time ago. The alumina is prepared at works near Larne, County Antrim, and then despatched to Foyers in Inverness, where abundant water-power enables electricity to be generated cheaply. The Foyers installation is so far the largest water-power plant in the United Kingdom. The quantity of alumina extracted at the Larne works last year was 850 tons, value 15,300*l.*, which produced 310 tons of aluminium, value 45,880*l.* With regard to sodium, the Aluminium Company, of Oldbury, near Birmingham, are practically the only makers of this metal in the United Kingdom. The quantity made last year was about 85 tons. At the present market price this output would be worth 12,750*l.* It is regretted that statistics concerning the quantities of magnesium and potassium are unobtainable.

THE total output of gold ore (auriferous quartz) in the United Kingdom in 1897 is given by Dr. Foster, in the report above referred to, as 4517 tons, the total value at the mines being 6282*l.* Turning to the ores of copper, lead, tin and zinc, Dr. Foster's tables do not present a satisfactory picture. Copper mining is a decaying industry. The output of lead ore is also declining; last year it was only 35,338 tons, being the smallest recorded during the last half-century. We only now produce one-half the amount of lead ore we did twenty-five years ago, and the same remark applies in the case of tin ore, while the values in both cases have decreased to one-fourth. The output of zinc ore, 19,278 tons, almost the same as it was in 1896, does not reach the average of the last quarter of a century. Of the so-called non-metallic minerals, clay, limestone, sandstone and slate are the most important, the value of the output in each case exceeding one million sterling. A new feature in the volume consists of county summaries, showing by means of tables the output of each county. This will prove of great service to those who are specially interested in the welfare of any particular county.

FROM Prof. Kr. Birkeland we have received a reprint of a paper by him in the *Archives des Sciences physiques et naturelles*, on the phenomena of attraction or "suction" of kathodic rays by a magnetic pole. Since the author's previous paper in 1896, the subject has been treated from a theoretical standpoint by Poincaré, whose views have been put to an experimental test in a recently published paper by Wiedemann and Wehnelt. In the meantime Prof. Birkeland has made a new series of experiments on the subject, and his present paper not only affords a completion of Wiedemann and Wehnelt's investigation, but furnishes a simple method for determining the relation existing between the velocity of the kathodic rays and the difference of potential (between anode and kathode) under which these rays are emitted.

MR. B. G. TEUBNER, of Leipzig, has commenced to bring out a new and important mathematical work in German, entitled the "Encyclopaedia of the Mathematical Sciences." The object of the Encyclopaedia is to give in a concise form but with the greatest possible completeness, and in a manner adapted for easy reference, all the newest results in both pure and applied mathematics. It is also proposed to show by carefully-prepared literary sketches, the historical development of mathematical methods since the beginning of the century. The Encyclopaedia will consist of six volumes, of about 3840 pages, of which the first will treat of Arithmetic and Algebra; the second, of Analysis; the third, of Geometry; the fourth and fifth, of Applied Mathematics; and the sixth, volume of historical and allied matter. In the detailed table of contents the absence of any reference (specific or indirect) to "Trigonometry" or "Circular Functions" is somewhat remarkable, seeing that even such a subsidiary subject as "Inversion" has a whole section devoted to it under Geometry. The editors are Dr. H. Burkhardt (Zürich) and Dr. W. Franz Meyer (Königsberg), and the work is being published under the auspices of the Imperial and Royal Academies of Munich and Vienna and the Royal Society of Göttingen. From the prospectus we learn that "lengthy demonstrations will in general be omitted."

THE French Meteorological Office has recently issued its *Annales* for 1896, comprising three large quarto volumes. The first volume, under the title of "Memoirs," contains a discussion by M. Fron of thunderstorms observed during the year, with charts showing their distribution for each day on which a storm occurred. M. Moureau publishes the details of the magnetic observations at Parc St. Maur, with a summary of the characteristics of the principal disturbances. These and the simultaneous variations of earth currents are also shown by means of curves. There is an important work by M. Brillouin on contiguous winds and clouds, in which the author studies the very complicated phenomena that may be produced in a region where two atmospheric strata mix together, and in which the amount of cloud is unequal. M. Angot discusses the problem of the barometric measurement of heights, and has prepared new tables to facilitate the calculations. The attention of scientific men has been drawn to this question by the observations made during recent international balloon ascents. A contribution to the rainfall of the region of the Upper Nile is made by M. de Martonne. Monthly and yearly values are given for some forty stations, collected from various sources. There are also other papers of a minor character. Vol. ii. contains the results of observations made at various stations. The colonial stations have been considerably extended, especially in Africa, and some observations are given for Timbuctoo, in the centre of that continent. Vol. iii. is devoted entirely to the discussion of rainfall observations; the number of stations included in the work exceeds two thousand.

IN connection with the journal *Photography*, silver and bronze medals are offered for the best sets of lantern slides relating to the subjects named in the following sections:—(1) The rivers of Great Britain; (2) the commercial ports of Great Britain; (3) the old churches of the United Kingdom; (4) British trees, plants, and flowers; (5) scenes among fishermen; (6) striking natural scenes in Great Britain (rocks, caves, trees, waterfalls, &c.). The competing slides should be in the hands of the editors by December 10. The object of the competition is to renew sets of similar slides which have for several years been circulated by *Photography*, without charge or fee, and have been largely used in schools for purposes of instruction.

TWO papers on the food of certain cuckoos and shrikes, based on investigations by Prof. F. E. L. Beal and Dr. S. D. Judd, are contained in *Bulletin* No. 9 of the Division of Biological Survey (U.S. Department of Agriculture). In the laboratory of the Biological Survey 109 stomachs of the yellow-billed cuckoo (*Coccyzus americanus*), and forty-six of the black-billed cuckoo (*C. erythrophthalmus*), taken between May and October, were examined. The results show that of the 155 stomachs of both species only one contained any vegetable food. It appears that the insect food of these birds consists of beetles, grasshoppers, cicadas, bugs, ants, wasps, flies, caterpillars, and spiders, of which grasshoppers and caterpillars constitute more than three-fourths. The great majority of the insects found in the stomachs were harmful kinds. It is a matter of common observation that cuckoos feed largely on caterpillars, and stomach investigations not only confirm this, but show that, unlike most other birds, they eat freely of hairy and bristly species. Nearly half of the food of the birds examined was found to be caterpillars. Considering the number of grasshoppers, locusts, and other insects that cuckoos eat, in addition to caterpillars, it is evident that from an economic point of view these birds are two most valuable species, and as they have not been convicted of doing any harm, they should be protected and encouraged in every possible way. Besides insects proper, Prof. Beal found a number of spiders in the stomachs he examined, most of them the long-legged kinds commonly known as "daddy longlegs" (*Phalangidae*). One stomach contained seven, the mass of tangled legs looking like a bunch of coarse hair. When we consider the disagreeable odour of these spiders, their long legs, and the fact that their bodies have the texture of sandpaper, we are again forcibly reminded that tastes differ. But the most remarkable thing which the birds examined had eaten was a small tree frog (*Hyla*). which had been swallowed whole!

DR. S. D. JUDD investigated the food of two species of shrikes, and the results are given in the *Bulletin* referred to in the foregoing note. One species, the loggerhead shrike (*Lanius ludovicianus et sub-spec.*) is a permanent resident of the United States; the other, the butcherbird (*Lanius borealis*), is a migrant from the north. From the investigation it appears that the food of the butcherbird and loggerhead, as shown by 155 stomachs collected during every month in the year, and in an area extending from California to the Atlantic coast, and from Saskatchewan to Florida, consists of invertebrates (mainly grasshoppers), birds, and mice. During the colder half of the year the butcherbird eats birds and mice to the extent of 60 per cent., and ekes out the rest of its food with insects. In the loggerhead's food, birds and mice amount to only 24 per cent. A table showing percentages of principal elements of food of the butcherbird and loggerhead indicates that the loggerhead's beneficial qualities outweigh 4 to 1 its injurious ones. Instead of being persecuted the bird should, therefore, receive protection.

Cornwall is to be congratulated upon the success of the efforts its Technical Instruction Committee are making for the benefit of fishermen, through Mr. J. T. Cunningham, the County lecturer on fishery subjects. The report of the executive committee for fisheries for the year 1897-98, is almost entirely devoted to a valuable statement by Mr. Cunningham on records of sea fishing and sea temperature, experiments on oyster culture and lobster rearing, fish and net curing, and other matters concerning the science and handicraft of sea fisheries. Among the interesting items referred to in his report, is the fact that at the beginning of every season male crabs are always more numerous in the pots than females. Their number increases gradually till May or June, when it gradually diminishes; while the number of females taken does not reach its maximum till June or July, and then they are twice as numerous as the males, and they usually continue to be more abundant than the males until the end of the season. On the other hand, practically the same number of male lobsters are caught as female. The observations of surface temperature, made in connection with these fisheries, are of great service in indicating the influence of temperature upon the number of fish taken in various months. With regard to lobster rearing, Mr. Cunningham reports that important and gratifying success has been obtained, and much precise knowledge has been gained concerning the particular details of treatment on which the life and death of the larvae depend. His observations have convinced him that the opinion that swimming lobster larvae in the sea capture and feed upon other swimming creatures, especially other small swimming crustaceans, is wrong. He holds that lobster larvae are, like the adults, carrion feeders, and are not in the habit of pursuing or capturing live food at all.

Messrs. Duckworth announce for publication "A Glossary of Botanical Terms," by Mr. B. Daydon Jackson; and a "Text-book of Agricultural Botany," by Mr. J. Percival.

Messrs. J. M. Dent and Co. announce that after the December number *Natural Science* will be published by Mr. Young J. Pentland, of Edinburgh, who has acquired all future rights.

Messrs. W. Wesley and Son have just issued a new Catalogue (No. 132 of their Natural History and Scientific Book Circular), giving a descriptive and classified list of 1500 books and pamphlets on the natural history of Great Britain and Ireland. The arrangement of works under the names of the English counties, Wales, Scotland and Ireland, will be of service to collectors of local fauna and flora.

The following new editions of works already reviewed in Nature have been received:—The third edition of Prof. Grenville Cole's "Aids in Practical Geology" (London: Charles Griffin and Co.) The work has been completely revised and enlarged; many additions of practical service to the geologist have been made, and all important factors of geological progress since 1893, when the second edition appeared, have been taken into consideration.—Messrs. Slingo and Brookes's well-known volume on "Electrical Engineering for Electric Light Artisans and Students" has been published in a revised and enlarged edition by Messrs. Longmans, Green, and Co. The work now occupies 780 pages, and it provides students with sound information concerning direct and simple alternating currents, the machinery and apparatus connected therewith, and their most important applications.—The tenth edition of Mr. C. Haughton Gill's "Chemistry for Schools" (Edward Stanford) has been published. Dr. D. Hamilton Jackson is responsible for the revision of the book and the additions made to bring it into line with the new regulations in chemistry for the London University Matriculation Examination.—The second edition, revised and enlarged, of Dr. Lassar-Cohn's "Chemistry in Daily Life," translated by Mr. M. M. Pattison Muir, has been published by Messrs. H. Grevel and Co.

