THURSDAY, JUNE 19, 1890.

BRITISH AND ORIENTAL CICADIDÆ.

Monograph of the British Cicadæ, or Tettigidæ. By G. B. Buckton, F.R.S. Illustrated by more than Four Hundred Coloured Drawings. (London: Macmillan and Co., 1890.)

A Monograph of Oriental Cicadidæ. By W. L. Distant. (Calcutta: Indian Museum. London: H. S. King and Co. 1890.)

THE insects forming the family of the Cicadidæ of Westwood are among the largest of the Homoptera, and by far the largest number of the known species are to be met with in the warm regions of the world. Some fifty years ago but one species of this family seems to have been recorded from Great Britain—it was found in the New Forest, and figured by Curtis as *Cicada anglica.* Curtis thought it did not sing, because a specimen kept in confinement by Mr. Dale for two or three days was mute. Kirby and Spence, however, were informed that it was very noisy, and, adds Prof. Westwood, "analogy would lead to the belief that it does sing, the drums of *C. orni* not being comparatively larger." Weaver found the pupa-case of this insect attached by the legs to the stem of a fern.

Great have been the changes within the last half-century, during which all the above-mentioned well-known names, but that of the respected Professor of Zoology at Oxford, have been numbered among those of the dead; and now the number of the species of the "British Cicadæ"—using this word, however, in a wider sense—is about 230. Mr. G. B. Buckton, F.R.S., so well known for his excellent monograph of the British Aphidæ, has published the first two parts of an illustrated monograph of our native "froghoppers and grassflies."

Although not of large size, like their tropical brethren, our native species are of great interest, and as to this date there has been no serious attempt to publish an adequately illustrated history of even the European forms, the appearance of this monograph is all the more welcome, and its publication will, no doubt, very greatly facilitate the study of these insects.

It is proposed that this monograph shall be published in eight quarterly parts, and these will be illustrated by about eighty coloured plates. Part 1 was issued in January, and Part 2 in April of this year.

The monograph opens with an introduction, in which the author tells us that he proposes to treat his subject under the following heads: Etymology, and the ancient notices of the Cicada or Tetix; classical allusions and poetic myths relating to them; a biographical sketch of the writings and investigations of authors who have considered the subject; a terminology and description of the parts available for classification; general remarks as to their life-history, reproduction, &c.; diagnosis of species, accompanied by coloured representations of the British species of these insects; notes on variation and distribution; remarks as to the probable antiquity of the group, as shown by their remains in the rocks, amber, and fossil resins; and, in addition to all this, in an appendix, there is to be a bibliographical list of the chief modern authors who have studied the Cicadæ; and a short list of ancient and modern quotations, for reference and for use by the curious. Certainly, we have here the programme of a very large and entertaining volume.

We would suggest that Mr. Buckton should not limit his bibliographical list to the "chief authors who have studied the group," but that he should, if even at the cost of cutting out some of the folk-lore, make this list a complete one. Indeed, if we are to judge of the promise by the present performance, it would perhaps be wiser for the author to dwell more on the descriptive and bibliographical portions of his work, than on those appertaining to the literature thereof, for no small research, of a peculiarly special character, would be necessary before one could successfully write the history of the ancient notices of the Cicadæ, recall all the classical allusions that have been made to them, or even give an account of the early scientific writings about them. We agree with the author that "the ordinary scope of a monograph is the description of the forms, life-history, distribution, &c., of the species contained in it," and hence we regret that he should have added so much to his labours by venturing, in this volume, on other fields of research, with which there seems to be some proof that he has not been so familiar. Thus, on p. iii. of the introduction we read that "the first English author who wrote on the Cicada was Dr. Thomas Moufat, or Mouffet, an English physician, who flourished in the reign of James I. In 1634 he wrote, in folio, a curious Latin treatise on zoology, having for its title, 'Insectorum sive minimorum Animalium Theatrum.'" On p. xx. we find a short account of this book, which is said to be "somewhat rare"; it is therefore reasonable to conclude that Moufet's volume was in our author's hands, but, if so, he could never have read over the dedicatory epistle, from which he, however, quotes, with any care. If we are able to judge by his spelling of Moufet's name, even the title-page was not carefully examined. A glance at Hagen's "Bibliotheca Entomologica," or at Burmeister's "Manual of Entomology," would have guarded the author from a great many mistakes.

Moufet was a physician living in London; he was born in 1550, and, according to Burmeister, he died in 1604. From the little known of him, it does not appear that he was an entomologist. His little volume, "Nosomantica," treating of the prognosis of disease, was published in 1588, and he died "in poverty." Conrad Gesner had laboured hard to complete his "Historia Animalium" by a history of insects, based on Wotton, but he died before he had made much progress with it. Pennius took up the subject, worked at it for 15 years, and then too died, leaving many drawings and fragments of descriptions in manuscript; whereupon Moufet tells us he arranged these descriptions in order, "added to them the light of oratory which Pennius wanted, and so constructed" the history referred to. Born during the reign of Mary, Moufet flourished in the days of Elizabeth, and died at or about the time King James ascended the throne. In 1634, when the work of Wotton (of Oxford), Gesner, and Pennius first saw the light, as "wove together" by Moufet, Charles I. was on the English throne; and, as can be read in the "Epistola," not of Moufet, but of Sir Theodore de Mayerne, in which he dedicates the work to the illustrious Sir William Paddy, Moufet constructed the "History" hoping to acquire fame by dedicating his compilation to the Virgin Queen; but she, as Hume notes, was no great lover of literary or scientific men, and the dedication appears not to have been accepted, and shortly after her death Moufet also died. "Great poverty at home" then delayed the publication, so the book lay for a long time in obscurity, until it was offered to Sir T. de Mayerne by Moufet's apothecary, Darnello. It lay even then in de Mayerne's library for a long time, subject to the attacks of "moths and cockroaches," and that through no fault of his, "but the printers demanded too much money." In the month of May 1634, dedicated to no sovereign, but to the "ever illustrious Paddy," and having been approved of by "Guliel. Bray, of Lambeth Palace," it was published in London by Hope.

Remembering who wrote the "Epistola," it is funny to find Mr. Buckton writing, "Moufat was a true naturalist, and well loved the subject of which he treated. In his Epistola he dilates on the pleasure, &c., felt by the operator, &c." Instead of Moufet one should read Th. de Mayerne, and the operator was William Paddy, and whoever may have been the Frenchman referred to in this same paragraph, assuredly it was not Réaumur, for he was not born until half a century after the publication of Moufet's work. Perhaps enough in the way of criticism has been written about this subject, but it seems right to call attention to the numerous errors of translation from Moufet's Latin text, which errors are the more to be deplored as the translation given in the Rev. Edw. Topsel's "History of Animals," published in 1658, is fairly accurate, and could have been easily referred to. In referring to the text where Moufet writes about the song of the Cicadæ, Mr. Buckton deplores (p. xii.) its want of clearness, and even ventures to emend it, but Moufet's text is not correctly quoted, and in the translation the whole sense of the original is quite lost. Moufet, no doubt, omitted the word λαλιστέραν from his quotation, but the words cited by Athenæus mean, "I have never seen one more loquacious, no, neither a Cercopia, nor a jay, nor a nightingale, nor a Tettiga, nor a turtledove."

Moufet may or may not have been disgusted with the luxury of his day, and possibly partook of the Puritanic spirit of the age (p. xxi.), but he could not have intended that the word "magistræ" should have been translated (even with a ?) as "mistresses": the whole of the translation here is indeed curious; the "health-giving diet of one's forefathers," is interpreted to mean the "health-giving tables of the *better* sort"!

Leaving this portion of the subject, we pass on to briefly notice the diagnoses of the species and the coloured plates. We trust that we are not hypercritical if we suggest that the student should have had some clue to the classification adopted in this portion of the work; such may be given in the introduction, but, if so, we presume it will have to explain the sequence of the species and genera in the text, which surely ought to have explained itself. Thus, under the heading, British Cicadæ, comes the genus Cicadetta, and this is followed by "II. Membracidæ, Stål.," with two genera, Centrotus and Gargara, after which we find "Fulgorinæ, Stål.," with III. Tettigometridæ, IV. Issidæ, and V. Cixiidæ; but the next group, the Delphacidæ, is not numbered, so there is no help given one from the name-endings, or the numerals, or, we may add, the typography, as to what Mr. Buckton regards as a family, or a sub-family, nor can we be quite sure always even of the names that he would adopt for the forms described, as, for example, the species given on pp. 28 and 29.

We have thus pointed out a few of the blemishes that to some slight extent mar the early pages of this important and interesting work, and the literary student could easily point out many more in the already published pages of the introductory chapter, still this part is of but secondary value to the entomologist, and a little more attention to the part containing the diagnoses of the genera and species, and the recording the habitats of *all* the British forms, are points that can be easily attended to in the future parts. Of 41 species of the genus Liburnia recorded as British, twenty-seven have no British habitat quoted, unless, indeed, in some few cases, such phrases as "common on marshy lands," "uncommon in England," and "from Mr. Douglas's collection," are to be regarded as such.

The figures are drawn on stone by the author: those of the perfect insects enlarged are very characteristic and pretty, those of the anatomical details would be improved by a little more distinctness in their outlines.

While the British Cicadæ are thus being monographed by Mr. Buckton, those of the Orient are being monographed by Mr. W. L. Distant, the first two parts of a "Monograph of Oriental Cicadidæ" having been published by order of the trustees of the Indian Museum, Calcutta. Part I. is dated July 1889, and Part II. December 1889. They are in large quarto, each containing twenty-four pages and two plates. We have no words but those of praise for this splendid work, which the trustees of the Indian Museum are to be warmly congratulated on publishing. Mr. Distant leaves us in no doubt as to the forms of which he treats: they belong to the sub-order of the Homoptera, and to the family of the Cicadidæ; for this family he adopts two divisions—the sub-families Cicadinæ and the Tibiceninæ—while the diagnoses of the genera and the species leave nothing to be desired. The typography is excellent. A word of commendation is also due to Mr. Horace Knight, to whom the drawing of the figures from nature has been intrusted; one figure on each plate of a species of each genus is represented in colours.

Mr. Distant proposes in this work to fully describe and figure all the species known from continental India and Ceylon, the islands in the Bay of Bengal, in Burma, Tenasserim, the Malay Peninsula, the length and breadth of the Malayan Archipelago, including, but extending eastward of, New Guinea; and, lastly, Eastern Asia, including China and Japan. Thus this monograph will include all the Oriental species. We trust the author may bring this work, so auspiciously commenced, to a successful issue.

MACHINE DESIGN.

The Elements of Machine Design. By Prof. W. Cawthorne Unwin, F.R.S. (London and New York: Longmans, Green, and Co., 1890.)

THIS is the eleventh edition of an excellent and most useful book for engineers and students in the engineering departments in our technical colleges. Prof. Unwin is so well known in the profession that any work of his is sure to receive full attention and careful study; for even in the present day one unfortunately often sees machinery and engineers' tools, the design and construction of which give us cause to wonder how they manage to work at all. The author is one of those Professors whose books are eagerly sought after by practical men for guidance. To say this is to say very much indeed, for engineers have to make their machines "pay" and creditable to themselves; a bad machine tool in a shop is very soon found out by the repairs it requires, and the quality of the work it can produce.

In this, the new edition of the work, the author has found it necessary to divide the book into two parts, the first of which is now before us. It deals principally with the general principles of design, fastenings, and transmissive machinery.

The author, well knowing the conditions of every-day work in the drawing office and shops, has, we are glad to observe, used throughout the standard English units of weight and length. Another good point is that the mathematics used in the calculations are well within the range of the average engineer; at the same time accuracy is obtained in the results, although useless refinements are omitted.

In the chapters on rivetted joints, and the one on journals and the friction of the same in their bearings, the experimental results obtained from experiments inaugurated by the Institution of Mechanical Engineers are fully described and the results tabulated; and they are embodied in the chapters in many useful forms suitable for the guidance of engineers. Under the heading of rivetted joints, it may be interesting to observe that the question of punching *versus* drilling steel or iron plates has solved itself in, at any rate, one first class bridge works in the north, and in this particular works the invariable practice is to drill all the holes throughout the bridge work because it is cheaper, with suitable machinery, to do so. On p. 97 the author does not say whether his remarks apply to boilers as well as other constructions, but to punch an iron boiler-plate is considered bad practice, and a punched steel plate, even if it is annealed afterwards, certainly comes under the same head.

In most of the locomotive works in this country the boilers are drilled, finally, after all the plates are in position, the barrel being fitted to the fire-box casing after each portion has been drilled; and certainly no good locomotive builder would use a punched steel plate in a boiler, even after annealing. One eminent locomotive superintendent, we believe, uses punched steel boiler shell plates; this is probably the only exception in this country, and is generally considered risky and not sound practice. On p. 140 the system of applying the direct stays to the crown of locomotive fire-boxes might have been added and illustrated with advantage.

The illustrations are particularly good, and all represent good practice. The thanks of engineers are due to Prof. Unwin for placing within their reach a volume in which theory and practice are judiciously treated to their great advantage. N. J. L.

OUR BOOK SHELF.

Investigation of the Fur-Seal and other Fisheries of Alaska. Report from the Committee on Merchant Marine and Fisheries of the House of Representatives. (Washington: Government Printing Office, 1889.)

THE fisheries of Alaska are among the great questions of the day, and those of our legislators who wish to take part in the inevitable debate on the subject will do well to possess themselves of the present volume, and digest the large amount of information that it contains. As is well known, the fur-seal fisheries of the Northern Pacific, which supply the ladies' jackets so much prized in Europe, are rented by the Alaska Commercial Company, and produce a considerable revenue to the United States. It is therefore a standing grievance among our American friends, that, as shown by the testimony collected in the present Report, the number of seals on the Prybiloff Islands, whence the principal supply is derived, "has materially diminished during the last two or three years." This is attributed to the fact that a large number of British vessels, "manned by expert Indian seal-hunters," have frequented Bering's Sea, and destroyed "hundreds of thousands of fur-seals." It is shown that, of the seals thus killed on the ocean, not more than one in seven is secured, because a wounded seal sinks so quickly. Thus, for every thousand seal-skins realized by the British sealing-vessels, some seven thousand seals are killed. Now, during the three years 1886–88, it appears that the number of what the Americans call "illicit skins" secured by the British traders was over 97,000, so that, if these calculations are correct, it follows that nearly three-quarters of a million of fur-seals were destroyed by British vessels during that period. American citizens, we are told, "have respected the law, and have made no attempt to take the seals."

While we fully sympathize with the Americans in their view that the fur-seal is a most useful animal, and deserves protection by special legislation, it seems to be doubtful whether they have any right, in their praiseworthy efforts in this direction, to turn a large tract of the Northern Pacific into a "*mare clausum*," without obtaining the consent of other nations. But the arguments by which they justify this somewhat strong proceeding are fully set forth in the present volume, and deserve special study. We may also commend Mr. Dunn's Report as containing a large amount of information on the history and habits of *Callorhinus ursinus*, and some excellently drawn illustrations of what the Americans consider to be the only legitimate method of obtaining this animal's skin.

Pond Life: Algæ and Allied Forms. By T. Spencer Smithson. (London: Swan Sonnenschein and Co., 1890.)

"THE Young Collector" series, to which this hand-book belongs, deals generally with classes of objects which can be permanently preserved. The present volume describes plants which, as the author says, "are not well adapted for preservation." His task, therefore, has been to give an account of the structure and habits of these plants, and to explain how they may be procured in the best form for observation. He begins with information about the apparatus required, then treats of the Algæ as a class, and the main divisions into which they have been separated by botanists, and in most of the remaining part of the book describes species, "choosing as types of each genus such species as are most likely to be met with, and leaving out those which are either rare or possess few points of interest for the beginner." Mr. Smithson himself points out that the volume leaves much to be sought elsewhere; but, if used intelligently, it will do sound work by preparing the way for wider study.

Rambles and Reveries of a Naturalist. By the Rev. William Spiers, M.A., F.G.S., &c. (London: Charles H. Kelly, 1890.)

MR. SPIERS does not profess to give in this little book a full account of any one of the subjects with which he deals. His aim has been "to awaken or to stimulate a love for Nature in the minds of some who may not as yet have suspected what wondrous and ever-varying beauty lies everywhere about us, in ditch and pond, in rock and stone, in river and sea, on earth and in the skies." With this end in view, he describes, in a series of short sketches, various phenomena which he himself has had opportunities of observing; and he does his work so well that to a good many readers his book may be of considerable service. There is nothing new or brilliant in Mr. Spiers's descriptions; but they are fresh and clear, and display not only a genuine love for Nature, but a capacity for appreciating the scientific significance of many different orders of facts. Besides other essays, the volume includes papers on seaweeds, rambles in Cornwall, a visit to the Channel Tunnel, St. Hilda's snake-stones, tiny rock-builders, and an evening at the microscope.

Sketches of British Sporting Fishes. By John Watson. (London: Chapman and Hall, 1890.)

A PREFATORY note to the "Sketches" tells us that "the subject-matter has, for the most part, been gleaned directly from the waterside, and should be looked upon more as the notes of a naturalist than the jottings of an angler." Accordingly, it was with anticipation of interest that we turned to the opening chapter, on salmon.

So little is known of the natural history of the salmon, and so great is its value, both for sport and for food, that we eagerly scan the pages of a naturalist and an angler who may tell us what he has seen and knows. Mr. Watson has nothing to tell us. He disposes of the salmon in 12 pages, and the impression produced upon us is that his acquaintance with that noble fish is confined to the fishmonger's slab, and to the dinner-table.

The chapter on trout is little more satisfactory. That on grayling is by another hand.

LETTERS TO THE EDITOR.

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Coral Reefs, Fossil and Recent.

DR. VON LENDENFELD has (June 12, p. 148) quoted cases to contest my statement that there are no coral reefs whose slopes are known to descend steeply to greater depths than about 4000 feet. I must take these seriatim.

(1) "Fitzroy's no-bottom sounding of 7200 feet at a distance of 6600 feet from the breakers at Keeling Island."

I hope I shall not be misunderstood when I say that I cannot accept this as conclusive evidence. Experience daily shows us how little confidence can be placed in a single deep sounding, taken before the days of suitable apparatus, and with no description of the means employed, either to fix the position exactly, or to obtain the cast. It may be correct, but on the other hand it may not be.

(2) "Maldives, &c., rise from a bank of 1000 fathoms very abruptly."

I cannot find any deep soundings near these groups at all. One sounding of 1243 fathoms at a distance of 10 miles is the closest.

(3) "Bermudas rise abruptly out of a depth of 12,000 to 13,000 feet."

There is only one sounding of 12,000 feet anywhere near Bermuda, and as that is six miles from the nearest shallow water, the isolated Challenger Bank, it represents a slope of only 19°.

In point of fact, very few slopes of coral formations have yet been accurately measured. Among the most remarkable that I know are:—

Bougainville Reef in Coral Sea, which drops perpendicularly from the water-level to 360 feet; at a mean slope of 76° to 780 feet; and at 53° to 1500 feet.

Dart Reef, in same sea, has a mean slope of 64° to 1200 feet.

Macclesfield Bank, a so-called, "drowned" atoll, in China Sea, has a mean slope of 51° to 4200 feet, and possibly more.

The existing conditions of the steep outer slopes of atolls are sufficiently astonishing. All I wish to maintain is that we should argue upon proven facts, and not assumptions, which tend to exaggerate difficulties, and to lead us astray.

