THURSDAY, APRIL 6, 1899.

BIRDS.

Birds. By A. H. Evans, M.A. Being Vol. ix. of the Cambridge Natural History. Pp. xvi + 635. (London: Macmillan and Co., Ltd., 1899.)

IN getting together the material for this exceedingly elaborate treatise upon birds, Mr. Evans has shown an industry and discrimination which must at once be obvious to those who have any acquaintance with the enormous literature of the group. When one thinks of the many long handbooks upon British birds only, and the extensive series of large monographs upon special families, it is really a matter for wonder how the author can have condensed into rather less than 600 pages an account of the external characters of so huge a number of existing and extinct genera and species. Mr. Evans has accomplished this difficult task by compressing into as few words as possible the salient characters, or, in many cases, character, of the genus or species, and by a rigid economy in the matter of anatomical and "bionomical" fact. This is not mentioned by way of an introduction to a criticism of the method adopted by the author. It is obviously desirable that the volumes of the Cambridge Natural History should preserve an approximate equality in size; while to give two volumes to birds —which would be necessary were their structure treated of as exhaustively as are the external features—would destroy the perspective of the series. Besides, as Mr. Evans mentions in his preface, several works, such as Dr. Gadow's contribution to Bronn's "Thierreichs," Fürbringer's colossal work, the anatomical sections in Prof. Newton's "Dictionary of Birds," and more recently still Mr. Beddard's "Structure and Classification of Birds," have rendered it less incumbent upon the author to deal more fully with this branch of ornithology. These considerations have clearly made it difficult for Mr. Evans to decide how much anatomy he should include in his work.

Only twenty-two pages are devoted to structure, geographical distribution, classification, and migration. It is unnecessary to point out that to deal with all these important matters, even in the briefest fashion, twenty-two pages is not quite adequate. It would have been better, perhaps, to have allowed the characters of the feathers and pterylosis, beak and claw, and colour and moult, to have absorbed more of this limited space, and to have slightly expanded the "terminology," with, perhaps, an explanatory diagram of the skeleton and of the viscera. In the part relating to feathers and external characters generally, that much discussed matter, "Quintocubitalism," is not even favoured with a mention, let alone a definition. It is unnecessary to point out that many other facts of importance in classification share the same neglect.

As to this latter department, Mr. Evans follows Dr. Gadow's scheme, for which we have a very considerable respect. But it really does not matter greatly in a book of this kind which of the more reasonable schemes is selected. "Quot homines tot sententiæ" (with a very distinct accent on the last word) is a maxim which might have been created for the science of ornithology. The schemes afloat are so very numerous, and so frequently based not upon anatomical fact but upon opinion. The only consolatory thought is that one of them must be right—so complex are the varied combinations and permutations.

As to the smaller details of classification, we are not always in accord with Mr. Evans. He places the African "Shoe bill," *Balaeniceps*, definitely with the herons, in the very same family Ardeidæ. The bird has not, it is true, been studied anatomically in anything like an exhaustive fashion; but enough is known, in our opinion, to militate against this placing of it. Indeed, in his prefatory sketch of the characters of the Ardeæ, the author admits that the bird "might well stand alone in a sub-family" opposed to other herons, or that it might even be handed over to the storks. This opinion is, however, abandoned when the actual bird comes to be treated of—a course of action which shows a somewhat unnecessary vacillation. Some ornithologists also would deprecate the isolation of the flamingo, and would definitely term it a stork. But it is not possible to be dogmatic upon the point. In the arrangement of the cuckoos among themselves, Mr. Evans follows Captain Shelley's British Museum Catalogue of the group. It does not appear to us to be requisite to allow so many as six sub-families; but with the exception of the Neomophinæ, concerning the merits of which defective anatomical knowledge does not permit of an opinion, the sub-families do represent grades of structural diversity. This, however, is only the case if we refer to each the genera which cluster round that particular genus from which the sub-family takes its name. To place among the Cuculinæ, typified by the common cuckoo of this country, the eastern *Eudynamis* and *Scythrops* is a very serious offence against the teachings of anatomy. It is to *Phoenicophäes* that the two genera in question are plainly most nearly allied. The matter is of further importance on account of Prof. Fürbringer's extremely reasonable suggestion that these three genera stand near to the base of the cuckoo tribe; a view supported—it should be added—by the former existence (as shown by fossil remains) of the genus *Phoenicophäes* in Europe.

While Mr. Evans has been unnecessarily liberal in his sub-divisions of the Cuculidæ, he has hardly done justice to the structural diversity that exists among the auks. If it was necessary to divide the cuckoos at all—and we are fully in accord with the author in so doing—it is at least equally advantageous to sub-divide the not more homogeneous family of the Alcidæ. The genera *Lunda* and *Fratercula* are connected by closer bonds than either is with *Uria* and *Alca*, while the two latter form an alliance which is equally patent to the anatomist.

It has been hinted that Mr. Evans is somewhat sparing of anatomical fact in this volume. At the same time he by no means ignores in places anatomical characters. For the most part we find that these characters are quite accurately described. But here and there is a slip. For instance, the Anseriformes are defined by the possession of two pairs of sterno-tracheal muscles, "a marked point of distinction from other Carinate birds." It is unquestionably true that this is a characteristic feature of the large group of ducks, geese, swans, and screamers; but they are not the only birds which possess this double allowance of extrinsic tracheal muscles. Among the Gallinaceous birds, which present in other points of structure such unexpected likenesses to the Anseres, the same arrangement of the muscles is met with. And the list of birds with two pairs of extrinsic tracheal muscles is not exhausted by the facts mentioned. The number of rectrices in the owls is not invariably twelve, as seems to be implied by Mr. Evans. It is certainly usually so, but one genus is said to possess only ten. This criticism may be fairly regarded as rather pedantic; not so, however, our objection to Mr. Evans' statement that in the owls "the syrinx is bronchial." There is no qualification of this bare sentence, which is only partially true. Many owls, it is well known, do show a purely bronchial syrinx—an interesting likeness to the goat-suckers; but in others the normal tracheo-bronchial syrinx is evident.

At intervals among the exceedingly conscientious descriptions of colour and plumage are to be discovered a few notes upon the customs and habits of the bird tribe. Mr. Evans is particularly emphatic upon the varied sounds and songs—an exceedingly useful part of the subject to lay stress upon. But the tyro, who is probably often puzzled by the notes of a strange bird, will be baffled by the variety of utterances which are described as occasionally pertaining to the same species. One species, for instance, is credited with so great a variety of noises that they can only be expressed by "a bray, a croak, a harsh cackle, a diabolical scream, a puppy's whine, or a soft whistle." "The voice of the goatsucker' is generally hollow, but is described in various cases as a "croak," a "loud shrill cry," a "sad whistle," a "jarring note," or a "moan." We presume that under these extraordinarily diverse tones more than one species is included, though it is not quite apparent from the text. The croaking of the goatsucker reminds us of a slight error in Prof. Newton's "Dictionary of Birds." The professor, under the heading "Night Raven," observes that it is "a bird frequently met with in fiction, but apparently nowhere else." Now it is quite curious that this word immediately follows "Nightjar" in the work referred to—curious because John Hunter, under the heading "Night-Raven," described the anatomy of the nightjar or goatsucker.

Under what may be termed "bionomics," Mr. Evans has some remarks upon mimicry, not condensed into a chapter or section, but scattered through the body of the work. These remarks are not numerous, only four instances being given, or at least noted, in the index. They are entirely among Passerines, and do not include, to our mind, the most striking examples of superficial likeness between birds remote from each other in the system. One of the very best instances is the likeness between the large ground cuckoo *Carpococcyx* and a gallinaceous bird. The cuckoo suggests a pheasant, in its size chiefly and gallinaceous strut, but the colours are not at variance with the view that it represents a genuine case of selective variation in the direction of a gallinaceous model; and it will be remembered that in the birds which the cuckoo suggests there are formidable spurs, which seem to form an excuse for the attempt at protection. But Mr. Evans is unnecessarily redundant when he speaks of this singular phenomenon as "unconscious mimicry"; no one could seriously urge a conscious attempt to put on the appearance of some other bird. But such instances do not form the strongest argument that the believer in mimicry has at his disposal. The advantage is not plain in most cases, and the question of genetic alliance, and therefore genuine likeness based upon affinity, is not to be settled at once. Prof. Garrod, it will be remembered, thought the cuckoos to be not far from the Gallinæ, and there is much to be said for his view.

Such general questions as migration are not neglected by the author. But he has not found space to treat of the matter in a comprehensive way. It may be that more has been written upon this subject elsewhere than it will quite bear; but Mr. Evans has erred—if he has erred—upon the absolutely opposite side. The astounding statements of Herr Gätke upon the prodigious velocity of the migration flight are, we are relieved to find, not accepted; in this matter Mr. Evans might well have quoted Mr. Whitlock's interesting criticisms of Dr. Gätke's calculations. The remarkable way in which individual birds are believed to return to the immediate neighbourhood whence they set forth, is strikingly illustrated by an instance which we have recently seen quoted, though the event itself happened in the year 1834. A gentleman resident in Poland captured a stork and fixed a collar round its neck made of iron, and inscribed "Haec ciconia ex Polonia." The following year the bird returned to his estate, but this time with a golden collar bearing the inscription, "India cum donis mittit ciconia polis." We can answer for the accuracy of the quotation, though the grammar seems to have suffered on behalf of the scansion.

The concluding remark which we have to offer about Mr. Evans' book can contain nothing but praise. The illustrations are most admirable, which indeed might be expected from their author, Mr. G. E. Lodge. The majority, at any rate, are by that well-known draughtsman; some few are from other sources. F. E. B.

A LADY'S DIGGINGS IN EGYPT.

The Temple of Mut in Asher: an Account of the Excavation of the Temple and of the Religious Representations and Objects found therein, as illustrating the History of Egypt and the main Religious Ideas of the Egyptians. By Margaret Benson and Janet Gourlay; the inscriptions and translations by Percy E. Newberry. Pp. xvi + 391. (London: John Murray, 1899.)

THE Egyptian goddess Mut was the wife of the god Åmen-Rā and the second member of the great Theban triad of deities, which consisted of herself, her spouse Åmen-Rā and her son Chonsu, the god of the Moon. She symbolised nature and was regarded as the mother of all things, as indeed her name *Mut*, "the mother," implies. Her temple at Karnak, situated in a district termed *Åsher* by the ancient Egyptians, stood to the south-west of the great temple of Åmen-Rā, to which it was connected by a long avenue of sphinxes. A little to the north-west stands the temple of Chonsu, her son, from which another avenue of sphinxes led to Luxor.

Her temple is thus, with the exception of a small temple of Rameses III., the southernmost of those that form the Karnak group. Although in consequence of its ruined condition it, perhaps, receives little attention from the passing tourist, its importance has long been recognised, and it has been frequently studied and described. As Mariette pointed out, although its structure has suffered more than that of others at Karnak, its interest is considerable; for we have in it an entire temple, with its surrounding wall, its pylons, sphinxes and sanctuary, and its sacred lake, which encloses the temple on the south in the form of a great horse-shoe.

Towards the middle of the present century the condition of the temple was probably very much more perfect than it is at present, for about the year 1840 it appears to have been used, with Mohammed Ali's permission, as a stone-quarry during the erection of a saltpetre manufactory in the neighbourhood. The British Museum possesses two manuscript maps of the temple by Burton and Hay, which were probably made between 1830 and 1840; and as Burton's evidently belongs to the period before the saltpetre factory was built, its value as evidence of the former condition of the temple is great. At this period the walls may have stood several feet above the ground, so that the ground-plan of the temple could be traced without difficulty. After they had been levelled, the plan of the temple could only be made out by removing the débris from the bases of columns and the foundations of walls that still remained.

Neither of the plans of the temple made by Burton and Hay was published, and the first published plan is that of Lepsius, made during the Prussian Survey in the years 1842–45. In 1869 Duemichen published a copy of part of the inscription of the time of Tirhakah from the walls of a small chamber in the temple, and in June 1872 de Rougé read a paper before the Academie des inscriptions, in which he translated passages from the inscription Duemichen had published. Since Lepsius' survey, however, no detailed examination of the temple was undertaken until Mariette partly excavated the site, and in 1875 published in his "Karnak" the results of his excavation in the form of the plan which has been regarded as the authoritative plan of the temple up to the present time. Mariette also published a fuller copy of the inscription from the chamber of Tirhakah, which was again republished in 1890 by M. Urbain Bouriant, together with another mutilated inscription from the western wall of the temple. In 1891–93 Sir Norman Lockyer spent three seasons in Egypt studying the orientation of the principal Egyptian temples, among which he included the temple of Mut; as the result of his investigations, he provisionally assigned the date of its foundation to about B.C. 3500.

Such is a brief sketch of the principal surveys and studies of the temple and its inscriptions that had been made up to the time Miss Benson began her work on the site. After a visit to Egypt in 1894, Miss Benson tells us she first entertained the idea of undertaking some excavation, and in the following year she obtained permission to clear away some of the earth that still covered the ruins of the temple of Mut. For three seasons Miss Benson and her friend, Miss Gourlay, have occupied themselves in removing débris, and, though they have made no very startling discoveries, they have succeeded in correcting Mariette's plan of the temple in several details, and in the course of their work have found a number of inscribed statues and fragments.

The first year of excavation was devoted to the outer court of the temple, and did not yield many finds, the most important being a statue of a royal scribe with the cartouche of Ȧmenḥetep II.; and as this was found apparently *in situ*, it served to throw back the date of the temple's foundation, which Mariette had assigned to Ȧmenḥetep III. During the next two seasons the colonnaded court, the hypostyle hall, and the chambers built around the sanctuary of the goddess were cleared. Mariette, in his plan, though in the main correct, had indicated that these chambers were arranged symmetrically; but Miss Benson, by a more complete clearing of the foundations, has shown that such a symmetrical arrangement was not strictly adhered to. It was to be hoped that her excavation would have rendered it possible to assign dates to the various portions of the temple, and this has been done for several portions that were left uncertain by Mariette; unfortunately, however, sufficient evidence has not been found for dating considerable parts of the structure. In his "Dawn of Astronomy" Sir Norman Lockyer has emphasised the importance of ascertaining such dates where possible, for subsequent additions to a temple may considerably interfere with the original design of its orientation. The fresh data obtained by Miss Benson, however, so far as they go, are in favour of the early period assigned to the foundation of the temple by Sir Norman Lockyer.

Of the finds made by Miss Benson in the course of her excavation, the seven fragments of inscribed stelæ and the inscriptions on the Sekhet statues are unimportant. Of the thirty-one inscribed portrait-statues and fragments, perhaps the most interesting is a statue of Sen-Mut, the architect of Queen Ḥâtshepset, who reigned about B.C. 1600. Ḥâtshepset's name is chiefly associated with the beautiful temple at Dêr el-baḥari, on one of the walls of which is sculptured her famous expedition to the land of Punt. The erection of this temple was the chief architectural work of her reign, and Sen-Mut was the architect who carried out her instructions. In the statue found by Miss Benson he is represented kneeling and holding before him a Hathor-headed shrine, while both the body of the statue and its pedestal bear inscriptions giving his parentage and the offices he held. That Sen-Mut was the queen's favourite, and a powerful official, is well attested by the records that we have of him. Another statue of him and his funeral stela are preserved at Berlin, while his portrait is sculptured in one of the compositions at Dêr el-baḥari; from these monuments, and from an inscription on the rock at Aswân the main facts concerning Sen-Mut's career have long been known. Another find of some interest are five blocks of stone which formed part of a wall of a chamber in the temple built by Piānchi, King of Ethiopia, about B.C. 766. From the sculptures on them we learn that this monarch, following Queen Ḥâtshepset's example, undertook a foreign expedition with the object of bringing the riches of the South to Thebes. The ships which formed the expedition are represented returning laden with cargo, and from the plants, palm-nuts, &c., depicted, it seems not improbable that the expedition penetrated to the region south of Khartûm. The blocks are roughly carved, and of course, whether from an artistic or historical point of view, cannot be compared with the famous reliefs of the expedition to Punt; but they, at least, bear witness to a foreign expedition of Piānchi that is not elsewhere recorded.

It will be seen, therefore, that Miss Benson and Miss Gourlay have had some reward for their three seasons' work; and, although surface-excavation at Karnak is not a very arduous or difficult undertaking, it is not unreasonable that they should be proud of having obtained the first permission to excavate given to women in Egypt. Whether their example will be followed by other ladies remains to be seen, though we think on the whole such work is perhaps better left to the male professional digger, who can camp on the spot, and having a knowledge of Arabic is naturally better able to control his men, and can check to some extent the thefts of the smaller antiquities. Of the general plan of the book in which Miss Benson and Miss Gourlay, with Mr. Newberry's help, have published the results of their work, one word must be said. The excavation of a temple site, which results in correcting a previously published ground-plan, and in recovering a number of statues of secondary importance, is of the highest interest to the expert, but does not appeal to the general public. Yet Miss Benson has more than doubled the size of her book by adding sketches of the religion and the history of Egypt. In the preface it is stated that this has been done for the benefit of those who, "without technical knowledge, feel the fascination and interest of Egypt." But for this class of reader it cannot be said that at the present day there is any lack of sound popular histories. In fact, in describing her diggings, Miss Benson should have addressed herself only to the expert; he would have been contented with Parts ii. and v. of the book, and the result would have been a very much handier volume.

OUR BOOK SHELF.

Die Medial-Fernrohre. By L. Schupmann. Pp. 145. (Leipsig: B. G. Teubner, 1899.)

Every one is familiar in a general way with the optical parts of a reflector and refractor, the former containing in its optical series a parabolic reflector with a smaller reflector and eyepiece, and the latter consisting of an objective of two different kinds of glass for the elimination of colour, and the necessary eyepiece near the focus.

The "medial-fernrohr" and "brachymedial-fernrohr," both of which are discussed here, may each be described generally as being a combination of a refractor and reflector, for the functions of both an objective and a curved reflecting surface are required.

In the principle involved in this new method of construction it is possible to produce an achromatic telescope with the employment of only one *kind* of glass, and the author, who had a telescope made as a preliminary trial of the system he was investigating, says "derartige Systeme hätten also zur Not schon konstruiert werden können, bevor man die verschiedene Dispersion der Glasarten kannte."

The achromatism in the telescopes here under discussion is obtained by making the objective of one kind of glass, and, before the focus is reached, of intercepting the light rays by a curved mirror near the surface of which another lens of a different kind of glass is placed. The light rays thus pass twice through the second lens.