The additions to the Zoological Society's Gardens during the past week include two Pumas (*Felis concolor*, ♂ ♀) from the Argentine Republic, presented by Mr. Ernest Gibson; a Hamster (*Cricetus frumentarius*), European, two Bennett's Wallabies (*Macropus bennetti*) from Tasmania, a Gentoo Penguin (*Pygosceles taeniatus*) from the Falkland Islands, a Gold Pheasant (*Thaumalea picta*, ♀) from China, two Elephantine Tortoises (*Testudo elephantina*) from the Aldabra Islands, deposited; two Japanese Deer (*Cervus sika*, ♂ ♂) from Japan, received in exchange; an Axis Deer (*Cervus axis*, ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

The Leonids.—Unfortunately for observers in the neighbourhood of London, a fog more or less continuously hung over their heads on the nights of Sunday, Monday and Tuesday, and rendered observations of the expected meteor shower impossible. Observers at the Solar Physics Observatory, South Kensington, were only able to observe two or three meteors during these nights. One of special brilliance, on the night of Monday, at 10.48 G.M.T., shot across the sky from east to west, its paths extending at least 45 degrees. This was undoubtedly a fine Leonid; but its path could not be traced, as fog and cloud hid the stars from view.

Mr. R. H. Scott informs us that the meteorological reporter at Jersey (Mr. Fisher, at St. Aubin's) has reported to the Meteorological Office that a meteor shower was observed there at 10 p.m. on Monday.

From a New York telegram, published in one of the daily papers, we gather that the shower was observed in America on the night of the 14th, but it did not attain the expected brilliancy. Prof. Young is said to describe the shower observed as faint, while at the Lick Observatory the meteors were said to be small and not marked by extraordinary brilliancy.

A Globe for Meteor Observers.—Every observer, who has plotted the trails of meteors on star charts, knows that errors of projection arise which are due to the difficulty of plotting accurately the whole length of the trail. If the points of beginning and end of the visible trail be mentally noted and marked off on a star chart, then the line joining these two points does not represent the actual trail of the meteor, in consequence of the fact that the path in the heavens is on a sphere, and that on the chart is on a plane. To avoid such discrepancies it is always better to use a celestial globe, when this difficulty is eliminated. The ordinary celestial globe is, however, not well adapted for recording meteor observations, as it must be illuminated from the outside by a lamp, as the observer is in the open air.

A globe arranged especially for use when such observations are being made, recently invented by M. Pietro Maffi, is described in the French journal *Cosmos*, and should be found very serviceable. It consists of a glass hollow sphere mounted on two vertical supports, and capable of adjustment as regards latitude. The outer surface is marked with the brighter stars of each constellation and the Milky Way, and there is also means of obtaining directly the right ascension and declination of any point on the globe. The inside contains two small electric lamps in connection with accumulators in the stand, so that the whole surface of the sphere with the constellations may be seen clearly. Directly a meteor is seen its path can be plotted, and when note is taken of its exact position a wet sponge is all that is necessary to erase it.

The globe and stand, as it appears in the illustration accompanying the article, seems rather elaborate; but there is apparently no reason why a more simple and cheaper form should not be made, for its use then would be more widely distributed.

EPHEMERIDES OF COMETS AND PLANETS.—Many readers of this column will be very glad to know that from the beginning of next year it will not be necessary to turn up back numbers of the *Astr. Nach.* to find the ephemerides of comets and planets for observational purposes. Prof. H. Kreutz tells us (*Astr. Nach.*, No. 3527) that he has been asked from several sides to supply this information separately; that is, in addition to that published in the numbers of that journal, and he has made the following arrangements. Those who are subscribers to that journal may, by paying an additional sum of ten marks yearly, obtain such information, provided that notice of such requirement is given directly, and applications are sufficient in number to indicate a decided want in this respect. We hope that many of our readers will think fit to take advantage of this very useful change.

We may mention that it would be a good opportunity for keeping to one system of publication, and that the Right Ascensions, for instance, might always be given in time, and not in degrees, as is often the case.

THE NEBULOUS REGION ROUND 37 CYGNI.—Although there may be countless nebulae in the heavens symmetrical in form, there are others which seem to have absolutely no sense of regularity. Such a nebulous region is that comprised between 20h. 51m. 24s. and 21h. 0m. 43s. in Right Ascension, and in Declination between +42° 56'·5 and +44° 51' (epoch 1900). An excellent reproduction of the greater part of this region appears in *Knowledge* for November, and is taken from a fine negative obtained by Dr. Isaac Roberts in October 1896. A careful study of the photographic appearance of this large cosmical mass shows, as Dr. Roberts remarks, several indications of fission, as well as evidence of *loci* of vortical disturbance in different regions; but, for the main part, irregularity is the striking feature. In his description of this nebulous region, Dr. Roberts brings up the question of the connection between the stars apparently immersed in the nebulous mass and the nebula itself. In regular, such as spiral, nebulae, Dr. Roberts had previously come to the conclusion that many of the stars immersed are not stars, as we are generally acquainted with, but star-like condensations, the result of condensations in the nebula itself. In the irregular nebula in question, in which the whole surface area of this cloud of nebulosity is covered with stars, ranging from the ninth to the seventeenth magnitude, very few of the stars can be pronounced "as being actually involved in, and forming part of, the nebulosity." Dr. Roberts' evidence for assuming that those what he terms "apparently finished stars" are between us and the nebulosity is, in his own words, as follows:—"If the stars were beyond the nebulosity their photo-discs would on the negative appear less bright, and their margins be more or less nebulous; whereas only those stars which appear involved in the nebulosity present these appearances. Of course, it is a fair subject for argument that those nebulous stars which appear to be involved in the nebulosity are not so in reality, but seem thus because they are beyond it in our line of sight. But this argument is much weakened, if not entirely destroyed, when we find on examination of the negative that those faint, star-like condensations are not only nebulous themselves, but they follow the curvatures found in various parts of the nebulosity; thus we are driven to infer that the stars are the nearer bodies to us, and that the nebulosity lies beyond the stars."

WOLSINGHAM OBSERVATORY CIRCULAR, NO. 48.—Mr. T. E. Espin in this circular informs us that a star, magnitude 8·4, Type IV., not in B.D. was found on November 13 in Right Ascension 4h. 19m. 49s., Declination + 39° 32' (55). The star in the Andromeda Nebula was seen on November 10, closely following the nucleus. The one found in 1886 was preceding.

THE EXTRACTION OF NICKEL FROM ITS ORES BY THE MOND PROCESS.[1]

THE Mond process marked an entirely new departure in metallurgical practice and in the principles which had hitherto guided it. It depended on the remarkable property possessed by nickel of forming a volatile compound with carbon-monoxide, from which metallic nickel might be released if the gaseous compound was heated to 180° C.

The methods hitherto employed for extracting the metal from its ores involved concentrating the nickel either as a sulphide (matte or regulus), or as arsenide (speise), followed by either dry or wet treatment; and the metal had to be refined, mainly with a view to separate it from associated carbon.

In 1889 Dr. Ludwig Mond, in collaboration with Dr. Carl Langer, had been engaged upon a method for eliminating the carbon-monoxide from gases containing hydrogen. They had been guided by the observation that finely-divided nickel removed carbon from carbon-monoxide at a temperature of 350° C., converting it into carbon-dioxide, whereas the dissociation of carbon-monoxide by heat alone, according to Victor Meyer and Carl Langer, remained incomplete at the high temperature of 1690° C. The experiments were carried out in conjunction with Dr. Friedrich Quincke; finely-divided nickel, formed by reducing nickel oxide at 350° C. by hydrogen, being treated with pure carbon-monoxide in a glass tube at varying temperatures. The gas escaping from the apparatus was ignited, and while the tube containing the nickel was cooling, the flame became luminous, and increased in luminosity as the temperature sank below 100° C. Metallic spots were deposited on a cold plate of porcelain held in this luminous flame, and on heating the tube through which the gas was escaping a metallic mirror was obtained, while the luminosity of the flame disappeared. These metallic deposits were found to be pure nickel. Nickel carbonyl was then isolated in a liquid state, and it was possible to produce it with facility in any desired quantity. It could be readily distilled without decomposition, but on being heated to 150° C., the vapour was completely dissociated, pure carbon-monoxide being obtained and the nickel being deposited in a dense metallic film upon the sides of the vessel.

No other metals which were submitted to investigation showed indications of combining directly with carbon-monoxide except iron. The discovery that in a mixture of metals only nickel and iron would form volatile compounds with carbon-monoxide, and that they could, therefore, be separated from the other metals, induced Dr. Mond to arrange experiments with ores containing nickel, cobalt, iron and copper, such as "kupfer-nickel" and "pyrrhotine." The experiments afforded such promising results that apparatus of considerable size, though still within the limits of the resources of a laboratory, was set up, and in it several pounds of ore could be treated with carbon-monoxide. The principal nickel ores which were metallurgically treated contained the nickel in combination with arsenic and sulphur, besides other metals and gangue. These ores had first to be submitted to calcination, in order that the nickel might be present in the form of oxide, and to drive off, as far as practicable, the arsenic, sulphur, and other volatile bodies. The resulting oxide of nickel was treated with reducing gases, such as water-gas or producer-gas, in order to convert the oxide of nickel into finely divided metallic nickel, and the material containing it was cooled to about 50° C., and was treated with carbon-monoxide.

In 1892 an experimental plant on a large scale had been erected at Smethwick, near Birminghan. The process began with "Bessemerised" matte; it ended with the market product, commercial nickel. The Bessemerised matte proceeded to the first operation of dead roasting, after which the matte contained 35 per cent. of nickel, 42 per cent. of copper, and about 2 per cent. of iron. It then passed to the second operation for the extraction of part of the copper (about two-fifths) by sulphuric acid, the copper being sold as crystallised sulphate of copper. The residue from this process contained about 51 per cent. of nickel, and it passed to the third operation for reducing the nickel. Incidentally, the remaining copper was reduced to the metallic state, care being taken to avoid reducing the iron. This was effected in a tower provided with shelves, over which mechanical rabbles passed, the reducing agent being the hydrogen contained in water-gas. The temperature did not exceed 300° C., and should be kept lower when much iron was present. From this tower the ore was conveyed continuously to the fourth operation of volatilisation, in which part of the nickel was taken off by carbon-monoxide and formed the compound nickel carbonyl. The formation of this volatile compound was effected in a tower similar to the reducing tower, but the temperature was much lower, and did not exceed 100° C. From the volatiliser, the ore was returned

[1] Abstract of paper read at the Institution of Civil Engineers, on November 8, by Prof. W. C. Roberts-Austen, C.B., F.R.S.

to the reducer, and it continued to circulate between the reducing and the volatilising stages for a period which varied between seven days and fifteen days, until about 60 per cent. of the amount of nickel had been removed as nickel carbonyl. The residue from this operation, amounting to about one-third of the original calcined matte, and not differing much from it in composition, was returned to the first operation and naturally followed the same course as before. The nickel carbonyl produced in the fourth operation passed to a decomposer, which consisted either of a tower or a horizontal retort heated to a temperature of 180° C., so as to decompose the nickel carbonyl and release the nickel in the metallic form, either on thin sheets of iron or, preferably, on granules of ordinary commercial nickel. Carbon-monoxide was in turn also released, and was returned to the volatiliser for taking up a fresh charge of nickel. When the operation was in progress, the gaseous carbon-monoxide and the partially reduced oxide of nickel and copper continuously revolved in two separate circuits, which joined and crossed each other in the volatiliser. The commercial product contained 99·8 per cent. of nickel.