With regard to Dr. von Lendenfeld's explanation of the limitation of depths of lagoons, I must await a better before I am convinced. My point is that it is very remarkable that no matter how vast the lagoon, and how deep the steep outer slopes, no lagoon has more than a certain depth, and that such a limited depth that isolated coral heads can spring out of it; and I cannot make this general fact fit with a general theory of subsidence, even when varied by occasional elevations.

The "drowned" atolls are no deeper than others whose rims are at the surface; *vide* Great Chagos Bank, and Suadiva Atoll in Maldives. W. J. L. WHARTON.

June 14.

ELECTRO-MAGNETIC RADIATION.[1]

IN order to discover whether actions are propagated in time or instantaneously, we may employ the principle of interference to measure the wave-length of a periodic disturbance, and determine whether it is finite or no. This is the principle employed by Hertz to prove experimentally Maxwell's theory as to the rate of propagation of electro-magnetic waves. In order to confine the experiments within reasonable limits we require short waves, of a few metres' length at most. As the highest audible note gives waves of five or six miles long, and our eyes are sensitive only to unmanageably short waves, it is necessary to generate and observe waves whose frequency is intermediate between them, of some hundred million vibrations per second or so. For this purpose we may use a pair of conducting surfaces connected by a shorter or longer wire, in which is interposed a spark-gap of some few millimetres' length. When the conductors are charged by a coil or electrical machine to a sufficiently high difference of potential for a spark to be formed between them, they discharge in a series of oscillations, whose period for systems of similar shape is inversely proportional to the linear dimensions of the system so long as the surrounding medium is unaltered. When the surrounding non-conducting medium changes, the period depends on the electric and magnetic specific inductive capacities of this medium. Two such systems were shown: a large one, whose frequency was about 60 millions per second; and a small one, whose frequency was about 500 millions per second. The large one consisted of two flat plates, about 30 cm. square and 60 cm. apart, and arranged in the same way as is described by Prof. Hertz in *Wiedemann's Annalen*, April 1888. The

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[1] Friday Evening Lecture delivered at the Royal Institution, on March 21, by Prof. G. F. Fitzgerald, F.R.S.

smaller vibrating system consisted of two short brass cylinders terminating in gilt brass balls of the same size, and arranged in the same way as the smaller system described by Prof. Hertz in *Wiedemann's Annalen*, March 1889. This latter system was placed in the focal line of a cylindrical parabolic mirror of thin zinc plate, such as that described by Prof. Hertz in this paper.

These generators of electro-magnetic oscillations may be called electric oscillators, as the electric charge oscillates from end to end. A circle of wire, or a coil in which an alternating current ran, or, if such a thing were attainable, a magnet alternating in polarity, might be called a magnetic oscillator. A ring magnet with a closed magnetic circuit is essentially an electric oscillator, while a ring of ring magnets would be essentially a magnetic oscillator again. The elementary theory of a magnetic oscillator can be derived from that of an electric oscillator by simply interchanging electric and magnetic force. Electricity and magnetism would be essentially interchangeable if such a thing existed as magnetic conduction. The only magnetic currents we know are magnetic displacement currents and convection currents, such as are used in unipolar and some other dynamos. It is in this difference that we must look for the difference between electricity and magnetism.

In order to observe the existence of these electro-magnetic oscillations we can employ the principle of resonance to generate oscillations in a system whose free period of oscillation is the same. A magnetic receiver may be employed, consisting of a single incomplete circle of wire broken by a very minute spark-gap, across which a spark leaps when the oscillations in the wire become sufficiently intense. In order that a large audience may observe the occurrence of sparks, the terminals of a galvanometer circuit were connected, one with one side of the spark-gap, and the other with a fine point which could be approached very close to the other side of the spark-gap. It was observed that, when a spark occurred in the gap, a spark could also be arranged to occur into the galvanometer circuit, and, with a delicate long-coil galvanometer (that used had 40,000 ohms resistance), a very marked deflection can be produced whenever a spark occurs. This arrangement we have only succeeded in working comparatively close to the generator, because the delicacy required in adjusting the two spark-gaps is so great. It can, however, be employed to show that the sparks produced in this magnetic resonant circuit are due to resonance by removing this receiver from the generator to such a distance that sparks only just occur, and then substituting for the single circuit a double circuit, which, except for resonance, should have a greater action than the single one, but which stops the sparking altogether. An electric receiver was also used, which was identical with the generator, and had a corresponding, only much smaller, spark-gap between the two plates. When the plates are connected with the terminals of the galvanometer, upon the occurrence of each spark the galvanometer is deflected. It is not so easy to obtain sparks when the plates are connected with the galvanometer as when they are insulated, and it is this that has limited the use of this method of observation. By making the first metre or so of the wires to the galvanometer of extremely fine wire, so as to reduce their capacity, we have found that the difficulty of getting sparks is less than with thick wires. We have not observed any effect due to the thickness of the wires after a short distance from the receiver.

In the case of the small oscillator, a receiver exactly like the one described by Prof. Hertz in his second paper already quoted was placed in the focal line of a cylindrical parabolic mirror, and its receiving wires were connected with the wires leading to the galvanometer by some very fine brass wire. With the large-sized generator and receiver, which were placed about 3 metres apart, it was shown that the sparking was stopped by placing a thin zinc sheet so as to reflect the radiations from a point close behind the receiver. By means of a long india-rubber tube hung from the ceiling, it was shown how, when waves are propagated to a point whence they are reflected, the direct and reflected waves interfering produce a system of loops and nodes, with a node at the reflecting point. It was explained that these nodes, though places of zero displacement, were places of maximum rotation, and that the axis of rotation was at right angles to the direction of displacement. It was explained that an analogous state of affairs existed in the electro-magnetic vibrations. If the electric force be taken as analogous to the displacement of the rope, the magnetic may be taken as analogous to its rotation, and the two are at right angles to one another. In the ether the electric node is a magnetic loop, and *vice versâ*. Though the two are separated in loops and nodes, they exist simultaneously in a simple wave propagation, just as in a rope when propagating waves in one direction the crest of maximum displacement is also that of maximum rotation. It was explained that by placing the reflector at a quarter of a wave-length from the receiver this would be at an electric loop, and have its sparking increased. It may thus be shown that there are a series of loops and nodes produced by reflection of these electromagnetic forces, like those produced in any other case of reflected wave-propagation. This was Hertz's fundamental experiment, by which he proved that electro-magnetic actions are propagated in time, and by some approximate calculations he verified Maxwell's theory that the rate of propagation is the same as that of light. It follows that the luminiferous ether is experimentally shown to be the medium to which electric and magnetic actions are due, and that the electro-magnetic waves we have been studying are really only very long light waves.

A rather interesting deduction from Maxwell's theory is that light incident on any body that absorbs or reflects it should press upon it and tend to move it away from the source of light. Illustrating this, an experiment was shown with an alternating current passing through an electro-magnet, in front of which a good conducting plate of silver was suspended. When the alternating current was turned on the silver was repelled. It was explained that as the silver could only be affected by what was going on in its own neighbourhood, and that if sufficiently powerful radiations from a distant source were falling on the silver, it would be acted on by alternating magnetic forces, this experiment was in effect an experiment on the repulsion of light, which was too small to have been yet observed, even in the case of concentrated sunshine. These slow vibrations are not stopped by a sheet of zinc, though much reduced by a magnetic sheet like tin-plate, though the rapid ones are quite stopped by either—thus showing that wave-propagation in a conductor is of the nature of a diffusion.

In all cases of diffusion where we consider the limits of the problem, terms involving the momentum of the parts of the body must be introduced. It appears from elementary theories of diffusion as if it were propagated instantaneously, but no action can be propagated from molecule to molecule, in air, for instance, faster than the molecules move, *i.e.* at a rate comparable with that of sound. In electro-magnetic theory corresponding terms come in by introducing displacement currents in conductors, and it seems impossible but that some such terms should be introduced, as otherwise electro-magnetic action would be propagated instantaneously in conductors. The propagation of light through electrolytes, and the too great transparency of gold leaf, point in the same direction.

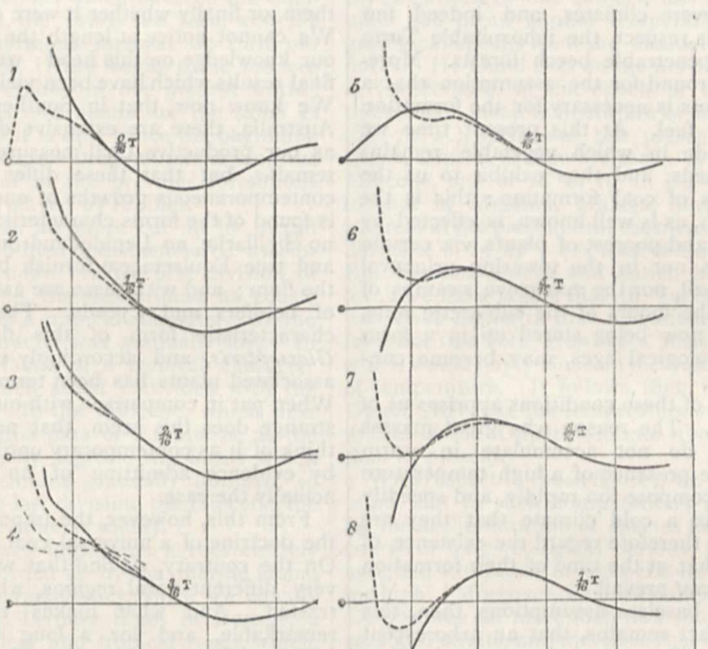
The constitution of these waves was then considered, and it was explained that if magnetic forces are analogous to the rotation of the elements of a wave, then an ordinary solid cannot be analogous to the ether because the latter may have a constant magnetic force existing in it for any length of time, while an elastic solid cannot have continuous rotation of its elements in one direction existing within it. The most satisfactory model, with properties quite analogous to those of the ether, is one consisting of wheels geared with elastic bands. The wheels can rotate continuously in one directionn, and their rotation is the analogue of magnetic force. The elastic bands are stretched by a difference of rotation of the wheels, and introduce stresses quite analogous to electric forces. By making the elastic bands of lines of governor balls, the whole model may have only kinetic energy, and so represent a fundamental theory. Such a model can represent media differing in electric and magnetic inductive capacity. If the elasticity of the bands be less in one region than another, such a region represents a body of higher electric inductive capacity, and waves would be propagated more slowly in it. A region in which the masses of the wheels was large would be one of high magnetic inductive capacity. A region where the bands slipped would be a conducting region. Such a model, unlike most others proposed, illustrates both electric and magnetic forces and their inter-relations, and consequently light propagation.

In the neighbourhood of an electric generator the general distribution of the electric and magnetic forces is easily seen. The electric lines of force must lie in planes passing through the axis of the generator, while the lines of magnetic force lie in circles round this axis and perpendicular to the lines of electric force. It is thus evident that the wave is, at least originally, polarized. To show this, the small-sized oscillators with parabolic mirrors were used, and a light square frame, on which wires parallel to one direction were strung, was interposed between the mirrors. It was shown that such a system of wires was opaque to the radiation when the wires were parallel to the electric force, but was quite transparent when the frame was turned so that the wires were parallel to the magnetic force. It behaved just like a tourmaline to polarized light. It is of great interest to verify experimentally Maxwell's theory that the plane of polarization of light is the plane of the magnetic force. This has been done by Mr. Trouton, who has shown that these radiations are not reflected at the polarizing angle by the surface of a non-conductor, when the plane of the magnetic force in the incident vibration is perpendicular to the plane of incidence, but the radiations are reflected at all angles of incidence when the plane of the magnetic force coincides with the plane of incidence. Thus the long-standing dispute as to the direction of vibration of light in a polarized ray has been at last experimentally determined. The electric and magnetic forces are not simultaneous near the oscillator. The electric force is greatest when the electrification is greatest, and the magnetic force when the current is greatest, which occurs when the electrification is zero: thus the two, when near the oscillator, differ in phase by a quarter of a period. In the waves, as existing far from the oscillator, they are always in the same phase. It is interesting to see how one gains on the other. It may be worth observing, again, that though what follows deals with electric oscillators, the theory of magnetic oscillators is just the same, only that the distribution of magnetic and electric forces must be interchanged. Diagrams drawn from Hertz's figures published in *Wiedemann's Annalen* for January 1889, and in NATURE, vol. xxxix. p. 451, and in the *Philosophical Magazine* for March 1890, were thrown on the screen in succession, and it was pointed out how the electric wave, which might be likened to a diverging whirl ring, was generated, not at the oscillator, but at a point about a quarter of a wave-length on each side of the oscillator, while it was explained that the magnetic force wave starts from the oscillator. It thus appears how one gains the quarter-period on the other. The outflow of the waves was exhibited by causing the images to succeed one another rapidly by means of a zoëtrope, in which all the light is used and the succession of images formed by having a separate lens for each picture and rotating the beam of light so as to illuminate the pictures in rapid succession.

As the direction of flow of energy in an electro-magnetic field depends on the directions of electric and magnetic force, being reversed when either of these is reversed, it follows that in the neighbourhood of the oscillator the energy of the field alternates between the electric and magnetic forms, and that it is only the energy beyond about a quarter of the wave-length from the oscilllator which is wholly radiated away during each vibration. It follows that in ordinary electro-magnetic alternating currents at from 100 to 200 alternations per second, it is only the energy which is some 3000 miles away which is lost. If an electro-magnetic wave, having magnetic force comparable to that near an ordinary electro-magnet, were producible, the power of the radiation would be stupendous. If we consider the possible radiating power of an atom by calculating it upon the hypothesis that the atomic charge oscillates across the diameter of the atom, we find that it may be millions of millions of times as great as Prof. Wiedemann has found to be the radiating power of a sodium atom in a Bunsen burner, so that, if there is reason to think that any greater oscillation might disintegrate the atom, it is evident that we are still a long way from doing so. It is to be observed that ordinary light-waves are very much longer than the period of the vibration above referred to. Dr. Lodge has pointed out that quite large oscillators in comparison to molecules—namely, about the size of the rods and cones in the retina—are of the size to resound to light-waves of the length we see, and so might be used to generate such waves. This seems to show that the electro-magnetic structure of an atom must be more complicated than a small sphere or other simple shape with an oscillating charge on it, for the period of vibration of a small system can be made long by making the system complex, *e.g.* a small Leyden jar of large capacity with a long wire wound many times round connecting its coats, could easily be constructed to produce electro-magnetic waves whose length would bear the same proportion to the size of the jar as ordinary light-waves do to an atom. The rate at which the energy of a Hertzian vibrator is transferred to the ether is so great that we would expect an atom to possess the great radiating power it has. This shows, on the other hand, how completely the vibrations of an atom must be forced by the vibrations of the ether in its neighbourhood, so that atoms, being close compared with a wave-length, are, in any given small space, probably in similar phases of vibration. It is interesting to consider this in connection with the action of molecules in collision as to how far the forces between molecules after collision is the same as before. In the same connection the existence of intra-atomic electro-magnetic oscillations is interesting in the theories of anomalous dispersion. An electro-magnetic model of a prism with anomalous dispersion might be constructed out of pitch, through which conductors, each with the same rate of electro-magnetic oscillation, were dispersed. In theories of dispersion a dissipation of energy is assumed, and it may be the radiation of the induced electro-magnetic vibrations. These can evidently never be greater than the incident electro-magnetic vibration, on account of this radiation of their own energy. In some theories a vibration of something much less than the whole molecule is assumed, and the possibility of intra-atomic electro-magnetic oscillations would account for this. Some such assumption seems also required, in order to explain such secondary, if not tertiary, actions as the Hall effect and the rotation of the plane of polarization of light, which are, apparently at least, secondary actions due to a reaction of the matter set in motion by the radiation on this radiation.

Some further diagrams were exhibited, plotted from Hertz's theory by Mr. Trouton, to whom much of the matter in this paper is due. They are here reproduced, and show eight simultaneous positions of the electric and magnetic waves during a semi-oscillation of an electric oscillator. The dotted line shows the electric force at various points, and the continuous line the magnetic force. In the first diagram the magnetic force is at its maximum near the origin, while the electric force there is zero. In the second the magnetic energy near the origin has partly turned into electric energy, and consequently electric force begins. The succeeding figures show how the magnetic force decreases near the origin, while the electric force grows, and the waves already thrown off spread away. The change of magnetic force between Figs. 4 and 5 is so rapid, that a few dashed lines, showing interpolated positions, are introduced to show how it proceeds. It will be observed how a hollow comes in the line showing electric force, which gradually increases, and, crossing the line of zero force at about a quarter of a wave-length from the origin, is the source of the electric wave, which, starting with this odds, picks up and remains thenceforward coincident with the magnetic wave. From this origin of electric waves they spread out along with the magnetic waves and in towards the origin, to be reproduced again from this point on the next vibration. These electric and magnetic forces here shown as coincident are, of course, in space in directions at right angles to one another, as already explained. The corresponding diagrams for a magnetic oscillator are got by interchanging the electric and magnetic forces.

A further experiment was shown to illustrate how waves of transverse vibration can be propagated along a straight hollow vortex in water. It was stated that what seemed a possible theory of ether and matter was that space was full of such infinite vortices in every direction, and that among them closed vortex rings represented matter threading its way through the ether. This hypothesis explains the differences in Nature as differences of motion. If it be true, ether, matter, gold, air, wood, brains, are but different motions. Where alone we can know what motion in itself is—that is, in our own brains—we *know* nothing but thought. Can we resist the conclusion that all motion is thought? Not that contradiction in terms, unconscious thought, but living thought; that all Nature is the language of One in whom we live, and move, and have our being.



THE CLIMATES OF PAST AGES.[1]

II.

We need not enter on a detailed description of the other vegetable types of the Coal-measure formation; we can only note the abundant occurrence of tree-ferns, and the existence of not very numerous conifers, which amid this strange vegetation are the forms most nearly related to those of our present world.

The geographical extent of this typical flora was extraordinarily great; we trace it from the shores of the Atlantic through the northern half of the Old World to China, and it is also greatly developed in the eastern half of the United States. There, and in China, are the greatest developments of beds of coal. Besides these, we find similar deposits with nearly the same vegetation in the far north, in the American polar archipelago, in Spitzbergen, and Nova Zembla. It is these facts that have led to the conclusion, already mentioned, that in the Carboniferous period a uniform climate prevailed from the equator to the pole, together with a dense atmosphere rich in carbon-dioxide, and impenetrable to the solar rays. And yet a simple examination of the facts assures us that all these suppositions are groundless. In so far as regards the character of the flora, we really know nothing of the temperature requisite to the Calamites, Lepidodendra,

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[1] Translation of a Lecture delivered by the late Dr. M. Neumayr before the Society for the Dissemination of Natural Science, at Vienna, on January 2, 1889. Continued from p. 151.

Sigillariæ, and other extinct types. Conifers grow now in very severe climates, and only the tree-ferns really indicate warm climatic conditions. At the present day their chief development is in the tropics, and they require, not indeed great heat, but the absence of frost. We do not, however, know that this was equally the case in former ages; in the Carboniferous period, the highest division of the vegetable kingdom, now so dominant, the flowering plants, were either non-existent, or were sparsely represented only by a few early forms, and it is by no means improbable that these types in their gradual extension have exterminated the tree-ferns in the colder regions to which they formerly extended, and that these latter have lost the power which they once possessed of withstanding frost.