In these pages the author takes the case of the medial-fernrohr first, and discusses the optics of its system very thoroughly, using terms in the discussion which are generally considered inappreciable in other systems. The result of the investigation speaks very highly for this class of instrument, and the greater the aperture the more efficient does it seem to become. It would naturally be thought that the employment of two lenses and a reflector would tend to diminish very considerably the brightness of the image, especially as the rays pass twice through one of the lenses. We are told, however, that comparing an ordinary refractor and a "medial" of 12 cm. aperture, the brightness of the image in the case of the former exceeds that of the latter by 15 per cent.; but comparing apertures of 34 cm., the medial has the advantage of 7 per cent. For equal apertures of 1 metre the "medial" exceeds the refractor by as much as 30 per cent.

Towards the latter part of this volume the author describes the method of mounting and adjusting an instrument of large dimensions. He then discusses the "brachymedial" telescope, adapting the formulæ obtained in the previous portion of the book to this form of instrument. Here he also describes a telescope of large aperture on this principle, but although optically it does not attain the efficiency of the "medial," yet the fact, that the length of the tube is very considerably shortened by a more compact arrangement of the optical parts, may counterbalance this deficiency.

In conclusion, we recommend to our astronomical and physical readers the work before us; for it is only by such investigations that further advance in our present instrumental equipment can be made.

Mathematical and Physical Tables. By James P. Wrapson and W. W. Haldane Gee. Pp. viii + 215. (London: Macmillan and Co., Ltd., 1898.)

In these pages the compilers have brought together a most useful set of tables and formulæ which should be found of great service, both in the class-room and laboratory. The first part of the book is devoted simply to tables: these include, among others, four-place logarithms and antilogarithms, natural and logarithmic sines, cosines, and tangents, tables of squares, square roots, cube roots, &c. In the next section the reader has brought before him the chief formulæ in pure and applied mechanics; here, for example, he can find at a glance the lengths of curves, areas and volumes of solids, plane and spherical trigonometrical formulæ, formulæ used in analytical geometry, and others connected with dynamics, pendulums, elasticity, and hydraulics.

The other sections, the contents of which are too numerous to mention, consist of tables of pure and applied physics, which should be found very useful, and formulæ in pure and applied physics, which include optics, heat, magnetism, electrostatics, electro-chemistry, electro-magnetic induction, and alternating currents, &c. The volume concludes with an appendix containing other useful miscellaneous information, and an index.

If the book be used judiciously, and employed simply as a means of reminding the student of formulæ and data which may have grown rusty by disuse, its value is to be recommended; but it should not be given to young students, who would probably work out problems without knowing the why and wherefore of the expressions they are using.

For neatness and conciseness, and the numerous clearly printed diagrams, the volume will be found a desirable source of information, and considerable pains seems to have been taken to bring the data up to date.

La Photographie Animée. By Eug. Trutat, Director of the Natural History Museum, Toulouse. Pp. xii + 185. (Paris: Gauthier-Villars, 1899.)

THIS volume, introduced by a preface by M. Marey, the well-known chronophotographer of animals and human beings in motion, for purposes of study, will be found useful to all interested in the subject of animated photography.

The author devotes the opening chapter to a short review of the history of the subject, explaining the application of the phenomenon of persistence of vision in such early instruments as the phénakisticope and zootrope of Plateau and Clerk Maxwell.

He then traces the evolution of the apparatus from the multiple cameras of Muybridge, Anschutz, Londe and his own to the first employment of a fixed plate by M. Marey, and then to the continuous band machines of Marey, Edison, Demeny and others. In this chapter will be found well-illustrated descriptions of most of the French machines which have proved successful.

The third and concluding chapter deals with the various manipulations necessary for obtaining the photographs, and afterwards exhibiting them. The operations of exposure, development, and printing of the positive film are lucidly explained, and then details are given for the management of the film in the lantern.

There is no doubt of the usefulness of the treatise, but its value is somewhat lessened by the descriptions being almost entirely confined to French apparatus, the author giving no signs of being familiar with the successful machines which have been produced outside his own country.

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 Interferometer.

THE questions raised by Mr. Preston (NATURE, March 23) can only be fully answered by Prof. Michelson himself; but as one of the few who have used the interferometer in observations involving high interference, I should like to make a remark or two. My opportunity was due to the kindness of Prof. Michelson, who some years ago left in my hands a small instrument of his model.

I do not understand in what way the working is supposed to be prejudiced by "diffraction." My experience certainly suggested nothing of the sort, and I do not see why it is to be expected upon theoretical grounds.

The estimation of the "visibility" of the bands, and the deduction of the structure of the spectrum line from the visibility curve, are no doubt rather delicate matters. I have remarked upon a former occasion (*Phil. Mag.*, November 1892) that, strictly speaking, the structure cannot be deduced from the visibility curve without an auxiliary assumption. But in the application to radiation in a magnetic field the assumption of symmetry would appear to be justified.

My observations were made with a modification of the original apparatus, which it may be worth while briefly to describe. In order to increase the retardation it is necessary to move backwards, parallel to itself, one of the perpendicularly reflecting mirrors. Unless the ways upon which the sliding piece travels are extremely true, this involves a troublesome readjustment of the mirror after each change of distance. The difficulty is avoided by the use of a fluid surface as reflector, which after each movement automatically sets itself rigorously horizontal. If mercury be contained in a glass dish, the depth must be considerable, and then the surface is inconveniently mobile. A better plan is to use a thin layer standing on a piece of copper plate carefully amalgamated. A screw movement for raising and lowering the mercury reflector is still desirable, though not absolutely necessary.

RAYLEIGH.

Theory of Functions.

IN his review of our book on "Analytic Functions" (NATURE, February 23), Prof. Burnside makes three specific charges of inaccuracy; we shall show that the inaccuracy is his, not ours.

(1) One charge relates to the difference of two convergent series. There is an elementary and well-known theorem which states that the difference of two convergent series $\sum_1^\infty a_n$ and $\sum_1^\infty b_n$ is equal to $\sum_1^\infty (a_n - b_n)$, no matter whether the convergence of the series be unconditional or conditional. Prof. Burnside has, then, fallen into a very serious error when he says of this very operation of subtraction that "the rearrangement involved is one which cannot be used with conditionally convergent series, as indeed the authors have shown most clearly in an earlier chapter." We must add that there is no "rearrangement," and that we have tried in § 68 to put the reader on his guard against this very error of Prof. Burnside.

(2) A second charge relates to infinite products. In § 109 we consider a certain infinite product $\Pi(1 - \alpha_n)$; in regard to this product, Prof. Burnside complains that we have not explained "what is implied in calling such a product convergent." As a matter of fact we treat an infinite product as an instance of an infinite sequence, and convergence for infinite sequences has been already explained in § 47. He falls into another inaccuracy when he says that "if $\Sigma\alpha_n$ is greater than unity, all that has been proved is that $\Pi(1 - \alpha_n)$ is less than unity and greater than some definitive negative quantity." We have proved much more than this, namely that there is a limit for the numbers $\prod_1^n (1 - \alpha_n)$, when n tends to infinity (see § 45).

We did not intend to go into the case where the sequence associated with an infinite product converges to zero, because there is as yet no final agreement as to whether the product is or is not to be called convergent in this case. The product in § 109 does not converge to zero. Prof. Burnside does not allude to this point; but we should like, nevertheless, to take this opportunity of saying that we ought to have added a proof that the convergence of $\Sigma\alpha_n$ excludes this special case, instead of assuming that the reader knows the proof, as given, for instance, in Hobson's "Trigonometry."

(3) The third charge relates to our use of the word "infinity" on p. 3. This word "infinity," in the earlier parts of the higher arithemetic, has but one accepted meaning; to quote the words of M. Tannery, "la notion de l'infini dont il ne faut pas faire mystère en mathématiques se réduit à ceci; après chaque nombre entier il y en a un autre." We have used the word "infinity" in this, its legitimate sense. Failure to perceive the "variable" character of infinity has led to many misconceptions in the past. We cannot understand Prof. Burnside's objection except on the supposition that he has, for the moment, confused this "variable" infinity with the discredited "constant" infinity.

On the score of accuracy we wish to point out that we gave two chapters to elliptic functions, not three, as the reviewer states; and that Log x is not *defined* (the italics are the reviewer's) by means of a piece of string and a cone. We *define* the logarithm by means of an equiangular spiral, in a way somewhat similar to that used in Clifford's "Common Sense of the Exact Sciences," and we indicate, incidentally, a mechanical construction of the curve.

It is always an ungracious task to reply to a review, especially when it is in general appreciative, and written by a mathematician of acknowledged standing; but in the circumstances we felt that we had no alternative. We believe that Prof. Burnside will be the first to recognise that his specific criticisms are based on misconceptions.

J. HARKNESS.
F. MORLEY.

Philadelphia, March 14.

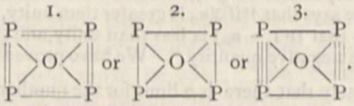
THE criticism on the passage quoted from p. 3 of the book by Profs. Harkness and Morley (NATURE, February 23, p. 347) turns on the fact that, in dealing with number divorced from measurement, the authors have used the phrase "an infinity of objects" without an explicit statement of its meaning. I am not sure that I understand the passage in their letter which refers to this point; but it seems to me to imply that the distinction between "finite" and "infinite" is one which does not require definition. This is not the only accepted view. It is not, for instance, the view taken in Herr Dedekind's book, "Was sind und was sollen die Zahlen." As regards the opening sentences of Chapter xv., the authors have apparently misunderstood the point of my objection. With the usually received definition of convergence of an infinite product, $\Pi(1-\alpha_n)$, if convergent, is different from zero. So far as the passage quoted goes, $\Pi(1-\alpha_n)$ might be zero; and it is therefore not shown to be convergent, if the usual definition of convergence be assumed. As to the passage quoted from p. 232, I must express to the authors my regret for having overlooked the fact that the particular rearrangement, there made use of, has been fully justified in Chapter viii. Whether Log x is or is not, at the beginning of Chapter iv., defined by means of a string and a cone, will be obvious to any one who will read the whole passage (p. 46, line 16, to p. 47, line 9) leading up to the definition.

W. BURNSIDE.

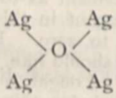
The Tetravalency of Oxygen.

THE discovery of the new and easy method of preparing the pure oxygen phosphide OP_4 (or Le Verrier's phosphorus suboxide) by the acidification of the solution of phosphorus in "alcoholic potash," should draw attention to the remarkable fact that the oxygen atom can be therein symmetrically represented *only* as tetravalent and not divalent, and that it is probably an inorganic, non-carbon, closed-ring molecule:—

1. 2. 3.



Without discussing the valency or "validity," as I have termed it, of phosphorus, my point is that oxygen is here tetravalent. The silver analogue (?) OAg_4, or

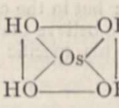


is known. And doubtless others. There ought, therefore, to be no further hesitation in definitely accepting the oxygen atom in the CO molecule as tetravalent, and in proceeding with investigations which shall elucidate, in terms of physical chemistry, the remarkable fact of the "alternating valency" of the non-metallic atoms. Oxygen has as clearly two valence values as phosphorus and nitrogen.

The change in the valency of oxygen seems to be connected with "polymerisation." In 1888 I wrote in the *Philosophical Magazine*, vol. xxv. p. 232:—

"The stimulus—if I may so term it—to polymerisation in these cases seems to be due to the development, in ways that current research are seeking to explain, of the higher valency, or, as it may be preferably termed, the validity of oxygen, or the tervalidity, for instance, of chlorine in "hydrochloric acid solution" used as "a polymeriser," H—Cl = =. It is significant that there are few cases of polymerisation where oxygen is not concerned. . . . Probably more ring formulæ in *in*organic bodies will come to light, and they will affect the maypole hypothesis—as it may be called—of the domination of the central polad atom."

This prediction has, I believe, since been verified, and the benzene-ring style of Kekulé is no longer viewed as exclusively characteristic of carbon. The remarkable volatile osmium compound is probably



where oxygen is tetravalent as it is in Friedel's $(H_3C)_2OClH$.

I would, therefore, venture to suggest that possibly my two papers in the *Phil. Mag.* may still be worth half an hour's attention from gentlemen who would find some curious compounds therein discussed—compounds whose physical chemistry may perhaps now be found worth examination in the light of the newer methods, and also in the light of the developing investigations concerning the ether and the intra-molecular equipoise of the atoms constituting the chemical molecule.

Banbury, March 25. J. F. HEYES.

THE PROGRESS OF WIRELESS TELEGRAPHY.

WE recently referred to the first messages transmitted over the English Channel between the South Foreland and Boulogne by Marconi's system of wireless telegraphy. During last week the English station was visited by Prof. Fleming, who has written his experiences to the *Times*.

An idea of the present state of the problem can be conveniently gathered from this communication. First as to the certainty of the results obtained, Prof. Fleming states:

"Throughout the period of my visit messages, signals, congratulations, and jokes were freely exchanged between the operators sitting on either side of the Channel and automatically printed down in telegraphic code signals on the ordinary paper slip at the rate of twelve to eighteen words a minute. Not once was there the slightest difficulty or delay in obtaining an instant reply to a signal sent. No familiarity with the subject removes the feeling of vague wonder with which one sees a telegraphic instrument merely connected with a length of 150 feet of copper wire run up the side of a flagstaff begin to draw its message out of space, and print down in dot and dash on the paper tape the intelligence ferried across thirty miles of water by the mysterious ether."

Signor Marconi by much work has arrived at great simplicity.

"With the exception of the flagstaff and 150 feet of vertical wire at each end, he can place on a small kitchen table the appliances, costing not more than 100*l*. in all, for communicating across thirty or even one hundred miles of channel. . . . The distance to which effective signalling extends varies as the square of the height of the rod. A wire 20 feet high carries the effective signal one mile, 40 feet high four miles, 80 feet sixteen miles, and so on."

We are very glad to print the following extract from Prof. Fleming's letter:

"The general public are not much concerned with questions of priority or with the claims or suggestions of rival experimentalists, but they are interested in ascertaining the serious possibilities of that which has been actually achieved. Signor Marconi has never hesitated to acknowledge that he has built upon the foundations laid by others, but a vast gulf separates laboratory experiments, however ingenious, from practical large scale demonstrations conducted with all that regularity and freedom from failure which is the absolute condition of their public utility.

"I cannot help thinking that the time has arrived for a little more generous appreciation by his scientific contemporaries of the fact that Signor Marconi has by minute attention to detail, and by the important addition of the long vertical air wire, translated one method of space telegraphy out of the region of uncertain delicate laboratory experiments and placed it on the same footing as regards certainty of action and ease of manipulation, so far as present results show, as any of the other methods of electric communication employing a continuous wire between the two places. This is no small achievement."

There can be no doubt that what this system will do in the future for those who live on coast-lines or go down to the sea in ships is destined to be of great importance. Already the usefulness of light-ships is increased tenfold. The fleet manœuvres of the future may be flagless. Sea routes by means of an international concert pitch may be turned into exchanges—but there is no end of the possibilities thus opened out by this new development of the results of the study of the "useless."

Prof. Fleming, in the course of his long and interesting letter, points out the importance of some new Board of Trade regulations "for the use of the ether"—the term is distinctly good—lest vagrant electric waves should interfere with the official ones. Even the ether, then, may yet be dismissed with costs.

A CHAPTER IN THE HISTORY OF SPECTRUM ANALYSIS.

WHEN I began to endeavour to apply the principles of spectrum analysis to the investigation of the nature of the heavenly bodies in 1865, the then idea, based upon Kirchhoff and Bunsen's work of 1859, was

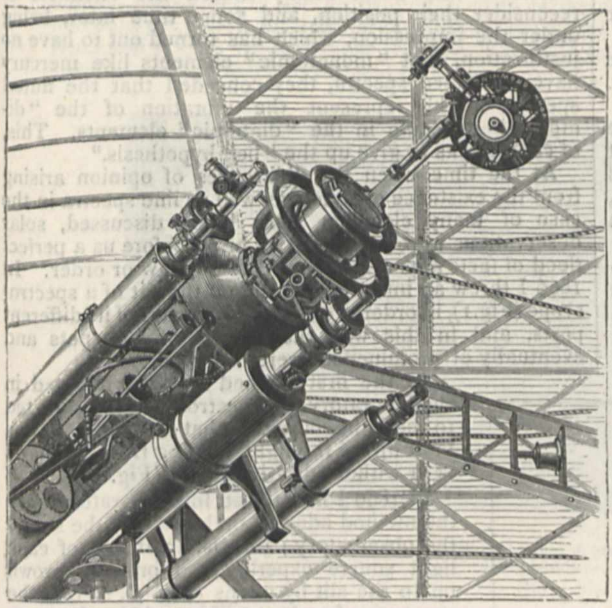


FIG. 1.—Spectroscope attached to a large refractor which throws an image of the sun on the slit plate.

that the spectrum of a chemical element was one and indivisible—that it could not be changed by temperature or by anything else.

Looking back it is easy to see now that this idea largely depended upon the fact that in the early days low flame temperatures were generally employed, and that it so happens that the substances best visible in the flame, and which were therefore chosen to experiment upon, such as sodium, calcium, potassium, and the like, give us line spectra at low stages of heat.

Hence the first spectroscopic ideas entirely agreed with those of the chemist, that the chemical "atom," defined by a certain "atomic" weight was a manufactured article, indivisible, indestructible. Chemical elementary substances were either composed of these atoms, these indivisible units, or of "molecules" consisting of one or two of them, hence the terms "monatomic" and "diatomic" molecule.

The difference between the spectra of the same element in the solid and gaseous states, in which we have first a continuous and secondly a line spectrum, was ascribed to the restricted motion of the atom in the solid and its freedom in the gaseous state—it was a question of "free path." The difference between the states which gave us the continuous and discontinuous spectra was a physical difference having nothing to do with chemistry. According to the kinetic theory of gases, the particles of all bodies are in a state of continual agitation, and the difference between the solid, liquid and gaseous states of matter is that in a solid body the molecule never gets beyond a certain distance from its initial position. The path it describes is often within a very small region of space. Prof. Clifford, in a lecture upon atoms, many years ago illustrated this very clearly. He supposed a body in the middle of a room held by elastic bands to the ceiling and the floor, and in the same manner to each side of the room. Now pull the body from its place; it will vibrate, but always about a mean position; it will not travel bodily out of its place; it will always go back again.

We next come to fluids. Concerning these we read: "In fluids, on the other hand, there is no such restriction to the excursions of a molecule. It is true that the molecule generally can travel but a very small distance before its path is disturbed by an encounter with some other molecule; but after this encounter, there is nothing which determines the molecule rather to return towards the place from whence it came than to push its way into new regions. Hence in fluids the path of a molecule is not confined within a limited region, as in the case of solids, but may penetrate to any part of the space occupied by the fluid.