The author proceeded to a description of the working as he saw it in full operation in Smethwick a few months ago. The plant had been working for some time, and about 80 tons of nickel had already been extracted from different kinds of matte. The results were quite satisfactory, and pointed to the conclusion that the process was well able to compete with any other process in use for the production of metallic nickel.

The process would always occupy a prominent position in chemical history, and there appeared to be no reason why it should not play an important part in metallurgical practice. Its application in Canada to the great nickeliferous district of Sudbury would probably contribute to the development of the resources of the great Dominion.

NATURAL HISTORY NOTES FROM YUNNAN.[1]

I LEFT Mengtze in the end of January with a caravan of mules, some forty, carrying stores, &c. I had three mule-loads, *e.g.*, of silver. The journey here took eighteen days, rather easy stages. The country passed through was very varied. I was in good spirits, rode nearly all the way, and enjoyed the trip very much. I crossed three large rivers *en route* by pontoon and suspension bridges; the latter very well made, of iron rods joined by rings at the ends, the best specimens I have seen of Chinese blacksmith's work. At these river crossings we reached low levels, about 1800 feet above the sea, and came into tropical vegetation, which I never find at all interesting. At Yuenchiang, on the Red River, the ugly-looking shrub *Calotropis gigantea* was in flower, and there was a great display of the tree-cotton, *Bombax*, in flower, without any leaves, looking like an artificial candelabrum affair more than a living tree. These and some *Areca* palms were the only things of note. At the higher levels vegetation was at a dead point and I collected very little, one or two species of *Clematis*, two Rhododendrons: the very curious *Scolopendrium Delavayi*, which I had never seen before, I found one day on a shady bank where I stopped for tiffin. I also found, at the same place, two plants of *Abutilon sinense*, which had been sent by me from Ichang, and an *Antrophyum*, which may be new. I also came across *Lonicera Bournei* in flower; it is of no value as an ornamental plant. There was very little forest until after Talang, when we passed one or two days through almost continuous pine forest, varied here and there by little woods of evergreen oaks. Here, rather to my surprise, I learned that the peacock exists in the wild state, and it is quite common in the forest south of Szemao. These pine forests had not a plant in flower amongst them. I noticed, however, two little woods made up of an *Abies*, new to me, but I only found one cone. However, I am not pretending now to give any account of the trip botanically, as it would require too much time to get my notes in order at the moment. On the eighteen days I may have collected about thirty plants in flower. At one or two places I might have done a lot of collecting if I could have stayed for a day or two, but I was travelling on official business, and could not tarry.

The main interest of the route was the aborigines, or non-Chinese races. Chinese here and there dwell on the little tracts of good land which are found in the high-lying valleys and plains of the plateau, and I passed through five or six largish towns mainly peopled by Chinese. But the larger part of the population was made up of aborigines. Whether the ethnology of this part of the world will ever be satisfactorily explained is doubtful. There seems to be the same variety in the human being as exists in the vegetable world in the same region, and there is a strange blending of races of Chinese, Malay, Negrito, perhaps even Caucasian here.

The greatly increased interest in China at home will, I hope, give a stimulus to the study of the history of the social evolution of the Chinese, which is calculated to bring out many important lessons for ourselves. There have been, as it were, two parallel developments of the human race, one on the west of Europe-Asia, the other on the east side, very little dependent on each other. At the start, the Chinese seem to have been fairly equal to the Westerners; and even in the middle ages, judging from the way in which mediaeval travellers wrote, Chinese civilisation was quite as good as that of Europe. The decay of manly spirit, brought about by the idea that war is immoral, the low position of woman, the absence of an hereditary aristocracy holding up ideas of honour and probity and constantly acting as a check on philistinism, the government by officials selected by competitive examination in ancient classics and trivialities akin to Latin verse, all these causes must have been acting disastrously to have brought an intelligent race into such a low position.

There is a good deal of wooded country at no great distance from Szemao, and the mountains run up to nearly 6000 feet, but there is an absence of the sharp and precipitous kind of mountain and valley, and the flora in consequence is very uniform and not nearly so interesting as Mengtze nor so rich in species. Hills clad with pine and oak are almost barren in interesting plants, and I haven't come on any of those dark ravines and steep wooded cliffs which are the joy of the botanical collector. There is a great absence (perhaps the autumn will make a better show) so far of ferns and herbaceous plants. What one collects is mainly trees and shrubs and climbers. There is a fair number of epiphytic orchids. The common plants are not the common plants of Mengtze, in fact the two floras are very different. Szemao will possibly turn out very like the Shan country where Sir Henry Collett collected, and Indian forms not hitherto recorded from China are frequent enough. One curious thing occurs here as well as at Mengtze, *i.e.* the occurrence of two or three species of the same genus in precisely the same locality and often flowering at the same time.

The woods near Szemao are full of birds, and the notes are exquisite, and to be heard in perfection in these days of showery weather, for the rainy season has begun. When the sun gets out the cicadas start such a racket that one can hear nothing else. I have not told you of the jungle-fowl; this is, I believe, *Gallus bankiva*, the original form of the farmyard fowl. They are very common in the forests and woods here, and are simply gorgeous. They are glorified bantams, the colours having a brilliancy that seems abated in the domesticated kind. They crow and cackle and behave in the woods just as a farmyard fowl would do, only they are a little shyer of man. Occasionally one sees a flying-squirrel, a big black one, sailing in the air from tree to tree, and I saw the other day what I thought was a calf; it turned out to be a red-coloured deer, which speedily bolted with an upturned tail, white beneath like a rabbit's. It is very hard to believe that this particular deer, which only occurs, so far as I know, one or two together, never a herd, derives much advantage in life from this white-signal tail.

In many of the Mengtze and Szemao trees and shrubs the flowers occur on the branches below the leaves, and not on the peripheral surface of the tree, as in ordinary cases. Many lianas have this peculiarity. These are all forest plants, and I think the explanation is that in forests there are two surfaces open to insect-visitors, the top of the forest and the bottom. Some trees and shrubs and climbers cannot get to the top, so they have their flowers at the bottom. But of course this explanation is only a guess. There is no time for me to make any observations of the kind necessary; if one could spend six months on end in a forest, one could observe, measure, &c. The *Mucuna sempervirens* of Ichang was a splendid example of this peculiarity. There was in one specimen a dense wall of foliage climbing over trees, interlaced with them, &c., nearly 200 feet by 100 feet, while the main trunk of the climber close to the ground was covered with

[1] Abridged from a letter to Mr. Thiselton-Dyer, from Dr. Augustus Henry, published in the Kew *Bulletin* for November (No. 143).

flowers which were easily visited by thousands of insects of all kinds.

There is quite a little group of shrubs which occur on the banks of rivers (and often in beds of streamlets) that overflow. These shrubs are submerged often and are not hurt. These *fluviatile* shrubs have a certain facies, very difficult to describe. There must be thirty or forty species of them in the Yunnan river valleys. These shrubs do not occur elsewhere than on banks or in the beds of streams. The last one I have found is a very fine species of *Ficus*. This class of shrub would be a nice enough problem for some one to work out.

I hope you will try and get a young Cambridge or Oxford botanist to come to this part of the world, do some naturalist work, and collect seeds and live plants for cultivation. China is a very easy country to travel in, and expenses of travelling are not heavy. The mountainous regions of Yunnan and Szechwan are very healthy besides.

TECHNICAL INSTITUTIONS IN ENGLAND.

THE current number of the *Record* of Technical and Secondary Education contains some encouraging statistics with regard to the establishment, extension, and adaptation of permanent technical schools and institutions in England since the passing of the Technical Instruction Act, 1889. The primary object of the inquiry (the results of which are now published) was to ascertain the amount of money sunk in buildings for technical education by local authorities, but some valuable information regarding certain details of organisation was obtained at the same time, and has been incorporated in the article. The statistics do not take into consideration either higher grade schools or schools governed by schemes under the Endowed Schools Acts and other secondary schools; but they refer to all the various types of permanent technical institutions, and, consequently, there are included domestic economy schools, agricultural and horticultural schools and colleges, farm schools, and dairy schools. It was intended to take in university colleges so far as they included technical or agricultural departments, but it is stated that as these institutions are largely non-local in character, supplying certain educational requirements in large areas by reason of the financial assistance of local authorities, they have been excluded. The following summary, given by the *Record*, shows the developments which have taken place, but, as has been mentioned, it only partly represents the progress made since 1889, as it does not take into consideration secondary schools serving the purpose of technical institutes.

The figures which appear under the head of "counties" refer to those institutions which have been directly established, and are being exclusively maintained, by County Councils.

(1) NUMBER OF MUNICIPAL SCHOOLS EXISTING OR IN COURSE OF ESTABLISHMENT :—

	Schools.
(*a*) In 47 county boroughs	71
(*b*) In 130 non-county boroughs and urban districts	132
(*c*) In 30 counties	36
Total number of municipal schools in England	239

This number does not include—(1) those voluntary technical schools which will probably become municipal institutions in the near future, viz. the schools in three county boroughs and three non-county boroughs and urban districts; (2) the municipal classes, held in rented premises, in 29 non-county boroughs and urban districts.

(2) INCREASE IN ANNUAL LOCAL CONTRIBUTIONS FROM THE RATES :—

	Amount voted in 1895-6 £	Amount voted in 1897-8 £	Increase in Rate-aid. £
(*a*) In 15 county boroughs	7,488	17,429	9,941
(*b*) In 70 non-county boroughs and urban districts ...	6,791	15,960	9,169
In 85 localities	£14,279	£33,389	£19,110

These figures are exclusive of—(1) those relating to Sheffield, where an additional sum of 1000*l.* will be raised by rate during the current year; (2) a total sum of 29,777*l.*, which has been voted from the general rate funds of urban authorities, and applied entirely to *capital* purposes.

