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PRESTWICH'S "GEOLOGY"

Geology: Chemical, Physical, and Stratigraphical. By Joseph Prestwich, M.A., F.R.S., F.G.S., Correspondent of the Institute of France, Professor of Geology in the University of Oxford. Vol. I. *Chemical and Physical.* (Oxford: Clarendon Press, 1886.)

THE last few years have been signalised in the annals of geological science by the publication of a number of excellent treatises and students' text-books. Not to mention the appearance of new editions, admirably brought up to date, of such deservedly popular works as Dana's "Manual of Geology," and Credner's "Elemente der Geologie," and of the "Geology for Students and General Readers" of Prof. Green, which is unfortunately still incomplete, we have the revisions of Lyell's "Students' Elements of Geology" by Prof. P. M. Duncan, and of Phillips' "Manual of Geology" by Prof. Seeley and Mr. Etheridge, in which last the additions and alterations are so numerous as to make it practically a new work; among text-books altogether new must be especially noticed the valuable and almost encyclopædic treatises of Dr. Archibald Geikie and Prof. de Lapparent—the necessity for works of this class being shown by the fact that in the course of less than four years they have both reached a second edition. As compared with all these works, however, the new treatise by Prof. Prestwich will be found to cover a somewhat different ground, and indeed to occupy a perfectly unique position among them. It deserves, therefore, to be regarded from another point of view, and to be judged by a somewhat different standard from any of them. Its author is the acknowledged Nestor of British geology, and it may be safely affirmed that no living geologist has contributed in a greater degree to the advance of the science by his important original researches. More than fifty years have elapsed since his first original contribution to geological science was published, and from that date forward his activity has been ceaseless; it is worthy of remark, that while his earliest papers were occupied with the description of some of the oldest formations—the Old Red Sandstone of Banffshire, and the Carboniferous of Coalbrookdale—his later researches have been devoted to the Tertiary and post-Tertiary deposits. But among the writings of Prof. Prestwich will be found some of the most philosophical contributions to geological literature—among which may be especially cited his memoirs on the circulation of underground waters, on the thickness and extension of deep-seated rock-masses, and on the age and relations of the deposits which have yielded the oldest known relics of the human race. A general survey of the wide fields of geological science, by one so especially qualified to undertake the task, may be expected to be of the greatest interest and value; and the expectation is not disappointed by the work now before us.

Upon the death of Prof. Phillips, the University of Oxford, in seeking for a worthy successor to that erudite and versatile geologist, wisely determined to invite Mr. Prestwich to accept the vacant Chair. His able inaugural dissertation on "The Past and Future Work of Geology," which was published in 1875 (see NATURE, vol. xi. pp. 290 and 315), sufficiently informed the world of the position with regard to the different schools of geological philosophy which the new Oxford Professor was prepared to take up. In the present work we find his matured conceptions of the subject, tested in the best possible manner as to their mode of presentation, by having been made the basis of a number of successive courses of University teaching. Every geologist will turn with interest to these pages, and even where they feel compelled to differ from the conclusions arrived at by their author, will render a just homage to the great learning and the judicious thought which are everywhere conspicuous in them.

In the plan of the work, we think Prof. Prestwich has shown the soundest judgment. After a brief introduction treating of the objects and methods of the science, the author proceeds to "give the reader a general sketch of the nature and distribution of the materials with which the geologist has most commonly to deal;" by the avoidance at the outset of difficult and doubtful questions of cosmology, and by reserving those "theoretical questions connected with cosmical and physical phenomena, which relate to the evolution of the globe and historical geology" to the end of the work, he best consults the needs of students—leading them up gradually from a survey of what is simple and known to a consideration of what is complex and unknown.

The two chapters dealing with chemistry, mineralogy, and petrography, large portions of which are printed in small type as being unnecessary for the general reader, would have proved more satisfactory if the author had been more fortunate in the selection of his authorities. Recent researches in those departments of science have been so numerous and of such importance, that few geologists would regard the "Gesternlehre" of Cotta, or the rehabilitation of Cordier's work by Charles D'Orbigny, as affording a sound basis for a summary of the present state of knowledge on these subjects.

But when the author comes to a discussion of the results of the decomposition of the igneous and metamorphic rocks, he is dealing with questions to which he has evidently devoted a large amount of personal study, and his own observations are of the highest value, while his citations from the works of other authors, are in nearly all cases of a very judicious character. The line of argument adopted in the work is necessarily interrupted for a time by the necessity for giving some account of the "place and range of past life," and here, as in the chapters dealing with minerals and rocks, the author has perhaps not consistently followed the best guides. But in the chapters on the formation of sedimentary strata, on the relations of littoral and deep-sea deposits, and on meteorological agencies and the circulation of underground waters, we recognise the hand of a master; and these portions of the work will be found to be worthy of the most careful study. In the very excellent chapter on ice and ice-action, we find many quotations from recent authorities on the subject; and some very interesting illustrations and descriptions are taken from Kane's "Voyages."

In the chapter on volcanoes, the author explains the theory which he first broached at the York meeting of the British Association, and which he has since elaborated in a paper read before the Royal Society. Upon this subject, as well as upon numerous others discussed in the remaining chapters of the volume, geologists will be glad to have brought together the results of the author's extensive study and exceptionally wide experience. While every one will be glad to see the excellent use which has been made of the splendid researches of Delesse and Daubrée, it might perhaps have been better if the author had relied less implicitly upon some others among the older school of French geologists. All will look forward with interest to the appearance of the second and concluding volume of the work, which will treat of stratigraphical geology, and the broader and more theoretical aspects of the science.

The present volume is worthy of the University Press, from which it is issued: well printed upon excellent paper, and illustrated by numerous woodcuts; these have been derived from other standard works, or are founded on sketches by the late Dr. Buckland, while not a few of them bear testimony to the fact that recent publications, like those of the U.S. Geological Survey of the Territories, have been freely placed under contribution; the general appearance of the book is all that could be desired. Among the six excellent folding plates, the first place must be assigned to the beautiful reproduction, on a small scale, of Marcou's geological map of the world, which has been revised by its author for the present work.

We cannot better conclude this notice of a very important contribution to geological literature than by quoting the sentences in which the author himself defines his position towards the different schools of geological thought.

"The fundamental question of *time* and *force* has given rise to two schools, one of which adopts uniformity of action in all time,—while the other considers that the physical forces were more active and energetic in geological periods than at present.

"On the Continent and in America the latter view prevails, but in this country the theory of uniformity has been more generally held and taught. To this theory I have always seen very grave objections; so I felt I should be supplying a want by placing before the student the views of a school which, until of late, has hardly had its exponent in English text-books.

"The eloquence and ability with which uniformitarianism has been advocated, furthered by the palpable objections to the extreme views held by some eminent geologists of the other school, led in England to its very wide acceptance. But it must be borne in mind that uniformitarian doctrines have probably been carried further by his followers than by their distinguished advocate, Sir Charles Lyell, and also that the doctrine of non-uniformity must not be confounded with a blind reliance in catastrophes; nor does it, as might be supposed from the tone of some of its opponents, involve any questions respecting uniformity of law, but only those respecting uniformity of action.

"I myself have long been led to conclude that the phenomena of geology, so far from showing uniformity of action in all time, present an unceasing series of changes dependent upon the circumstances of the time; and that, while the laws of chemistry and physics are unchangeable and as permanent as the material universe itself, the exhibition of the consequences of those laws in their operation on the earth has been, as new conditions and new combinations successively arose in the course of its long geological history, one of constant variation in degree and intensity of action."

Extreme Catastrophists—if indeed any such have escaped extinction during the evolution of modern geological philosophy—will find little in the way of comfort in the above sentences, or indeed in any part of the volume before us. The most pronounced Uniformitarian, on the other hand, will find equally little to take exception to in the general tone of Prof. Prestwich's conclusions; he will perhaps only ask that before recourse is had to non-uniformity in the action of existing causes, the incompetency of the uniform action of those causes to produce any particular phenomena shall be distinctly demonstrated.

THE PICTORIAL ARTS OF JAPAN

The Pictorial Arts of Japan. With 80 Plates and Chromolithographs, and numerous Engravings on Wood and Copper, and with General and Descriptive Text. By W. Anderson, F.R.C.S., late Medical Officer H.M. Legation in Japan. To be complete in 4 parts. Part I. General History. (London: Sampson Low, 1886.)

OF the aboriginal inhabitants of Japan we know but little. A Polynesian element with some Melanesian admixture probably predominated in the southern, as an Aino element did in the northern, islands. Of the latter race the shell-mounds that line the coasts of the main island have afforded many interesting relics, among others fragments of pottery showing a simple ornamentation recalling the zigzags and curves characteristic of the Zuñi and Tesuke pottery of Arizona and New Mexico. It was in the main island, often called Hondo, that the history of Japan began. Not more than two or three centuries probably before the compilation in the eighth century of the Kojiki, the oldest extant document in Japanese literature, a colony of Ural-Altaic origin occupied the broad plain that extends from the northern shores of the Japanese Mediterranean to the foot of the Kiyoto hills. Tradition points to previous settlements of the strangers on the southern islands, whither, pushed by some Central Asian stress from their former home in the Korean peninsula, they had wandered across the narrow waters that separate the Land of Freshness from the Land of Dawn. The new-comers did not easily subdue the Aino tribes, remnants of whom in the north and east still existed, when Yoritomo was created Barbarian-beating Generalissimo in the thirteenth century. More or less amalgamation took place between conquerors and conquered, but the former did not wholly lose their purity of blood, and, to this day, broad physical differences distinguish the peasantry from the more aristocratic strata of the population—differences amply and graphically rendered in the innumerable drawings of Hokusai. These founders of the Japanese State, which, despite the assertions of native writers, can boast of no high antiquity, were a simple folk, living principally on fish and the produce of the chase, clothed in hemp and cloth made of broussonetia bark, and dwelling in wattled huts roofed with bark and reeds. It was not until they were touched by Chinese civilisation that they entered upon the evolutionary course which has ended in a somewhat naïve preference of the civilisation of the West.

There is a difference between symbolism, which all races of men have practised, and art. The reindeer and mammoth drawings of the Cave-men show that a faculty of correct and even spirited drawing was developed at a very early stage in man's history. But the art, like the civilisation, of Europe cannot be traced to the Cave-men; both are a heritage from the Greeks, distributed westwards by Rome. Nor was Greece itself other than an apt soil for the development of seeds brought from Egypt. Who that has gazed upon the wall-paintings of the tomb of Ti at Sakkarah, or has seen the wonderful wooden statue at Boulak, known as the Shêkh-el-Beled, or the sculptures on the sandstones of Wady Mughara, or the figures cut on the obelisk of Heliopolis, can doubt that the men who did these works, full of truth, grace, and vigour, were the worthy foregoers of the sculptors of Greece and the painters of Italy? As the message of Greek art was borne by Rome to the West, so was it carried by the Macedonian conqueror to the northern frontiers of India, where the Buddhists of the Punjab countries pressed it eagerly into the service of their religion. In the sculptures of the monasteries of Yusufzai (Swat frontier of the Punjab) brought to light by Major Cole, R.E., and admirably photographed in the magnificent publication of the Indian Government issued under the title "Preservation of the National Monuments of India," this Græco-Buddhic art is amply exemplified, and most interesting it is to trace in these remains both the reposeful strength which the mobile Greek admired, and the vigorous action which pleased the contemplative Asiatic. Among the most striking of the sculptures are a figure of Maya being borne to the Trayastrinsha heaven, recalling, and probably suggested by, the work of Leochares (B.C. 365) known as "Ganymede carried off by Jupiter's Eagle," the figure of Prince Siddhartha before he left home to become a mendicant, and the wonderful group representing the death of the Buddha, with the face of Devadatta full of evil glee behind the couch. The best Buddhistic works of China or Japan, in comparison with these remains of early Græco-Buddhic art, are merely feeble grotesques, in which the majesty and grace of the prototypes have degenerated into strained pose and gesture and the lifeless prettiness of craftsmanship. The great gateways of the Tope at Sanchi, it may be noted *en passant*, bear a curious resemblance to the *torii* of Shinto shrines in Japan, the principal difference being the presence of a third cross-piece in the former, and the elaborate sculpture of their elements.

In the first century of our era the Buddhist apostles reached China, in the fourth they were in Korea, and in the sixth in Japan. Their art they bore with them, and used as a means of propagation of their doctrines. In the plates numbered 1, 2, 4, 8, 9, 10, and 70—the last a particularly interesting representation by the Chinese artist of the eighth century, Wu-tao-tsu, of the eight Nirvana of Buddha—the characteristics of this missionary art are well displayed, and may be instructively compared with the Yusufzai sculptures.

But even the art of Egypt had its birthplace elsewhere than in the Nile Valley. Recent investigations of the earliest monuments tend to prove, as Mr. Bertin has lately shown, that the Egyptians were not of a Semitic, but of an Equatorial African, stock. Mr. Bertin advances also good grounds for supposing that the Bushmen of South Africa, whose rock-paintings every traveller who has seen them has extolled for their faithfulness and vigour, came of the same or an allied stock. The Bushmen are by no means a degraded, though a stunted, race. They have no kinship with the Negro or even with the Bantu races. Their skulls are well-formed and free from prognathism, and their meagre physique may perhaps be due to the hardship they endured in their secular wanderings over the vast deserts that intervene between the equator and the tracts to the north of the Cape Colony. A psychological connection is thus established between South Africa and the Far East which is worthy of being more fully investigated. There are many facts which tend to add force to this theory, startling though it may appear, which I have no space to dwell upon. I may, however, cite the analogy which seems to exist between the Bushman clicks and the Chinese tones, the former having a similar relation to consonantal to that which the latter possess to vocalic sounds.

Up to the middle of the last century the Chinese school of painting, more or less directly developed from Buddhistic art, held sway in Japan. Its history and the modifications it underwent in the latter country are admirably set forth by Mr. Anderson, and to his account I must refer the reader. About the period referred to a sort of revolt took place in Japan against Sinicism generally. The great Shinto revivalists, Mabuchi, Motöori, and Hirata, scouted Buddhism and Confucianism with equal emphasis. There is a quaintness in their logic which is not unamusing. Motöori, for instance, defends the Shinto lack of a moral code by the answer that the very possession of a moral code was a badge of inferiority, proving as it did the need of it, a need which Japan did not feel, as the people had merely to obey the Mikado, the direct descendant of the Sun Goddess, to be assured of their righteousness. While this religious renaissance was preparing the way for the return of the Mikado to power, a sort of Giottesque revolution took place in art: classicism was, though only partially, abandoned, and a Realistic school (*ukiyoye*) came into existence, of which the master spirit was Hokusai, who died at the age of ninety in 1849. In the *Hiyaku shō den* (the Hundred Heroes) are sketches made by him at the age of eighty-eight. The *ukiyoye* school was that of Japanese art *par excellence*, to a very considerable degree freed from Chinese trammels, and full of the lively and mobile spirit of the people. Mr. Anderson gives a good account of it, but hardly so full as it merits. Nor is it adequately represented in the present instalment, though doubtless it will be so in the complete work. The woodcut, after a drawing by Hokusai, called "The Maniac" (Pl. 37) is a fine example of his fluent drawing and skill and breadth in composition. The last is to me by far the most interesting phase of Japanese art. So it was to Motöori, whose very sensible observations on the subject have recently been translated by that excellent scholar, Mr. Basil Hall Chamberlain (*Tr. As. Soc. Jap.*, vol. xii. p. 223). To Sinico-Japanese art, Motöori, though an enthusiastic patriot, preferred Chinese art, especially the finished pictures of the Chinese, and their rapid drawings of birds, flowers, fish, insects, and the like, and again I share his opinion.

The *ukiyoye* style may indeed justly be regarded as the highest expression of the art of the furthest East. Its limitations are sufficiently obvious and not without interest. The quite childish drawing of quadrupeds is singular. The Japanese artists could draw them well enough when they chose. In one of the volumes of that interminable romance, the "Satomi Hakkenden"—there are more than fifty volumes—there is a splendid sketch of a bull, the wild boar is not seldom vigorously delineated, and Mr. Anderson gives a spirited and fairly correct drawing of a deer (Pl. 31). The explanation probably is that quadrupeds did not interest the Eastern artist, the chase, save of the wild boar, was little in vogue, and rather discouraged by Buddhism. The human face was regarded generally as a mere accessory, and conventionally rendered. There were exceptions: Kikuchi Yosai drew faces vigorously, though even he seemed to limn a profile with difficulty, and the portrait-sculptors of the seventeenth century displayed considerable power. No attempt whatever seems to have been made to portray human beauty of face or form, and the renderings of female beauty are insipid in the extreme, as well seen in the sketch of an ancient hetaira, somewhat truculently called "Hell" Reigan (Pl. 41). The stronger emotions, however, are delineated with a power that would have delighted Darwin, exemplifying admirably his descriptions of the modes of facial expression of the passions of anger, fear, despair, and horror. In some of the novel-illustrations I have seen disdain, reflection, and slyness admirably portrayed. But the softer sentiments are either not depicted at all, or depicted after a purely conventional fashion. The Turanian countenance is not expressive, save of the stronger emotions. A curious mode of portraying anger is to paint streaks of red round the eyes and along the principal lines of expression, nor is the device altogether ineffectual. The Japanese flush with anger, but not with shame; indeed, the feeling of shame they seem to possess but in a minor degree. In some other particulars their modes of expression are peculiar. They nod assent (*unadzuki*), but do not shake their head in dissent. They talk without gestures and with little emphasis of accent, but with curious changes of note and intonation. They never kiss; mothers even do not kiss their children, and they have singularly few terms of endearment. By way of compensation they have few terms of abuse, and no oaths. Their individuality is small, reflected in the curious impersonality of their language (see some excellent remarks on this characteristic by Mr. Aston, *Tr. R. As. Soc.*, vol. xii.); every Japanese is through life a member of a family, or, if a head of a family, a member of some guild or fraternity; he never "paddles his own canoe." Thus may be explained, in great measure, their neglect of humanity in art.

On a future occasion I trust to be allowed to offer a few more remarks on some points in connection with the art of Japan that seem to me interesting in relation to it as a phase in human history. Meanwhile I must not omit a recognition, not the less hearty because necessarily brief, of the value of Mr. Anderson's labours, the extent of which my own studies enable me to appreciate. This is not the place to enlarge upon the artistic merits of his work; they have been, or will be, sufficiently appreciated elsewhere. But the stores of folklore he has gathered together form a contribution to our scientific knowledge of man of extreme importance, and his account of the development of Japanese art is as interesting as it is instructive. The present instalment is admirably got up, and the illustrations, particularly the chromolithographs by Greve of Berlin, are of unsurpassed excellence. Altogether the work promises to be of equal interest and value to the student of man and society, to the lover of art, and to the collector of Oriental curiosities.

F. V. DICKINS

THE EVOLUTION OF THE PHANEROGAMS

L'Évolution des Phanérogames. Par MM. Saporta et Marion. Second Notice. (Paris: Alcan, Boulevard St. Germain, 1885.)