Now we have the motion of the molecule in the solid and the fluid. How about the movement in a gas? "A gaseous body is supposed to consist of a large number of molecules moving very rapidly." For instance, the molecules of air travel about twenty miles in a minute. "During the greater part of their course these molecules are not acted upon by any sensible force, and therefore move in straight lines with uniform velocity. When two molecules come within a certain distance of each other, a mutual action takes place between them which may be compared to the collision of two billiard balls. Each molecule has its course changed, and starts in a new path.

The collision between two molecules is defined as an "encounter"; the course of a molecule between encounters a "free path." "In ordinary gases the free motion of a molecule takes up much more time than is occupied by an encounter. As the density of the gas increases the free path diminishes."

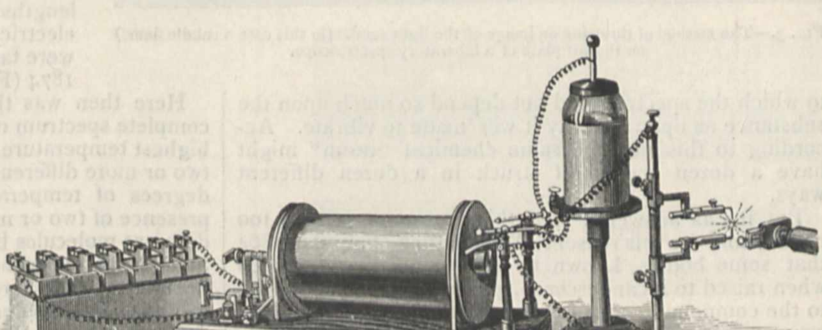


FIG. 2.—The first method of work with the slit of the spectroscope close to the light source. In the experiment illustrated the light source is an electric spark produced by an induction coil with Leyden jar in circuit. The slit end of the collimator is shown to the right.

It will be seen at once that on the view first held that the difference between continuous and discontinuous spectra depended simply upon the solid and gaseous states, no solid could give us a line spectrum; and the well-known absorption spectra of didymium glass and other solid bodies would be impossible.

Another important series of facts was soon brought to the front. Plücker and Hittorf in the year 1865 announced that "there is a certain number of elementary substances which when differently treated furnish two kinds of spectra of quite a different character, not having any line or band in common." The difference in character to which reference is here made consists in the spectrum produced at the lower temperature being composed of flutings, which are replaced by lines when the higher temperature is reached.

This was the first blow aimed at the general view—one element one spectrum—to which I have referred above. It was met in two ways.

Taking the line spectrum as representing the true vibration as the chemical unit, I have already shown that the continuous spectrum was explained as due to its physical environment, the solid or liquid state. This, then, had not to be considered from the chemical point of view.

The fluted spectra were boldly ascribed to "impurities," but not always wisely, for, to get rid of the difficulty presented by the two spectra of hydrogen, two perfectly distinct spectra were ascribed to acetylene. Again the "bell-hypothesis" was suggested, according to which the spectrum did not depend so much upon the substance as upon the way it was made to vibrate. According to this view the same chemical "atom" might have a dozen spectra if struck in a dozen different ways.

But it was answered that this argument proved too much; and for this reason. Mitscherlich showed in 1864 that some bodies known to be chemical compounds when raised to incandescence, give us a spectrum special to the compound; that is, they have a spectrum of their own; no lines of either of the constituents are seen.

I showed later that when the temperature was sufficient to produce decomposition the lines of the elementary bodies, of which the compound was composed, made their appearances according to the temperature employed. And I also showed that precisely the same thing happens with regard to the fluted and line spectra of the same chemical element. We may get the first alone at a low temperature. We may increase the temperature and dim it slightly, some lines making their appearance; and next, by employing a very high temperature, we can abolish the fluted spectrum altogether and obtain one with lines only.

Since then the difference between the two spectra of the same element was no more marked than the difference between the spectrum of a known compound and its constituents after the compound had been broken up by heat; it was as logical to deny the existence of compound bodies as to deny that more molecular complexities than one were involved in spectral phenomena.

Attacks like these finally caused the chemists to reconsider their position, and some time later, being under the impression, which has turned out to have no justification, that "monatomic" elements like mercury have not fluted spectra, they conceded that the fluted spectra might represent the vibration of the "diatomic" molecule in the "diatomic" elements. This, of course, was to give up the "bell-hypothesis."

At the time when the differences of opinion arising from the existence of fluted as well as line spectra in the case of many elements were being discussed, solar observations were beginning to bring before us a perfect flood of facts apparently devoid of any law or order. In 1866 I threw an image of the sun on the slit of a spectroscope (Fig. 1), in order to observe the spectra of its different parts, and in this way the spectra of sun-spots and eventually of prominences were observed.

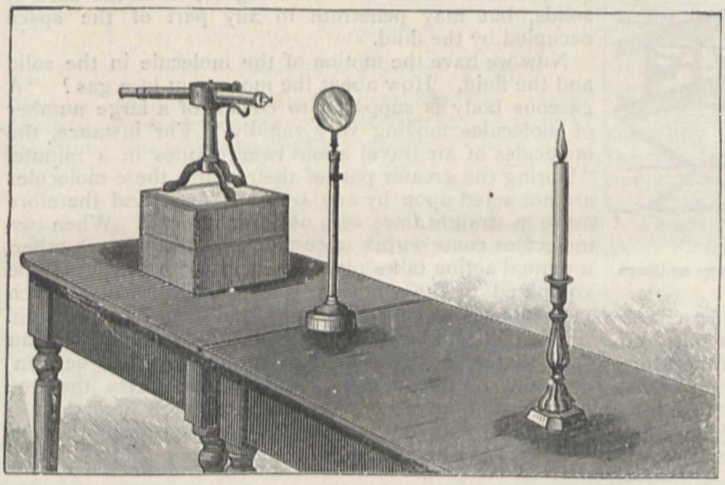


FIG. 3.—The method of throwing an image of the light source (in this case a candle flame) on the slit plate of a laboratory spectroscope.

In the first method of work adopted in the laboratory the spectroscope was directed to the light source, so that the spectrum was built up of the light coming from all parts of it without discrimination (Fig. 2).

In 1869 I introduced into laboratory work the method adopted in the case of the sun in the observatory; that is, an image of each light source experimented on was thrown on to the slit by a lens (Fig. 3), so that the spectrum of each part of it could be observed, and some of the results obtained by the new method were the following:

The spectral lines obtained by using such a light source as the electric arc or spark were of different lengths; some only appeared in the spectrum of the centre of the light source, others extended far into the outer envelopes. This effect was best studied by throwing the image of a horizontal arc or spark on a vertical slit. The lengths of the lines photographed in the electric arc of many metallic elements were tabulated and published in 1873 and 1874 (Figs. 4 and 5).

Here then was the first glimpse of the idea that the complete spectrum of a chemical element obtained at the highest temperature might arise from the summation of two or more different line spectra produced at different degrees of temperature, and therefore bringing us in presence of two or more molecular complexities; that is, different molecules broken up at different temperatures. So soon as experiments in the laboratory had given a definite result with regard to the spectrum of a metal in this way, I proceeded to study the sun with a view of determining how that metal behaved in the sun.

This involved, first, photographs of the solar spectrum with its dark lines, photographic comparisons of these dark lines with the bright lines constituting the spectra of the metallic elements. This enabled us to compare the total light given by each light source with the light received from all parts of the sun indiscriminately.

Next the spectra of different parts of the sun—chromosphere and prominences and spots—were compared with different parts of the light source, the core of the arc, and the centre of the spark, and the outer regions of both.

It will be seen that the inquiry now had a very broad base, and it could be immediately tested in many ways at every stage.

Wonderful anomalies were at once detected, lines known to belong to the same chemical element behaved differently in several ways. Some were limited to spots, others to prominences, and in solar storms different iron lines indicated different velocities. In the spectrum of the hottest part of the sun open to our inquiries, the region namely immediately overlying the photosphere which I named the chromosphere the anomalies became legion; suffice to say that in the hottest part of the sun we could get at, the spectrum of iron then represented in Kirchhoff's map by 460 lines in the ordinary solar spectrum was reduced to three lines.

temperatures than those previously employed were doing for chemistry what previous similar inquiries had done; namely, indicating the existence of finer constituents in matter supposed at each point of time to be elementary.

This was the first glimpse of dissociation in relation to the production of changes in the line spectrum.

By the year 1872 the work of Rutherfurd and Secchi on stellar spectra enabled the base of the inquiry to include the stars as well as the sun. In some of the stars the existence of hydrogen, magnesium and carbon were beyond question. The point that first struck me was that in white stars like α Lyræ and Sirius, with continuous

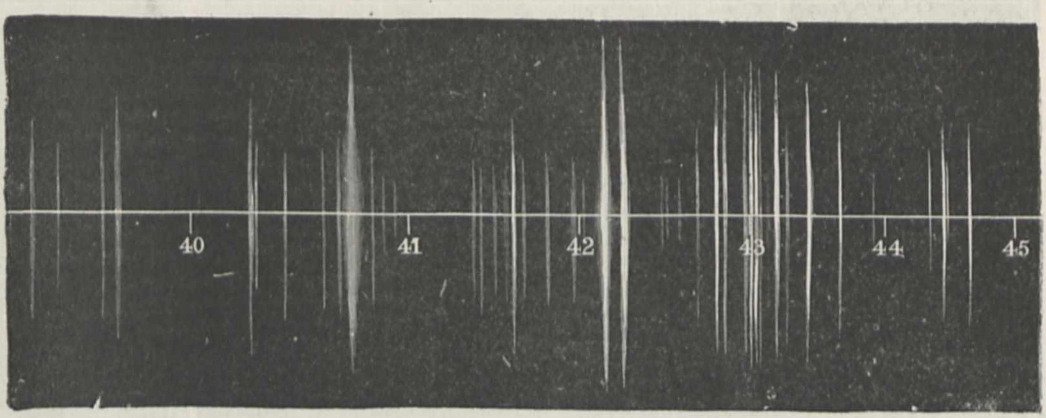


FIG. 4.—The long and short lines. Copy of a photograph taken with a vertical slit when compounds of strontium and calcium were volatilised between horizontal carbon poles.

It was no longer a question merely of settling the difficulties raised by the observations of Plücker and Hittorf.

Many observations and cross references of this kind during the next few years convinced me that the view that each chemical element had only one line spectrum

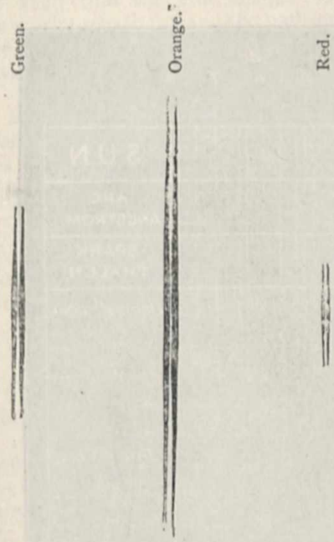


FIG. 5.—The longs and shorts of sodium taken under the same conditions, showing that the orange line extends furthest from the poles.

was erroneous, and that the results obtained suggested that the various terrestrial and solar phenomena were produced by a series of simplifications brought about by each higher temperature employed. That is, that the new instrument, the spectroscope, showed that higher

spectra extending far into the violet—stars therefore hotter than their fellows of a yellow or red colour—we had to do with hydrogen almost alone.

It was in 1873 that I first called the attention of the Royal Society to the very remarkable facts which had even then been brought together regarding the possible action of heat in the sun and stars. Referring more especially to the classification of stars by Rutherfurd, I wrote as follows:[1]

"I have asked myself whether all the above facts cannot be grouped together in a working hypothesis which

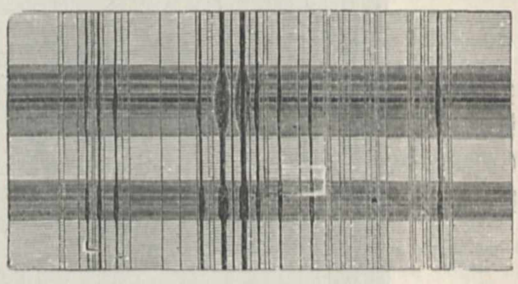


FIG. 6.—Spectrum of a sun-spot as compared with the general spectrum, showing that certain metallic lines (sodium and calcium in this instance) are widened. The darker portion represents the spectrum of the spot.

assumes that in the reversing layers of the sun and stars various degrees of 'celestial dissociation' are at work, which dissociation prevents the coming together of the atoms which, at the temperature of the earth and at all artificial temperatures yet attained here, compose the metals, the metalloids, and compounds."

Subsequently in a private letter to M. Dumas, who took the keenest interest in my solar work, I wrote, "Il semble que plus une étoile est chaude plus son spectre est simple."

[1] *Phil. Trans.*, vol. clxiv. part 2, p. 491.

I also pointed out the close relation of hydrogen to calcium, magnesium, and other metals (it was on this

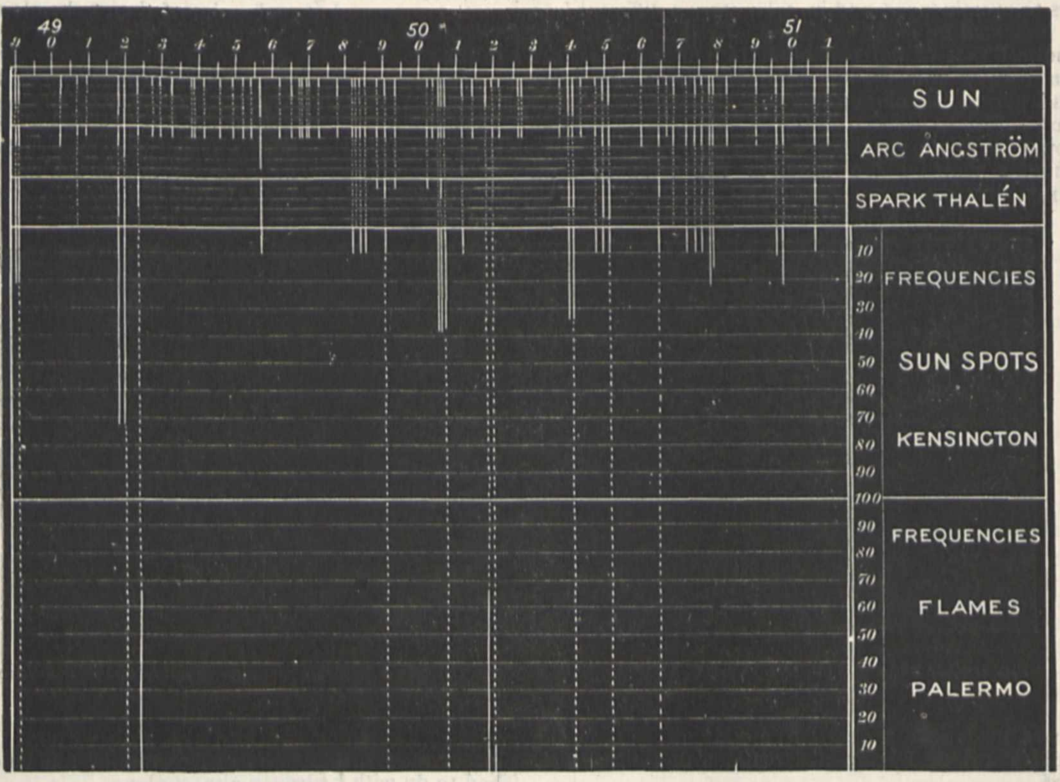


Fig. 7.—Iron spot lines at Kensington confronted with iron prominence lines at Palermo.

ground that I had named the substance which gave D^3, which always varied with hydrogen, helium), and the

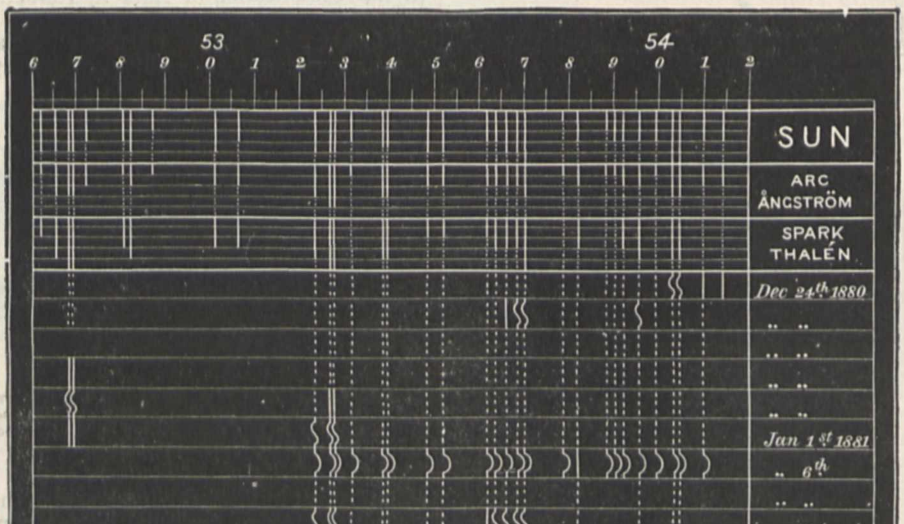


Fig. 8.—Different rates of motion registered by different iron lines.

absence of all other terrestrial gases from the solar spectrum. An interesting discussion at the Paris Academy of Sciences was thus concluded by M. Dumas:

"En résumé, quand je soutenais devant l'Académie que les éléments de Lavoisier devaient être considérés, ainsi qu'il avait établi lui-même, non comme les éléments *absolus* de l'univers, mais comme les éléments *relatifs* de l'expérience humaine ; quand je professais, il y a longtemps, que *l'hydrogène* était plus près des métaux que de toute autre classe de corps ; j'émettais des opinions que les découvertes actuelles viennent confirmer et que je n'ai point à modifier aujourd'hui."[1]

One of the replies to my working hypothesis was that the various chemical elements probably existed in different proportions in the different stars, and that it so happened that in Vega and Sirius one of them, hydrogen, existed practically alone.

In 1878 I went further, and showed that thousands of solar phenomena which had been carefully recorded during the previous years could only be explained by assuming that the changes in the various intensities of lines in the line spectrum itself indicated successive dissociations. I pictured the effect of furnaces of different temperatures, and I wrote as follows :[2]

"It is abundantly clear that if the so-called elements, or, more properly speaking, their finest atoms—those that give us line spectra—are really compounds, the compounds must have been formed at a very high temperature. It is easy to imagine that there may be no superior limit to temperature, and therefore no superior limit beyond which such combinations are possible, because the atoms which have the power of combining together at these transcendental stages of heat do not exist as such, or rather they exist combined with other atoms, like or unlike, at all lower temperatures. Hence association will be a combination of more complex molecules as temperature is reduced, and of dissociation, therefore, with increased temperature, there may be no end."