Another fact that has been adduced to prove the former prevalence of a warm climate, is the great thickness of the beds of coal, which, it was assumed, could only have been formed by a luxuriant vegetation stimulated by a high temperature. But this also is incorrect; remarkably rich plant-growths are to be met with also in countries with very severe climates, and indeed few countries surpass, in this respect, the inhospitable Terra del Fuego, with its impenetrable beech forests. Moreover, there is no good ground for the assumption that a luxuriant growth of plants is necessary for the formation of thick beds of fossil fuel. At this present time we know of but one mode in which vegetable remains accumulate in thick beds, and thus exhibit to us the first step of the process of coal formation: this is the formation of peat, which, as is well known, is effected by the most inconspicuous and poorest of plants, viz. certain kinds of mosses. It is not in the towering primæval forests of India and Brazil, nor the mangrove swamps of tropical coasts, but in the moors of the sub-arctic zone, that plant-remains are now being stored up in a form that, in the course of geological ages, may become converted into beds of coal.

A closer examination of these conditions apprises us of certain important facts. The reason why great masses of vegetable remains do not accumulate in warm countries is that, in the presence of a high temperature the decaying plants decompose too rapidly, and speedily disappear; it is only in a cold climate that they are preserved; and we may therefore regard the existence of coal-beds as a proof that at the time of their formation a high temperature did *not* prevail.

Out of the mass of baseless assumptions, then, this tolerably well-founded fact remains, that an arborescent vegetation of the Carboniferous period presents itself in 76° of northern latitude, whereas, at the present day the northern limit of tree-growth nowhere exceeds 72°; and if we assume that there has been no displacement of the earth's axis of rotation, we must conclude that in these high latitudes the mean temperature of the year was formerly some degrees warmer than at this present time; in the temperate zone we may infer, with some probability, a cool climate with moderate heat in the summer and cold in the winter, and with but little frost: in fact, an insular climate, such as our knowledge of the distribution of land and sea in that age presupposes.

So far we have regarded only the conditions obtaining in the north temperate zone and the polar regions. These, however, show certain peculiarities of distribution. The greatest coal deposits are all in the temperate zone, and chiefly concentrated in its middle and northern regions. The most northerly of the great deposits of the productive Coal-measures are those of Scotland, the most southerly those on the border of the central plateau of France; such as lie further north or south are of little importance. In North America, it is true, they extend considerably further south, but none reach to the 30th parallel of latitude; while, in the north, they extend into British North America. The coal of China occurs in the northern provinces, in Shansi, Shensi, and Honan.

Thus we find that the greater deposits are restricted to a zone of variable width, the southern limits of which are between 30° and 45°, the northern between 50° and 60° N. lat.; beds of true coal of the same age are not indeed entirely wanting outside these limits, but they are rare; as a rule we meet with only the characteristic plants, and these gradually disappear as we proceed further south. In a few instances they may be traced as far as Northern Africa and the peninsula of Sinai; but between the tropics the typical flora of the coal formation seems to fail entirely; not a single instance of their occurrence can be cited; and their first reappearance seems to be in the southern temperate zone in the coal-fields of Southern Brazil.

For a long time it was very doubtful what explanation should be given of this phenomenon, whether plant-bearing deposits of this age were altogether wanting in the tropical zone, or whether their development was of so different a character that we had failed to identify them, or finally whether it were due to some other cause. We cannot notice at length the gradual development of our knowledge on this head; we can only sketch out the final results which have been yielded in the last few years. We know now, that in Southern Africa, in India, and Australia, there are extensive deposits of the same age as our productive Coal-measures, with abundant plant-remains, but that these differ very greatly from the contemporaneous growths of our own region. No trace is found of the forms characteristic of our Coal-measures, no Sigillariæ, no Lepidodendrons, no Calamites. Ferns and true Equisetaceæ furnish by far the greater part of the flora; and with these are associated a small number of conifers and Cycads. The commonest and most characteristic form of this flora is the fern genus *Glossopteris*, and accordingly the whole assemblage of associated plants has been termed the *Glossopteris* flora. When put in comparison with our European coal flora, so strange does this seem, that no one would venture to think of it as contemporary until it had been established, by evidence admitting of no question, that such is actually the case.

From this, however, the important result follows that the doctrine of a universal coal flora is altogether false. On the contrary, we find that we have to deal with two very different floral regions, which stand strongly contrasted. And what makes this contrast especially remarkable, and for a long time hindered its true interpretation, is that the *Glossopteris* flora of India, Australia, and South Africa is nearly related to the European flora of a much later period, viz. the Trias.

But the most striking fact connected with this flora is that its first appearance, whether in South Africa, India, or Australia, is associated with deposits of fine argillaceous sand, with numerous stony fragments varying in size from small pebbles to gigantic blocks of many hundredweight, irregularly embedded; they consist for the most part of rocks that do not occur anywhere in the neighbourhood, and must therefore have been transported from a distance, and moreover some among them are scored and scratched. These phenomena, which manifest themselves in three far-distant localities, and according to the latest intelligence seem to recur also in Brazil, bear such striking evidence of the agency of ice in the formation of these deposits, that any doubt on this head seems scarcely any longer admissible, however much it may startle us to find great ice-masses and floating icebergs at the time of the coal formation in regions so far from the poles.

From the facts we have recounted, bearing on the climate of the Coal-measure period, it is abundantly manifest that everything runs counter to the assumption of a uniform and warm terrestrial climate from the equator to the poles. Geographically we have sharply contrasted floras, and we have moreover widely distributed deposits, in the formation of which great masses of ice must have played a part, and thus the old views are utterly overthrown. But when we go further, and seek to learn from the facts before us what the conditions really were, we are quickly admonished that our knowledge is as yet far too small to admit of any definite representation of these conditions. We may say with much probability that the differences of the floral regions must be ascribed to differences of climate, and that, locally, the temperature was so low as to allow of the formation of great masses of ice; but anything beyond this is quite uncertain, and no one of the assumptions that have been made to explain the conditions of that epoch has any claim to validity. Those early ages present us with so much that is strange to us, the unknown is so vast in comparison with what we know, that we dare not as yet attempt any generalization of our knowledge.

We pass over the formations which succeed the Coal-measures, viz. the Permian, the Trias, the Jura, and the Chalk, and after this enormous interval we turn our attention to Tertiary times. Here begin those modern developments that have resulted in our present world; the chief types of animals and plants are the same as those of our own day; and it is only since the beginning of Tertiary times that mammals predominate among the fauna of the land, whereas in the previous formations this leading part had been played by reptiles.

At that time Europe was far more cut up by inland seas than it now is, and formed a dismembered assemblage of islands and peninsulas. In the first division of the Tertiary age, the Eocene, the seas around its coasts were tenanted by animals of a tropical character. In the later subdivisions, this character was gradually lost. In the Oligocene, a marine fauna of a tropical character extends only to a line which about coincides with the northern limit of the Alps. In the Miocene, which next follows, the fauna even of this part of Europe is, at the utmost, sub-tropical; and, by degrees, the forms which give evidence of a warm climate gradually diminish, so that towards the end of the last division, the Pliocene, the conditions were almost the same as to-day.

What we know of the land organisms agrees entirely with these indications afforded us by the marine fauna, at least in their leading characteristics, since we equally find, at the beginning of Tertiary times in Europe, a predominance of sub-tropical and tropical types, which, later on, were replaced by a flora representative of a temperate climate. In detail, indeed, there are many and not unimportant deviations. Thus, for instance, the flora the remains of which are preserved in the calcareous tufa of Sezanne in Champagne, or in the marls of Gelinde, belongs to the Lower Eocene. The forms here represented are such as at the present time are peculiar to the southern part of the temperate or the sub-tropical zone; numerous evergreen oaks, laurels, cinnamon and camphor trees, various Myrtaceæ, Araliaceæ, figs, magnolias, &c.; many forms point decidedly to a tropical climate, but among them we find also, walnut trees, limes, alders, willows, ivy, and vines, which have an opposite character. Palms and cycads, the specially characteristic forms of hot climates, are absent, or at any rate have not been detected. On the whole, botanists are inclined to infer for that epoch in Central Europe such a climate as now obtains in Southern Japan in 33° N. latitude.

We meet first with truly tropical floral characters in somewhat later deposits, viz. in the Middle and Upper Eocene. At that time there flourished on the mainland and islands of Europe great palms and a number of other plants, whose nearest relatives now exist in tropical Africa, India, and Australia. To judge from the land flora, there was then a maximum of warmth in our neighbourhood (Vienna), from which up to the end of Tertiary times a continuous fall took place. In the Oligocene and Lower Miocene the prevailing character is still that of a tropical or sub-tropical region, but the number of forms that now live in temperate regions has considerably increased; such as now live in Australia occur in remarkable quantity. Then in the Upper Miocene of Central Europe we meet with a flora such as at the present day characterizes the warmer parts of the temperate zone, and in which forms allied to the present flora of North America are especially prominent. In the Pliocene, the latest subdivision of the Tertiaries, the change has progressed still further, and at its end we find in our neighbourhood an assemblage of plants nearly recalling that of the present day, with but a slight intermixture of those of warmer regions.

We may grant generally that these facts prove the existence in Tertiary times of a warmer climate than now prevails in Europe, even though there may be great differences of opinion as to the amount of the difference. Heer, to whom we are indebted for the most important investigations of this subject, has endeavoured to determine the mean annual temperature at certain definite geological epochs from the characters of their respective floras. He found that on the northern border of the Alps in Switzerland, at the epoch of the Upper Oligocene, there was a mean temperature of between 20° and 22° C. (68°–72° F.), such as at the present day is that of Cairo, Tunis, Canton, or New Orleans; at the time of the Upper Miocene, one of 18° or 19° C. (64°–66° F.), corresponding to that of Messina, Malaga, Madeira, and Nagasaki; whereas at the present time the mean annual temperature of Zurich is 8°·73 (47°·7 F.), that of Geneva 9°·67 C. (49°·4 F.). But whereas Geneva and Zurich now lie high above sea-level, we have proofs that in Tertiary times the sea-level was much higher in that neighbourhood than now, therefore that this flora grew at a small height above the sea, which would imply alone an increase of about 3° C. (5½° F.) of temperature. It follows, then, that at the time of the Upper Oligocene the temperature was about 9° C. (16° F.), in that of the Upper Miocene about 7° C. (12° F.) higher than at present.

With respect to these figures, we must, however, bear in mind that in such computations no allowance is made for the acclimatization of species and whole genera in the course of long geological periods, and therefore that the assigned variations of temperature are almost certainly too high. Moreover, we must remember that, at that time, Europe was far more than now interpenetrated by inland seas and straits, and therefore that its climate was more insular, the summers being cooler and the winters warmer than now. But whatever weight we give to these considerations, they are alone insufficient to account for the whole of the difference between the Eocene and the present floras. We must perforce admit that other and deeper-lying causes have co-operated in producing the observed differences.

The examination of the Tertiary floras of high northern latitudes leads us very decisively to a similar conclusion. The various English, American, Danish, and especially the Swedish expeditions have discovered in numerous localities the Tertiary plant-remains of the polar regions, the floras of which have been worked out by Heer. Places which are now among the coldest known spots of the earth have yielded the remains of a rich forest vegetation; nay, within the polar circle itself are found plants which at the present time find even our own latitudes too cold for them. The most northern point from which we have plant impressions is Grinnell Land in the North American archipelago, in 81°45′ N. lat. Its present mean annual temperature is about – 20° C. (4° F.). The flora consists chiefly of conifers, among which are our common pine, two species of fir, and the American swamp cypress (*Taxodium distichum*); with these are associated elms, limes, birches, poplars, hazel, and some others, the temperature requisite for which is estimated at about 8° C. (46° F.).

Much richer is the fossil flora of Spitzbergen, between [illegible]d 78⅔° N. lat. Here also conifers are dominant; among foliage trees are present several poplars, also willows, alders, beeches, birches, large-leaved oaks, elms, plane trees, walnuts, magnolias, maples, and others; accordingly the climate of Spitzbergen at that time must have been much the same as the present climate of Northern Germany. A still warmer climate is indicated by the fossil flora of Greenland, which may be compared with the present flora of the shores of the Lake of Geneva.

These are by no means the only instances of a similar kind; analogous discoveries have been made at many different points in high northern latitudes; for instance in Siberia on the lower Lena, on the New Siberian Islands, in Kamtschatka, Alaska, Sitka, Banks Land, and some other points. It is not yet certainly determined to what part of the Tertiary period these fossil remains belong. While some regard them as Miocene or Upper Oligocene, others consider them to be Eocene; and good reasons may be assigned for both these opinions. Whatever may be the final decision is for our present purpose a matter of minor importance. The point we have to insist on is that in the polar regions, the mean temperature of which is now below the freezing-point, and in which only some of the lowest plants exist, there was in Tertiary times a rich forest growth. The difference between those times and the present was so great that for Grinnell Land we cannot estimate it as less than 27° C. (49° F.).

Such a change is absolutely inconceivable so long as we continue to regard as unalterable the present position of the places in question with reference to the pole. We cannot imagine any change in the distribution of land and water, in marine currents, or in any other influential factor, which, at a time comparatively so little distant from the present, could have brought about a luxuriant forest growth in Grinnell Land. This has long been recognized, and in many quarters it has been contended that the only explanation possible is a displacement of the earth's axis of rotation. To this the answer has been that the stations that have yielded the Tertiary plant-remains form a circuit around the pole, a chain from which, as an English geologist has expressed it, the pole can no more escape than a rat from a trap in a ring of terriers.

In point of fact there is no need for assuming so considerable a displacement of the pole since the beginning of Tertiary times. There is, however, ample room within the circle of the northern Tertiary plant stations for such a change, and there are valid grounds for such an assumption. For nowhere do the Tertiary plants reach so far north, and yet nevertheless testify so strongly to the existence of a warm climate as in the quadrant in which lie Grinnell Land, Greenland, and Spitzbergen; when we pass over to the opposite quadrant we find precisely the opposite case, for the Tertiary plants of Alaska, in North-Western America have, in north latitude 60°, scarcely more the character of a southern flora than those of Spitzbergen in lat. 78°.

From these considerations, it seems not improbable that, at the time when these Tertiary plants lived, the pole really had not the same position as now, but was displaced from 10° to 20° in the direction of North-Eastern Asia. The circumstances of the Tertiary deposits in other places outside the polar regions agree very well with this view. In Europe, as we have seen, a very warm climate prevailed universally, but when we turn to other countries we meet with a different result. The flora of the Tertiary formations of the United States give no indication of any essential increase of temperature, and the fossil plants of the probably Miocene and Pliocene formations of Japan, according to the admirable investigations of Nathorst, point to a colder climate than that which now prevails. These facts are obviously eminently favourable to the idea of a displacement of the pole. Curiously enough, we find in the yet but little known Tertiary deposits of the southern hemisphere a somewhat striking confirmation of this view, inasmuch as the marine Tertiary Mollusca which occur in several parts of the Chili coast, do not contain a single species indicative of a warmer climate than that of the present day.

Thus, then, it seems very probable that the position of the pole in Tertiary times was different from that of to-day, and only became as at present at the close of that era. But on this assumption the extreme contrasts are only somewhat palliated, the greater divergences somewhat reduced: no complete explanation is afforded of the phenomena. Whatever position we may assign to the pole, those places in which Tertiary forest trees are found were in any case far nearer to it than is the present northern limit of tree-growth; and when we compare the fossil floras of Europe and Japan, we find that the first shows a much greater departure from the present state of things in the direction of a warmer climate than does the latter in the opposite direction. Thus we are led to the conclusion that the climate of Tertiary times in general was somewhat warmer than that of our own day, but by no means to such an extent as that of the lands specially favoured through the displacement of the pole, viz. Grinnell Land, Greenland, Spitzbergen, and Western and Central Europe.

When from the Tertiary age we take another step forward in time, and reach the Pleistocene, the immediate forerunner of our present age, we meet with quite another picture. The remarkable characteristics of this period have been set forth by a skilled hand in this place, and I need only refer to them in a few words and in so far as is specially important in connection with our present subject.

At the setting in of the Pleistocene, the climate seems to have been somewhat warmer than at present: figs, laurels, and vines grew wild in Central Europe, and among animals, we meet with certain fresh-water Mollusca (*Cyrena fluminalis*) which afford a similar indication. Then followed through the greater part of the Pleistocene that extension of enormous ice masses, which, issuing from Scandinavia, Finland, and the Russian Baltic provinces, covered a great part of Europe and advanced to England, Holland, the base of the mountains of Central Germany, the Carpathians, and in Russia as far as Kiew, Woronesch, and Nishni Novgorod. England, Scotland, and Ireland were almost completely glaciated, the ice-sheet covered nearly the whole Alpine region, a broad ice-girdle lay in front of its northern base, and even the small hill-ranges of Central Europe and some of the greater ranges of Southern Europe developed independent glaciers. On a still greater scale, similar phenomena present themselves to us in North America, and in Northern Asia the greater mountains were then glaciated. Also further south, in the Himalaya and the Karakorum were enormous glaciers, and the same in the neighbourhood of the equator in the Sierra di Santa Martha in the northern part of South America. In the southern hemisphere, traces of glaciers occur very extensively in the southern part of the same continent, and according to many accounts also in South Africa.

It was long doubtful whether the glaciation of the northern and southern hemisphere took place simultaneously; but there is now no longer any doubt that such was really the case. Attempts have been made to explain the formation of great ice-accumulations without any depression of temperature, nay even in warm climates, solely as the result of an excessive precipitation of rain and snow, and in consequence of the prevalence of warm winters and cool summers; but these views are wholly untenable; a depression of temperature is testified to, not only by the extension of the glaciers, but also by the vegetable and animal denizens of the land and the sea. When in Pleistocene deposits of the Mediterranean basin we find Mollusca suddenly appear which now live only in the German Ocean, no other explanation is possible than that the temperature at that time was low.

We need not indeed conclude that an excessive degree of cold was necessary to produce the phenomena of the Glacial period; the height of the snow-line at that time has been computed for many of the mountains of Europe, and from this it has been deduced that the extreme reduction of temperature was at the utmost 6° C. (11° F.), and possibly considerably less. Much has been said and written of the causes which brought about the cold of the Glacial period. Very thoughtful and also very jejune hypotheses have been put forward, all of which have this one characteristic in common, that in some one particular or another they are strongly opposed to the actual facts, and have therefore no validity. With our present knowledge, any explanation is quite impossible. We must content ourselves with recognizing that the cooling was simultaneous, and, as far as research has yet gone, extended over the whole of the globe. It is, then, obviously impossible to attribute it to a displacement of the pole, for in that case a part of the earth must have experienced an increase of temperature; and, in addition to this, we certainly cannot suppose any considerable change in the position of the pole within so comparatively short an interval as separates us from the Glacial epoch. The uniform extension of the phenomenon excludes all those attempted explanations which appeal to geological or geographical changes of the earth's surface, a different distribution of land and sea, changes in the ocean currents, &c., and all points to some agency external to the earth, and therefore acting on it as a whole.

We must specially notice one other circumstance in connection with the Glacial period. It has been observed in many places that the glacial deposits with their scratched pebbles and irregular heaping of their materials do not form a continuous mass, but that, between a lower and upper deposit of glacial character, there is an intermediate bed showing no trace of ice action; at different places, the remains of animals and plants have been met with in this intermediate bed which indicate a somewhat warmer climate, though slightly colder than the present. Thus, in the slaty coal of Utznach and Dürnten in Switzerland, which belongs to this formation, have been found only the remains of plants still growing in the neighbourhood, with the single exception of the mountain pine, which no longer exists in the low plains of Switzerland, but has withdrawn to Alpine heights. These so-called inter-glacial deposits attain in places to a considerable thickness. They show us that during the great Glacial period there intervened a very decided recurrence of a warmer temperature, during which the great ice masses melted away; and from all the indications, this interval, according to human reckoning, must have lasted thousands of years. This page of the earth's history has for us this especial interest, that the oldest certain indications of man's existence in Europe are found in these inter-glacial deposits.

