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THE NORWEGIAN NORTH ATLANTIC EXPEDITION—CRUSTACEA

The Norwegian North Atlantic Expedition, 1876-78. *Zoology*. XIV. *Crustacea*, IA. and IB. By G. O. Sars. 4to, pp. 280, with 21 Plates and a Map. (Christiania, 1885; London: Sampson Low and Co.)

NO better evidence can be adduced of the growing influence exercised by the cultivation of natural science than in the more ready tendency displayed by the Governments of various countries to promote expeditions for scientific purposes, and a greater willingness to furnish the necessary means for the publication of the results in a satisfactory manner.

Nor can we fail to look back with a feeling of pride to the work accomplished by our own early naturalists, from the days of Banks and Solander (1768-77) to the voyage of the *Beagle* (1844-46), which the "Journal" and "Observations" of Charles Darwin have for ever rendered famous; and down to the recent magnificent results of the *Challenger* Expedition, which its grand publications are rapidly unfolding to us.

The prospectus of the "Norwegian North Atlantic Expedition" was announced in NATURE, June 2, 1881 (p. 108), and the memoir on the *Gephyræa* by Danielsson and Koren was noticed September 29, 1881 (p. 506).

The present memoir by Prof. G. O. Sars, on the Crustacea, occupies two quarto parts, illustrated by upwards of 600 figures drawn by the author himself from the actual specimens by means of the camera lucida, "thus affording a sufficient guarantee for their accuracy."

One cannot fail to be struck by the results obtained by the author in so comparatively limited an area, over part of which, at least, other naturalists must have already worked. The map embraces the ocean from the North of Scotland to Spitzbergen, and from Norway to Iceland and Greenland; or from 55° to 80° north latitude, and from 40° east to 25° west longitude.

"To avoid repetition in stating localities," writes Prof. Sars, "I have given a list of all the stations at which the dredge or the trawl was used, along with the date, position of the vessel, depth, bottom-temperature, nature of the bottom, and character of the apparatus. With but very few exceptions crustaceans were obtained at all these stations. Moreover, pelagic forms were collected with the surface-net at many other stations not included in the list. At the coast stations enumerated in conclusion, a smaller dredge was made use of, either from a boat, or from the ship when lying at anchor.

"The stations enumerated in the list have all been accurately set off on the map in which the curves of depth for 500, 1000, 1500, and 2000 fathoms are drawn as dotted lines. In the map will also be found a more strongly-marked curve, indicating the boundary-line between the cold and temperate areas, determined from the observed bottom-temperatures. This curve also forms the natural limit of the ocean valley—reaching 2000 fathoms in depth, and "filled at the bottom with ice-cold water—which shelves from the Polar Sea to the tract between Norway and Iceland, and, in the form of a narrow wedge, terminates in the so-called 'Færœ-Shetland Channel,' where it is cut off by a narrow transverse ridge from the great depths of the Atlantic" (p. 2).

Of the list of results of work performed at the eighty-six stations, either by means of the dredge, the trawl, or with "swabs," sixty give a bottom temperature of from 0° C. to 2° C. only; the highest reading being 7° C., in 237 fathoms close to the coast of Norway; whilst the average temperature at the bottom is only 2°·3 C., or 3° above freezing-point.

The deepest dredging recorded in Sars' list is 1861 fathoms, with a bottom-temperature of 1°·2 C.; but at p. 159 he mentions that *Harpinia abyssi* was obtained from a depth of 2215 fathoms.

In treating of the crustacean fauna of this region Prof. Sars has not included forms previously established as belonging to the Norwegian *littoral* fauna. Such forms will be fully treated of elsewhere, in a work on the Crustacea of Norway, to be shortly executed by the author.

Some idea may be formed of the extreme Arctic facies presented by the Crustacea inhabiting the depths of this cold-water area of the North Atlantic by noticing the relative proportions of species appertaining to each order recorded by Sars.

Thus of the BRACHYURA 1 species only is recorded (*Scyramathia Carpenteri*, Norman) from 220 fathoms; a form described and figured in Sir Wyville Thomson's "Depths of the Sea" (p. 175). From the slight development of the eyes and their light-coloured pigment it was probably blind, as is the case commonly with the crustacea from great depths.

Of the ANOMOURA only 1 species (*Eupagurus tricarinatus*, Norman) was taken in about 98 fathoms.

Four species of CARIDEA are described, the largest of these, *Sclerocrangon salebrosus* (Owen), appears to be widely distributed in the northern seas, ranging from 100 to 459 fathoms; but off the coast of Kamschatka, according to Tilesius, it is found abundantly in shallower water, and serves as an article of food to the natives. It ranges from Norway to Spitzbergen (p. 25).

Of the two species of *Bythocaris*, *B. leucopis* G. O. Sars, and *B. Payeri*, Heller, it is interesting to notice that the young of this Caridian do not pass through the usual post-embryonic metamorphosis, or larval stage, but on quitting the remarkably large ova, they are seen to be provided with the full number of appendages observed in the parent. All the species are true deep sea forms (1110 fathoms!), the eyes being unusually small and imperfect.

The next form, *Hymenodora glacialis*, has legs, as in the Schizopods, with a well-developed natatory branch (exopodite) attached to the outer side of the second joint. The eyes are small and imperfectly developed. It would seem to lead a semi-pelagic life, but it was taken at a depth varying from 452 to 1862 fathoms in the cold area.

Of the Schizopoda five species of "opossum shrimps," *Mysidæ*, were taken: *Erythrops gracilis* in from 263 to 498 fathoms, and *Pseudomysis abyssi*, Sars, from 1110 to 1280 fathoms! *Boreomysis nobilis*, 6 mm. long, was taken off the north-west coast of Spitzbergen, 80° N. lat. in 450 fathoms. The most remarkable of these deep-sea *Mysidæ* is the *Boreomysis scyphops*. "The eyes in this species are remarkable alike in character and form, their outer surface being, instead of convex, considerably hollowed, which gives them a well-nigh calyciform appearance. They are attached comparatively close together, in a vertical position, with the convex surface turning in and the concave out! Of any specific ocular pigment, or indeed of any visual elements whatsoever, no trace could be detected in the recently-taken specimens, and subsequent examination fully confirmed the absolute want of such" (p. 57).

One species of *Cumaceæ*, *Diastylis nodosa*, was obtained in 125 fathoms at Ice Sound, Spitzbergen. It has no trace of eyes, the ocular tract being *quite flat*, nor can any pigment or other visual element be seen (p. 63).

Of the ISOPODA 18 species are recorded, including members of the families *Apseudidæ*, *Tanaidæ*, *Anceidæ*, and *Arcturidæ*.

Sphyrapus serratus, Sars, is one of the most remarkable of these, both in the form of its body, of which 13 somites can be seen, and 19 paired appendages (Plate 21). The first pair of legs springing from the posterior part of the cephalic segment, are like the chelipeds in more highly-developed Crustaceans (Decapods), exceedingly powerful prehensile organs. The second pair of legs are as long as the entire body, and are flattened and armed with powerful spines, as if intended to serve as fossorial organs. Five pairs of slender walking appendages follow, succeeded by five pairs of biramous swimming-feet and a pair of long multiarticulate caudal appendages, also branched. The whole appearance of this creature is most remarkable, but it has a strong resemblance to *Apseudes*.

Sphyrapus serratus occurs in the open sea between Norway and Iceland, and also west of Spitzbergen, varying in depth from 1163 to 1333 fathoms, and ranging from the 63rd to the 78th parallel of latitude. Eyes, in a strict sense, are entirely wanting; no trace of visual elements whatever could be detected (p. 68). Of the *Tanaidæ*, three species, belonging to as many genera, were taken; all in deep water and all blind (Plate 7). The colour of these forms, as a rule, is a uniform white.

Not least remarkable among the Isopoda are the *Anceidæ* (Plate 8), most insect-looking of all the Crustacea! "The so-called mandibles (Fig. 6), which, however, as pointed out by Prof. Dohrn, both as regards their position relative to the buccal orifice and their general development, can scarcely be held to be homologous with the mandibles in other Crustaceans, are attached to the foremost margin of the head, and project freely in front of it, admitting of being moved horizontally one towards the other, like a pair of forceps" (p. 88). The larvæ (Praniza) are even more insect-like in appearance than the adult forms (see Plate 8, Figs. 13 and 27). The females of these forms are probably parasitic upon deep-sea fishes, or other large deep-sea animals. The males and larvæ of three species described were dredged from 72° to 78° N. lat., in from 658 to 1215 fathoms; they are all blind forms.

Many beautiful forms of *Arcturidæ*, *e.g.* *Arcturus baffini*, *A. tuberosus*, *A. hystrix*, &c., are figured and described. *A. baffini* was taken as far north as the west coast of Spitzbergen in 416 fathoms, and *Glyptonotus megalurus*, an allied form ranging from 63° to 78° N. latitude, inhabits depths of from 1081 to 1710 fathoms in ice-cold water!

Another strange form is the *Eurycope gigantea*. In this isopod the 2nd pair of antennæ are prodigiously developed, being more than four times the length of the whole body.

The *Amphipoda*, 45 in number, form by far the largest group of Crustacea obtained by the Expedition.

Harpinia abyssi (G. O. Sars, Pl. 13, Fig. 5) was obtained from a depth of from 350 to 2215 fathoms! It is distributed from 63° to 75° N. latitude and from 16° E. long. to 12° W. long. *Epimeria loricata*, a very handsome form, with spinose segments (Pl. 14, Fig. 2) was dredged near Spitzbergen in 260 fathoms. *Oidiceros macrocheir* dredged off Jan Mayen in 1004 fathoms was destitute of eyes (Pl. 15, Fig. 4). *Melita pallida*, another blind form (Pl. 15, Fig. 1), was found inhabiting deserted *Teredo*-burrows in an old piece of wood, dredged up from a depth of 1333 fathoms off the west coast of Spitzbergen.

Several *Caprellæ*, attached to hydroid polyps, were brought up in the dredge from a rocky bottom at 180 fathoms.

Eucheta norvegica, a copepod of the family *Calanidæ* was taken at no fewer than twelve stations at *very considerable* depths, whereas the surface-net at these localities, even when sunk considerably below the upper layer, never contained a single example.

Of the *Cirripedia*, *Scalpellum strömii* was dredged in 870 fathoms attached to the tubes of *Tubularia indivisa*. Five other species were obtained, namely, *S. cornutum*, *S. hamatum*, *S. vulgare*, *S. angustum*, and *S. striolatum*, the last attached to sponges. These are mostly white, and were collected from numerous localities and from varying depths.

A remarkably slender tubular form of *Balanus crenatus* was met with in 21 fathoms north of Beeren Island.

A Rhizocephalon (fam. *Peltogastridæ*) named *Stylon Hymenodore* attached parasitically to the abdomen of *Hymenodora glacialis* (dredged from a depth of 1861 fathoms) completes the list of forms described.

Thirteen additional species are referred to in a supplement as noticed by other writers since the present work was handed in to the scientific editor.

One is struck by the fact that of the 82 species of crustacea obtained by Sars, at these great depths in this ice-cold area, 61 are considered to represent distinct genera! Can it be that the conditions of life are here so hard, and the struggle for existence so severe, that each individual peculiarity becomes intensified, and that not only the cold, but the *darkness* may produce considerable changes in their organisation? Absence of *colour* and absence of *sight* are the prevailing characteristics exhibited by nearly every species; whilst the loss of eyes seems to stimulate the development of all the tactile organs to supply their place, so that we find the same idiosyncrasies of structure manifested in these depths of the ocean as have been observed in the Crustacea of the American and Austrian caves, and from the dark underground waters of various parts of the world.

We should fail in our duty to the author and also to the scientific reader did we omit to point out for high commendation the admirably-prepared plates which accompany Prof. Sars' Memoir, all the details of which are most beautifully and accurately rendered.

And lastly, but by no means the least meritorious feature of the work, is the generous concession made to English and American scientific readers, by the kindness of the Swedish Government, who have caused this Memoir to be printed throughout in a double column, the left hand of every page being printed in Swedish and the right hand in English.

Much praise is also due to the editor and author for the careful manner in which the English portion of the work has been passed through the press, and for the very great care bestowed on its translation from the original Swedish.

CHARLES DARWIN

Charles Darwin. By Grant Allen. "English Worthies," Edited by Andrew Lang. (London: Longmans, Green, and Co., 1885.)

Charles Darwin, und sein Verhältniss zu Deutschland. Von Dr. Ernst Kraus. Darwinistiche Schriften, Nr. 16. (Leipzig: Ernst Günther, 1885.)

IT is a curious illustration of the change which has passed over the English mind, that already the name of Darwin should head the list of a projected series of popular books, not on eminent men of science, but on "English Worthies." This first member of the series is, as might have been expected from its authorship, a pleasing and favourable specimen of a kind of literature for which the public appear to entertain so keen a relish. For it is not only clear and picturesque in style, but is also evidently written *con amore*. Indeed, it was impossible for any man of common sense or common sensibility to have come into any kind of relation with Mr. Darwin, without being stirred by feelings of hero-worship, and Mr. Allen's reverential love for the hero is a natural tribute fittingly rendered to the lofty nature and mighty influence for whose loss the universal grief is still so fresh.

As a biographical sketch the little volume is decidedly a success. It gives in brief compass and good language the history of Mr. Darwin's antecedents, of his life and work, of his relation to contemporary thinkers, and of his presumable influence upon subsequent thought. All of which is done without losing sight of the desirability, in a popular treatise, of upholding the element of romance—a kind of treatment to which the character, the life, and the work of Darwin unite in lending themselves, as it were, by nature.

In his review of the course of thought upon the theory of evolution prior to Darwin, Mr. Allen is judicious; and his speculations upon the probable position of this theory at the present time if Darwin had not lived, are interesting—tending, as they do, to show how indispensable was the work of the great naturalist in focusing the facts and showing the method. Or, to quote a somewhat happy metaphor of his own, "Darwin was not, as most people falsely imagine, the Moses of evolutionism, the prime mover in the biological revolution; he was the Joshua who led the world of thinkers and workers into full fruition of that promised land which earlier investigators had but dimly descried from the Pisgah-top of conjectural speculation."

Almost the only criticisms we have to advance relate to matters of opinion. Thus, for instance, the following passage seems to us absurd:—"Strange to say, the abortive theory [of Pangenesis] appeared some years later than Herbert Spencer's magnificent all-sided conception of 'Physiological Units,' put forth to meet the self-same difficulty. But while Darwin's hypothesis is rudely materialistic, Herbert Spencer's is built up by an acute and subtle analytical perception of all the analogous facts in universal nature. It is a singular instance of a crude and essentially unphilosophic conception endeavouring to replace a finished and delicate philosophical idea." Now we can very well understand any one who has read both the theories including them in the same condemnation, as too highly speculative, devoid of verification, and so forth. But we cannot understand any one thus exalting the one to the disparagement of the other—and least of all so on the ground that Darwin's version is "rudely materialistic." Where can there be room for any other element than the "materialistic" in the case of an hypothesis which has to do with facts purely physiological? The objection to Spencer's version we have always taken to consist precisely in its "acute and subtle perception of all the analogous facts in universal nature," whereby we are gradually translated beyond the world of physiology altogether, until we may exclaim with St. Paul—"Whether I am in the body or out of the body I cannot tell."

And this leads us to a second criticism of a more general nature. Mr. Allen, we think, is too fond of comparing the work of Darwin and Spencer, and when doing so appears to us to attach an altogether undue merit to what he calls the "deductive" as distinguished from the "inductive" method. The work of these two great Englishmen is so unlike that, even though it has been expended upon the same subject-matter, it always seems to us a great mistake to compare them; we might almost as well seek to compare the work of an historian with that of a poet. "What an ex-tra-or-dinary wealth of thought that man has," was once observed to the present writer by Mr. Darwin: "when I first read his 'Principles of Biology' I was speechless with admiration; but on reading it again I felt in almost every chapter—Why, there is here at least ten years' work for verification." Now this is surely a sound judgment, and one, moreover, in no way disparaging to the genius of Mr. Spencer. But if it is a sound judgment, surely also it shows the mistake of comparing his genius with that of the man who wrote the passage more than once quoted by Mr. Allen—"After five years' work I allowed myself to speculate on the subject."

Again, with reference to the relative values in biology of the deductive and inductive methods, Mr. Allen appears to us behind the age. To quote only one passage, he says:—"The English intelligence in particular shows itself as a rule congenitally incapable of appreciating the superior logical certitude of the deductive method. Englishmen will not even believe that the square on the hypotenuse is equal to the squares on the containing sides until they have measured and weighed, as well as they are able by rude experimental devices, a few selected pieces of rudely shaped rectangular paper." Now, it is easy to sustain the doctrine here implicated with examples drawn from Euclid; but biology is not mathematics, and if any one truth more than another is necessarily and forcibly brought home to the intelligence of a biologist—be he "English" or otherwise—it is the truth that in his science it is safer to cut out his materials in the way of experiment, than it is to build up his propositions in the way of deduction. Therefore, it is not without good reason that a proved "soundness" in this way of inductive research should be regarded as the best title to a place among men of science as distinguished from men of letters. "To be sound," says our author, "is everywhere of incalculable value;" and to be sound in the present sense, "to have approved one's self to the slow and cautious intelligence of the Philistine classes, is a mighty spear and shield for a strong man; but in England, and above all in scientific England, it is absolutely indispensable to the thinker who would accomplish any great revolution. Soundness is to the world of science what respectability is to the world of business—the *sine quâ non* for successfully gaining even a hearing from established personages." And long may it continue so. Surely this acknowledgment of the supremacy of the inductive over the deductive methods has been gained by a sufficiently long struggle in the past, and surely the tardiness of this acknowledgment has been fraught with evils sufficiently conspicuous to render somewhat grotesque the term "Philistine classes" as thus applied to the devotees of observation and experiment.

There is only one other passage upon which we have anything resembling a criticism to pass, and we notice it the more readily because, while it relates to a somewhat important matter of fact, the fact is one the unwitting and quite excusable misstatement of which by the present biographer furnishes a good opportunity for rendering its true complexion. In his chapter on "The Period of Incubation of the Origin of Species" Mr. Allen says:—

"His way was to make all sure behind him, to summon up all his facts in irresistible array, and never to set out upon a public progress until he was secure against all possible attacks of the ever-watchful and alert enemy in the rear. Few men would have had strength of mind enough to resist the temptation offered by the publication of the 'Vestiges of Creation,' and the extraordinary success attained by so flabby a presentation of the evolutionary case: Darwin resisted it, and he did wisely. We may, however, take it for granted, I doubt not, that it was the appearance and success of Chambers' invertebrate book which induced Darwin, in 1844 (the year of its publication), to enlarge his short notes 'into a sketch of the conclusions which then seemed to him probable.' This sketch he showed to Dr. (now Sir Joseph) Hooker, no doubt as a precaution to insure his own claim of priority against any future possible competitor. And having thus eased his mind for the moment, he continued to observe, to read, to devour *Transactions*, to collate instances, with indefatigable persistence for fifteen years longer."