In 1878 I went back to the study of the changes in the line spectra in relation to the changes observed when known compounds were dissociated, and after discussing certain objections I submitted the conclusion that the known facts with regard to the changes in line spectra "are easily grouped together, and a perfect continuity of phenomena established on the hypothesis of successive dissociations analogous to those observed in the cases of undoubted compounds."[3]

It is thus seen that the conclusions to which my spectroscopic work up to the year 1880 had led me, tended in exactly the same direction as that indicated by more purely chemical inquiries thus referred to by Berthelot in that year :—

"L'étude approfondie des propriétés physiques et chimiques des masses élémentaires, qui constituent nos corps simples actuels, tend chaques jour d'avantage à les assimiler, non à des atomes indivisibles, homogènes et susceptibles d'eprouver seulement des mouvements d'ensemble, . . . il est difficile d'imaginer un mot et une notion plus contraires à l'observation ; mais à des édifices fort complexes, doués d'une architecture spécifique et animés des mouvements intestins très variés."[4]

NORMAN LOCKYER.

1 "Chemistry of the Sun," p. 205.
2 *Proc. Roy. Soc.*, vol. xxviii. p. 169. See also "Chemistry of the Sun," chap. xviii.
3 *Roy. Soc. Proc.*, vol. xxviii. p. 179.
4 *Comptes rendus*, 1880, vol. xc. p. 1512.

DRIFT-BOTTLES AND SURFACE CURRENTS.

THE rather anomalous results arrived at by some recent investigators who have employed the float or bottle method of ascertaining the surface movements of the waters of the sea, make the discussion of a large number of these observations of special value at the present time. Such is to be found in Dr. Schott's able and elaborate paper on the "Flaschenposten" in the possession of the Deutsche Seewarte, published a short time ago in the *Archiv*.

After an historical introduction, in which it appears that the earliest recorded current observation of this kind is about a century old, Dr. Schott describes the material at his disposal, which consisted of about 600 records found up to the end of the year 1896. One important point here brought out is that no consistent difference can be observed in either direction or rate of drift between empty floats and floats loaded so as to ensure complete immersion.

In arranging the records obtained from each of the great oceans, the first place is, of course, given to the North Atlantic, which includes no less than 452, or 70 per cent. of the whole. The North Atlantic records are subdivided into six sections—those from floats set adrift in the North Sea and the English Channel ; in the west wind region north of 30° N. lat. ; in the north-east trade wind region ; those in the south-east trade wind region which were recovered beyond its northern limit ; those in the region of south-west monsoons, and in the Mediterranean. The charts appended to the paper, of which we reproduce a specimen, contain only a selected number of the drift-tracks dealt with ; full details are given in tabular form. In the case of the other oceans, the whole of the observations are represented ; the South Atlantic and the Pacific are each treated as a whole, while the Indian Ocean is divided into the monsoon region, the south-east trade belt, and the "brave west winds."

Summing up in a final paragraph, Dr. Schott concludes that on the whole the method of drift-bottles yields valuable information both as to the direction and speed of surface currents. From this, however, the monsoon regions are expressly excepted : the number of bottles found within the period of one monsoon is necessarily small, and the few found give unsatisfactory results. As specially favourable instances, Dr. Schott quotes his results in the Bay of Biscay, disproving the existence of Rennell's current (no reference is made, by the way, to the work of Hautreux) ; in the West Indies, where the concentration of immense quantities of surface-water from the coast of Portugal and from the South Atlantic is clearly shown ; in the west wind drift of the southern hemisphere, and in the splitting of the southern equatorial current off the east coast of Madagascar. In this connection special stress is rightly laid on the record of two bottles, one loaded with sand and the other not, thrown overboard from the s.s. *Paranagua* in 13° 49' N. lat. and 25° 34' W. long., and picked up together on the island of Santiago (Cape Verd Island) after a journey of 131 miles in twenty-one days, the direction being north-east by east *with* a weak current (whose existence was shown by independent observations recorded in ships' logs), and *against* the wind blowing at the time. In estimating the *speed* of current, the float method is found to be much less valuable, inasmuch as we can rarely be certain that the float is picked up immediateiy after it has reached the spot where it is found. Reasonably accurate estimates can only be looked for where a number of floats gives approximately the same result.

The justness of Dr. Schott's conclusions, so far as they go, seems to admit of little doubt, but we could have wished that his final statement of them, which will probably be much more widely read than the detailed discussion in the body of the memoir, had been expressed in a more guarded manner, and that to it he had added a note of warning, pointing out not only the extremely limited nature of the information afforded by the method, but the great risk of misinterpreting its results. Taking first the question of *direction* of surface currents : on the whole, the surface currents in perfectly open sea, clear of all land influences, follow the direction of the wind, and the float or bottle naturally takes the course common to both. Near land, the direction of the surface current is determined by three factors : first, and most important, the form of the coast line ; second, the prevailing wind ; and third, a gravity factor, due to differences of level caused by off- and on-shore winds, inequalities of density, &c. In this second case the current may, and often does, move in a direction forming a considerable angle with the wind; and the float may in most cases follow the current. That it does so in certain cases Dr. Schott has shown; but nearly every paper on this subject contains "erratics," and this is no exception. Several records are distinctly unsatisfactory; the most flagrant case is, perhaps, that of two identical floats started together in 1° 44′ N. lat. and 27° 16′ W. long., one of which was found on the coast of Nicaragua, and the other on the coast of Sierra Leone. Compare this with Mr. Russell's results on the east coast of Australia. "In view of the well-known southerly current on this coast, it is remarkable that so few of the papers found seem to go with it, and that the majority of the papers found go against the current." Evidently great caution is necessary in applying the method. What seems most likely is that the relation of the movement of the float to that of the wind and of the surface water is really a function of the strength of the wind and the sea disturbance, and of the density of the water. A float may make headway with a current against a light wind; but if it meets with a cyclone, it, and perhaps a skin of water with it, may be removed from the surface of the current, a merely local disturbance transferring it to another member of the oceanic circulation: yet the "record" of this float would show the two currents as a continuous stream.

With regard to deductions as to the speed of a current based on records from floats, we are almost inclined to go further than Dr. Schott, and to regard such as practically valueless. The whole tendency of recent investigation has been to show that steady forward movement of surface water only occurs when there is a distinct "head" of water strongly controlled by the shape of the land; the best examples being the north and south currents moving polewards on the eastern sides of the great land masses. These currents come to an end as soon as they get clear of the land, and their waters are distributed by "drift" currents controlled primarily by the prevailing winds, but subject to continual variation, according to the relative amounts of denser and lighter water supplied by the true currents. But the movements of the drift currents are irregular in the extreme; the water goes backwards and forwards, the horizontal course of any particle of water, perhaps, resembling that of a particle of the air in the winds which drive it. The track of the *Fram* across the Polar Sea is probably a generalised form of such a course. In effect, there is no "river in the ocean," and we are not in general justified in joining the beginning and end of the course of a float by a continuous line. Under these circumstances, one is inclined to suspect that the wind has a considerable share in the high average velocities sometimes obtained for surface drifts from float observations.

Dr. Schott says little or nothing about the precise limitations which are to be observed in the interpretation of the float records, and it seems a pity that here

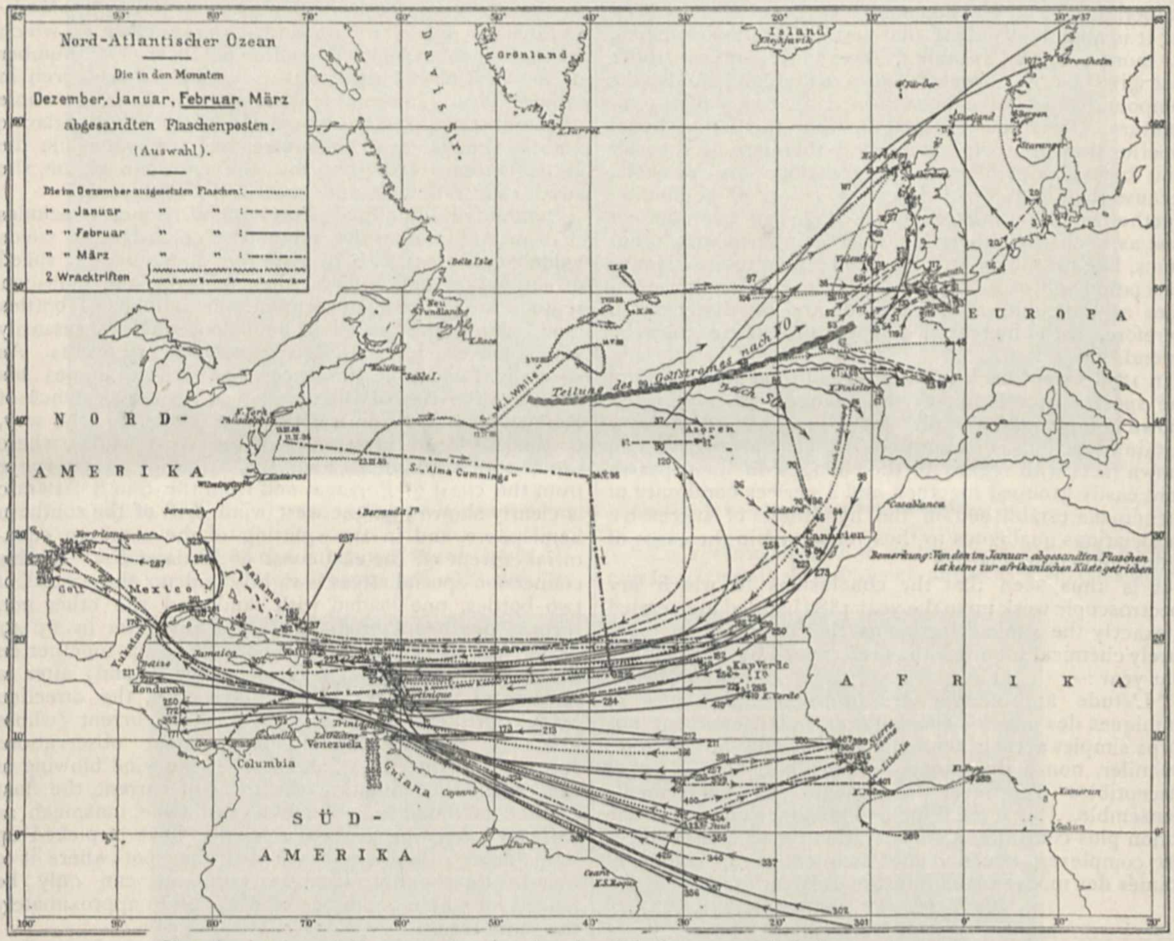


Chart showing the probable courses of a selected number of the floats cast adrift during the winter months.

again there is no note of warning. Assuming, for the moment, that floats give a trustworthy record of the movement of the surface water in which they are immersed, they give little information about the real motion of any body of water in their course. A single example must suffice to illustrate this. Set a float adrift in the Gulf of Mexico, and it is found in the Shetland Islands; we cannot conclude that water has come from the Gulf of Mexico to the Shetlands—the current which brought the float to the banks of Newfoundland may have there become an under-current, and the float cannot follow it; it remains on the surface, and is borne eastward by water which may have come from Davis Strait or Denmark Strait, where no floats are set adrift. No doubt the fact that the float took the course it did is interesting, and with sufficient knowledge of the mechanism, obtained from other sources, it may be of great value; but even in a simple case, like that quoted, the greatest caution must be exercised, and the uncertainty becomes still greater in channels and enclosed seas.

It seems fair, from Dr. Schott's discussion, to conclude that observations by means of floats are likely to contribute valuable additions to our knowledge of the surface movements of the waters of the sea, when taken in conjunction with systematic observations by more precise methods, of which the distribution of temperature and salinity is probably the most satisfactory. Taken by themselves, the float observations are liable to be seriously misleading. H. N. D.

A COMPARATIVE STUDY OF VISUAL ACCOMMODATION.

So recently as five years ago but little was known with certainty regarding the refraction and accommodation in animals with "camera eyes." It is Beer's[1] credit to have made an exhaustive investigation of a large number of animals with the aid of all modern ophthalmic methods, in addition to an experimental method of his own devising, namely, electrical stimulation of the enucleated eye.

No mechanism for accommodation is known in the facetted eyes of crabs and insects; according to Exner it is not required. The retina is comparatively thick, and moving an object from 80 cm. to 1 mm. from the eye causes an alteration of but ·01 mm. in the position of the image.

A similar argument was supposed to hold good for the rabbit's eye, even by such a distinguished physiologist as Magendie; but this was due to an experimental error. Kepler, although ignorant of accommodation, recognised, nevertheless, that the image on the retina must be sharp for visual purposes.

Speaking generally, an eye must be able to accommodate proportionally to (1) its own size, (2) the width of the pupil, (3) the closeness of the retinal mosaic. Whilst, therefore, an emmetropic man can see plainly at 6 m. without exercising his accommodation, those animals—and there are many—with much larger eyes must, for the same acuity of vision, already accommodate at that distance. On the other hand, it is not so important for large animals to have so near a "near-point" as small animals. Small animals, *e.g.* birds, have to see objects clearly even when quite close; they require a large range of accommodation. Larger animals, except monkey and man, who bring things close to the eye with the hand, can get on with a "near-point" of 1–½ m., corresponding to a range of accommodation of 1–2 diopters; this is so with the horse and ruminants.

Three modes of accommodation are possible: (1) alteration in the refractive power of the various media; (2) alteration of the curvature of the refracting surfaces; (3) alteration of the relative position of lens and retina.

This first method, by which Grimm in 1785 sought to explain accommodation, has never been actually observed. The last was formerly a favourite theory. It was taken up by Kepler and by Scheiner. Many absurd reasons were given, and mechanical hypotheses constructed by various individuals to support this theory; nobody thought of examining the actual facts. In certain classes of animals, viz. cephalopoda, fishes, amphibia and reptiles, accommodation is effected by alteration of the distance between lens and retina. In man, however, as in all mammalia, also in birds, lizards and tortoises, this is brought about by alteration in the curvature of the lens.

It is also obvious that active accommodation may be for a near (positive) or for a far (negative) point, viz. the resting eye may be adjusted for distance or proximity respectively.

The dibranchiate cephalopoda are the only invertebrates in which accommodation has been observed; and, although they have "camera eyes," their type, as a whole, is far inferior to that of vertebrates. By retinoscopy it was determined that their eye is normally adjusted for the near-point. The extent of this normal myopia varies between two and (as an extreme limit) ten diopters. The mechanism of accommodation is as follows:—The bulb forms half of a rough ellipsoid. At the equator is a flat, strong cartilaginous ring, separating the anterior flattened portion of the eye from the posterior ellipsoidal part. Behind the ring the sclerotic is comparatively soft and yielding. In the anterior wall of the eye is also a strong muscular ring with radial fibres running from the cartilaginous ring to the ciliary body, which is firmly attached in the equatorial region of the lens. When this muscle contracts it pulls back the whole anterior wall of the eye, including the ciliary body and lens, towards the interior of the eye. The resulting increased pressure would tend to make the bulb of more spherical shape, with consequent increase of distance between lens and retina, but the thinner consistency of the posterior half leads to an actual diminution of the antero-posterior diameter, and consequent approximation of retina to lens. The iris, which lies practically outside the bulb, and also serves as lid, although extremely sensitive to light, takes no part in accommodation.

Teleostean fishes, although the structure of their eye bears a superficial resemblance to that of the cephalopod eye by virtue of the spherical form of the lens, but is in other respects vertebrate in structure, are, nevertheless, myopic and accommodate for distance. The thickness of the retina, with its sensitive layer on the outer side (in the cephalopoda it is on the inner side), introduces a factor which has to be taken into account when estimating the refraction. Thus an apparently hypermetropic eye may prove to be really myopic. The comparative opacity of water, which does not allow of distinct vision for any great distance, accounts for their permanent myopia, but their range of accommodation is sufficient to enable fishes to focus parallel rays on the retina. But since their cornea is not, as was formerly supposed, flattened, when taken out of water they are so highly myopic that the correction which they possess would be of no value or account. The mechanism of the adjustment is different to that of the cephalopoda, and quite different to that of the vertebrata.

Ciliary body, ciliary muscle, zonula, spaces of Fontana do not exist in fish-eyes, and the iris does not glide on the lens, but is generally quite free. The spherical lens is suspended from above by a strong triangular band; below is the structure known as the "companula," which, from its function, Beer re-names the "retractor lentis"; it draws back the lens nearer to the retina during accommodation. This can be seen on electrical stimulation of the recently enucleated bulb, even after removal of the cornea.

[1] "The Accommodation of the Eye in the Animal Kingdom." By Th. Beer, Lecturer on Comparative Physiology in the University of Vienna.

Although the muscle is unstriped, it acts quicker than the iris. Accommodation is paralysed by atropin in fishes, but not in cephalopoda.

Even deep-sea fishes, living at a depth where the light does not affect a photographic plate exposed for a whole day, possess an accommodating apparatus.

Amphibians that accommodate, usually do so for near objects; not, however, like most other animals, by altering the curvature of the lens, but by increasing the distance between retina and lens through contraction of the ciliary muscle. This muscle is unstriped, and the rate of accommodation is slow.

Accommodation is altogether absent in frogs. In these, too, the curvature of the cornea is so great, that in water the hypermetropia, for which no correction exists, is as high as twenty-five diopters. Toads, which can more easily than frogs catch worms and near still objects, possess a slight power of accommodation.

Serpents accommodate in a similar manner, but quicker, because instead of a ciliary muscle, which is generally absent, they possess a striped circular muscle inserted in the attachment of the iris. Contraction thereof presses forward the lens, without altering its curvature, towards the cornea. This mechanism of accommodation increases the pressure in the posterior chamber, and is consequently destroyed by opening the latter.

Accommodation is in all such cases effected by *relaxation*, as originally asserted by Helmholtz, not by increased tension, of the suspensory structures of the lens. By this action it becomes more spherical and of shorter focus. This change can be seen to have taken place in some animals even after removal of the supporting ligaments.

Of Amphibians many have a range of accommodation from ten to fifteen diopters. In animals of nocturnal habits accommodation is least developed. It is in birds that it reaches its highest perfection. Crampton's muscle is here the chief factor in producing relaxation, the inner layer of the cornea being pulled *back*, and not the parts behind the ciliary region forward, as in mammals, where the choroid is pulled forward.

It has been supposed that tightening the zonula caused increased pressure in the vitreous, and thereby increased the curvature of the front surface of the lens. Since, however, electrical stimulation of the enucleated cat- or monkey-eye produces nearly identical results, whether the bulb be intact or cut into (in the latter case there can be no increased pressure in the vitreous), this theory can hardly be correct.

Man has the greatest range of accommodation. After him come monkeys (up to ten diopters), then cats and other beasts of prey, and seals. Larger-eyed animals accommodate at a greater distance, but through a lesser range than man. Horses can also accommodate. But many animals, *e.g.* dog, use their noses rather than their eyes to examine near objects.