Similar evidence of an interruption of the Glacial period by one of greater warmth is met with in many other parts of the Alpine region, and also on the plains of Northern Germany, in Scandinavia, England, and in different parts of North America, and we must therefore conclude that it was of general occurrence, and that the changes of temperature which brought about the glaciation of an enormous extent of land, and subsequently set it free from its icy covering, were not regularly progressive, but consisted of many changes and oscillations. . . .

Thus we have sketched in a few hasty outlines what we know of the climatic conditions of three periods of the earth's history which are of especial importance for judging such questions. The first of these, of hoar antiquity, was that of the Coal-measures. We have ascertained the existence of distinct floral regions, which in all probability were determined by differences in the distribution of heat; moreover, we have found in deposits far distant from each other evidence of ice action. But in all other points the conditions are so far removed from any of which we have experience, that any further inference is hardly possible. At the utmost we may conclude from the limitation of the greater coal-beds to the temperate zone that the position of the earth's axis and of the pole did not differ very greatly from those of the present day.

When we turn to the much younger formations of the Tertiary age, the conditions are somewhat clearer. In them we recognize, in the first place, the operation of purely local agencies, the distribution of land and water, of ocean currents, &c., but we must also confess that these play but a subordinate part. We have also seen that in certain regions, viz., in Europe, Greenland, Grinnell Land, &c., there prevailed a much warmer climate, which, however, we do not recognize in America; while in Japan, as inferred from the vegetation, the temperature of Tertiary times seems to have been lower than it now is; and we have found in a displacement of the pole and the earth's axis the only probable explanation of these phenomena.

This cause does not, however, suffice to explain all anomalies, and we must assume for all parts of the earth the prevalence of a somewhat warmer climate, an increase perhaps of a few degrees only, which manifests itself particularly in the vegetation of the polar regions.

In the Pleistocene epoch, which is, comparatively speaking, so near to our own, the problem is so far simplified, that one of the two principal factors which determined the deviation from our present climatic conditions—the displacement of the earth's axis—was no longer present; or rather, having regard to the shortness of the time that has since elapsed, was so unimportant that its influence is not traceable. Apart from purely local circumstances, we have, as far as we can judge, only to deal with uniform oscillations of temperature over the whole earth anomalies of the same general character as brought about the general elevation of climatic temperature in the Tertiary age.

If we follow the march of these vicissitudes of temperature, evidently determined by some cosmical agency, we find at the beginning of Tertiary times a moderately warm climate; then a rise during the Eocene, and then a gradual cooling, interrupted possibly by some oscillations, down to a degree nearly corresponding to that now prevailing, at the beginning of the Pleistocene epoch. Then the cooling continued below the present temperature, to a minimum at the time of the greatest glaciation of the land; then a re-warming in the inter-glacial period nearly up to the present temperature; after which cold and glaciation regained the upper hand, finally to give way to the present conditions, which are about midway between the greatest warmth of the Tertiary age and the greatest cold of the Pleistocene.

One fact stands out conspicuously, viz. that these changes progressed very irregularly, and were subject to much oscillation, and the period during which we can approximately follow the course of the change is much too short to enable us to learn the law that regulated it. We cannot decide whether oscillations like those of the Pleistocene will be repeated, and we are now progressing towards another temporary Glacial period, or whether we have to expect the return of a warmer temperature such as prevailed in Tertiary times, or, finally, whether the outcome of all the deviations will be a lasting refrigeration of our climate.

Just as little can we determine at present by what agency all these vicissitudes are brought about; most plausible and simple would it certainly be were the sun a variable star that at different periods emits different quantities of heat; but for this or any other assumption there is no proof forthcoming. This enigma, like so many others, will some day be solved by man's searching intelligence, but, like all other acquisitions of science, this goal can be won only by assiduous and patient labour. Haply the triumph may not be for our generation; but what we may certainly accomplish is to prepare the way to it, by an accurate and critical collection of the facts.

H. F. B.

NOTES.

It is expected that about fifty foreign men of science will be present at the Leeds meeting of the British Association. A good many manufacturing firms have promised to open their works during the time at which the meeting is being held; and a Guide to Leeds and the surrounding district, with accounts of the various industries, is being prepared. There will, of course, be excursions to the more interesting places within easy reach of Leeds. The first *soirée* will be given by the Mayor, the second by the Executive Committee. The Yorkshire College will give an afternoon reception.

The London Mathematical Society has awarded the De Morgan Memorial Medal (given triennially) to Lord Rayleigh, Sec.R.S., for his researches in mathematical physics. The previous awards have been to Profs. Cayley and Sylvester. The medal will be presented at the annual meeting in November next.

The *conversazione* of the Society of Arts, as we have already announced, will take place at the Natural History Museum, Cromwell Road, on Friday, June 27. The galleries will be lighted with electricity, so that the authorities of the Museum will have a good opportunity of judging how far the electric light is suitable for the building. If the experiment is successful, the system will no doubt soon be permanently established. It may be hoped that in that case the public will not be excluded during an interval between twilight and the lighting of the electric lamps. That plan has been tried at the British Museum, and the results are not encouraging. If the national collections are to have a fair chance of attracting visitors, they must be open continuously from morning until the hour when they are closed for the night.

The anniversary meeting of the Royal Geographical Society was held on Monday, Sir E. M. Grant Duff, the President, occupying the chair. Mr. Douglas W. Freshfield announced that the Patron's Medal had been awarded to Emin Pasha, and the Founders' Medal to Lieutenant F. E. Younghusband. The Murchison Grant was awarded to Signor Vittorio Sella, for his journey in the Caucasus; the Cuthbert Peek Grant to Mr. E. C. Hore, for observations on the physical geography of Tanganyika; and the Gill Memorial to Mr. C. M. Woodford, for three expeditions to the Solomon Islands. Scholarships and prizes were awarded to students in training colleges. Dr. R. W. Felkin attended, upon instructions by telegram from Zanzibar, to receive the medal for Emin Pasha. The President, in handing the medal to Dr. Felkin, congratulated him upon having done much to make the work of Emin known in England. The Society was not based upon politics, and they simply saw in Emin Pasha one who had from early life given a great deal of attention to botany, natural history, and other subjects. Dr. Felkin, in acknowledgment of the medal, referred to the great services rendered by Emin Pasha to science. Afterwards the Report of the Council was read, and Sir E. M. Grant Duff delivered his presidential address.

At the meeting of the Scientific Committee of the Royal Horticultural Society on June 10, Mr. Morris called attention to the fact that the Royal Society had assigned £100 "on the recommendation of the Government Grant Committee, for an inquiry into the composition of London fog, with special regard to the constituents of fog injurious to plant life." An informal conversation followed with reference to chemical investigations to be undertaken at the laboratory of University College, under the superintendence of Dr. Oliver.

A deputation from the Sanitary Institute lately visited Brighton, and met the Mayor and other members of the Committee for the purpose of further considering the Congress and Exhibition to be held in the Pavilion buildings at the end of August. The large dome of the Pavilion, the Corn Exchange, and the Picture Gallery, are all devoted to the Exhibition, but the applications for space are considerably in excess of previous years, and probably some difficulty will be found in accommodating exhibitors. Sir Thomas Crawford is the President. At one of the meetings of the Congress a lecture will be delivered by Mr. W. H. Preece, F.R.S. Dr. B. Ward Richardson, F.R.S., will address a meeting of the working classes.

The thirty-seventh Report of the Department of Science and Art has been issued.

A lecture on the use of alloys in art metal-work, delivered by Prof. Roberts-Austen at the Society of Arts on May 13, is printed in this week's number of the Society's Journal. It is a lecture of great value and interest, and all who read it will cordially agree with the author that "an effort should be made to induce British artificers to employ the materials and methods which their Japanese brethren have used for centuries with such remarkable effect."

In France much interest is being taken in the question whether a University shall be established in Paris. At a meeting of the General Council of the Paris Faculties, held last Saturday at the Sorbonne, it was agreed that a University with five faculties (Protestant theology, law, medicine, science, and literature), and an upper school of pharmacy, should be formed. "The principal effects of the constitution of the University," says the Paris correspondent of the *Times*, "will be to permit the faculties to make arrangements for the organization of instruction (under the form of schools or institutes) of which the elements are at present scattered in several faculties, and to facilitate a sort of general instruction of a philosophical character, to which the professors of all the faculties will contribute, and which will be addressed to the students. The University will grant, besides professional degrees, diplomas of purely scientific studies to native and foreign students."

M. Deflers has just returned to France from his extremely arduous exploration of Southern Arabia at the instance of the Minister of Public Instruction in France. He has brought back large collections of both living and dried plants for the Museum of Natural History.

The Museum of Natural History in Paris has also received a considerable collection of dried plants gathered in Madagascar by M. Catat.

M. Balansa is about to return to Tonkin for the purpose of continuing his botanical explorations there; and M. Thollon to the Congo, from which he has already sent interesting collections.

A Laboratory of Vegetable Biology was opened at Fontainebleau on May 15. It is under the control of M. G. Bonnier, Professor of Botany at the Sorbonne, Paris, to whom applications for leave to pursue researches in the Laboratory should be addressed.

THE Königsberg Physikalisch-Oekonomische Gesellschaft has recently invited a comprehensive discussion of the observations of ground-temperature at Königsberg, published in the Society's *Schriften*, as bearing on our knowledge of heat-movements in the earth and their causes. Attention is called to a previous work by O. Frölich, published at Königsberg in 1868. For the best treatment of the subject a prize of £15 is offered. Papers (in any language), with motto, to be sent in before February 1, 1891.

Science announces that Lieutenant J. P. Finley, of the U.S. Signal Corps, has gone to San Francisco to take charge of the Pacific Coast weather service.

WE learn from *Science* that a work of great importance to navigators is to be undertaken in connection with the report of the U.S. Eclipse Expedition to West Africa, under the direction of Prof. D. P. Todd. This is the preparation of a set of daily weather-maps of both oceans from October to May inclusive, the entire period of the cruise of the U.S. steamship *Pensacola*. The U.S. Hydrographic Office calls attention to the importance of the subject, and the exceptional opportunity presented for utilizing the data already at hand, together with such additional data as may be contributed for this purpose by various Government offices and individual navigators. The scheme determined upon consists in the preparation of a weather map for each day at noon, Greenwich mean time, from October 1, 1889, to May 31, 1890, inclusive, for the entire area between latitude 70° north and 60° south, longitude 20° east and 100° west. In addition to the Greenwich noon observations that are kept regularly for the Hydrographic Office by nearly two thousand voluntary observers, it is earnestly desired that other navigators of these waters, within the limits of time and place mentioned above, may forward to that office such data from their log-books as may be useful in this connection, selecting those observations that come nearest to noon, Greenwich mean time, and stating as many details as possible regarding wind, weather, state of the sea, and velocity and set of currents. Data from land stations are also very important, especially such as are not accessible in any published records. To make this great undertaking a success, however, there must be further and cordial co-operation among the nations interested in the meteorology of this vast area, and among navigators of every nationality. It has long been the desire of the U.S. Hydrographic Office to begin the publication of a pilot chart of the South Atlantic and west coast of South America, and the present undertaking will furnish an admirable basis for this work.

A PAPER on the Mannesmann weldless tubes was lately read before the Society of Arts by Mr. J. G. Gordon, the chair being occupied by Sir Frederick Bramwell, who referred to the importance and interest of the subject, and to the extraordinary means by which the desired result was attained. The process consists in the solution of a purely kinematical problem, viz. the arranging of the velocity ratio of a pair of aconoidal rolls so as to change a solid piece delivered to them at one end into a hollow tube passed out at the other. These rolls revolve at about 200 to 300 revolutions per minute, and by their action on the hot and therefore plastic steel stretch it and make a hollow in the centre. The substance of the metal must be sufficiently homogeneous and plastic, and, in passing through the rolls, it undergoes a violent twisting and stretching action. The bar, in fact, in its passage through the rolls, is twisted as a thread is twisted in a spinning-machine, the material being drawn from the interior. This action was illustrated by one of the exhibits, which consisted of a bar, the ends of which were slightly drawn down under the hammer, so that the rolls could not act on them. A hollow was thus produced in the solid bar of metal, the contents of which were tested by Prof. Finke, of Berlin, and found to contain 99 per cent. of hydrogen of its total volume; the remaining 1 per cent. he considered to be probably nitrogen. In the carrying out of the process, 2000 to 10,000 horse-power is required for from 30 to 45 seconds, according to the dimensions of the tube. Although this is all the time actually required to convert a bar 10 to 12 feet long and 4 inches in diameter into a tube, a certain amount of time is required to adjust the guides, to deliver the bar to the rolls, and to remove the finished tube. The time so spent is employed to accumulate energy in a fly-wheel 20 feet in diameter, weighing 70 tons, and revolving 240 times in a minute, the periphery of which therefore revolves at 2·85 miles per minute; by this means, a steam-engine of 1200 horse-power is quite sufficient to do the work. A peculiar feature of these rolls is that the resulting tube is a test of the material and process. If the metal is homogeneous throughout, and well melted, well rolled, and carefully heated, it makes a perfect tube; but if there is a flaw in the metal, or if it has not been properly heated, the rolls cannot make a tube out of it. The paper, which was illustrated by photographs of the mills and engines, led to a very interesting discussion, in which Sir Frederick Bramwell, Prof. A. B. W. Kennedy, Mr. Alexander Siemens, and others took part.

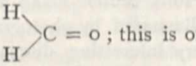
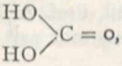
MESSRS. J. AND A. CHURCHILL have issued the fifth edition of Prof. F. Clowes's "Treatise on Practical Chemistry." The present edition contains several emendations and additions. The author explains that the work is intended to furnish a course of laboratory instruction in practical chemistry, which may precede the higher training of the professional and pharmaceutical chemist and the medical man, and the more special training of the technical chemist and the chemical engineer.

THE sixth volume of "Blackie's Modern Cyclopædia," edited by Dr. Charles Annandale, has been published. The volume begins with "Mona," and ends with "Postulate." The articles, like those of the previous volumes, are remarkably clear, concise, and accurate.

THE third number of the "Indian Museum Notes" consist of a careful paper on silkworms in India, by Mr. E. C. Cotes. The author confines attention to those species which are actually utilized in India for the production of silk.

MR. W. F. KIRBY, author of "A Synonymic Catalogue of Diurnal Lepidoptera," will publish shortly with Messrs. Gurney and Jackson, Mr. Van Voorst's successors, "A Synonymic Catalogue of Neuroptera Odonata or Dragon-flies." He hopes to bring out afterwards the first volume of his "Catalogue of Lepidoptera Heterocera," a work which has engaged his attention for nearly twenty years.

THE editor of the *Naturalists' Gazette* has in the press an "Illustrated Hand-book of British Dragon-flies," which will contain a full description of all the species indigenous to the British Isles, in addition to a quantity of other information.

A NEW gas, methylene fluoride, CH_2F_2, has been obtained by M. Chabrié, and a preliminary account of it will be found in the current number of the *Comptes rendus*. Its chlorine, bromine, and iodine analogues, CH_2Cl_2, CH_2Br_2, and CH_2I_2, have long been known. Methylene iodide is a liquid at ordinary temperatures which solidifies about 4° C. to brilliant leafy crystals. The liquid boils at 182°. Methylene bromide is a liquid boiling at 81°, and the chloride is also a liquid boiling at 41°. The fluoride, completing the graduation of the series, is now shown to be a gas. It is obtained by heating the chloride with silver fluoride. Methylene chloride is generally prepared from methyl iodide, which is placed in a retort, covered with water, and a stream of chlorine slowly allowed to pass through it. The very volatile methylene chloride distils over, as it is formed, into a condenser which is strongly cooled by a freezing mixture. In order to prepare the new gas, the methylene chloride is placed in a tube of Bohemian glass along with the proper quantity of pure silver fluoride, and the tube sealed before the blowpipe. It is then heated for half an hour to 180°. In an actual experiment 1·7 gram of methylene chloride and 5·08 grams of anhydrous silver fluoride, specially purified by the method described by Gore, were employed. Upon opening the tube, a great rush of gas occurs, which on collection and analysis is found to consist of methylene fluoride. The density of the gas compared with air was found to be 1·82, which agrees very closely with the theoretical density, 1·81, required for the formula CH_2F_2. Alcoholic potash is found to absorb it completely. Hence, in order to obtain a measure of the amount of carbon contained in the gas, a measured volume was absorbed in alcoholic potash and then treated with acetic acid and potassium permanganate. The alcoholic potash appears to convert it into formaldehyde,  $\begin{matrix}H\\H\end{matrix}\!\!>C=O$; this is oxidized by the potassium permanganate to carbonic acid,  $\begin{matrix}HO\\HO\end{matrix}\!\!>C=O$, and the acetic acid consequently liberates a volume of carbon dioxide equal to the volume of methylene fluoride experimented upon. This affords a ready mode of demonstrating at the same time the principal properties of the gas and its composition as regards the amount of carbon contained in it. Experiments are now in progress from which it is hoped some knowledge will be gained concerning its physiological action, which it will be interesting to compare with that described by MM. Regnault and Villejean in case of methylene chloride. In a recent communication by M. Moissan upon carbon tetrafluoride, CF_4, an account of which was given in NATURE (May 15, p. 67), it was recommended that metallic tubes should always be employed in these reactions with silver fluoride, inasmuch as fluorides of carbon attack glass with production of carbon dioxide and silicon tetrafluoride; for instance, $CF_4 + SiO_2 = CO_2 + SiF_4$. But M. Chabrié finds that if hard Bohemian glass is used, the product contains only mere traces of the two gaseous impurities mentioned, and, as glass is so much more convenient to manipulate, considers it advisable to use it. The methylene fluoride prepared in the above manner was quite sufficiently pure for all practical purposes.

THE additions to the Zoological Society's Gardens during the past week include a Common Marmoset (*Hapale jacchus*) from South-east Brazil, presented by Mr. Percy Standish; a Malbrouck Monkey (*Cercopithecus cynosurus* ♂) from the Upper Shire, two Grand Galagos (*Galago crassicaudata*) from Mandala, Shire Highland, East Africa, presented by Mr. John W. Moir; two Common Marmosets (*Hapale jacchus*) from South-east Brazil, presented by Mr. W. Norbury; a Common Fox (*Canis vulpes* ♂), British, presented by Mr. Atkins; a Great Crested Grebe (*Podiceps cristatus*), British, presented by Mr. T. E. Gunn; two Green Lizards (*Lacerta viridis*), three Wall Lizards (*Lacerta muralis*), a Dark-Green Snake (*Zamenis atrovirens*), four Common Snakes (*Tropidonotus natrix*), four Marbled Newts (*Molge marmorata*), an Edible Frog (*Rana esculenta*) from the South of France, presented by the Rev. F. W. Haines; eighteen Young Green Turtles (*Chelone viridis*) from Ascension Island, presented by Captain Robinson; a Silvery Gibbon (*Hylobates leuciscus*) from Java, deposited; a Philippine Paradoxure (*Paradoxurus philippensis*) from Zebu Island, Philippines, three Japanese Teal (*Querquedula formosa* ♂ ♀ ♀) from North-east Asia, purchased; an Angora Goat (*Capra hircus*, var., ♂), two Yellow-legged Herring Gulls (*Larus cachinnans*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on June 19 = 15h. 52m. 18s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4244	—	Bluish.	16 43 56	+47 48
(2) 367 Birm.	6	Red.	15 59 21	+47 32
(3) α Serpentis	2	Yellow.	15 38 54	+ 6 46
(4) α Coronæ	2	Bluish-white.	15 30 0	+27 5
(5) U Cassiopeiæ... ...	Var.	Red.	0 40 11	+47 39

Remarks.