IN our previous notice (vol. xxxii. p. 289) of this important work we traced the evolution of gymnosperms down to a certain point. Prof. Williamson soon afterwards communicated the chief point of difference between his views and those of our authors, in a very interesting letter (NATURE, vol. xxxii. p. 364). We were not able at the time to follow the subject farther, and this was of less consequence, as the points at issue, though extremely important in themselves, are not claimed to be in the direct line of evolution of the existing phanerogams. The palæozoic heterosporous cryptogams, with exogenous stems, are chiefly interesting, from the evolutionary standpoint, for the light they throw on what must probably have been the structure of the common ancestors, from which they, as well as the gymnosperms, were derived. A *résumé* of what is known regarding the ancestry of the Eocene Coniferæ will shortly be published by the Palæontographical Society, the compilation of much of which has been directly assisted by Prof. Williamson himself, and has also been revised in part by Mr. Carruthers. As it is not claimed by Saporta and Marion in any way that angiosperms have been evolutionised from gymnosperms, even through the Gnetaceæ, it is unnecessary to pursue that branch of the subject farther now. The interest of the work centres, in fact, in the attempt to trace the ancestry of the monocotyledons and dicotyledons, groups which to ordinary observers seem to appear with startling abruptness in the geological record.

The differentiation of angiosperms, no less than gymnosperms, originally took place, it would appear, in pre-Carboniferous times, the ancestral forms common to both being heterosporous cryptogams, destitute of exogenous wood. The fundamental difference at starting seems to be that, in the gymnosperms, one of the macrospores contained in the ovule immediately absorbs all the rest, enabling their evolution to proceed with rapidity; while in the angiosperms there is a period of struggle among the macrospores before one finally obtains the advantage and obliterates its fellows. Want of space renders it impossible to give any account of the steps by which the authors have traced out this process. The common source, at a remote period, of the monocotyledons and the dicotyledons, is assumed from the fact that the early stages of the development of the embryo, in some of the former, approach nearer to dicotyledons than to plants of their own class. Moreover, the essential organs, the carpels, stamens, petals, and the fruits, are sufficiently analogous to indicate a common origin. The problem attempted is to reconstruct the "pro-angiospermic" stage whence these two opposite lines have issued. The fully-developed leaves of monocotyledons and dicotyledons embrace many varieties, from the most simple to others that are immensely complicated. Those of the former are generally of the more simple kind, but in the aroïds and Smilax they equal in complexity, and resemble, dicotyledons. It is not among these, however, but, as in zoology, among the embryonic stages, that the ancestral forms are likely to be traced. In many young plants the first leaves are very different in form and structure to those born when maturity is reached. Examples are given of sheathing, amplexicaul phyllodes in *Cicer aristinium*, *Quercus pubescens*, *Asparagus officinalis*, and some *Rosaceæ*, most of which are scale-like and parallel-veined. In *Aralia nymphecæfolia* the stipules embracing the young shoot are of considerable size and adherent for some time. They have a fine parallel venation which scarcely anastomoses and resembles not only the fully-developed leaves of some monocotyledons, but the petals and sepals of many flowers. In *A. Sieboldi* the bracts enveloping the buds and young shoots are similarly constructed, the petiole and true leaf barely emerging from their summit. The sheath, representing the primitive leaf, is in some Umbelliferæ, as the fennel, more important than the secondary leaves, and in one dicotyledon at least, *Eryngium bromeliæfolium*, the latter are not developed at all, the leaves resembling those of a yucca. On the other hand, it is not every monocotyledon that has preserved its primitive leaves only. In *Canna indica*, for example, the inferior and sheathing portion represents the primitive leaf, the middle part, or petiole, the original mucronate apex, and the blade the secondary leaf. Nearer the flower-spike only the primitive leaf-development remains in the form of bracts. The same characters are observed in *Strelitzia regina*. In Aroïdeæ the first leaves are simple and sheathing and the second as complex as those of dicotyledons. *Smilax* furnishes an example of a monocotyledon which has elaborated precisely the same kind of secondary leaf as a dicotyledon. In the grasses the primitive leaf appears on the underground rhizome as a sheath, later reduced to the ligule, while the ribbon-leaf is the homologue of the ordinary dicotyledonous leaf. In palms the primitive leaves are traceable in germination and later in the spathes and bracts, and in an altered condition in the leaf petiole, only the fan part being the secondary leaf.

The ancestral "pro-angiosperms" are supposed to have borne leaves such as are found diminished or masked in so many of their existing descendants—that is, entire, more or less elongated, ribbon-like leaves, amplexicaul at the base, attenuated and mucronate at the apex, and traversed by numerous longitudinal veins, connected by transverse veinlets, or even areolated.

Monocotyledons have, as a class, preserved their primitive foliary appendages more perfectly than dicotyledons, in which they are frequently so reduced as to be barely traceable as lateral expansions of the petiole, or in minute stipules.

The flower is an organ common to both, and must, therefore, have been produced before the two classes had become differentiated. The relative simplicity of structure seen in their several parts is thus explicable—sepals, petals, and bracts being frequently almost reproductions, as to form and venation, of the vagina, or the first sheath leaves, which in many plants succeed the cotyledons, and the terminal mucro can also sometimes be detected. Examples of primitive flowers are seen in *Magnoliaceæ*, *Ranunculaceæ*, and *Nymphaceæ*, but others have doubtlessly been profoundly modified to meet the needs of fertilisation. That the sexual leaves bearing the micro- and macro-sporangia—stamens and pistils—are similarly modified leaves, is also apparent in the case of *Magnolia*. Originally the "pro-angiospermic" flower must have consisted of an axis bearing the sexual appendages spirally disposed one above another, the microsporangial leaves at the base, and the ovule-bearing ones above. Though the flower has became consolidated through the shortening of the axis, its primitive spiral arrangement is traceable in a multitude of angiosperms.

Even the stems in the two classes are not really fundamentally different, the permanent presence or the absence of a productive region of cambium alone sufficing to have originated the two divergent types. In the remote past, before even the seasons were well defined, the cambium layer may have existed in an irregular or fugitive manner in the "pro-angiospermic," as it did in the "pro-gymnospermic" stem, and thence increasing differentiation have produced the two parallel series forming respectively at last dicotyledonous and monocotyledonous stems. Branching probably took place in such primordial stems by means of solitary terminal buds, accompanied perhaps by a restricted number of lateral ones, after the fashion of the screw-pines, aroids, and aloes.

Such was the nature of plants in their "pro-angiospermic" stage. Even the initial difference in the number of cotyledons characterising each class is explicable by supposing them to have been originally of unequal size, and that progressive differentiation led, in the one direction to equalisation, and in the other to suppression. The inequality is preserved in *Nymphaceæ*, which thus serve to diminish the difference in this respect between the two classes. J. STARKIE GARDNER

OUR BOOK SHELF

A Tangled Tale. By Lewis Carroll. With Six Illustrations by Arthur B. Frost. Pp. 152. (London: Macmillan and Co., 1885.)

THE first half of this delightful book consists of ten chapters, or "knots," as they are labelled by the author. Each of these contains a quaint and humorous description of some romantic episode, imagined in order to furnish occasion for proposing certain ingenious mathematical problems to the younger actors in the drama.

The author states that his intention was to embody these questions in each knot "like the medicine so dexterously, but ineffectually, concealed in the jam of our early childhood." This, however, may be noted: in the several doses presented in the volume before us the patient may assimilate all the jam, and, at will, reject the medicine.

The fun and humour with which these sketches sparkle may be enjoyed—and the many sly hits to be found therein may be appreciated—by those who are unwilling or unable to grasp the mathematical question involved.

And for another class of readers there is furnished, in an appendix which fills the latter half of the book, plenty of strong medicine ready to be taken undiluted, if so they choose.

"A Tangled Tale" having originally appeared as a serial in the *Monthly Packet*, many of the fair readers of that magazine, and also some of their brothers, sent up answers month by month to the questions proposed, and these are dealt with in the appendix referred to. A special interest, befitting the occasion, seems to have been taken by several subscribers in the problems so gorgeously dressed up and so prettily put before them. Not only were solutions forthcoming which were correct and neat, but now and then a talented contributor emulates the author himself in regard to the manner and style of wording the answer.

Whilst ample meed of praise is awarded to all good work—and the author's fund of humour never seems to fail him, causing even the initials of his anonymous correspondents to lend themselves to give point to his remarks—where there is any falling away from his high standard of excellence, the criticisms are serious enough, and, if need be, severe.

Fallacies and shortcomings in the answers are laid bare as mercilessly as in the treatise on "Euclid and his Modern Rivals." But great care is taken to bring home to the mind of the hapless contributor a full sense of the error into which he has fallen; and there are several very apt analogies and illustrations to be found in this appendix.

When the sketches first appeared, editors of the puzzle columns of other magazines must have been filled with "mingled feelings of admiration and despair" as they viewed these emanations from Lewis Carroll. And we do not doubt that very many mathematical tutors, as well as their pupils, will read with pleasure and profit "A Tangled Tale," and the appendix thereto, and feel deeply grateful to the author for publishing in book form this further example of his genius.

Mr. Frost has furnished a very striking frontispiece; and of his other illustrations the last may be specially noted here, wherein is depicted a martyr to experimental science who remains steadfast and unmoved at his observations whilst the waves close round and drown him: this is apropos of an exceedingly clever and entirely new "hydrostatic paradox," which the author has invented by dressing up, with the aid of Lardner's "Physics," a very old fallacy.

A. R. WILLIS

Tableaux-Résumés des Observations Météorologiques faites à Bruxelles pendant une période de cinquante années (1833-1882). Préparés par A. Lancaster. I. "Température de l'Air." (Bruxelles, 1886.)

IN this pamphlet of seventy-nine pages Mr. Lancaster admirably resumes the observations of the temperature of the air made at Brussels during the fifty years from 1833, when they were begun by Quetelet, to the end of 1882. An important consideration kept in view throughout is the climatological aspects of the observations, particularly the frequency or rareness of certain temperatures in different times of the year which bear more or less immediately on vegetable and animal life.

From 1833 to 1877 the observations were made with thermometers exposed 10 feet above the ground at a north window of the Observatory; but from the beginning of 1878 the thermometers were placed in a Stevenson screen in the garden at a point 16 feet distant from their original position, and at a height of 4½ feet from the ground. Care was taken at the time of the change to make a double series of observations in the two positions, from which it is shown that the earlier series were 0°·8 too high as compared with the observations made with the Stevenson screen arrangement.

The mean annual temperature of Brussels is 50°·5. The highest annual temperature was 53°·8 in 1834, and the lowest 47°·1 in 1879, there being thus a difference of 6°·7. During the fifty years the greatest difference in the temperature which occurred in the space of any one day was 37°·4 on January 26, 1881. In midsummer the daily maximum occurs at 3.22 p.m., and the minimum at 3.39 a.m.; but in midwinter these phases of the temperature occur at 3.39 p.m. and 6.36 a.m. As regards very high temperatures, 90°·0 or upwards has not been observed earlier than June 6 or later than August 19; and as regards the periods of intensest cold, a temperature of zero Fahr. has been recorded only between January 16 and 26. The longest continued frost was in 1845, when from February 7 to March 22, or during 45 days, the temperature fell to freezing or lower each successive day. The hardest winter was that of 1844-45, when temperature fell to freezing on 90 days, and the most open winter that of 1846-47, when frost was recorded only on 17 days. January 1838 was characterised by severe and long-continued frosts, temperature falling to 23°·0 or lower on 22 consecutive days, to 14°·0 on 13 days, and to 5°·0 on 5 days. The absolutely lowest temperature recorded was −4°·4 on January 25, 1881, and the highest 95°·4 on July 19 of the same year. The five mean coldest consecutive days of the year are January 8 to 12, the mean being 34°·4; and the five warmest July 15 to 19, the mean being 66°·5. A second maximum occurs from August 13 to 17, when the mean is 65°·5. The most marked interruptions in the annual rise of temperature from January to July are February 10 to 14, April 10 to 14, the middle of May, and June 26 to July 2; and the interruptions in the fall of temperature from July to January are August 13 to 17, already referred to, and December 4 to 7. The interest of these temperature interruptions is their wide geographical range, and no quite satisfactory explanation of them has yet been given.

Notes from the Leyden Museum. Vol. VII. (1885.)

UNDER the able editorship of Dr. F. A. Jentink, the Director of the Leyden Museum, these "Notes" have been published as quarterly parts throughout the past year. They form a volume of some 286 pages, which is illustrated by 10 plates. Among the more important "Notes" we may mention a monograph of the genus Cuscus by the Editor. Four species are recognised: *C. orientalis, C. celebensis, C. maculatus,* and *C. ursinus.* Gray's species, *C. celebensis*, is re-described, if indeed the original description can be said to merit such a title, being contradictory on most important points, and illustrated with figures of another species. In a note on some rare mammals the same author describes and figures *Hapalemur griseus* and *H. simus*, and notes the occurrence in Central Africa of *Epomophorus comptus* and *E. gambianus*, of *Paradoxurus stigmaticus* and *Ptilocercus lowii* from North-East Sumatra. He also re-describes, almost re-discovers, the rare *Antelope doria*, Ogilby, and *A. longiceps*, Gray. Mr. J. Büttenkofer's "List of Birds collected in Western Liberia" is prefaced by a most interesting account of the Zoological Expedition sent out to the West Coast of Africa under the supervision of the late Prof. Schlegel. Two years and a half (1880–82) were spent in Western Liberia, of which a rough sketch-map is given. The ordinary temperature of Liberia may tolerably well be compared to that of a European summer, rarely ever becoming insupportable. It has its dry and rainy seasons, and produces an extremely rich flora and fauna. His companion, C. F. Sala, died in June 1881, and Mr. Büttenkofer, from ill-health, was obliged to leave for Holland in April 1882. Mr. Stampfli has since gone out, and is now carrying on very successful researches in the southern portions of the district. This paper on the birds of Liberia is to form but one of a series devoted to the different groups collected.

The entomological notes are numerous, and contain descriptions of new or rare Coleoptera by C. Ritsema, J. W. van Lansberge, Neervoort van de Poll, Ant. Grouvelle, Rev. H. S. Gorham, and E. Candèze. Some of the new species are represented on coloured plates. F. M. van der Wulp continues his descriptions of exotic Diptera.

Mr. Schepman describes and figures a new Neritina from Gara, already named in manuscript by E. von Martens as *N. subocellata*, and Dr. Horst gives an account of a new Entozoon from the coeca of *Struthio molybdophanes*, Rchw. This Nematoid (*Sclerostoma struthionis*) is figured on Plate 8.

A title-page and table of contents and index accompany the October part.

Solutions of Weekly Problem Papers. By the Rev. J. J. Milne, M.A. (London: Macmillan and Co., 1885.)

IN our notice of the "Weekly Problem Papers" we gave an account of the contents, and accorded to the collection a warm welcome. In the meanwhile we have gone through the major part of the papers, and have found the questions to be well suited for their purpose: here and there may occur occasional apparent exceptions, but if these are exceptions, then were they also in the original papers in which they occur.

The solutions are neat, and in several cases elegant, and in all the object of "increasing a student's stock of mathematical knowledge, and of teaching him to put it to a practical use" is well kept in view. Mr. Milne is prodigal in his solutions, as not unfrequently he gives two solutions of the same question. To compensate for the non-insertion of solutions already printed in the collections of Answers to the Tripos questions of 1875 and 1878, Mr. Milne appends a collection of an equal number of questions of a similar character, with their solutions; these, with two appendices, occupy about one-fifth of the whole volume. The two notes are on the geometrical theory of envelopes and geometrical maxima and minima.

We congratulate Mr. Milne on having so successfully carried out his design. We believe it was originally in his plan to trace each question up to its original proposer, but this very difficult task he has apparently abandoned, at any rate he makes no sign in this first edition.

The two books together will be of great service to students who cannot afford to read with a private tutor, as the solutions are written out in proper style, and are as full in explanation as a student could expect them to be. There is a long list of errata, but of these the greater part are due to the original papers. We have noticed several more, but this is not the place for indicating them, as the student will in most cases be able to detect them himself.

LETTERS TO THE EDITOR

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

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

Heat Quantities

IT would be a great advantage if some definite and generally-accepted meaning were attached to the words "capacity for heat," "thermal capacity," and "specific heat." As it is I find that scarcely two text-books give the same definitions of these quantities. The French writers, Verdet, Jamin, Fourier (Freeman's translation), and Deschanel (Everett's translation), seem to agree in considering specific heat a quantity of heat; so also do B. Stewart and Daniell. Prof. Everett, in "Units and Physical Constants," defines thermal capacity as a quantity of heat, and on p 79 says that specific heat is also a quantity of heat, while on p. 80 he points out that it is of zero dimensions. In Maxwell's and Garnett's books it is pointed out that specific heat is a ratio, but in the latter the term is used loosely later in the book. Glazebrook and Shaw, in their "Practical Physics," make it a ratio.

To avoid all this confusion, I would propose the following definitions, on the analogy of the accepted ones for density and specific gravity:—

"*Capacity for Heat.*—The quantity of heat required to raise the temperature of any body, under given conditions, by 1° C., is called its 'capacity for heat' under those conditions.

"*Thermal Capacity.*—The quantity of heat required to raise the temperature of unit mass of any material, under given conditions, by 1° C., is called its 'thermal capacity' under those conditions.

"*Specific Heat.*—The ratio of the thermal capacity of any material to the thermal capacity of some standard material is called its 'specific heat.' This standard material is usually water at 0° C. or 4° C."

If some such definitions as these were generally accepted, a good deal of confusion would be avoided, and the analogy with density and specific gravity would be accentuated. At the same time the word "specific" would be defined to mean that the quantity to which it was applied was a ratio, and of zero dimensions. HARRY M. ELDER

Wellington College, Wokingham

Permanent Polarity of Quartz

THE second note in NATURE (Feb. 4, p. 325) contains an account of an "important discovery" by Dr. O. Tumlirz, which is wrong in all essential particulars. The permanent polarity discovered by that gentleman is not diamagnetic, but paramagnetic. Nor is it correct that "Dr. Tumlirz appears to think that these facts negative Becquerel's theory of diamagnetism," but on the contrary, he takes some trouble to show that his experiments are in complete agreement with that theory.

"Becquerel's theory (1850)" seems to be a name for the one discussed, though not adopted, by Faraday in his second paper (1845) on the subject, that all matter is magnetic, the medium occupying an intermediate position between so-called paramagnetic and so-called diamagnetic bodies.

ARTHUR SCHUSTER

[Dr. Tumlirz's own words are: "Der Bergkrystall ist hiermit der erste diamagnetische Körper der eine dauernde Polarität zeigt."—ED.]

Variegated Iridescent Halo

ABOUT noon of January 28 the sky was overcast by a horizontal cloud, and an iridescent halo appeared directly beneath the sun. The length of the arc was not measured, but appeared to be nearly or quite 90°. The most of the halo was of a bright yellow colour, but not far from the middle of the arc was a spot of green separated on each side from the yellow arcs by a short neutral tract. The green spot was nearly round, and its breadth (as measured from the sun) was nearly the same as that of the yellow arcs, and the angular distances from the sun of both the yellow and green arcs were approximately equal. Iridescent halos are seen here every year, usually as complete circles about the sun, and often showing all or several of the prismatic colours, each forming its own definite ring at different distances from the sun. Such halos appear through a thick haze, or quite thin cloud. This variegated halo appeared through a cloud so dense that the position of the sun could barely be made out by the eye, and it was of very variable density. The borders of the coloured arcs were not clearly defined, and apparently the light had been somewhat scattered by reflection upon the lower cloud particles. The temperature was several degrees below the freezing-point of water. Twice before during the present winter short monochromatic arcs of halos have appeared, once a yellowish-green, once a bright red.