The anatomical investigations of Hess and Heine have confirmed most of these ophthalmosopic and experimental results.

Thus the two historical inventions of Helmholtz, the ophthalmometer and the ophthalmoscope, are invaluable not only to ophthalmology, but also to comparative physiology.

NOTES.

THE first of the two annual soirées of the Royal Society will be held on Wednesday, May 3. This is the soirée to which gentlemen only are invited.

THE Deputy-Master of the Trinity House, with a committee of Elder Brethren, accompanied by Lord Rayleigh, their scientific adviser, and Captain the Hon. F. C. P. Vereker, of the Board of Trade, arranged to leave London yesterday for Dover, with the object of making an official inspection of the wireless telegraphy system as experimentally in operation between the South Foreland lighthouse and the East Goodwin light vessel.

REUTER'S correspondent at Buenos Ayres reports that despatches received from Punta Arenas, Patagonia, announce the arrival there of the *Belgica* with the members of the Belgian Antarctic Expedition, under the command of Lieutenant de Gerlache. The *Belgica* is said to have remained fast in the ice for two months.

LORD RAYLEIGH and Prof. G. H. Darwin have been elected honorary members of the New York Academy of Sciences.

DR. T. J. J. SEE, who was recently appointed professor of mathematics in the U.S. Naval Observatory, has been nominated as chief of the American Nautical Almanac Office.

PROF. MILNE-EDWARDS, director of the Paris Natural History Museum, and president of the Geographical Society, has received the Grand Cross of the Swedish Order of the Polar Star from King Oscar II.

MR. J. STANLEY GARDINER, Balfour student of the University of Cambridge, and Mr. L. Borradaile have gone to the Island of Minikoi, situated between the Maldive and Laccadive Islands, to study the formation of coral reefs with special reference to the depth at which the reef-building coral organisms live, the food of the coral polyps, the influence of currents upon coral formations and upon the distribution of life near them, and the inter-relationship existing between the various organisms which occur on a coral reef. It is also proposed to survey the Maldive Islands with a view to obtaining information as to their mode of formation. Mr. C. F. Cooper will join the expedition during the summer.

SIR JOHN LUBBOCK, in a letter to Monday's *Times* on the Government Telephone Bill, draws attention to a point that is often lost sight of. He points out that if the telephone becomes national property, not only will the State lose money, but "the results as regards the progress of applied science will, in my judgment, be even more disastrous. Those who have hitherto devoted thought and time, energy and capital, to apply the results of scientific discovery to practical purposes are now told, that while, of course, if their enterprise does not pay, they must bear the loss, on the other hand, if it succeeds, Government will pass an Act of Parliament to deprive them of any advantage." In support of this opinion he quotes the late Mr. Varley as follows :—"The introduction of protectionism in so important an industry as telegraphy has given the postal executive a grip hold of applied electricity, and has enabled them to crush practically out of existence pioneers in telegraphy and applied electricity. English telegraph enterprise no longer exists, and America, which twenty years ago was electrically in the rear of this country, is now England's teacher. At the present time not only does she take premier rank in dynamo-electric developments, but practically all the telegraphic advances which have been made since the passage of the Telegraph Act have originated from American genius." . . . "The sole object I have in view in writing is to bring home to the British public, if I can, the evil consequences of the un-English retrograde policy of converting applied science into a Government trading monopoly." This point should engage the attention of the Society of Telegraph Engineers.

A FEW facts with regard to irrigation in India were mentioned by Lord Curzon on Monday, in replying to an address of welcome from the people of Lyallpur, a new town founded as a result of the Chenab irrigation scheme. The Viceroy pointed out that four years ago Lyallpur, now a flourishing township and mart for agricultural produce, was a barren, uninhabited jungle. In six years 1,000,000 acres have been brought under cultivation at a capital outlay of 1,500,000*l*., while the net revenue for the last year was 16 lakhs of rupees, or a return of nearly 7½ per cent. of the capital expended. It is now estimated that the total value of the crops in a single year equals the capital cost of the entire works. At the present time in the Punjab alone 9500 miles of main and branch canals have been constructed, not including 10,500 miles of small distributaries. The total area irrigated by these means, which in 1868 amounted only to 1,000,000 acres, in 1878 to 1,300,000 acres, and in 1888 to 2,300,000 acres, has risen, owing to the startling progress of the last decade, to 5,200,000 acres. These figures form a striking testimony to British philanthropy, organising power, and engineering skill.

THE sixty-seventh annual meeting of the British Medical Association will be held at Portsmouth on August 1-4. The present President is Sir Thomas Grainger Stewart; and the President-elect is Dr. John Ward Cousins. An address in medicine will be delivered by Sir Richard Douglas Powell, Bart.; and an address in surgery will be delivered by Prof. Alexander Ogston. The scientific business of the meeting will be conducted in twelve sections—namely medicine, surgery, obstetrics and gynæcology, State medicine, psychology, anatomy and physiology, pathology, ophthalmology, diseases of children, pharmacology and therapeutics, laryngology and otology, and tropical diseases.

THE Liverpool Marine Biology Committee have arranged their usual Easter party for dredging and other zoological work at the Port Erin Biological Station. The station will be full of workers during not only the present week, but throughout April. In addition to members of the committee there are students from Liverpool, Manchester, Cardiff, Newnham and other colleges. The Lancashire Sea Fisheries steamer is at Port Erin, and several days will be spent in trawling and tow-netting in the deep water between the Isle of Man and Ireland. A long hose-pipe and pump will be used for obtaining plankton from the bottom waters, and a closing tow-net will also be tried. Another section of the work consists in the collection of fish spawn for the Lancashire hatchery. The hatching boxes at present contain over four million developing embryos of plaice and cod.

IN view of the visit of the British Association to Glasgow in 1901, an effort is being made to draw up complete lists of the fauna, flora, and geological features of the Clyde district. A natural history sub-committee has been formed, the Convener being Prof. John Young; Vice-Convener, Prof. Malcolm Laurie; and Secretary, Rev. G. A. Frank Knight, Almanarre, Garelochhead. A leaflet, showing the scheme of work that has been sketched out, has been prepared with the hope of obtaining help from various quarters to assist the different compilers in their labours. Information is specially desired as to (1) distribution of species; (2) papers in magazines, journals, and transactions of societies, which might otherwise be overlooked; and (3) names of workers in the different departments who might be willing to assist. The scope of the inquiry, as arranged by the committee, is "the natural drainage area of the Clyde, and of all the sea lochs which form extensions of its estuary." The northern limit, therefore, is the watershed beyond the head of Loch Fyne, and the southern boundary has been defined as a line drawn between the Mull of Cantire and the most southerly point of Ayrshire. Further information can be obtained from the Secretary.

WE learn that some recognition will shortly be made of the invaluable services rendered to geological science by the Rev. Thomas Wiltshire, Professor Emeritus of Geology in King's College, London. Of late years Mr. Wiltshire's labours have not been of a nature to bring his name prominently before the public, but he has been toiling quietly as the honorary secretary and editor of the Palæontographical Society. That Society has now published fifty-two quarto annual volumes, and some thirty of these have, we believe, been edited by Mr. Wiltshire. These volumes each contain forty or fifty plates of fossils, and two hundred or more pages of letter-press, dealing with organic remains of all classes. The interest attaching to these volumes is world-wide, and so is their reputation. Immense credit is undoubtedly due to Mr. Wiltshire, and it is pleasing to learn that the members of the Palæontographical Society (of whom Dr. Henry Woodward, F.R.S., is president, and Mr. R. Etheridge, F.R.S., treasurer) have decided to present him with a testimonial, towards which subscriptions (not limited to members of the Society) are now being received.

A FEW particulars as to the progress which is being made by the Royal Commission on Sewage Disposal are given in the *Lancet*. It is stated that, in addition to hearing evidence and visiting a number of sewage works, the Commission have been engaged in determining a number of important questions relating to the desirability or not of laying down chemical and bacteriological standards which should be obtained by effluents, whether in the case of domestic sewage only or of such sewage combined with trade refuse. For this purpose they have employed experts of their own, and it is understood that the staff of chemists and bacteriologists has just been increased, so that the effluents from works of different character can be systematically studied, almost hourly by day and by night, under varying conditions of temperature and rainfall. No statement can as yet be made as to the term over which these experiments must extend; but it is quite clear that they are at present only in an initial stage, and that, in so far as bacteriological results are concerned, the Commission are dealing with a subject as to which little expert evidence is available, and that the matter will have to be examined very deliberately and exhaustively before useful inferences can be drawn. These experiments are being carried out under the supervision of a committee of the Royal Commission, consisting of Sir Richard Thorne, F.R.S., Prof. Michael Foster, F.R.S., and Prof. Ramsay, F.R.S.

A PAPER, by Mr. W. C. Peckham, in the April number of the *Century*, on the liquefaction of gases in general, and the work of Prof. Dewar and Mr. Charles E. Tripler, of New York, in particular, contains some remarkable pictures of experiments with liquid air. The method used by Mr. Tripler to liquefy air is the same as that employed by Dr. Linde and Dr. Hampson. Air is compressed to between two thousand and three thousand pounds per square inch, and cooled by water flowing round the pipes containing it. As it escapes it expands, and is therefore cooled, and this colder air is made to pass around the pipes so as to reduce the temperature of the air in them. The result of this self-intensification is a continual reduction of temperature within the pipes until the temperature of liquefaction, $-312°$ Fahr., is reached. A laboratory form of this apparatus produces from thirty to forty gallons of liquid air in ten hours. In fifteen minutes after the engine is started liquid air can be drawn off. A number of experiments, many of which exemplify results obtained by Prof. Dewar, are described and illustrated in the article. One of the most striking experiments is performed by placing over a cool fire a tea-kettle containing some liquid air. "The heat of the fire evaporates the liquid, and a stream of vapour of air shoots out of the spout to a great height. It looks like steam from a kettle of boiling water. In a very short time water poured into the kettle may be taken out as ice, and the bottom of the kettle is found to be coated with solid carbonic acid frozen from the fire, which glows intensely a hand's breadth away. Yet liquid air will boil with apparently the same violence if set upon a cake of ice." Popular knowledge of the phenomena of liquid air in America is due almost entirely to Mr. Tripler, whose experiments with gallons of liquefied air have excited considerable interest.

THE remarkable discoveries, made in the first place by Japanese botanists, respecting the mode of fertilisation in Gymnosperms, have been followed up by two further papers published in the *Journal of the College of Science* of Tokyo (vol. xii., parts 2 and 3). Prof. Hirasé gives (in French) a further contribution to our knowledge of the impregnation and biology of *Gingko biloba* (*Salisburia adiantifolia*); and Prof. Ikeno (in German) a further account of the development of the sexual organs and the process of impregnation in *Cycas revoluta*. The mature pollen-grain of *Gingko* consists of three cells of unequal size; the largest is the vegetative cell; a small intermediate cell is the antheridial cell; the smallest exterior cell is inactive. From the largest of these cells is produced the pollen-tube, which branches and spreads over the surface of the nucellus. The intermediate cell divides into a body-cell and a stalk-cell; the contents of the body-cell again divide into two antherozoids, their formation being accompanied by the appearance of attraction-spheres. In the formation of the archegone of *Cycas* three periods may be distinguished—the "primordium" (*Anlage*) period, the period of growth, and that of maturity. The pollen-grain consists of two small prothallium cells and a large embryonal cell. Shortly after pollination the pollen-grain produces a tube. The inner prothallium-cell divides into a body-cell and a stalk-cell: in the former appear two centrosomes. Shortly before impregnation the nucleus of the body-cell, or spermatogenous cell, divides into two nuclei, and the cell itself into two spermatids. The nucleus of each spermatid forms an antherozoid, with a nucleus, and a tail composed of cytoplasm. In the nucleus of the oosphere is a crater-like cavity, which the antherozoid enters, in order to fuse with the nucleus of the oosphere.

FROM a paper by Mr. E. S. Salmon on the genus *Fissidens* in the *Annals of Botany*, we learn that the following is the geographical distribution of this genus of mosses, the first number in each case being the total number of species, and the second the number of endemic species:—Europe, 32, 13; Asia, 92, 84; Africa, 159, 140; North America, 74, 49; South America, 118, 106; Pacific, 60, 50.

WE learn from the Allahabad *Pioneer Mail* that Dr. Stein, the learned Principal of the Oriental College at Lahore, has published, through the Punjab Government Press, a detailed report of the results of his examination of the archæological remains in Buner. Dr. Stein accompanied Sir Bindon Blood's force in the expedition to the Buner country in December 1897, and had a unique opportunity of investigating the ruins, rock sculptures, and inscriptions of a portion of the ancient Udyana which had previously been inaccessible to scholars. The monograph is certain to be read with great interest by archæologists both in India and in Europe.

SOME interesting statistics with reference to the seal and whale fishery in 1898 are given by Mr. Thomas Southwell in the *Zoologist* for March. The total number of seals taken by the fleet of eighteen steamers, of the aggregate capacity of 5595 tons, and manned by 3802 seamen, which left St. John's, Newfoundland, in March 1898, for the Gulf fishery grounds, was 241,708, of a net value of about 80,000*l*., as compared with 126,628, valued at 32,564*l*., in the previous season. In addition to these, about 30,000 seals were taken by the sailing vessels and by the shore fishermen. Mr. Southwell states that the seal fishery in the Greenland seas, so far as the Dundee vessels are concerned, has practically become a thing of the past, and, such as it is, has almost drifted into the hands of the Norwegian vessels. Right whales were extremely scarce during the season; the absence of the whales from Greenland seas being attributed to fine weather and light ice. The bottle-nose whale fishery, which was once so productive, is now quite discarded by the British vessels. The total catch of the Dundee fleet in 1898 was 6 right whales, 984 white whales, 591 walrus, 779 seals, and 80 bears, yielding 297 tons of oil and 112 cwts. of bone.

THE United States Department of Agriculture has recently been devoting a good deal of attention to dietary studies amongst the poor in different parts of the country, and the Office of Experimental Stations has already issued quite a number of different bulletins on this subject. The most recent addition to the series is one on dietary studies in Chicago in 1895 and 1896. To obtain satisfactory statistics on this subject is by no means easy, for the data sought include the character, amount, and cost of food consumed during a given length of time, the age, sex, and occupation of the different members of the various families selected, the number of meals taken by each person, and, as far as possible, the financial and hygienic conditions of the family in question. To facilitate the conduct of an inquiry of so personal a nature, two ladies from the Hull Settlement, which is situated in a densely-populated district of Chicago, were asked, on account of their intimate acquaintance with the neighbourhood and inhabitants, to collect the information required. Families of Italians, French Canadians, Russian Jews (both orthodox and unorthodox), and Bohemians were chosen; whilst for comparison three American families were also included: in all, about fifty studies were carried through more or less satisfactorily. National prejudices and idiosyncrasies render living for foreigners more expensive in many instances than for natives, as, for example, is the case with Italians, who cannot be persuaded to exchange the imported oil, wine, and cheese for the far less costly food to be obtained in American cities. The result of the inquiry emphasises the need for education among the poorer classes, both in household management and cookery, and also in the selection of foods. Taste and cost are usually the only considerations to which attention is paid; the fact that foods vary greatly in nutritive value is rarely, if ever, recognised. In many cases families could obtain a more nutritious diet for the sum expended, or an equally nutritious diet for a smaller sum.

WE have received from Profs. A. Riccò and G. Saya a copy of the results of hourly simultaneous meteorological observations made at four stations between the Observatory at Catania (altitude 65 metres) and the Etna Observatory (altitude 2947 metres): the heights of the intermediate stations being respectively 705 and 1886 metres. The observations were only made on July 26–28, 1897, and March 27 and 28, 1898, so that we shall merely refer briefly to one or two of the general results. In July the diminution of temperature, with respect to altitude, was less between the two lower stations than between the upper ones; while in March the diminution was somewhat less, and much more uniform. In July the relative humidity decreased between the first and third stations, but increased between the third and fourth. In March there was scarcely any decrease between the first and second stations, and a considerable increase between the second and fourth stations. The same authors have sent us a copy of the results of observations of air and sea temperature, and of the colour of the water, in the Adriatic and Ionian Seas. The maximum temperatures, especially of the surface water, are nearly equal in the two seas, but the minima of the Adriatic are much lower than those of the Ionian Sea. These results are based upon observations made during one year.

MR. J. BAXENDELL has sent us a copy of the report of the Fernley Observatory, Southport, for the year 1898. The observatory is maintained by the corporation of that town, and is one of the best equipped in the United Kingdom. Observations have been regularly taken at Southport for nearly thirty years, having been commenced by the late Mr. J. Baxendell (father of the present observer), who was a meteorologist of considerable repute. The results, it is almost unnecessary to say, show evidence of very careful work, and include, in addition to the usual elements, observations of sea-surface and underground temperature, evaporation, ozone, &c., and a useful table, containing comparisons with other health resorts, is appended to the report. All such stations vie with each other in giving good reports of bright sunshine. Southport possesses both the Campbell-Stokes burning recorder and the Jordan photographic recorder, and we are glad to see that the records of the latter are "fixed" before being measured. Possibly this is not done at all stations; when measured before "fixing" the results are somewhat exaggerated, as compared with those of the burning recorder. We should scarcely have expected the sunshine at Guernsey (where a photographic recorder is used) to have been greater than at Jersey.

WE have received together the 13th and 14th reports of the State Entomologist of the State of New York for 1897 and 1898, on "Injurious and other Insects." The report for 1897, which was edited by the late Dr. J. A. Lintner, calls attention to the decline in the spread of the formidable San José scale, and deals briefly with a large number of insects, some few being noticed at greater length. The two plates represent *Tenthredo rufopectus*, Norton, and three species of *Uroceridae*, and the spines of the larva of *Eacles imperialis*, Drury (after Packard). The report for 1898 is edited by Dr. Ephraim Porter Felt, Acting State Entomologist. During that year much damage was caused to trees in America by the attacks of various caterpillars. The general character of this report is similar to that of the last; both are illustrated with woodcuts in addition to the plates, and both contain useful bibliographical notices relating to various insects discussed. The report for 1898 contains nine plates, several of which are devoted to illustrations of trees defoliated by caterpillars to an extent fortunately not often seen in Europe. The other plates illustrate various moths, caterpillars, *Coccidae*, and the cast skin of a *Tarantula*.