(1) The spectrum of this nebula, according to an observation made by Dr. Huggins in 1866, is a continuous one, but this result does not appear to accord with Smyth's description of it as "a fine planetary nebula, . . . large, round, and of a lucid pale blue hue." The G.C. description of it is: "Very bright; large; round; disk with faint, possibly resolvable, border." I know of no later observation of the spectrum than that referred to, but it is important that it should be confirmed, as the colour alone would lead to the supposition that there is something in addition to continuous spectrum. It is indeed possible that we have here a case of a nebula intermediate in condensation between those which give a spectrum of bright lines, and those which give a so-called "continuous" spectrum. In any case the apparent discrepancy between colour and spectrum should be investigated, for it is generally understood that planetary nebulæ with a bluish colour give bright lines.

(2) Dunér describes the spectrum of this star as a magnificent one of Group II. "All the bands 2–10, 6 included, and possibly 1, are visible. They are of extraordinary width and entirely black. The spectrum is totally discontinuous." The usual more detailed observations should be made.

(3) This star has a spectrum generally described as similar to the solar spectrum. The usual more detailed observations, as to whether the star is increasing or decreasing in temperature, are required.

(4) The spectrum of this star is a well-marked one of Group IV., but so far we have no information as to the temperature of the star relatively to others with almost similar spectra.

(5) The spectrum of this variable has not been recorded. The range of variation is from 8·5 to 14 in about 260 days. There will be a maximum about June 20. A. FOWLER.

OBSERVATIONS OF METEORS.—The May number of the *Monthly Notices* of the Royal Astronomical Society contains a catalogue of 918 radiant-points of meteors observed by Mr. Denning at Bristol since 1873, together with a mass of information pertaining to their determination. The total number of meteors seen from 1873 to 1889 was 12,083, and the paths of 9177 of these were registered. The following table shows the horary rate of apparition of the meteors during the various months of the year:—

January ...	6·5	July	11·3
February ...	4·9	August	11·3
March	6·6	September ...	10·3
April	6·6	October... ...	11·8
May	5·2	November ...	11·3
June	4·9	December ...	8·9

The mean horary rate of apparition is therefore 8·3. This is less than would be observed from a place where there is no interference with the light and smoke of a large town, some observations made by Mr. Denning in a different locality increasing the mean horary number to 11·4.

The observations were almost equally distributed between the morning hours, and were usually made between the third and first quarter of the moon, because a bright sky is very effective in obliterating meteor-showers, and therefore moonlight meteors are commonly rare.

As to the relative numbers which appear during the night, the maximum appears to be attained between 2 and 3 a.m., when the rate is nearly double that observed in the early hours of the evening. Two or three meteors have frequently been noticed to appear at nearly the same time and from the same radiant, the probable explanation in such cases being that the two objects originally formed one mass, which suffered disruption owing to the vicissitudes encountered in planetary space.

The average length of path of all the meteors registered is 10°·9. The average height of either fireballs or shooting-stars has been computed, from thirty-eight instances, to be—

Beginning height ... 71·1 miles.
End height ... ... 48·2 ,,

From a comparison of a large number of other similar results, the following general average has been deduced :—

Beginning height ... 76·4 miles (683 meteors).
End height ... ... 50·8 ,, (736 ,,).

If fireballs and shooting-stars are separated, the usual heights of disappearance are : fireballs, 30 miles ; shooting-stars, 54 miles. A considerable amount of information as to the radiant-points, stationary and otherwise, has been brought together ; and, with the catalogue, they render Mr. Denning's paper one of a very important character.

BROOKS'S COMET (*a* 1890).—The following ephemeris has been computed by Dr. Bidschof (*Astr. Nach.*, 2970), and is in continuation of that previously given (vol. xlii. p. 138). The elements have been found from observations at Cambridge, March 21, and Vienna, April 18 and May 24 :—

$$T = 1890 \text{ June } 1{\cdot}5360 \text{ Berlin Mean Time.}$$

$$\left.\begin{aligned}\omega &= 68^\circ\ 54'\ 39''{\cdot}9\\ \Omega &= 320\ \ 20\ \ 32{\cdot}2\\ \iota &= 120\ \ 33\ \ \ 5{\cdot}4\end{aligned}\right\}\text{Mean Eq. 1890·0.}$$

$$\log q = 0{\cdot}280524$$

Ephemeris for Berlin Midnight.

1890.	R.A.	Decl.	Brightness.	1890.	R.A.	Decl.	Brightness.
	h. m. s.	° ′			h. m. s.	° ′	
June 21	15 59 27	+65 19·5		July 12	13 48 18	+55 30·9	2·10
,, 22	49 35	65 5·3	3·08	,, 13	45 11	54 58·1	
,, 23	40 4	64 48·6		,, 14	42 15	54 25·6	
,, 24	30 56	64 29·4		,, 15	39 29	53 53·3	
,, 25	22 13	64 8·1		,, 16	36 53	53 21·2	1·93
,, 26	13 55	63 44·9	2·88	,, 17	34 27	52 49·5	
,, 27	6 0	63 20·0		,, 18	32 9	52 18·1	
,, 28	14 58 29	62 53·6		,, 19	29 59	51 47·0	
,, 29	51 22	62 25·9		,, 20	27 56	51 16·3	1·77
,, 30	44 38	61 57·1	2·68	,, 21	26 0	50 45·9	
July 1	38 16	61 27·3		,, 22	24 11	50 15·9	
,, 2	32 15	60 56·7		,, 23	22 29	49 46·3	
,, 3	26 35	60 25·4		,, 24	20 52	49 17·0	1·63
,, 4	21 14	59 53·6	2·48	,, 25	19 21	48 48·1	
,, 5	16 12	59 21·4		,, 26	17 56	48 19·7	
,, 6	11 27	58 48·8		,, 27	16 36	47 51·7	
,, 7	6 59	58 15·9		,, 28	15 21	47 24·1	1·50
,, 8	2 46	57 42·9	2·29	,, 29	14 10	46 56·9	
,, 9	13 58 48	57 9·9		,, 30	13 4	46 30·1	
,, 10	55 6	56 36·8		,, 31	12 1	46 3·8	
,, 11	51 36	56 3·8		Aug. 1	11 1	45 37·9	1·38

PHOTOGRAPH OF BROOKS'S COMET (*a* 1890).—A photograph of this comet was obtained at Algiers on May 22 by M. Ch. Trépied (*Comptes rendus*, June 9, No. 23). Two hours' exposure was found necessary.

ASTRONOMICAL TELESCOPES.[1]

BEFORE speaking of the enormous instruments of the present day, with their various forms and complicated machinery, it will be well to give some little time to a consideration of the principles involved in the construction of the telescope, the manner in which it assists the eye to perceive distant objects, and in a brief and general way to the construction and action of the eye as far it affects the use of the telescope, all as a help to consider in which way we may hope to still further increase our sense of vision.

I will ask you to bear with me when I mention some things that are very well known, but which if brought to mind may render the subject much more easy. Within pretty narrow limits the principles involved in the construction of the telescope are the same whatever form it ultimately assumes. I will take as an illustration the telescope before me, which has served for the finder to a large astronomical telescope, and of which it is really a model. On examination we find that it has, in common with all refracting telescopes, a large lens at one end and several smaller ones at the other ; the number of these small lenses varies according to the purpose for which we use the telescope. Taking out this large lens we find that it is made of two pieces of glass ; but as this has been done for a purpose to be presently explained which does not affect the principle, we will disregard this, and consider it only as a simple convex lens, to the more important properties of which I wish first of all particularly to draw your attention, leaving the construction of telescopes to be dealt with later on.

Stated shortly, such a lens has the power of refracting or bending the rays of light that fall upon it : after they have passed through the lens the course they take is altered ; if we allow the light from a star to fall upon the lens, the whole of the parallel rays coming from the star on to the front surface are brought by this bending action to a point at some constant distance behind, and can be seen as a point of light by placing there a flat screen of any kind that will intercept the light. For all distant objects the distance at which the crossing of the rays takes place is the same. It entirely depends on the substance of the lens and the curvature we give to the surfaces, and not at all upon the aperture or width of the lens. The brightness only of the picture of the star, depends upon the size of the lens, as that determines the amount of light it gathers together. If, instead of one star we have three or four stars together, we will find that this lens will deal with the light from each star just as it did with the light of the first one, and just in proportion to the distance they are apart in the sky, so will the pictures we see of them be apart on our screen. So if we let the light from the moon fall on our lens, all the light from the various parts of the moon's surface will act like the separate stars, and produce a picture of the whole moon (in the photographic camera the lens produces in this manner a picture of objects in front of it which we see on the ground glass). When we attempt to get pictures of near objects that do not send rays of light that are parallel we find that as the rays of light from them do not fall on the lens at the same angle to the axis the picture is formed further away from the lens. The nearer the object whose picture we wish to throw upon the screen is to the lens, the further the screen must be moved. If we try this experiment we will find, when we have the object at the same distance as the screen, the picture is then of the same size as the object, and the distance of the screen from the lens is twice that which we have found as the focal length ; on bringing the object still nearer the lens, we find we must move the screen further and further away, until when the object is at the focus the picture is formed at an infinite distance away, or, what is more to our purpose, the rays of light from an object at the focus of a convex lens go away through the lens parallel, exactly as we have seen such parallel rays falling on the glass come to a focus, so that our diagram answers equally well whatever the direction of the rays ; and this holds good in other cases where we take the effect of reflection as well as refraction.

We can also produce pictures by means of bright concave surfaces acting by reflection on the light falling upon them. Such a mirror or concave reflecting surface as I have here will behave exactly as the lens, excepting, of course, that it will form the picture in front instead of behind. The bending of the rays in the case of the convex lens is convergent, or towards the axis, for all parallel rays ; if we use the reverse form of lens—that is, one thicker at the edge than in the middle—we find the reverse effect on the parallel rays ; they will now be divergent, or bend away from the axis ; and so with reflecting surfaces if we make the concavity of our mirror less and less, till it ceases and we have a plane, we will get no effect on the parallel rays of light except a change of direction after reflection. If we go beyond this and make the surface convex we then will have practically the same effect on the reflected rays as that given to the refracted ray by the concave glass lens.

As regards the size of the picture produced by lenses or mirrors of different focal length, the picture is larger just as the focal length is greater, and the angular dimension is converted into a linear one on the screen in due proportion. Now, as we

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[1] Discourse delivered at the Royal Institution on Friday, May 30, 1890, by Mr. A. A. Common.

shall assume that the eye sees all things best at the distance of about nine inches, we may say that the picture taken with a lens of this focal length gives at once the proper and most natural representation we can possibly have of anything at which we can look. Such a picture of a landscape, if placed before the eye at the distance of nine inches, would exactly cover the real landscape point for point all over. A picture taken with a lens of shorter focal length, say four inches, will give a picture as true in all the details as the larger one, but if this picture is looked at, at nine inches distance, it is not a true representation of what we see ; in order to make it so, we must look at it with a lens or magnifier. With a larger picture one can look at this at the proper distance, which always is the focal distance of the lens with which it was obtained, when we will see everything in the natural angular position that we have in the first case.

But if, instead of looking at this larger picture, which we may consider taken with a lens of say ninety inches focal length, at a distance of ninety inches, we look at it at a distance of nine inches, we have practically destroyed it as a picture by reducing the distance at which we are viewing it, and we have converted it into what is for that particular landscape a telescopic picture ; we see it, not from the point at which it was taken, but just as if we were at one-tenth of the distance from the particular part that we examine. A telescope with a magnifying power of ten, would enable us to see the landscape just as we see it in the photograph, when we examine it in the way I have mentioned.

Having thus seen how a lens or mirror acts, we will turn our attention to the eye. Here we find an optical combination of lenses that act together in the same way as the single convex lens of which we have been speaking. We will call this combination the lens of the eye. It produces a picture of distant objects which in the normal eye falls exactly in focus upon the retina. We are conscious that we do see clearly at all distances beyond about nine inches.

At less than this distance objects become more and more indistinct as they are brought nearer to the eye. From what we have seen of the action of the lens in producing pictures of near and distant objects, we know that some movement of the screen must be made in order to get such pictures sharply focussed, a state of things necessary to perfect vision. We might therefore suppose that the eye did so operate by increasing when necessary the distance between the lens and retina, but we know that the same effect is produced in another way ; in fact, the only other way. The eye by a marvellous provision of nature, secures the distinctness of the picture on the retina of all objects beyond a distance of about 9 inches, by slightly but sufficiently varying the curvature of one of the lenses ; by an effort of will, we can make the accommodating power of the eye slightly greater, and so see things clearly a little nearer ; but at about the distance of 9 inches, the normal eye is unconscious of any effort in thus accommodating itself to different distances. The picture produced by the lens of the eye whose focal length we will assume to be six-tenths of an inch falls on the retina, which we will assume further to be formed of a great number of separate sensible points, which, as it were, pick up the picture where it falls on these points, and through the nervous organization, produce the sense of vision. Possibly when these points are affected by light, there may be some connective action, either produced by some slight spherical aberration of the lens or otherwise ; but I do not wish to go any further in this matter than is necessary to elucidate my subject. What I am concerned with now is the extent to which the sensibility of the retina extends. Experiment tells us that it extends to the perception of two separate points of light whose angular distance apart is one minute of arc, or in other words at the distance we can see best, two points whose distance apart is about 1/400 of an inch.

This marvellous power can be better appreciated when we remember that the actual linear distance apart of two such points on the retina is just a little more than 1/6000 of an inch.

In dealing with the shape of small objects the difference between a circle, square, and triangle, can be detected when the linear size of either is about 1/2000 of an inch. It may be therefore fairly taken that these separate sensible points of the retina are somewhere about 1/12,000 part of an inch apart from each other. Wonderfully minute as must this structure be, we must remember, as we have already shown, that the actual size of the image it deals with is also extremely small. This minuteness becomes apparent when we consider what occurs when we look at some well-known object, such as the full moon. Taking the angular diameter of the moon as 30 minutes of arc, and the focal length of the eye at six-tenths of an inch, we find the linear diameter of the picture of the full moon on the retina is about 1/200 of an inch, and assuming that our number of the points in the retina is correct, it follows that the moon is subject to the scrutiny of 2800 of these points, each capable of dealing with the portion of the picture that falls upon it.

That is to say, the picture, as the retina deals with it, is made up of this number of separate parts, and is incapable of further division, just as if it were a mosaic. I think this is really the case, and as such a supposition permits us to explain not only what occurs when we assist the eye by means of the telescope, but also what occurs when we use the telescope for photographing celestial objects, we will follow it up.

In the case of the eye we suppose the image of the moon to be made up through the agency of these 2800 points, each one capable of noting a variation in the light falling upon it. In order to make this rather important point plainer, I have had a diagrammatic drawing made on this plan. Taking a circle to represent the full moon, I have divided it into this number of spaces, and into each space I have put a black dot, large or small, according to the intensity of the light falling on that part of the image as determined by looking at a photograph of the moon. You will see by the picture of this moon the effect produced. It represents to those who are at a sufficient distance the moon much as it is really seen in the sky.

We can now with a lens of the same focal length as the eye obtain a picture of the full moon exactly of the size of the actual picture on the retina, and if we take a proper photographic process we can get particles of silver approximately of the same sizes as the dots we have used in making our diagram of the moon ; the grouping is not exactly the same, but we may take it as precisely so for our purpose. I have not any photographs of the full moon of this size, but I have some here of the moon about five, seven, and eight days old, which give a good idea of what I mean by the arrangements of the particles of silver being like our diagram.

It is now quite apparent that if we can by any means increase the size of the picture of the moon on the retina or make it larger on the photographic plate, we would be able to employ more of our points or particles of silver, and so be able to see more clearly just in proportion as we increase the size of the picture in relation to the size of the separate parts that make it.

Now the telescope enables us to do this for the eye, and a longer focussed lens will give us a larger photographic picture.

Let us assume that by means of the telescope we have increased the power of the eye one hundred times. The picture of the moon on the retina would now be one-half inch diameter, and instead of employing 2800 points to determine its shape, and the various markings upon it, we should be employing 28,000,000 of these points ; and similarly with the photograph, by increasing the size of our lens we will obtain a picture made up of this enormous number of particles of silver. But we can go further in the magnification of the picture on the retina—we can also use a still longer focus photographic lens.

A power of magnification of one thousand is quite possible under favourable circumstances ; this means that the picture of one two-hundredth of an inch would be now of five inches in diameter, so we must deal with only a portion of it. Let us take a circle of one-tenth of this, equalling one-hundredth of our original picture, which in the eye, unaided by the telescope, would have a diameter of one two-thousandth of an inch, or an area of less than one five-millionth of a square inch. This means that with this magnification, we have increased the power so enormously that we are now employing for the photographic picture two thousand eight hundred million particles of silver, and in the eye the same degree of increase in the number of points of the retina employed in scrutinizing the picture piece by piece as successive portions are brought into the central part.

Photography enables me to show that the result I have given of the wonderful effect of increasing the optical power is perfectly correct as far as it is concerned. We will deal with a part only of the moon, representing, as I have just said, about one-tenth of its diameter, or one-hundredth of its visible surface. Two such portions of the moon are marked, as you see, on the diagram. I have selected these portions as I am able to show you them just as taken on a large scale by photography so that you can make the comparison in the most certain manner ;

but let us first analyze our diagrammatic moon—let us magnify it about ten times, and see what it looks like.

I now show you a picture of this part of the diagram, inclosing the portions I wish to speak about, magnified ten times, so that you can see that about twenty-eight of our points, and by supposition twenty-eight of our particles of silver on the photographic plate, make up the picture. You will see that these dots vary in size; the difference is due to the amount of light falling within what we may call the sphere of action of each point, and should represent it exactly. The result can hardly be called a picture, as it conveys no impression of continuity of form to the mind. We have got down to the structure or separate parts, and to the limit of the powers of the eye and the photographic plate, of course on the assumption we have made as to the size of the points in the one case and the particles of silver in the other. I will now show you the same parts of the moon as represented by the circles on our diagram exactly as delineated by photography. You now see a beautiful picture giving mountains, valleys, craters, peaks, and plains, and all that makes up a picture of lunar scenery. We have thus seen how the power of the eye is increased by the enlargement of the picture on the retina by the telescope, and also how, by increasing the size of the photograph, we also get more and more detail in the picture.

We know we cannot alter the number of those separate points on the retina which determine the limit of our powers of vision in one direction, but we may be able to increase enormously the number of particles of silver in our photographic picture by processes that will give finer deposits, and so, in conjunction with more perfect and larger photographic lenses, we may reasonably look for a great improvement in our sense of vision—it may be even beyond that given by the telescope alone; although it always will be something in favour of the telescope that the magnification obtained in the eye is about fifteen times greater than that obtained by photography when the image on the retina is pitted against the photograph of the same size, unless we use a lens to magnify the photograph of the same focal length as the eye, in which case it is equal. But we *may* go much further in our magnification of the photographic image. In other ways there is great promise when we consider the difference in the action of the eye and the chemical action in the sensitive film under the action of light. As I pointed out in the discourse I gave about four years ago in this theatre, the eye cannot perceive objects that are not sufficiently illuminated, though this same amount of illumination will, by its cumulative effect, make a photographic picture, so that there are ways in which the photographic method of seeing celestial bodies can be possibly made superior to the direct method of looking with a telescope.