(3) TECHNICAL SCHOOLS ESTABLISHED, EXTENDED, ADAPTED, &c., SINCE 1889 :—

	County Boroughs.	Non-County Boroughs, &c.	Counties.	Totals.
Number of schools established...	42	89	32	163
" " in course of establishment	12	56	4	72
" " extended or adapted ...	14	46	—	60
" " transferred, purchased, or presented...	29	43	—	72

(4) PROVISION OF SITES FOR TECHNICAL SCHOOLS :—

	County Boroughs.	Non-County Boroughs, &c.	Totals.
(*a*) Sites *purchased* by the local authority ...	20	44	64
(*b*) Sites *given* by the local authority	8	17	25
(*c*) Sites *presented* by private individuals or Trustees ...	7	28	35

(*d*) Sums *involved* in the acquisition of sites	in 23 county boroughs ...	£122,528
	in 66 non-county boroughs and urban districts ...	55,214
	Total for 89 localities	£177,742

(5) CAPITAL EXPENDITURE UPON TECHNICAL SCHOOLS SINCE 1889 :—

	Total sum involved. £	No. of localities.	No. of buildings
(*a*) County boroughs ...	1,351,193	50	69
(*b*) Non-county boroughs and urban districts	826,376	166	172
(*c*) Counties	112,853	22	26
Totals	2,290,422	238	267

To this sum an amount of 50,229*l.* should be added as representing the value of sites for technical schools presented by local authorities, Trustees, and private individuals in 18 localities, and included under section (4) of this summary; an aggregate amount of 2,340,651*l.* is thus shown as having been absorbed by the establishment of technical institutions in England from the year 1889. It should, however, be pointed out that the figures under (*c*) do not include the sums spent by the County Councils of Cambridgeshire, Lancashire, Warwickshire, and the East, North, and West Ridings of Yorkshire upon their municipal institutions.

Of the funds dealt with above, it is only possible to give the derivation of a sum of 1,854,036*l.*, which is apportioned among the different authorities as follows :—

	County Boroughs. £	Non-County Boroughs, &c. £	Counties. £	Totals. £
Loans	657,183	259,047	27,350	943,580
Donations and subscriptions, and Trust funds ...	309,716	178,436	13,567	501,719
Residue grant ...	167,642	92,366	71,936	331,944
Science and Art Department's grants	11,101	35,915	—	47,016
Lump sums from rate fund	18,036	11,741	—	29,777
	£1,163,678	577,505	112,853	1,854,036

In order the more easily to grasp the significance of these figures and to facilitate comparison, it may be well to present the following table of percentages :—

	County Boroughs. Per cent.	Non-County Boroughs, &c. Per cent.	Counties. Per cent.
Loans	56½	45	24¼
Donations and subscriptions and Trust funds ...	26¾	31	12
Residue grant	14¼	16	63¾
Science and Art Department's grants and lump sums from rate fund ...	2½	8	—
Per cent.	100	100	100

From this table it will be seen that, while the largest proportion of capital expenditure in the county and non-county boroughs, &c., is met by loans, the main source of supply of the County Councils is still the Residue grant. The extent of the assistance rendered by voluntary effort is also clearly indicated, as well as that given by the Science and Art Department; the smaller urban districts appear to have received the largest contributions from these two sources.

THE BRITISH ASSOCIATION.

BRISTOL MEETING.

SECTION K (BOTANY).

OPENING ADDRESS BY PROF. F. O. BOWER, SC.D., F.R.S., PRESIDENT OF THE SECTION.

SHORTLY before we met last year in the hospitable Dominion of Canada, two biologists, whose work relates to the questions I propose to discuss to-day, passed away. In both cases their services to science had received honourable recognition in this country. Johannes Japetus Smith Steenstrup, who had been for more than thirty years a foreign member of the Royal Society, died June 20, 1897, at the advanced age of eighty-four; Julius von Sachs, also a foreign member of the Royal Society, died May 29, 1897, aged sixty-five.

The former of these, a zoologist, was probably best known in this country for his work on "Alternation of Generations," a translation of which was published by the Ray Society in 1845. The title-page describes the phenomenon as "a peculiar form of fostering the young in the lower classes of animals." Botanists should remember that this term "alternation," which they often use in a sense peculiarly their own, was originally applied to the course of development in certain animals, by Chamisso in 1819. The first general statement of the subject from the zoological side was by Steenstrup in the work already named; even there no mention is made of such phenomena in plants, until the concluding paragraph, where there is an allusion in very general terms to the course of events in the life of seed-bearing plants. But when we remember that it was only in 1848 that Suminski discovered the antheridia and archegonia borne upon the prothallus of a Fern, we see plainly that Steenstrup could not have used the term "alternation" in the sense in which it is now generally applied to plants. The interest for us as botanists will therefore be that Steenstrup suggested in his work on alternation in animals how in the life of plants successive phases exist, and that these are comparable to those which he described in many animals.

The work of Sachs, on the other hand, has influenced every one of us. Some, including myself, have had the great advantage of his direct personal guidance; all must have derived pleasure as well as profit from his writings. I shall not here attempt any general summary of the achievements of this great man, for that has been done efficiently by the scientific press at large. I shall merely allude to one feature of his work, viz. the style of its presentment to the reader. He was always clear, usually concise. He was, in addition to his power as an investigator, a master with the pencil, as well as with the pen. It was this combination of qualities which made him the great text-book writer of his time. Never perhaps has a volume more fairly reflected the position of a science at the moment of its publication than did that of Sachs. It resembles the work of a snap-shot camera, and, like any instantaneous photograph of life in motion, it has fixed and perpetuated awkward positions. The morphological system of the time was stiff and unpromising; the text-book accurately depicted this, but it did not suggest or anticipate future developments; it did not bear the softened image of a longer exposure; it presents to us the angular attitude of a moment.

The powers of Sachs as a writer found their best scope in his "History of Botany," a work which will always retain its value as a masterly exposition of the results of very wide reading, arranged with a literary skill which is unfortunately rare among scientific men. I lay stress upon this power of Sachs as a writer, apart from his record as an investigator, because he was strong where so many of us are weak. The truth is that little effort is made by men of science to use a concise and transparent style; for the most part we write by the aid of such instincts as nature has given us; few cultivate composition. But it should, I think, be impressed upon the young aspirant that, when he writes, it is one of his first duties to consider his readers' convenience; he must use all endeavours to convey forcibly the result of his inquiry, but to make the least possible demand upon the patience of his readers. I should like to see certain papers selected as models of construction, to be studied as such by all candidates for our higher degrees; we should naturally include in the list those of the best masters of style in foreign languages, and among them would rank the late Julius von Sachs.

THREE PHASES OF MORPHOLOGICAL STUDY.

It will be in your memory that the Address of last year's Sectional President was largely devoted to branches of our science which touch the material and economic interests of man. It was pointed out to us how certain fungal diseases diminish agricultural profits to an extent which may be estimated in millions of pounds yearly. Beneficent microbes were also mentioned, such as those which govern the aroma and maturing of butter and cheese; these and many others, the study of which lies properly within the province of botany, affect not only the health, but, at the most varied points, the comfort and prosperity of mankind.

It is unnecessary for me to dwell further upon these matters, or to urge again the utilitarian argument for the proper support of botany. I propose, on the other hand, to invite your attention this morning to the Morphology of Plants. This is a department of science pure and simple. The results which it brings have not, and cannot be expected to have, any money value in the markets of the world. The present time is one of unusual bustle and change in morphology, consequent upon the discovery of new facts and the introduction of new methods. The development of the study may be divided into three periods, we ourselves standing upon the threshold of the third. The earliest phase was that of description and delineation of what might be observed of the mature form of plants; this includes the work of the herbalists and of the earlier systematists, who thus furnished the basis for classification. It is true that the mere description was enriched at times by comparisons made, but these often took a capricious form, as is shown by the many curious allusions which still survive in the nomenclature. Erasmus Darwin satirised the imaginative comparisons indulged in by early writers in his "Loves of the Plants"; an instance of this is seen in his lines referring to the legendary organism, half animal, half plant, suggested by the peculiar form of *Dicksonia* (*Cibotium*) *Barometz*:—

> "Cradled in snow and fann'd by arctic air
> Shines, gentle Barometz, thy golden hair.
> Rooted in earth each cloven hoof descends,
> And round and round her flexile neck she bends;
> Crops the gray coral moss, and hoary thyme,
> Or laps with rosy tongue the melting rime.
> Eyes with mute tenderness her distant dam,
> Or seems to bleat, a *Vegetable Lamb*."

The tendency to comparison thus already perceptible asserted itself strongly in the next phase of our study, to which it gave its character. And now the need arose for observing development; this was initiated by Schleiden, and carried to a triumphant climax by Hofmeister. Passing from the hands of these pre-Darwinian to those of post-Darwinian writers, the comparisons, while remaining virtually the same, received a new significance. Observers now pushed their inquiries into the details of anatomical structure and development, and in many cases attached an importance beyond what is justifiable to minute similarities or differences of cell-cleavage. Thus what might be called "cellular morphology" became a feature of the period. It has, however, been in a measure discredited by the excessive zeal of some of its votaries, who drew large conclusions from slight facts; a salient example of this is furnished by studies concerning segmentation of the ovum. But we must not assume that because it has been pursued indiscreetly, the study of segmentation is effete; there is still scope for valuable observation, which will bear a reasonable burden of argument; though conclusions from such a source must be compared with those derived from other data, and a due estimate of them must be made accordingly.

Morphology has lately passed to a third stage—that of experiment—with a view to ascertaining the effect of external agencies in determining form, and the limits of variability under varied circumstances. Development of itself shows only how a part originates; it does not demonstrate what it is, nor what it may become under special conditions. This new and growing phase of experimental morphology, together with comparison from the point of view of descent, now tends to supersede the formal morphology of the second period, which in many minds implied or assumed ideal types or creative plans. It has become a general view that the facts of morphology are but the stereotyped facts of physiology, form being determined by function, but under the check of heredity. This third experimental phase of the study of plant-form is directed, as it were, to the very setting of the types, before the stereotype plate is cast. We watch nature's compositor at work, but we also ascertain that the plate itself, after it is cast, is much more plastic than some of us had thought.

These three phases of morphological inquiry have naturally overlapped one another; we recognise, however, that first description, then formal comparison, and now experiment, have been the leading features in morphological investigation during these successive periods.

HOMOLOGY.