Now, we have quoted the whole of this passage because it serves to convey, in clearly expressed language, what is a very general misapprehension with regard to the length of "the incubation period." But Mr. Darwin has himself told the present writer that the reason why he was so long in publishing his theory was simply because he wished to be fully persuaded in his own mind as to its truth before he incurred the moral responsibility of giving it to the world. Most of all those twenty years were occupied in collecting evidence, and in that process of self-criticism which he used to call "meditation," with the single-minded view of self-persuasion. Here was surely a nobler motive, and one more worthy of an "inductive mind," than that of accumulating evidence merely in order to make out a good "case." We doubt whether the popularity of the "Vestiges" exercised the smallest influence upon Mr. Darwin's motives. He had no desire to make a stir merely in order to secure a literary success; and therefore he felt that the more attention his work was likely to attract the more pernicious was it likely to prove, unless it was throughout founded upon truth. Neither was he actuated by any petty regard for priority. The reason why he showed his notes to Dr. Hooker was because he entertained a higher regard both for the learning and the judgment of this friend than he did for those of any other man.

By a curious coincidence Dr. Kraus's biography of Darwin appears in Germany about the same day as Mr. Allen's in England. As we have thus received the two by consecutive posts, it is impossible to avoid comparing them. And the comparison is interesting, as showing the differences between the public tastes to which the biographies are respectively addressed. While the English volume is a pleasing sketch of a great life, the German counterpart is an honest piece of history. Dr. Kraus has spared no pains in making his work thorough. He has carried his investigations through the smallest detail of Mr. Darwin's life and labours; he has brought together a number of letters written by Lyell, Hooker, Haeckel, Müller, &c., and also by Darwin himself; he has given a methodical account of the opinions entertained upon Darwinism by all the naturalists of any note in Europe and America who have either written or spoken upon the subject; and he has done all this without losing sight of the strong personal interest which attaches to the character of the immortal Englishman.

Many of Mr. Darwin's own letters just alluded to are translations of those written to Prof. Henslow during the voyage of H.M.S. *Beagle*, and printed for private circulation among the Fellows of the Philosophical Society of Cambridge. But all the others are translations of letters now printed for the first time—the originals having been lent for this purpose to Dr. Kraus by Haeckel, Preyer, Fritz Müller, and others. These letters are all more or less effective in displaying the distinctive qualities of their author's mind; but if we were requested to indicate one more than another which is of interest in this respect, we should mention the one to Haeckel in which the following passage occurs. The original English is not given:—

"Ich hoffe, dass Sie mich nicht für unverschämt halten werden, wenn ich eine kritische Bemerkung mache: Einige Ihrer Bemerkungen über verschiedene Autoren erscheinen mir zu streng, obwohl ich kein gutes Urteil über diesen Gegenstand habe, da ich ein so kümmerlicher Schulknabe im Deutschlesen bin. Ich habe indessen von verschiedenen ausgezeichneten Autoritäten und Bewunderern Ihres Werkes Klagen über die Härte Ihrer Kritiken vernommen. Dies scheint mir recht unglücklich, denn ich habe seit lange beobachtet, dass grosse Strenge die Leser verführt, die Partei der angegriffenen Person zu ergreifen. Ich kann mich bestimmter Fälle erinnern, in denen Herbigkeit direkt das Gegenteil der beabsichtigten Wirkung hervorbrachte. Mit Sicherheit empfinde ich, dass unser guter Freund Huxley, obgleich er viel Einfluss besitzt, noch weit grösseren haben würde, wenn er gemässigter gewesen und weniger häufig zu Angriffen übergegangen wäre. Da Sie sicherlich eine grosse Rolle in der Wissenschaft spielen werden, so erlauben Sie mir, als älterem Mann, Sie ernstlich zu bitten, über das nachzudenken, was ich zu sagen gewagt habe. Ich weiss, dass es leicht ist zu predigen und scheue mich nicht, zu sagen, dass, wenn ich das Vermögen besässe, mit treffender Schärfe zu schreiben, ich meinen Triumph darin setzen würde, den armen Teufeln das Innere nach aussen zu kehren und ihre ganze Albernheit blosszustellen. Nichtsdestoweniger bin ich überzeugt, dass dies Vermögen nicht gut thut, sondern einzig Schmerz verursacht. Ich möchte hinzufügen, dass es mir, da wir täglich Männer von denselben Voraussetzungen zu entgegengesetzten Schlüssen kommen sehen, als eine zweifelhafte Vorsicht erscheint, zu positiv über irgend einen komplizierten Gegenstand zu sprechen, wie sehr sich auch ein Mensch von der Wahrheit seiner eigenen Schlüsse überzeugt fühlen mag. Und nun, können Sie mir meine Freimütigkeit vergeben? Obgleich wir einander nur ein einziges mal begegnet sind, schreibe ich Ihnen, wie einem alten Freunde, denn das sind meine Empfindungen Ihnen gegenüber."

The chief value of the German biography consists in its setting forth the early recognition, the rapid spread, and the present acceptance of Darwinism in Germany. Dr. Kraus has always an easy case where he is displaying the old truth about a prophet among his own kindred. It was not until after we had well stoned our prophet that the nation began to recognise the reality of his mission; and, as Dr. Kraus remarks, it was not until after we had lost him that England was awakened to the true magnitude of her greatest son. So it was that, Samson-like, he slaughtered his enemies even in his death, and this on a scale which would have astonished no one more than himself, could he have lived to see it.

Dr. Kraus's narrative everywhere glows with an enthusiastic admiration of Mr. Darwin's character, and on this account he deems no trait of thought, expression, or even of movement, too trivial for the purpose of rendering a mind's-eye portrait to his reader. On the whole, this word-painting is accurate, and the workmanship in good taste. As he himself remarks, however, exception may perhaps be taken in the latter respect to his having entered upon the religious opinions of the naturalist. But as he has only collected material upon this subject which had already been published, and as he re-publishes this material in an excellent spirit of toleration towards all varieties of religious belief, we do not ourselves think that he can be justly said to have overstepped the limits of good feeling.

From this brief notice it may be gathered that Dr. Kraus's book is both a thorough and an interesting piece of biographical work; and we must not forget to add that its interest is enhanced by two portraits of Darwin (one, the last that was taken, and the other a likeness of him as a young man), a picture of his house in Kent, and a facsimile of one of his letters.

GEORGE J. ROMANES

OUR BOOK SHELF

British Zoophytes; an Introduction to the Hydroida, Actinozoa, and Polyzoa found in Great Britain, Ireland, and the Channel Islands. By A. S. Pennington, F.L.S. (London: L. Reeve and Co., 1885.)

THE object of this book is to furnish a handy, and at the same time reliable, manual of British zoophytes, using this term in somewhat the same sense as Dr. Landsborough did; and the author aims at making it do for the present generation of students what the reverend doctor's "Popular History of British Zoophytes" did for those of a former one.

In so far as the object of the author has been to furnish a catalogue of the Polyzoa and most of the Cœlenterata of the British Isles, this has been fairly fulfilled, and, as far as we have been able to judge, the catalogue is in most instances a reliable one; but the student will not find it a ready help to the determination of the species; for though in most cases the diagnoses of the genera are given, yet it is but rarely that there is enough of a hint given as to the specific characteristics of a form to enable its name to be even guessed at; so that the working biologist interested in naming the species he collects must still have by him the works of Gosse, Hincks, and Busk. The usefulness of this volume would undoubtedly have been vastly increased if the labour had been gone through of giving analytical tables of both the genera and species, and it seems to us very undesirable that new species should be introduced into a work like this without detailed diagnosis. The size of the volume need not have been greatly increased if a uniform diagnosis of the species had been attempted, for then no doubt would have been curtailed the quotations, often of no scientific value, from the writings of Dalyell and others.

We have also to regret that the list of the habitats seems to us not to have been judiciously selected. Thus, in the case of some of the rarer forms, it is not unusual to find the exact English localities given, but these followed by such indefinite indications as "Irish" or "Scotch" coasts.

In the introductory chapter we find a somewhat ambitious attempt to write the history of the progress made from 1599 to the present time in our knowledge of "zoophytes." We have no wish to be critical on the facts mentioned, but to find the writings of Trembley, Peyssonnel, Réaumur, Ellis, and Fabricius quoted, and the name of Esper, emphatically the eighteenth-century authority on this "group," not even alluded to, strikes us as curious.

As long as the author had the writings of Hincks, Busk, or Gosse to depend on, there he has been at his ease; but in the few cases where he has had to go unaided, as among the Alcyonaria, it is evident that he would have been the better for some help. In such instances, as indeed all through his work, he would have found more assistance from "Carus Prodomus Faunæ Mediterraneæ" than from isolated papers in our scientific journals.

The bibliography in Appendix A is quite unworthy of the name. From it alone no student would, without assistance, find out even what the authors wrote about. Fancy bibliographical references in these modern days, and in a work written for the present generation, of this style:—

1742. Réaumur, "Histoire des Insectes."
1821. Deslongchamps, "Encyclopédie Méthodique."
1838. Milne-Edwards, "Recherches sur les Polyps."
1864. Rev. A. M. Norman, "Contributions to 'Ann. of Nat. Hist.,'" &c.
1884. Andrés, "Die Actinien."

In Appendix B—the glossary—many words are given without any explanation of their meaning; thus, while we learn that *aperture* is "an opening or orifice," and that *orifice* is an "opening," that *apex* is "the top of anything," &c., we have such words as the following left unexplained: *avicularia*, *bathymetrical*, *calyx*, *epistome*, *funiculus*, and so on.

It is just on such matters as we criticise that we have a right to expect in a compilation that care should be taken. The general usefulness of such a volume depends on the way in which each detail is worked out. Motives that the reader of the preface will understand make us refrain from any criticism on the plates, save that the figures are for the most part of necessity from the originals in Van Voorst's well-known series.

Handbook of Jamaica, 1885-86. (London: Stanford.)

THIS is one of the most comprehensive books of the kind that has come under our notice. Everything connected with this interesting colony finds a place in it. The history of the island, for instance, and the geographical description of it might be read with advantage by the most general reader. Of special interest to scientific readers is the full account of the public gardens and plantations, now under the efficient control of Mr. Morris, whose reports we have noticed from time to time as they were published. In the "Handbook," however, a history of the department since 1774 is given; and it is curious to notice the influence it has had on the prosperity of the island. Except pimento, "that child of nature," and a few others of comparatively little value, most of the staple products of Jamaica are derived from exotics or plants introduced from other parts of the globe. Thus the sugar-cane, in its several varieties, coffee, the mango, logwood, cinnamon, the bamboo, mulberry, mimosa, camphor, clove and pepper plants, and many other products of great commercial importance to the island, were unknown a century and a half ago. The manner in which they were brought in is given from historical sources. Thus, that most important industry, cinchona-planting, was only introduced in 1861, on the recommendation of the late Sir William Hooker; the first seeds were planted in the Botanic Gardens, and the first plants reared and distributed from there. In 1884 73,533 lbs. of cinchona bark, valued at 16,327*l*., were exported from Jamaica. Many other examples of the great economical benefits of these Botanic Gardens on Jamaica might be selected from the interesting historical account of them given in this handbook. The sketch of the Jamaica Institute is also of much interest.

Syllabus of a Course of Lectures on Physiology, delivered at Guy's Hospital. By Dr. P. H. Pye-Smith. (London: J. and A. Churchill, 1885.)

THIS volume consists of the outlines of lectures given from time to time by the author at Guy's. The author, in publishing it, aims at giving the student a help to systematic reading and self-examination, as also to recall to all who take an interest in physiology, the chief facts of this important subject.

Consisting, as it does, of the heads and indications of subjects, this work is one that naturally cannot, in the ordinary sense of the term, be read through, nor will it serve in any way to cram a student for an examination: but we have kept it by us, and from time to time returned to its pages with ever-increasing interest. Though long past the period of life usually described as the "student stage," the ideas presented to us in this book, whether concerning facts, theories, or the deeply-interesting history of the subject, have compelled us to become students again, and we feel it a duty to urge our younger brethren, who are engaged in their first studies of physiology, to consult this little volume, as it is meant to be consulted; and if there be in them the smallest measure of an aspiration for a knowledge of a science as important as it is fascinating; if they be earnest, honest students, they will thank us for calling their attention to a volume which, in a small space, compasses so vast a subject.

Nature and Her Servants; or, Sketches of the Animal Kingdom. By Theodore Wood. (London: Society for Promoting Christian Knowledge, 1886.)

THIS is a well-intentioned little work, illustrated by a set of, for the most part, unobjectionable woodcuts. It is intended for the young, and so scientific terms have been almost wholly discarded. The author states very correctly, that in order to impress a fact upon the mind of a child, that fact must be presented in an interesting and attractive manner, and it is presumably bearing this in mind that he has selected the title to his volume, for children soon learn to know all that is attractive and interesting in the conception of a servant, and the child that reads the introductory chapter to these sketches of the animal kingdom will have this subject brought before him in full detail. But as the thinking child reads on, will his tender mind not be frightened at the notion so forcibly dwelt upon by Mr. Wood, that this serving Nature means that the strong servants should kill and swallow the weak; that while by one law of "Mistress Nature" the servants are to increase and multiply, by another law of the same Dame the feeble and the little ones are destroyed by the strong and the big, and that it is thus that these servants, now become foes, "fulfil their trust." The young inquirer who reads this on the first page may find it hard to agree with the statement on the last page, that "Nature is a good mistress, and provides her servants with all that they may require."

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.*]

The Whole Duty of a Chemist

I HAVE read with much interest your article on "The Whole Duty of a Chemist." To me it appears perfectly clear that he who does good work in professional science and is paid highly for it, is accorded less honour than he who does equally good work in original scientific research and receives no personal payment, because the former receives a pecuniary reward for his labour whilst the latter does not; the least honour is given where there is the least self-sacrifice. The man who does original research with the ultimate object of securing remunerative scientific employment, works with a less unselfish motive and object than he who does such research from a pure love of truth and a desire to benefit mankind. But whilst the pursuit of new knowledge has always been considered a nobler occupation than the pursuit of money, most scientific investigators do some remunerative work, and workers in both departments are necessary for the general welfare. G. GORE

Institute of Scientific Research, Birmingham

The Recent Star-Shower

LA pluie extraordinaire des étoiles filantes du 27 Novembre, 1872, s'est renouvelée cette année le même jour et à peu près avec la même intensité. D'après les télégrammes et les rélations que nous avons reçus en grand nombre jusqu'à présent, il résulte que le phénomène en question a été remarqué dans toute l'Italie depuis les Alpes jusqu'à l'extrémité de la Sicile, et qu'il se produisit partout sous les mêmes formes. Il commença à la tombée du jour. A Tarente, à 5 heures du soir, les étoiles jaillissaient et filaient en lignes si compactes qu'elles perçaient de temps en temps l'obscurité déjà avancée de la nuit. A Palerme quelques-uns de nos anciens élèves ingénieurs comptèrent 4600 météores de 5h. 15m. à 6h. 30m. A cette heure la pluie météorique se manifestait en plusieurs autres endroits avec une abondance tout à fait surprenante.

Dans notre Observatoire on commença à explorer le ciel à 6h. du soir (temps moyen de Rome). Nous avons suivi la même méthode que je suivis in 1872; les observations actuelles peuvent en conséquence être comparables avec celles d'alors. Comme j'eus déjà plusieurs fois l'occasion d'exposer cette méthode je crois à propos de l'omettre ici. Je me bornerai à rapporter les résultats obtenus de 15 en 15 minutes; et âfin de mieux éclaircir ma rélation, je vais donner ci-dessous un tableau dont la seconde colonne indique le nombre des observateurs chaque quart d'heure ; et la troisième l'état de l'atmosphère en dixièmes de ciel libre. La quatrième colonne contient le nombre des météores réellement comptés, et la dernière le nombre supputé des étoiles, c.á.d., le nombre qu'on aurait dû avoir par conjecture si les observateurs eussent été toujours au nombre de quartre et que le ciel eût été toujours serein.

Durée de l'observation	Nombre des observateurs	Dixièmes de ciel découvé	Nombre des météores	
			Observés	Calculés
6.0 — 6.15	2	10	2800	5600
6.15— 6.30	2	10	3400	6200
6.30— 6.45	2½	10	3400	6200
6.45— 7.0	3	10	4500	6000
7.0 — 7.15	4	10	6200	6200
7.15— 7.30	4	7	3500	5000
7.30— 7.45	3½	7	3100	4900
7.45— 8.0	4	7	3200	4600
8.0 — 8.15	4	7	3100	4400
8.15— 8.30	4	7	1700	2400
8.30— 8.45	4	6	1500	2500
8.45— 9.0	4	5	1000	2000
9.0 — 9.15	4	5	800	1600
9.15— 9.30	4	4	600	1500
9.30— 9.45	4	4	500	1200
9.45—10.0	3	3	234	1000
10.0 —10.8	4	3	312	1000
Heures 4.8	—	—	39,546	623,000

Le ciel fut obscurci à 10h. 8m. par un épais brouillard, qui le déroba à nos yeux tout le reste de la nuit. Ailleurs aussi de noires vapeurs voilèrent le ciel à la même heure et même avant. Seulement dans quelques localités de montagne et du midi où le ciel se conserva serein jusqu'à l'heure la plus reculée de la nuit on affirma d'un commun accord qu'à 11 heures le phénomène était presque fini. Les observations que nous avons faites dans les deux soirées suivantes du 28 et du 29 nous conduisirent à un résultat identique.

En 1872 nous en vûmes 33,000 dans l'espace de six heures ; cette fois-ci, quoique les observateurs ne fussent pas toujours au nombre de quatre comme à cette époque, nous en avons compté 39,000.

En 1872 l'abondance des étoiles atteignit son maximum entre 7h. 45m. et 8h. 45m. ; cette année au contraire le maximum avait eu déjà son commencement, quand le phénomène prit à paraître, ainsi que le prouvent les nombres calculés à Moncalieri dans les deux premières heures, lesquels sont presque constants. Beaucoup ont assuré que dès la nuit du 26 au 27 on vit une grande foule des météores sillonner les airs ; ici le ciel était chargé. Les observations des régions orientales répandront plus de lumière sur ce sujet.

En 1872 nous étions toujours au nombre de quatre observateurs et nous comptâmes 18,600 étoiles filantes pendant les deux heures proches du maximum, tandis que cette fois-ci, dans le même temps et presque toujours au nombre de deux ou de trois, nous sommes parvenus à la chiffre imposante de 29,800. Nous nous hâtons cependant de dire que toutes ces chiffres ne donnent qu'une estimation approximative de l'apparition, puisque pendant ces deux heures on ne comptait guère chaque météore, mais les groupes des étoiles seulement (et pas même tous), qui se succédaient presque sans interruption.

Par conséquent les résultats obtenus dans ce temps ne représentent que la cinquième ou la sixième partie et peut-être sont-ils inférieurs au réel.

Je crois donc ne pas m'abuser en assurant que le nombre des étoiles apparues dans le temps de nos observations n'a pas été au-dessous de 150 à 160 milles. Le spectacle qui s'offrit à nos yeux pendant les deux premières heures du maximum était surprenant, et tel qu'on trouverait de la peine à la décrire. De toutes les parties du ciel il pleuvait des masses d'étoiles semblables à des nuages cosmiques qui ce fondaient. Elles étaient suivies de traces lumineuses, et beaucoup de ces étoiles surpassaient celles de première grandeur ; quelques-unes même étaient de véritables bolides. La marche était, en général, lente, et la couleur prédominante était le rouge tant à Moncalieri qu'ailleurs occasionnée par les nombreuses vapeurs éparses dans l'atmosphère. Les météores qui se trouvaient le plus près des régions irradiantes étaient très courts : aussi plusieurs n'étaient-ils que des points flamboyants par lois de prospective.

La plus grande partie jaillissait de la région même dont elles irradiaient en 1872, et laquelle se trouve entre Persée, Cassiopée, et Andromède. On ne distinguait aucun centre sécondaire comme dans les soirées ordinaires de la plus grande affluence.