THE seventh volume of the *Annalen der Sternwarte in Leiden*, published by the director of the observatory, Prof. H. G. van de Sande Bakhuyzen, contains four contributions, three of which, as we are told, have been previously distributed among some observatories. The first of these, by the director himself, is devoted to investigations on the period of rotation of the planet Mars, and to variations of his spots. The author has made use of all the available data, and finds that in Schrœter's and Herschel's time a very dark spot, similar in form to that of Syrtis Major, but fifty to fifty-five degrees distant from the latter, was situated in the region of Cyclopium. This, he says, indicates large variations in this part of Mars' surface. He is also led to adopt new values for the time of transit of the zero of longitude and the period of rotation, this value for the latter being 24h. 27m. 22·66s., with a mean error of ± 0·0132s. The second paper, also by himself, gives an account of an apparatus he has devised for determining the absolute personal equation of an observer in making transit observations. The chief part of the apparatus consists of a movable and fixed prism, the velocity of the former being known. By a simple arrangement of mirrors, the artificial star can be viewed in the transit instrument when the latter is set at any altitude. Prof. J. C. Kapteyn is the author of the third memoir, which deals with the determination of the parallaxes with the meridian circle; while the last paper contains the determination of the difference of longitude between Leyden and Greenwich, by Prof. H. G. van de Sande Bakhuyzen and his brother, Dr. E. F. van de Sande Bakhuyzen; the final value adopted for the difference of longitude being given as 17m. 55·891s.

MESSRS. DULAU AND CO. have issued a catalogue of books and papers on astronomy and meteorology offered for sale by them.

THE twenty-ninth volume of the *Proceedings* of the London Mathematical Society has just been published in two parts. The volume contains papers read before the Society from November 1897 to November 1898; but as brief abstracts of these papers have appeared in our reports of Societies, it is unnecessary to do more now than announce their publication in a complete form.

THE fifteenth edition of Kirkes' well-known "Handbook of Physiology," by Prof. W. D. Halliburton, F.R.S., has just been published by Mr. John Murray. The results of important researches in all branches of physiology have been incorporated, and the order in which the subjects are treated has been modified, the section on the central nervous system being now placed towards the end of the book. The additions and alterations will increase the value of the volume to teachers and students.—A second edition of "Chemistry for Photographers," by Mr. C. F. Townsend, has been published by Messrs. Dawbarn and Ward, Ltd.

THE additions to the Zoological Society's Gardens during the past week include a Bay-thighed Monkey (*Cercopithecus ignatus*, ♂), a Green Monkey (*Cercopithecus callitrichus*) from Liberia, presented by Mr. J. F. Braham; a Lesser White-nosed Monkey (*Cercopithecus petaurista*) from West Africa, presented by Captain F. E. Bishop; a Cape Jumping Hare (*Pedetes caffer*) from South Africa, presented by Mr. W. Champion; a Vulpine Phalanger (*Trichosurus vulpecula*) from Australia, presented by Mr. S. Humble; a Pel's Owl (*Scotopelia peli*) from West Africa, presented by Lieut. E. V. Turner, R.E.; two Ravens (*Corvus corax*), European, presented by Mr. Francis Walpole; a Giraffe (*Giraffa camelopardalis*, ♂) from South-east Africa, two Elands (*Orias canna*, ♂ ♀) from the Transvaal, a Bless-bok (*Damaliscus albifrons*, ♂) from South Africa, deposited; a Red-faced Ouakari (*Ouacaria rubicunda*, ♀) from the Upper Amazons, a Naked-throated Bell-bird (*Chasmorhynchus nudicollis*), three Blue-bearded Jays (*Cyanocorax cyanopogan*) from Brazil, purchased.

OUR ASTRONOMICAL COLUMN.

TUTTLE'S COMET (1899 *b*).—

Ephemeris for 12*h*. *Berlin M.T.*

1899.	R.A. h. m. s.	Decl. ° ′ ″	Br
April 6	3 0 50·6	+26 25 6	0·96
8	8 21·0	25 47 8	1·02
10	15 49·8	25 7 48	
12	23 16·7	24 27 7	1·07
14	30 41·4	23 45 6	
16	38 3·9	23 1 44	1·12
18	45 24·2	22 17 1	
20	52 42·1	21 31 0	1·18
22	3 59 57·3	20 43 43	
24	4 7 10·0	19 55 12	1·23
26	14 20·2	19 5 30	
28	21 28·0	18 14 38	1·28
30	4 28 33·2	+17 22 40	

The comet is increasing in brightness, and is moving in a south-easterly direction through Aries and Taurus. On the 17th it passes close to the Pleiades, and on the 30th it is close to α Tauri (Aldebaran). A telegram from Herr Wolf, of Heidelberg, states that the comet is about the 11th magnitude (*Ast. Nach.*, Bd. 148, No. 3552).

Meteor Observations from a Balloon.—The March number of the *Bull. de la Soc. Astr. de France* contains an article by M. A. Hansky, of the Meudon Observatory, describing his attempt to observe the Leonid meteor shower in November 1898 from a balloon. The experiment was so far successful in that the observers saw a number of meteors, the sky to them being perfectly clear, although to the people on *terra firma* in Paris the sky was completely clouded over. This was at a height of only about 150 metres. Attention is also drawn to the other astronomical observations which might be made in this way when otherwise impossible on account of local weather conditions. Among these may be mentioned the observation of solar and lunar eclipses, the zodiacal light, the gegenschein, and auroræ, which last are probably very frequent but masked by atmospheric glare or insufficient transparency of the air at low levels.

The first balloon ascent for astronomical purposes, so far as is known, was made by the Russian chemist Prof. Mendeléef, who, by this means, was enabled to observe the total eclipse of the sun in 1867.

Planetary Perturbations.—In *Ast. Nach.*, Bd. 148, No. 3549, Prof. S. Newcomb draws attention to a source of error in the formulæ used in computing the positions of the planets at future or past epochs. In the method of "special perturbations," which is often used, a small error in the decimal places, arising from superfluous numbers or uncertainty in the data is, from the nature of the integrals, accumulative with the *time*. Prof. Newcomb takes integrals of the various types commonly used, and shows the relative errors introduced in their evaluation. The mean longitude, requiring two integrations for its determination, will have a more rapidly accumulating error than the other elements, and hence its computation should be done with special care.

U.S. Naval Observatory.—We have just received the report of the superintendent of the Naval Observatory for the fiscal year ending June 30, 1898, embodying the several reports made by the directors of the various departments.

The 26-inch refractor has almost exclusively been used on faint and difficult objects, including observations of D'Arrest's and Giacobini's comets; measures of Titan and Japetus for a new determination of the mass of Saturn (1 : 3491·8), this appearing to be affected by the different brightness of the two, so new measures of Rhea and Japetus are to be made; forty-one complete measures of position angle and distance of Neptune's satellite were obtained, and the diameters of Mercury and Venus on all possible occasions.

Some interesting experiments were made to spectroscopically determine the *colour curve* of the 26-inch. The minimum focal length occurs about E, from which the focal plane for F is 0·142 inches, and for G (iron) 1 233 inches distant.

The 12-inch has been used in observing comets, minor planets, occultation of stars, and eclipses of Jupiter's satellites. In past years this instrument has been used for the exhibition of celestial objects to the public on two evenings each week; this has now been limited to one evening. The number of visitors during the year has been nearly 1500.

Two new instruments have been acquired, a 6-inch transit circle and a 5-inch altazimuth, both marking a new departure in being made entirely of steel, in the endeavour to reduce to a minimum the changes due to flexure and temperature. Both instruments have been made by an American firm, this choice being seemingly justified by their performance after installation.

The new tables of the planets Mars, Uranus and Neptune are nearly completed ready for publication.

The department of nautical instruments, chronometers and watches was under a great strain during the war with Spain, all available instruments in the country being purchased, and many having to be sent out without the complete tests usually applied before acceptance.

The magnetic department is likely to be abandoned owing to the serious disturbing effects of the currents leaking from the numerous electric plants in the immediate vicinity.

ALLOYS OF IRON AND NICKEL.

At the Institution of Civil Engineers, on March 28, a paper was read on "Alloys of Iron and Nickel," by Mr. R. A. Hadfield. The effect upon iron of gradually increasing amounts of certain added foreign elements has already been investigated by Mr. Hadfield in experiments upon the mechanical and physical properties of alloys of iron and manganese, silicon, aluminium and chromium, both in the cast and forged conditions. The present paper contains the results of a similar investigation of the alloys of iron and nickel. The addition of nickel, either by conferring greater homogeneity or by some particular combination with the iron or carbon present, or both, appears to confer properties upon the alloy equivalent to an annealing, or, if annealing be employed, to reduce the stress produced by forging; it does this even in the annealed material without injuring or seriously lowering the elastic limit. In this respect, therefore, its presence is of considerable practical utility. It would appear there is considerable room for improving the material of propeller-shafts. The well-known Russian metallurgist, Chernoff, has stated that steel is composed of crystals of metallic iron cemented by carbide of iron, the result being that in the case of nickel-iron alloys the inter-crystalline spaces (a subject which has been fully and carefully investigated by Mr. T. Andrews, F.R.S., as regards ordinary steel) are much more completely filled, and the cohesion consequently rendered more powerful. The points of solidification of the cement and crystals are nearer, thus producing or maintaining a more intimate interweaving of the elements. In support of this theory may be cited the fact that without doubt nickel-iron alloys show less tendency to segregation, which apparently indicates that the combinations formed at high temperature are more intimately maintained when the alloy cools and becomes solidified than is the case with ordinary steel. That scientific evolution disregards nationalities is well illustrated in the case of the metal nickel and its developments. The Swedish chemist Cronstedt, at work in his laboratory, conceived the idea that the deceptive Kupfer-Nickel is a metal containing a new element; from him it passed to other Swedish investigators, thence to those of German, French, and other nationalities. Attempts were made by Faraday to artificially produce meteoric iron; similar efforts were afterwards made in France, Germany and elsewhere. Apparently a period of inactivity followed, during which, however, first one and then another experimenter added facts to the general stock. Finally, as regards the metallurgy of nickel as applied to iron and steel, Marbeau, in France, applied the matter experimentally; Schneider, in France, perfected its application to a number of purposes, chiefly armour-plates; Riley brought the manufacture to a practical issue in England; American and Canadian enterprise followed in the perfecting of cheaper methods of producing nickel, and a new product—nickel steel—appeared. It has taken about 125 years to arrive at the stage reached to-day. No one person, no one nationality, can lay claim to its discovery, to its inception. If a chart were constructed on genealogical lines, how much would be seen to have sprung from the laboratory experiments by Cronstedt? But, if it were possible, long before the work of the Swede would be discovered the early workers of the old world in China and Japan, about whose work dates and facts were inaccessible. Apparently, to modern ideas, mention of nickel appears to have struggled into existence between the years 1600 and 1700, but the metal must have been well known, not merely centuries, but almost thousands of years earlier.

THE PRESENT STATE OF EVOLUTION.[1]

Only a little less than fifty years have passed since the publication of Darwin's "Origin of Species," and the general acceptance by naturalists of the theory of descent. Since 1848 the sciences of embryology, cytology, and comparative anatomy based on embryology—or, as it is now called, morphology—have been placed on a firm foundation. It is but little over half a century since the uniformitarian views of Lyell were promulgated. The cell doctrine was born in 1839; the view that pro-

[1] Prologue of an address entitled "A Half-Century of Evolution, with special reference to the Effect of Geological Changes on Animal Life," delivered by Prof. Alpheus S. Packard before the Section of Zoology of the American Association for the Advancement of Science at the Boston meeting (fiftieth anniversary) on August 22, 1898.

toplasm forms the basis of life was generally received forty years since; fifty years ago the doctrine of the conservation of forces was worked out, and already by this time had the idea of the unity of nature dominated the world of science.

On the fiftieth anniversary, therefore, of our Association, it may not be out of place, during the hour before us, first, briefly to inquire into the present state of evolution and its usefulness to zoologists as a working theory, and then to dwell more at length on the subject of the effect of geological changes on animal life.

The two leading problems which confront us as zoologists are: What is life? and, How did living beings originate? We must leave to coming centuries the solution of the first question, if it can ever be solved; but we can, as regards the second, congratulate ourselves that—thanks to Lamarck, Darwin, and others, in our day and generation—a reasonable and generally accepted solution has been reached.

Time will not allow us to attempt to review the discoveries and opinions which have already been discussed by the founders and leaders of the different schools of evolutionary thought, and which have become the common property of biologists, and are rapidly permeating the world's literature.

It may be observed at the outset that, if there is any single feature which differentiates the second from the first half of this century, it is the general acceptance of the truth of epigenetic evolution as opposed to the preformation or incasement theory, which lingered on and survived until a late date in the first half of the present century.[1] The establishment of the epigenetic view is largely due to exact investigation and modern methods of research, but more especially to the results of modern embryology and to the fairly well digested facts we now have relating to the development of one or more types of each class of the animal kingdom.

To use a current phrase, the evolution theory is now held as come to stay. It is the one indispensable instrument on which the biologist must rely in doing his work. It is now almost an axiomatic truth that evolution is the leaven which has leavened the whole lump of human intellectual activity. It is not too much to claim that evolutionary views, the study of origins, of the beginning of organic life, the genesis of mental phenomena, of social institutions, of the cultural stages of different peoples, and of their art, philosophy, and religion—that this method of natural science has transformed and illuminated the philosophy of the present half-century.[1]

It is naturally a matter of satisfaction and pride to us as zoologists that, though evolution has been in the air from the days of the Greek philosophers down to the time of Lamarck, the modern views as to the origin of variations, of adaptation, of the struggle for existence, of competition, and the preservation of favoured organs or species by selection are the products of single-minded zoologists like Darwin, Wallace, Fritz Müller, Semper, and Haeckel. It is the work of these men, supplemented by the labours of Spencer and of Huxley, and the powerful influence of the botanists, Hooker and Gray, all of whom contributed their life-long toil and efforts in laying the foundation-stones of the theory, which has brought about its general acceptance among thinking men. It is these naturalists, some of them happily still living, who have worked out the principle of evolution from the generalised to the specialised, from the simple to the complex, from chaos to cosmos.

The doctrine of evolution has been firmly established on a scientific basis by many workers in all departments of biology, and found not only to withstand criticism from every quarter, but to be an indispensable tool for the investigator. The strongest proof of its genuine value as a working theory is that it has, under the light shed by it, opened up many an avenue of inquiry leading into new fields of research. It is based on the inductive method, the observation and arrangement of a wide series of facts. Moreover it explains a vast complex of facts, and enables us to make predictions, the true test of a scientific theory. Biology is not an exact science, hence the theory is not capable of demonstration like a problem in mathematics, but is based on probabilities, the circumstantial evidence being apparently convincing to every candid, well-trained mind.

The methods and results of natural science, based as they now are on evolutional grounds, have, likewise, appealed to the historian, the philologist, the sociologist, and the student of comparative religion, whose labours begin with investigations into the origins.

It goes without saying that, thanks to the initiative of the above-named zoologists, every department of intellectual work and thought has been rejuvenated and rehabilitated by the employment of the modern scientific method. All inquiring minds appreciate the fact that, throughout the whole realm of nature, inorganic as well as organic, physical, mental, moral and spiritual, there was once a beginning, and that from a germ, by a gradual process of differentiation or specialisation, the complex fabric of creation has, by the operation of natural laws and forces, been brought into being. All progress is dependent on this evolutionary principle, which involves variation, adaptation, the disuse or rejection of the unfit, the use or survival of the fittest, together with the mechanical principle of the utmost economy of material.

Though the human mind has its limitations, and the chief arguments for evolution have been drawn from our observations of the history of our own planet, and of the life existing upon it, the nebular hypothesis teaches us that the same process has determined the origin of other worlds than ours, and applies in fact to all the other members of our solar system, while with little doubt the principle may be extended to the entire universe.

At all events evolutionary modes of thinking have now become a second nature with philosophic, synthetic minds, and to such any other view is inconceivable. We teach evolution in our colleges and universities, and the time is rapidly approaching, and in some instances has already come, when nature-

[1] The theory of incasement (*emboitment*), propounded by Swammerdam in 1733, was that the form of the larva, pupa and imago of the insects pre-existed in the egg, and even in the ovary; and that the insects in these stages were distinct animals contained one inside the other, like a nest of boxes, or a series of envelopes, one within the other; or, in his own words: "*Animal in animali, seu papilio intra erucam reconditus.*" Réamur (1734) also believed that the caterpillar contained the form of the chrysalis and butterfly, saying: "Les parties des papillon cachées sous le fourreau de chenille sont d'autant plus faciles à trouver que la transformation est plus proche. Elles y sont neanmoins de tout temps." He also believed in the simultaneous existence of two distinct beings in the insect. "Il serait très curieux de connaître toutes les communications intimes qui sont entre la chenille et le papillon . . . La chenille hache, broye, digere les aliments qu'elle distribué au papillon; comme les mères préparent ceux qui sont ports aux fœtus. Notre chenille en un mot est destinée à nourrir et à defendre le papillon qu'elle renferme" (Tome i, 8^e Mémoire, p. 363).

It was not until 1815 that Herold exploded this error, though Kirby and Spence in 1828, in their "Introduction to Entomology," combated Herold's views, and maintained that Swammerdam was right. As late as 1834, a century after Swammerdam Lacordaire in his "Introduction à l'Entomologie," declared that "a caterpillar is not a simple animal, but compound," and he actually goes so far as to say that "a caterpillar, at first scarcely as large as a bit of thread, contains its own teguments threefold and even eightfold in number, besides the case of a chrysalis, and a complete butterfly, all lying one inside the other." This view, however, we find is not original with Lacordaire, but was borrowed from Kirby and Spence without acknowledgment. These authors, in their "Introduction to Entomology" (1828), combated Herold's views, and stoutly maintained the old opinions of Swammerdam. They based their opinions on the fact, then known, that certain parts of the imago occur in the caterpillar. On the other hand, Herold denied that the successive skins of the pupa and imago existed as germs, holding that they are formed successively from the "*rete mucosum*," which we suppose to be the hypodermis of later authors. In a slight degree the Swammerdam-Kirby and Spence doctrine was correct, as the imago does arise from germs, *i.e.* the imaginal discs of Weismann, while this was not discovered by Herold, though they do at the outset arise from the hypodermis, his *rete mucosum*. Thus there was a grain of truth in the Swammerdam-Kirby and Spence doctrine, and also a mixture of truth and error in the opinions of Herold.

The discovery by Weismann of the imaginal discs or buds of the imago in the maggot of the fly, and his theory of histolysis, or of the more or less complete destruction of the larval organs by a gradual process, and his observation of the process of building up of the body of the imago from the previously latent larval buds, was one of the triumphs of modern biology. It is therefore not a little strange to see him at the present day advocating a return to the preformation views of the last century in the matter of heredity. Of course it goes without saying, as has always been recognised, that there is something in the constitution of one egg which predestines its becoming an insect, and in that of another, which destines it to produce a chick.