With some celestial objects this has been already done: stars too faint to be seen have been photographed, and nebulæ that cannot be seen have also been photographed; but much more than this is possible: we may be able to obtain photographs of the surface of the moon similar to those I have shown, but on a very much larger scale, and we may obtain pictures of the planets that will far surpass the pictures we would see by the telescope alone.

I have mentioned that the distance at which the normal eye can best see things is about nine inches, as that gives the greatest angular size to the object while retaining a sharp picture on the retina; but, as many of us know, eyes differ in this power: two of the common infirmities of the eyes are long- or short-sightedness, due to the pictures being formed behind the retina, in the first case, and in front of it in the other. Towards the end of the thirteenth century it was found that convex lenses would cure the first infirmity, and, soon afterwards, that concave lenses would cure the second, as can be easily seen from what I have said about the action of these lenses; so that during the fifteenth and sixteenth centuries the materials for the making of a telescope existed; in fact, in the sixteenth century, Porta invented the camera obscura, which is in one sense a telescope. It seems very strange that the properties of a convex and concave lens when properly arranged were not known much earlier than 1608. Most probably, if we may judge from the references made by some earlier writers, this knowledge existed, but was not properly appreciated by them. Undoubtedly, after the first telescopes were made in Holland in 1608, the value of this unique instrument was fully appreciated, and the news spread rapidly, for we find that in the next year "Galileo had been appointed lecturer at Padua for life, on account of a perspective like the one which was sent from Flanders to Cardinal Borghese." As far as can be ascertained, Galileo heard of the telescope as an instrument by which distant objects appeared nearer and larger, and that he, with this knowledge only, reinvented it. The Galilean telescope is practically, though not theoretically, the simplest form. It is made of a convex lens in combination with a concave lens to intercept the cone of rays before they come to a focus, and render them parallel so that they can be utilized by the eye. It presents objects as they appear, and the picture is freer from colour in this form than in the other, where a convex eye-glass is used. It is used as one form of opera-glass at the present time. Made of one piece of glass in the shape of a cone, the base of which is ground convex, and the apex slightly truncated and ground concave, it becomes a single-lens telescope that can be looked upon just as an enlargement of the outer lens of the eye.

Galileo was undoubtedly the first to make an astronomical discovery with the telescope: his name is, and always will be, associated with the telescope on this account alone.

Very soon after the introduction of the Galilean telescope, the difficulties that arise from the coloured image produced by a single lens turned attention to the possibility of making a telescope by using the reflecting surface of a concave mirror instead of a lens. Newton, who had imperfectly investigated the decomposition of light produced by its refraction through a prism, was of opinion that the reflecting principle gave the greatest possibilities of increase of power. He invented, and was the first to make, a reflecting telescope on the system that is in use to the present day; thus the two forms of telescope—the refracting and reflecting—came into use within about 60 years of each other. It will be perhaps most convenient in briefly running through the history of the telescope, that I should give what was done in each century.

Commencing, then, with the first application of the telescope to the investigation of the heavenly bodies by Galileo in 1609, we find that the largest telescope he could make gave only a magnifying power of about 30.

The first improvement made in the telescope, as left by Galileo, was due to a suggestion—by some attributed to Kepler, but certainly used by Gascoigne—to replace the concave eye-lens that Galileo used by a convex one. Simple as this change looks, it makes an important, indeed vital improvement. The telescope could now be used, by placing a system of lines or a scale in the common focus of the two lenses, to measure the size of the image produced by the large lens; the axis or line of collimation could be found, and so the telescope could be used on graduated instruments to measure the angular distance of various objects; in fact, we have now in every essential principle the true astronomical telescope. It is useless as an ordinary telescope, as it inverts the objects looked at, while the Galilean retains them in their natural position. The addition, however, of another lens or pair of lenses reinverts the image, and we then have the ordinary telescope. It was soon found that the single lens surrounds all bright objects with a fringe of colour, always of a width of about one-fiftieth of the diameter of the object-glass, as we must now call the large lens; and as this width of fringe was the same whatever the focal length of the object-glass, the advantage of increasing this focal length and so getting a larger image without increasing the size of the coloured fringe became apparent, and the telescope therefore was made longer and longer, till a length of over one hundred feet was reached; in fact, they were made so long that they could not be used. A picture of one of these is shown, from which it can be easily imagined the difficulties of using it must have been very great, yet some most important measurements have been made with these long telescopes. Beyond the suggestions of Gregory and Cassegrain for improvements in the reflecting telescope, little was done with this instrument.

During the eighteenth century immense advances were made in both kinds of telescopes. With the invention of the achromatic telescope by Hall and Dollond, the long-focussed telescopes disappeared.

Newton had turned to the reflecting telescopes believing from his investigations that the dispersion and refraction were constant for all substances; this was found not to be so, and hence a means was possible to render the coloured fringe that surrounds bright objects when a single lens is used less prominent, by using two kinds of glass for the lens, one giving more refraction with somewhat similar dispersion, so that while the dispersion of one lens is almost corrected or neutralized by the other, there is still a refraction that enables the combination to be used as a lens giving an image almost free from colour.

In 1733, Hall had made telescopes having double object-glasses on this plan, but never published the fact. Dollond, who had worked independently at the subject, came to the conclusion that the thing could be done, and succeeded in doing it; the invention of the achromatic telescope is with justice, therefore, connected with his name.

Although this invention was a most important one, full advantage could not be taken of it owing to the difficulty of getting disks of glass large enough to make into the compound object-glass, disks of about four inches being the largest diameter it was possible to obtain. With the reflecting telescope, unhampered as it always has been by all except mechanical difficulties, advance was possible, and astronomers turned to it as the only means of getting larger instruments. Many most excellent instruments were made on the Newtonian plan. The plan proposed by Gregory was largely used, as in this instrument objects are seen in their natural position, so that the telescope could be employed for ordinary purposes.

Many were also made on the plan proposed by Cassegrain. The diagrams on the wall enable you to at once see the essential points of these different forms of reflectors.

About 1776, Herschel commenced his astronomical work; beginning with reflecting telescopes of six or seven inches, he ultimately succeeded in making one of four feet aperture with these instruments. As everyone knows, most brilliant discoveries were effected, and the first real survey of the heavens made.

Herschel's larger telescopes were mounted by swinging them in a surrounding framed scaffolding that could itself be rotated. The smaller ones were mostly mounted on the plan of the one now before us, which the Council of the Royal Astronomical Society have kindly allowed me to bring here. The plan nearly always used by Sir William Herschel was the Newtonian, though for the larger instruments he used the plan proposed years before by Le Maire, but better known as the Herschelian, when the observer looks directly at the large mirror, which is slightly tilted, so that his body does not hinder the light reaching the telescope. In all cases the substance used for the mirrors was what is called speculum metal.

During the present century the aperture of the refracting telescope has increased enormously; the manufacture of the glass disks has been brought to a high state of perfection, particularly in France, where more attention is given to this manufacture than in any other country. Early in the century the great difficulty was in making the disks of flint glass. M. Guinand, a Swiss, beginning in 1784, succeeded in 1805 in getting disks of glass larger and finer than had been made before, and refractors grew larger and larger as the glass was made. In 1823 we have the Dorpat glass of 9·6 inches, the first large equatorial mounted with clock-work; in 1837 the 12-inch Munich glass; in 1839 the 15-inch at Harvard, and in 1847 another at Pulkowa; in 1863 Cooke finished the 25-inch refractor which Mr. Newall gave, shortly before his death last year, to the Cambridge University.

This telescope the University has accepted, and it is about to be removed to the Observatory at Cambridge, where it will be in charge of the Director, Dr. Adams. In accordance with the expressed wish of the late Mr. Newall, it will be devoted to a study of stellar and astronomical physics. There is every prospect that this will be properly done, as Mr. Frank Newall, one of the sons of the late Mr. Newall, has offered his personal services for five years in carrying on this work. Succeeding this we have the 26-inch telescope at Washington, the 26-inch at the University of Virginia, the 30-inch at Pulkowa, and the 36-inch lately erected at Mount Hamilton, California—all these latter by Alvan Clark and his sons. By Sir Howard Grubb we have many telescopes, including the 28-inch at Vienna. Most of these telescopes have been produced during the last twenty years, as well as quite a host of others of smaller sizes, including nearly a score of telescopes of about 13 inches diameter by various makers, to be employed in the construction of the photographic chart of the heavens, which it has been decided to do by international co-operation.

The first of these photographic instruments was made by the Brothers Henry, of the Paris Observatory, who have also made many very fine object-glasses and specula, and more important than all, have shown that plane mirrors of perfect flatness can be made of almost any size; the success of M. Lœwy's new telescope, the equatorial *coudé*, being entirely due to the marvellous perfection of the plane mirrors made by them.

The reflecting telescope has quite kept pace with its elder brother.

Lassell in 1820 began the grinding of mirrors, he like Sir William Herschel working through various sizes, finally completing one of 4 feet aperture, which was mounted equatorially. Lord Rosse also took up this work in 1840; he made two 3-foot specula, and in 1845 finished what yet remains the largest telescope, one of 6 feet aperture. All these were of speculum metal, and all on the Newtonian form. In 1870, Grubb completed for the Melbourne Observatory a telescope of 4 feet aperture, on the Cassegrain plan, the only large example. This is of speculum metal. In 1856 it was proposed by Steinheil, and in 1857 by Foucault, to use glass as the material for the concave mirror, covering the surface with a fine deposit of metallic silver in the manner that had then just been perfected. In 1858, Draper, in America, completed one on this plan of 15 inches aperture, soon after making another of 28 inches. In France several large ones have been made, including one of 4 feet at the Paris Observatory: in England this form of telescope is largely used, and mirrors up to 5 feet in diameter have been made and mounted equatorially.

Optically the astronomical telescope, particularly the refractor, has arrived at a splendid state of excellence; the purity of the glass disks and the perfection of the surfaces is proved at once by the performance of the various large telescopes. No limit has yet been set to the increase of size by the impossibility of getting disks of glass or working them, nor is it probable that the limit will be set by either of these considerations. We must rather look for our limiting conditions to the immense cost of mounting large glasses, and the absorption of the glass of which the lenses are made coming injuriously into play to reduce the light-gathering power, though it will be probably a long time before this latter evil will be much felt.

With the reflecting telescope the greater attention given to the working and testing of the optical surface has enabled the concave mirror to be made with a certainty that the earlier workers never dreamed of. The examination of the surface can be made optically at the centre of curvature of the mirror in the manner that was used by Hadley in the beginning of the last century, and revived some years ago by Foucault, who brought this method of testing specula to a high degree of perfection; in fact, with the addition of certain methods of measuring the longitudinal aberrations we have now a means of readily testing mirrors with a degree of accuracy that far exceeds the skill of the worker. It enables every change that is made in the surface during the progress of the figuring, as the parabolization of the surface is called, to be watched and recorded, and the exact departure of any part from the theoretical form measured and corrected; mirrors can be made of very much greater ratio of aperture to focal length. I have one here where the focal length is only 2¼ times the aperture: such a mirror in the days of speculum metal mirrors with the methods then in use would have necessarily had a focal length of about 20 feet. The difference in curvature between the centre and edge of this mirror is so great that it can be easily measured by an ordinary spherometer, amounting as it does with one of 6 inches diameter to 3/10,000 of an inch, an amount sufficient to make the focus of the outer portion about 1 inch longer than the inner when it is tested at the centre of curvature. The diagram on the wall, copied roughly from one of the records I keep of the progress of the work on a mirror during the figuring, shows how this system of measurements enables one to follow closely the whole operation.

The use of silver on glass as the reflecting surface is as important an improvement in the astronomical telescope as the invention of the achromatic telescope. It gives a permanency to a good figure once obtained that did not exist with the mirrors of speculum metal. To restore the surface of silver to the glass speculum is only a small matter now. How readily this is done may be seen by the practical illustration of the method I will give. I have here two liquids—one a solution of the oxide of silver, and another a reducing agent, the chief material in solution being sugar. I pour the two together in this vessel, the surface of which has been cleaned and kept wet by distilled water, which I shall partly empty, leaving the rest to mix with the two solutions; you will see in the course of about 5 minutes the silver begin to form, eventually covering the whole surface with a brilliant coating that can be polished on the outer surface as bright as that you will see through the glass.

Reflecting telescopes have advantages over the refracting telescopes in many ways, but in some respects they are not so good. They give images that are absolutely achromatic, while the other form always has some uncorrected colour. They can be made shorter, and as the light-grasping power is not reduced by the absorption of the glass of which the lenses are made, it is in direct proportion to the surface or area of the mirror. They have not had in many cases the same care bestowed upon either their manufacture or upon their mounting as has been given in nearly every case to the refracting telescope. Speaking generally, the mounting of the reflecting telescope has nearly always been of a very imperfect kind—a matter of great consequence, for upon the mounting of the astronomical telescope so much depends. To direct the tube to any object is not difficult, but to keep it steadily moving so that the object remains on the field of view requires that the tube should be carried by an equatorial mounting of an efficient character. The first essential of such a mounting is an axis parallel to the axis of rotation of the earth. The tube, being supported on this, will follow any celestial object, such as a star, by simply turning the polar axis in a contrary motion to that of the earth at the same rate. If we make the telescope to swing in a plane parallel to the polar axis, we can then direct the telescope to any part of the sky, and we have the complete equatorial movement. There are several ways in which this is practically done: we can have a long open-work polar axis supported at top and bottom, and swing the telescope in this, or we can have short strong axes. As examples of the first, I will show you pictures of the mountings designed for Cambridge and Greenwich Observatories some forty years ago by Sir G. Airy, lately and for so long our eminent Astronomer-Royal; and as examples of the other form, amongst others, the large telescope lately erected at Nice, and also the larger one at Mount Hamilton, California, now under the direction of Prof. Holden.

The plan of bringing all the various handles and wheels that control the movement of the telescope and the various accessories down to the eye end, so as to be within reach of the observer, is carried to the highest possible degree of perfection here, as we can see by an inspection of the picture of the eye end of this telescope. The observer with the reflecting telescope is, with moderate-size instruments, never very far from the floor, but in the case of the Lick telescope he might have to ascend some thirty feet for objects low down in the sky; but, thanks to the ingenuity of Sir Howard Grubb, to whom the idea is due, the floor of the whole Observatory is made to rise and fall by hydraulic machinery at the will of the observer—a charming but expensive way of solving the difficulty, as far as safety goes, but not meeting the constant need of a change in position as the telescope swings round in keeping up with the motion of the object to which it is directed. The great length of the tube of large refractors is well seen in this picture of the Lick telescope; it suggests flexure as the change is made in the direction in which it points, and the consequent change of stress in the different parts of the tube.

The mounting of the reflector has been treated, if not so successfully, with more variety than in the case of the refractor as we shall see from the pictures I will show you, especially where the Newtonian form is used. The 4-foot reflector at Melbourne is mounted on the German plan, in a similar way to a refractor, and an almost identical plan has been followed by the makers of the 4-foot at the Paris Observatory. Lassell, who was the first to mount a large reflector equatorially, used a mounting that may be called the forked mounting, the polar axis being forked at its upper end, and the tube of the telescope swinging between the forks: a very excellent plan, dispensing with all counterpoising. Wishing to obtain certain conditions that I thought and think now favourable to the performance of the reflector, I devised a mounting where the whole tube was supported at one end on a bent arm; a 3-foot mirror was mounted on this plan in 1879, and worked admirably. The Newtonian form demands the presence of the observer near the high end of the telescope, and the trouble of getting him there and keeping him safely close to the eye-piece is very great. As we see from the various photographs, several means have been employed to do this, none of them quite satisfactory.

All the refracting telescopes of note in the world are covered by domes that effectually protect them from the weather; these domes are in some cases comparable in cost with the instruments they cover. It is not surprising, therefore, that efforts have been made to devise a means of getting rid of this costly dome and the long movable tube.

It was suggested many years ago that a combination of plane mirrors could be used to direct light from any object into a fixed telescope. This idea in a modified form has often been used for special work, one plane mirror being used as we see in the picture on the screen to throw a beam of light into a telescope fixed horizontally; for certain kinds of work this does admirably, but the range is restricted, as can be easily seen, and the object rotates in the field of view as the earth goes round. The next step would be to place the telescope pointing parallel to the axis of the earth and send the beam of light into it from the mirror, which could now be carried by the tube so that by simply rotating the tube on its own axis the object would be kept in the field of view. Sir Howard Grubb makes a small telescope on this plan, and some years ago proposed a somewhat similar plan. A sketch of this plan I will show you. You will see, however, that here again the range is restricted, and, to use the telescope, means would be required to constantly vary the inclination of the small mirror at one-half the rate of inclination of the short tube carrying the object-glass.

By the use of two plane mirrors, however, the solution of the problem of a fixed rotating telescope tube placed as a polar axis is solved. By having such a telescope with a plane mirror at an angle of 45° to the axis of the telescope in front of the object-glass, we can, by simply rotating the telescope, see every object lying on the equator; and by adding another similar plane mirror at an angle of 45° to the axis of the telescope, *as bent out at right angles by the first plane mirror*, and giving the mirror a rotation perpendicular to this axis, we obtain the same power of pointing the telescope as we have in the equatorial. The idea of doing this was published many years ago, but it was left to the skill and perseverance of M. Lœwy, of the Paris Observatory, to put it into practical use. He devised, and had made, a telescope on this principle, of 10½ inches aperture, which was completed in 1882. It has proved itself an unqualified success, and many other larger ones are now being made in Paris, including one of 23 inches aperture, now nearly completed, for the Paris Observatory.

A lantern copy of a drawing of this latter telescope will be thrown on the screen, in order that you may see what manifest advantages exist in this form of telescope. There is but one objection that can be urged—that is, the possible damage to the definition by the plane mirrors; but this seems, from what I have seen of the wonderful perfection of the plane mirrors made by the Brothers Henry, to be an unreasonable one—at any rate not an insurmountable one. In every other respect, except perhaps a slight loss of light, this form of telescope is so manifestly superior to the ordinary form that it must supersede it in time, not only for general work, but for such work as photography and spectroscopy.

ANNUAL VISITATION OF GREENWICH OBSERVATORY.

THE Report of the Astronomer-Royal to the Board of Visitors of the Royal Observatory, Greenwich, was read at the Annual Visitation on June 7. The Report presented refers to the year 1889 May 11 to 1890 May 10, and exhibits the state of the Observatory on the last-named day.

With respect to astronomical observations it is noted that, at the request of Dr. Gill, special attention has been paid to the oppositions of the minor planets Victoria and Sappho. Victoria has been observed 15 times on the meridian, and Sappho 9 times; while 244 observations have been made of 41 comparison stars for Victoria, and 151 observations of 42 stars for Sappho. At the request of Dr. Auwers, observations of the Sappho stars will be renewed in the autumn of this year, and an investigation made of the variation of personality with magnitude, for use in reducing the observations to a uniform system.

The Lassell, south-east, Sheepshanks, and Shuckburgh equatorials are in good working order. Great difficulty has been experienced at times in turning the south-east dome, and a careful examination shows that this may be largely due to the irregular shape of the cannon balls on which it rolls, and to a sagging of the dome curb in some parts.