Je mis tous mes soins à déterminer exclusivement la position du radiant, ce qui ne présentait aucune difficulté. Voilà de quelle manière je m'y pris. Je fixai attentivement sa position approximative, et ensuite je traçai sur le papier le chemin de quelques-uns de ces météores qui se détachaient autour de ce point. J'achevai de cette façon presque 190 trajectoires, dont chacune à son tour en représente une infinité d'autres, qui suivaient le même chemin. En partageant ces trajectoires en trois groupes, j'ai obtenu les trois positions suivantes :—

	h. m.		Radiant
A	7 35	... ...	$\alpha = 22^\circ, \delta = +44^\circ$
,,	8 20	... ...	$\alpha = 26, \delta = +43$
,,	9 8	... ...	$\alpha = 28, \delta = +42$

Ces points sont compris entre ϕ et γ d'Andromède et le troisième point est tout près de cette dernière étoile.

Mon savant collègue, M. Schiaparelli, eut pour résultat :—

	h. m.		Radiant
A	6 35	... ...	$\alpha = 15^\circ, \delta = +45^\circ$
,,	7 12	... ...	$\alpha = 18{\cdot}5, \delta = +44$
,,	8 7	... ...	$\alpha = 23, \delta = +42$

En conclusion, la grande quantité de météores observés de ces jours est la même que l'on vit en 1859 et en 1872 ; elle se présente avec un interval le de 13 ans qui correspond à la double période de la comète de Biela-Gambart, avec laquelle cet essaim météorique a des rélations immédiates. P. J. DENZA

De l'Observatoire de Moncalieri, 30 Novembre

ON the night of November 27, when off the Straits of Gibraltar, time 6 p.m. (6.45 G.M. time), weather fine and clear, sky cloudless, I witnessed what must be of rare occurrence in high northern latitudes—a fine meteoric display, unsurpassed by anything of the kind I had the good fortune of seeing before.

From a point in the heavens situated about the Elliptical Nebula (31 M) in Andromeda radiated in all directions towards the horizon, an immense number of meteors, numbering, as far as I could count, about 30 per second from 6th mag. to beyond the apparent magnitude of Venus, as best seen, the greater number of which were apparently of the 2nd to 4th mag. and plainly visible through a space of 40°, travelling, as far as I could judge, about 10° per second.

This display continued without intermission to about 10.30 p.m., when the light of the rising moon put an end to its brilliancy, though the larger meteors still continued to be seen throughout the night.

I noticed one large meteor in particular, far surpassing Venus in magnitude and splendour, burst forth from a spot about 10° south-west of Markab, and, after traversing a space of 60° in a south-westerly direction, suddenly disappear, leaving behind it a long train of bluish-white light, which after the disappearance of the meteor began to assume different forms. The lower part of the line recurved on its path, travelling to the south-east, east, and north, until it finally joined the upper line, forming an elongated ellipse which appeared plainly to the naked eye for the space of 15 minutes. It had the appearance, when viewed through a pair of binoculars, of a light white cloud that the moon is just illuminating, and in its passage across A and V Aquarius (as the body of it still held a northerly direction) it made them to appear as if light steam was passing across them. It finally disappeared like round nebula to assisted sight after being seen for over 25 minutes.

The whole display might be likened to a huge umbrella, 31 M, the radiating point being taken for the point or apex and the shooting-stars the ribs.

The most brilliant and largest meteors held invariably a south-easterly to south-westerly direction, and the radiating-point was situated during the display from 30° east to 30° west of the zenith. JOHN M'KEAGUE

s.s. *Acadia*, December 3

A MAGNIFICENT meteoric shower was visible here on November 27, from soon after sunset till the sky was clouded over at midnight. The maximum appears to have been between 8 and 9 o'clock.

At 6h. 30m. two observers watching opposite parts of the sky counted 850 meteors in 5 minutes. At 7h. 50m. (5h. 30m. Greenwich mean time) seven observers divided the heavens among them and together counted 525 in one minute. We all agreed that we had not been able to count all that we saw, so that this number is probably too small. At any rate I do not think the number of meteors visible between 7 and 9 p.m. was at any time less than 500 per minute. At 10h. I alone counted 210 in two minutes, facing the north, which was then partially clouded.

The majority were small, though quite a number were estimated as brighter than Venus. Nearly all had trails, usually bluish-white, but frequently reddish in the case of the larger ones. At no time did I see a trail remain visible more than three seconds after the disappearance of the meteor; some friends saw one which remained visible about five minutes, changing its shape a number of times and gradually vanishing.

I determined the radiant-point carefully by tracing back the paths of a great many. I place it 2° or 3° north-west of γ Andromedæ, R.A. 25° or 26°, δ + 43° or + 44°. And three times during the evening I saw meteors appear at this point, grow brighter, and die away at the same place, as though coming directly towards the observer. ROBERT H. WEST

Syrian Protestant College, Beyrout, Syria, November 30

I CAN quite confirm what Dr. Main says about the display of meteors being finer in this neighbourhood than seems generally to have been the case, as far as the accounts which I have seen enable me to judge. My attention was not called to the falling-stars until the display was past its best; but at about 7.30 p.m. I held my opera-glass steadily on one spot, and, watch in hand, counted the numbers which crossed the field of the glass, and, repeating this two or three times, I found that more than one per second crossed the field. The exact figures were eleven each ten seconds.

I hastily set up a Coulomb's torsion electrometer, and found the air highly charged with electricity, which may possibly not be without interest in considering the auroral displays mentioned in NATURE. ARTHUR WM. WATERS

Davos Dörfli, Switzerland

THE Bielid meteors were observed here last evening (November 27) in considerable numbers. I had been closely occupied during the day, and had quite forgotten that they were due, so that I did not look out for them immediately after dark, and probably missed the maximum of the shower. On going out of the house at 7.15 (Eastern Standard Time, 5h. later than Greenwich) my attention was at once attracted by seeing two meteors in the sky together, quickly followed by others. In walking about 100 yards I counted 12, and in 10 minutes 36. Up to 7.45 about 100 were recorded in all; in the next 15 minutes only 3 or 4 appeared, and at 8 o'clock I discontinued the observations. Five or six were of the 1st magnitude; about half of the whole number were of the 2nd or 3rd, and the rest of them were mostly very small. The colour, both of the meteors and of their trains, was always *reddish*—never greenish like that of the Leonids. The tracks were generally short, in very few cases exceeding 20°, and the motion was rather slow. The position of the radiant, determined largely by three nearly stationary meteors, but agreeing also with results obtained by plotting other meteor paths, was about 2° north-west from γ Andromedæ:—α, 1h. 50m.; δ, 43°·5. The radiant was not a point but rather a region about 4° long north and south, and 2° wide. C. A. YOUNG

Princeton, N.J., U.S.A., November 28

THE meteors were seen here till 7h. 10m., when the sky became overcast. They were first noticed before it was dark, as early as 4h. 35m. and they were then abundant, though smaller ones must have been hidden by the twilight. Although cloudless, the night was not favourable, and below an altitude of 45° there was much haze, so as to sensibly dim meteors that descended below this altitude.

Scarcely any of the meteors had an apparent nucleus. Nearly all appeared as sparks, and at first scarcely increased in size, at last suddenly blazing out into a considerable number of sparks, and then almost immediately disappearing, and there was an absence of the usual lingering streaks. The meteors were remarkably abundant, especially before 6 p.m. At 5h. 25m., facing south-east, I counted 21 in ten seconds, whilst a gentleman facing north-west counted 17. The number seen between 5 p.m. and 6 p.m. must have been at least 100 per minute. Though the meteors were almost constant, they were spasmodic as regards numbers, for every few minutes suddenly the air seemed full of them. Very few were erratic, and the stream going in one direction was a conspicuous feature. Meteors passing over the same path were alike as regards the length of their paths, with this qualification: the smaller ones were invisible sooner than those of larger size.

The average size of the larger ones was not great, yet early in the evening they were larger than those seen in 1872. The largest was seen exactly at 5h. 30m.; this passed close to α Andromedæ, and it was about equal to the apparent size of Venus.

Near the point of convergence, not far from γ Andromedæ, the paths were very short, and on the point itself there were no less than five that blazed out and died away without moving, three were mere points, and two were equal to Sirius in apparent size; they also disappeared at their maximum brightness, but did not separate into sparks.

There was evidently a second point of convergence near η Cassiopeiæ, and these meteors were not as brilliant as those converging to γ Andromedæ; no stationary ones were, however, noticed here.

The general colour was that of ordinary gas, a few only being intensely blue. Those nearer the horizon had a red look, but this was owing to the haze, for their colour only changed when passing into this haze.

In 1872 one meteor—a mere spark—I saw between me and a hill (80 feet elevation), and again to-night two others passed between me and a hill. These must have fallen to the ground. They were moving almost horizontally.

The display this evening, both as regards numbers and general appearance, was very similar to that of 1872

Shirenewton Hall, Chepstow, November 27 E. J. LOWE

P.S.—Owing to work in connection with the General Election this report was unfortunately not posted on the 28th. The 28th was unusually clear, with an absence of clouds; but from 8 p.m. till 9 p.m. I only noticed two meteors, and they came from the direction of Cassiopeia.

THE expected meteoric shower was seen here between 8 and 8.30 on the evening of November 27. The weather was misty and the sky partly overcast.

The stars fell in great numbers from an invisible (owing to cloud obscuration) radiant-point in or near Andromeda, and with a slow motion, their movement being more like that of large flakes of snow falling gently through the air than that of shooting-stars. Numbers fell, apparently perpendicularly; the greater part, however, took a southerly and south-westerly direction, very few taking an eastern and north-east course. The display was intermittent, occasionally nearly ceasing, and then showering in great numbers, which it was found impossible to count. About 8.30 p.m. the sky was completely overcast, and drizzly rain was falling.

The meteors were of uniform size and of the same colour—a bright whitish-yellow. A few of larger size were seen, and these were somewhat darker and their flight more prolonged.

WM. H. LYNE

British Cemetery, Scutari, Constantinople

WITH reference to Mr. Haslam's remarks (NATURE, December 10, p. 128) as to the coincidence of auroræ with star-showers, I believe that their combined apparitions have no meaning further than that resulting from pure accident. I have seen many rich star-showers without the faintest visible trace of auroral manifestation, and many bright displays of auroræ have been watched here without indication of an associated star-shower. Still, the circumstance that certain meteoric displays have occurred contemporaneously with other phenomena, is interesting (though not, perhaps, significant, as intimating any physical connection), and such records should always be preserved, as possibly having a value which further observations may elucidate.

During the great meteoric shower of November 13, 1866, several observers detected a faint diffused light of an auroral character in the northern sky.

In 1880 there were bright displays of auroræ on August 11, 12, and 13. Whilst watching the Perseid meteor-shower on those nights, I noticed the successive appearances of streamers and light condensations in the northern quadrant. On the 13th the sky was much lighter than usual, though no streamers or bright glows were visible. The moon set before 10h., yet at midnight the air was not dark; objects around were discernible with remarkable facility beneath the luminous gauze of the aurora apparently diffused over the sky.

During the present month I have been engaged in a series of observations of the Geminid meteor-shower. On December 7, 8, 9, and 10, I especially noticed singular light-radiations, like broad films of faint white cloud, in the northern region. These were very striking on December 9, and persistent during several hours. The most conspicuous of these radiations stretched out of the horizon in the north-north-east, and ran obliquely a considerable distance east, where it enveloped the stars of Virgo and Leo. The effect was somewhat similar to that of the zodiacal light, but in the present case the phenomenon had an evident tendency to remain in the vicinity of the horizon. On its upper side I distinguished very faint indications of streamers, with alternating spaces rendered very dark by contrast. The appearances were, however, so constant, that they can hardly be associated with the characteristic mobile forms of ordinary auroræ. The sky generally was very light, and I have specially noticed this fact on several other occasions this month, when an observer might readily have assumed that the moon, in one of her quarters, was present in the firmament.

A suffused glowing of the whole sky such as that now recorded has not infrequently been visible here in past years during the progress of meteoric observations. Though no definite auroræ (in its normal features) can be descried, there is obviously some modification often present imparting to the firmament those peculiarities of aspect and tint which are far too striking to escape notice. I believe that scarcely a very clear night passes but there may be traced, with a critical eye, some feeble traces of auroræ, or their closely-allied phenomena. Could these ever-varying sky-tints be studied in a fine climate, I feel assured we might look for some interesting results.

Bristol, December 12 W. F. DENNING

The Supposed Fall of an Aërolite in Naples

THE late beautiful meteoric display, which was well seen here, has given rise to a somewhat ludicrous incident. The local papers on Sunday evening and Monday contained an account of an aërolite that had fallen in the Strada Fiorentino, one of the principal thoroughfares of the town; that this stone weighed between 6 and 7 kilogrammes, and had nearly struck some people who were passing. This announcement was followed by a description of the stone from the pen of one of the professors in the University, together with an account of meteorites in general.

Such a display of erudition, coinciding with the bombardment our earth has had from Biela's comet, prevented my sleeping all night, and, as early as etiquette would allow, I paid a visit to the house of the two professors, the *happy* (?) possessors of this would-be aërolite. The first examination convinced me that we had to deal with a shoemaker's lapstone of Vesuvian lava, the patina being nothing more than the polish of grease, dirt, with wear and tear. A small fragment was given me, which, after being sectionised, showed a typical leucitophyre of Vesuvius—probably the lava of 1631 from "La Scala" quarries.

I should not have written to you had it not been that such confirmation had been given by men of position, whom I have now obliged to admit their mistake. Probably, however, the report of the fall of this supposed aërolite has already spread, so that I fear it may be included in lists of historic meteorites.

I may say that the stone had probably fallen or been thrown from one of the roofs of the neighbouring high houses.

December 9 H. J. JOHNSTON-LAVIS

The Rotation-Period of Mars

IN the number of NATURE of November 26 (p. 81), Mr. Proctor mentions one or two points in my investigation of the rotation-period of Mars, requiring correction.

The first is, that I did not use Proctor's final result for the period, but one which he published in 1869, differing 0·02s. from the former. As I intended to determine from the whole series of Mars's drawings the correction of the rotation-period, it was perfectly indifferent what value I adopted in my calculations; the only condition was, the error should not be so great as to cause an erroneous interpretation of the Mars pictures of former years. A difference of 0·02s. in the adopted rotation-period changes the position of the markings in the drawings of Huygens and Hook but 2°·9, so that for my purpose I could adopt Proctor's value of 1869 as well as that of 1873. I chose the former, as it seemed, after a preliminary reduction of Schrœter's observations, to be nearer the truth; but my results would have been absolutely the same had I chosen the second.

The second remark of Mr. Proctor's is the following:—"Prof. Backhuyzen, like Mr. Denning some time since, has taken Kaiser's result uncorrected for the clerical errors—very seriously affecting it—which I detected in 1873." As I have used only Kaiser's *original observations*, and no *result whatever, corrected or uncorrected*, I must conclude that Mr. Proctor has not read my paper very accurately; when he does so, he will see that he is wrong. At the same time he can see that on p. 58 the time of Hook's observations is given, "March 12, 12h. 20m. and 12h. 30m.," and from the indication on p. 55, that there is an integral number of revolution-periods of 24h. 37m. 22·74s. between 1862, November 1, 9h. 55m. mean time Berlin, and 1672, January 1, 22h. 11·0m. mean time Paris, he may conclude, after a slight calculation, that I did not count the years 1700 and 1800 as leap-years. My results are, therefore, free from the errors Mr. Proctor indicated; I hope I made no other.

It seems, however, very difficult to avoid them wholly. Mr. Proctor, for instance, who occupied himself so much with the subject, writes, in the above-mentioned number of NATURE of November 26: "Kaiser counts three days too many in comparing Hook's observation with his own: one day, through a mistake in correcting for change of style and two days (apparently) from counting the years 1700 and 1800 as leap-years." That number of *three* days must be *one* day, for Kaiser indicated as the time of Hook's observations, March 13, 12h. 20m. new style, instead of March 12; by this error the number of days from 1862, November 1, till Hook was one day too small; but, by counting 1700 and 1800 as leap-years, Kaiser added two days too many, so that the total error was one day, and not three. Mr. Proctor's conclusions, based on the latter assumption, are naturally erroneous.

H. G. VAN DE SANDE BAKHUYZEN

Leyden Observatory, December 9

Ventilation

IN NATURE for December 10 (p. 132) I note the suggestion:—"In all new buildings where efficient ventilation is desired, it would be preferable to construct a shaft at one side of, or surrounding the chimney-flue, with an inlet near the ceiling of the room, and the outlet at the level of the chimney-top, so that the air escaping from the room would have its temperature kept up by contact with the chimney, thus aiding the up-draught, whilst the risk of reflux of smoke would be avoided." In building my own house some eight years ago this system was adopted in every room, the outlet over the chandelier being carried across to the side of the chimney of the same room, the two flues being carried up side by side to the chimney stack, each outlet having its own cowl. In practice this has proved a total failure, from the simple fact that the fire-flue is both longer, owing to its starting at a lower level, and that it is also hotter than the other. In the absence of any fire there is a strong upward current in both, but the instant the fire is lighted the upward current in the ceiling ventilator stops, and in a few minutes is reversed, the cold air and collected smoke from the chimney outlet coming in with such force that we have been compelled to make up every ceiling ventilator in the house except one, which, although useless when a fire is lighted, is not a nuisance. Many other experiments in automatic ventilation were tried, so that in case one system failed others might be available, and I regret to say that the only useful remnant of the experiment is the ventilation from the entrance hall, which apparently penetrates with good effect into every room in the house; unfortunately the flue in the entrance hall is one which has a persistent down draught, and we are unable to warm the air in the hall and passages.

There appears to be no rule without exception for automatic ventilation; in one room we have Tobin's ventilators, the opening on the outside facing direct north; these have to be closed always when the room is occupied, as the cold air, after rising a short distance, descends on the heads of the occupants. In another room are four similar shafts built in the wall, the bottom inlets facing west; these shafts are always left full open in the severest weather, the draught being usually downwards and outwards; but why this occurs I cannot form the remotest conception, unless eddies are caused by an adjoining building. There is no doubt that external conditions affect the practical ventilation of any building, and no absolute rules are applicable in every case alike. THOS. FLETCHER

Warrington

Rain at Smyrna

THE telegraph has informed us of a heavy fall of rain at Smyrna on Wednesday, November 25, but few particulars have yet reached us, except some from Capt. Stabb, Corr. Mem. Soc. of Arts. The storm began at six in the morning, and in a few hours 6 inches were registered. When the quay, or embankment, was proposed some years ago, in conjunction with the Council of Public Works I endeavoured to provide an efficient outfall sewer underneath it, but we were defeated by local intrigue. The drainage of Smyrna is worse than in the time of Strabo, by the large additions taken in from the shelving bay. The torrents pouring down from Mount Pagus (the Castle Hill) came through the Turkish quarter, causing the fall of some twenty houses, and washing bodies out of the Turkish and Jewish graveyards. On reaching the lower streets the sewers ceased to act, and cellars, stores, and warehouses were flooded, causing a loss of 50,000*l*. worth of opium, cotton, valonia, and other merchandise, and much damage to private houses. The River Meles overflowed its shallow bed, and reached the Point Railway Station, destroying some market gardens. In the suburb of Bournabat two houses were brought down, and the Cassuba railway embankment was damaged. The storm seems not to have reached beyond Manisa (Magnesia ad Sipylum), over Mount Sipylus, on one side, and Turbalu, beyond the Smyrna plain, on the other. No such inundation has taken place within memory. HYDE CLARKE

The Sea-Mills at Argostoli

WITH reference to the inquiry of your correspondent, Surgeon Lloyd Thomas, in your issue of the 10th instant (p. 129), there is a short paper on this subject by Capt. H. P. Shilston, entitled "On Curious Natural Phenomena in Cephalonia," to be found in the *Transactions* of the Liverpool Geological Association, vol. i. (Liverpool: Henry Young, 1881). The writer describes the inflow of water to the land through crevices in the limestone, as observed by him, and gives an explanation by Prof. Ansted, M.A., F.R.S., who considers that the phenomenon arises from the large amount of evaporation, within range of the district, by which the level of the subterranean stores of water is kept constantly below the level of the sea, notwithstanding the joint supply of rain- and sea-water. O. W. J.