The iridescent clouds recently seen by so many of your correspondents in Scotland appear to be, in part at least, the same phenomenon as here recorded, only in this case there was enough of the arc visible to definitely show that the colour was due to an iridescent halo. G. H. STONE

Colorado College

White Rainbows

PROBABLY A. E. E. will find some answer to his questions in the under-mentioned articles:—R. Smethurst, "Account of a White Solar Rainbow" (*Mem.* Phil. Soc., Manchester, iii. pp. 176-78, 1819); Aug. Bravais, "Observations sur le Phénomène de l'Arc-en-Ciel blanc" (*Proc. Verb.* Soc. Philom., pp. 33-35, 1846; *Ann. de Chimie*, xxi. pp. 348-61, 1847); F. Moigno, "Nouvelle Théorie de l'Arc-en-Ciel blanc" Moigno, *Cosmos*, [illegible] pp 107-11, 1852-53); G. J. Symons, "On a White Rain- or Fog-Bow" (*Quart. Journ.* Met. Soc., October 1875).

A. RAMSAY

A Nocturnal Hymenoptera of the Genus *Bombus*

MR. LEONARDO FEA, Assistant in the Museo Civico of Genoa, who is now engaged in a zoological exploration of Upper Burmah, and who has extensive experience in collecting insects, writes to me from Mandalay that, on the night of January 17, as he was walking in the "compound" of Dr. Barbieri, the moonlight being very bright, he was surprised to hear humming around a clump of flowering acacias. He at once proceeded to fetch a net, and, on capturing the hummers, found, to his no small astonishment, that he had got a fine species of the genus *Bombus*, of a uniform fulvous colour.

Being unaware that the fact of a nocturnal melliphagous Hymenoptera (all of which are eminently diurnal) has been before noticed, I should be glad to hear of any other cases to the point.

G. DORIA

Genoa, February 16

PHYLLOXERA AT THE CAPE

WE have received from a correspondent in South Africa some details of the long-dreaded appearance of the Phylloxera in the vineyards of the Cape Colony. As long ago as 1880 the importation of living plants in any form or shape was forbidden by the Cape Government. This measure was so strictly enforced that consignments of young beech-trees from England and of tree-ferns from New Zealand were not allowed to be landed.

In 1884 the prohibition was for a short time relaxed. But it was speedily revived, under a penalty of 500*l*. or two years' imprisonment with or without hard labour in the case of any one infringing it.

The insect has now, notwithstanding, actually appeared in a few vineyards near Cape Town, and in two others about twenty-four miles off.

Fortunately the Cape Government has competent scientific advice at hand. Mr. Roland Trimen, F.R.S., the Director of the South African Museum, and a well-known entomologist, attended the Phylloxera Congress at Bordeaux in 1881 as the representative of the Cape Colony. A Commission to examine and report on the outbreak has been appointed, consisting of Mr. Trimen, of M. Péringuey his assistant, and of Prof. Macowan, F.L.S., Director of the Botanic Garden. M. Péringuey is a Bordeaux man and a good entomologist; he first drew Mr. Trimen's attention to some suspicious-looking mites on a slide which had been taken from a Cape vineyard by the doctor of a French ship, about Christmas.

Two or three of the vineyards are simply swarming with Phylloxera. But in others it appears to have only recent centres. Unfortunately sulpho-carbonates and carbon bisulphide are little more than names in the colony, and it has been necessary to telegraph for a supply. Pending the arrival of the insecticide, the vines are being uprooted and burnt. The result so far is encouraging, and the small range of the insect leads to the hope that it may be well kept under if not stamped out.

OSCAR SCHMIDT

EDWARD OSCAR SCHMIDT was born at Torgau on February 21, 1823. When he had finished his preliminary education he was sent to Berlin, where he had the advantage of studying natural history, to which his mind early had a bent, under the superintendence of Johannes Müller and Ehrenberg. Schmidt, however, proceeded to Jena to take his degree in 1849, and he held the post of Professor of Natural History in the University until 1855. His "Manual of Comparative Anatomy," which went through several editions, was first published in 1849. Appointed Professor of Zoology in the University of Cracow in 1855, he was obliged, two years afterwards, to quit the country, owing to some unfortunate political complications, and he took refuge in Gratz. He was appointed Professor of Zoology and Comparative Anatomy in the University, and in time was made its Rector. During many of his vacation tours he visited the Ionian Islands and other places on the shores of the Adriatic, and, diligently working out the fauna of this almost tideless sea, he became more and more interested in the natural history of the Sponges, with the result that in 1862 he published his well-known and important work, "Die Spongien des Adriatischen Meeres," to which two supplements were issued in 1864 and 1866, followed in 1868 by a third supplement, which formed part of a new work on "Die Spongien der Küste von Algier." It will be conceded that this work of Schmidt's marked an epoch in the history of this interesting sub-kingdom. The enormous progress made in our knowledge of the natural history of the Sponges during the twenty-four years that have elapsed since Schmidt put forward his classification, and the immensely improved methods of research, may be said to have revolutionised the subject; but Schmidt's work will always be of value, and the merit of having grasped the leading features of the classification of the Sponges will generally be awarded to him. That he for the most part failed to perceive the proper specific and generic characters of the forms he describes and figures is not to be much wondered at. In 1870, leaving the Sponge fauna of the Mediterranean, he published his "Grundzuge einer Spongien Fauna des Atlantischen Gebietes," which was followed in 1874 by an account of the Sponges collected by the German Expedition to the North Sea; and his latest contribution to this subject was his work on "Die Spongien des Meerbusen von Mexico, 1879–1880." In 1872 he was appointed Professor of Zoology to the University of Strasburg, returning thus once more to his fatherland.

Though his works on "The Natural Sciences and the Philosophy of the Unconscious" and on "The Descendance Theory of Man and Darwinism" passed through several editions, and were translated into French, they need not be more particularly referred to here. As already noticed in these volumes, Prof. Schmidt died at Strasburg on January 17 last.

THE STORY OF BIELA'S COMET[1]

I ASK you to listen to-night to the story of Biela's comet. I will weave into the story enough of astronomy to justify its place in this course as a lecture.

This story has none of the interest which human passions give to stories of human life, and yet if it shall not be to you as interesting as a novel, it will be because I shall spoil the story in telling it to you. It is a true story. In other words, I mean to separate sharply what we know from what we guess.

One hundred and two years ago last night (March 8, 1772) a Frenchman named Montaigne, in the provincial city of Limoges, found a comet. He did what little he could with his small telescope to mark its place in the heavens, but it was not much that he could do. The comet was a faint one, not to be seen by the naked eye, and had a short tail, only one-eighth as long as across the disk of the moon. He did not dream that that little foggy speck of light was to be one day one of the most interesting comets in the solar system; in fact, that he himself was to be known to history only for having first seen it. This little comet is the hero of my story—a hero from humble life. Montaigne wrote to Paris of his discovery, and they saw it three or four times before it disappeared.

[1] A Lecture delivered by Prof. H. A. Newton, on March 9, 1874, at the Sheffield Scientific School of Yale College, U.S. The renewed interest in Biela's comet created by the great shower of meteors on November 27 last justifies giving space for this lecture. From the *American Journal of Science*.

Thirty-three years later, November 1805, another Frenchman, named Pons, saw the comet. It passed rapidly from the northern heavens, and in a month went below our horizon. It came this time very close to the earth, and I shall in a moment tell you how it appeared. It was visible to the naked eye, even in strong moonlight. Twenty years later, February 1826, an Austrian officer, Von Biela, again found the comet. So soon as an orbit could be computed, it was seen that the three comets of 1772, 1805, and 1826 were the same body. This has since been known as Biela's comet. Its exact path around the sun could now be told. Let me show it to you.

Let us look down upon the solar system from a point several hundreds of millions of miles north of it. Looking southward we should see the sun in the centre. The earth, with its moon, would travel around the sun in a path or orbit denoted by the circle in the figure (Fig. 1).

It goes about the sun once a year, being, on the 10th days of January, April, July, and October, at the points so marked on the diagram. The motion is opposite to that of the hands of a watch. Outside, five times as far from the sun as is the earth, will be the huge planet Jupiter, a part of whose path you see. It goes about the sun once in twelve years. The paths of the other planets are not in the figure, as I have nothing to say about them to-night. In the figures which I show you the earth's orbit is twenty inches in diameter, or one inch to nine million miles. An express railway train travelling all the time for a fortnight would pass over about the thousandth of an inch in this figure. The comet's path is the ellipse. Around this ellipse it travelled three times in twenty years, or once each 6$\frac{2}{3}$ years. When nearest to the sun, or at perihelion, it went within the earth's orbit, and when most distant it passed beyond Jupiter.

The comet's motion is very unequal. At D it moves very slowly. As it falls towards the sun the sun's attraction makes it move faster and faster, so that it whisks rapidly by P. As it then rises from the sun on the other side of the orbit, the sun not only turns it ever out of the straight path it would move in, but it stops its upward momentum, so that when it reaches D again it has only its old velocity with which to repeat its circuit. At P its velocity is twenty-eight miles, at D four miles, a second. In fact, to pass over the part lying apparently outside of Jupiter's orbit, just half of the whole 6$\frac{2}{3}$ years is required. I said *apparently* outside, for another fact must be noticed: while Jupiter and the earth may be said to move in the same plane, that of the figure, the comet's orbit, lies at an angle. Suppose the ellipse to be a metal ring, and let it turn about the line A B as a hinge, the part A D B rising toward you, and the part A P D retreating from you. The parts near D must rise about the half-diameter of the earth's orbit to give the true position of the two planes. Notice that the comet's and the earth's orbits cut each other

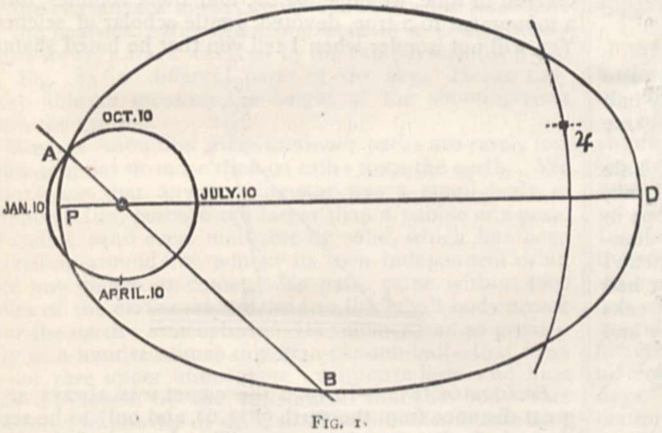

Fig. 1.

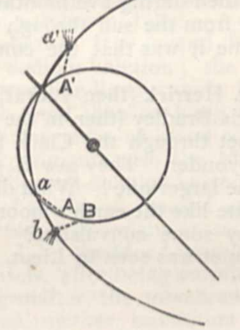

Fig. 2.

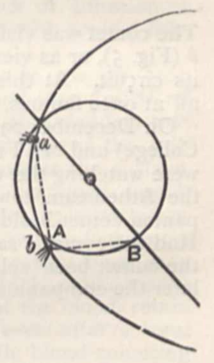

Fig. 3.

at the node on the line A B. The importance of this fact will by and by appear. The two orbits seem to cut each other at another point (below P), it is true, but because of the angle of the planes the cutting is only apparent.

Like all other comets, this one was visible only when near the earth and near the sun. Through the outer part of its path it was never seen, even with a telescope. The comet was seen in 1826 for the third time.

Positions in 1772 *and* 1805.—In March 1772 it was first seen from A in the direction A *a* (Fig. 2). It was last seen four weeks later from B in the direction B *b*. In November 1805 Pons found it when the earth was at A′ and the comet at *a*′ (Fig. 2). Both the earth and the comet were going to the node, the comet going faster than the earth. The earth passed the node just ahead of the comet. I have told you that the comet was then visible to the naked eye even in moonlight, and well it might be. On December 8, with the scale of the figures before you, it was only $\frac{3}{8}$th of an inch from the earth at the node. On the same scale the moon is $\frac{1}{40}$th of an inch from the earth. The comet passed $\frac{1}{10}$th of an inch outside the earth's orbit, but the earth was already past that point.

Dr. Schröter describes the comet: To the naked eye it was (Dec. 8) a large round cloud of light nearly as large as the moon. In a 13-foot telescope it had the same appearance, though it was much smaller, and it had a bright, star-like nucleus. The nucleus had not sharp edges, not even a definitely round form, but was like a light shining through a fog. Its diameter was about 112 miles, or, if we take its central light, 70 miles; speaking roughly, as large as the State of Connecticut. The whole cloud, as seen in the telescope, was some 6000 miles in diameter; to the naked eye perhaps 30,000 miles. How much smaller than 70 miles was the hard part of the nucleus we cannot say.

Position in 1826.—In 1826 it was first seen from A in the line A *a* (Fig. 3). Astronomers followed it with care, as they had come to know that it was a comet of short period, and not many such were then known. Its path then crossed just inside the earth's orbit at the node, but only $\frac{1}{500}$th of an inch in the diagram, or 20,000 miles, in fact, from it.

Position in 1832.—Six and two-thirds years brings us to 1832, and you can readily imagine with what interest this first-predicted return was watched for. Some of you also remember the widespread, though groundless, fears at that time of a collision of the earth and the comet. The comet was first seen by Sir John Herschel in September. In his 20-foot reflecting telescope he saw it pass centrally over a group of small stars of the 16th or 17th magnitude. The slightest bit of fog would have at once blotted out the stars. Through the comet, however, they looked like a nebula, resolvable, or partly resolvable, into stars. How thick the cometic matter was we do not know. Its extent laterally, was not less than 50,000 miles. Again, M. Struve saw it pass centrally over a star of the 9th magnitude. A like star was seen in the telescope at the same time, so that he was able to say that the comet did not dim in the least the one which it covered. The comet, as the figure (Fig. 4) shows, was in 1832 always at a great distance from the earth.

Another $6\frac{3}{8}$ years brings us to 1839. The comet came to perihelion, at P, in July. The earth and comet were on opposite sides of the sun both before and after July, and of course the comet was not seen.

Position in 1845.—Another circuit was finished in 1845-6.

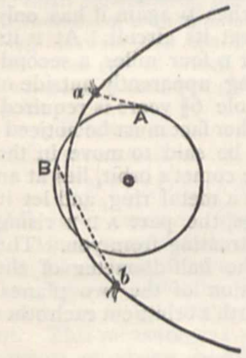

FIG. 4.

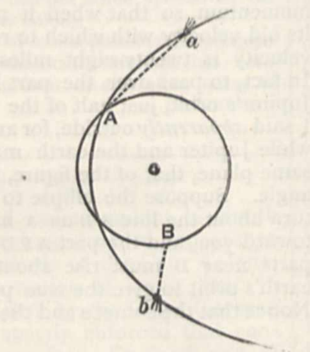

FIG. 5.

The comet was visible then during five months, from *a* to *b* (Fig. 5), or as viewed from the sun through nearly half its circuit. At this time it was that the comet became all at once famous.

On December 29 Mr. Herrick (then Librarian of Yale College) and Mr. Francis Bradley (then in the City Bank) were watching the comet through the Clark telescope in the Athenæum tower yonder. They saw a small companion comet beside the larger one! What did it mean? Had the comet a satellite like the earth's moon? Or had the comet been split by some convulsion? Two weeks later the companion comet was seen by Lieut. Maury and

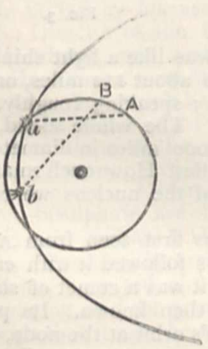

FIG. 6

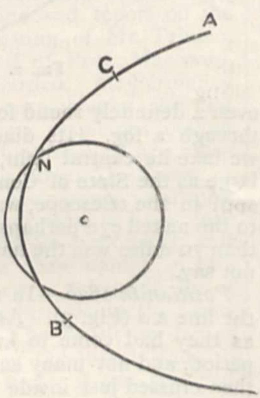

FIG. 7.

Prof. Hubbard at Washington, and two days after that it was seen by two or three European astronomers.

Changes were seen in the larger telescopes that increased the mystery. The faint companion grew in size and brilliancy. Each comet threw out a tail. Then the smaller one had two tails. Then the larger one had a pointed, or diamond-shaped, rather than a round head. Two nuclei were seen in the larger one, and it also had two tails. An arch of light was thrown over from one to the other. For some days in February the companion was the brighter of the two. Presently three tails were seen running from the primary, and three cometary fragments (one observer says five) around its nucleus. What could it all mean? Do you wonder that astronomers were excited by these wizard changes?

The companion comet was seen in Washington by Maury and Hubbard two weeks after it was seen here by Herrick and Bradley. Prof. Joseph Hubbard was the son of a resident of New Haven, well known to many of you from his connection with the New Haven Bank. Prof. Hubbard was graduated two years before (in 1843) at this College, and was now Professor in the Naval Observatory at Washington. He took up the study of the motions of the two Biela comets as special work, outside of his hours on duty. How faithfully he worked, four thick manuscript volumes of figures might tell. I cannot show you those books. They form, since Prof. Hubbard's death, a cherished memento in the possession of a friend. But I have brought another of Hubbard's volumes from the College Library, one of three upon the comet of 1843, in order to show you by what patient labour some of the results of astronomy must be wrought out. In your school days you called it a wondrously long sum that covered both sides of the slate. On the leaves of this book there are, as you see, one, two, three, and in some cases, I think, even four thousand figures upon the page. You will, I am sure, excuse me from telling in detail to-night how we learn about the sizes, distances, and motions of the comets. Eight or ten such volumes of figures, to be increased in time, we hope, by the four Biela volumes, form a monument to a true, devoted, gentle scholar of science. You will not wonder when I tell you that he hated shams.

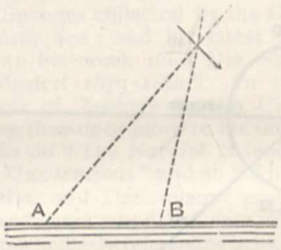

FIG. 8.

Position in 1852.—In 1852 the comet was always at a great distance from the earth (Fig. 6), and only to be seen through the largest telescopes. The changes of size and brightness of the two comets were remarkable, and as they could but just be seen, sometimes one and sometimes the other alone was visible; which one it was that a person saw at any time was only told by computation afterwards.

The two comets were now eight or ten times as far apart as they had been seven years before. They were at the point P, 1,250,000 miles apart. Prof. Hubbard found that he could not tell which comet of 1852 was preceding and which following in 1845. One supposition agreed as well with the observations as the other.

Perhaps the knowing ones among you have noticed that the arc from the node to the point marked Jan. 10 in the first diagram is too large for one month, for in 1772 the earth passed the node Dec. 9. But you will notice that when the comet is at D, and the large planet Jupiter is near by, he draws the comet toward the plane of the figure. The result is to bring the comet down to meet the earth's orbit farther from P. The node thus went back from Dec. 9 to Nov. 27, a distance of 12 days, or 12° in the circle. The figure represents this last orbit. By the same cause the inclination was reduced one-fourth, or from 17° to 12°.

Since September 1852 (with one doubted exception to be spoken of), neither of the two Biela comets had been seen. In 1859 their path was to us behind the sun. In 1866 they should have been at the point P on January 26.

A better chance of seeing them could hardly be. They were at all times to be away from the sun's light, and when nearest to the earth not more than one-fifth the sun's distance. The paths were carefully computed, and the action of all the planets, notably that of Jupiter, allowed for. A dozen observers for months swept the heavens with their telescopes, but not the slightest trace of the comets was seen.

Again, they should have come to perihelion a year ago last autumn (Oct. 6, 1872), but, as I suppose, neither of them was seen. With the loss of its hero, our story would seem to come to an end. I must ask your indulgence, however, for another chapter.