The tube for the 28-inch refractor, which is of special construction, has been made by Sir H. Grubb in preparation for the object-glass which is now being figured. The experimental

4-inch object-glass referred to in the last Report was mounted on the Sheepshanks equatorial, and 18 photographs were taken with it last summer, the lenses being separated for photographic achromatism, and the crown lens reversed to correct for the spherical aberration introduced by the separation. The best distance of separation was determined, and the photographs obtained were found to be quite satisfactory. The completion of the 28-inch object-glass has been delayed presumably by the pressure of work on the 13-inch photographic telescopes, which have engaged so much of Sir H. Grubb's attention, but it is hoped that the new refractor will be ready for mounting very shortly.

The 13-inch photographic refractor, with 10-inch guiding telescope, by Sir H. Grubb, has been lately mounted in the new 18-foot dome, and one or two trial photographs have been taken with it.

Since the date of the last Report, 14 occultations of stars by the moon (9 disappearances and 5 reappearances) and 13 phenomena of Jupiter's satellites have been observed with the equatorials, or with the altazimuth. These observations are completely reduced to the end of 1889. The occultation of Jupiter by the moon on August 7 was observed with 5 instruments.

Comets have been observed with the Sheepshanks equatorial on 11 nights as follows: Comet *a* 1889 on 6 nights, Comet *d* 1889 on 1 night, Comet *a* 1890 on 4 nights.

The conjunction of Mars and Saturn on September 19 was observed with the south-east equatorial under favourable atmospheric conditions, and nineteen differential observations made of right ascension and north polar distance.

As regards spectroscopic and photographic observations, 457 measures have been made of the displacement of the F line in the spectra of 36 stars, and 20 of the *b* line in the spectra of 5 stars for determinations of motions of approach or recession. Observations of Algol on 7 nights confirm, as far as they go, the previous results indicating orbital motion. The observations of Spica made in past years are found by Prof. Bakhuyzen to be tolerably well represented on the hypothesis of orbital motion with a period of 4 days 0·386 hours, which agrees well with that recently discovered by Dr. Vogel with his photographic method. As the series of observations with the 12¾-inch refractor (extending over 15 years) will be shortly brought to a conclusion, it is proposed to discuss them with a view to the detection of orbital motion. The spectra of R Andromeda, χ Cygni, and Uranus, have been examined on several occasions, and Comet *e* 1889 on 1 night.

The sun has been free from spots on 211 days in the year 1889, the longest spotless period being October 23 to December 11. There were also eight other spotless periods of more than a fortnight. The mean daily spotted area in 1889 was 78, as compared with 89 for 1888: but the mean daily area for the latter half of the year was nearly twice as great as for the earlier half, being 103 as compared with 53. Again, the mean distance of spots from the equator was 5°·46 in the first six months, and 14°·72 for the last six; and both these facts thus point to the middle of the year 1889 as a well-defined date for the sun-spot minimum.

The following are the principal results for the magnetic elements for 1889:—

Mean declination	17° 34′·9
Mean horizontal force	3·9494 (in British units). 1·8210 (in metric units).
Mean dip	67° 22′ 52″ (by 9-inch needles). 67 23 58 (by 6-inch needles). 67 25 36 (by 3-inch needles).

In the year 1889 there were only two days of great magnetic disturbance, but there were also about twenty other days of lesser disturbance, for which tracings of the photographic curves will be published, as well as tracings of the registers on four typical quiet days.

The mean temperature of the year 1889 was 48°·8, being 0°·4 below the average of the preceding 48 years. The highest air temperature in the shade was 86°·6 on August 1, and the lowest 18°·7 on March 4. The mean monthly temperature in 1889 was below the average in all months excepting May, June, and November. In February and December it was below the average by 2°·4 and 2°·2 respectively, and in May above by 3°·9.

The mean daily motion of the air in 1889 was 245 miles, being 39 miles below the average of the preceding 22 years. The greatest daily motion was 736 miles on October 7, and the least 25 miles on September 3. The greatest pressure registered was 15 lbs. on the square foot on October 7.

The number of hours of bright sunshine recorded during 1889 by the Campbell-Stokes sunshine instrument was 1156, which is about 146 hours below the average of the preceding 12 years, after making allowance for difference of the indications with the Campbell and Campbell-Stokes instruments respectively. The aggregate number of hours during which the sun was above the horizon was 4454, so that the mean proportion of sunshine for the year was 0·260, constant sunshine being represented by 1.

The rainfall in 1889 was 23·3 inches, being 1·3 inches below the average of the preceding 48 years.

It was mentioned in the last Report that the Indian invariable pendulums had been mounted in the Record Room under General Walker's supervision. The three pendulums have each been swung 8 times, at pressures of both 2 inches and 27 inches, and the observations completely reduced, giving the following results for number of vibrations in a mean solar day, reduced to vacuum, a temperature of 62°, an infinitely small arc, and sea-level; the corresponding values obtained at Kew being appended for comparison:—

Pendulum.	Greenwich.	Kew.
4	86,165·54	86,166·50
6	86,065·70	86,066·61
11	86,117·04	86,117·03

The tabulation of results for the period of the fifty years of observations will be completed at the end of this year, and will be useful for many purposes. In the twenty years' meteorological reductions, the values were grouped generally in months, mainly for the determination of diurnal inequalities of the thermometer and barometer. In the tables of meteorological averages now proposed, however, the values will be grouped by days, so as to exhibit mean values for each day of mean daily temperature, maximum, minimum, barometer, velocity of wind, frequency of gales, rainfall, and cloud, obtained from the Greenwich observations of fifty years, 1841–90.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Cambridge.—The following are the speeches delivered by the Public Orator (Dr. Sandys, tutor of St. John's College) in presenting Sir Andrew Clark, Mr. Jonathan Hutchinson, Dr. John Evans, Prof. Sylvester, and Mr. A. J. Ellis for honorary degrees on June 10:—

Salutamus deinceps salutis ministrum, Aesculapii e filiis unum, quem idcirco praesertim Machaona nominaverim quod saeculi nostri oratorum cum Nestore ipso totiens consociatus est;—nisi forte, Romano potius exemplo delectatus, mavult Asclepiadis illius disertissimi nomen mutuari, quo medico et amico utebatur Lucius Licinius Crassus, saeculi sui oratorum eloquentissimus. In re publica partium liberalium studiosus, in re privata liberalitate singulari insignis, non modo medicinae sed etiam philosophiae et religionis penetralia ingressus est. Etiam antiquos meministis quondam non de corporis tantum salute sed etiam de rebus fere omnibus quae vitam anxiam et sollicitam reddant, ab ipso Aesculapio solitos esse oracula exposcere. Viri talis igitur, velut iurisconsulti Romani, domus, est velut civitatis oraculum, unde cives eius, ut Apollo Pythius apud Ennium dicit, consilium expetunt, non salutis tantum sed etiam "summarum rerum incerti," quos incepti certos "compotesque consili dimittit." Ergo virum, quem aut litterarum aut scientiae aut medicinae doctorem nominare potuissemus, iuris doctorem non immerito creamus.

Duco ad vos medicinae professorem emeritum, Regii Medicorum Collegii Londinensis praesidem, baronettum insignem, suavem, eruditum, eloquentem, Andream Clark.

Etiam alter Aesculapii filiorum, Podalirius (nisi fallor), hodie nobis sese praesentem obtulit, quem a fratre suo idcirco disiungere neque possumus neque volumus, primum quod professoris in munere quondam erat collega eius coniunctissimus, deinde quod forte quadam domum vicinam atque adeo proximam incolit, denique quod dignitate non minore Collegio alteri praesidet, ubi

Britanniae chirurgi per tot annos quasi penates suos posuerunt. Medicinae studiosis nota sunt scripta eius per seriem longam edita, in quibus pars ea medicinae quae manu curat illustratur, et litterarum monumentis mandatur. Neque silentio praeterire possumus quaecumque de pathologia praesertim, quam quondam profitebatur, accuratissime scripsit; scilicet mortem ipsam, quae aliis tacet, huic velut rerum naturae vati et interpreti constat esse eloquentem. Neque prorsus intacta relinquimus quicquid de morborum contagione disputavit. Medicorum nemo fortasse Horatii verba in re medica saltem eruditius illustravit:—

> delicta maiorum immeritus lues.

Duco ad vos Regii Chirurgorum Collegii praesidem, chirurgum illustrem, JONATHAN HUTCHINSON.

Archaeologiae studia nonnulli certe arida mentis nutrimenta arbitrantur. Hic autem etiam difficili in materia ingenii sui non minus facilis quam felicis alimentum invenit, qui etiam silices duros diu habuit in deliciis, ex ipsoque saxo doctrinae scintillam saepenumero excudit,

> suscepitque ignem foliis atque arida circum
> nutrimenta dedit, rapuitque in fomite flammam.

Quicquid lapidis, quicquid aeris, quicquid auri et argenti Britannia antiqua usurpabat, assidue conquisivit; conquisitum erudite illustravit. Britanniae nummorum investigator acerrimus, propterea etiam ultra fretum Britannicum numismate aureo honoris causa donatus est. Neque antiquis tantum thesauris operam dedisse videtur, sed etiam Societatis Regiae praefectus aerario, tot scientiis auxilium quotannis certatim flagitantibus, pecuniae publicae dispensator providus, aequus, benignus exstitit. Quondam Geologicae, iamdudum Numismaticae Societati praepositus, nunc etiam Antiquitatis peritorum Societati maximae summa cum dignitate praesidet. Quot scientiarum trans provincias aquilas suas felices tulit! Quid si non (velut alter ille quem hodie expectabamus)—quid, inquam, si non "nomen ab Africa lucratus rediit," tamen laudes eius Musae nullae "clarius indicant, quam Calabrae Pierides, neque

> si chartae taceant quod bene feceris
> mercedem tuleris.

Audite igitur ipsum Ennium viri huiusce praeconia praesagientem:—

> doctus, fidelis,
> suavis homo, facundus, suo contentus, beatus,
> scitus, secunda loquens in tempore . . .
> multa tenens antiqua.

Duco ad vos virum de antiquitatis studiis praeclare meritum, JOANNEM EVANS.

Plusquam tres et quinquaginta anni sunt elapsi, ex quo Academiae nostrae inter silvas adulescens quidam errabat, populi sacri antiquissima stirpe oriundus, cuius maiores ultimi primum Chaldaeorum in campis, deinde Palestinae in collibus, caeli nocturni stellas innumerabiles, prolis futurae velut imaginem referentes, non sine reverentia quadam suspiciebant. Ipse numerorum peritia praeclarus, primum inter Londinenses Academiae nostrae studia praecipua ingenii sui lumine illustrabat. Postea trans aequor Atlanticum plusquam semel honorifice vocatus, fratribus nostris transmarinis doctrinae mathematicae facem praeferebat. Nuper professoris insignis in locum electus, et Britanniae non sine laude redditus, in Academia Oxoniensi scientiae flammam indies clariorem excitat. Ubicunque incedit, exemplo suo nova studia semper accendit. Sive numerorum θεωρίαν explicat, sive Geometriae recentioris terminos extendit, sive regni sui velut in puro caelo regiones prius inexploratas pererrat, scientiae suae inter principes ubique conspicitur. Nonnulla quae Newtonus noster, quae Fresnelius, Iacobius, Sturmius, alii, imperfecta reliquerunt, Sylvester noster aut elegantius explicavit, aut argumentis veris comprobavit. Quam parvis ab initiis argumenta quam magna evolvit; quotiens res prius abditas exprimere conatus, sermonem nostrum ditavit, et nova rerum nomina audacter protulit! Arte quali numerorum leges non modo poetis antiquis interpretandis sed etiam carminibus novis pangendis accommodat! Neque surdis canit, sed "respondent omnia silvae," si quando, inter rerum graviorum curas, aevi prioris pastores aemulatus,

> Silvestrem tenui musam meditatur avena.

Duco ad vos Collegii Divi Ioannis Socium, trium simul Academiarum Senatorem, quattuor deinceps Academiarum Professorem, IACOBUM IOSEPHUM SYLVESTER.

Claudit seriem viri eiusdem aequalis, qui doctrinae rudimentis primum Salopiae, deinde Etonae, denique Trinitatis in Collegio maximo imbutus, eadem in Academia isdem e studiis lauream suam primam reportavit. Sed ne his quidem finibus contentus, etiam musices mysteria perscrutatus est, et philologiae provinciam satis amplam sibi vindicavit. Quanta perseverantia etiam contra consuetudinem, ut Quintiliani verbis utar, "sic scribendum quidque iudicat, quomodo sonat"! Quanta subtilitate de linguae Graecae et Latinae vocalibus disputat; quam minuta curiositate etiam patrii sermonis sonum unumquemque explorat! A poetis nostris antiquioribus exorsus, non modo saeculorum priorum voces temporis lapsu obscuratas oculis et auribus nostris denuo reddidit, sed etiam nostro a saeculo in dialectis variis usurpatam litterarum appellationem, signis accuratis notatam, posteritati serae cognoscendam tradidit. Venient anni (licet confidenter vaticinari) quibus dialectorum nostrarum tot varietates, non minus quam Arcadum et Cypriorum linguae antiquae, hominum e cognitione prorsus obsolescent; tum profecto viri huiusce scriptis cura infinita elaboratis indies auctus accedet honos.

> Mortalia facta peribunt,
> nedum sermonum stet honos et gratia vivax.

Interim a nobis certe sermonis Britannici conservator anim, grati testimonium, honoremque diu debitum, diu duraturum accipiet.

Duco ad vos philologum insignem, ALEXANDRUM JOANNEM ELLIS.

At the annual election at St. John's College, on June 16, the following awards in Mathematics and Natural Science were made:—

Mathematics—Foundation Scholarships continued or increased: Bennett (£100), Reeves (£80), Alexander (£70), Dobbs (£60), Finn (£50), Gedge (£40), Hough (£80), Chevalier (£60), Pocklington (£80), Rosenberg (£50). Foundation Scholarships awarded: Wills (£60), Owen (£50), Schmitz (£40), Pickford (£40), Maw (£40). Exhibitions: Dobbs, Wills, Finn, Owen, Schmitz, Pickford, Maw, Robertson, Bloomfield, Spaight, Ayers, Morton. Proper Sizarship: Le Sueur. Natural Science—Foundation Scholarships continued or increased: Groom (£60), Hankin (£40), Horton-Smith (£40), Hewitt (£80), Lehfeldt (£80), Woods (£40). Foundation Scholarships awarded: Blackman (£40), MacBride (£60), Cuff (£40), Whipple (£40). Exhibitions: Woods, Baker. Proper Sizarship: Baker. Hutchinson Studentship for Pathological Research, Hankin. Wright's Prizes: Mathematics, Hough; Natural Science, Hewitt, Lehfeldt, MacBride. Hughes Prize for most distinguished student of the third year, Bennett (Mathematics). Hockin Prize for Experimental Physics, Lehfeldt.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 12.—"A Record of the Results obtained by Electrical Excitation of the so-called Motor Cortex and Internal Capsule in an Orang Outang (*Simia satyrus*)." By Charles E. Beevor, M.D., F.R.C.P., and Victor Horsley, B.S., F.R.S. (From the Laboratory of the Brown Institution.)

Having been engaged for some time in investigating the representation of motor function in the cortex of the bonnet monkey, we thought it advisable to perform the same in an anthropoid as likely thereby to gain a closer insight into the modes of representation in man.

We first describe the peculiarities noticeable in the configuration of the convolutions in the orang.

As in the bonnet monkey, after narcotization with ether, we divided the cortex into squares of 2 millimetres side, and excited the same with minimal stimuli from the secondary coil of an inductorium; a remarkably high intensity of the stimulus being required.

General Results.—The mode of representation of motor function was found to be highly specialized. The general plan was identical with that seen in the bonnet monkey in that the representation of each segment and part of the body in the orang was arranged in the same order as that according to which we found the representation of the primary movements to be grouped in the macacque monkey.

In addition to this, the areas for the representation of the different parts of the body we found not to be continuous with each other, but that between the areas of representation (for instance, of the face and the upper limb) there were regions of inexcitable cortex showing a degree of differentiation not obtained in the lower monkey.

A further remarkable evidence of specialization was noticeable in the fact that excitation of any one point elicited rarely more than one movement, and only of one segment, *e.g.* simple flexion of the elbow. Consequently, any sequence of movement or march was conspicuously infrequent.

Finally, the character of each movement and its localization was recorded.

After the cortex had been removed, we proceeded to stimulate the fibres of the internal capsule, and the results obtained confirmed those obtained from the bonnet monkey, and at the same time showed the relative position of the cortical areas.

The internal capsule was exposed by removing half of one hemisphere by a horizontal section; the outlines of the basal ganglia were then transferred to paper ruled with squares of 1 millimetre, and the resulting movement obtained by stimulating each of these squares contained in the internal capsule was recorded. The movements obtained correspond generally with the results which we have in another paper presented to the Royal Society, and read on December 12, 1889.