Liverpool, December 12

IN answer to the question of Mr. J. Lloyd Thomas in the last number of your valuable paper (p. 129) respecting the sea-mills of Argostoli, we beg to inform you that we have published, "Die Insel Kephalonia und die Meermühlen von Argostoli, Versuch einer Lösung dieses geophysikalischen Räthsels," von Prof. K. W. M. Wiebel, mit 1 Karte, 3 Skizzen und 5 Holzschnitten, 1873.

Hamburg, December 12 L. FRIEDERICHSEN AND CO.

Friction and Molecular Structure

I SHALL feel obliged if you will kindly allow me to ask any reader of NATURE whether moderate friction can so change the molecular structure of glass as to account for the following fact:—Last night, about twenty minutes after a paraffine lamp had been lighted and had been burning steadily, its glass chimney suddenly burst into small fragments at the exact place at which, about an hour before, I had rubbed it with a piece of brown paper in order to remove soot from the interior. The chimney was thoroughly annealed, having been in constant use for more than three years. The flame was not high; the night was not frosty; the glass was uniformly thin at the place of fracture, which was six inches above the top of the flame, and two inches below the top of the chimney. The part which had not been rubbed is quite uninjured: not even a crack extending into it, while the rubbed part is shattered. EDWARD GEOGHEGAN

Bardsea, December 1

The Resting Position of Oysters—A Correction

MR. J. T. CUNNINGHAM in his letter of November 28 (p. 129), after showing that *Pecten opercularis* must rest on its right valve, goes on to say:—Of *Pecten maximus* I cannot speak with certainty, and therefore leave to Mr. Arthur Hunt the responsibility of stating that there is a difference in respect of position in the two species." So far from my having hinted that any species of Pecten rests on other than the right valve, my letter, to which Mr. Cunningham refers, concludes with the plain statement, "in each case the mollusk rests on the same valve." The point to be noticed is that in *Pecten maximus* the right valve is most convex, and in *Pecten opercularis* the left valve. A. R. HUNT

Radiolaria

I HAVE recently had the pleasure of finding, in the London Clay, a number of well-preserved specimens representative of several species of Radiolaria, most of which, I have good reason for thinking, differ from any known fossil or recent forms. It was my intention to submit them to the Geological Society during the present month, but circumstances prevent this being done. The delay may lead to an extension of the list, especially if I am fortunate enough to meet with a microscopist kind enough to assist in the examination of material yet untouched. W. H. SHRUBSOLE

Sheerness-on-Sea, December 14

THE CONTINUITY OF THE GERM-PLASMA CONSIDERED AS THE BASIS OF A THEORY OF HEREDITY [1]

THE thoughts developed in this most interesting and important essay were first expressed in a lecture delivered to students of the University of Jena last winter. They were reduced to writing in the spring, and completed for publication in June. The author received Oscar Hertwig's essay on the "Theory of Inheritance," and Kölliker's "On the importance of the Cell-nuclei for the Processes of Heredity," after his manuscript was complete. In the matter of the extreme importance of the nucleus he agrees with both these authors.

As was stated in reviewing here two years ago Prof. Weismann's memoir "On the Origin of the Sexual Cells of the Hydromedusæ," all his memoirs abound in original views and suggestions, which render them of peculiar and widely-spread interest. The present is no exception to the rule. It is intended in this article to give a kind of abstract of the memoir, composed largely of a series of translated passages: for the fuller development of details, the history of the development of ideas on the subject, and controversial matters, readers are referred to the original, which is an octavo of 122 pages.

"How is it," asks the author in commencement, "that in the case of all higher animals and plants, a single cell is able to separate itself from amongst the millions of most various kinds of which an organism is composed, and by division and complicated differentiation to reconstruct a new individual with marvellous likeness, unchanged in many cases even throughout whole geological periods?" The question is a hard one indeed, and the various attempts which have been made to solve it,

[1] "Die Continuität des Keimplasma's als Grundlage einer Theorie der Vererbung." Von Dr. August Weismann, Professor in Freiburg i. B. (Jena: Verlag von Gustav Fischer, 1885.)

though most useful as temporary advancements of speculation on the problem, such pre-eminently as Darwin's theory of pangenesis have failed hitherto to dispose of it satisfactorily. It is impossible now to believe that every cell of the organism can give off gemmules which exist at all times in all regions of the body, become collected in the generative cells, and are capable of becoming metamorphosed in regular order back again into the different cells of the organism.

The problem must be considered anew, and the present essay deals not with the entire subject of heredity, but with the fundamental question, How is it that a single cell of the body unites within itself the entire tendencies of inheritance of the whole organism? There are only two physiologically conceivable possibilities by which germ-cells endowed with such peculiar powers as we know them to possess can be produced. Either the substance of the parent germ-cell after passing through a cycle of changes required for the construction of a new individual possesses the capability of producing anew identical germ cells, or *the germ-cells arise as far as their essential and characteristic substance is concerned, not at all out of the body of the individual, but direct from the parent germ-cell.*

It is this latter view which Prof. Weismann holds to be correct, and maintains in the present essay, and which he terms the theory of the continuity of the Germ-plasma. On this theory heredity depends on the fact that a substance of peculiar chemical and even more special molecular composition passes over from one generation to another. This is the "germ-plasma," the power of which to develope to a complicated organism depends on the extraordinary complication of its minutest structure. At every Ontogenesis a portion of the specific germ-plasma which the parent egg cell contains is not used up in producing the offspring, but is reserved unchanged to produce the germ-cells of the following generation.

It is plain that this supposition reduces the question of heredity to one of growth. The germ-cells of all succeeding generations being merely pieces of the same substance as the first, and of the same molecular structure, when nourished under similar conditions, must run through a similar series of stages of development, and yield the same final products.

After combating objections which may be raised to the theory on the score of the heredity of "acquired modifications," it is pointed out that the germ-cells on it appear no longer as a product of the body, at least as far as their essential part, the germ-plasm, is concerned; they are rather to be regarded as something standing opposed to and separate from the entirety of cells composing the body, and the germ-cells of succeeding generations are related to one another as are a series of generations of unicellular organisms derived from one another by a continuous course of simple division into two.

Jäger's and M. Nussbaum's views approached very near those of Weismann; but these authors inferred a continuity of the germ-cells themselves. Such a continuity of cells survives at present in but very few cases. In nearly all instances the generations of germ cells start from the parent, as very minute particles of germ-plasma only, to form, nevertheless, the basis of the germ cells of the next generation.

The author claims for his theory that even should it require to be abandoned in the future, it nevertheless represents a stage in our knowledge of the problem which must be passed through, which must be clearly stated and carefully worked out, whether the future prove it true or false. With this view of it he develops it in three chapters, the first of which deals with the conception of the germ-plasma.

THE GERM-PLASMA.—It now seems established that the only actual carrier of the tendency of heredity is the highly organised nuclear substance; fecundation consists in a union of nuclei; the surrounding cell substance has no immediate participation in the result. E. van Beneden's splendid researches on Ascaris led far towards this conclusion in showing that the nucleus of the egg cell does not fuse in any irregular manner with that of the sperm cell, but that the nuclear loops of these two bodies arrange themselves opposite one another in regular order, two and two, and thus construct the new nucleus, the segmentation nucleus. Van Beneden, as is well known, viewed the two nuclei concerned as half nuclei male and female respectively, the union of which produced an entire nucleus of hermaphrodite nature. Weismann, on the other hand, speaks of "the union of the nuclear substance of the maternal and paternal individual." Strasburger has shown that in the fecundation of phanerogams the nucleus alone of the sperm cell (pollen tube), not the cell body also, enters the embryo sac to conjugate with the nucleus of the ovicell. Strasburger, led by Van Beneden's results, concluded that the occurrence of heredity depends on the transmission of a nuclear substance of specific molecular structure. This specific nucleo-plasma of the germ cell is what Weismann terms germ-plasma. This germ-plasma is, however, by no means identical with Nägeli's idioplasma. The idioplasma, according to Nägeli, is a network which stretches through the entire body, and in fact constitutes the specific molecular basis determining its manner of the body's existence. The general conception of a molecular basis of the organism governing its existence and opposed to the mere nutrient plasma is a fine and original one, and worthy of much merit, but in its detailed development Nägeli's theory cannot now be accepted. Even if the cell bodies are everywhere connected by fine outgrowths in all vegetable and animal pluricellular organisms, as recent research seems to show, the network present is one of nutrient plasma, not of idioplasma, for the determining molecular basis is confined to the nuclei which are not so connected. Moreover, there can be no one single substance such as idioplasma of identical composition permeating the whole body. On the other hand each different kind of cell in each organism must contain its specific kind of idioplasma, or rather nucleo-plasma regulating its peculiar mode of existence.

The author quite agrees with Strasburger in considering the "specific peculiarities of organisms as centred in the cell-nuclei," and also in many points in his statement that "from the nucleus issue forth molecular stimuli into the surrounding cytoplasma, which, on the one hand, govern the processes of change of material in the cells, and, on the other, give to the growth of the cytoplasma conditioned by the nutrition, a certain character peculiar to the species." A valuable confirmation of this position is afforded by A. Gruber's experiment on Infusoria, that, though artificially separated fragments of Infusoria without any portion of the nucleus can live for some time, they never are able to regenerate themselves, whereas fragments containing part of the nucleus always do so. The nature of the nucleo-plasma undergoes a regularly ordered series of changes during ontogeny. The simplest view to take is, that at each division of the nucleus the specific plasma of the nucleus divides itself into two halves, differing in their essential composition, so that each resulting cell-body also, its character being determined by the nucleus, becomes re-fashioned. Thus, in the case of any Metazoon, for example, the two first segmentation spheres would undergo such change that the one would contain only the tendencies of heredity of the endoderm, and the other only those of the ectoderm, and so on throughout. Against such a supposition, however, stands the fact that is observed in instances of indirect division of nuclei during the process of kariokinesis, each mother nuclear loop of the nuclear plate splits exactly in two lengthways into two halves. Each daughter nucleus thus receives exactly the same supply of these, and it would appear as if the two nuclei could not differ, but must be exactly identical. Strasburger, therefore, considering this identity a fundamental fact, concludes that the difference between the two must arise subsequently to their separation as the effect of unlike nutrition. It is urged by Weismann, in antagonism to this view, which would be fatal to his theory, that all that is really proved by the fact is that at every division of a nucleus an equal mass of maternal and paternal nuclear substance passes to form each daughter nucleus, but that it is by no means shown that the quality of the parent nuclear plasma must be identical on both sides. On the other hand, from the effect of the daughter nuclei on their respective cell-bodies, which are most commonly different both in size and texture, it seems proved that they are usually different in quality. As well-marked examples may be cited the polar vesicles. In the case of some Mollusca the egg gives off, by the indirect method of nuclear division, two polar vesicles, one after another, and each of these divides into two. The four polar vesicles perish, whilst the nucleus of the ovum remaining in the yolk combines with the sperm nucleus, and, making use of its own cell body, becomes the embryo. The reason for the difference here must be that the quality of the nucleus of the polar body is different from that of the ovicell.

In accordance with Nägeli's views, then, the molecular structure of the germ-plasma must be so much the more complicated the more complex the organism is which is to be developed out of it: and further, it can be stated that the nuclear substance must become successively less and less complicated as ontogeny proceeds, in proportion as the foundations which yet have to be evolved out of any cell and of which the nuclear plasma is the molecular expression, become gradually less in number. The general nucleo-plasma becoming thus gradually more and more simple in molecular structure, soon loses its capability of reproducing the entire organism; it cannot by any process be metamorphosed back again into the immensely complicated germ-plasma. Only the nucleo-plasma of the original segmentation nucleus is germ-plasma—that is, possesses the structure by the regulating action of which on the process of growth the entire organism can be evolved. In many cases, from the moment of the first division of the ovum into two blastomeres the one blastomere loses the power of reproducing the whole organism out of itself alone, since one resulting blastomere represents the future epiblast, the other the hypoblast. Somatic nucleo-plasma cannot become converted into germ-plasma.

Phylogenetically the germ cells did not originate at the termination of ontogeny, but at its commencement, as is well shown by the conditions existing amongst certain lower chlorophyll containing organisms such as Pandorina and Volvox. The phyletic origin of the first germ cells must evidently be sought amongst the earliest polycellular organisms differentiated by division of labour. In the genus Pandorina of the Volvocineæ no such division of labour has as yet arisen; each spherical colony is composed of exactly similar flagellate cells (each with an eye-spot, chlorophyll contents and pulsatile vacuole) embedded in a common colourless jelly (*homoplastide*). These colonies reproduce themselves alternately by asexual and sexual process; in the latter case the copulating individuals are not yet distinguishable from one another as male and female; in either case every cell of the colony remains as yet a complete unicellular organism capable of separate reproduction. In Volvox, another genus of the same family, a *heteroplastid* condition has been attained and the separation into somatic and generative cells has been effected. The spherical colony consists of two sorts of cells—numerous small flagellate cells and much fewer large germ cells devoid of flagella. The latter alone can effect the production of a new Volvox sphere and can do this in two ways, either asexually (parthenogenetically) or after impregnation by small actively-moving spermatozoa formed out of certain of their own number. Now, as Kirchner has shown, the germ cells become separated off from the somatic cells early in the segmentation of the volvox ovum before the escape of the young heteroplastid from the egg-coverings, which is exactly as should be according to Weismann's views.

Here is proof that there is no intervention of somatic cells in the course of growth between the parent germ-cell and the daughter germ-cells, but that the latter arise directly from the parent germ-cell, and thus the continuity of the germ plasma is established as a fact for the commencement of the phyletic development series. In later times, with increasing complexity of the organism, the time of the separation of the germ-cells became gradually more and more postponed in most cases, and at the present period often occurs quite late at the end of the entire ontogeny. If in the egg of Diptera the first two nuclei which separate themselves by division from the segmentation nucleus of the egg form the reproductive cells, this is a proof that they receive the entire germ-plasma of the segmentation nucleus unchanged.

There are, however, scarcely any theoretical grounds against the supposition that unmodified germ-plasma might be mingled with the nuclear substance of the somatic cells; on the other hand, it would appear *a priori* very conceivable that all somatic cells might contain some unmodified germ-plasma. The fact that a complete Begonia plant with fruit and fertile seed can be grown from a Begonia leaf, whilst in the case of many other plants no such result can be obtained, seems to show that in certain plants the cells, or perhaps only certain cells, of the leaf contain germ-plasma, whilst in others unchanged germ-plasma is not present in the leaves at all, or in very minute quantities only. In the case of the mosses, where almost every cell of the roots, leaves, and axial shoots can become a complete plant, probably all, or nearly all, the component cells must contain an adequate supply of germ-plasma.

The Meaning of the Polar Vesicles.—The egg-cell must contain two kinds of nucleo-plasma or idioplasma, namely, germ-plasma and histogenetic plasma. During its growth it has to accumulate yolk and to form surrounding membranes, in some cases to form a micropyle, and otherwise adapt its cell-body to the production of the future embryo. Therefore, besides the germ-plasm it carries, it requires another kind of specific nucleo-plasma just like every other histologically differentiated cell. This histogenetic plasma cannot be the same as that which subsequently governs the development of the embryo, and which arises from the infinitely complex germ-plasma. As soon as the egg is ripe for fertilisation it is necessary that the histogenetic plasma should be got rid of, in order to leave the germ-plasma free to act, and the extrusion of the polar bodies is the removal of this *ovogenous* nucleo-plasma. This is an entirely new theory as to the significance of the polar bodies, and directly opposed to all those which would see the extrusion of a male element in the act.

In the case of the male sperm-cell, also, two kinds of nucleo-plasma are present—germ-plasma and spermogenous nucleo-plasma. As soon as the spermatozoon is ripe, the spermogenous nucleo-plasma is cast off as the equivalent of the polar body. Strasburger has lately described a large number of instances amongst plants of different groups, in which processes resembling the extrusion of polar bodies accompany the ripening of the generative elements of both sexes. And it is probable that similar conditions will in time be discovered to exist in other plants.

On the Nature of Parthenogenesis.—The fact of the formation of the polar bodies, considered in the light of the theory of the sexuality of the germ cells, has been freely made use of to explain the occurrence of parthenogenesis. Balfour suggested "that the function of forming polar cells has been acquired by the ovum for the express purpose of preventing parthenogenesis." Weismann naturally cannot agree with this view, since he regards the extrusion of polar bodies merely as the removal of the ovogenous or spermogenous nucleo-plasma. At the time his memoir was written there were no instances in which it had been ascertained with absolute certainty whether polar vesicles are formed or not in the case of ova about to develop parthenogenetically; but in a postscript at the end of the memoir he is able to announce that he has discovered that in the case of parthenogenetic summer eggs of the Daphnidæ a polar vesicle of distinct cellular structure occurs. This is sufficient proof of the incorrectness of the older theory, and he further adduces the fact that in the case of the honey-bee the same identical egg can be made to develop either after fertilisation or parthenogenetically, according to the act of the queen, showing that the parthenogenetic and sexual eggs are of the same essential nature. The difference between the two must lie in the quantity of germ-plasma which they respectively contain. In order that the segmentation nucleus of the ovum may proceed to the process of ontogenesis, it must possess a certain mass. Even amongst higher vertebrates it is known that an unfertilised egg may occasionally go through the first few stages of segmentation, then, however, always failing through lack of the requisite power.

When impregnation takes place and the substance of the nucleus of the sperm-cell becomes added to that of the ovum, the combined mass of germ-plasm becomes powerful enough to carry through all the stages of ontogeny to the end. In the case of certain animals where the ovum remains unfertilised, and thus unchanged after the extrusion of the ovogenous nucleo-plasma, if a special supply of nourishment reaches its germ-plasma, this increases in amount by growth, and thus attains the mass requisite to start the ontogenetic process, with the result that parthenogenetical development takes place. In the ordinary sexual process it is the sudden doubling of the mass of the nucleus by the copulation that starts the segmentation of the ovum. It is the increase of the mass of the nucleus which gives the stimulus to segmentation, the disposition to which was already there before. The difference between eggs requiring fertilisation, and those not requiring it supervenes after the ripening of the egg and the extrusion of the ovogenous plasma. The phyletic inheritance of the capability of parthenogenetic development rests on a modification of the power of growth of the egg nucleus.