The ideal aimed at in the study of the morphology of plants is to trace their real relationships and mode of origin, on the basis of the widest observation—in short, to reconstruct the evolutionary tree. In order to make comparison possible, or at least manageable, a terminology is necessary, and this not only of the plants themselves, but also of their parts. We may for the moment leave on one side that summing up of morphological opinion represented by the systematic arrangement of plants in a taxonomic system. I propose to-day to discuss not the classification of plants, but the classification of the parts of plants, their grouping according to their *homology*. And here I use a word which is probably explained to every class of elementary students; it is one of those terms a meaning of which is indeed revealed to the babes of the science, while those who teach are not at one as to its definition. We need not enter now into the various opinions which have been held on this point, nor need we make any antiquarian research into the introduction or early use of the word *homology*; it will suffice to state that it was already firmly established in the science before views as to descent gave it any intelligible meaning. We speak of the homologies recognised by Hofmeister, but it should be remembered that their great discoverer did not put an evolutionary interpretation upon them. Sachs points out in his history how "the theory of descent had only to accept what genetic morphology had already brought to view." Nevertheless, much remained ingrained in the very texture of the science which was incompatible with evolutionary thought. This was so even in the text-book of Sachs itself. The categories of root, stem, leaf, and hair are there laid down, and the parts classed under these several heads were held to be *homologous*. In their definition all those characters which refer to function were put aside, the definitions relating to origin and relative position; the reproductive organs were grouped with the rest, with the result that these parts were described as bearing a varying morphological value. But this purely formal morphology is now dead; it long survived a mere passive belief in evolutionary views, but their active practice has strangled it. The first step towards emancipation was the recognition of sporangia as parts *sui generis*. Eichler, agreeing with Braun and Strasburger, found it "highly probable according to the theory of descent" that such a structure as the ovule has universally the same morphological dignity. It remained for Goebel to make the general statement that sporangia stand in a category by themselves, and are probably not the result of modification of any vegetative part. It was in this way that the phylogenetic factor was first asserted as bearing on a question of importance in the morphology of plants. Adherents of descent no longer passively accepted the direct results of investigation; they began actively to check and control the interpretation of them; but this position was not attained till more than twenty years after the publication of Darwin's "Origin of Species." Since then, however, views as to descent have taken an increasingly important place in the province of morphology, till at the present moment a far-reaching comparison of allied forms, assisted by experiment, is the most potent instrument in the hands of the morphologist.

But various writers admit in varying degree this factor of comparison as controlling other considerations. There is indeed a wide range of difference on this point. I will cite only two extreme views. On the one hand is the view of Strasburger, which he enunciated so early as 1872. The enthusiasm for evolution in the Jena school found its botanical expression in the aphorism, "The highest problem of morphology is to explain the form of plants, but this problem can only be solved genealogically." This statement is repeated in a more definite form in Strasburger's text-book: "Phylogeny is thus the only real basis for morphology."

At the other extreme is the method of physiological organography put forward by Sachs in his Lectures. I am aware that he subsequently modified his views; I merely quote the system which he propounded in 1882, as being the antithesis to that of Strasburger. For in the physiological organography descent is hardly taken into account at all; parts which are plainly of distinct origin by descent are classed together. This organography of Sachs, though introduced with all its author's charm of style, never convinced the botanical world, for it treated plants too much as the creatures of present circumstance. It may be taken as illustrating the extreme reactionary swing of the pendulum from the non-physiological attitude of the formal morphologists; a protest against the exclusion of function from the morphological arena. The protest was salutary, but its form was extravagant.

Let us now consider whither "phylogeny, as the only real basis of morphology," may lead us. Let us take as our provisional view that *homology* in the strictest sense implies repetition of individual parts, in successive generations, just as the hand of the child repeats in position and qualities the hand of the mother. Though among seed-bearing plants, for instance, this repetition may apply for the plant-body as a whole, it will be at once apparent that such repetition as regards the individual is found in comparatively few cases in plants. The continued embryology of all the higher forms, the indefinite number of the parts successively produced, and the variety in detail of their arrangement show that in the strictest sense repetition of individual parts cannot be traced. In a pan of seedlings of the Sunflower, raised from seed of the same parent, the cotyledons in all cases may be regarded as homologous in the strictest sense, as they correspond in origin, number, position, and form to like parts in the parent. In a similar way the first root of the seedling appears to be individually identical with the first root of the parent, or of any other seedling of the batch. In those plants in which a foot or suspensor is present occupying a constant position with regard to the parts of the embryo, it will not be doubted that within near lines of affinity the foot in any one specimen corresponds to that of any other. The exact repetition which is thus found to exist may be regarded as the most complete type of homology.

Starting from this repetition of individual parts in plants nearly related, there is a divergence by gradual steps in two directions: Firstly, in the individual plant, where the later formed parts may assume forms and positions which may even raise a question of their essential correspondence. Thus in the batch of Sunflower seedlings there may be a varying number of leaves, with varying transition from the decussate to the alternate arrangement, intervening between the cotyledons and the capitulum. As they vary in number and position these cannot in the strictest sense be accepted as individually comparable, each to each by descent—the lineal representatives of like individual parts in the parent. The lateral roots also, though all essentially similar, do not correspond each to each, either in number or in position.

Again, to go a step further, a Fern prothallus produces antheridia and archegonia; their number and position are not uniform; by conditions of culture we have them under control, and can induce antheridia only, or we can induce a formation of archegonia upon the upper surface, where they are usually absent. Plainly these cannot be held severally as the exact representatives of like individual parts in a previous generation. Another exceptional, but most interesting, case is that of *Aspidium anomalum*, Hk. and Arn., which Sir William Hooker remarks is possibly an abnormal form of *Aspidium* (Polyst.) *aculeatum*, Sw. In this Fern the sori, instead of being all on the lower surface, as in allied Ferns, are often upon the upper surface of the leaf. There is no sign of torsion to explain the anomaly, while the sori themselves present no structural peculiarity except that they are sometimes quite destitute of indusium. There has doubtless been a transfer of developmental capability from the usual position of the sori to the anomalous one. In case of such transfers as these we do not doubt that the parts in question are to be ranked as comparable to those in the normal position; we contemplate here, as in the case of the Sunflower leaves, an essential correspondence, but not an individual repetition of the parts, and we learn that parts thus essentially corresponding to one another may be transferred to unusual positions.

Secondly, in plants more or less nearly related, those which are less akin may show so slight a similarity in detail that again questions of the essential correspondence of the parts may arise. Within nearer circles of affinity these questions will affect only the appendages of minor importance, which show less constancy of occurrence and arrangement, such as emergences and hairs; but in case of plants less nearly akin the degree of correspondence of the larger members may become a matter of debate. Take, for instance, the three great phyla of living Pteridophytes, the Ferns, Equiseta, and Lycopods. While the sporophyte as a whole in each of these may be accepted as homologous by descent with that of the others, the question as to the true correspondence by descent of the leaves must still be open for discussion. It is a tenable view that the three phyla arose separately from a non-foliar ancestry, and that the assumption of a foliar development, having in each case a different habit, and a different relation to the sporangia, led to the distinctiveness of the three stocks. Opinion on the point of homology by descent of the leaves of these Pteridophyta must at present remain in suspense; but the case is different with the leaf of Pteridophytes as compared with the leaf of Bryophytes: unless the whole morphological system of the time be in error, we shall be right in maintaining that these foliar developments have been distinct in origin from the first.

Now all the foliar parts above quoted would in a system of merely formal morphology fall into the category of "leaves." But if phylogeny be accepted as the only real basis of morphology, we must be prepared to split up the category based on mere time, place, and mode of origin, and to recognise in some cases repetition of individual parts; in others essential correspondence, but not individual repetition, owing sometimes to transfer of developmental capability; in other cases again, a possibility of distinct origin by descent not actually proved; and lastly a reasonable certainty of distinct origin. The practical question for the morphologist is, having recognised these facts for himself, how is the matter to be best made intelligible to others?

A reconsideration of the term "homology" will thus be necessary; is it to be applied equally to such parts as are connected by lineal descent, and also to those which we have good reason to believe have resulted from parallel development in quite distinct phyla? Or, to put a finer point upon our inquiry, are we to distinguish in any way the cases of "individual repetition" from those of "essential correspondence"? In the latter case I think no good end would be served at present by accentuating this distinction by terms: the steps of divergence are so slight and gradual. None the less should it be clearly borne in mind that comparisons of parts commonly ranked as homologous in the plant body are based on a less complete individual correspondence than that of parts usually compared in the animal body.

But the case is different in dealing with parallel developments, and some doubt arises whether parts which probably, or it may be certainly, have arisen by separate evolutionary sequence in distinct phyla are to be classed as homologous in the same sense as those directly related by descent. This question was long ago taken up on the zoological side by Prof. Ray Lankester, and it was shown that the old word "homology" covered two things recognised as distinct from the point of view of descent. He defined as *homogenous* "structures which are genetically related, in so far as they have a single representative in a common ancestor." On the other hand, "when identical or nearly similar forces or environments act on two or more parts of an organism which are exactly or nearly alike: further, if, instead of similar parts in the same organism, we suppose the same forces to act on parts in two organisms, which parts are exactly or nearly alike, and sometimes homogenetic, the resulting correspondences called forth in the several parts in the two organisms will be nearly or exactly alike. . . . I propose to call this kind of agreement *homóplasis* or homóplasy." Now this distinction of terms requires also to be observed in plant-morphology, and I am surprised that it has never yet been adopted by botanists, though we have long recognised cases of parallel development. I do not propose now to spend time in assigning these terms to familiar cases: but to take the examples already cited, the leaf of a Fern would be homoplastic, though not homogenetic with the leaf of a Moss; or, taking examples from plants more nearly akin, it would appear possible that the leaves of the three distinct phyla of living Pteridophytes show merely homoplasy not a true homogeny.

The successive foliage leaves of most plants are assumed in the individual to be the result of a mere repetition of development. But it is quite a possible view that in the plant-body (as is contemplated in the animal in those cases of "serial homology" which Lankester recognises as homoplastic) homoplasy may have had a place. We must inquire whether all those structures which we designate "leaves" have actually been the result of a development identical, or at least essentially similar as regards their origin in the race. The problem is, given a plant with numerous leaves of various form and function, to unravel the real story of their evolution. Two distinct factors may be contemplated as possibly occurring even in the individual, viz.:

(1) *Homogeny* of genetically related parts, with or without repetition of the parts formed.

(2) *Homoplasy*, an origin of two or more distinct categories of parts, not genetically related, on the same organism.

Working upon either of these, and thus complicating the problem by obliterating such distinctions as may have existed at first, may be the phenomenon of *metamorphosis*. This has lately received its evolutionary definition at the hands of Prof. Goebel, as restricted to those cases where there has been an obvious change of function. We see how change of function accounts for various forms of leaf in certain cases; but it does not follow that all leaf-forms on the same plant were so produced, by metamorphosis of a single original type.

The Lycopodineae are particularly interesting in illustration of this point. It appears probable that *Phylloglossum* is a more primitive type than other living Lycopods; it has two kinds of leaf, the protophylls borne in irregular number and arrangement on the protocorm, and the sporophylls of different form from these, and arranged regularly on the strobilus: commonly there are no intermediate steps between them. This condition in a plant, which on general grounds of comparison we believe to be primitive, is certainly interesting, and we shall ask whether the two types of leaf have not arisen by distinct evolutionary sequence? In the genus *Lycopodium* there are certain species, such as *L. Selago*, which show alternately sterile and fertile zones; examining the limits of the sterile zones, we find at the base of each leaf an atrophied sporangium, similar in position to that borne by a sporophyll. When we compare this condition with that of *Phylloglossum* it appears probable that the successive zones are the result of a metamorphosis of a strobilus, which had a continuous apical growth, and unlimited repetition of sporophylls, but that some of these suffered atrophy of their sporangia, with the correlative effect of a larger vegetative development. A differentiation of the strobilus thus results in the plant as we see it, a production of foliage leaves by sterilisation of sporophylls. Recognising this, some may suggest that the protophylls originated in the same way. It is possible that they did; but it is equally possible, and, in view of the peculiar case of *Phylloglossum*, I think more probable, that in these plants we have an example of homoplastic development of parts distinct as to descent, while the limits of the two still evident in *Phylloglossum* became obliterated in the more complex case of *Lycopodium*. The proof of the point will be difficult or even impossible, but the eyes of botanists should certainly be open to recognise such individual homoplasy, should it occur, and to inquire whether it has really had a place in plant-development.