Physical Society, May 16.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—Lord Rayleigh exhibited and described an arrangement of Huyghens's gearing in illustration of electric induction. This gearing consists of two loose pulleys mounted on the same axle, with an endless cord laid over them, the loops or bights of which carry weighted pulleys whose planes are parallel to the axis on which the upper pulleys turn. If one of the latter pulleys be started to rotate, the other one turns in the opposite direction until such time as the speed of the first one becomes constant. Whilst this constant speed is maintained, the second pulley remains stationary, one weight being raised and the other lowered, but on retarding the motion of the first pulley, the second begins to turn in the same direction as the first. It will be noticed that the phenomena are analogous to those which occur in electric induction, where starting or increasing a current in one circuit induces an opposite current in a neighbouring circuit, whilst decreasing or stopping a current induces one in its own direction. Lord Rayleigh pointed out that in this apparatus there is nothing strictly analogous to electric resistance, for the friction does not follow the same law. The analogy, he said, was complete as regards there being no change of potential energy, and the mathematical equations for the kinetic energy of the system are precisely the same as those given by Maxwell for electric induction.—Dr. S. P. Thompson made a communication on Dr. Kœnig's researches on the physical basis of music, in the course of which Dr. Kœnig performed numerous novel and interesting experiments, clearly illustrating the subject to a crowded audience. After referring to the classical researches of the great mechanician, and to the remarkable precision with which his ingenious and unique acoustical apparatus is constructed, Dr. Thompson said the subject with which he wished to deal could be divided into two parts, the first relating to *beats*, and the second to the *timbre* of sounds. On the question of beats considerable discussion had taken place as to whether they formed independent tones if they were sufficiently rapid. Different authorities had come to different conclusions on the subject, the disagreement probably arising from the impure tones used in their investigations. Dr. Kœnig, however, had succeeded in making tuning-forks whose sounds are very nearly pure tones, and by the aid of such forks had conclusively answered the question in the affirmative. Before proceeding to show experimentally the truth of the conclusions arrived at, Dr. Thompson said it was necessary to define exactly the meaning of the term "harmonics." By this he meant tones whose frequencies are *true integral multiples* of their fundamental. This, he said, might seem to be identical with the "upper partial tones" of Helmholtz or the "overtones" of Tyndall, but such was not the case, as the upper partial tones of piano-wires, &c., are not true integral multiples of the fundamentals, for the rigidity of the wire comes into play, and prevents the subdivision being exact. According to Helmholtz's theory, two tones harmonize when they do not produce beats of sufficient slowness to grate upon the ear, and the frequency of the two sets of beats were supposed to be equal to the difference and the sum of the frequencies of the two fundamental tones. In investigating the subject, Kœnig finds it necessary to distinguish between primary and secondary beats, and also that primary beats belong to two categories. These categories he calls "inferior" and "superior" respectively, and the frequencies of the two sets correspond respectively to the positive and negative remainders obtained by dividing the number representing the number of vibrations in the tone of lowest pitch into the corresponding number for the higher tone. For example, two forks of 100 and 492 vibrations produce beats having 92 and 8 as their vibration frequencies, for

$$492 = 100 \times 4 + 92,$$

and also

$$492 = 100 \times 5 - 8.$$

A set of "superior" beats of 8 per second and an "inferior" beat-tone of 92 per second may be heard when two such forks are sounded together. These primary beats or beat-tones act as independent tones and produce secondary beats. Tertiary ones may also be obtained. To demonstrate the existence of beats to the large audience assembled, Dr. Kœnig had provided two large tuning-forks with resonators about 4 feet long. One of the forks gave 64 vibrations per second, and the other 128, but the latter had sliding weights, whereby its frequency could be made anything between 128 and 64. Adjusting the weights so as to give 72, and bowing both forks, the beats of about 8 per second were distinctly heard at the extremity of the room. By varying the weights so that the fork gave 80, $85\frac{1}{3}$, 96, $106\frac{2}{3}$, 112, 120, and 128 vibrations successively, beats of various frequencies were produced, and it was remarkable to note that tones of 64 and 120 produced 8 beats a second exactly like 64 and 72. When the forks made 64 and 96 vibrations—*i.e.* at an interval of a fifth—then the inferior and superior beats agree in frequency, viz. 32, and by careful observation a low tone of about this pitch could be heard. If the tones sounded simultaneously differ by more than an octave, the same law for the numbers of beats holds good, whilst Helmholtz's difference and summation tones law, is inapplicable. This was shown by sounding a fork and its double octave slightly mistuned by weighting; slow beats were quite evident, although the difference in the frequencies of the primary notes was large. Similarly forks vibrating approximately at rates in the proportions 1 : 5 and 1 : 6 gave slow beats. Coming to the main question, as to whether beats when sufficiently rapid blend into tones just as primary shocks do, Dr. Thompson briefly recalled the various arguments for and against such an effect, and then Dr. Kœnig proceeded to experimentally prove the affirmative. Taking two forks tuned to 2048 and 2304 vibrations respectively (ratio 8 : 9) and sounding them simultaneously, the middle C of the piano (256) was distinctly heard. The same beat tone resulted from forks having frequencies in the ratio of 8 : 15, whose negative remainder was 256. Various other tones were sounded simultaneously in pairs, and in all cases the corresponding beat-tone was quite distinct. In these experiments the existence of nodes and loops in air was particularly noticeable, for as Dr. Kœnig turned the tuning-forks in his hand, the intensity of the beat-tones heard at a particular spot varied enormously. The experiments were carried a step further by impressing vibrations of different frequencies on one and the same body: the beat-tones in this case were quite perceptible. In carrying this out, Dr. Kœnig had constructed steel bars of approximately rectangular section, whose periods of vibrations were different in two directions at right angles. Striking one face of the bar a certain note resulted, whilst a blow on an adjacent face produced a different one. When the bar was struck on the edge joining the two faces, both the notes could be heard as well as the beat-tone resulting therefrom. The experimenter had gone still further, and made such bars so short that neither of the fundamental notes are within the limits of audition, but the resulting beat-tone can be heard quite distinctly. In all cases the frequency of the beats agrees with that calculated from Dr. Kœnig's formula, and secondary beats follow the same law. It was then pointed out that not only beats, but the maxima of a series of pulsations varying in intensity will, if isochronous and sufficiently rapid, give tones, just as a series of primary shocks do. This was illustrated by tuning-forks, and by directing a stream of air issuing from a slit against a notched rim of a rotating disk. A further confirmation was given by a modified disk siren; in this the holes, instead of being of the same size all round a circle, increase to a maximum and then decrease again, there being several sets of such holes in one circumference. When this was put in operation, notes corresponding in pitch to the number of holes and also to the number of sets of holes, could be heard. A wave siren was also used to illustrate the same fact. The matter was further illustrated by moving a tuning-fork towards a wall or other reflecting surface at various velocities. According to Doppler's principle, as the fork recedes from the observer and approaches the wall, the frequency of the direct waves is less and that of the reflected waves greater than that of the fork, and these two series of waves produce beats. By sufficiently increasing the velocity and using a fork of high pitch, the beats blend into tones. Coming to the second half of Dr. Kœnig's researches, Dr. Thompson said that Helmholtz contended that the *timbre* of musical sounds was not affected by differences of phase amongst the component tones; on this point, however, Kœnig had come to the opposite conclusion. To illustrate graphically why phase should affect *timbre*, a number of diagrams were exhibited, some showing the resultant wave-form produced by combining a tone with its harmonics of equal intensity, when the differences of phase between them were 0, $\frac{1}{4}$, $\frac{1}{2}$, and $\frac{3}{4}$ respectively; whilst others represented the wave-forms when the harmonics and the fundamental were of different intensities. The effect of phase on the shape of the wave-form was very marked. The subject was treated experimentally by means of a wave siren, against which a stream of air issuing from a slit could be directed. By inclining the slit to one side of the radius or the other, the phases of the component waves could be altered, and this had a marked effect on the character of the sound produced. Illustrations of Kœnig's multiple wave sirens, both of the cylinder and disk forms, were next shown, and the results of investigations made with the apparatus described. From these results it appears to be impossible to produce the *timbre* of instruments such as trumpets, clarionets, &c., by any combination of a tone and its pure harmonics. This led to the investigation of impure harmonics. By plotting and combining curves it was shown that the wave-form obtained from a tone and impure harmonics changes in successive periods; this peculiarity was observed to exist in a record taken from a vibrating string. Various disks with wavy edges of different form were spun before an air slit, and the varying character of the resulting sounds as the slit was turned, demonstrated. Before concluding, Dr. Thompson remarked that the word "*timbre*" requires to be re-defined, for the rigidity of strings, wires, &c., and the interference of the wood and metal parts of organ pipes and other wind intruments generally, prevent the formation of pure harmonics. A model consisting of vibrating strips placed vertically or inclined was exhibited to show the different kinds of *timbre*. The differences between mixtures and compounds of tones was pointed out, and the inability of the ear to distinguish between pure and impure sounds referred to. Lord Rayleigh thought more information was required on the important subjects brought forward, and asked in what class of musical sounds are the overtones strictly harmonious. He could admit that in piano wires they may not be so, but he was not quite so clear about organ pipes. He said he was filled with admiration by the perfection of the apparatus displayed, and expressed a wish that such mechanical acousticians could be found on this side of the Channel. Mr. Bosanquet said he had been carefully over the ground investigated by Dr. Kœnig. He believed Dr. Kœnig was the first to get at the facts concerning beats, but it was difficult to admit all that had been said about them. However, the chief difference between authorities seemed to be one of language. Owing to the lateness of the hour he could not discuss the question fully, and so asked to be allowed to reserve his opinion on the matter. As regards *timbre*, he thought the experiments on the effects of phase were not conclusive. The sounds of wind instruments such as trumpets, he said, depended greatly on who produced them. It was no easy matter to bring out their full sweetness, and it was comparatively few persons who could ever attain perfection. He ventured to think that in a properly used instrument none of the harmonics are out of tune. Mr. Blaikley agreed with Lord Rayleigh about piano wires, and as regards wind instruments he could hardly think that the overtones were so inharmonious as Dr. Thompson would have him believe. In fact, Mr. Stroh had obtained wave-forms for him from various instruments, but in none of them was there any discontinuity such as shown on one of the diagrams exhibited. However, he was of opinion that there is something in *timbre* not accounted for by the ordinary theory. The President said that in view of the production of audible sounds by the beats from notes beyond the range of audition, it might be possible to demonstrate that insects produce sounds inaudible to the human ear by putting several together in a box, and listening for the beat-tones. Dr. Kœnig acknowledged the most cordial vote of thanks accorded to himself and Dr. Thompson.

Zoological Society, June 3.—Prof. W. H. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of May 1890, and called special attention to a pair of Hartebeests (*Alcelaphus caama*), and a pair of Swainson's Long-tailed Jays (*Calocitta formosa*), acquired by purchase; and to a pair of Beatrix Antelopes (*Oryx beatrix*), presented by Colonel E. C. Ross, Consul-General for the Persian Gulf.—Mr. Sclater exhibited and made remarks on two young specimens of Darwin's Rhea (*Rhea darwinii*), obtained by Mr. A. A. Lane in the province of Tarapacà, Northern Chili, and forwarded to Mr. H. H. James.—Mr. Sclater exhibited and made remarks on a flat skin of a Zebra, received from Northern Somaliland, which appeared to be referable to Grévy's Zebra (*Equus grevyi*).—Mr. A. D. Michael read a paper on a collection of non-parasitic *Acarina* lately made in Algeria, where he had found the *Acarina* less abundant than in England, and, indeed, almost absent from the true southern vegetation. The species met with were not of larger size than the British. The collection consisted almost entirely of Oribatidæ, and contained examples of 46 species belonging to 15 genera. Amongst them were 8 species new to science, 27 were British, and the rest South European. Amongst the new species were a remarkable new *Cæculus*, there being previously only one known species of this curious genus, which forms a separate family. There was also a new *Notaspis*, which had not been found in Europe, but had been received from the shores of Lake Winnipeg, in Canada. There were likewise some very singular new species of the genus *Damæus*, and a triple-clawed form of *Nothrus anauniensis*.—Mr. Frank E. Beddard read a paper on the anatomy of the Fin-foot (*Podica senegalensis*). The paper dealt chiefly with the myology and osteology of this doubtful form. The conclusion arrived at was that it showed most resemblance to the Rails, but that in its muscular anatomy it agreed in many particulars with the Grebes and Divers.—Mr. O. Thomas read some notes on the specimens of Mammals obtained by Dr. Emin Pasha, during his recent journey through Eastern Africa, as exemplified in the specimens contained in two collections presented to the British Museum and the Zoological Society respectively.—Mr. G. A. Boulenger read a paper containing the descriptions of two new species of the Siluroid genus *Arges*, from South America.—A communication was read from Mr. James Yate Johnson, containing descriptions of five new species of fishes from Madeira.

Linnean Society, May 24.—Anniversary Meeting.—Mr. W. Carruthers, F.R.S., President, in the chair.—The Treasurer presented his Annual Report, duly audited; and, the Secretary having announced the elections and deaths of Fellows during the past year, the President proceeded to deliver his annual address. In this he dealt with the distribution of British plants both before and after the Glacial period, making special allusion to the discoveries of Mr. Clement Reid amongst the vegetation of the Cromer Forest Bed, and showed that the forms which have come down to us at the present day do not differ in any respect from the same species found in the Glacial beds.—A vote of thanks was moved by Sir Joseph Hooker and seconded by Mr. Stainton to the President for his excellent address, with a request that he should allow it to be printed, and carried unanimously.—On a ballot taking place for new Members of Council, the following were declared to be elected:—Dr. P. H. Carpenter, Dr. J. W. Meiklejohn, Mr. E. B. Poulton, Mr. D. Sharp, and Prof. C. Stewart. On a ballot taking place for President and Officers, the following were declared to be elected:—President: Prof. Charles Stewart. Secretaries: B. D. Jackson and W. P. Sladen. Treasurer: Frank Crisp.—The Linnean Society's gold medal for the year 1890 was then formally awarded and presented to Prof. Huxley for his researches in zoology.

Entomological Society, June 4.—The Right Hon. Lord Walsingham, F.R.S., President, in the chair.—The Secretary exhibited, on behalf of Mr. J. Edwards, Norwich, two specimens of *Ilybius subæneus*, Er., and a single specimen of *Bidessus unistriatus*, Schr. Mr. Champion alluded to the fact that the only recorded British specimens of the first-mentioned beetle had been taken many years ago at Peckham. Lord Walsingham, in alluding to the exhibit, referred to the list of Norfolk

Coleoptera compiled some years ago by Mr. Crotch, which appears to have been lost sight of.—Mr. McLachlan alluded to the damage done by insects to orange-trees in Malta, and stated that the Rev. G. Henslow had lately been studying the question; one of the chief depredators was the widely-spread "fly," *Ceratitis citriperda*, well known as devastating the orange. He found, however, that another and more serious enemy was the larva of a large Longicorn beetle (*Cerambyx miles*, Bon.), which bores into the lower part of the stem and down into the roots, making large galleries; in all probability the larva, or that of an allied species, is the true *Cossus* of the ancients. Lord Walsingham stated that a species of *Prays* allied to *P. olcellus* and our common *P. curtisellus* was known to feed in the buds of the orange and lemon in Southern Europe.—The following papers were communicated, and were read by the Secretary:—Notes on the species of the families *Lycidæ* and *Lampyridæ* contained in the Imperial Museum of Calcutta, with descriptions of new species, and a list of the species at present described from India, by the Rev. H. S. Gorham.—A catalogue of the Rhopalocerous Lepidoptera collected in the Shan States, with notes on the country and climate, by Dr. N. Manders, Surgeon, Medical Staff. The latter paper contained a very interesting description of the chief physical features of the Shan States and neighbouring parts of Burmah.

Mathematical Society, June 12.—J. J. Walker, F.R.S., President, in the chair.—The President announced that the Council had unanimously awarded the De Morgan Memorial Medal to Lord Rayleigh, Sec.R.S., for his writings on mathematical physics.—The following papers were read:—On simplicissima in space of n dimensions (third paper), by W. J. C. Sharp.—Rotatory polarization, by Dr. J. Larmor.—Parabolic note, by R. Tucker.—Prof. Greenhill, F.R.S., communicated a paper by Prof. Mathews on the expression of the square root of a quartic as a continued fraction, and one by R. Russell on modular equations.—The President gave a brief sketch of a paper by A. R. Johnson, on certain concomitants of a system of conics and quadrics, and on the calculation of the covariant S of the ternary quartic.

PARIS.

Academy of Sciences, June 9.—M. Hermite in the chair.—On the movement of a prism, resting on two supports, submitted to the action of a variable normal force following a particular law, applied at a determined point of the axis, by M. H. Resal.—Theory of the state produced near to the wide opening of a fine tube where the threads of a liquid which flows there have not acquired the normal inequalities of velocity, by M. J. Boussinesq.—Action of the alkalies and alkaline earths, alkaline silicates, and some saline solutions on mica: production of nepheline, sodalite, amphigene, orthoclase, and anorthite, by MM. Charles and Georges Friedel.—On the fauna of deep parts of the Mediterranean around Monaco, by the Prince of Monaco. Some dredging operations carried on at various depths up to 1650 metres show that, at certain parts at least of these regions, the Mediterranean Sea is by no means devoid of inhabitants as has been previously asserted.—Observations of Brooks's comet (a 1890), made with the *coudé* equatorial of Algiers Observatory, by MM. Rambaud and Renaux. The observations of position extend from May 10 to 31.—Photographic observation of Brooks's comet made at Algiers Observatory, by M. Ch. Trépied (see "Our Astronomical Column").—On a particular case of the movement of a point in a resisting medium, by M. A. de Saint-Germain.—Propagation of light in gold-leaf, by MM. Hurion and Mermeret.—On the amplitude of the diurnal variation of the temperature, by M. Alfred Angot. The author shows how the diurnal temperature variation in any station on the earth may be expressed by the formula—

$$a = \frac{K}{r^2}(A + B \sin l + C \cos 2l),$$

in which K is a function of cloudiness, and = 1 when the sky is clear, A, B, and C are coefficients depending only upon the geographical position of the station and its climatological characters, l the sun's longitude, and r the distance of the earth from the sun.—Electrolysis of fused aluminium fluoride, by M. Adolphe Minet. The author finds a mixture of 40 parts of the double fluoride of aluminium and sodium with 60 parts of sodium chloride to give him the best results yet obtained.—On the isomeric states of chromium sesquibromide: the blue sesquibromide, by M. A. Recoura. A method of preparing the solid hydrated bromide, $Cr_2Br_6, 12H_2O$, corresponding to the violet solutions is given. It is shown that the grey-blue solid obtained is less stable than the green crystals formerly described, whereas the violet solutions corresponding to the blue solid salt are more stable than the green solutions; thermochemical data are given in confirmation.—On the estimation of zinc in the presence of iron and manganese, and its separation from those metals, by M. J. Riban. The zinc is separated as sulphide from a solution to which has been added an excess of sodium thiosulphate.—On the composition of clays and kaolins, by M. Georges Vogt.—On the synthesis of the fluorides of carbon, by M. C. Chabrié.—On the products of saccharification of amylaceous matters by acids, by M. G. Flourens.—On the decomposition of organic manures in the soil, by M. A. Muntz.—On the anatomy of horny sponges of the genus *Hircinia*, and on a new genus, by M. H. Fol.—On the circulatory system in the carapace of decapodous Crustacea, by M. E. L. Bouvier.—On two new species of Coccidia, parasitic on the stickleback and sardine, by M. P. Thélohan.—Interesting nuclear modifications of the nucleolus which may ultimately throw some light on its signification, by M. E. Bataillon.—On a hymenopterous insect injurious to the vine, by M. E. Olivier.—On the diversities and similarities in some dentary systems of mammifers, by M. Heudes.—Researches on the development of the seminal integuments of Angiosperms, by M. Marcel Brandza.—On the nature of the phosphate beds of Dekma (département de Constantine), by M. Bleicher.—On the existence of marine deposits of the Pliocene age in the Vendée, by M. G. Vasseur.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Japan and the Pacific: M. Inagaki (Unwin).—The Mineral Resources of Ontario (Toronto).—A Treatise on Practical Chemistry and Qualitative Analysis, 5th edition: Dr. F. Clowes (Churchill).—Primo Resoconto dei Risultati della Inchiesta Ornitologia in Italia; Parte Seconda, Avifauna Locali: E. H. Giglioli (Firenze).—The Species of Ficus of the Indo-Malayan and Chinese Countries, Appendix: Dr. G. King (Calcutta).—Sammlung von Vorträgen und Abhandlungen, Dritte Folge: W. Foerster (Berlin, Dümmler).—Lehrbuch der Verg. Entwicklungsgeschichte der Wirbellosen Thiere, Specieller Theil, Erstes Heft: Dr. E. Korschelt and Dr. K. Heider (Jena, Fischer).—The Life and Letters of the Rev. Wm. Sedgwick, 2 vols.: J. W. Clark and T. McK. Hughes (Cambridge University Press).—The Forest Flora of South Australia, Part 9: J. E. Brown (Adelaide).—Les Bactéries, 2 vols.: A. V. Cornil and V. Babes (Paris, Alcan).—Physiological Botany: Dr. G. L. Goodale (Macmillan).—An Elementary Treatise upon the Method of Least Squares: G. C. Comstock (Arnold).—The Lepidopterous Fauna of Lancashire and Cheshire: J. W. Ellis Leeds (McCorquodale).—La Révolution Chimique Lavoisier: M. Berthelot (Paris, Alcan).—Beiträge zur Geologie Syriens, Die Entwickelung des Kreidesystems in Mittel- und Nord-Syrien; eine Geognotisch-Paläontologische Monographie: Dr. Max Blanckenhorn (Berlin, Friedländer).—Zur Kenntniss der Fauna der "Grauen Kalke" der Sud-Alpen: Dr. L. Tausch v. Gloeckelsthurn (Wien, Hölder).—The Law and Practice of Letters Patent for Inventions: L. Edmunds and A. W. Renton (Stevens).

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