I suppose that each one of you has often seen a shooting-star. On a clear night you have seen a bright point of light travel quickly across the sky, as though a star had been shot from its place in the firmament. It may, if it was a large one, have broken into sparks as it disappeared, or have left a cloudy train along part of its path for an instant; or perhaps it was so faint even that you could not be quite sure that you saw anything. Some of you have seen those shooting-stars by hundreds in star showers.

Until near the close of the last century, poets dreamed, and other men guessed, about these objects, but knew nothing. Two German students, Brandes and Benzenberg, found out, and told us, that these bright flights were in the upper parts of the atmosphere. From the two ends of the city a track always appeared to be in the same part of the heavens. But when one went to a village many miles away, a track was seen by the two persons (at A and B, Fig. 8), in different parts of the sky. Hence they were able to measure the height of the shooting-stars from the ground.

We now know that these luminous paths are rarely less than 40 miles or more than 90 miles from the earth. We also know that any shooting-star was a small body, of unknown size, perhaps not larger than a pebble or a grain of coarse sand even, undoubtedly solid, which has been travelling around the sun in its own independent orbit, like any planet or comet. Its path came within 4000 miles of the earth's centre, and so the small body struck into the earth's atmosphere. Its velocity was so great—fifty or a hundred times that of a cannon-ball—that even in our rare upper atmosphere an intense light and heat was developed by the resistance, and the body was scattered in powder or smoke. These bodies before they come into the air, I call meteoroids. It is only when they have reached our atmosphere and begin to burn that we ever see them. They are then within 90 miles of the ground.

(*To be continued.*)

ON THE COAGULATION OF BLOOD[1]

BRÜCKE'S researches on the conditions of coagulation of blood have shown that, on the one hand, contact with foreign bodies makes blood coagulate, and, on the other, that contact on all sides with the fresh vascular wall obviates coagulation (Durante). Lacker has proved the influence of foreign bodies on blood-coagulation by microscopic observation of coagulation in its first stages. In partial contradiction to these results was the observation of Grünhagen that blood, when received into glycerine, and so long as it did not mix, remained liquid. To determine the nature of these influences the following experiments were made. Blood was drawn under oil from the carotid artery of a dog, and let stand at ordinary indoor-temperature; after twenty-four hours it was *not* coagulated. Then the blood was drawn into a vessel smeared inside with vaseline, and it too *did not coagulate*. When it was stirred with an oiled glass rod, no fibrin was separated; but when, even after several hours, part of this blood was poured into an ungreased vessel, it coagulated in a few minutes. Moreover, contact with an ungreased glass rod sufficed to make the blood in the greased vessel coagulate outwards from the rod.

Further experiments showed that the drying of the upper layers of the blood, and the presence of small quantities of dust, caused coagulation even in the greased vessel; if this was guarded against, the blood remained liquid for days, and the corpuscles sank to the bottom, the plasma remaining as a clear liquid above.

After pouring out the blood, the greased walls of the glass vessel showed neither blood-colouring matters, nor traces of a separated albuminous body. A repetition of these experiments at 37° C. gave the same result. In all the experiments blood was also, for comparison, drawn off into ungreased vessels, and in all these it coagulated, at the most, in a quarter of an hour.

In further experiments a small vaseline-lined glass tube was used as a canula; and the blood drawn through this into vaseline-lined vessels also remained uncoagulated.

When the outer orifice of a canula inserted in the carotid was closed, the blood column in it pulsated, without showing the least sign of coagulation even after two hours.

In all these experiments there was nowhere in the vessels with which the blood came into contact even a point for adhesion—such a point would have caused in shorter or longer time coagulation of the whole mass of blood. Thus the coagulative influence of foreign bodies appears to be due to their adhesion.

But to demonstrate that the anti-coagulative property of the vascular walls is due to the lack of adhesion, a further series of experiments was made with soaked fish-bladders and parchment-tubes.

The membranes lay several hours in 0·6 per cent. chloride of sodium solution; the blood was drawn off through a vaseline-lined canula into the bladders and tubes, which were then so hung in a litre of the salt-solution that the mass of blood was under the surface. In these experiments also the blood remained liquid, the surrounding salt-solution having no coagulative effect, while some of the blood, poured after twenty-four hours into an ungreased porcelain vessel for comparison, soon coagulated. Like the blood-vessels, which, unlike manufactured vessels, after being emptied of the blood, retain no colouring-matter, the membranes, even after several days, showed neither imbibition with blood-colouring matter, nor any trace of coagulated fibrine. Thus, by soaking in salt-solution, a property of the blood-vessels was imparted to the fish-bladders and parchment-tubes.

It can hardly be doubted, then, that while, on the one hand, lack of adhesion prevents blood from coagulating, so, on the other, the presence of adhesion gives the impulse to coagulation.

[1] By Ernst Freund, in *Wiener medicinische Jahrbücher*, 1885, Heft 1.

INOCULATION AS A PRESERVATIVE AGAINST CONSUMPTION

M. VERNEUIL has lately published a letter to the editor of the *Gazette hebdominaire*, M. Lereboullet, in which he proposes to set on foot an experimental inquiry into the possibility of finding some method of "attenuating" the presumed *virus* of tubercle, so as to make inoculation therewith practically useful against consumption, either as a prophylactic measure, like vaccination against small-pox, or as a means of cure, like Pasteur's inoculations in hydrophobia.

Three thousand francs have already been subscribed, and the respectable names of Cornil, Bouchard, Damaschino, and Potain are mentioned among those who approve of the investigation.

It must however be remembered (1) that with the exception of hydrophobia, an exception still on trial, no human disease but small-pox is known which can be prevented by inoculation; (2) that of epizootic diseases anthrax is only in certain cases guarded against by Pasteur's attenuated virus; (3) that the dependence of consumption on Koch's *Bacillus tuberculosis* is far from established; (4) that its fatality is very far below that of small-pox or hydrophobia, and its treatment far more successful.

Consumption is the most important disease of temperate climates, both by its prevalence, its mortality, and its incidence on young adults; so that the sacrifice of a few rabbits or cats for even a remote chance of controlling its ravages is well justified. But the chance is, we fear, remote.

NOTES

THE friends and former students of Prof. P. J. Van Beneden, of Louvain, are about to celebrate there the fiftieth year of his professorship. Since the year 1836 this distinguished *savant* has occupied the position of Professor of Zoology at the Louvain University, and it is proposed to present him, on the occasion of his jubilee, with a gold medal bearing his portrait. After half a century of teaching and the accomplishment of a vast amount of other work, Prof. Van Beneden still remains fresh in mind and body. His writings have embraced with equal success various branches of biological science, and have gained for him a reputation of the first rank, which has just been crowned by the award of the Cuvier Prize by the Academy of Sciences of Paris. There is no doubt that the proposed demonstration to honour Prof. Van Beneden on his jubilee will find a ready echo in this country, where he possesses numerous friends.

IN a recent debate in the French Chamber of Deputies on a Bill permitting any person by will to regulate the conditions of his funeral, a clause was added at the instance of M. de Mortillet, the eminent anthropologist, enabling any person to dispose of his body in favour of educational or learned societies. M. de Mortillet stated that the Autopsy Society founded by Broca had been allowed to retain the brains of Gambetta, Dr. Bertillon, and two journalists, but the authorities might at any time take these away from its museum, as also any bones or skeletons. The proposition was adopted by 268 votes to 198.

WE are glad to receive a copy of the *Annual Companion to the "Observatory."* Its object is to give, in a collected form, the whole of those Ephemerides which have hitherto been printed month by month in the *Observatory*. This issue is regarded as an experiment, and the editors ask for suggestions for the improvement of future *Companions*, and for criticisms on the present one. In future it is intended to issue it with the December number or before. The principal sources from whence the Ephemerides have been derived are as follows:—The "Meteor Notes" have been taken principally from the valuable series of papers by Mr. Denning in vols. i., ii., and iii. of the *Observatory*, supplemented from the British Association *Reports*. Mr. Denning has also kindly revised them. The Ephemerides for the physical observations of Jupiter and Mars are derived by permission from those calculated by Mr. Marth, and published by the Council of the Royal Astronomical Society in the *Monthly Notices*. The Ephemerides for the satellites of Mars, Saturn, Uranus, and Neptune are taken from the *American Nautical Almanac*, corrected, in some cases, for recent observations made at Greenwich. The elements of occultations and times of eclipses of Jupiter's satellites are extracted from the English *Nautical Almanac*. The Catalogue and Ephemerides of Variable Stars are derived from the *Annuaire du Bureau des Longitudes*. The publication will certainly be of much practical value.

ON the evening of Wednesday, February 17, Prof. A. B. W. Kennedy and the Committee of the Engineering Society held a successful *soirée* at University College, London, in connection with the College Society. Visitors were received in the engineering laboratory, where machinery was in motion, and Mr. A. S. Ashcroft's autographic stress diagram apparatus was shown in action. All the available space was occupied with exhibits. The College Society organised a show of photographs and photographic apparatus in the library, where Messrs. Clarke and Clarke exhibited their method of printing by gaslight. The *soirée* was attended by about 1000 visitors.

LAST autumn the run of salmon up most of our rivers, especially those falling into the North Sea, was quite unprecedented. It is worthy of notice that at the same time (that is from August to November) they ascended, in equal, if not still more remarkable, numbers, the rivers that flow into the North Pacific Ocean, as well on the Japan side as the American. The "canning companies" in British Columbia were quite unable to obtain boxes and barrels quickly enough to keep pace with the supply, and fine large fish were sold for a cent apiece. Had this abundance of the Salmonidæ (sea-trout and bull-trout were as numerous in proportion as salmon) been confined to this part of the world, one might have supposed that an epidemic amongst dog-fish had enabled a much greater number of smolts to escape at the mouths of the rivers on their descent than commonly do; but under the circumstances some more satisfactory explanation seems to be required. Possibly in some manner the quantity of ice in northern waters on both sides of America had an influence upon these fish, or those that prey upon them in the deep water.

THE Italian Ministry of Agriculture has just undertaken an interesting experiment. Half a million of fish eggs were artificially hatched, and the young brood has been distributed all over the centre of the Lake of Como. If the experiment succeeds fairly well, it will be taken up on a large scale, and the department will undertake the re-stocking of the Italian waters. Efforts will be made immediately to revive and extend the rearing of lobsters.

THE old Tour St. Jacques la Boucherie, Paris, celebrated in connection with Pascal's experiments on atmospheric pressure, is now the site of a Laboratory of Physics. The inauguration took place on January 13. The tower was lighted by incandescent lamps.

LARGE sulphur deposits are reported to have been recently discovered on the southern slopes of the Caucasus.

ECUADOR was visited by natural calamities during January, which probably have been the cause of great loss of life. On January 12 the sky in and around Guayaquil was of a dark red colour, as if coloured by an immense conflagration. Detonations heard in the direction of Cotopaxi, and accompanied by earthquakes and subterranean noises, showed that some volcanic eruption was in progress. The noise and shocks lasted for two days and nights. At Yaguachi, opposite Guayaquil, a rain of ashes was observed. It is feared that the town of Latacunga, which is situated at the foot of Cotopaxi, is destroyed.

A REMARKABLE effect of lightning has been recently reported by Prof. L. Weber in a German serial. At Ribnitz, in Mecklenburg, during a violent thunderstorm, with rain and hail, about 6 a.m., the lower pane of a window on the first floor was broken by lightning, and a jet of water was thrown upwards through the aperture to the ceiling, where it detached part of the ceiling, and this, falling with the water, broke a small cigar-table below. Three bucketfuls of water were afterwards taken from the room. The hole in the window was like that from a bullet, and there were radial cracks. The path of the lightning is not very clear, but that it passed through the glass could not be doubted. Some cigars on the table, it may be mentioned, were carbonised. As to the jet of water, Prof. Weber rejects the hypothesis of a sudden generation of vapour forcing up water from the street. Another explanation offered is that the lightning, passing through the window to the street, generated a vortex of air about itself with vacuum in the interior, through which the water was driven as through a tube. A third hypothesis remains, viz. that a conical *trombe* struck the street, was reflected, and passed through the window in the form of a jet of water. In this case the lightning would merely have accompanied or preceded the *trombe*. Prof. Weber seeks further light on such phenomena.

ON the evening of January 9 a very fine display of the aurora borealis was seen in the southern parts of Norway.

A MAGNIFICENT meteor was seen by the station-master at Leangen Station, in the north of Norway, on January 16, at 8.15 a.m., it being still dark. He states that the meteor first looked like a small star, but, approaching with great velocity, soon attained the size of a cheese-plate. It had a dazzling white light, very like the electric, and was clearly visible, being below the clouds in the upper part of the sky. When it had passed the zenith and reached the eastern horizon, it separated into several parts, which gradually became extinguished. It left a trail for a few seconds, brownish-yellow in colour. Another meteor, to which we referred last week as having been seen at Aas, near Christiania, at 5.30 p.m. on January 5, was also seen in various other parts of the province of Smaalenene, even as far south as Frederikshald, near the Swedish frontier (distance from Aas about 100 kilometres = 63 miles). It appeared there in the constellation Taurus, at 5.15 p.m., and moved in a north-westerly direction. It left a long bright trail, and its passage was, according to some, accompanied by a faint hissing.

ON New Year's Eve an earthquake was felt in the central parts of Norway, particularly at Elverum and Löiten, where the houses shook. Another shock was felt in the province of Christiansand, at about 4 a.m. on January 16, followed by vivid flashes of lightning. In several houses the doors sprang open, and furniture, &c., was moved. A girl was thrown out of bed in one place. The barometer was very low at the time, but remained the same as on the previous day.

PROF. LOMMEL has recently described (*Wied. Ann.* 1) an aërostatic balance for determining the specific gravity of gases. It is useful for lecture experiments. Under one scale of a balance is hung, by means of a wire, a closed glass balloon, which is inclosed in a glass vessel having in its cover a small hole for the wire. This vessel has a side tube, with stopcock, near the bottom. The instrument being balanced while air is in the vessel, another gas is allowed to stream in and displace the air, whereupon the balloon rises or sinks according as the gas is heavier or lighter than air. By adding weights in one scale or the other equilibrium is restored, and one finds how much more or less a volume of gas equal to that of the balloon weighs than the same volume of air at the same temperature and pressure.

WE have received the Calendars of the University College of Aberystwith and Cardiff for the Session 1885-86, and the reports of work in both cases are very satisfactory, showing, as they do, a considerable increase in the number of students, and in the general scope of the educational work. We have examined with especial interest the Aberystwith Calendar, for it will be remembered that during last summer the College there was almost wholly destroyed by fire. The Council met the situation by taking a large hotel, where the work of the institution is carried on apparently without any serious inconvenience. The Principal of this College calls attention to a question which requires the careful consideration of the responsible authorities of the three University Colleges of Wales, and which, for the sake of the equitable distribution of the prizes and scholarships of these institutions, it is to be hoped may speedily be settled. Principal Edwards points out the danger that healthy and legitimate rivalry between the Colleges is in danger of degenerating into a bid for students by the offer of money bribes, and he quotes the case of a student who wandered from one to the other, taking scholarships at all three by recommencing his course at each in succession. There is apparently no regulation preventing a graduate of one beginning as an undergraduate at each of the others, and carrying off the prizes to the disadvantage of *bonâ-fide* students. Unfortunately, the negotiations which have been undertaken to prevent this grave abuse have hitherto proved unsuccessful, but it behoves the authorities concerned to prevent this misapplication of money so nobly subscribed for education by all classes of the Welsh people. Two very interesting and suggestive tables will be found at page 25 of the Aberystwith Calendar. The first gives the ages of the students: 76 are over 20 years of age, 22 over 25, and 5 over 30. The second contains the occupations of the parents, and shows in the most marked way the struggles which, to their infinite credit, Welsh parents make to educate their children. This trait in the Welsh character is well known, but we have not seen it exhibited in this definite, concrete manner before.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus* ♀) from India, presented by Miss Douglas; a Chacma Baboon (*Cynocephalus porcarius* ♀) from South Africa, presented by Mr. F. Radcliffe; a Ring-tailed Coati (*Nasua rufa* ♀) from South America, presented by Miss A. Pagella; an Orange-winged Amazon (*Chrysotis amazonica*) from South America, presented by Mr. G. F. Richards; two Feline Douracoulis (*Nyctipithecus vociferans*), two Silky Marmosets (*Midas rosalia*), a Razor-billed Curassow (*Mitua tuberosa*), a Mantled Buzzard (*Leucopternis palliata*) from Brazil, a Raccoon (*Procyon lotor*) from North America, purchased; a Collared Fruit Bat (*Cynonycteris collaris*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE NEW STAR IN THE GREAT NEBULA IN ANDROMEDA.—As the *Nova* in Andromeda was the first object of its kind to which accurate photometric methods of observation were applied, Prof. Seeliger of Munich has taken the opportunity of investigating whether the observed variations of brightness throw any light on the physical history of the phenomenon. If we suppose that the surface-temperature of a "new" star is suddenly increased by an enormous quantity, and, in consequence, the brightness increased to a corresponding extent, and assume that the latter is proportional to an arbitrary power, n, of the temperature, then the light curve constructed from the observations will be a curve which represents the nth power of the successive temperatures of a cooling body. Prof. Seeliger has deduced an expression for the temperature of a sphere at any time, t, on the assumption that the sphere is homogeneous with respect to the conduction of heat, that at the time $t = 0$ it has the same temperature throughout its interior, and that the temperature of the surrounding medium is zero. If, then, h be the brightness corresponding to a temperature θ, we have $\theta = h^{\frac{1}{n}}$, and using Pogson's scale for transforming brightness into stellar magnitude, there results a formula for the magnitude of the cooling star at any time. For the purpose of comparing his formula with Herr Müller's photometric observations of the *Nova*, extending from 1885 September 2 to October 13, Prof. Seeliger assumes that $n = 1$, and that the epoch for which $t = 0$ is 1885 August 27d. 8h. Berlin M.T. He also uses quite approximate values for the constants involved in his formula, the more accurate determination of which would be a work of difficulty. Under these circumstances he gets a very fair agreement between the observed and computed values, which would, he considers, be improved by using more accurate values of the constants, and of the epoch for which $t = 0$. The magnitude corresponding to this latter epoch is 7·73. Considering that there is evidence to show that the Andromeda nebula is, in part at least, a star-cluster consisting of a vast number of faint stars, Prof. Seeliger thinks it not improbable that the blazing forth of the *Nova* may have been due to a collision which caused an enormous development of heat and light. At all events, the fact that his formula represents the observations tolerably well appears to him to be sufficient evidence to show that the supposed conditions are not, in the main, at variance with the actual circumstances of the case.