Returning now to homoplastic development in distinct groups of plants, the morphology of the *foot* provides interesting material for comparison, and especially so since there is no question of repetition here; for the comparison is between parts of which only one appears on each individual plant.

The term foot has been applied to that part of the embryo in Pteridophyta which serves to connect it physiologically with the prothallus; the term has also been used for the base of the seta in Bryophytes. Parts performing a similar function, but not referable, as in other Phanerogams, to the metamorphosis of cotyledons, are also found in *Gnetum* and *Welwitschia*.

In the Bryophyta what is usually called the foot is no definitely specialised structure; it is merely the absorbent base of the seta. It would appear probable that in the Bryophyta a true homogeny holds in all cases, as the requirement for it will have been uniform; and its basal position is also uniform, though some difference of detail does appear in the relation of this absorbing body to the first segmentation of the embryo.

In the Pteridophyta it is exceedingly difficult to be sure of the correspondence by descent of the foot in distinct types, and indeed it should not be assumed that a specialised absorbent organ was always present, though general surface-absorption will naturally have taken place in all archegoniate embryos; indeed the condition of some upright embryos is such that a foot would never have been described, were it not for comparison with other types. In *Equisetum*, *Isoëtes*, *Botrychium*—all forms without a suspensor, and with an upright growing embryo—the hypobasal half of the embryo, with or without a root, is absorbent as in the Bryophyta, and is described as a foot; it is quite possible to see in them the continuation of a primitive absorbent organ. This may also be the case in the Marattiaceae, and it is specially noted by Campbell that "in *Marattia* all the superficial cells of the central region become enlarged and act as absorbent cells for the nourishment of the embryo." From such types we may imagine the more specialised foot of the Leptosporangiate Ferns to have been derived by a localisation of the absorbent function on one side only, which would be a natural consequence of the embryo taking the prone, in place of the vertical position.

A different course of events probably occurred in the Lycopodineæ. I am disposed to think that here the suspensor represents nothing more than a specialised part of the primitive absorbent organ; this seems to be indicated by the details as shown in Treub's figures of *L. cernuum* and *L. Phlegmaria*, in which the suspensor is continuous with the foot. But what is then the "foot" of *Selaginella*, which is quite apart from the suspensor, the root intervening? On this point I think we obtain light from *Welwitschia* and *Gnetum*, for in these we see an absorptive organ formed at a comparatively late period; and it corresponds in position and function, though not in time of origin or details of structure, with that of *Selaginella*. I conclude that the "foot" of *Selaginella* is probably a later formation, not comparable as regards descent either with the foot of *Lycopodium*, or with the "feeder" of *Welwitschia* or *Gnetum*. The latter are plainly of recent independent origin, as comparison shows, and their actual position is defined according to the position of the seed in germination. Probably, then, there is homoplasy in such cases, not true homogeny.

Similarly with such structures as the pinnae, stipules, indusium, corona, and still more so with such inconstant bodies as emergences and hairs; when we speak of the "homologies" of these parts it is rarely the *homogeny*, or identity by descent, which we mean to express; usually it is only *homoplasy*, a comparison of parts similar, it may be, in form and position, or even in development and function, though not shown to be comparable by descent.

ALTERNATION.

But the questions above discussed are mere matters of detail, compared with that great enigma of the alternation of generations in green plants, or of alternation at large. This is, after all, a question of degree of homology, not now of the parts only, but of the whole plant or "generation." How this greatest of all adaptations was really initiated, we cannot expect to bring to the point of demonstration; at best we can only venture opinions of probability. Still this discussion commands at present more widespread interest among botanists than any other in the sphere of plant morphology.

There was a time when the attempt was made to reduce all plants to one scheme as regards their life-cycle, a method which not only prevented elasticity of theory, but was responsible for some unfortunate comparisons. It was characteristic of the period when the text-book of Sachs reigned supreme; we find it there definitely laid down that "the doctrine of alternation has the object of reducing to one scheme the main phases of the life of all plants which bear sexual organs." But the controversy between Pringsheim and Celakovsky had, as one of its results, the recognition of various types of life-history, not of one scheme only. The tendency at present is towards the opposite extreme; the frequency of the parallel developments now recognised has led some to accept a comprehensive polyphyletic view as regards alternation, and wherever difficulties of comparison arise, to take refuge in the plausible suggestion that the organisms compared represent altogether distinct lines of descent. But the view which should be confidently upheld, is that even where this may actually be the case useful comparisons may yet be made; and that the method of progress within one phylum may illustrate the probable mode of progress in another. The green Algæ may thus throw light upon the probable origin of the sporogonium in the Bryophytes, though they may in no sense be in the line of their descent; the Bryophytes may suggest valuable ideas for the comparative study of the Pteridophytes, though they may not represent their actual ancestry.

It is the alternation as seen in these green plants that I propose to discuss. Writers have distinguished various types of alternation, including under the term divers modes of "alternation of shoots"; and it should be remembered that this was the original sense of the word alternation as applied by Steenstrup. But gradually the issue in the case of green plants has been simplified, and the question now centres round that alternation of phases which some of us describe as "antithetic," while others believe the phases to be really "homologous" as regards their origin.

Briefly put, the question is, How was the first start made? Has the neutral generation or sporophyte been the result of change of any other part of the sexual generation than the zygote itself? If so, the alternation is of *homologous* generations; if not, then the alternation is what is styled *antithetic*. The whole discussion is like a purely historical inquiry, but with the minimum of documentary evidence; for on this point the fossils give scanty help. In the absence of more direct evidence we are thrown back on other aguments, such as those based on comparison of normal specimens, and secondly upon the study of abnormalities. I shall not attempt to treat the matter exhaustively; it will, however, be necessary for me to deal with certain points in the discussion which were raised in the able address of Prof. Scott at Liverpool. He there restated Pringsheim's view of homologous alternation as against the antithetic. I propose now to consider three matters which I think are most material to the discussion, viz. (1) the bearing of the Algae and certain Fungi on the question; (2) the comparison from the Bryophyta; and (3) the argument from abnormalities.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—At the conjoint examination for entrance scholarships, just completed, the following awards have been made in Natural Science. At Pembroke College: Scholarship, G. H. Delf, Camberwell Grammar School, 40*l.* At Gonville and Caius College: Exhibition, M. M. L. Rittenberg, Tonbridge School, 30*l.* At Jesus College: Scholarship, J. Hewitt, Derby School, 40*l.* At Christ's College: Scholarship, C. H. B. Epps, City of London School, 40*l.*; exhibition, R. B. S. Sewell, Weymouth College, 30*l.* At St. John's College: Scholarship, G. C. E. Simpson, Mill Hill School, 60*l.*; Lupton and Hebblethwaite Exhibition (open *pro hac vice*), J. F. Hough, Mason University College, Birmingham; Johnson Exhibition (open *pro hac vice*), B. E. Mitchell, Brighton Grammar School. At Emmanuel College: Scholarship, H. U. B. Banham, Ipswich Grammar School, 40*l.*; exhibition, A. C. H. Rothera, Market Bosworth School, 30*l.* Clare College: Scholarships of 60*l.* to E. B. Bailey, Kendal Grammar School, and W. Cartwright, Middlesburgh Grammar School. Trinity College: Minor Scholarship of 75*l.* to C. S. Coles, University College, London; Exhibitions of 40*l.* to J. Frame, Mason College, Birmingham; C. W. Hutt, St. Paul's School, London; T. C. James, Aberystwyth University College; H. Lambert, Perse School, Cambridge.

Mr. F. G. Hopkins has been appointed University Lecturer in Chemical Physiology.

The degree of LL.D. will be conferred on Lord Kitchener of Khartoum on November 24.

The Clerk Maxwell Studentship in Experimental Physics will be vacant at the end of this term. Candidates, who must have worked in the Cavendish Laboratory, are to send their names to Prof. Thomson by December 9.

It is proposed that Advanced Students shall be admitted to Part II. of the Mechanical Sciences Tripos, and that for the B.A. degree they shall be required to attain the standard of the Second Class at least.

The General Board of Studies have proposed a scheme for the establishment of an Allen Research Studentship under the bequest of the late A. W. G. Allen. The studentship is of the value of 250*l.*, is tenable for one year, and is open in alternate years to students proposing approved courses of research in (1) literary subjects, or (2) scientific subjects.

Seventy-five candidates have presented themselves during the past year for the Sanitary Science Examination. Of these thirty-nine were successful in obtaining the University diploma in Public Health.

The Engineering Laboratory Syndicate have lost no time in proposing a plan for the Hopkinson Memorial Building. The new wing will adjoin the present laboratory, and provide a lecture-room, three laboratory-rooms, and small rooms for students engaged in research. For the completion of the plan some 500*l.* will be required, in addition to the 5000*l.* generously given by Mrs. Hopkinson and her children. It is expected that the building will be ready for occupation in October 1899.

Mr. W. N. Shaw, F.R.S., was on November 10 appointed Assistant-Director of the Cavendish Laboratory for one year.

IN Berlin the flowers gathered in the town gardens are placed in the municipal schools for the purpose of furthering the study of botany. Arrangements have now been made by the London School Board, and will come into operation in April next, whereby a gardener will collect, pack, and forward to the schools of the Board botanical specimens and flowers, budding plants, leaves, &c., required for teaching botany or for object-lessons, or for the combination of drawing and object-lessons.

IT is expected (states the *Athenaeum*) that the London University Commission will commence its sittings this month. Mr. Bailey Saunders, the secretary, has been collecting information in Germany, especially concerning the organisation of higher commercial education, which will be made an important element in the newly constituted university, with the co-operation, it is hoped, of the London County Council. It is probable that the headquarters will be removed from Burlington Gardens. Christ's Hospital is talked of as the new site.

THE Calendar of the Imperial Tientsin University for the year 1897 has been received. The University was established towards the end of 1895, and its faculty includes several graduates of colleges in the United States. Mr. C. D. Tenney is the president, Prof. Oliver C. Clifford occupies the chair of chemistry and physics, Prof. E. G. Adams the chair of civil engineering, and Prof. N. F. Drake the chair of mining. Most of the tutors and teachers are natives of China. It is announced that last year his Excellency Li Chung-ta'ng showed his good will towards the University by a present of a 4-inch telescope, a phonograph, and several things for the physical laboratory.