More than ten years ago Weismann expressed the conviction that "the physiological value of the sperm-cell and egg cell are identical," and now that the body of the egg cell can hardly have ascribed to it a higher value than that of a common nutrient ground for the two nuclei during the act of impregnation, this position seems a very secure one, Strasburger fully agrees, and states that "Sperm-nucleus and egg-nucleus do not differ in their nature." If it were possible to introduce by artificial means into any egg, immediately after the change of the germinal vesicle into the egg-nucleus, the egg-nucleus of another egg of the same species, it is probable that the two nuclei would copulate just as if a ripe sperm-nucleus had penetrated into the egg in the usual way, and a direct proof would thus be given that the egg and sperm-nucleus are in fact identical. The technical difficulties are too formidable to permit of this experiment being made, but a partial confirmation is afforded by Von Berthold's discovery that in certain algæ Ectocarpus and Scytosiphon, not only a female, but also a male parthenogenesis occurs. Further, the occurrence of conjugation must be regarded as a proof of the correctness of this view. There can scarcely be any further doubt that conjugation is the sexual reproduction of unicellular organisms. Amongst these usually the two conjugating cells are externally absolutely alike, and probably they are so internally also, but there are some low forms, such as Volvox, where a difference between the two is already fully established, huge egg shells and minute zoosperms being produced. The identity of the sperm nucleus and egg nucleus here insisted on only regards their essential fundamental structure and composition; each is in certain finer details necessarily peculiar, as transmitting the idiosyncracies of its own parent stock.

H. N. MOSELEY

THOMAS ANDREWS, F.R.S.

DR. ANDREWS, whose death we announced a fortnight ago, was a native of Belfast (born December 19, 1813), and spent his whole life there. His father was a linen-merchant, in good position; and he received his early education at the Academy and at the Royal Academical Institution of Belfast. Thence he went to Glasgow, where he studied under Dr. Thomas Thomson, the well-known Professor of Chemistry, and learned practical work in his laboratory. He had, next, a successful undergraduate career in Trinity College, Dublin; where he distinguished himself in Classics as well as in Science. Having spent some time in Paris, in the laboratory of Dumas; and having obtained his Medical Degree in the University of Edinburgh, in 1835; he devoted himself to medical practice in his native town. In this he was highly successful; but he continued to devote his leisure, small as it was, to scientific research; publishing numerous papers on chemical and physical subjects. To these we will recur, but it is noteworthy that in 1844 he received one of the Royal Medals, in the gift of the Royal Society, for his purely scientific discoveries, before he finally gave up professional practice. He was the first lecturer appointed to teach Chemistry in the Royal Belfast Academical Institution, and he resigned this post, as well as gave up practice, when appointed in 1845 to the Vice-Presidentship of the "Northern College," now Queen's College, Belfast. The Presidents and Vice-Presidents of these new Irish institutions were appointed some years before the Colleges were opened, or the Professors elected, in order that the Government might have their advice and assistance in maturing the whole scheme. Andrews was thus associated with another justly-distinguished Irishman, Sir Robert Kane; and it is mainly to their labours and foresight that the Queen's Colleges, when at last opened, appeared before the world in full working order.

It had been understood from the first that Andrews was to be the Professor of Chemistry in Belfast; but, when the time for appointing Professors arrived, he was required (as a matter of form, merely) to produce a few Testimonials. These he obtained at once, in the highest terms, from such men as Thomas Graham, Humphrey Lloyd, MacCullagh, &c., and they need not be given here. But it may be interesting to show, as briefly as possible, the opinions of two of the greatest of foreign chemists. Liebig wrote (November 10, 1845) as follows:—"Ich hege die volle Ueberzeugung dass der Platz um den Sie sich . . . bewerben, keinen würdigern Besitzer finden dürfte. Sie haben viele Jahre hindurch mit den grössten Schwierigkeiten zu kämpfen gehabt, um der warmen Neigung welche Sie für die Naturwissenschaften hegen Nahrung zu geben, und weit entfernt dass Ihr Muth und Eifer dadurch gelähmt worden wäre, haben Sie durch Ihre letzten wichtigen Arbeiten über die Wärme bey chemischen Verbindungen dargethan, dass die Beschäftigung mit der Wissenschaft ein Bedürfniss ihres Geistes ist." Dumas (November 29, 1845) wrote:—"Vos titres à la nouvelle fonction à laquelle vous aspirez sont si clairs et si évidents que je ne concevrais guère que vous n'y fussiez point appelé . . . mais, tout en enseignant la chimie, n'oubliez pas que vous comptez au nombre des physiciens distingués de votre pays." The rest of this letter is not personal, but refers to the impossibility of separating chemistry from physics, and to the important aid which each of these sciences constantly obtains from the other.

Andrews was as successful in his Professorship as he had formerly been as a Practitioner. He soon gathered large classes, alike for general chemistry and for practical laboratory work. All his spare time, for the greater part of every working day, was spent in his private laboratory. Here he delighted to receive his scientific friends, and to engage eagerly in conversation with them while his hands were busy with the steady, deliberate construction or adjustment of apparatus for his next research. His habits were of an extremely temperate, almost abstemious, character. From his early breakfast, to his somewhat late dinner-hour, he never partook of either food or drink; and used to say that a man required only two meals a day. It is to be feared that his persistence in this habit in his later years tended materially to reduce his strength.

He was elected to the Royal Society in 1849. In 1852 (at Belfast), and again in 1871 (at Edinburgh), he presided over the Chemical Section of the British Association. He was President of the Association in 1876 (at Glasgow), having declined the appointment in a former year in consequence of the state of his health. He was a corresponding member of the Royal Society of Göttingen and an Honorary Fellow of the Royal Society of Edinburgh. He received honorary degrees from various Universities. But he valued this class of distinctions simply as tokens of the esteem and good wishes of the donors; and in the somewhat delicate matter of a civil title he shared the opinion, and followed the practice, of his cherished friend Faraday. In 1879 he resigned his appointments in Queen's College, and thenceforth led a very retired life, though still vividly interested in the progress of science, till his death on November 26 last.

The only purely literary works of Andrews, so far as the writers of this notice are aware, were his two extremely thoughtful and learned *Chapters of Contemporary History*. The first, entitled *Studium Generale*, was published in 1867, when attempts were being made to cripple the usefulness of the Queen's Colleges. The reasons for the appearance of the second, published in 1869 with the title *The Church in Ireland*, are still too painfully prominent to require special mention.

Dr. Andrews married, in 1842, Jane Hardie, daughter of Major Walker, of the 42nd Highlanders. He is survived by his widow, by three daughters, and by two sons, the elder of whom is Major in the Devonshire Regiment, and the younger a member of the Irish Bar.

His first published paper on a chemical subject is on the composition of the blood of cholera patients. He showed that it differed from normal blood only by having a smaller proportion of water. Much more important both in itself and as showing the bent of his mind to the borderland between Chemistry and Physics is a paper on galvanic cells with strong sulphuric acid as the exciting liquid. The question is of course really that of the electrolysis of strong sulphuric acid, and Dr. Andrews showed that the composition of the gas given off at the cathode varies in a remarkable manner with the temperature. This is quite in accordance with what we now believe as to the constitution and dissociation of strong sulphuric acid, but at the time the paper was written nothing was known which could lead any one to suspect such a variation.

We now come to one of his great works—the determination of the heat evolved during chemical action. In three series of investigations he determined the heat given out in the formation of neutral, acid, and basic salts, by the action of acid on base; in the displacement of one metal in a salt by another; in the formation of oxides; and in the formation of chlorides. In this great research we see the character of the man, his clear view of what was to be observed, his distinct recognition of the sources of experimental error, and the simple but effectual means he took to get his results free from the effects of such disturbing causes. Especially worthy of note is his use of solutions so dilute that further addition of water produced no sensible thermal change.

The well-known experiments of Favre and Silbermann were published not long after Andrews' first papers on this subject. It is interesting to notice that where these observers differ from Andrews, subsequent investigations, particularly those of Berthelot and of Thomsen, have shown that Andrews was right.

In 1855 Andrews communicated to the Royal Society a paper of great importance and interest on Ozone. This remarkable substance had been studied by Schönbein, its discoverer, Marignac, De la Rive, Berzelius, Williamson, Fremy and Becquerel, and Baumert, but its nature still remained a mystery. Is ozone always the same thing, or are the ozone of electrolysis, that of the electric machine, and that formed during the slow oxidation of phosphorus, different bodies very like one another in properties?

Some experiments seemed to show that ozone contained nothing but oxygen, others that it was an oxide of hydrogen containing a larger proportion of oxygen than water does. The question was exactly of the kind to attract Andrews and to call out his peculiar powers of investigation.

By a series of experiments remarkable for simplicity and delicacy, and perfect adaptation to the purpose in view, he proved that "ozone, from whatever source derived, is one and the same body, having identical properties and the same constitution, and is not a compound body, but oxygen in an altered or allotropic condition."

The investigation into the nature of ozone was continued by Andrews and Tait, and the results published in their paper *On the Volumetric Relations of Ozone and the Action of the Electrical Discharge on Oxygen and other Gases* (*Phil. Trans.*, 1860). These results led directly to the theory of the constitution of ozone now universally held; indeed that theory is distinctly stated by Andrews and Tait, although not further discussed on account of its supposed improbability.

Among smaller works we may mention the discovery of minute particles of metallic iron in various rocks, particularly basalts.

None of his chemical papers can be read without some new idea being communicated to the reader, however well acquainted he may be with the subject.

The investigation, however, by which Andrews is, and will continue to be, best known, was that *On the Continuity of the Liquid and Gaseous States of Matter* which formed the subject of the Bakerian Lecture in 1869, and again in 1876.

One of the earliest of Faraday's researches was devoted to the liquefaction of gases, and he succeeded with all but a few, which were in consequence, till very recently, distinguished as "non-condensable." But he expressed the conviction, founded on experiment, that even these could be liquefied by the conjoint action of sufficient pressure and sufficient reduction of temperature.

Another extremely ingenious experimenter, Cagniard de la Tour, had approached the subject from the opposite side; and had shown that liquids, such as water and sulphuric ether, could be changed into something which was certainly not liquid, by sufficient rise of temperature without any great increase in volume.

Regnault, also, had measured with his unrivalled precision the compressibility of various gases; and had called attention to the curious differences which they show in their modes of divergence from Boyle's Law. And Natterer, by employing pressures of some thousands of atmospheres, had arrived at other startling results.

The whole subject was in that chaotic state which naturally precedes the advent of the Kepler who is to marshal, under a few general statements, each intrinsically simple, the mass of apparently irreconcilable phenomena.

Andrews' classical researches completely effected this simplification. Guided by the results of Regnault, he selected carbonic acid as the substance whose behaviour was made the subject of exhaustive study through wide ranges of temperature and of pressure. He devised an extremely ingenious form of apparatus for the purpose, had the coarser metallic parts constructed under his own eye by a remarkably skilful mechanic; and himself made and calibrated the glass portions, purified with great care and skill the gas to be operated on, and finally fitted up the whole with unwearying patience.

The simpler and more prominent results of this splendid research may be briefly summed up as follows:—

(*a*) When carbonic acid is maintained at any temperature whatever *above* 30°·9 C., it cannot even in part be condensed into liquid by any pressure however great.

(*b*) If the temperature be *below* 30°·9 C., the gradual increase of pressure ultimately leads to liquefaction; but the pressure of the vapour in presence of the liquid is less as the temperature is lower.

(*c*) A cycle of operations, in Carnot's sense, can be performed on liquid carbonic acid in such a way that, during the first stage of the expansion we have optical proof of the existence of liquid and gas side by side in the same vessel; while on compressing again at a higher temperature, and finally cooling down to the original temperature and volume, the whole contents are once more liquid; though at no stage of the latter part of the operation is there any appearance of the joint presence of two different states of matter. It is this fact which suggested the title of the paper.

(*d*) The key to the explanation of observed deviations from Boyle's Law was furnished by his study of the isothermals of carbonic acid at temperatures not much above 30°·9 C. For Andrews's measurements show that the product of pressure and volume (which, by Boyle's Law, should be constant) diminishes with volume to a minimum, and thereafter rises rapidly as the volume is farther diminished.

Many other valuable results, such as the great compressibility of liquid carbonic acid, especially at temperatures near to 30° C.; the alteration of surface-tension of the liquid, and of its angle of contact with glass, as the temperature is raised, &c., appear as mere side-issues of this investigation.

The discovery of this Critical Temperature, or Critical Point, soon led to the liquefaction (and in certain cases even to the solidification) of the gases which had been called "non-condensable." Andrews' work had supplied all the necessary hints for the adaptation of his apparatus to such a purpose. In fact the main requisites were (1) to work on a larger scale, (2) to employ very low temperatures, and especially (3) to provide a means of ensuring *sudden* relaxation of pressure. The work of Pictet, Cailletet, v. Wroblewski, Amagat, &c., on this subject, followed as a natural and immediate consequence of that of Andrews.

The writers have, like many others, seen and admired the confident manipulation, by Andrews, of wide sealed tubes, half full of liquid carbonic acid; how he, knowing the soundness of his own glass-blowing, boldly heated such tubes in the flame of a Bunsen lamp, till the liquid entirely disappeared, and pointed out with eager enjoyment the extraordinary phenomena presented as the contents cooled nearly to the critical temperature. The whole tube seemed, for a short time, to be filled with a substance presenting, to an exaggerated degree, the appearance of a mixture of water and alcohol before diffusion has sensibly operated.

We have spoken of Andrews's remarkable skill in manipulation, and of his unwearied patience. But even these were eclipsed by the perfect calmness with which, though on the very verge of an important discovery, he attended to every point of minute and laborious detail; so that his first successful experiment was as exactly carried out and recorded as was its future repetition. This was all the more remarkable in that he was usually, especially in public, a man of a highly nervous and excitable temperament.

An excellent French and German scholar, he kept himself always well acquainted with the most recent progress of science, whether chemical or physical. He constructed his own dividing-engine for the calibration of the exquisite thermometers which he made for his researches on heat; and his air-pump (in which he took special delight) was furnished with numerous valuable improvements all devised by him for particular applications. His laboratory books were models of ample, but not superfluous, detail.

He was, personally, a man of simple unpretending manner; conscientious almost to an extreme, but thoroughly trustworthy and warm-hearted; an excellent example of the true Christian philosopher.

NOTES

We regret to announce the death of Mr. Alfred Tribe, the well-known chemist, on November 26, at the age of forty-six years. We defer to next number a notice of Mr. Tribe's career.

A meeting of the subscribers to the testimonial to Dr. H. Woodward, F.R.S., for twenty-one years editor of the *Geological Magazine*, was held on the 15th inst., at the rooms of the Geological Society, Burlington House, when Prof. T. G. Bonney (Pres. Geol. Soc.) presented to that gentleman, on behalf of the subscribers, a silver tea and coffee service and a cheque for 253*l*. On making the presentation, Prof. Bonney addressed Dr. Woodward, referring to the invaluable services he had rendered to science during the twenty-one years he has had charge of the *Geological Magazine*. In replying, Dr. Woodward referred briefly to the career of the *Magazine* and its predecessors, to the many distinguished men that had been connected with them, and to the period of remarkable scientific interest thus covered. Over 200 names are on the list of subscribers to the testimonial.

The 10th instant was a red-letter day at the French Institute. M. Bertrand was introduced as member of the Académie Française, and read an address which, according to custom, was devoted entirely to thanking his brother Academicians and to eulogising his immediate predecessor, M. Dumas. The address was acknowledged by another oration from M. Pasteur, who after having summarised the life of M. Bertrand, gave a long and interesting account of the career of M. Dumas. Great enthusiasm prevailed throughout the proceedings, and the hall was crowded.

The number of patients in the special clinic of M. Pasteur is increasing daily, several arriving from foreign countries. No less than forty were inoculated on December 14 before the Minister of Agriculture, who acknowledged the services rendered by Pasteur to humanity. Among the patients is an officer of the Czar's body-guard, just arrived from St. Petersburg.

A biography of the late Sir William Siemens is being prepared, at the desire of the executors, by Dr. William Pole, F.R.S., Hon. Secretary of the Institution of Civil Engineers, and author of the "Life of Sir William Fairbairn, Bart." Dr. Pole will be grateful for the loan of any of Sir William's letters, or for any information of importance. Address, Athenæum Club, S.W.

On Saturday, last week, the rainfall at the Ben Nevis Observatory amounted to 4·991 inches, and on the Sunday following to 3·604 inches, there having thus fallen on these two days fully eight inches and a half. During this time the wind was westerly and north-westerly, force 2 of Beaufort's scale, with occasional squalls. On the Monday the wind was still westerly, and though no rain fell from 2 a.m. to 10 a.m., the rainfall for the twenty-four hours was 1·225 inch.

THE death is announced of Xavier Ullesberger, the well-known palæontologist, at Ueberlingen, on the Lake of Constance, at the age of seventy-nine. His special distinction is the discovery of the lacustrine villages in the Lake of Constance, at Nussdorf, Maurach, Uhldingen, and Sipplingen. The extensive collection of Celtic and prehistoric objects which he got together in the course of his researches is preserved at Stuttgart.

THE death of Mr. Edwin Ormond Brown, Assistant-Chemist to the War Department, occurred on Saturday last. Mr. Brown had been engaged in the chemical establishment at the Royal Arsenal, Woolwich, for about thirty years, and has been instrumental in the improvement of gun-cotton and other explosives, besides rendering useful services in other matters.

THE Earl of Iddesleigh gave a very sensible address the other evening to the students of the Exeter Science Classes, of which he is the patron. He spoke of the rapidly growing feeling in favour of scientific education, and of the results it had already accomplished for the individual and the nation. After referring to misconceptions as to what technical education really meant, the Earl said:—"There is no doubt that by technical education you may mean one of two things. You may either mean an advance in the teaching of the scientific principles on which industrial processes depend, or you may mean the teaching of those processes practically and illustratively in the school rather than in the workshop. There is a very great difference between these two things. It may be desirable at times to give a certain amount of instruction in schools of a practical character, but that must not be pushed very far. It must be borne in mind that technical teaching, to be of real service, must be obtained in the course of work itself in the workshop. But with regard to the principles on which the processes of manufacture rest, those principles can only be taught in the schools, and it is to the development of those principles that we ought to pay most attention. That is the advantage which in schools of this kind you get. The study of pure and unapplied science is interesting to every one, but it does not at once appear that it has a direct bearing on the pursuits and callings of those who indulge in it. I daresay you may have heard the old question put, 'What is the use of any purely scientific discovery?' For answer another question was asked, 'What is the use of a baby?' The question is, What will it turn out to be? How will it be brought up, and how will it be brought into play?" The Earl of Iddesleigh then quoted the following passage from Prof. Huxley:—"'If you could, in the first place, keep your elementary scholars long enough to put them through a fair training in the principles of which the application lies in the special direction of metallurgy, and, secondly, if you could secure that they should acquire a special proficiency in such subjects, I think that would be a most admirable thing to do; but I thing the way it would work out under the present conditions would be this, that you would have special classes set up to grind young fellows without any knowledge of principles in that which would be no better than a rule of thumb. I do not believe that that would be of the smallest good. I believe, on the contrary, that it would do endless harm, because there would be a sort of pre-supposition that these young men really had the knowledge which would enable them to advance and improve their methods, whereas in reality the knowledge they possessed would be nothing more than a little of the ordinary *technique* of their business varnished over with scientific phraseology.' What you want," the Earl said, "is what Prof. Huxley says you want, as far as you can get it—the education to turn upon the principles on which the true science is based, as distinguished from the application of the science. Having got that, you will find that there will be abundance of opportunity to apply to the arts which you are about to prosecute the principles you have learnt. These principles I believe to be the main secret of what is sometimes called scientific, and sometimes called technical, education."