PRESENT STATE OF THE SOLAR ACTIVITY.—The sunspot maximum, after some remarkable oscillations, was definitely attained about the close of 1883, the interval from the period of minimum having been nearly two years longer than usual. Since that date there has been a steady diminution in both the numbers and areas of the sunspots. M. R. Wolf gives (*Comptes rendus*, vol. c. No. 3, and vol. cii. No. 3) the following values for his relative numbers for the last three years: 1883, 63·7; 1884, 63·3; 1885, 50·3. The diminution in the last part of 1885 was particularly marked, there having been a vigorous rally in the months of May, June, and July, followed by a rapid decline. The relative numbers for the last three months of the year fell far below the mean of the twelvemonth. The figures given by M. Tacchini closely correspond to those given by M. Wolf, as the following table will show. The last three columns give Tacchini's numbers:—

1885	Wolf's numbers	Relative frequency	Relative size	Daily number of groups
January	31·4	19·57	43·19	4·33
February	67·2	23·81	77·33	5·96
March	46·6	16·23	44·92	2·92
April	54·6	15·10	56·86	3·48
May	80·5	18·68	86·21	5·80
June	82·1	22·36	132·76	5·21
July	61·4	15·41	90·22	4·45
August	47·7	11·20	44·70	3·40
September	43·4	9·14	59·20	3·31
October	42·6	12·55	55·64	3·09
November	26·8	6·35	22·90	2·30
December	18·9	4·84	21·44	2·12

Faculæ have not shown so rapid a decline, but there has been a distinct falling off in these also; the difference, however, is at present noticed rather in a loss of brilliancy than of apparent area. But hitherto the prominences have shown but slight indications of a participation in the falling off so strongly exhibited by the spots. The following numbers, derived from papers by the Rev. S. J. Perry in the *Observatory* for February 1885 and 1886, show indeed a slight increase of energy for 1885 over 1884:—

Year	Mean height of chromosphere, excluding prominences	Mean height of prominences	Mean extent of prominence arc
	″	″	° ′
1880	7·93	23·46	23 21
1881	8·04	24·61	33 18
1882	8·24	24·55	40 57
1883	8·27	27·23	41 24
1884	7·94	25·74	29 6
1885	8·00	28·67	28 25
Mean 1880–1885	8·07	25·71	32 45

The mean extent of prominence arc is thus the only element which seems to point to the maximum being past.

But if the prominences do not show any close correspondence to the behaviour of the spots, M. Wolf finds that the variations of magnetic declination have responded to their changes fairly closely. In the paper alluded to above he gives, side by side with his "relative" spot numbers, the monthly means of the variations in magnetic declination as observed at Milan. The mean observed value for 1885 is 7′·95, whilst 7′·88 is the mean value computed from the formula M. Wolf had deduced in former years for Milan, $v = 5'{\cdot}62 + 0{\cdot}045\ r$, where r is the relative sunspot number for the year.

Prof. Spörer points out (*Comptes rendus*, vol. ci. No. 26) that the spots have not been equally distributed between the two hemispheres during the period of maximum, but that throughout the years 1883, 1884, and 1885 there has been a nearly constant predominance of southern spots over northern; whereas in the period from minimum to the end of 1882 the predominance rested, on the whole, with the northern hemisphere. This alteration has also been accompanied by somewhat of a check in the regularity of the progress of the spots towards the equator, which is usually so marked in the interval from one minimum to the next.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 FEBRUARY 28—MARCH 6

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

At Greenwich on February 28

Sun rises, 6h. 49m.; souths, 12h. 12m. 41·5s.; sets, 17h. 36m.; decl. on meridian, 7° 52′ S.: Sidereal Time at Sunset, 4h. 10m.

Moon (New on March 5) rises, 3h. 39m.; souths, 8h. 6m.; sets, 12h. 34m.; decl. on meridian, 18° 16′ S.

Planet	Rises	Souths	Sets	Decl. on meridian
	h. m.	h. m.	h. m.	° ′
Mercury	7 5	12 28	17 51	7 59 S.
Venus	5 28	11 4	16 40	5 19 S.
Mars	18 7*	0 53	7 39	8 23 N.
Jupiter	19 41*	1 45	7 49	0 4 S.
Saturn	11 21	19 32	3 43*	22 45 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

March	h.	
3	5	Saturn stationary.
5	...	Annular eclipse of the Sun; not visible in Europe.
6	12	Mars in opposition to the Sun.
6	18	Mercury in conjunction with and 0° 8′ north of the Moon.

Variable-Stars

Star	R.A.	Decl.		
	h. m.	° ′		h. m.
U Cephei	0 52·2	81 16 N.	Mar. 3,	20 57 *m*
Algol	3 0·8	40 31 N.	Feb. 28,	23 24 *m*
			Mar. 3,	20 13 *m*
λ Tauri	3 54·4	12 10 N.	,, 2,	18 12 *m*
R Lyncis	6 51·9	55 29 N.	,, 2,	*M*
ζ Geminorum	6 57·4	20 44 N.	,, 4,	21 30 *M*
V Geminorum	7 16·8	13 19 N.	,, 6,	*M*
δ Libræ	14 54·9	8 4 S.	,, 4,	22 36 *m*
U Coronæ	15 13·6	32 4 N.	,, 5,	20 13 *m*
V Coronæ	15 45·5	39 55 N.	,, 5,	*m*
U Ophiuchi	17 10·8	1 20 N.	,, 3,	5 28 *m*
			and at intervals of	20 8
X Sagittarii	17 40·4	27 47 S.	Mar. 3,	0 0 *m*
			,, 5,	21 30 *M*
W Sagittarii	17 57·8	29 35 S.	,, 6,	21 30 *M*
β Lyræ	18 45·9	33 14 N.	,, 6,	0 0 m_2
δ Cephei	22 24·9	57 50 N.	,, 3,	0 0 *M*
R Aquarii	23 37·9	15 55 S.	,, 3,	*M*

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

Meteor Showers

The first week in March furnishes the most favourable nights of the month for meteor observation, but none of the great periodical showers occur at this time. Amongst the radiants represented are the following:—

Near γ Orionis, R.A. 80°, Decl. 6° N.; near α Persei, R.A. 50°, Decl. 48° N.; near η Bootis, R.A. 205°, Decl. 17° N.; near ξ Sagittarii, R.A. 280°, Decl. 17° S.; Ursa Major, R.A. 180°, Decl. 60° N.

BIOLOGICAL NOTES

CONTINUITY OF PROTOPLASM.—M. L. Olivier proposes, in the *Comptes rendus*, the following methods for determining the connection between the protoplasmic contents of adjacent cells:—(1) Photography. This distinctly reveals the perforation by canals of the cell-walls, when photographed direct with a magnifying power of 300–700 diams. (2) Direct observation, with powers magnifying from 700–900 diams. Though successful in some instances, this method is for the most part wholly insufficient. (3) Staining sections. This was successful in many plants in demonstrating the protoplasmic connection. (4) Injection into organs. When injected slowly, under pressure, with a fluid capable of colouring protoplasm, if the injection took place in a sufficiently uniform way, the canals were rendered visible. By the methods thus indicated continuity of protoplasm was established in the stem and leaves of the box-tree, and in *Ficus elastica*.

NEW EDIBLE FUNGUS.—Mr. Colenso calls attention to the rapidly-increasing value as an article of export from New Zealand of *Hirneola polytricha*. This mushroom, first described from the East Indies and Java by Montagne, is of various sizes and shapes, some specimens measuring even a few inches. It is found in New Zealand growing on the trunks of trees, both on living and on decaying ones, especially on the latter while standing, particularly on the stems of *Corynocarpus lævigata* and on *Melicytus ramiflorus*. Both of these are endemic. The former is mostly confined to the sea-shore, where it often forms dense and continuous thickets. The latter tree is scattered plentifully throughout the country. When dry, the mushroom becomes shrivelled up, and is as hard as horn; when wet, it is soft and elastic, almost subgelatinous. It grows in compact gregarious masses. The market for this fungus is China, where it is largely used by the Chinese in soups. It appears that another species of the same genus indigenous in North China has long been an article of commerce. Mr. Berkeley notes of our British species, *H. auricula-judæ*, that it was once a popular remedy for sore throats, and adds that it is still occasionally sold at Covent Garden Market. The New Zealand species is plentiful, and obtained at little cost, the drying of it being an easy matter. Originally the price paid to collectors was a penny per pound; now it is nominally twopence halfpenny, while its retail price in China is five times this. The *declared* value per ton at the Customs ranges from 33*l*. to 53*l*. a ton, and is doubtless much below its real value. During the last twelve years some 1858 tons of this fungus were exported, chiefly from the ports of Auckland and Wellington, and of a declared value of almost 80,000*l*.—(*Trans.* Penzance Nat. Hist. and Antiq. Soc., 1884–85.)

WORMS IN ICE.—Prof. Leidy had examined a block of ice which was part of the stock of ice stored at Moorestown, N.J., and had been nearly a year in store; it was full of air-bubbles and water-drops. On being melted a number of worms were liberated, and proved to be in a living and quite active condition. It is probable that while imprisoned in the ice they may not have been frozen, but perhaps remained alive in a torpid state in the water-drops; but it seems remarkable that these animals should remain so long alive in the ice, and yet die, as they did, almost immediately, in the melted ice-water. Of course the fact points to the advisability of not employing spongy ice as an article of food. Dr. Leidy, believing the form to be as yet undescribed, gives a diagnosis of it under the name *Lumbricus glacialis*; it is from 4 to 6 centimetres long, of from 35 to 50 segments; oral segment unarmed, eyeless; succeeding segments with four rows of podal spines, in fascicles of three.—(*Proc. Nat. Sci. Philadelphia*, December 22, 1885.)

STAR-FISHES FROM SOUTH GEORGIA.—Dr. Studer describes a small collection of star-fishes made by Dr. v. d. Steinen, the naturalist of the German Polar Expedition in 1882–83, who had a meteorological station at South Georgia. Of the 14 species collected, 9 belonged to the family Stelleridæ, and 7 of these were new, 5 to the Ophiuridæ, of which 4 were new. Most were collected in quite shallow water. The general character of the fauna is like that of Kerguelen Land; and, to assist the comparison, Dr. Studer gives a comparative list of the known species from the South American (Falkland Islands, Magellan Straits) district and that of Kerguelen Land. All the new species are well figured in two plates which accompany the memoir.—(*Aus dem Jahrbuch der wissens-Anstalten zu Hamburg*, xi. 1885.)

GEOGRAPHICAL NOTES

THE last number (15) of the *Journal* of the Straits Branch of the Royal Asiatic Society contains several papers of much geographical interest. In the first, Mr. Swettenham describes a journey across the Malay peninsula from Kwala Bernam in Perak, through Pahang to Kwala Pahang on the east coast. The paper is in the form of a journal, but, unfortunately, the accompanying sketch-map is so defective as to be quite useless to assist the reader in following the narrative. Père Couvreur, of the Missions Etrangères at Singapore, contributes an account of a recent journey through Laos from Bangkok to Ubon, a town on the Szimûn, a tributary of the Mekong, including a visit to the ancient Khmer city of Puthai-Saman, the monuments and architecture of which make it similar to the renowned Angkor, but on a reduced scale. To the people of the country these magnificent ruins are the work of avals, so completely has all trace of the great civilisation of which they are the eloquent witnesses disappeared from Cambodia. This paper is also in the form of a journal, but is not accompanied by a map. There are several other papers of interest (such as the translation of old Valentyn's account of Malacca, and the account of the Dutch expedition of 1877–79 to the interior of Malaçca), but these are not original to the Straits Asiatic Society.

THE *Verhandlungen* of the Berlin Geographical Society just published (Band xiii., No. 1) contains an account, by Dr. Wolff, of the journey of the expedition sent out by the African Society in 1884 from San Salvador to the Quango and back, and Dr. Diener writes on the mountain system of Lebanon, on which he has also a paper, already noticed, in the February *Petermann*. The current *Zeitschrift* (Band xx., Heft 6) of the same Society is largely occupied by a bibliography of the works, papers, maps, &c., relating to geography published during the year ending November 1885. There is, however, a curious list of the lengths and drainage areas of 376 rivers of the world. These figures are necessarily approximate only in most cases. It is noticeable that, while the Mississippi is 5882 kilometres in length, and the Amazon only 4929, the drainage area of the former is less than half that of the latter, the figures being: Mississippi, 3,201,545, and the Amazon 7,337,132, square kilometres. The only other paper in the number is an exceedingly interesting one by Herr Rohde, on the Terenos tribe, which inhabits the district to the west of the Brazilian town of Miranda, and stretches as far as the Bolivian frontier. They are really Chaco Indians who have migrated from Bolivia. The writer describes, all too briefly, their appearance, mode of life, occupation, and customs—especially their festivals.

THE Austrian traveller, Mr. C. Hermann, who started on a West African exploring tour in April last, has returned to Vienna. Having visited Liberia, Cameroon, Eloby, Gaboon, and other points on the coast, he arrived at the Congo early in July, and expected the arrival of Dr. Lenz at Banana. In order to engage the necessary porters for Dr. Lenz's expedition he went to Loango, but returned to Banana without having succeeded. He left Dr. Lenz on October 20 and returned to Europe.

DR. BERNHARD SCHWARZ writes from Monrovia (Liberia), under date January 23, as follows:—"As chief of an official expedition 'for the investigation of the up-country districts of Cameroon,' I reached Cameroon on November 6 last, and thence I proceeded eastwards with forty Bakwiri porters (from the Cameroon Mountain) on the large main road leading to the interior over the magnificent slopes of the Cameroon Peak. I penetrated through the immense virgin forests, which are peopled with elephants, and in which coffee, india-rubber, &c., grow, and and safely reached the interior of our colony, never before visited by a white man on account of the energetic resistance offered to all traders by the natives. Here live the Bafarami in the Bafon Land, hitherto not even known to the world by name. They cultivate the soil and keep cattle, and are comparatively civilised. I visited Kumba and Kimendi, their large towns, but on account of an attack made upon us by 500 armed slaves I could not see the upper Calabar, which must have been quite near. The maps hitherto existing of this interesting district, which may be of the highest importance for the whole future of our colony, are either insufficient or else quite wrong."

THE SUN AND STARS[1]

WHEN we have to consider the stars taken in their entirety, it is obviously convenient that we should begin with the sun, because in that way we shall be enabled to go outwards from the known; since it is easily to be understood that it is within

[1] A Course of Lectures to Working Men delivered by J. Norman Lockyer, F.R.S., at the Museum of Practical Geology. Revised from shorthand notes.

our power to know very much more about the sun, which is the star that lies nearest to us, than about the other stars, so far as detailed structure, at all events, is concerned, for the reason that the sun, although actually so far away, is relatively very near to us.

The Sun's Distance[1]

The distance of the sun we may take to be about 93,000,000 of miles, and, although that seems a long way in terrestrial reckonings, twice that distance is the smallest base line which astronomers can use in dealing with the distance of the star which is next nearest to us, to say nothing of the millions of others more remote. The sun, from being relatively so near to us, appears as a body of a different order. The stars proper, however powerful the telescope with which we regard them, appear to us as the finest points imaginable, whereas the sun gives us the appearance of a circular disk, this disk being the projection of a sphere. That part of the sun with which we are most familiar is in fact a sphere of something like 860,000 miles in diameter; hence, taking the diameter of this world of ours roughly at 8000 miles, the diameter of the sun is more than 100 times as great.

The moon, the nearest celestial body to us, journeys round us at the relatively small distance of about 240,000 miles. The accompanying drawing will enable us to compare the orbit or path of the moon round the earth and the space which it incloses, with the actual circumference of the sun (Fig. 1).

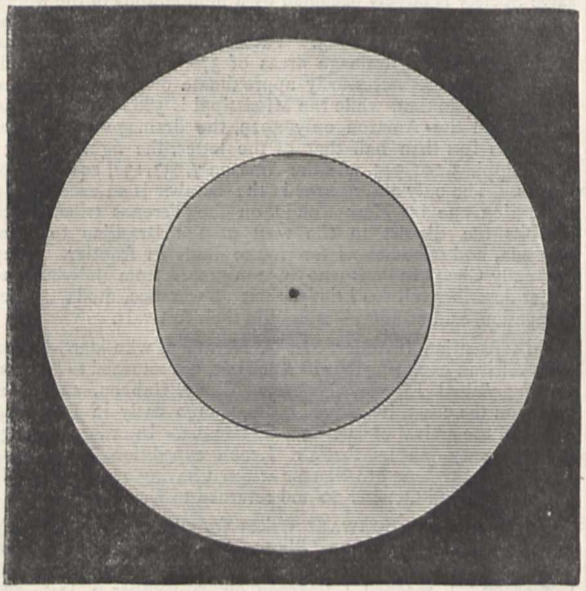

FIG. 1.—The orbit of the Moon and the circumference of the Sun compared.

In the centre we have the earth very much larger than it ought to be, and the inner disk represents the space included in the orbit of the moon. The outer one represents the disk of the sun; so that it is clearly seen that if the sun were a soap bubble it would be quite easy for the earth's attendant satellite, the moon, to carry on its revolution inside it; in fact the orbit of the moon would not lie very much past half-way from the centre of the earth to the circumference of the sun.

The Sun's Envelopes

The next point which has been made clear by the work of the last qnarter of a century, let us say, is that this central nucleus which we see ordinarily and call the sun, is only, after all, a very small part of it. Outside it there is a shell very exquisite in colour. This might be described as a sort of sea surrounding the central nucleus, if that expression did not give the idea that the nucleus itself was a solid, which it is not.

This first envelope, the *chromosphere*, as it is called, represents a sort of atmosphere or sea surrounding what is named the *photosphere*, to a height varying, say, from 5000 to 10,000 miles. I have said that it is exquisite in colour. It is of an intensely brilliant pearly white at the base, and of a magnificent scarlet, as a rule, higher up. But this is not the outside of the sun by any means. Further from the centre, again, there is another region, which we may call the inner corona, overlying the photosphere and chromosphere; the height of this atmosphere—I mean the distance from the top of it on the average down to the photosphere—we may again roughly take at something like 100,000 miles. Not very many years ago, in the text-books we were told that the earth's atmosphere only extended to a height of 50 miles, so that it can be easily recognised that we are dealing with an atmosphere on a very large scale indeed when we come to touch the sun.

These portions of the sun's atmosphere are only very rarely seen under the best conditions. We can feel, so to speak, the lower reaches every day in our observatories, but we can only see them when the sun is eclipsed. When, in an eclipse, we can get a good sight of the inner corona, what we see is very beautiful indeed, because we not merely get a pearly shell of light, which, roughly speaking, may be taken to be 100,000 miles high, but we see stretching into it from and through the chromosphere beautiful and curious objects called "red flames," or "prominences," or "protuberances."

Have we yet finished with the solar envelopes? No; there is still another. There is still an upper atmosphere, and to this I must ask you to give a height of anything you like between half a million of miles and a million and a half; and I speak thus indefinitely for the reason that the exact limit is at the present moment occupying those who are concerned in these matters. The limit of course you will understand is a limit which can only be determined during eclipses. Now the sun is only eclipsed for something less than a week in a whole century over the whole earth, and I suppose that if an observer of eclipses were to give his whole time to them he could not spend more on the average than six minutes every two years, so that the time is not excessive in which the astronomer either has to make observations or to make up his mind as to what he sees. You must not, therefore, be surprised when I give you this large choice. If we call it roughly a million of miles, we at all events shall not be very far off the truth, even supposing the height to be constant; but it would appear indeed that the height varies every time we have a chance of observing it. On that point we shall have a great deal to say further on.

When we come to this outer atmosphere, we pass from one with a more or less concentric boundary, to one with a most irregular outline, full of strange forms varying in an almost inexplicable manner from eclipse to eclipse.

In the eclipse of 1878 one of the observers who took special precautions to shield his eye from any brighter light at the moment that the eclipse took place, imagined, or saw, the fainter portions of the solar corona, or some solar surrounding, extending to several diameters. The outer corona is not only very strange in its appearance, but wonderful in colour, and full of detail for a considerable distance from the dark moon.

To sum up some of the principal points—by no means all of them—we may say, first, that its outline is very irregular; that there seems to be a flattening, or very often two opposed flattenings, at opposite ends of a solar diameter. This tends to make the thing look very often more or less square. In all parts of it, irregularly (by which I mean that you cannot predict quite where they will be), you get radial rifts in which the light is much less intense than elsewhere.