ALDERMAN JOHN HOPKINSON, the members of his family, and near relatives, have offered to the Owens College, Manchester, in memory of the late Dr. John Hopkinson, a gift of 1600*l.*, to cover the expense of building the dynamo house connected with the new physical laboratory. It is hoped that by additional contributions from friends who desire to see a suitable memorial of Dr. Hopkinson in Owens College, where he was a student for three years, it may be possible to complete and equip the annexe containing in addition to the dynamo house a number of other rooms devoted to electrotechnics, and that the whole may be known as the "Dr. John Hopkinson Electrotechnical Laboratory."

SOCIETIES AND ACADEMIES.

LONDON.

Physical Society, November 11.—Mr. Shelford Bidwell, F.R.S., President, in the chair.—The discussion on Mr. Albert Campbell's paper on the magnetic fluxes in meters and other electrical instruments was resumed. Prof. Ayrton said he wished to offer some remarks on behalf of Mr. Mather and himself. The paper would, perhaps, have received more adequate discussion at the Institution of Electrical Engineers, for it was chiefly of a technical character. The importance of neutralising the effect of leads when using instruments with very weak fields, such as a Siemens' electro-dynamometer, should be emphasised. In instruments of the Kelvin-balance type where two opposed coils carry two opposed currents, the field spreads at the edges; the true "working" flux is not that directly between the coils. Mr. Campbell would have done better if he had used a long search coil wound round one of the swinging coils, forming part of a vertical cylinder. It would have been well also to have supplied some experimental proof that the astatic arrangement of the swinging coils of the Kelvin balance makes the instrument independent of the earth's field. The effect of the earth's field is of the order 0·2, so that with instruments of the Weston type, with a field of the order 1000, it was sometimes assumed, erroneously, that the readings were practically independent of the earth's H. Prof. Ayrton's own tests showed that by turning a Weston voltmeter towards different points of the compass, the errors in a particular case were far greater than might be predicted from the above ratio; the induction in the voltmeter pole-space, due to the earth's field, was much higher than 0·2; the earth's field was exaggerated by the iron pole-pieces; it was not necessary to suppose that the magnetism of the permanent magnet caused the variation. The error observed was about 0·2 per cent. in a horizontal field, and 0·8 per cent. when the field of the voltmeter was parallel to the earth's induction. Here the induction in the gap was 1200, and H = 0·2. In tests relating to the Ayrton and Perry magnifying-spring voltmeter, it was more important to know the B in the air-space *near* the iron than the B *within* the iron. Eddy currents might account for the extraordinary results obtained with the Shallenberger meter. Mr. J. H. Reeves described a method he had adopted for measuring the effect of stray fields upon ammeters and voltmeters. The instrument to be tested is first mounted on a stand and is brought under the influence of a large coil carrying a current. In this way, fields of known magnitude can be superimposed on the working field, throughout the range of the instrument, and the change of deflection due to them can be observed. From these known values, the working field can be deduced. For let the current in the solenoid of the instrument at any moment be A amperes, producing a corresponding unknown working field of magnetic force X. Then X is proportional to the solenoid current, as measured by the indications of the instrument. If a magnetic force x is superimposed on X, then x is measured by x/X of A. If x is known, the working field X can be calculated from the change of deflection produced by the superposition. With Evershed ammeters, the field measured in this way was in one instrument 200, and in another 226; or about one-third of Mr. Campbell's figure (700) for the Evershed ammeter. Mr. Campbell's value of B did not represent the working field, but the field at the end of one of the fixed pieces of iron. Mr. Campbell, in reply, said he thought the theory of electrical instruments to be well within the limits of physics, and he had for that reason presented the paper to the Physical Society. The position chosen for the search coil in the Kelvin-balance tests may not have corresponded to the working flux, but it was near to the right position, and he had carefully specified the position chosen. His results as regards the Weston instrument differed from those of Prof. Ayrton, the errors he had observed for the particular ammeter used were under 0·1 per cent. The earth's field probably produced an effect different for different Weston instruments, according to the degree of saturation of the permanent magnet. In Mr. Campbell's tests, the Weston instrument did not have an iron case.—A paper by Prof. W. B. Morton, on the propagation of damped electrical oscillations along parallel wires, was then read by Prof. J. D. Everett. In a paper published in the *Phil. Mag.* for September 1898, Dr. E. H. Barton compared the attenuation of electrical waves in their passage along parallel wires, as experimentally determined by him, with the formula given by Mr. Heaviside in his theory of long waves. He finds close agreement as regards the effect of a terminal resistance, but large discrepancy in the case of the attenuation constant. Prof. Morton now investigates how far the results should be modified when it is supposed, as under actual conditions, that the oscillations propagated from the origin are damped, and that the circuit is not balanced, as in the ideal case of distortionless transmission. He finds (1) that the velocity of propagation is increased, while (2) the attenuation is increased, and (3) with infinite resistance between the ends of the wires, the waves are, as before, reflected completely with phase unchanged. As the resistance is diminished the amplitude of the reflected waves is decreased, and a phase-difference is introduced. For a certain value of the resistance the reflected amplitude is a minimum, and the phase-difference is $\pi/2$. When the resistance is zero there is again complete reflection, with the phase-difference π; *i.e.* the waves are reversed. The result is that the reflection-factor for amplitude *seems* to pass continuously from (+1) to (−1) *without* passing through zero. Using the numbers given by Dr. Barton, it is found that the corrections to the simple theory are extremely small, so that in actual cases the damping may be ignored, and the circuit may be regarded as distortionless.—Mr. Oliver Heaviside (abstract of communication): Mr. Heaviside, using his own notation, exhibits mathematically the connection between the case investigated by Mr. Morton, of a wave-train arising from a damped source, and the standard case of an undamped source. The cause of the attenuating coefficient coming out twice as great in Dr. Barton's experimental conditions, as when the resistance is calculated by Lord Rayleigh's formula, is attributed to lack of correspondence between the experimental conditions and those of the ideal theory. For: (1) The external resistance, of unknown amount, is ignored. (2) It is not certainly to be expected that the formula in question is true for millions of vibrations per second. It may, however, be concluded from the experiments that the theory furnishes an approximation to the real resistance. (4) The magnetic vibrations to which the wires are subjected are not long-continued and undamped, as assumed by the formula. When a wave-train passes any point on a wire, its surface is subjected to an impulsion vibration lasting only a very minute fraction of a second; a vibration, moreover, which is very rapidly damped. So there is no definite resistance, and the resistance is greater than according to Lord Rayleigh's formula. (5) Perhaps, also, the terminal-reflections involved in Dr. Barton's calculations may introduce error.—The President proposed votes of thanks to the authors, and the meeting was adjourned until November 25.

Chemical Society, November 3.—Prof. Dewar, President, in the chair.—The following papers were read:—Determination of the equivalent of cyanogen, by G. Dean. By determining the quantity of potassium bromide which will react with a known weight of silver cyanide dissolved in nitric acid, the equivalent of cyanogen was ascertained to be 26·065; if the atomic weight of carbon be 12·01, that of nitrogen is 14·055.—The composition of American petroleum, by S. Young. The hydrocarbons boiling between 25° and 115° contained in American petroleum are isopentane, pentane, pentamethylene, isohexane, hexane, methylpentamethylene, benzene, isoheptane, heptane, methylhexamethylene and toluene.—The separation of normal and isoheptane from American petroleum, by F. E. Francis and S. Young. The presence of naphthenes in American petroleum renders it impossible to separate pure iso- and normal heptane from this source by fractional distillation; the impure heptane was brominated, and the heptyl bromides separated by distillation. The pure hydrocarbons were then isolated by reduction with a copper-zinc couple.—The boiling points and specific gravities of mixtures of benzene and normal hexane, by D. H. Jackson and S. Young. It is impossible to separate pure normal hexane by distilling mixtures of this hydrocarbon with benzene.—The action of fuming nitric acid on the paraffins and other hydrocarbons, by F. E. Francis and S. Young. The isoparaffins react readily with fuming nitric acid yielding nitro-compounds, but the normal paraffins are only very slowly attacked.—A composite sodium chlorate crystal in which the twin law is not followed, by W. J. Pope. In a composite crystal of sodium chlorate a four-fold axis of symmetry of the one individual was found to coincide in direction with a three-fold symmetry axis of the other; the plane (011) on the latter is parallel to the plane (010) upon the former.—Stereoisomeric bromonitro- and chloronitro-camphors, by T. M. Lowry. Nitric acid acts on bromo- or chloro-camphor with production of only one nitro-derivative in each case; on brominating or chlorinating nitrocamphor in alkaline solution, however, two stereoisomeric nitro-halogen derivatives are obtained in each case.—Camphoryloxime (camphonitrophenol), by T. M. Lowry. Camphonitrophenol is an oxime of camphoric anhydride.—The formation of ethereal salts of polycarboxylic acids, by S. Ruhemann and A. V. Cunnington.—Note on the action of light on platinum, gold and silver chlorides, by E. Sonstadt. During the action of light on wet silver chloride, a subchloride, hydrogen chloride and hydrogen peroxide are formed.—Methanetrisulphonic acid, by E. H. Bagnall. Fuming sulphuric acid acts upon diacetylbenzidine, its dichloro-derivative, α-acenaphthalide and acetanilide with formation of methanetrisulphonic acid, $CH(HSO_3)_3$.—The nutrition of yeast, by A. L. Stern. An increase of nutriment beyond a definite limit does not materially increase the amount of nitrogen assimilated by yeast, the percentage of nitrogen in the yeast, the weight of the yeast, or the amount of sugar fermented.—The yellow colouring matters of *Rhus cotinus* and *Rhus rhodanthea*. Part vi., by A. G. Perkin. Venetian sumach, the leaves of *R. cotinus*, contains myricetin; the leaves of *R. rhodanthea*, the yellow cedar of New South Wales, contain quercetin and gallotannic acid. The stems of both plants contain fisetin.—Colouring matters of the New Zealand dyewood "puriri," by A. G. Perkin. The New Zealand tree, "puriri" (*Vitex littoralis*) contains two colouring matters as glucosides; these are vitexin $C_{15}H_{14}O_7$ or $C_{17}H_{16}O_8$ and homovitexin $C_{16}H_{16}O_7$ or $C_{18}H_{18}O_8$. The former yields a penta- or hexa-acetyl derivative.—Cannabinol, by T. B. Wood, W. T. N. Spivey, and T. H. Easterfield. A number of derivatives of cannabinol are described.—Derivatives of hesperitin, by A. G. Perkin. Hesperitin combines with sodium or potassium acetate yielding crystalline compounds; the examination of these shows hesperitin to have the composition $C_{32}H_{28}O_{12}$. The azobenzene derivative and hexacetyl derivative of hesperitine have been prepared.

PARIS.