[1] It is worthy of mention that just fifty years ago, in his "Future of Science," written in 1848, at the age of twenty-five, Renan, who first among philosophers and students of comparative philology adopted the scientific method, *i.e.* the patient investigation of as wide a range of facts as possible, wrote: "I am convinced that there is a science of the origins of mankind, and that it will be constructed one day, not by abstract speculation, but by scientific researches. What human life in the actual condition of science would suffice to explore all the sides of this single problem? And still, how can it be resolved without the scientific study of the positive data? And if it be not resolved, how can we say that we know man and mankind? He who would contribute to the solution of this problem, even by a very imperfect essay, would do more for philosophy than by half a century of metaphysical meditation" (p. 150). Again he says: "The great progress of modern thought has been the substitution of the category of *evolution* for the category of the '*being*,' of the conception of the relative for the conception of the absolute, of movement for immobility. Formerly everything was considered as 'being' (an accomplished fact); people spoke of law, of religion, of politics, of poetry in an absolute fashion. At present everything is considered as in the process of formation" (p. 169).

studies, and the facts of biology forming the grounds of the evolutionary idea, will be taught in our primary and secondary schools.

The rapidity with which evolutionary conceptions have taken root and spread may be compared to the rankness of growth of a prepotent plant or animal on being introduced into a new territory where it is free from competition. It has indeed swept everything before it, occupying a field of thought which hitherto had been unworked by human intelligence.

The immediate effect and a very happy one, of the acceptance of the theory of descent on working zoologists, is to broaden their minds. Collectors of insects and shells, or of birds and mammals, instead of being content simply to acquire specimens for their cabinets, are led to look during their field excursions for examples of protective mimicry, or to notice facts bearing on the immediate cause of variation. Instead of a single pair of specimens, it is now realised that hundreds and even thousands collected from stations and habitats wide apart are none too many for the study of variation as now pursued.

The race of "species grinders" is diminishing, and the study of geographical distribution, based as it is on past geographical changes and extinctions, is now discussed in a far more philosophical way than in the past. The most special results of work in cytology and morphology are now affording material for broad work in phylogeny and heredity.

On the other hand it must be confessed that, as the result of the acceptance of evolutionary views, our literature is at times flooded with more or less unsound hypotheses, some tedious verbiage and long-winded, aerial discussions, based rather on assumptions than on facts. But on the whole, perhaps, this is a healthy sign. Too free, exuberant growths will be in the long run lopped off by criticism.

One tendency should be avoided by younger students, that of too early specialisation, and of empirical work without a broad survey of the whole field. In some cases our histologists and morphologists rise little above the intellectual level of species describers. Expert in the use of the microtome and of reagents, they appear to have but little more general scientific or literary culture than high-class mechanics. The chief antidote, however, to the danger of narrowness is the lessons derived from evolutionary thought and principles.

Finally, as a proof of the value of evolutionary ideas to the present generation, let us suppose for a moment, if it were conceivable, that they should be blotted out. The result, it is safe to say, would be equivalent to the loss of a sense.

It is a matter of history that when a new idea or principle or a new movement in philosophy or religion arises, it at first develops along the line of least resistance; the leaders of the new thought acquire many followers or disciples. Soon the latter outstrip their teachers, and go to greater extremes; modifications of the original simple condition or theory occur, and as the final result there arise schisms and differentiations into new sects. This has happened in science, and already we have evolutionists divided into Lamarckians and Darwinians, with a further subdivision of them into Neolamarckians and Neodarwinians, while the latter are often denominated Weismannians. Some prefer to rely on the action of the primary factors of evolution, others believe that Natural Selection embraces all the necessary factors, while still others are thoroughly persuaded of its inadequacy.

The result of this analytical or differentiating process will probably be an ultimate synthesis, a belief that there is a complex of factors at work. Of these factors those originally indicated by Lamarck, with the supplementary ones of competition and natural selection bequeathed by Darwin, are the most essential and indispensable, and it is difficult to see how they can be displaced by other views. Meanwhile all agree, and it was never more firmly established than at this moment, that there is and always has been unceasing energy, movement, and variation, a wonderful adaptation and harmony in nature, between living beings and their surroundings.

The present status of evolution in its different phases or attitudes since the time of the appearance of Darwin's "Origin of Species" may be roughly pointed out as follows:—

(1) The claim by some thinkers of the inadequacy of Darwinism, as such, or Natural Selection, to account for the rise of new species, and the assignment of this factor to what they believe to be its proper place among the other factors of organic evolution.

(2) The renaiscence of Lamarckism under the name of Neolamarckism, being Lamarckism in its modern form. This school relies on the primary factors of evolution, on changes in the environment, such as the agency of the air, light, heat, cold, changes in climate, use and disuse, isolation, and parasitism, while it regards natural, sexual, physiological, germinal and organic selection, competition or its absence, and the inheritance of characters acquired during the lifetime of the individual, as secondary factors, calling into question the adequacy of natural selection as an initial factor.

(3) The rise of the Neodarwinian school. While Darwin, soon after the publication of the "Origin of Species" somewhat changed his views as to the adequacy of natural selection, and favoured changes in the surroundings, food, &c., as causes of variation, his successors, Wallace, Weismann, and others believe in the "all-sufficiency" of natural selection. Weismann also invokes panmixia, or the absence of natural selection, as an important factor; also amixia, and denies the principle of inheritance of acquired characters, or use-inheritance.

(4) A third school or sect has arisen under the leadership of Weismann, who advocates what is in its essence apparently a revival of the exploded preformation, encasement, or "evolution" theory of Swammerdam, Bonnet and Haller, as opposed to the epigenetic evolutionism of Harvey, Wolff, Baer, and the majority of modern embryologists. On the other hand, there are some embryologists who appear to accept the combined action of epigenesis and evolution in development.

(5) Attention has been concentrated on the study of variations and of their cause. Opinion is divided as to whether variation is fortuitous or definite and determined. Many now take exception to the view, originally held by Darwin, that variations are purposeless and fortuitous, believing that they are, for example, dependent on changes in the environment which were determined in early geological periods. For definite variation Eimer proposes the term orthogenesis. Minute variations dependent on climatic and other obscure and not readily appreciable causes are now brought out clearly by a system of varied and careful quantitative measurements.

(6) More attention than formerly is given to the study of dynamical evolution, or kinetogenesis; to the effect of external stimuli, such as intermittent pressure, mechanical stresses and tensions by the muscles, &c., on hard parts. Originally suggested by Herbert Spencer, that the ultimate cause or mechanical genesis of the segmentation of the vertebrate skeleton was due to transverse strains, the segmentation of the bodies of worms and arthropods, as well as of vertebrates, has been discussed by recent workers (Rider, Cope, Meyer, Tornier, Hirsch, and others). Here should be mentioned the work done in general physiology, or morphogenesis, by Verworn, Davenport, and others. Also the discoveries of Pasteur, and the application by Metschnikoff and of Kowalevsky of phagocytosis to the destruction and renewal of tissues during metamorphosis, bear closely on evolutional problems.

(7) A new field of research founded by Semper, Vilmorin, and Plateau, and carried on by De Varigny, is that of experimental evolution, involving the effects of artificial changes of the medium, including temperature, food, variation in the volume of water and of air, absence of exercise, movement, &c. Also should be added horticultural experiments which have been practised for many years, as well as the results of acclimatisation.

Here should be mentioned the experiments bearing on the mechanics of development (Entwickelungsmechanik der organismen), or experimental embryology, of Oscar Hertwig, Roux, Driesch, Morgan and others, and the curious results of animal grafting and of mutilations of the embryos, obtained by Born and others, as well as the regeneration of parts. The remarkable facts of adaptation to new and unfavourable conditions of certain embryos are as yet unexplained, and have led to considerable discussion and research.

(8) The *à priori* speculations of Darwin, Galton, Spencer, Jaeger, Nusbaum, Weismann and others, based on the results of the labours of morphologists and cytologists, have laid the foundation for a theory of the physical basis of heredity, and for the supposition that the chromatin in the nucleus of reproductive cells is the bearer of heredity. The theory has already led to prolonged discussions, and opened up new lines of work in cytology and embryology.

(9) The subject of instinct, discussed from an evolutional point of view, both by morphologists and psychologists, particularly by Lloyd Morgan, has come to the front, while mental evolution has been discussed by Romanes and others.

With all these theories before us, these currents and counter-currents in evolutional thought bearing us rapidly along, at times perhaps carrying us somewhat out of our depth, the conclusion of the whole matter is that in the present state of zoology it will be wise to suspend our judgment on many theoretical matters, to wait for more light, and to confine our attention meanwhile to the observation and registration of facts, to careful experiments, and to repeated tests of mere theoretical assumptions.

Meanwhile we may congratulate ourselves that we have been born and permitted to labour in this nineteenth century, the century which in zoological science has given us the best years of Lamarck's life, a Cuvier, a Darwin, a Von Baer, an Owen, an Agassiz, a Haeckel, a Spencer, and a Huxley—the founders of modern zoology—who have sketched out the grander features of our science so completely, that it will, perhaps, be the work of many coming years to fill in the details.

Prof. Packard discussed in detail the geological causes of variation and of the extinction and renewal of species, but space cannot, unfortunately, be found to reproduce this portion of his address.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

PROF. W. M. DAVIS, who now holds the chair of Physical Geography in Harvard University, has been elected Sturgis Hooper professor of geology in the same university.

UNDER the will of the late Mr. Vincent Stuckey Lean, the Trustees of the British Museum receive 50,000*l*., to be devoted to the improvement and extension of the library and reading-room of the Museum.

THE Technical Education Board of the London County Council has awarded one of its commercial scholarships to Mr. W. M. Poole, of Magdalen College, Oxford, who is an assistant-master of the Merchant Taylors' School. The scholarship is of the value of 150*l*., and will enable Mr. Poole to proceed to some of the commercial colleges in foreign countries and study their method of commercial teaching.

THE agricultural experiments carried out by Prof. William Somerville in the counties of Cumberland, Durham, and Northumberland, and the Durham College of Science, Newcastle-upon-Tyne, should prove of distinct benefit to agriculture in the North of England. The work is done in connection with the County Councils of the counties mentioned, and it forms a most valuable branch of technical education. The seventh annual report just issued contains accounts of numerous field experiments, the results of which are of scientific as well as practical value. Satisfactory evidence of interest in this phase of technical education is shown by the fact that a great demand has arisen in the county of Durham for lectures explanatory of the experimental results. Investigation has thus been the means of quickening an interest in the science of agriculture.

AT the ninth meeting of University Extension and other students to be held in Oxford from July 29 to August 23, a number of lectures will be given to illustrate, as far as possible, the more remarkable contributions to science during the period 1837 to 1871. Prof. Francis Gotch, F.R.S., will deal with the physiology of sensation; Prof. E. B. Poulton, F.R.S., will deal with the modes in which the colours, markings, and attitudes of animals are of value for the struggle of life; Prof. W. J. Sollas, F.R.S., will lecture on the geology of Oxford; Prof. H. A. Miers, F.R.S., will lecture on the growth of a crystal; Mr. G. C. Bourne and Mr. A. W. Brown will deal with the growth of the living organism; Dr. Arthur Ransome, F.R.S., will lecture on hygiene; Mr. G. J. Burch on wireless telegraphy; Mr. H. N. Dickson on the influence of climate; and Dr. Reginald Farrar on anthropology. There will also be classes in geology (with field excursions conducted by Prof. Sollas), and in biology.

THE London County Council has delegated to its Technical Education Board such new powers and duties as it has acquired by receiving recognition under Clause vii. of the Directory of the Department of Science and Art (see p. 498). The Board is communicating with the various schools and institutions which are in receipt of grants both from the Board and from the Department, with a view to making the necessary arrangements for carrying out the provisions of Clause vii. The new Board recently appointed consists of twenty representatives from the London County Council, three from the London School Board, two representing the City Parochial Charities Foundation, three appointed by the City and Guilds of London Institute, three from the London Trades Council, and one each from the Incorporated Association of Headmasters and the National Union of Teachers. There are in addition two co-opted members. The Board have decided to introduce some important changes into the regulations for intermediate county scholarships, and the alterations will take effect in the examination to be held in June, 1900.

SCIENTIFIC SERIALS.

American Journal of Science, March.—Studies in the Cyperaceæ, by T. Holm. This article deals with the genus *Lipocarpha*, formerly regarded as a species of Hypælyptum.—The constitution of the ammonium-magnesium phosphate of analysis, by F. A. Gooch and Martha Austin. The presence of ammonium chloride or other ammonium salt is necessary in the precipitation of manganese as the ammonium-manganese phosphate by microcosmic salt, in order that the precipitate may have the ideal constitution NH_4MnPO_4. Further research shows that the chemical constitution of the precipitate rather than mechanical contamination and varying solubility may be responsible for observed variations in the weight of the residue derived by the ignition of the similar salt of magnesium NH_4MgPO_4, precipitated by an excess of a soluble phosphate from the solution of a magnesium salt.—The crystal symmetry of the minerals of the mica group, by T. L. Walker. The examination of crystals not only by the goniometer, but also by the etching progress, and as regards their optical, electric, thermal and cohesive properties, has had the effect of a widespread degradation of crystals from systems of higher to systems of lower degree of symmetry. This tendency is especially shown by minerals of the mica group. Biotite, phlogopite, rubellan and lepidolite are not monoclinic but triclinic, while muscovite is either monoclinic, or, if it be triclinic, it is so very finely polysynthetically twinned that we cannot find a triclinic individual large enough to respond to the optical or etching method.—Imperfectly known and new Actinians, by A. E. Verrill. The specimens described include a new genus *Phelliopsis*, having a general appearance and habit resembling *Phellia*, and two new species, *Anthopleura japonica* and *Bunodactis manni*.—Some American fossil Cycads: Part i., the male flower of Cycadeoidea, by G. R. Wieland. The living Cycads constitute one of the most ornate and characteristic orders of plants, and occupying, as they do, a position on the border-land between the higher Cryptogams and the lower Phanerogams, their ancestral relationship is of considerable interest. The author has examined a collection of rare fossils from the Rocky Mountains, now in the museum of Yale University. The results strengthen the belief that the relation between Ferns and Cycads must have been a very close one.—Footprints of Jurassic Dinosaurs, by O. C. Marsh. One of the most interesting geological discoveries during the past season in the Black Hills region was a locality of footprints made by Dinosaurian reptiles in deposits of Jurassic age. These footprints are the first found in American Jurassic strata. They are all tridactyle, of large size, and were evidently made by some of the great Dinosaurs known to have lived during Jurassic time.—A new Kansas meteorite, by H. L. Ward. This meteorite was found in Ness County, and weighs 417 grms. It no longer has the usual black crust.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 2.—"On Hydrogen Peroxide as the active Agent in producing Pictures on a Photographic Plate in the Dark." By Dr. W. J. Russell, V.P.R.S.

In previous papers the author has been shown that certain bodies are able, in the dark, to act on a photographic plate and produce a picture. The present communication shows that in all the cases which have been examined, and probably in all others of a similar kind, the action which occurs is due directly or indirectly to the presence of hydrogen peroxide.

March 9.—"A Preliminary Note upon certain Organisms isolated from Cancer, and their Pathogenic Effects upon Animals." By H. G. Plimmer, Pathologist, and Lecturer on Pathology and Bacteriology, St. Mary's Hospital, London. Communicated by Prof. J. Rose Bradford, F.R.S.

During the past six years I have been studying the cell-inclusions found in cancer, and for this work I have had to examine 1278 cancers taken from various organs and parts. Out of this large number of cases there have been a few—nine in all—in which the cell-inclusions have been extremely numerous; so that at the growing edge, and even far into the tumour, scarcely a cell could be found without an inclusion, sometimes with as many as thirty-six, even, of these inclusions in one cell; and these bodies have been similar to those which Metschnikoff, Ruffer, and others, as well as myself, have regarded and described as parasites, standing in causal relationship to the disease.

It will be seen that I mean by "cell-inclusions" those bodies found in cancer cells which are not recognisable as any known degeneration, and which do not form any part of the cell.

I have succeeded in isolating from the last of these cases, an organism, which is pathogenic, in a peculiar manner, to certain animals, and whose virulence I have been able to keep unimpaired for some months.

Previous Work on the Experimental Production of Tumours in Animals.

The only work, I think, that needs mention here is that of Sanfelice, in Cagliari, and of Roncali, in Rome. Sanfelice has produced tumours in animals with organisms which he isolated from infusions of various fruits; and they both have isolated organisms from cancers. But Sanfelice's organism appears to have been very difficult to isolate in a virulent form from human cancer, and to keep virulent; so that in his last paper,[1] he treats only of the organisms derived from fruit infusions, and of their effects upon animals. Most of their statements are doubted by the German pathologists, including such a good observer as Baumgarten. But I do not find any reason to doubt any of Sanfelice's statements; and I think that he deserves the greatest credit for removing the study of the ætiology of cancer from the histological to the experimental region of work.

On the Method of Isolation adopted.

The cancer, from which the organisms described were isolated, and with which my experiments have been made, was taken from the breast of a woman aged thirty-five years; it had a history of only two months' duration, and it was growing rapidly at the time of the operation. Immediately after removal, I examined a fresh scraping, and, finding such an extraordinary number of the bodies I have mentioned in the cells, I cut thin slices from the growth, which I placed with a little of the juice scraped from the cut surface in a flask containing the following liquid, which was of course carefully sterilised. This medium consisted of an infusion made from cancer, just as the ordinary beef infusion is made, to which was added, after careful neutralisation, 2 per cent. of glucose and 1 per cent. of tartaric acid. Upon this medium scarcely any bacteria, however hardy, will grow; so that contaminations are not common.

Then, remembering that in the body these organisms were under anaërobic conditions, I exhausted the air from my flasks, and passed hydrogen into them, finally sealing them up. This I have found is of great importance as regards the maintenance of the virulence; and I find that my cultures are as active now as they were four months ago. By these means, I got, after from three to five days, a pure culture of the organism described, which has been kept growing in this, and various other media, ever since.

Morphology and Relation to Media.

The organism is apparently a saccharomyces; but, according to some authorities (such as De Bary, Cuboni, and Duclaux), the saccharomycetes are nothing but the developmental stages of fungi which really belong to either the Phyco-, Asco-, or Basidio-mycetes. Moreover, they state that in some species of mycelium-forming fungi, single parts, especially conidia, can grow in the saccharomyces form on certain nutrient media; so I will not attempt to locate this organism at present. Sanfelice and Roncali, however, definitely state that the organisms they have isolated are Blastomycetes.

When grown in the medium described, these organisms produce a cloudiness which becomes visible in about forty-eight hours, and increases till about the sixth day, when the growth sinks to the bottom, the medium then becoming clear; no scum or pellicle is formed.

When grown on this medium solidified with agar, the organisms form small round colonies which remain separate; after some weeks the colour, which was originally white, becomes yellow; the colonies do not attain a large size at any time.

Gelatin is not liquefied, but the growth on this medium is never luxuriant. On potato a thick white layer is formed, which in about two weeks will cover the entire surface, changing then to a yellowish brown colour.