I have, then, indicated that the sun that we see is not the whole of the sun. Hence, when we study the stars we shall probably find that we have not only to take into account the phenomena presented by the sun as we ordinarily see it, but others associated with those parts of the sun which are only revealed to us from time to time, and the possibility that such phenomena as we see on the sun may be enormously intensified in other bodies.

The Sun's Rotation

The next question that we have to put concerning the sun is this:—Since it is a sphere as the earth is, and since the earth rotates on its axis, does the sun rotate on its axis? How are we to answer that question?

[1] The most recently determined value of the sun's distance depends upon Prof. Newcomb's determination of the velocity of light. This velocity Prof. Newcomb values at 299,860 kilometres per second, with a probable error of 30 kilometres either way. Combining this with Nyrén's value of the constant of aberration 20"·492, the solar parallax = 8"·794, which gives a distance of 149·61 millions of kilometres. This equals 92,965,020 British statute miles.

In this way. Sometimes when we look at the sun it is a beautifully pure hemisphere, almost equally illuminated all over it with the exception of a darkening towards the edge, about which more presently; but at other times these conditions are altered. It is more or less covered with what are called spots. It is more than two centuries and a half ago since it was clearly demonstrated that these spots belong to the sun itself; and it will be clear, therefore, that if these spots really do belong to the sun, or, to be more precise, are phenomena occurring in the photosphere—which is the part of the sun which we usually see, the rotation of the sun will be demonstrated if these spots are found to travel regularly across its disk; whereas its fixity will be demonstrated if we find that the spots do not move at all. A very great deal of work has been done in this direction, and it has been determined beyond all question that the sun does rotate like the earth and like the other planets of our system, and that it rotates from west to east, contrary to the hands of a watch, as the earth does.

We next have to consider the plane of this rotation. The earth moves round the sun in a plane which we call the plane of the ecliptic; but we know that the earth does not rotate in this plane. There is a difference of 23½° between the equatorial and ecliptic planes. We therefore say that the earth's axis is inclined 23½° to the plane of the ecliptic. Further, if we wish to know the particular direction in which it is inclined, we must determine the longitude of the ascending node, and this done, we can determine the star towards which the earth's axis points.

Well, can we get out these facts with regard to the sun? Yes. The sun does not rotate in the plane of the ecliptic as we might first of all imagine that it would do. Its axis is inclined at about 7° to that plane, and its ascending node does not lie in the same direction in space as the ascending node of the earth, but it is distant from it some 73°. We have two very accurate determinations of these two sets of data. Carrington's first value gives—

	°	′
Longitude of node	73	40
Inclination	7	15

We see, although the sun's rotation does not take place in the plane of the ecliptic, it does not take place in a plane very far removed from it.

Spörer, a German observer, who has taken up this question since Carrington, makes some very slight changes. He gives—

	°	′
Longitude of node	74	36
Inclination...	6	57

The right ascension of the star towards which the sun's pole points is 18 hours 14 minutes, and its declination 64° N. It lies half-way between our own pole star, and the bright star *α* Lyræ.

We may now carry these considerations a little further. When the earth passes through the nodes of the sun's equator, that is

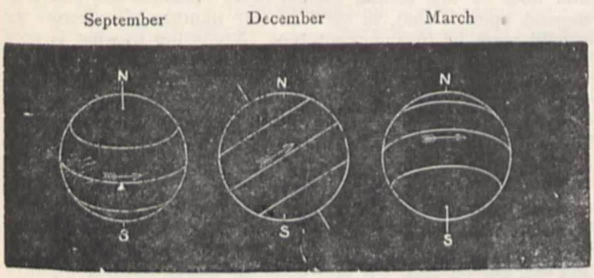

Fig. 2.—Position of the Sun's axis, and apparent paths of the spots across the disk, as seen from the Earth at different times of the year. The arrows show the direction in which the Sun rotates. The inclination of the axis is exaggerated, so that the effect produced may be more clearly seen.

to say, when the axis of the sun is at right angles to the line joining the centre of the sun and the centre of the earth, it will be perfectly clear that the spots will appear to travel straight across the disk. The two times of the year in which this occurs are June 3 and December 5. The two poles of the sun at those times are in fact on the sun's limb, and it is that condition really which makes the path straight (Fig. 2).

From June 3 to December 5 the north pole of the sun is gradually moving earthward. It will be clear, therefore, that during that time the spots, instead of travelling in straight lines across the disk, will gradually have their paths curved with the convexity downwards. When we have got to December, from that time to the next June it is the south pole that will be inclined towards the earth; and therefore the spots will then move with the convexity of their paths upwards.

So far I have said nothing about the period of the rotation of the sun. The question of the sun's rotation is not quite such a simple matter as it might appear at first sight. Here we must be quite honest to the first workers, and I must tell you that the actual facts appear to have been clear to a man who lived three centuries ago—Scheiner—who was the first to observe the spots with any very great and continuous care; he made what appeared to him the extraordinary discovery that the spots which were nearest to the sun's equator appeared to travel at a quicker rate than those which were nearer the sun's poles. The average time in which the spots appear to cross the sun is about 28 days. That you will understand is what we may term the

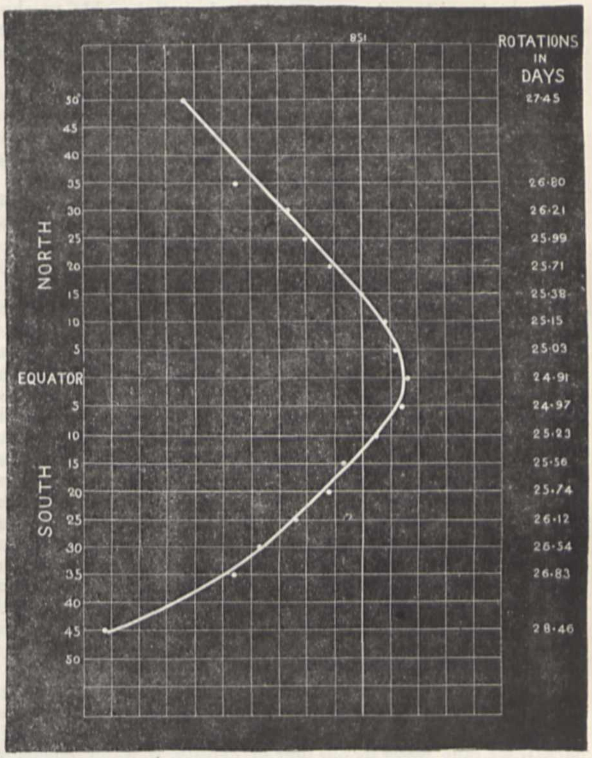

Fig. 3.—Curve showing the period of rotation of the photosphere in different latitudes north and south from Carrington's observations; 851′ of solar longitude per diem = rate of rotation in lat. 15° N. The vertical lines represent differences of 10′ of longitude, + to the right, − to the left, of the line cutting the curve in lat. 15° N.

synodic period, because the observations are made from the earth, which is moving in the same direction as the spots while the observations are being made. Making the correction for the movement of the earth, and getting the actual period, the 28 days have to be brought down to something like 26 for an average spot. Carrington, whose results for the plane of rotation I have already stated, also paid very great attention to this point, and to his work, and also to Spörer's, we owe very much of our present knowledge on this subject.

Carrington, from the observation of some thousand spots, came to the conclusion that the photosphere in which these spots are supposed to float really moves more rapidly at the equator than it does away from it, in the manner that Scheiner had suggested, in such a way that the movement at the equator really takes place in, as near as may be, 25 days, perhaps a little less; but that in latitude 30° there is a slackening off of a day and a half, so that it takes a spot in latitude 30° north or south not 25 days, but 26½ days to make its movement right round; if we go as high as latitude 45°, we have to add on still another day, and then we find that the rotation takes $27\frac{1}{2}$ days.

Several distinguished men have endeavoured to formulate a law, a mathematical statement, by which, given the movement in one latitude, we may determine it for all other latitudes, and several of them have very nearly succeeded in doing it; but they all confess that it does not amount to much at the end of the chapter; by which I mean that the formula after all contains no physical basis. It is what is termed an empirical formula.

The formulæ to which I have referred may be given in this place. In them all x = daily motion in minutes of solar longitude, and l = latitude. They are as follows:—

Carrington	$x = 865' - 165' \sin \frac{7}{4} l$
Faye	$x = 862' - 186' \sin^2 l$
Spörer	$x = 1011' - 203' \sin (41° \, 13' + l)$
Zöllner	$x = \dfrac{863' - 619' \sin^2 l}{\cos l}$

Certainly we have here, as I think I shall be able to show you by and by, one of the points in the mechanism of the sun which it behoves those interested in solar physics to work at with the utmost diligence.

We have now got the fact that the sun, like the earth, rotates on its axis; that the inclination of its axis to the plane of the ecliptic is much less than the inclination of our axis; that its node lies in a different longitude; and that the photosphere, instead of being a solid thing like the surface of our earth, is a something which makes its journey round the sun's centre in $2\frac{1}{2}$ days less time in the central portions than it does half-way between the equator and the poles.

The Sun's Density

Now then let us come to another point. We are accustomed, in dealing with the earth and comparing it with other planets, to refer to the density of the various bodies. We say, for instance, that the density of the earth is $5\frac{1}{2}$ times greater than the density of water; that is to say, that the earth put in one scale would weigh down $5\frac{1}{2}$ earths of the same size, if they were made of water, put in the other. And we say, further, that the density of the earth is about the same as the density of Venus and of Mars; but the density of the other planets is very much less. We know on the earth that water is less dense, for instance, than mercury. We know that spirit is less dense than water. We can, indeed, put water in a tumbler, and by proper means add the spirit so that it will float on the top of the water. We do not generally do that. Again, we put lead into water, and it sinks. We put a cork into water, and it floats. All these represent different orders of density. The same thing happens with regard to gases. We know that hydrogen is less dense than oxygen and nitrogen, and so on.

Now, what is the density of the sun? Is the sun denser than the earth? No; according to the books it is just about a quarter as dense as the earth, so that it is a little denser than water. In fact, if we take water as our unit of density, if water equals 1, the density of the sun is 1·444. If we take the density of the earth as 1, then the value is about 0·25—practically, a quarter.

Now, these are the values given in the books, but I think that possibly we must call them in question. They have been determined by taking the volume of the sun as given by the diameter of the photosphere—860,000 miles. Now, we have had to concede 100,000 miles for the height of one atmosphere above the photosphere, and 1,000,000 miles for another, and it is not fair that those atmospheres should be left out of consideration. If we include these atmospheres, though we do not alter the mass, we alter the volume. If we put the same mass into a bigger volume, we naturally reduce the density. Now, if we take the atmosphere of the sun as extending to 100,000 miles above the photosphere, that will give us a radius of 530,000 miles, instead of 430,000 miles, and we shall, as nearly as may be, double the sun's volume. Therefore we shall have halved the density. Instead of being a quarter as dense as the earth, it will only be one-eighth as dense; and, instead of being just denser than water, it will be a little over half the density of water. For my own part, I think that this 100,000 miles is not sufficient. I think that it is the minimum. I think that most students of solar physics would agree that a height for this purpose of 500,000 miles above the photosphere would be probably nearer the mark. That will give us exactly ten times the volume of the sun bounded by the photosphere, so that the densities will be reduced to the tenth; we shall get a density then of about one-eighth of water. This, of course, is the average density; it is the density of the whole volume in which the mass is supposed to be diffused—the mass which is a fact which we cannot get out of, and which has a definite relation to the mass of our own earth. Now if these arguments are of any value we must concede that the density of the sun is very low indeed, much lower than that of any planet or satellite with which we are acquainted; so that we are perfectly justified in saying that it is an enormous globe of gas, by which I do not mean that it is absolutely and completely gaseous to the core. The gases of the centre—gases under very great pressure—may put on the appearance, if they do not put on all the physical properties, of liquids; but be this as it may, in any region that we can get at, unfortunately limited to something like 400,000 miles away from the centre, we are undoubtedly dealing with masses of gas.

The Sun's Heat

Another point in which we find an enormous difference between the sun and any other body that we investigate in the solar system is this—that the sun is an *intensely heated* globe of gas. It is of no use to use any adverbs to tell you how hot it is, and, unfortunately, there are very few available facts; so that I must ask you to give your imagination play, and to believe that it is very, very hot. The values that have been suggested by various men of science vary between 18,000,000° and 3000°. You may take your choice. The fact is, I think, that we are not yet in a position to find out the very best method of determining the solar temperature and then marking it down in an absolutely perfect manner, for the reason that the more one knows about the problem, the more one sees how terribly complicated it is.

No doubt we have here a field of work of the very highest interest. Of course, when men of science have stated that the temperature of the sun is 18,000,000° or 3000°, they have referred to the temperature of that part of the sun which is available to our observation, and to the hottest parts of it. Naturally, if the sun be a heated globe of gas, on the outside it must be cool, so that they do not mean that this globe of gas is equally heated throughout, but that the hottest part of it—the part which sends us the effective heat which we try to measure—is at that temperature.

There is one other very interesting question connected with the remark that the atmosphere must cool to the outside. This time last century the idea was that the sun was a habitable globe just like the earth. An intense heat and light were granted to an exterior envelope, but it was imagined that there was a reflecting stratum inside which sent all the heat away earthward, and planet-ward, and star-ward with redoubled energy, while at the same time it shielded the inhabitants who were below this reflector from the direct light and heat of this envelope. That was Sir William Herschel's idea. We know now that these things cannot be so. If the walls, and ceiling, and floor of this lecture theatre were incandescent, you may depend upon it that, in spite of any number of reflectors we should soon be incandescent too. According to what is now known as Prevost's theory of exchanges, anything inside a heated chamber must, if you give it time, get to the temperature of the walls of that chamber, for the reason that the walls would be giving heat to the object inside, and the object would be sending the heat back again if it had a surplus of it, and you would get this exchange going on until the temperature of everything inside would be the same as the temperature of the envelope; so that we are now perfectly certain that, if the temperature of the photosphere of the sun, let us say, be 3000°, or 30,000°, or 3,000,000°, the temperature of the internal part of the sun will not be less. It may be much more. So that we have to give up all that beautiful idea of the habitability of the sun by creatures like ourselves.

Now, if this mass of gas, a million and a half of miles in diameter, let us say, is coolest outside, and hottest at the centre—which I think you will grant—there must be a gradual increase both of temperature and of pressure towards the centre. The observations which have been made during eclipses indicate with sufficient definiteness that there is an undoubted increase of temperature towards the centre, and that the various appearances which we get at the photospheric level really mean that at this point, where the pressure is greater than at any superior level—as the pressure in London is greater than the pressure on the top of Mont Blanc—the temperature also is higher, as is indicated by the extreme brightness of the objects seen, as compared with the dimming off of those parts of the solar atmosphere which are farther removed.

Now, can we watch this? Can we study it so that we can find out all about it? Well, not entirely. The photosphere which carries the spots to which I have referred allows us certainly to see the phenomena of the spots, but then it acts as a veil that prevents us seeing anything nearer the centre of the sun, whatever it is. It practically serves as a veil for all the underlying phenomena. Also, as I have mentioned, the outer corona is only visible for a few minutes in each generation; so that, when we attempt to watch the totality of the phenomena from the top of that magnificent radius down to that part of it which cuts the photosphere, there are difficulties of every kind supervening; we can only continuously and effectively study those regions of the atmosphere just above the photosphere, or in other words the phenomena included in the inner corona.

Absorption of the Sun's Atmosphere

But in addition to this there is something else that we can do, though this work is not so valuable, as its results are too general. We can study the general absorptive effect of the whole atmosphere above the photosphere by dealing with ordinary sunlight reflected from a cloud.

The three kinds of absorption which we recognise in spectrum analysis are these. First of all, we have a selective absorption which enables us to determine the presence of the incandescent vapour of any particular metal in the atmosphere of the sun.

Next it was pointed out in the year 1873[1] that the absorption of some elementary and compound gases is limited to the most refrangible part of the spectrum when the gases are rare, and creeps gradually into the visible violet part, and finally to the red end of the spectrum as the pressure is gradually increased. It looks very much as if all the permanent gases, or all gases and vapours at a temperature below that which enables them to give out bright lines or flutings, really possess this kind of absorption, and we know that the absorption of that kind at the sun is enormous, because the blue spectrum of the electric light is very much longer—six or seven or eight times—than the spectrum of the sun, because we get an ultra-violet radiation from the electric light which has been stopped in the atmosphere of the sun. As there are permanent gases in the sun's atmosphere the same conclusion is good for it also. If this absorption both here and at the sun were taken away, it is clear that the sunlight would be much bluer than it is at present. Prof. Langley, of the United States, who seems to be unaware of the results arrived at in 1873, has recently made the same announcement.

There is one other kind of absorption also. We have a general absorption—an absorption working equally upon all parts of the spectrum, which we may call general absorption in its true sense—such absorption, for instance, as we should get by mixing soot with water or smoking a glass and holding it in front of the sun—this would cause a considerable dimming of the light.

We can make this general examination of the atmosphere of the sun by simply observing the spectrum of sunlight reflected from a cloud; but it will be readily understood that, although in that case we shall be able to study the indications of selective absorption and the absorption of the blue end of the spectrum due to such gases as chlorine, and the general absorption of the spectrum due to the existence of solid particles; it will still be an inquiry which will only deal with the matter in its most general aspect, and we shall not be able to localise the exact regions in which these absorptions take place. Further we may say that the result of this study of the absorption of the solar atmosphere taken as a whole is chemical and statical merely. There is nothing dynamical about it. It tells us most important facts concerning the chemical constitution of the sun's atmosphere, taken as a whole, without localising the region in which any particular substance which we find to be absorbing is absorbing; but it does not tell us whether this atmosphere of the sun, which roughly we may accept as about a million of miles high, is in violent movement, or whether it is at rest.

There is, then, very much more to be done before we are fully in presence of the causes of the phenomena to which I have called attention, which stare us in the face every time we look at the sun, either when it is eclipsed, or when it is not.

J. N. LOCKYER

(*To be continued.*)

[1] *Phil. Trans.*, 1873, vol. clxiv. part 2, p. 491.

SCIENTIFIC SERIALS

THE numbers of the *Journal of Botany* for January and February contain no papers of very great importance. Messrs. H. and J. Groves record the addition of two new species to the British Characeæ: *Chara intermedia* and *Nitella capitata*, with figures of both.—Mr. J. G. Baker attempts to trace the relationship between the British and the Continental forms of the difficult genus *Rubus*.—Another addition to the British flora is recorded in *Equisetum littorale*, by Mr. W. H. Beeby.—Most of the other articles relate to descriptive or geographical botany.

THE most important paper in the *Nuovo Giornale Botanico Italiano* for January is an account by Sig. F. Morini of a new disease of cereal crops caused by the attacks of a hitherto undescribed parasitic fungus, *Sphærella exitialis*, allied to *S. graminicola* and *S. Tassiana*.—Sig. Pichi investigates the nature of the reddish-brown spots on the stem of *Bunias Erucago*, which he finds to come under the head of glandular emergences; and Sig. Cavara describes some singular anomalies and monstrosities in the flowers of *Lonicera*.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 14.—"The Coefficient of Viscosity of Air." By Herbert Tomlinson, B.A. Communicated by Prof. G. G. Stokes, P.R.S.

The author employed the torsional vibrations of cylinders and spheres, suspended vertically from a horizontal cylindrical bar, and oscillating in a sufficiently unconfined space. The bar was suspended by a rather fine wire of copper or silver attached to its centre, which, after having been previously subjected to a certain preliminary treatment with a view of reducing the internal molecular friction, was set in vibration.