THE Royal Geographical Society will hold a special meeting on Monday, December 21, to welcome Major Greely, the distinguished Arctic explorer, and hear him tell the story of his work and adventures in the Polar Seas.

THE Natural History Museum at Vienna has just been presented with 708 skulls collected through a series of years by Dr. Weissbach, who was for a long time director of the Austro-Hungarian hospital at Constantinople, and was a very distinguished anthropological investigator. Of the collection 195 are pure Turkish skulls, 131 Greek, 96 Servian or Croat, 48 Hungarian, 43 Armenian, 29 old Byzantine. There are also skulls of Maronites, Albanians, Koords, Asiatic Jews, &c.

AS No. 1 of the Special Papers of the Alabama Weather Service we have an interesting record of the weather from 1701 to 1885, by Capt. W. H. Gardner, of Mobile.

SCIENCE has, on the whole, no reason to quarrel with the results of the General Election. Sir Lyon Playfair is returned by a large majority for one of the divisions of Leeds; and Sir John Lubbock again takes his seat unopposed for London University. A new and very important addition to the ranks of scientific men in Parliament is Sir Henry Roscoe, who was elected after a sharp contest for South Manchester. His election is striking in one way, for he is the only Liberal returned in the six divisions of Manchester. Sir Edward Reed retains his former seat for Cardiff, but Prof. Rücker, of the Yorkshire College, failed to win a place. The medical profession is even more strongly represented than it was before. Dr. Foster, Professor of Medicine in the Queen's College, Birmingham, was elected for Chester, and Mr. Erichsen, the eminent surgeon, is a candidate for Edinburgh and St. Andrews Universities. Even amongst the followers of Mr. Parnell, there are members of the medical profession. Mr. Ernest Hart, however, has been rejected by Mile End.

WE have received the last part (vol. i. part 5) of the *Proceedings* of the Perthshire Society of Natural Science for the session 1884-85. It contains a series of museum notes, by Dr. Buchanan White, the indefatigable President for the current year, which are intended to form a guide to the museum subsequently; two papers on the comparative anatomy of the teeth, by Mr. James Stewart; the diatoms of the Tay, by Dr. Trotter; shells—their structure, growth, and use, by Mr. Coates; the climate of the British Islands, with special reference to Perthshire, by the Rev. A. Campbell; the native timber trees of Perth, by Mr. Lindsay; and some minor contributions. The presidential address is devoted to the exceedingly practical subject of explaining why the growth of the museum renders considerable increase of space necessary, and the cost of the consequent building operations. An appeal is also made for further specimens for the Perthshire collection of natural history—an appeal which we trust may meet with adequate response, for apart from the general public benefit of local museums as centres of education throughout the country, they are of universal scientific importance when they are made the depositories of specimens of the natural history, past and present, of their respective neighbourhoods. But to be of the fullest value in this respect, they must be made as complete as possible.

MESSRS. MACMILLAN AND CO. will publish next week the Essex Field Club Report on the East Anglian Earthquake of April 22, 1884. This Report has been drawn up by Prof. Raphael Meldola and Mr. William White, and will contain maps and several illustrations.

THE jubilee volume of the Statistical Society will shortly be published by Mr. Stanford, of Charing Cross. It will contain the proceedings of the jubilee meeting of the Society held in June last, and will comprise valuable papers by the President, Sir Rawson W. Rawson, Dr. F. J. Mouat, M. Levasseur, and Prof. Neumann-Spallart, at whose initiation the International Statistical Institute was then founded.

FRESH earthquake shocks have been felt in the district round M'sila during the last week and principally on Saturday, December 12. They have also been felt at Bordj-ban-areridj and Setif. The new road from Setif to Bordj has been cut by rocks falling from the surrounding mountains. A bridge has been destroyed and a railway station demolished. It is impossible up to this time to state whether the commotion originated in the Atlas or in the Hodna region, where M'sila is placed, not far from a large Sebbha, which, although almost dry in summer-time, contains a large quantity of water in rainy periods. Official documents will be sent to the Paris Academy of Sciences as soon as collected, but it is feared they will lack precision; no seismograph, so far as our knowledge goes, having been established in Algiers.

ADVICES from Smyrna in Asia Minor to November 25 state that a series of earthquakes, commencing on the 13th, had up to that time been felt at Denizlu in the interior, about 200 miles to the east. These disturbances were slight, but accompanied with subterranean noises.

WE are glad to see that the extremely valuable meteorological observations which were made at Sagastyr, the Arctic Station in the Delta of the Lena, during the years 1882 to 1884, are already being published. The last issue of the *Izvestia* of the Russian Geographical Society contains a preliminary report, by M. Yurgens, and several meteorological tables, namely, the observations in full, from September 1, 1882, to September 1, 1883, of the barometer, temperature of the air, of the surface of the soil and of the snow, and of the soil at a depth of 1 metre, the relative humidity, force of wind, and nebulosity; and the monthly averages of the above for each hour of the day. The magnetical observations for the same period are being calculated, as also those for 1883 and 1884. The whole, together with observations of the temperature of the soil at depths of 80 and 160 centimetres, temperature and density of water, tides, and auroræ will be published in a separate form. A map of the Delta of the Lena, based on new surveys, and a plan of the station accompany the report of M. Yurgens, which is very interesting, as it contains many details as to the life at the station, and varied information as to the Delta, and the excursions made during both summers. It is worthy of notice that the meteorologists of the station, although lost amidst tundras in the 73rd degree of latitude, were not so secluded from the world as might have been supposed. They received letters regularly from Yakutsk, together with newspapers and reviews, which reached them four months after their publication at St. Petersburg—a delay which is not so great if it be taken into account that letters take nearly one month to reach Irkutsk, the capital of Eastern Siberia. As to the frozen mammoth whose remains were explored by Dr. Bunge, only pieces of bones and traces of the contents of the stomach were found and brought to St. Petersburg.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus* ♀), two Grey-breasted Parrakeets (*Bolborhynchus monachus*) from Monte Video, presented by H.R.H. the Prince of Wales, K.G.; two West Indian Agoutis (*Dasyprocta cristata*) from West Indies, presented by T.R.H. Prince Albert Victor and Prince George Frederick of Wales; a Macaque Monkey (*Macacus cynomolgus* ♂) from India, presented by Mr. Jan Smidt; a Pig-tailed Monkey (*Macacus nemestrinus*) from Java, presented by Miss Ethel Rodger; two Tigers (*Felis tigris*) from Hyderabad, presented by the Nawab Salar Jungh Bahadur; a Cheetah (*Cynælurus jubatus*) from Afghanistan, presented by the Nawab Mahomed Hassan Ali Khan; a Tiger (*Felis tigris*).

OUR ASTRONOMICAL COLUMN

BRIGHT LINES IN STELLAR SPECTRA.—Mr. O. T. Sherman has continued his researches on the spectra of γ Cassiopeiæ and and β Lyræ, and announces in the *American Journal of Science* for December the discovery of no fewer than fifteen in the spectrum of the former star, and seventeen in that of the latter. The lines seen in γ Cassiopeiæ are as follows:—Hα, λ 6356, 6160, D_3, λ 5840, 5557·5, 5422, 5309·8, 5167·5, 4990, Hβ, λ 4623, Hγ, λ 4180, and Hδ, bright lines; and λ 6280, 5760, 5020, 4920, 4673·5, and 3993, dark lines. The bright lines agree closely in position with the principal lines observed by Prof. Young in the spectrum of the chromosphere.

Mr. Sherman has also examined a large number of other stars, and "*in each case* many or few bright lines have been seen, lines, so far as I know, formerly unsuspected." It is clear, if Mr. Sherman's observations can be satisfactorily confirmed, that we have here a most important discovery; but looking to the fact that these stars have probably been frequently observed by experienced spectroscopists without any bright lines being detected in them, whilst a false appearance of bright lines is readily produced in stellar spectra under certain circumstances, it would appear hazardous to accept Mr. Sherman's result without further evidence.

PHOTOMETRY OF THE PLEIADES.—A valuable memoir (*Mémoires de l'Academie Impériale des Sciences de St. Pétersbourg*, vii. série, tome xxxii. No. 6) by Herr Ed. Lindemann of Pulkowa, "On the Magnitudes of Bessel's Stars in the Pleiades," has recently reached us. A special point of interest lies in the fact that Profs. Pickering and Pritchard have likewise determined the brightness of many of these stars with their respective photometers, each assuming the magnitude of Merope, to which the other stars of the group are referred, as 4·22. Herr Lindemann has also adopted the same magnitude for Merope, which he has used as his standard star. He also employed, as reference stars, Celæno and Anon 32, the magnitudes of which he had determined to be 5·27 and 6·51 respectively. The stars, fifty-two in number, were each observed on two separate nights, only one star of the fifty-three observed by Bessel proving too faint for Herr Lindemann's telescope of five inches aperture. Comparing his own results with those of Profs. Pickering and Pritchard, Herr Lindemann finds, on the whole, a very gratifying agreement; twenty-five stars observed by Prof. Pickering showing a mean excess over the Pulkowa observations of 0·04 of a magnitude, and thirty-three stars observed by Prof. Pritchard giving a mean excess of 0·05. Prof. Pritchard's later observations give a yet smaller difference, viz. 0·01 of a magnitude. When it is remembered that the three photometers employed—Herr Lindemann using a Zöllner photometer—differed entirely in principle, construction, and method of employment, this close agreement would seem to indicate that each may be relied upon with very considerable confidence, when the differences of stellar magnitude determined by their means are not very great. The stars Nos. 1, 4, 21, 31, and 33 would appear to be variable, and possibly two others likewise. Pogson's scale has been employed for the conversion of the logarithm of the light of the star into magnitude.

FABRY'S COMET.—The following elements and ephemeris have been computed for this comet by Dr. H. Oppenheim:—

Perihelion Passage, 1886 March 9·7944 Berlin M.T.

$$\left.\begin{aligned}\omega &= 132^\circ\ 36'\ 19''\\ \Omega &= 32\ \ 17\ \ 32\\ i &= 47\ \ 18\ \ \ 0\end{aligned}\right\}\text{Mean Eq. 1885·0}$$

$$\log. q = 9·69654$$

Errors of the middle observation :—

$\delta\lambda = -8''$ $\delta\beta = -2''$.

Ephemeris for Berlin Midnight

1885	App. R.A. h. m. s.	App. Decl. ° ′	Log. Δ	Brightness
Dec. 20 ...	23 59 57 ...	+20 43·1 ...	0·0837 ...	1·4
22 ...	56 49 ...	44·3		
24 ...	53 52 ...	46·2 ...	0·0844 ...	1·5
26 ...	51 5 ...	49·0		
28 ...	48 29 ...	52·6 ...	0·0849 ...	1·6

The brightness on December 1 is taken as unity. The above elements differ considerably from those published by Dr. S. Oppenheim in the *Vienna Circular*, No. lvi., but appear to represent the observations better.

ASTRONOMICAL PHENOMENA FOR THE WEEK, 1885, *DECEMBER* 20–26

(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 December 20

Sun rises, 8h. 5m. ; souths, 11h. 57m. 59·2s. ; sets, 15h. 50m. ; decl. on meridian, 23° 27′ S. : Sidereal Time at Sunset, 21h. 48m.

Moon (Full on Dec. 21) rises, 15h. 17m. ; souths, 23h. 5m. ; sets, 6h. 58m.* ; decl. on meridian, 17° 48′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury ...	7 36 ...	11 45 ...	15 54 ...	20 58 S.
Venus ...	10 47 ...	15 17 ...	19 47 ...	17 41 S.
Mars ...	22 46* ...	5 25 ...	12 4 ...	7 2 N.
Jupiter ...	0 22 ...	6 23 ...	12 24 ...	0 38 S.
Saturn ...	16 19* ...	0 28 ...	8 37 ...	22 29 N.

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

Occultations of Stars by the Moon

Dec.	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image ° °
20 ...	γ Tauri ...	... 4 ...	4 11	near approach	44 —
20 ...	B.A.C. 1526	... 6 ...	17 6	near approach	151 —
21 ...	111 Tauri...	... 5½ ...	5 6 ...	5 56 ...	113 325
21 ...	117 Tauri...	... 6 ...	6 35 ...	7 6 ...	77 353
21 ...	B.A.C. 1930	... 6½ ...	17 46 ...	18 37 ...	38 253
25 ...	ξ Leonis ...	... 6 ...	3 55 ...	5 5 ...	80 276
26 ...	48 Leonis ...	... 6 ...	6 50 ...	7 48 ...	123 258

Phenomena of Jupiter's Satellites

Dec.	h. m.		Dec.	h. m.	
21 ...	1 35	II. occ. reap.	22 ...	5 54	I. tr. egr.
21 ...	5 17	I. ecl. disap.	22 ...	6 30	III. occ. reap.
22 ...	1 34	III. ecl. reap.	23 ...	3 14	I. occ. reap.
22 ...	3 35	III. occ. disap.	24 ...	0 22	I. tr. egr.
22 ...	3 38	I. tr. ing.	26 ...	7 11	II. tr. ing.

The Occultations of Stars and Phenomena of Jupiter's Satellites are such as are visible at Greenwich.

Dec.	h.	
21 ...	— ...	Sun at greatest declination south ; shortest day in northern latitude.
22 ...	5 ...	Saturn in conjunction with and 3° 58′ north of the Moon.
26 ...	11 ...	Saturn in opposition to the Sun.

Variable Stars

Star	R.A. h. m. s.	Decl. ° ′		h. m.
β Lyræ ...	... 18 45 50 ...	33 13·8 N. ...	Dec. 25,	0 0 *m*
R Lyræ ...	... 18 51 50 ...	43 47·7 N. ...	,, 26,	*m*
χ Cygni ...	... 19 46 9 ...	32 37·4 N. ...	,, 26,	*M*
η Aquilæ	... 19 46 37 ...	0 42·7 N. ...	,, 21,	17 0 *M*
δ Cephei	... 22 24 54 ...	57 49·6 N. ...	,, 23,	4 0 *M*
,,			,, 26,	23 0 *m*
R Andromedæ.	0 17 58 ...	37 56·4 N. ...	,, 23,	*M*
Algol	3 0 41 ...	40 30·7 N. ...	,, 22,	3 51 *m*
,,			,, 25,	0 40 *m*
ζ Geminorum...	6 57 17 ...	20 44·3 N. ...	,, 23,	19 0 *M*
δ Libræ ...	... 14 54 50 .	8 3·7 S. ...	,, 22,	19 5 *m*
,,			,, 25,	2 56 *m*

M signifies maximum ; *m* minimum.

Objects with Remarkable Spectra

Mr. O. T. Sherman, of Yale College Observatory, has recently called fresh attention to the spectra of γ Cassiopeiæ and β Lyræ, as he finds no fewer than seventeen bright lines in each. Both stars should therefore be examined as frequently and carefully as possible. β Lyræ is at minimum about midnight on Dec. 25.

There is an uncertainty about the ephemeris of R Andromedæ which renders observations of its brightness a matter of importance ; its spectrum, whilst resembling in several particulars that of the third type, possessing so many special characteristics, that it deserves the most careful attention directly the star has attained a sufficient magnitude.

THE RETURN OF THE LEONIDS IN 1885

Between November 5 and 13 inclusive we had densely overcast skies, so that no observations could be obtained here.

On November 14 weather improved, but it was not until the morning of the 15th that the clouds completely dispersed and enabled that uninterrupted view of the firmament which is so necessary to the successful recording of meteors. The three following nights were also brilliantly clear, though the severe frosts which occurred rendered open-air watching somewhat trying. I have summarised my results for the four mornings as follows :—

Date, 1885	Time of obs. a.m. h. h.	Actual duration of obs. h.	Meteors seen	Leonids	Radiant point ° °
Nov. 15 ...	3½ to 6 ...	2¼ ...	24 ...	4 ...	149 +21
16 ...	0¾ to 3 ...	2 ...	23 ...	0 ...	—
17 ...	0¾ to 4¾ ...	3½ ...	42 ...	6 ...	150 +22
18 ...	2¾ to 5¼ ...	2¼ ...	25 ...	1 ...	—
Nov. 15–18...	0¾ to 6 ...	10 ...	114 ...	11 ...	149½ +21½

Making certain allowances for the intervals occupied in registering the paths, &c., meteors fell at the rate of about 14 per hour for one observer. Of the total number seen 11 only belonged to the special shower of Leonids. The proportion of the latter to the meteors visible from all other streams was therefore as 1 to 10·4. Six of the Leonids appeared in Leo with much foreshortened tracks close to their radiant point, which admitted of very accurate determination. As usual, they left lines of phosphorescence which in several instances brightened most perceptibly about one or two seconds after the extinction of the nucleus. I have frequently noticed this after-glowing of the streaks which are so commonly generated by the swift meteors discharged from the radiants near the apex of the earth's way.

On the morning of the 15th the Leonid's furnished about two meteors per hour for one observer. On the 16th there was an apparent lull in the display, not one being observed. On the 17th there was a very distinct reappearance of the shower with the same relative intensity as on the 15th. On the 18th the shower had nearly become exhausted, for of 25 shooting-stars only one certainly could be assigned to the radiant in Leo.

It is extremely probable that the maximum took place, as it usually does, on the morning of the 14th, when unfortunately the sky was involved in clouds. But the observations now reported for the later nights of the display sufficiently prove there to have been a definite, though feeble, revival of the shower this year, and there can be no question that the Leonid meteor-orbit is continuous so far as our accumulating observations enable us to judge. Every November, as the earth crosses the node, meteors having the same radiant in the sickle of Leo are to be seen, and they exhibit all the characteristics typical of the Leonids during one of the major displays. There are doubtless some condensations in the orbit, giving rise to brighter showers in some years than in others, but a large number of further observations are required to determine the precise nature of these. There can be no doubt that there are certain occasions when fairly bright returns of these meteors pass wholly unobserved. Moonlight, cloudy weather, or the occurrence of a maximum in the daytime, may so much obliterate it as to induce entirely wrong impressions as to its comparative strength in successive years. We essentially require observers in widely different longitudes, and the continuity of annual records should be preserved as far as possible.

With regard to the display of the present year, the want of observations on November 12 and 13 will not allow us to form a safe judgment as to its character. Probably it has been below the average. As to the individual meteors recorded at this station, they were generally small, and, with one exception, need no comment. The brightest appeared at 5h. 3m. a.m. on November 15, and was estimated very nearly equal to Jupiter. Its path of 12° lay 5° south of β and γ Draconis, and it left a vivid streak there for about five seconds.

As to the numerous contemporary showers of this epoch, they are extremely interesting, and some of them were observed this year with unusual distinctness. In preceding years I have registered a considerable number of Taurids and Muscids at this epoch, but during my late observations not many were noticed. I was, however, watching a region of the heavens far removed from the radiant points of these showers—a fact which may in some measure explain the apparent paucity of their meteors. But on the 15th and 16th a few of the slow-moving Taurids were seen traversing long flights amongst the stars of Leo Minor and Ursa Major. At 2h. 20m. a.m. on November 16 one fell in a path of 17° slightly to the left, and very nearly parallel to the stars ζ and η of Ursa Major. It was brighter than Jupiter, and exhibited a small yellowish-white disk, varying in magnitude in a most curious manner. During its course the meteor appeared to halt and rekindle with increased impetus several times, and short trails of sparks were thrown off at the points of maximum brightness.