They will grow aërobically, but not well, at any rate at first; and they lose their virulence in a short time, when grown in this way.

Microscopically they are round bodies, frequently growing in clumps, with a central portion which stains deeply, and, in most cases, with a thin, strongly refractile capsule, which sometimes shows a double contour; but young forms can be seen which are without a capsule. The size varies from 0·004 mm. to 0·04 mm.

Their reproduction appears to be by budding; but I have fancied that I have also seen, in a few instances, endogenous budding.

These bodies correspond morphologically with those found in the original tumour, and also with those described by Ruffer and myself, and by some others of those who have worked at the microscopical appearances of cancer.

Experimental Results.

These can be summarised under the four following divisions. Up to the present, I have not been able to make any such experiments upon animals as would allow of the easy bringing of the organisms into contact with a likely epithelial surface, with the exception of the cornea; but, through the kindness of Dr. Bradford, I have been enabled now, at the Brown Institution, to inoculate a bitch in the mammæ, but the time is as yet too short to enable me to make any statement as to the result.

The cultures used in the experiments were made in the medium previously described.

(1) Negative results. Rabbits inoculated intravenously and intraperitoneally; and rabbits and guinea-pigs inoculated subcutaneously. The animals were killed in from fifteen days to fourteen weeks. I have left none longer than this.

(2) Those animals in which death was produced without any obvious lesion; but from the organs of which pure cultures of the organism were made. These consisted of rabbits which were trephined, the organisms being then placed under the dura mater. The organisms were present in the brain, cord, and viscera.

(3) Corneal inoculation, in rabbits, in which true neoplasms were produced. There was considerable proliferation of the corneal epithelium, which had forced its way in all directions from the point of inoculation, deep down into, and between, the fibrous layers of the corneal tissue. The organisms were found in the epithelial cells.

(4) Positive results: the animal dying with the production of new growths. These results are found in guinea-pigs inoculated intraperitoneally. Death ensues in from thirteen to twenty days: and the liver, lungs, and peritoneum are found studded with new growths of a white colour, which are of an endothelial nature. Pure cultures could be made from the growths.

The important point of this work is: the experimental production of malignant tumours in animals by an organism isolated from a malignant tumour in man. That these experimental tumours are, so far with one exception, of endothelial origin is due to the fact that until I was enabled to inoculate a dog, I found it very difficult to get the organism in contact with likely epithelium; all the above methods of inoculation, save one, could only bring them into contact with endothelial surfaces. The corneal experiment is the only one in which an epithelial surface was tried; and in this case the great proliferation of the epithelium, the appearances of the organisms in the cells, and the irritation produced, are very striking.

[1] "Zeitschrift für Hygiene," 1899.

The deductions which I think may fairly be made from these observations and experiments are as follows:

(1) That there are certain cancers, which occur very rarely, in which there are in enormous numbers, intracellular bodies of the kind described by Ruffer, myself, and others, as parasitic Protozoa. (From the rarity of these cases and their comparatively acute course, one is tempted to think that they are not due to the same cause as ordinary cancers; but there is really no more difference between them and ordinary cancers than between acute and chronic tubercle.)

(2) That these intracellular bodies can be isolated and cultivated outside the body.

(3) That these cultures, when introduced into certain animals, can cause death, with the production of tumours; so far, with the exception of the corneal growths, of endothelial origin; and that pure cultures can be made from these growths, which, when inoculated into suitable animals, will produce similar tumours.

Linnean Society, March 16.—Dr. A. Günther, F.R.S., President, in the chair.—Dr. John Lowe communicated some observations on the fertilisation of *Araujia albens*, G. Don, a Brazilian climber, which in the south of England grows in the open air. Last summer it was blooming freely in Lord Ilchester's garden at Abbotsbury, where the flowers were visited by numbers of butterflies, diurnal moths, humble-bees, wasps, and large flies, many of which were captured and imprisoned for a time in the pinching-bodies (*Klemm-körper* of Müller). All these insects, with the exception of some humble-bees, in their visits to the nectar left their proboscis behind, and sometimes a leg, being not strong enough to detach the pinching-body. Dr. Lowe described the structure of the pinching-bodies, which are flat horny plates situated, above the nectar-cups, at each angle of a five-sided hollow cone in the centre of the flower, in which is placed the stigma. There is only a small opening at the apex and a narrow slit at the base of each facet of the cone. To the upper point of the pinching-body the pollinia are attached. When an insect has its proboscis caught in the slit, which narrows always to its point, it can only escape by tearing away the body with its pollen-masses or by leaving its proboscis in the slit. In the former case it carries the pollinia to the next flower it visits, and thus effects cross-fertilisation by leaving the pollen-mass between the anther-wings, whence it rapidly passes into the cone. He had received a number of flowers of *Araujia* from Mr. Benbow, the gardener at Abbotsbury, in some of which he found the proboscis of a butterfly or moth in each of the five angles of the cone, showing the great destruction of insect-life caused by the plant. Mr. N. E. Brown, who has made a special study of the Asclepiadaceæ, gave an interesting account of the manner in which the pollinia reach the stigma; and some further remarks were made by Mr. A. W. Bennett.—Mr. P. Chalmers Mitchell read a paper on so-called "quintocubitalism" in the wing of birds. He showed that the terms "aquintocubital" and "quintocubital," applied to birds because of certain conditions in the wings, were misleading, and proposed the new terms "diastataxy" and "eutaxy." From general considerations based on the anatomy and osteology of *Columbae*, he concluded that the eutaxic forms were clearly more highly specialised forms, and that they had been derived from diastataxic forms. Comparative anatomy making it exceedingly probable that "diastataxy" is the primitive condition among birds, Mr. Mitchell proceeded to show that the primitive existence of a gap was not difficult to explain.—Mr. W. P. Pycraft read a paper entitled "Some facts concerning the so-called 'aquintocubitalism' in the bird's wing." He showed, by means of a series of lantern slides, that "aquintocubitalism" was due to a shifting, backwards and outwards, of the secondary remiges 1–4 and of the horizontal rows of coverts 1–5. The terms—suggested by Prof. E. Ray Lankester—"stichoptilous" and "apoptilous" were proposed as substitutes for the older and less convenient terms quinto- and aquinto-cubitalism. All wings, it was shown, are, in the embryo, stichoptilic, and later may become apoptilic. Hence the author felt inclined to regard the former as the more primitive arrangement. Prof. E. Ray Lankester, F.R.S., in some remarks upon the two preceding papers, gave reasons for preferring the terms "stichoptilous" and "apoptilous" in substitution for those which had been adopted by the authors. Both authors were agreed on the main issue, at which they had arrived independently, one through the study of development, the other through that of adult anatomy.

Geological Society, May 18.—W. Whitaker, F.R.S., President, in the chair.—Relations of the chalk and drift in Möen and Rügen, by Prof. T. G. Bonney, F.R.S., and the Rev. Edwin Hill. These two islands are separated in a north-westerly to south-easterly direction by about thirty-five miles of sea. They both exhibit at many spots the chalk and drift, in relations which are peculiar and abnormal. Some geologists maintain that the glacial beds have been included in the chalk by a series of acute folds; others that they have been dropped down by a series of faults; others, again, explain the relationship as the result of ice-action. Simple faulting appears to be insufficient, while it is a circumstance not easily explained by earth-movement or ice-action that the axes of the folds in the chalk strike roughly east-north-east to west-south-west in Möen, and north and south in Rügen. The authors then describe a series of sections in Möen, which lead them to the following conclusions: (*a*) The chalk dominates greatly over the clay, the latter being often merely a local phenomenon. (*b*) The chalk is stained brown, and the clay streaked with chalk for a few inches from the junction. (*c*) The clay is often a mere facing to the chalk, or occupies semi-cylindrical or wedge-shaped cavities, which sometimes seem to terminate above sea-level. (*d*) The clay seems often associated with superficial ravines, which are probably never much prolonged below the sea-level. The chalk is strongly folded, but rarely, if ever, faulted, and there is no evidence to connect the intercalations of drift with faults. Numerous sections in Rügen are then described, which (though there are differences in detail) present a general resemblance to those in Möen, and, as a rule, have no resemblance to those near Cromer.—A critical junction in the County of Tyrone, by Prof. Grenville A. J. Cole. The investigations of the author have led him to the conclusion that the granite of Eastern Tyrone is identical with that of Slieve Gallion.

PARIS.

Academy of Sciences, March 27.—M. van Tieghem in the chair. Obituary notice of M. Charles Naudin, by M. Ed. Bornet.—M. Gaudry announced the death of Prof. O. C. Marsh, Correspondant in the Section of Mineralogy.—The Perpetual Secretary announced the death of Prof. G. H. Wiedemann, Correspondant in the Physical Section.—Obituary notice of Prof. Wiedemann, by M. Mascart.—The deformation of surfaces of the second degree, by M. G. Darboux.—The effect produced upon the motion of inclination of a bicycle by the lateral displacements given by the rider, by M. J. Boussinesq.—Note on some properties of the radiation of uranium and radio-active substances, by M. Henri Becquerel. The intensity of the uranium radiations, as measured by their photographic action, appears to undergo no diminution with time, since some uranium compounds enclosed in a leaden box since May 1896, and hence absolutely shut off from all known sources of radiation, still have the same action upon a photographic plate as when first set up. The rays do not appear to be capable of polarisation, all attempts to repeat two early experiments giving positive results in this direction having failed. Bodies, such as glass, receiving these rays, give off a secondary radiation very similar in nature to that observed by M. Sagnac for the X-rays.—On the explosive aptitude of acetylene when mixed with inert gases, by MM. Berthelot and Vieille. Two sets of mixtures were examined, acetylene and hydrogen, and acetylene and coal gas. These were gradually compressed, and the pressure zone determined within which the propagation of the explosive wave was possible.—Preparation and properties of crystallised calcium phosphide, by M. Henri Moissan. By carefully heating an intimate mixture of carbon and calcium phosphate in the electric furnace, it is possible to obtain a crystallised calcium phosphide containing only traces of calcium carbide and unreduced phosphate. The phosphide obtained in this way forms reddish-brown crystals, which fuse only at a very high temperature. Chlorine is without action in the cold, but a violent reaction sets in at 100° C. Analyses show that the composition of the phosphide is Ca_3P_2.—On the properties and applications of aluminium, by M. A. Ditte. Copper-aluminium alloys containing from three to six per cent. of copper are more readily attacked by aqueous solutions than aluminium itself; the copper remaining untouched forms innumerable couples tending to accelerate the solution of the more easily oxidisable metal.—Observations of Swift's comet (1899 *a*), made with the large equatorial of the Observatory of Bordeaux, by MM. G. Rayet and F. Courty.—Observations of the sun, made at the Observatory of Lyons, during the last quarter of 1898, by M. J. Guillaume. The results are formulated in three tables giving the number and area of sun-spots, their distribution in latitude, and the distribution of the faculæ in latitude.—On functions defined by a Taylor's series, by M. L. Leau.—On some arithmetical properties of analytical functions, by M. Paul Staeckel.—On the existence of fundamental functions, by M. W. Stekloff.—On functions of several variables, by M. H. Lebesgue.—On the magnetic elements in Roumania on January 1, 1895.—A question of priority concerning the equation $k - i/(k + 2)d =$ constant, between the dielectric constant and the density, by M. D. Negreano. This relation was given theoretically by H. A. Lorentz in 1880, and proved experimentally by the author in 1887 to be true for several hydrocarbons. The Mosotti-Clausius formula, $g = k - i/k + 2$, where g is the distance between the molecules, is not identical with the above, as the density does not appear.—On the Wehnelt commutator, by M. H. Pellat.—On a new apparatus designed to show the space relation of radiographs, and to search for foreign bodies, by M. A. Londe.—Transformation of the Galilean telescope into a range-finding instrument, by M. G. Humbert.—On the mixture of gases and the compressibility of gaseous mixtures, by MM. Daniel Berthelot and Paul Sacerdote. Experiments were carried out on mixtures of carbon dioxide and sulphur dioxide, air, and a mixture of hydrogen and oxygen.—On the decomposition of carbonic oxide in presence of metallic oxides, by M. O. Boudouard. The oxides of iron, nickel and cobalt were studied, at a temperature of 650°, the softening point of the glass tubes used, and the composition of the gaseous mixture determined as a function of time of contact with the oxide.—On the decomposition of carbon dioxide in presence of carbon, by M. O. Boudouard. A similar set to experiments to those described in the preceding paper, but with charcoal taking the place of the metallic oxides.—On the dissociation of mercuric oxide, by M. H. Pélabon.—Action of the bis-diazoic chlorides of benzidine, of ortho-toluidine, and of dianisidine upon the malonates of ethyl and methyl, by M. G. Favrel.—Detection of mercury in the produce of vines treated with mercurial broths, by MM. Leo Vignon and J. Perraud. The amounts of mercury found are so small that, in the author's opinion, the utilisation of mercuric chloride for the treatment of diseases of the vine is subject to no serious objections from the hygienic point of view; but the action on the plant itself is so serious, that for this reason alone corrosive sublimate should not be used in combating diseases of the vine.—Remarks by M. Berthelot on the preceding paper.—Toxic albumen extracted from the flesh of the eel, by M. Elophe Bénech.—Morbid predispositions of the puerperal period, by M. A. Charrin.—Action of the pancreas upon the diphtheric toxin, by MM. Charrin and Levaditi. The pancreas exerts an attenuating action upon bacterial toxins.—Death from continuous electrical currents, by MM. J. L. Prevost and F. Battelli.—Researches on the sensitive nerve terminations in voluntary striated muscle, by M. D. Poloumordwinoff.—*Botrytis cinerea* and the disease of *la toile*, by M. Beauverie.—On a tachylite from the bottom of the N. Atlantic, by M. P. Termier.—On the captive balloon ascents of March 24, by M. Léon Teisserenc de Bort.

DIARY OF SOCIETIES.

THURSDAY, APRIL 6.

LINNEAN SOCIETY, at 8.—On *Carex Wahlenbergiana*: C. B. Clarke, F.R.S.—On the Discovery and Development of Rhabdites in Cephalodiscus: F. J. Cole.

FRIDAY, APRIL 7.

GEOLOGISTS' ASSOCIATION, at 8.—The Geology of Brittany, with Special Reference to the Whitsuntide Excursion: Dr. Charles Barrois.

SATURDAY, APRIL 8.

GEOLOGISTS' ASSOCIATION.—Cycling Excursion—Winchfield to Wokingham. Director: H. W. Monckton.

MONDAY, APRIL 10.

SOCIETY OF CHEMICAL INDUSTRY, at 8.—The Industrial Technical Treatment of Sherry and of British Colonial Wines: Dr. J. T. W. Thudichum.
VICTORIA INSTITUTE, at 4.30.—Babylonian Deities: Theo. G. Pinches.

TUESDAY, APRIL 11.

ROYAL INSTITUTION, at 3.—Zebras and Zebra Hybrids: Prof. J. Cossar Ewart, F.R.S.
INSTITUTION OF CIVIL ENGINEERS, at 8.—Paper to be further discussed: Alloys of Iron and Nickel: Robert Abbott Hadfield.——And, time permitting: Buenos Aires Harbour Works: James Murray Dobson.
MINERALOGICAL SOCIETY, at 8.—On a Mass of Meteoric Iron lately found in Patagonia: L. Fletcher, F.R.S.—On Langbeinite from the Mayo Salt Mines, in the Punjab: F. R. Mallet.—On the Use and Advantages of a Three-Circle Goniometer: G. F. Herbert Smith.
ROYAL PHOTOGRAPHIC SOCIETY, at 8.—The Correct Exposures to be given to Photographs of the Corona: E. W. Maunder.

WEDNESDAY, APRIL 12.

SOCIETY OF ARTS, at 8.—Telephones: John Gavey.
GEOLOGICAL SOCIETY, at 8.—(1) Fossils in the University Museum, Oxford. I. Silurian Echinoidea and Ophiuroidea; (2) On the Occurrence of Sponge-Spicules in the Carboniferous Limestone of Derbyshire: Prof. W. J. Sollas, F.R.S.—On Spinel and Försterite from the Glenelg Limestone: C. T. Clough and Dr. Wm. Pollard.

THURSDAY, APRIL 13.

ROYAL INSTITUTION, at 3.—The Atmosphere: Prof. J. Dewar, F.R.S.
MATHEMATICAL SOCIETY, at 8.—Note on the Characteristic Invariants of an Asymmetric Optical System: T. J. Bromwich.—Concerning the Four Known Simple Linear Groups of Order 25920, with an Introduction to the Hyper-Abelian Linear Groups: Dr. L. E. Dickson.—On the Direct Determination of Stress in an Elastic Solid, and on the Stress in a Rotating Lamina: J. H. Michell.—The Theorem of Residuation, Noether's Theorem, and the Riemann-Roch Theorem: Dr. F. S. Macaulay.
INSTITUTION OF ELECTRICAL ENGINEERS, at 8.

FRIDAY, APRIL 14.

ROYAL INSTITUTION, at 9.—Earth Currents and Electric Traction: Prof A. W. Rücker, Sec. R.S.
ROYAL ASTRONOMICAL SOCIETY, at 8.
MALACOLOGICAL SOCIETY, at 8.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—On Birds and Stipules: Sir John Lubbock (Paul).—National Association for the Promotion of Technical and Secondary Education, 11th Annual Report (London).—Massachusetts Institute of Technology, Boston, Annual Catalogue, 1898–99 (Boston).—Text-Book of the Embryology of Invertebrates: Drs. Korschelt and Heider, Vol. 2, translated by M. Bernard (Sonnenschein).—Report of Observations of Injurious Insects, &c., during the Year 1898: E. A. Ormerod (Simpkin).

PAMPHLETS.—The Future of the Metric and Imperial Systems of Weights, &c.: J. Manning (Sonnenschein).—Une Excursion Electrotechnique en Suisse (Paris, Gauthier-Villars).

SERIALS.—Contemporary Review, April (Isbister).—National Review, April (Arnold).—National Geographic Magazine, March (Washington).—Lehrbuch der Allgemeinen Chemie: Dr. W. Ostwald, ii. Band, 2 Teil, 4 Liefg. (Leipzig).—Fortnightly Review, April (Chapman).—Reliquary and Illustrated Archæologist, April (Bemrose).—Journal of Botany, April (West).—Proceedings of the Royal Society of Victoria, Vol. xi., new series, Part 2 (Melbourne).—Journal of the Royal Agricultural Society of England, Vol. x. Part 1 (Murray).—Observatory, April (Taylor).—Notes from the Leyden Museum, October, 1898 (Leiden).—Minnesota Botanical Studies, Part 2 (Minneapolis).—Chambers's Biographical Dictionary, Part 1 (Chambers).—Ditto, English Dictionary, Part 1 (Chambers).—Natural Science, April (Pentland).

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