Academy of Sciences, November 7.—M. Wolf in the chair.—Preparation of lithium-ammonium, calcium-ammonium, and the amides of lithium and calcium, by M. Henri Moissan. At low temperatures lithium and calcium dissolve in liquid ammonia, forming deep blue solutions similar to those obtained from sodium and potassium. The lithium and calcium-ammoniums are more stable than the other analogous alkali compounds. Analyses showed that these substances are represented by the formulae $LiNH_3$ and $Ca(NH_3)_4$; both catch fire in contact with air at the ordinary temperature. —Remarks by M. Guntz relating to the preceding communication.—Preliminary note on the presence of free hydrogen in atmospheric air, by M. Armand Gautier. Air taken from the open sea or from mountains at high altitudes is very free from combustible hydrocarbons, traces of which are always present in the air of towns. The pure air, however, contains small quantities of free hydrogen amounting to about 11 to 18 c.c. per 100 litres, about one-half the carbonic acid present in the same air.—Comparison between the methods of Lagrange and Gauss for the resolution of entire numbers of indeterminate equations of the second degree, by M. de Jonquières.—Effects of the section of the nerves of the *sphincter ani* on the functions, physiological and anatomical properties of this muscle, and on the organism in general, by MM. S. Arloing and Edouard Chantre. If the muscle is completely isolated from the spinal column, real incontinence does not necessarily follow, the elasticity of the sphincter being sufficient to cause the expulsion of the faeces and urine.—On the genesis of epithelium, by MM. Armand Sabatier and Étienne de Rouville. It is generally admitted that epithelium is always capable of regenerating itself, and that it borrows nothing from the neighbouring tissues. This is contrary to the results obtained by the authors, who find that in many cases the underlying conjunctive tissue also takes an active part in this regeneration.—Observations on the sun, made at the Observatory of Lyons during the first quarter of 1898, by M. J. Guillaume.—Geodesic, magnetic, and astronomical surveys of Madagascar, by M. P. Colin.—On the convergence circle of some series, by M. Lean.—On stability, by M. J. Andrade.—Hertzian telegraphy without wires between the Eiffel Tower and the Pantheon, by M. E. Ducretet. The space over which the signals were transmitted was about four kilometres, and the clearness of the record showed that this distance could be considerably increased.—On the compound winding of alternators of constant voltage, by M. Maurice Leblanc. An alternator constructed on the principles here laid down was found to have a very constant voltage, even when changed rapidly from no load to full load. The residual magnetism is sufficient to render the dynamo self-exciting.—Contribution to the study of the boric ethers. Properties of triethyl borate, by M. H. Copaux. Chlorine gives trichlorethylether, $CH_2Cl_2.CHCl.O.C_2H_5$; sodium ethylate in absolute alcohol gives a precipitate of $BNa(OC_2H_5)_4$.—Combinations of phenyl-hydrazine with the halogen salts of the alkaline earth metals, by M. J. Moitessier. A description of the preparation of $CaBr_2.3H_2O.4C_6H_5N_2H_3$; and $SrI_2.4C_6H_5.N_2H_3$.—Estimation and detection of gelatine in gums and food substances, by M. A. Trillat. The aqueous solution of the gum is treated with formaldehyde. The gelatine is rendered insoluble.

and can be weighed after thorough washing by decantation. Test experiments gave results within one per cent.—The culture of some lower organisms in modified sea water, by MM. P. Duflocq and P. Lejonne. Several pathogenic organisms can become accustomed to grow in a medium containing sea water, also certain moulds (*Aspergillus niger*, *Muguet*, *Actinomyces*).—Action of the bacterium of sorbose on the aldehydic sugars, by M. Gabriel Bertrand. Arabinose, dextrose, and galactose are converted by the action of the sorbose bacteria into arabonic, gluconic, and galactonic acids respectively.—Instantaneous submarine photography, by M. Louis Boutan. In the earlier attempts at submarine photography, at least half an hour's exposure was required. The apparatus has now been so far improved that photographs have been taken of fish at distances at 1·5 tc 2 metres from the lens, without any artificial light being necessary.—The post-larval stages of *Arenicolae*, by M. Pierre Fauvel.—The cephalic eyes in Lamellibranchs, by M. Paul Pelseneer.—On the Chamydomonadineae, by M. P. A. Dangeard.—On some new facts in the geology of the Dauphiny Alps, by M. W. Kilian.—On some lakes in the Eastern, Upper, and Lower Pyrenees, by MM. André Delebecque and Etienne Ritter.—Barometer movements on the meridian of the moon, by M. A. Poincaré.

DIARY OF SOCIETIES.

THURSDAY, NOVEMBER 17.

ROYAL SOCIETY, at 4.30.—Further Note on the Sensory Nerves of the Eye-Muscles: Prof. Sherrington, F.R.S.—Further Observations on the Effects of Partial Thyroidectomy: W. Edmunds.—Contributions to our Knowledge of the Formation, Storage, and Depletion of Carbohydrates in Monocotyledons: J. Parkin.—An Experiment in Search of a Directive Action of One Quartz Crystal on another: Prof. Poynting, F.R.S., and P. L. Gray.—The Electrical Conductivity and Luminosity of Flames containing Vaporised Salts: Prof. Smithells, H. M. Dawson, and H. A. Wilson.

LINNEAN SOCIETY, at 8.—On some Spiders from Chile and Peru, collected by Dr. Platte of Berlin: F. Pickard Cambridge.—The Botanical Results of a Journey into the Interior of Western Australia; with some Observations on the Nature and Relations of the Desert Flora, and on the Probable Origin of the Australian Flora as a whole: Spencer Le M. Moore.

CHEMICAL SOCIETY, at 8.—Preparation of Hyponitrite from Nitrite through Oxyamidosulphonate: Dr. E. Divers, F.R.S., and T. Haga.—(1) Absorption of Nitric Oxide in Gas Analysis; (2) Interaction of Nitric Oxide with Silver Nitrate; (3) Preparation of Pure Alkali Nitrites; (4) The Reduction of an Alkali Nitrite by an Alkali Metal; (5) Hyponitrites: their Preparation by Sodium or Potassium and Properties: Dr. E. Divers, F.R.S

FRIDAY, NOVEMBER 18.

INSTITUTION OF CIVIL ENGINEERS, at 8.—The Production of Liquid Air, and its Application to Chemical and other Industries: Cecil Lightfoot.

QUEKETT MICROSCOPICAL CLUB, at 8.

MONDAY, NOVEMBER 21.

SOCIETY OF ARTS, at 8.—Acetylene: Prof. Vivian B. Lewes.

IMPERIAL INSTITUTE, at 8.30.—The Stalactite Caves of New South Wales: Frederick Lambert.

TUESDAY, NOVEMBER 22.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Paper to be further discussed: Electrical Transmission of Power in Mining: William Beedie Esson.

ANTHROPOLOGICAL INSTITUTE, at 8.30.—The Hill Tribes of the Central Indian Hills: their Ethnology, Customs, and Sociology (with Lantern Illustrations): Wm. Crooke.

WEDNESDAY, NOVEMBER 23.

SOCIETY OF ARTS, at 8.—Long Distance Transmission of Electric Power: Prof. George Forbes, F.R.S.

GEOLOGICAL SOCIETY, at 8.—Note on a Conglomerate near Melmerby, Cumberland: J. E. Marr, F.R.S.—Geology of the Great Central Railway—Rugby to Catesby: Beeby Thompson.—On the Remains of *Amia* from Oligocene Strata in the Isle of Wight: E. T. Newton, F.R.S.

THURSDAY, NOVEMBER 24.

ROYAL SOCIETY, at 4.30.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—Rotatory Converters: Prof. S. P. Thompson, F.R.S.

FRIDAY, NOVEMBER 25.

PHYSICAL SOCIETY, at 5.—On the Properties of Liquid Mixtures: R. A. Lehfeldt.—On certain Diffraction Fringes as applied to Micrometric Observations: L. N. G. Filon.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—La Cytologie Expérimentale: Dr. A. Labbé (Paris, Carré).—Publications of the British Fire Prevention Committee, Vol. 1 (1 Waterloo Place).—Allgemeine Biologie: Prof. M. Kassowitz, i. Band (Wien, Perles).—General Report on the Operations of the Survey of India Department during 1896-97 (Calcutta).—From Matter to Man: A. R. Dewar (Chapman).—Wild Animals in Captivity: A. D. Bartlett (Chapman).—Leçons de Chimie Physique: Prof. J. H. Van 't Hoff, translated by Prof. Corvisy, Part 1 (Paris, Hermann).—Traité Élémentaire de Mécanique Chimique: Prof. P. Duhem, Tome iii. (Paris, Hermann).—Through Arctic Lapland: C. Hyne (Black).—The Total Solar Eclipse, January 22, 1898 (Dehra Dûn).—Traité d'Astronomie Stellaire: Prof. C. André, 1 Partie (Paris, Gauthier-Villars).—Practical Inorganic Chemistry for Advanced Students: C. Jones (Macmillan).—Birds of the British Isles: J. Duncan (Scott).—Aperçus de Taxinomie Générale: J. P. Durand (Paris, Alcan).—Life of Man on the High Alps; A. Mosso, translated by E. L. Kiesow (Unwin).

PAMPHLETS.—Review of Mineral Production in India for 1897 (Calcutta).—Replica di Krupp alla Protesta del Signor Bashforth: translated by F. Bashforth (Cambridge University Press).—Mines and Quarries: General Report and Statistics for 1897, Part 3, Output (Darling).—Temperature Entropy Chart; Captain Sankey (Rugby, Frost).—Sections and Thickness of the Lower Silurian Formations on West Canada Creek and in the Mohawk Valley: C. S. Prosser and E. R. Cumings (New York).—The Classification and Distribution of the Hamilton and Chemung Series of Central and Eastern New York: J. Hall and C. S. Prosser, Part 1 (New York).—Zwanzig Briefe g. z. Jöns Jakob Berzelius und Christian Friedrich Schönbein, 1836–1847, Herausgegeben von Prof. Kahlbaum (Basel, Schwabe).

SERIALS.——Morphologisches Jahrbuch, 26 Band, 2 Heft (Leipzig).—Scribner's Magazine, November (Low).—Observatory November (Taylor).—Encyklopaedie der Naturwissenschaften, Erste Abthg., 73 and 74 Liefg.; Dritte Abthg, 44 and 45 Liefg. (Breslau).—Engineering Magazine, November (222 Strand).—Himmel und Erde, November (Berlin).—Quarterly Journal of the Geological Society, November (Longmans).—American Naturalist, October (Ginn).—Astrophysical Journal, October (Chicago).—The Process Photogram, November (Dawbarn).—Transactions of the Edinburgh Field Naturalists' and Microscopical Society, Vol. iii. Part 7 (Blackwood).—American Journal of Science, November (New Haven).—Psychological Review, November (Macmillan).—American Journal of Mathematics, October (Baltimore).—Journal of the Chemical Society, November (Gurney).

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