The coefficient of viscosity of air was obtained from observations of the diminution of the amplitude of vibration, produced by the resistance of the air to the oscillating spheres or cylinders attached to the horizontal bar, arrangements having been made so that the vibration-period of the wire should remain the same, whether the cylinders or spheres were hanging from the bar or not. In deducing the value of the coefficient of viscosity from the logarithmic decrement, the author has availed himself of the mathematical investigations of Prof. G. G. Stokes.[1]

Five sets of experiments were made with hollow cylinders and wooden spheres, in the construction and measurement of which considerable care was taken. When the cylinders were used arrangements were made to eliminate the effect of the friction of the air on their ends. The following are the results:—

Cylinders

Length in centimetres	Diameter in centimetres	Vibration-period in seconds	Temperature of the air in degrees Centigrade	Coefficient of viscosity of the air in C.G.S. units
60·875	2·5636	6·8373	12·02	0·00018171
60·885	0·9636	7·0590	14·63	0·00018122
60·875	2·5636	3·0198	11·69	0·00018024
53·175	2·5636	2·9994	10·64	0·00017845
Spheres				
	6·364	2·8801	9·35	0·00017820

Maxwell has proved[2] that the coefficient of viscosity of air is independent of the pressure and directly proportional to the absolute temperature. We can, therefore, calculate from the above data what would be the value of the coefficient of viscosity at 0° C.; and when this is done, in the case of each of the five sets of experiments, we obtain the following values:—

Set of experiments	Coefficient of viscosity of air at 0° C.
1st	0·00017404
2nd	0·00017201
3rd	0·00017284
4th	0·00017359
5th	0·00017230

The mean of these numbers is 0·00017296 with a probable

[1] See Prof. Stokes's paper "On the Effect of the Internal Friction of Fluids on the Motion of Pendulums," *Trans.* Camb. Phil. Soc., vol. ix. Part II., 1850.

[2] *Phil. Trans.*, 1866, vol. clvi. Part I.

error of only 0·14 per cent. The formula for finding μ_t, the coefficient of viscosity of air at the temperature $t°$ C., is therefore—

$$\mu_t = 0·00017296 \left(1 + \frac{t}{273}\right).$$

The value of the coefficient of viscosity of air at 0° C., given above, though much nearer to that obtained by Maxwell than any which has been got by other observers, nevertheless differs from it by more than 8 per cent.[1]

January 28.—"On Local Magnetic Disturbance in Islands situated far from a Continent." By Staff-Commander E. W. Creak, R.N., F.R.S.

It has long been known that local magnetic disturbance has been found to exist to a considerable extent at St. Helena, Bermuda, and other islands. Observers in the islands adjacent to the west coast of Scotland have also found local disturbance existing in them, and, in order to determine its amount, have obtained normal values from curves of the magnetic elements calculated from neighbouring regions where observations have been made apparently free from magnetic disturbance.

In the case of islands situated far from a continent, however, normal values of the three magnetic elements may be obtained by the method of turning the ship in azimuth and observing on eight or more equidistant points of the compass in the process called "swinging," whereby the effects of the horizontal disturbing forces proceeding from the iron of the ship may be eliminated, and by occasional observations at well-selected land stations known to be free from local magnetic disturbance, the values of the vertical magnetic forces caused by iron in the ship may also be ascertained for all latitudes, and the necessary corrections applied to the observations.

A series of magnetic observations have been made on land in Bermuda, Madeira, Teneriffe, St. Vincent (Cape de Verde), St. Paul Rocks, and Sandwich Islands in the northern hemisphere and Tristan d'Acunha, Ascension, St. Helena, Kerguelen Island, New Zealand, and Juan Fernandez, in the southern hemisphere. The observations made in these several islands have been collected for this paper and compared with the normal values as observed at sea in their neighbourhood.

Throughout the discussion the term "blue" magnetism has been adopted to indicate that kind of magnetism which attracts the marked or north-seeking end of the needle, and "red" for that which repels it.

At Bermuda the most extensive series of observations has been made, and a strong focus of blue magnetism found to exist between Mount Langton and the lighthouse on Gibbs Hill.

The position of this focus was approximately defined by drawing, on a map of the western portion of Bermuda, lines of equal values of the disturbance from the normal for each element, and it was found that at one position eastward of this focus the westerly declination was increased 2° 39′, and at another, westward of it, diminished by 3° 5′. The disturbance of the inclination and vertical force gradually increases as the focus is approached, amounting in the inclination from +0° 11′ to 1° 47′, and in the vertical force from zero to +·314 (British units).

Bermuda may be taken as an example of the results generally found in the other islands under discussion, for the observations show that, north of the magnetic equator, the north point of the compass is invariably attracted inland towards some part of the island, and south of the magnetic equator it is repelled, showing marked divergence of results between observations made on the east and west coasts. The inclination and vertical force are, with rare exceptions, increased in the islands on both sides of the magnetic equator.

On the whole the local disturbances are not very large, but it may be remarked that they render the comparison of observations at different epochs very doubtful in value, unless the precise position of observation be rigidly adhered to.

Before dismissing the question of the actual observation, the results obtained at the Bluff, Bluff Harbour, New Zealand, are worthy of note—

		°	′	
Declination observed	On the summit of the Bluff ...	6	54	E.
	30 feet north ,, ...	9	36	W.
	,, west ,, ...	5	4	E.
	,, east ,, ...	46	44	E.
Normal from sea observations		16	20	E.

[1] Prof. Stokes, in a note at the end of the paper, has shown that a very small deviation from horizontality of the movable disks used by Maxwell would make the value of the coefficient obtained by him 8 per cent. too great.

On the summit of the Bluff there was thus shown to be a strong focus of red magnetism.

The general results tend to show that the magnetic disturbance in islands north of the magnetic equator is due to an excess of blue magnetism, and in those south of it to an excess of red magnetism compared with that due to the respective positions of the islands on the earth considered as a magnet.

In Sir G. Airy's treatise on magnetism, reasons have been given for believing that the magnetism of the earth is not due to sources external to it, nor specially existing on its surface, but that the source of its magnetism lies deep.

With these reasons in view, and the results obtained from the observations discussed, the possible conclusion is drawn that the excess of "blue" and "red" magnetism observed in the islands above-mentioned proceeds from portions of those islands which have been raised to the earth's surface from the magnetised part of the earth, forming the source of its magnetism.

For the numerical data upon which the preceding remarks have been founded, and descriptive map of the Bermuda magnetic disturbances, reference should be made to the original paper.

Linnean Society, February 4.—Sir John Lubbock, Bart., President, in the chair.—Mr. J. Dallas, of the Exeter Museum, exhibited a specimen of the somewhat rare glossy ibis (*Plegadis falcinellus*, L.), obtained from Mr. J. H. Clyde, of Bradsworthy Vicarage, Holsworthy, Devon, in whose possession it had been since killed in that neighbourhood, September 1851.—Mr. F. J. Hanbury showed a series of forms of the genera Hieracium and Carex gathered by him in Caithness and Sutherlandshire, all new to Britain, but representative of the Scandinavian flora.—Mr. C. Bartlett showed a remarkable African (?) caterpillar, 7 inches long, of a steel-gray colour, and abundantly hairy and spiny.—Mr. W. H. Beeby drew attention to an example of *Equisetum littorale* got by him on Bisley Common, Surrey, and hitherto not known as an English plant.—Mr. J. C. Sawer exhibited a sample of a superior sort of the essential oil of lavender and a spike of the plant, a cross-breed of varieties introduced by him from the Continent, and grown at Brighton.—Mr. A. Hammond showed a microscopic section of the integument of the larva of a dipterous insect (*Stratiomys chamæleon*), raising the question as to whether the polygonal areas of the cuticle, described by M. Villianes, were surface-markings only, or, as he held, cellular in character.—Mr. F. Darwin read a paper on the relation between the bloom of leaves and the distribution of the stomata. "Bloom" on leaves is used by him to mean a coating of minute particles of a waxy character, which is removable by hot water or ether. But gradations occur from a distinct and appreciable greasiness throwing off moisture to such as are easily wetted. A large series of leaves of different groups of plants have been studied by him, and for convenience in the analysis of data he has divided them into four classes. Leaves of Class I. are devoid of bloom on both surfaces, and yield 54 per cent. which have *no* stomata on the upper surface. In Class II. bloom is deficient above but present below, whereof 83 per cent. contain stomata on the leaves' lower surface. Class III. possesses bloom on the leaves above, but none inferiorly, and 100 per cent. of these have stomata on the upper surface. Class IV. have leaves with bloom on both surfaces, 62 per cent. of them having stomata above. From such analyses and other facts and data given, Mr. Darwin concludes that the accumulation of stomata accompanies that of bloom, and, other things being equal, that it is functionally protective against undue wetting by rain, and thus injury to the leaf-tissue.—In a communication by Mr. E. C. Bousfield, on the Annelids Slavina and Ophidonais, he criticises Herr Vejdovsky's new genus *Slavina*, and objects to his identification of *Nais appendiculata* and *N. lurida*, while giving a full description of the latter, and observing points of contrast. He also describes touch organs in Ophidonais, similar to those of Clavina, mentioning other points of similarity between the two. He further proposes to do away with the former genus, including its only species under Slavina.—Brigade-Surgeon E. Bonavio, in a paper afterwards read, asserts that the wild *Citrus hystrix*, D.C., is the grandparent of *Lima tuberosus*, *L. agrestris*, *Limonis feri*, *Limonellus aurarius*, and others, while also more distantly the grandparent of the cultivated true limes of India, Ceylon, &c. The reason why the lime has so persistently a winged petiole, according to him, is that this is derivative from the immense winged petiole of its progenitor *Citrus hystrix*.—Prof. Richard J. Anderson communicated a paper on the relative lengths of the segments of limbs in the chick during development between the sixth and twentieth days. On or even before the ninth day, the bones of the fore-arm and manus are longer than the corresponding segments of the leg and foot. Afterwards the tarso-metatarsus begins to lengthen, and maintains a greater relative size at the end of incubation.

Zoological Society, February 16.—Dr. St. George Mivart, F.R.S., Vice-President, in the chair.—Mr. Sclater exhibited a specimen of the new Paradise-bird, *Paradisornis rudolphi* of Finsch and Meyer, lately discovered by Mr. Hunstein in the Owen Stanley Mountains of New Guinea, and pointed out the characters in which it differs from typical *Paradisea.*—The Secretary exhibited, on behalf of Mr. Taczanowski, C.M.Z.S., the skin of an owl from the south-east of the Ussuri country, on the frontiers of Corea, which appeared to be referable to *Bubo blakistoni* of Seebohm.—Mr. E. Gerrard, Jun., exhibited heads and skulls of two African rhinoceroses (*R. bicornis* and *R. simus*), obtained by Mr. Selous in Mashuna-land.—Prof. Ray Lankester exhibited and made remarks on a drawing of a restoration of *Archæopteryx.*—Mr. Oldfield Thomas gave an account of a striking instance of cranial variation due to age, as shown in two specimens of the skull of the Canadian marten (*Mustela pennanti*), which presented extreme differences in the breadth of the zygomata, in the contraction of the interorbital space, and in the development of the occipital crest. Special stress was laid on the fact that such changes as these take place after the animal has attained maturity.—Mr. W. L. Sclater exhibited and described a new Madreporarian coral, which he proposed to call *Stephanotrochus moseleyanus.* The coral had been dredged in the Faroe Channel during the cruise of H.M.S. *Triton* in the summer of 1882. Some account of its anatomy and histology was also given.

Chemical Society, February 4.—Dr. Hugo Müller, F.R.S., President, in the chair.—The following note was read:—The chemical formula for wool keratine, by Edmund J. Mills, F.R.S.—A lecture was delivered on methods of bacteriological research from a biologist's point of view, by Dr. Klein, F.R.S. The object of the lecture was to bring before the Chemical Society the methods used at present by pathologists in the investigations of micro-organisms associated with disease. These methods, thanks to the investigations of Koch, are greatly in advance of those hitherto employed by chemists in the investigation of the activity of bacteria. The enormous amount of work that has been done by chemists since the memorable investigations of Pasteur on fermentation and putrefaction, if viewed in the light of modern bacteriological methods, is in a great measure unsatisfactory and imperfect, more so than will be conceded by chemists. This unsatisfactory state is chiefly due to the imperfect methods employed. Specific chemical action is ascribed to certain organisms, because these were found present in the substances examined, no regard being paid as to whether these organisms were alone active or whether they were only concomitant and dependent on the activity of others. Numbers of instances can be adduced to prove this: amongst them may be mentioned the assertions that alcoholic fermentation is produced by *Mucor racemosus*, and also by a bacillus besides saccharomyces; that the ammoniacal fermentation of urine is due to a bacillus; that the lactic acid fermentation is due to a micrococcus and also to a bacillus. To determine whether a definite chemical process is produced by a definite organism, and which, it is necessary to prove (1) that the substances to be acted upon are at the outset free of any accidental organisms; (2) that the particular organism to which the definite chemical activity is ascribed is the only one concerned in this process. The methods used must fulfil these elementary conditions, that is to say: (1) the materials used must be sterile at the outset, and protected from accidental contamination; (2) the specific organism must be obtained in pure cultivations, and this purified organism must be capable of producing the specific chemical change. Viewed in this light, few of the assertions hitherto made bear criticism. As one of the most striking instances it may be mentioned that, notwithstanding the enormous amount of knowledge gained by chemical research into the changes of proteid bodies during putrefaction, there is no reliable answer yet given to the questions—Which organism or organisms are concerned in this complex process? Which part of the process is due to which organism? Is the analytical process by which proteids are carried down to relatively simple nitrogenous principles done by one or more organisms and by which? Is the production of the alkaloids known as ptomaines due to the same organism or organisms which started the process of putrefaction? Another equally important series of investigations refers to the process of nitrification; here also no definite answer can be given. So also the chemical changes due to the growth of moulds are waiting for investigation. When chemical research begins to adopt such methods as are employed by pathologists, but not till then, its results will be unequivocal. The methods used for sterilising materials, for studying and recognising the morphological characters of organisms, for obtaining pure cultivations, and for inoculating nutritive materials with them were then minutely described.

Physical Society, February 13.—Annual General Meeting. Prof. F. Guthrie, President, in the chair.—Prof. T. H. Huxley and Mr. A. E. Mills were elected Members of the Society.—The President read the report of the Council. The Treasurer, Dr. E. Atkinson, presented his report, which was adopted. The meeting resolved that votes of thanks be accorded to the Committee of the Council of Education, the President and Officers of the Society, and the Auditors of the Society's accounts. The meeting then proceeded to elect officers for the forthcoming year, and a ballot having been taken, the following were declared elected:—President: Dr. Balfour Stewart, F.R.S.; Vice-Presidents: Dr. J. H. Gladstone, F.R.S., Prof. G. C. Foster, F.R.S., Prof. W. G. Adams, F.R.S., Sir W. Thomson, F.R.S., Prof. R. B. Clifton, F.R.S., Prof. F. Guthrie, F.R.S. (the above have filled the office of President), Prof. W. E. Ayrton, F.R.S., Shelford Bidwell, M.A., Prof. H. McLeod, F.R.S., Prof. W. Chandler Roberts-Austen, F.R.S.; Secretaries: Prof. A. W. Reinold, F.R.S., and Walter Baily, M.A.; Treasurer: Dr. E. Atkinson; Demonstrator: C. Vernon Boys; other Members of Council: Conrad W. Cooke, Prof. G. Forbes, F.R.S.E., Prof. F. Fuller, R. T. Glazebrook, F.R.S., Dr. J. Hopkinson, F.R.S., Prof. J. Perry, F.R.S., Prof. J. H. Poynting, Prof. A. W. Rücker, F.R.S., Prof. S. P. Thompson, Dr. C. R. Alder Wright, F.R.S.—Prof. Guthrie, in resigning the position of President, thanked his colleagues for the help they had afforded him since he became President of the Society in 1884; he also congratulated the Society upon the highly satisfactory state to which it had attained.—The meeting then resolved itself into an ordinary meeting. In the absence of the President, Prof. Balfour Stewart, the chair was occupied by Prof. G. C. Foster.—The following communications were read:—On experimental error in calorimetrical work, and on delicate calorimetrical thermometers, by Prof. U. S. Pickering. In conducting a great number of determinations of the heat of dissolution of a solid body in water, the author has had an opportunity of detecting the sources of error incident on such work, and by an examination of the results has not only obtained the mean error of a series of observations, but has been able to apportionate this error to its various causes. In the experimental work it was found that the presence of anything but air between the calorimeter and jacket was most injurious; the space should be entirely open, and no cover of any sort should be used. Before reading the thermometers, as pointed out by Berthelot, the top of the stem should be tapped for some time, otherwise the mercury lags behind the true temperature; but besides this thermometric error, which the author calls the "temporary error," is another effect which may be termed the permanent error, of a similar kind, which no amount of tapping will remove. He has found and verified by special experiments that a thermometer when rising is invariably too low, while when falling it is invariably too high. Error due to this, which varies in amount with different instruments, is avoided by conducting the whole experiment with a rising or with a falling thermometer. The thermometers employed in these experiments had a range of 15° C., and a total length of 600 mm. The experiments were performed at temperatures varying from $-1°$ to 26° C., and as it was important that the same thermometer should be used in different experiments, and even advisable to use the same part of the scale of the thermometer, the following expedient was devised: The thermometer was first heated to the highest temperature required in the experiment, and, by the application of a flame to the mercurial column just below the enlarged space at the end of the tube, that part of the mercury above the flame was broken off and driven into the space, where it remained when the thermometer was cooled. By this means the relative value of a scale division was only inappreciably affected, while the absolute value could be obtained from a single comparison with a standard. From an examination of the results obtained, the author concludes that for further accuracy in this kind of work we must look for improvements in the methods employed, the instruments having, he believes, attained to a state as near perfection as possible.—On some new forms of calorimeters, by Prof. W. F. Barrett. These instruments were constructed for the accurate and ready determinations of specific heats, notably those of liquids. In the first form the bulb of a thermometer is blown into the form of a cup of about 4 cubic centimetres capacity, which thus acts as a calorimeter. Into this cup the liquid is dropped directly from a burette, its temperature being observed by a thermometer in the burette, the mouth of which is closed by the end of the bulb of the thermometer, which is ground, and thus acts the part of a stopper, so that, on raising the thermometer, the liquid flows from the burette into the cup. The thermometer itself forms a balance, the horizontal stem acting as the beam is supported by a knife-edge, and a pan is attached to the further end by the addition of weights to which the weight of liquid added can be ascertained. In the second form a simple thermometer with a large bulb is used, the latter dipping into a silver vessel, into which the liquid is introduced as before.—Prof. S. P. Thompson exhibited a glass calorimeter, similar in construction to that of Favre and Silbermann ; water is used instead of mercury, the great density of which renders it unsuitable for use in so large a glass vessel.

Anthropological Institute, February 9.—Mr. Francis Galton, F.R.S., President, in the chair.—The election of Prof. Otio T. Mason, Prof. J. Ranke, Dr. G. Manouvrier, and Prof. J. Kollmann as Honorary Members, and of the Rev. W. Birks, J. G. Blumer, F. H. Collins, J. Spielman, and T. L. Wall, as Ordinary Members, was announced.—The President read a paper on recent designs for anthropometric instruments, and called particular attention to a number of instruments made by the Cambridge Scientific Instrument Company, and exhibited by Mr. Horace Darwin, who afterwards described them and showed the manner in which they are used.—M. Collin, of Paris, exhibited a traveller's box of anthropometric instruments and Topinard's craniophore.—Prof. A. Macalister read a paper on a skull from an ancient burying-ground in Kamchatka ; and Dr. G. Garson read a paper on the cephalic index, in which he proposed a system of nomenclature for international adoption which has already been accepted in principle by several of the leading anthropologists on the Continent.