Below I give a summary of all the radiant points derived from my a.m. observations on November 15-18 last :—

No.	Radiant ° °	No. of meteors	No.	Radiant ° °	No. of meteors
1	149½+21½	11	7	213+75	8
2	154 +41	12	8	157+74	4
3	166 +31	10	9	79+56	6
4	144 +50	6	10	73+42	5
5	125 +41	6	11	190+21	3
6	60 +28	5	12	100+41	5

Nos. 2, 3, 4, and 5 are important. They severally furnish meteors of the swift streak-leaving class. No. 6 represents the centre of a few Taurids, and Nos. 7 and 8 are positions derived from slow meteors not very far from Polaris. Nos. 9 and 10 are a pair of sharply-defined radiants in Auriga, and No. 11 is an entirely new shower which I observed on the morning of the 17th and 18th. Only three of its meteors were recorded, but their paths intersect at a point, and I believe the position is reliable.

The radiant No. 2 at 154° + 41°, near μ Ursæ Majoris, is the best of all, and has often been seen in previous years. It is evidently the same as the "very active shower" described by me in NATURE, vol. xv. p. 158, as observed here from the point 155° + 36°, on November 20-28, 1876. It is also identical with the shower seen from 155° + 35° by Father Perry at Stonyhurst College Observatory on November 13-15, 1879 (*Monthly Notices*, January, 1880, p. 140). Not only in November, but in many other months, does this particular radiant point manifest itself. I have summarised the positions from September to December, though the shower is still sustained with equal definiteness until May :—

Radiant ° °	Epoch	Observer or authority
155+41	Sept. 8-October	D. From Zezioli's obs.
156+41	September 15-16	D. Obs. in 1877
153+42	October 16	D. Obs. in 1877
152+38	October 14	D. Obs. in 1879
160+40	November 7, 1869	Tupman (estimated)
149+38	November 1-15, 1872	D. From Italian obs.
157+46	November 11-15	Denza
155+35	November 13-15	Perry. Obs. in 1879
154+41	November 14-17	D. Obs. in 1885
155+36	November 25-27	D. Obs. in 1876
154+39	December 6-7	Backhouse
160+40	December 8-9	Backhouse
152+43	December 9-12	D. From various foreign obs.
149+45	December 9	Schiaparelli and Zezioli

The positions marked "D." are those resulting from my own observations or reductions. The two radiants at 160° + 40°, by Tupman and Backhouse, may possibly relate to another bordering shower, but the position is very close to the mean of all at

$$154°{\cdot}4 + 40°{\cdot}4.$$

This is a shower (or series of showers) which eminently stands in need of further investigation. The radiant appears to be stationary and continuous for a long period. The shower at 166° + 31°, 10° north of δ Leonis, which I detected this year, has escaped me before, though it was seen at Stonyhurst in 1879, November 13-15, at 166° + 22°. As to position No. 5 in my present list, I saw that well on November 12, 1877, at 125° + 40°. With reference to the radiant No. 4 at 144° + 50°. close to θ Ursæ, I have not recognised it in November until this year, but in October last I determined a good radiant at 143° + 49° from meteors seen in the morning sky.

These circum-Leonid streams reappear with more or less distinctness every year, and their radiant points are sharply defined. It would be well to thoroughly study the durations of several of them, now that their positions have been ascertained with a considerable degree of accuracy by several observers.

Bristol, November 19 WILLIAM F. DENNING

NOTES FROM THE OTAGO UNIVERSITY MUSEUM

VII.—*On some Models Illustrating Phyllotaxis*

PHYLLOTAXIS is a subject which presents special difficulties to the student when illustrated only by diagrams and by actual specimens of plants. With these aids alone it

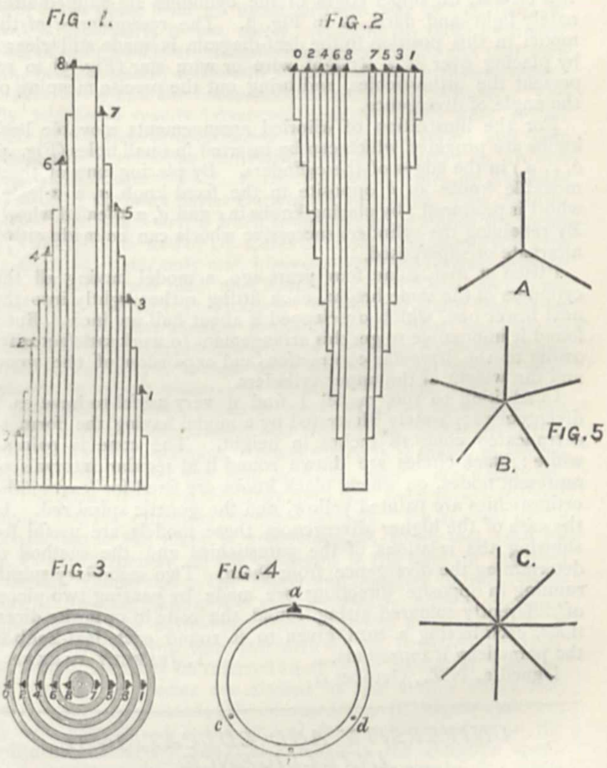

FIG. 1.—Vertical section of model, one-eighth actual size; 0-1, the leaf knobs.

FIG. 2.—The same, telescoped, the upper edges of all the cylinders being brought to one level.

FIG. 3.—The same, telescoped, and viewed from above.

FIG. 4.—One of the cylinders from above: *a*, the fixed leaf-knob; *b*, *c*, *d*, holes for the insertion of movable knobs.

FIG. 5.—Wire stars to represent orthostichies: A, for divergence of one-third; B, for two-fifths; C, for three-eighths.

entails an expenditure of time, out of all proportion to the importance of the subject, to make clear in a lecture or demonstration the principles of leaf-arrangement and the mode of construction of the leaf-diagram.

I find the model about to be described of great assistance in explaining these matters. It consists of a "nest" of nine concentric cylinders, made of some hard wood (Fig. 1), the outermost 2 inches high, the others increasing in height by regular increments of 2 inches up to 18 inches. The cylinders are ¼ inch thick, except the innermost, which is a solid rod 1 inch in diameter: they are made so as to slide easily upon one another in any direction. The exposed portion of each cylinder, 2 inches in length, represents an internode, its upper edge a node. On the upper edge of each is fixed a small wooden knob (Fig. 1, 0–8) representing a leaf-insertion, and of such a size as to project slightly beyond the cylinder to which it is attached. By revolving the cylinders these knobs can be made to take up positions representing any divergence from ½ to ⅜; higher divergences would of course require a greater number of cylinders and consequently a larger and more unwieldy apparatus. In the figures the cylinders are shown adjusted to a divergence of ⅜. The adjustment is facilitated by holding above the model a straight wire in the case of ½ divergence, or a 3-, 5-, or 8-rayed wire star (Fig. 5, A, B, C) in the case of higher divergence. The genetic spiral is best shown by winding a piece of string round the model.

To explain the construction of the leaf-diagram, the whole apparatus is "telescoped" by simply lifting the lower cylinder: as the latter is raised each cylinder in turn is caught by the projecting portion of the leaf-knob of the next higher cylinder, until finally the upper edges—*i.e.* the nodes—of all nine are brought to one level, or in other words, the internodes are suppressed (Fig. 2). Then by observing the model end-on (Fig. 3), nine concentric circles are seen, each representing a node, and having a leaf-insertion in the appropriate position. To make this clearer, the upper edges of the cylinders are painted alternately light and dark, as in Fig. 3. The resemblance of the model in this position to the leaf-diagram is made still clearer by placing over it a straight wire or wire star (Fig. 5) to represent the orthostichies, and bring out the precise meaning of the angle of divergence.

For the illustration of whorled arrangements movable leaf-knobs are provided which can be inserted in small holes (Fig. 4, *b*, *c*, *d*) in the edges of the cylinders. By placing one of these movable knobs in *b* opposite to the fixed knob *a*, a 2-leafed whorl is produced; by placing knobs in *c* and *d*, a 3-leafed whorl. By revolving the cylinders successive whorls can be made either alternate or superposed.

I tried at first, some four years ago, a model having all the cylinders of the same height, each fitting rather tightly into the next lower one, which overlapped it about half an inch. But I found it impossible to get this arrangement to work satisfactorily, owing to the irregular contraction and expansion of the wood and the weight of the upper cylinders.

In addition to this model I find it very useful to have each divergence separately illustrated by a model having the form of a truncated cone 18 inches in height. The cone is painted white: black circles are drawn round it at regular intervals to represent nodes, on which black knobs are fixed for leaves: the orthostichies are painted yellow, and the genetic spiral red. In the case of the higher divergences these models are useful for showing the relations of the parastichies and the method of determining the divergence from them. Two secondary spirals running in opposite directions are made by passing two pieces of differently coloured string round the cone in opposite directions, each having a turn given to it round each leaf-knob in the parastichy it represents. T. JEFFERY PARKER

Dunedin, N.Z., October 9

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—Dr. Vines has been appointed Honorary Secretary of the General Board of Studies for business connected with the lists of lectures issued by the Board.

Dr. A. S. Lea, Fellow of Gonville and Caius College, has been approved for and admitted to the degree of Doctor in Science.

A temporary iron dissecting-room for Human Anatomy is to be erected on space adjacent to the present Anatomical Schools. Over 145 men are engaged in dissection this term.

The honorary degree of Master of Arts has been conferred on Mr. Walter Heape, Demonstrator of Animal Morphology.

The Public Orator (Mr. J. E. Sandys), in presenting Mr. Heape for the degree, observed that Mr. Heape, *quantum inter lucrum et laudem intersit expertus*, had relinquished the brilliant prospects open to him in a mercantile career and had deliberately preferred to devote himself to scientific pursuits. In 1879 he was attracted to Cambridge by the high reputation of the late Mr. F. M. Balfour, and after working with him for three years in the newly-founded Morphological Laboratory, he had, during the three years that had elapsed since Prof. Balfour's lamented death, done good service as Demonstrator of Animal Morphology. In prosecuting his favourite studies he had considered nothing too small, nothing too great, for his attention; he had not only investigated the early development of the mole, but had also secured for the University Museum an exceedingly rare specimen of a wild male African elephant, which he had killed with his own hand in the South of Africa. "Magnum profecto est ultimâ ex Africâ spoliis opimis onustum redîsse; laudem vero majorem eidem distinat virtus—

"Diadema tutum
Deferens uni, propriamque laurum,
Quisquis ingentes oculo irretorto
Spectat acervos."

An examination for two minor Scholarships will be held at Downing College on June 1, 2, and 3, 1886. The examination in Natural Sciences will include most of the subjects of the Natural Sciences Tripos except Geology and Mineralogy, but no one will be examined in more than three subjects, and great weight will be attached to proficiency in one subject.

The Clothworkers' Exhibition of 52*l.* 10*s.* a year for Physical Science has been awarded to Mr. G. A. Shaw; the Exhibition of 30*l.* to Mr. J. Morgan.

The Antiquarian Museum is growing in value by the donations, both general and local, which it has received. The Curator, Baron von Hügel, has accomplished much in the arrangement of the objects, and has himself deposited valuable collections on loan.

SCIENTIFIC SERIALS

THE *Botanical Gazette* (Indianapolis) for September and October is chiefly occupied by a report of the papers read in the Botanical Section of the American Association for the Advancement of Science at its Ann Arbor meeting. These furnish satisfactory evidence of the good work doing in this branch of science on the American continent, and will not suffer from comparison with a similar record at any of the recent meetings of our own Association. The following are the titles of the papers read:—J. C. Arthur, proof that Bacteria are the direct cause of the disease in trees known as pear-blight.—C. R. Barnes, the process of fertilisation in *Campanula americana*.—C. E. Bessey, the question of bi-sexuality in the Zygnemaceæ.—C. E. Bessey, further observations on the adventitious inflorescence of *Cuscuta glomerata*.—T. J. Burrill, the mechanical injury to trees by cold.—D. H. Campbell, the development of the prothallia of ferns.—J. M. Coulter, on the appearance of the relation of ovary and perianth in the development of dicotyledons.—W. G. Farlow, notes on some injurious fungi of California.—E. L. Sturtevant, an observation on the hybridisation and cross-fertilisation of plants.—E. L. Sturtevant, germination studies.—As far as these papers are reported here, we may note Mr. Arthur's, Mr. Barnes's, Mr. Campbell's, and Mr. Coulter's as giving especially good evidence of a capacity for original work. Another interesting feature of this number is the report of the proceedings of the "Botanical Club," which held daily meetings during the session of the Association, with an attendance in all of no less than eighty-five members.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, November 19.—"On Variations in the Amount and Distribution of Fat in the Liver-Cells of the Frog." By J. N. Langley, M.A., F.R.S., Lecturer on Histology in the University of Cambridge.

The fat in the liver-cells is at its maximum amount in February and March. In April it rapidly decreases; from May until December it is present in comparatively small though varying amount.

Generally speaking, the fat globules form an inner zone in frogs which have hungered more than a week. In January, February, and March, however, the fat-globules are commonly more numerous in the outer part of the cells, often forming a distinct outer zone.

In December, when the fat in the liver is increasing in amount, cold increases the amount of fat stored up, and warmth decreases it.

The increase of fat, consequent on a decrease of temperature, occurs chiefly in the outer part of the cells.

The decrease of fat, consequent on increase of temperature, occurs chiefly or wholly at the outer part of the cells; as a rule, the number of globules in the inner part of the cells is increased.

Variations of temperature have much greater effect on the amount of fat in the liver in winter than in summer.

The ratio of fat formed to fat metabolised, depends in part upon certain unknown conditions of the body, independent of temperature or of food.

When frogs are fed, *e.g.* with worms, the fat in the liver at first decreases; after some hours it begins to increase, and becomes greater than at the beginning of digestion; towards the end of digestion it decreases again in amount, so that in one or two days the amount is normal. Whilst the fat is decreasing in amount, the globules usually decrease in size; whilst the fat is increasing in amount, the globules usually increase in size, and are found in the outer region of the cells. Later, as the fat returns to normal, the globules form more and more an inner zone.

Probably the metabolism as well as the formation of fat is more rapid in the outer than in the inner cell-region; and probably also there is in certain circumstances a transference of fat-globules from the outer to the inner part of the cells.

Each separate fat-globule appears to be slowly metabolised in the same way that mesostate granules in secretory glands are metabolised.

From June to August, peptone or dextrin, when injected into the dorsal lymph-sac of a frog, produces changes like those produced by feeding.

Mathematical Society, December 10.—J. W. L. Glaisher, F.R.S., President, in the chair.—Mr. A. E. Haynes, Hillsdale College, Michigan, was elected a Member.—The following communications were made:—On the numerical solution of cubic equations, by G. Heppel.—On a theorem in kinematics, by J. Y. Walker, F.R.S.—Note on the induction of electric currents in an infinite plane current sheet which is rotating in a field of magnetic force, by A. B. Basset.

Chemical Society, November 19.—Dr. Hugo Müller, F.R.S., President, in the chair.—The following papers were read:—Aluminium alcohols; part 3, aluminium ortho-cresylate and its products of decomposition by heat, by J. H. Gladstone, F.R.S., and Alfred Tribe.—Notes on the constitution of hydrated and double salts, by Spencer U. Pickering.—Some new vanadium compounds, by J. T. Brierly.—On the action of PCl_5 upon ethylic diethylacetoacetate, by J. W. James.—On the vapour-pressures of mercury, by W. Ramsay, Ph.D., and Sydney Young, D.Sc. After criticising Regnault's determinations of the vapour-pressures of mercury, the authors show that his results do not agree with the following generalisation, which has been proved to be true in twenty-two instances. A relation exists between the absolute temperatures of all bodies, whether solid or liquid, whether stable or dissociable, which may be expressed in the case of any two bodies by the equation

$$R' = R + c(t' - t),$$

where R is the ratio of the absolute temperatures of the two bodies corresponding to any vapour-pressure, the same for both; R′ is the ratio at any other pressure, again the same for both; c is a constant which may equal 0, or a small plus or minus number; and t' and t are the temperatures, absolute or Centigrade, of one of the bodies corresponding to the two vapour-pressures. When $c = 0$, $R' = R$, or the ratio of the absolute temperatures is a constant at all pressures; and when $c > 0$ or $c < 0$, its values may readily be determined either by calculation, or graphically by representing the absolute temperatures of one of the two bodies as ordinates, and the ratios of the absolute temperatures at pressures corresponding to the absolute temperatures of that body as abscissæ. It is found in all cases that points representing the relation of the ratio of the absolute temperatures of the two bodies to the absolute temperatures of one of them lie in a straight line. From this it follows that if the vapour-pressures of any one substance are known throughout, it is sufficient to determine accurately the vapour-pressures of any other substance at any two temperatures, sufficiently far apart, in order to be able to construct its whole vapour-pressure curve. The vapour-pressures of mercury have accordingly been measured with the greatest care at the temperatures 222°·15 C., 270°·3, 280°·2, 447°, and 448°. On comparing the ratios of the absolute temperatures of mercury and water, at pressures corresponding to those temperatures, they are found to agree with the equation $R' = R + c(t - t')$, where $c = 0{\cdot}0004788$, if the temperatures of mercury be chosen as ordinates. It is therefore possible to construct the complete vapour-pressure curve of mercury; the paper contains tabular statements of the values.

Linnean Society, December 3.—Sir J. Lubbock, Bart., President, in the chair.—Sir H. E. Maxwell, Bart., Lieut.-Col. L. Blathwayt, and Messrs. R. A. Bastow, S. J. Capper, C. Ford, G. B. Howes, J. H. Gurney, jun., W. H. Jones, W. F. A. Lambert, C. T. Musson, W. D. G. Osborne, D. Petrie, and G. Thom were elected Fellows.—The President announced from the chair, and there were read letters from (1) the Elizabeth Thompson Science Fund, U.S.A.; (2) Prix de Candolle; (3) Medals and money prizes of Roy. Soc. N.S. Wales.—The Secretary exhibited for M. Buysman a preparation of the floral parts of *Aconitum Napellus*, L.—Mr. V. I. Chamberlain exhibited and made remarks on a specimen of trap-door spider and nest from California.—D. C. Cogswell showed oil-paintings of *Eugenia jambos* and *Casparea porrecta* from Bermuda.—The Rev. G. Henslow read a contribution to the study of the relative effects of different parts of the solar spectrum on the transpiration of plants. His conclusions are: that his experiments prove that Wiesner's results are correct, and that transpiration *per se* (theoretically distinct from the purely physical process of evaporation, which takes place from all moist surfaces and bodies, dead or alive) is especially, if not solely, referable to those particular bands of light which are absorbed by chlorophyll, and that such light, being arrested, is converted into heat, which then raises the temperature within the tissues and causes the loss of water. The only additional results advanced tentatively are, that yellow light has a *retarding influence* upon transpiration, and that "life" has a retarding influence upon evaporation as distinct from transpiration.—Prof. T. S. Cobbold's notes on parasites collected by the late Charles Darwin was next read. This contains a letter from Mr. Darwin when transmitting the author the specimens in 1869, followed by Dr. Cobbold's own memoranda concerning eight of them, only one, however, *Distoma incerta*, proving new.—A paper was read, on *Castilloa elastica*, Cerv., and some allied plants, by Sir J. D. Hooker. The author states that under the name *Castilloa elastica* probably more than one species exists. The true plant first described by Cervantes has flowered and fruited in Ceylon; it is now fully described and figured, with remarks on allied plants also yielding Panama india-rubber. Seeds collected by Mr. Cross in 1875 failed to germinate, but cuttings were also introduced, and from them plants were distributed to various colonies. Some difficulty is found in propagating by cuttings, as the side branches, which are deciduous, will not strike root, but seedlings have now been raised at Peradenyia, and the culture is therefore assured. An account of the introduction of the plant is appended.—A paper was read by Mr. P. H. Carpenter, on the variations in the form of the cirri in certain Comatulæ. The shape and number of the cirrus-joints of *Antedon phalangium* vary so greatly, both in the same individual and in individuals from different localities, that, if the two extreme forms were met with in an isolated condition, they would assuredly be referred to different species of *Antedon*. The cirri of this species are classed by the author under four types:—(A) long-jointed, (B) intermediate, (C) square-jointed, (D) short-jointed. A is the typical form which occurs in the Mediterranean variety, but is also found in the Atlantic specimens, together with B, and also, but more rarely, C; while D is confined to individuals from the Minch and the Ross-shire coast, occurring together with C, which is rare in examples from the Atlantic, except in those dredged by the *Dacia* on the Seine bank.—The Secretary summarised a paper, by Mr. Joseph Baly, on the Colombian species of the genus Diabrotica, and in which the author divides the genus into two sections, dependent on the lengths of the second and third joints of the antennæ.