Royal Meteorological Society, February 17.—Mr. W. Ellis, F.R.A.S., President, in the chair.—Mr. G. Buchanan, Capt. G. H. Leggett, Dr. H. C. Taylor, J.P., and Mr. J. Tolson were elected Fellows of the Society.—The following papers were read :—General remarks on the naming of clouds, by Capt. H. Toynbee, F.R.Met.Soc. The author considers it important to keep to Luke Howard's nomenclature, leaving it to the observers to express by an additional word any peculiarity they notice in a particular cloud.—On the thickness of shower-clouds, by Mr. A. W. Clayden, M.A., F.G.S. From some measurements made by the author during the summer of 1885 he has come to the conclusion that clouds of less than 2000 feet in thickness are not often accompanied by rain ; and, if they are, it is only very gentle, consisting of minute drops. With a thickness of between 2000 and 4000 feet the size of the drops is moderate. As the thickness gets greater, the size of the drops increases, and at the same time their temperature becomes lower, until, when the thickness is upwards of 6000 feet, hail is produced.—On the formation of rain, hail, and snow, by Mr. A. W. Clayden, M.A., F.G.S. The author points out that all observations tend to show that, except under quite abnormal conditions, the temperature of the atmosphere falls as the height above sea-level increases ; and there seems no reason whatever for assuming that the law does not apply to that portion of the atmosphere which forms a cloud. Hence, if a drop were to be formed at or near the upper surface of a cloud, it would fall down into a region saturated with vapour at a temperature above its own. The result will be further condensation, producing a larger drop ; and this process will continue until it leaves the cloud. If its temperature is below the dew-point of the air it falls through, condensation will continue until it reaches the ground. However, it is obvious that this subsequent gain cannot bear any very large proportion to the growth while falling through the saturated cloud, from which the conclusion follows that the size of the drop must increase with the thickness of the cloud. The author suggests that condensation begins on the upper surface of the cloud by the cooling of some of the liquid cloud-particles. If this particle is cold enough it will solidify, and snow will be formed. Should it not be quite cold enough to solidify at once, owing to its minuteness, but remain still below the freezing-point, hail is formed. Finally, if the temperature is not low enough for either snow or hail, rain is produced.—On three years' work by the "chrono-barometer" and "chrono-thermometer," 1882–84, by Mr. W. F. Stanley, F.R.Met.Soc. The chrono-barometer is a clock that counts the oscillations of a pendulum formed by a suspended barometer. The upper chamber of the pendulum is a cylinder of an inch or more in diameter. By change of atmospheric pressure the mercury in the pendulum is displaced from the bottom to the top, and *vice versâ*. The rate of the clock is accelerated or retarded in proportion to the displacemen tof the mercury. The chrono-thermometer is a similar clock to the above, and the pendulum is also a barometer ; but instead of the lower chamber being exposed to pressure, the whole tube is inclosed in a second hermetically-sealed tube containing air. Atmospheric pressure being thus removed, the expansion of the included air by heat alone forces the mercury up into the vacuum-chamber, and alters the period of oscillation of the pendulum.

Victoria (Philosophical) Institute, February 15.—A paper on final cause, by Prof. Dabney, of Texas University, was read.

EDINBURGH

Mathematical Society, February 12.—Dr. R. M. Ferguson, President, in the chair.—Mr. William Harvey communicated several theorems in kinematics with geometrical demonstrations ; and Mr. R. E. Allardice submitted a proof, by Mr. T. Hugh Miller, of Lagrange's theorem.

PARIS

Academy of Sciences, February 15.—M. Jurien de la Gravière, President, in the chair.—Discourses pronounced at the obsequies of M. Jamin, by M. J. Bertrand on behalf of the Academy, and by M. L. Troost in the name of the Faculty of Sciences.—Remarks on the 172 tornadoes recorded in the United States during the year 1884, by M. Faye. From the scientific point of view the author considers that it seems definitely established that there is a definite portion of an area of low pressure within which the conditions for the development of tornadoes is most favourable. The special tornado reporters for the Signal Service are now endeavouring still more accurately to determine this "dangerous octant," as it is called in America. February 19, 1884, is mentioned as memorable in the history of these destructive phenomena. On that day no less than forty-five were recorded in the South-Eastern States, attended with a total loss of 800 lives, 2500 injured, 10,000 houses and buildings destroyed, and from 10,000 to 15,000 people left homeless.—Note on a prophylactic means of protecting the vine by destroying the winter egg of Phylloxera, by M. P. de Lafitte. This plan has now been tried with considerable success during the last three years at three different places in the department of Lot-et-Garonne. The State aid granted for the purpose having long been exhausted, growers have been encouraged by these results to continue the experiments at their own expense.—On the periods of the double integrals, by M. E. Picard.—On the theory of reciprocants, by M. R. Perrin. It is shown that the new forms introduced by Prof. Sylvester into mathematical analysis may be considerably simplified by the employment of a few general theorems here communicated to the Academy.—Note on the polhodie and terpolhodie (continued), by M. A. Mannheim.—Spectroscopic observations on the new star discovered by Mr. Gore in Orion, made at the Nice Observatory by MM. Perrotin and Thollon. This star presents a fine line spectrum stretching far into the violet, the red and especially the green being remarkably brilliant, while the yellow appears relatively dull. This suggested a certain analogy with the spectra of comets, only much more complicated, and the idea was confirmed by subsequent comparative observations made on α Orionis, which shows a characteristic continuous spectrum intersected by dark bands and lines. Notwithstanding the faint yellow bands, the new star would therefore appear to be of the same type as α Orionis.—Note on the deviation of the equipotential lines, and the variation of resistance shown by bismuth in a magnetic field, by M. Leduc.—On the electrolysis of the salts : influence of temperature, of the distance and surface of the electrodes, by M. Adolphe Renard.—Observations in connection with M. A. Millot's note on the "Products of Oxidation of Carbon by the Electrolysis of a Solution of Ammonia," by

MM. A. Bartoli and G. Papasogli. To M. Millot's statement that he failed to find mellic acid and its derivatives in the electrolysed ammoniacal solution, it is pointed out that the failure was doubtless due to the fact that his experiments were not conducted under the same conditions as those of the authors. —Note on a combination of acetic ether and chloride of magnesium, by M. J. Allain le Canu.—On the influence of the acid oxalate of ammonia on the solubility of neutral oxalate, by M. R. Engel.—On the γ-bromo and iodobutyric acids, $XCH_2—(CH_2)_2—CO(OH)$, by M. Louis Henry.—Note on the affinities of the Eocene floras of the West of France with those of North America, by M. Louis Crié. The attention of geologists and botanists is here directed to certain fossil plants occurring in the Eocene sandstones of West France, which present evident affinities to several species of the lignitic group described and figured by M. Leo Lesquereux in his "Contributions to the Fossil Flora of the Western Territories" (Washington, 1878). *Pteris Fyeensis*, Crié, *Lygodium Fyeense*, Crié, *Lygodium Kaulfussii*, Heer, *Asplenium Cenomanense*, Crié, and others are compared respectively with *Pteris pseudopennæformis*, Lesq., *Lygodium Dentoni*, Lesq., *Lygodium neuropteroides*, Lesq., *Gymnogramma Haydenii*, Lesq., &c.—Note on the subject of atmospheric disturbances—M. Faye's theory of whirlwinds, by M. Jean Luvini. It is shown that the slight convergence of the current towards the centre of great cyclones, as appears determined by observation, would be more opposed to the theory of absorption than to M. Faye's gyratory theory. A slight convergence near the ground is in fact a natural consequence of the principles regulating the movement of fluids.

Berlin

Physiological Society, December 11, 1885.—Dr. Gad spoke of an apparatus executed by him and set up in the demonstrating-room, designed to show the play of the valves of the heart. A short canula of 7 cm. in diameter was tightly fixed into the left auricle of a large bullock's heart, and the free end was closed in a water-tight manner by a plate of looking-glass. At the side of the canula was a short tube connected by an elastic tube with an upright bottle. A similar canula of 3 cm. in diameter was fastened into the aorta close over the semi-lunar valves, and its lateral tube conducted by an elastic pipe into a funnel through which the water flowing from the ventricle reached the bottle. A third canula was fastened into the apex of the heart, and connected with a thick-walled elastic ball, by the compressions and elastic expansions of which the vigorous operations of pressure and expansion required for the circulation of the water filling the apparatus were achieved. In the ventricle was placed a small Edison lamp, the conducting wires to which were, by means of a water-tight tube at the side of the third canula, directed outwards. When the elastic ball was rhythmically compressed, then the alternating play of the cardiac valves was seen through the two first canulæ, and, by means of a suitable mirror before the canulæ, might be exhibited to a large class.—Dr. Goldscheider reported on the results of an investigation into the nerve-endings at the pressure and temperature points, the existence of which he had demonstrated. In the expectation that specific terminal organs of the cutaneous nerves must, if they existed, be met with at the pressure points and points of cold and warmth, Dr. Goldscheider had cut out of his forearm, at the isolated pressure points and temperature points, small wedges of skin, and prepared them with arsenic acid and auric chloride, embedding in paraffin. Of the preparations he made a series of sections which in most cases showed longitudinal sections through the cutaneous nerves. The microscopical examination revealed that no Paccinian or Meissner corpuscles were situated either at the pressure or at the temperature points. On the contrary, the speaker found regularly at the pressure points, which he had previously marked by the prick of a needle, a bundle of medullated nerve-threads approaching close to the boundary of the corium. At this point the bundle split into two branches proceeding in opposite directions, and then further ramifying. These two divisions made their way mostly between the corium and epidermis, and but seldom penetrated as far as the second layer of the epidermis cells. So far as ends of the nerves were visible, they were situated between the cells and were pointed. On the temperature points a bundle of nerve-fibres were likewise seen to rise, but in this latter case they ended in a pretty narrow net of very fine, non-medullated threads, and never reached the epidermis. In the neighbourhood of the nets of the temperature nerves blood-capillaries were regularly met with. Dr. Goldscheider was of opinion that the cutaneous nerves possessed no specific terminal organs, but simply merged into narrower or wider nets, and that the sensitive points for pressure and temperature were situated at the spot where the terminal division of the nerve-bundles occurred.—Dr. Benda supplemented the address he delivered at the last meeting on spermatogenesis by hypothetical considerations regarding the significance of the microscopical figures found by him.

January 15.—Dr. Müllenhoff spoke of his observations respecting the structure of bee-cells. Producing specimens of combs and models, he handled the geometrical figure of the cells, the fact of which had been recognised so far back as the time of the Greek philosopher Pappus, and the measure of which had been taken by Réaumur, the cells forming a hexagonal column bounded on the side turned to the partition wall by a trilateral pyramid, on the other by a plain terminal surface. To account for the great regularity of the cells, Buffon had propounded that they originated in the mutual pressure of the wax-vesicles, and put this explanation to the proof by an experiment in which he filled up a vessel with peas, and stuffed the interstices with water, which caused the peas to swell. In point of fact the round bodies got thereby converted into precisely geometrical figures with trilateral terminal surfaces. They were, however, no hexagonal columns, but regular rhombendodecahedrons. Dr. Müllenhoff had now, by a long series of observations in beehives, studied the structure of the bee-cells, and had established that the bees, which, as was known, worked closely compacted together, first stuck a little thick wax disk to the wall, and then gnawed away at it till the plate had grown so thin that under the all-sided pressure, in accordance with the law respecting equilibrium figures of fluid membranes discovered by Plateau, they assumed the form of a half rhombendodecahedron with trilaterally pyramidal surfaces. The bees then proceeded to build on the six free edges by attaching to them small wax plates, and gnawing away at them till they had grown so thin that under the pressure of the neighbouring cells they took on the form of a hexagonal column. The column was made so long that the queen bee, in laying her eggs, rested with her posterior body on the floor of the cell, and, with her anterior legs, was able to take hold of the free edge of the column. The geometrically-regular figure of the bee-cells was accordingly conditioned by physical laws, and not by any knowledge inherent in the bees of geometrical laws in respect of the greatest economy of space and material. That without the co-operation of the Plateau laws the bees were able to achieve no regular cells was demonstrated by the queen-cells, which, constructed isolatedly, had the irregular form of a thimble.—Prof. du Bois-Reymond gave a short summary view of the investigations he had carried out in the past summer into living torpedoes, by means of which he had pretty well solved all the problems which were at all capable of being submitted to experimental test on the animals of the aquaria, which were very much reduced in strength and exhausted by inanition. Having first ascertained the direction of the cuticular current, he examined the polarisation-phenomena yielded by stripes of the electrical organ under the influence of foreign currents. He learned that homodromous currents, *i.e.* such as were directed in the same way as the direction of the shock, gave always a homodromous polarisation, while heterodromous currents never produced heterodromous polarisation, and only occasionally homodromous polarisation. This fact was capable of explanation by assuming that what appeared as homodromous polarisation was but a shock of the fish caused by the foreign current, a shock which of course could only be homodromous; or that the electrically polar molecules directed by the foreign polarising current were capable of being turned only in one direction. A decision between these two explanations could not be arrived at. Prof. du Bois-Reymond next examined the conductivity of the electrical organ, and ascertained that it conducted homodromous currents almost as well as did the muscle, but that it conducted heterodromous currents much worse, so that the electrical organ was almost half an insulator for heterodromous currents. The conduction power of the electrical organ of the torpedo was consequently irreciprocal. This irreciprocity of conduction obtained only for strong currents and for those of short duration. It was met with, moreover, only in the living organ. The defunct organ conducted considerably better than the living, and was equally good for the conduction of homodromous and heterodromous currents. The irreciprocity, finally, increased with the length of the organ stripe. This irreciprocity of conduction explained in a most highly interesting manner the powerful effect of the strokes directed outwards of electrical fish. Let us suppose a column of the electrical organ reaching from the back to the belly, then would the electrical currents of the organ diffuse themselves at the positive pole surface of the back, and in accordance with well-known laws respecting the distribution of electrical currents in an endless conductor, betake themselves to the negative pole surface of the belly. Were the organ a good conductor of its currents, then would the most intense threads of currents balance themselves in the organ, and only very faint ramifications of current penetrate into the water. These currents, however, had a heterodromous direction in the organ, and were therefore ill-conducted. The most intense threads of current were forced therefore to penetrate into the water, and were accordingly able to produce vigorous effects outwardly. The speaker had finally examined a phenomenon in the powerful electric nerves of the torpedo, which he had earlier had occasion to observe in other nerves. If a piece of nerve were cut off and the electromotory energy of the two transverse sections determined, then did the electric nerves show that the peripherical cross-section acted in an electromotory sense more powerfully than did the central. If both cross-sections were derived, then was an ascending current received in consequence thereof. This occurred with such regularity that the peripheral and the central nerve could be recognised on any piece whatsoever by the direction of the axial nerve current, which was opposed to the direction of the physiological action. In the sensory nerves Prof. du Bois-Reymond had found a reverse axial current directed from the centre to the periphery. He had the phenomenon then further investigated by Dr. Mendelssohn, and it was quite generally established that centripetal active nerves, such as the nerves of the senses and the posterior roots of the spinal nerves, always showed a descending axial nerve current, whereas centrifugal active nerves, such as the motory and the electric nerves, possessed an ascending axial nerve current. In the case of mixed nerves an axial nerve current could not be decidedly demonstrated.

Meteorological Society, January 12.—Dr. Hellmann laid before the Society in the form of a table, the results of the rain registration at the eleven stations to the west of Berlin for the six months from July to December, and drew attention to the fact that in the winter months the values yielded by the different rain-gauges coincided very closely, whereas in summer differences reaching as much as 50 per cent. occurred.—Herr Opel spoke of the quantities of water discharged by rivers, and in particular by the Elbe. In view of the great difference prevailing in the registrations of the amounts discharged at high water, it deserved to be noted as an indication of important progress that Herr Sasse, on the basis of a careful special investigation of the subject, had formulated the proposition that the curve of the quantities of water discharged formed a parabola to the high-water marks as abscissæ, but that the zero-point of the parabola lay deeper than the zero-point of the water-mark. From a long series of examples the author demonstrated the correctness of the formula, and directed attention to several singularities in the quantities of water discharged by the Elbe at various stations of its course through Germany, singularities which, while in part explicable by the tributaries, demanded further investigation. The speaker then discussed the question of the volumes of water in rivers, on which in quite recent times several scientific investigators had expressed an opinion to the effect that they had diminished in comparison with the volumes of water in the rivers last century. This diminution of volume was in large part attributed to the progress of the denudation of forests in the river districts. Herr Opel was, however, of opinion that these registrations of the rivers were rather related to the present well-ascertained lower state of the rivers at low water. Since at many places the beds of the rivers had, altogether irrespective of their profile, been enormously narrowed, the rivers at high water had, in consequence, dug themselves out a deeper channel, and in this way depressed the mass of waters. Rain returns did not, at all events, testify to any diminution in recent times in the supply of water. The observations on the amounts discharged by the rivers of Prussia have hitherto rested on very unsatisfactory bases. At a number of stations daily observations of water-marks were made. The average of these was then taken, and the monthly and yearly averages of these water-marks were used as a basis for the calculation of the monthly and yearly discharges. Seeing, however, that the amount of water discharged represented a parabola, it was impossible to calculate it from the height of the state of the water alone. The amounts of water corresponding with the average water-marks deviated, as was shown in a number of instances, very considerably from the average of the water volumes corresponding with the several high-water marks. Another source of error lay in the circumstance that the observations of water-marks were made only once a day, from which observations the monthly and yearly averages were deduced. In view, however, of the repeated and often important variations in the states of the water, once-a-day observations were really of little value. Hourly observations even would not suffice. What is required are self-registering gauges of the states of the water, as being the only means whereby to obtain trustworthy values for the amount of the river-discharges. Over and above this, in the case of the larger rivers, measurements of their respective quantities at low, mean, and high water should, every few years, be very carefully made and the parabola determined, from which the quantities discharged could then be calculated from the registered high-water marks with some degree of certainty.

BOOKS AND PAMPHLETS RECEIVED

"Class-Book of Geology": Dr. A. Geikie (Macmillan and Co.).—"Trigonometry for Beginners": Rev. J. B. Lock (Macmillan and Co.).—"Fourth Annual Report of the Board of Control of the New York Agricultural Experiment Station for the Year 1885" (Andrews, Rochester).—"The Co-operative Index to Periodicals," vol. i. No. 4 (New York).—"Hints for Land Transfer and a State Land-Bank": Nemo.—"Revista di Artiglieria e Genio," vol. i. (Roma).—"The Star-Guide": Latimer Clark and Herbert Sadler (Macmillan and Co.(.—"The Artist's Manual of Pigments": H. C. Standage (Lockwood and Co.).—"Observaciones Magnéticas y Meteorológicas del Real Colegio de Belen de la Compania de Jesus," January to March, April to June, 1885 (El Iris, Habana).—"Scientific Results of the Second Yarkand Mission—Aranelida": Rev. O. P. Cambridge (Government Printing-Office, Calcutta).—"The Comparative Anatomy of the Pyramid Tract": E. C. Spitzka (Jenkins, New York).—"Science for Nobleness, for Knowledge, and for Use": Sir H. W. Acland (K. Paul and Co.).

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