Zoological Society, December 1.—Prof. W. H. Flower, V.P.R.S., President, in the chair.—Mr. F. Day exhibited and made remarks on a very curious fish, supposed to be a hybrid between the Dab (*Pleuronectes limanda*) and the Flounder (*P. flesus*).—Mr. Sclater laid on the table specimens of some rare birds sent for exhibition by Mr. Whitely, of Woolwich, and called special attention to a Hornbill which seemed to prove that *Buceros casuarinus*, described by Mr. G. R. Gray in 1871 from the head only, was merely the young stage of *Bycanistes cylindricus*.—Mr. E. Lort Phillips exhibited a fine series of heads of antelopes obtained during his recent expedition to Somali-land in company with Messrs. James, and read notes on their habits and localities.—Mr. W. T. Blanford exhibited, on behalf of Capt. C. S. Cumberland, the head of a wild sheep from Ladak, supposed to be a hybrid between *Ovis hodgsoni* and *Ovis vignei*.—Mr. John Bland Sutton read a paper on the origin of the urinary bladder, in which he endeavoured to show that the atrophy of the gills in all forms of the vertebrates above the amphibians might possibly be explained by the assumption of embryonic respiration by the allantois.—A communication was read from Lieut.-Col. Swinhoe, containing the fourth part of his memoir on the Lepidoptera of Bombay and the Deccan. The present paper concluded his description of the Heterocera; and also contained descriptions of the Tortricidæ and Tineina, which had been worked out by Lord Walsingham.—A communication was read from Dr. R. W. Shufeldt, containing a memoir on the comparative osteology of the Trochilidæ, Caprimulgidæ, and Cypselidæ. Dr. Shufeldt came to the conclusion that the Trochilidæ should form an order by themselves, and were not nearly related to the Cypselidæ, which were only much modified Passeres.—Mr. F. E. Beddard read the second of his series of notes on the Isopoda collected during the voyage of H.M.S. *Challenger*. In the present paper the author treated of specimens referable to the family Munnopsidæ.—A communication was read from Mr. Martin Jacoby, containing descriptions of some new species and a new genus of Phytophagous Coleoptera.

Physical Society, November 28.—Prof. F. Guthrie, President, in the chair.—Mr. T. H. Blakesley was elected a Member of the Society.—The following communications were read:—On the calibration of galvanometers by a constant current, by Mr. T. Mather. A current is passed through the coils of a galvanometer, which may be of any form; the galvanometer is turned in a horizontal plane through any angle, which need not be recorded, and the deflection θ of the needle noted. The current is then broken, and the needle swings back, taking up its position in the magnetic meridian; the angle through which it turns to do this is also noted δ. This is repeated with the galvanometer in various positions and with the same current, and a curve is drawn showing the relation between the values of $\frac{\sin\theta}{\sin\delta}$ and corresponding values of θ. When the instrument is now used in its normal position it is readily seen that a current producing a deflection θ of the needle is proportional to the value of $\frac{\sin\theta}{\sin\delta}$ corresponding to θ, obtained in the calibration experiment which may be read off at once from the curve.—On a machine for the solution of cubic equations, by Mr. H. H. Cunyngham. This machine the author believes to be the only one hitherto constructed that gives the imaginary as well as the real roots of a cubic equation. A cubical parabola is drawn upon paper, the ordinates being the cube roots of the corresponding abscissæ. To find the roots of a cubic, first reduce it by Cardan's rule to the form $x^3 - Ax - B = 0$. Then measure off along Ox, a distance equal to B, and from this point, T, draw a line making an angle equal to $\cot^{-1}A$ with Ox. The ordinates of the points where this line cuts the curve are the roots of the equation. To find the imaginary roots when they exist, first find the real root as before; from this point draw a tangent to the branch of the curve the other side of Oy, then if this line cut the axis of x at a point Q, and α be the real root, the two imaginary roots are

$$\frac{\alpha}{2} \pm i\sqrt{\frac{QT}{\alpha}}.$$

Instead of actually going through the construction as above, the operation is preferably performed by applying a protractor with a tangent scale to the curve with its centre at T, setting it, and leading off the point of the curve cut by its edge.—On a machine for the solution of equations, by Mr. C. V. Boys. After mentioning Mr. Hinton's apparatus, lately shown to the Society, and briefly describing Mr. Kempe's equation-machine, Mr. Boys explained a machine he had constructed, consisting of a system of beams, each provided with a pair of pans, and working upon a fulcrum at the middle. The pans of the first beam are marked $+a$ and $-a$, those of the second $-b$ and $+b$, the next $+c$ and $-c$, and so on. Into these, weights equal in value to the coefficients a, b, c, &c., of an equation $a + bx + cx^2 + \ldots = 0$ are to be placed. A sliding joint is arranged to connect a point opposite the positive pan of each beam, with a rib at the back of the next lower one. Alternate beams are placed opposite one another, and each set can be slid past the other, the peculiar connecting-joints being able to slide past the fulcra and the pans on each beam. To solve an equation, the coefficient weights are placed in their pans, and the two sets of beams are made to slide past one another. At certain positions the beams change the direction of inclination. These positions of balance are noted on a scale, the readings of which are roots of the equation. When there are not more than two impossible roots, the machine will find them; for this purpose the real roots are first found and divided out, the resulting quadratic being placed on the machine. Instead of a change of inclination of the beam, a maximum or minimum of pressure is observed by a spring balance. The reading of the scale is then the real part of the root, and the square root of the pressure the impossible part.—Mr. A. Hilger exhibited and described a new driving clockwork of isochronous motion regulated by a fan-governor, and a new direct-vision spectroscope.

Geological Society, November 8.—Prof. T. G. Bonney, F.R.S., President, in the chair.—Henry M. Ami and R. Mountford Deeley were elected as Fellows of the Society.—The following communications were read:—Results of recent researches in some bone-caves in North Wales (Fynnon Beuno and Cae Gwyn), by Henry Hicks, F.R.S., with notes on the animal remains by W. Davies, F.G.S., of the British Museum (Nat. History). This paper contained the results of researches carried on in these caverns in the summers of 1883, 1884, and 1885 by Mr. E. Bouverie Luxmoore, of St. Asaph, and the author. The enormous collection of bones belonging to the now extinct animals of Pleistocene age obtained had been submitted for examination to Mr. W. Davies, and afterwards distributed to various museums. Several well-worked flint implements were also discovered in association with the bones. The following are the conclusions arrived at by the author, from the facts obtained during the explorations:—That abundant evidence has been furnished to show that the caverns had been occupied by hyænas, and possibly by other beasts of prey, as dens, into which portions of carcasses of various animals had been conveyed in Pleistocene times. The very great abundance of some animals, such as the rhinoceros, horse, and reindeer, and the frequent presence of bones belonging to young animals, proved that the plain of the Vale of Clwyd, with that extending northward under the Irish Sea, must have formed a favourite feeding-ground even at that time. The flint implements and worked bones showed also that man was contemporary with these animals. The facts perhaps, however, of greatest importance, made out during these researches, are those which bear on some questions of physical geology in regard to this area, which hitherto have been shrouded more or less in doubt. The views on the physical conditions in Pleistocene times of the areas in North Wales in which these and the other bone-caverns occur, so ably put forward by Sir A. Ramsay, appeared to the author to be strongly supported by the results obtained in these explorations. The ravine in which the caverns occur must have been scooped out previous to the deposition in it of the glacial sands and boulder-clays. This sand and clay, there seems good evidence to show, must have filled up the ravine to a height above the entrances to the caverns, and such sands and clays are now found at some points to completely fill up the caverns. How, then, did these sands and clays get into the caverns? Were they forced in through the entrances by marine action or by a glacier filling the valley? Or were they conveyed in subsequent to the deposition of the boulder-clay in the valley and surrounding area? The position of the caverns in an escarpment of limestone, at the end of a ridge of these rocks, with a sharp fall on either side, prohibits the idea that the material could have been washed in from the higher ground, as has been suggested by some in the case of other caverns, if it had anything like its present configuration. Moreover, there is scarcely any deposit now visible upon the limestone ridge, and there is no certainty that there ever was deposited there any great thickness of such a clay as that now found in the caverns. The general position also of the bones in some of the tunnels seems to indicate clearly that the force which broke up the stalagmite floor, in some places 10 to 12 inches thick, and stalactites 6 to 8 inches across, which thrust many of the large and heavy bones into fissures high up in the caverns and placed them at all angles in the deposit, must have acted from the entrance inwards, and the only force which seems to meet these conditions is marine action. The following seem to the author to be the changes indicated by the deposits. The lowest in the caverns, consisting almost entirely of local materials, must have been introduced by a river which flowed in the valley at a very much higher level than does the little stream at present. Gradually, as the valley was being excavated, and the caverns were above the reach of floods, hyænas and other beasts of prey occupied them, and conveyed the remains of other animals into them. Man also must have been present at some part of this period. Gradually the land became depressed, the animals disappeared, stalagmite was formed, and the sea at last entered the caverns, filling them up with sands and pebbles, and burying also the remains not washed out. Floating ice deposited in this sea the fragments of rocks derived from northern sources, and these became mixed with local rocks and clays brought down from surrounding areas. The greater part of the boulder-clay in the Vale of Clwyd was probably deposited as the land was being raised out of this Mid-Glacial sea. During the process of elevation the caverns became again disturbed by marine action and the upper fine reddish loam and the laminated clays were deposited. It seemed to the author impossible to avoid the conclusion that these caverns must have been submerged, and afterwards elevated to their present height of about 400 feet above the level of the sea, since they were occupied by Palæolithic man and the Pleistocene animals.—On the occurrence of the Crocodilian genus *Tomistoma* in the Miocene of the Maltese Islands, by R. Lydekker, F.G.S.—Description of the cranium of a new species of *Erinaceus* from the Upper Miocene of Œningen, by R. Lydekker, F.G.S.

Anthropological Institute, Nov. 24.—Mr. Francis Galton, F.R.S., President, in the chair.—The election of W. Seton Karr, E. Lawrence, Dr. R. Munro, and Dr. W. Summerhays, was announced.—Mr. C. H. Read exhibited a number of ethnological objects from Terra del Fuego.—The President exhibited on behalf of Dr. J. E. Billings, of the United States Army, a collection of composite photographs of skulls. There were in all twenty photographs, forming four series, referring respectively to Sandwich Islanders, Ancient Californians, Arapahoe Indians, and Witchitaw Indians; each composite was the mean of six adult male skulls.—Dr. Edward B. Tylor exhibited some Australian Tunduns or bull-roarers, and explained the manner in which they were used.—Mr. J. Theodore Bent read a paper on insular Greek customs, in which he described many ceremonies now used by the Christian inhabitants of the islands of the Ægean Sea that were obviously derived from, or survivals of, ancient Pagan customs. Mrs. Bent exhibited a collection of Greek dresses, drapery, and other objects from the islands referred to in the paper.—Mr. J. W. Crombie read a paper on the game of hop scotch, in which he traced the origin of the game to a period anterior to the introduction of Christianity, and showed that in early Christian times children had some rough idea of representing in this game the progress of the soul through the future world, and that the division of the figure into seven courts was on account of the belief in seven heavens.—Dr. E. B. Tylor gave a *résumé* of a paper by Mr. A. W. Howitt, on the migrations of the Kurnai ancestors (Gippsland).

Royal Microscopical Society, November 11.—Rev. Dr. Dallinger, F.R.S., President, in the chair.—The President referred in feeling terms to the death of Dr. W. B. Carpenter, C.B., a Fellow, and formerly President, of the Society, and a resolution of condolence with his family was passed. Prof. Stewart was appointed to represent the Society at the funeral. —Mr. Beck exhibited a portable form of his "Star" microscope, and Mr. Crisp exhibited a microscope with focussing adjustment by means of a piece of catgut, which, it was claimed, gave a very simple and easy motion.—Mr. J. Mayall, jun., exhibited the Helot-Trouvé electric photophore, which had been recommended as an excellent illuminant for microscopical purposes.—Mr. Groves exhibited a Barrett microtome, a very large form, intended for cutting sections of exceptional size.—Mr. Dowdeswell exhibited a cholera bacillus showing a flagellum at either end, one straight and the other coiled.—Mr. Nelson exhibited a very fine image of *Triceratium septangulatum*, showing markings in the areolation with a $\frac{1}{8}$-inch objective, and the low aperture of 0·29.—Mr. Badcock described an unknown fresh-water organism, closely resembling a Polycystin. The President suggested that it might possibly be one of the various forms of *Actinophrys*.—A paper was read by Mr. W. B. Turner, describing some new and rare Desmids; also one, by Dr. Giltay of Holland, on the proper mode of describing the amplifying power of a lens or objective.—Mr. Crisp read a paper on the limits of resolution in the microscope, in which he showed that whilst the limit with white light might be taken at 146,543 lines to the inch, the use of monochromatic light gave an increase to 158,845, and with photography to 193,037.—A paper was read by Dr. Lavis on preparing sections of pumice and other vesicular rocks.—It was announced that Mr. Mayall, jun., was about to give a course of five lectures on the microscope at the Society of Arts, illustrated by microscopes from the Society, and from Mr. Crisp's extensive collection.

Mineralogical Society, December 8.—L. Fletcher, M.A., President, in the chair.—The following papers were read:—On a glaucophane eclogite from the Val d'Aoste, by the Rev. Prof. Bonney, F.R.S.—Note on orthoclase from Kilima-njaro, by H. A. Miers, M.A.—Preliminary notice of penetration twins of arragonite from New Mexico, by R. H. Solly, F.G.S.—On some specimens of idocrase and garnet from the neighbourhood of Tzermatt, by Prof. W. J. Lewis.—Dr. Burghardt exhibited some pseudomorphs of native copper after arragonite, from South America.—Mr. Miers also exhibited some peculiar twins of calcite from Eyam, Derbyshire.

Entomological Society, December 2.—Mr. J. W. Dunning, Vice-President, in the chair.—Two new Fellows were elected.—Mr. F. Enock described experiments in mounting *Mymaridæ*, and exhibited photographs of the insects.—Mr. A. Eland Shaw exhibited an undetermined species of *Conicephalus*, which had been taken in a hothouse at Birmingham; it was believed to be an Australian or South American species. —Mr. G. T. Porritt exhibited two species of a melanic variety of *Ayrotis obelisca* from Sligo.—Mr. Dunning read a note on the election of honorary foreign members.

Victoria Institute, December 7.—A paper on the unreasonableness of agnosticism was read. The author treated the subject in such a manner as to make his essay specially valuable at this time.—Mr. E. Charlesworth read a paper on the skull of the gorilla, of which he exhibited a specimen considered the most perfect that had as yet reached England.

CAMBRIDGE

Philosophical Society, November 23.—Prof. Foster, President, in the chair.—The following communications were made:—On a new method of producing the fringes of interference, by L. R. Wilberforce, B.A. The author stated that in the course of an inquiry into the suitability of various forms of interference-fringes for certain investigations on the velocity of light upon which he had been engaged, he had been led to adopt the mode of production which was the subject of his paper. He briefly described the method, indicated the elements of its theory, and, by a comparison of his results with those of former experimenters, showed the great increase of accuracy attainable by means of it.—On the dielectric strength of mixtures of gases, by Dr. C. Olearski. The author described a series of experiments from which it followed that the dielectric strength of a mechanical mixture of two gases is intermediate between the strengths of its constituents.—On the mutual action of oscillatory twists in a vibrating medium, by A. H. Leahy, M.A.—On the transpiration stream in cut branches, by F. Darwin, M.A., and R. W. Phillips, B.A. The paper consists chiefly of an experimental criticism of Dufour's experiments on transpiration. The authors show that (contrary to Dufour's contention) there is an essential similarity between the natural current of water in a transpiring branch, and the current induced by pressure in a similar specimen; this similarity being understood to hold good under the conditions of Dufour's experiments, namely, when certain incisions are made, or when the branch is compressed in certain ways.

PARIS

Academy of Sciences, December 7.—M. Jurien de la Gravière, Vice-President, in the chair.—Determination of the differences of longitude between Paris, Milan, and Nice, by MM. F. Perrier and L. Bassot. The values obtained for the differences of longitude between the observatories of these places is found to be:—

Milan–Paris ...	+27m. 25·315s.		
Paris–Nice ...	– 19m. 51·513s.	27m. 25·325s.	Δ=0·01s.
Nice–Milan ...	– 7m. 33·812s.		

—Movement of the molecules of the so-called "solitary wave" propagated on the surface of a stagnant canal, and which after some agitation acquires a constant and uniform motion, by M. de Saint-Venant.—Experimental researches undertaken to determine the influence exercised by lesions of the spinal marrow on the form of the convulsions assumed by artificial epilepsy of cerebral origin, by M. Vulpian.—On the theory of algebraic forms in connection with M. Halphen's differential invariants, by M. Sylvester.—Note on the fresh experiments carried out in 1885 with the navigable balloon *La France*, by M. Ch. Renard. The results of three ascents during the months of August and September gave a mean velocity of about 6·7 metres per second, with 55 to 57 revolutions of the screw per minute. On the two last occasions the balloon was brought back to the point of departure, Chalais, near Meudon.—On the propagation of motion in an undefined fluid (1st part), by M. Hugoniot.—Discovery of a new comet in the constellation Andromeda at the Paris Observatory on December 1, by M. Fabry.—Observations made at the Observatories of Paris, Bordeaux, Lyons, and Algiers, presented by M. Mouchez.—On the employment of spherical convex glasses (boules-panorama) as solar signals, by M. Hatt.—Note on certain hyper-Fuchsian functions, by M. E. Picard.—On Lagrange's form of interpolation, by M. Bendisson.—Note on the trigonometric series, by M. H. Poincaré.—On the solutions common to several partially-derived linear equations, by M. R. Liouville.—On the holomorphous conditions of the integrals of the iterative equation, and on some other functional equations, by M. G. Kœnigs.—Remarks relative to a preceding communication on M. Kœnig's theorem, by M. Ph. Gilbert.—On the part played by the rotation of the earth in determining the deviation of running waters on the surface of the globe, by M. Fontes. The author considers it now demonstrated that terrestrial rotation has a perceptible influence on the tendency shown by rivers to corrode one side or the other of their banks.—Note on the spectrum of absorption of oxygen, by M. N. Egoroff. The results of the author's spectroscopic researches, combined with those of M. Thollon, completely explain the origin of the telluric bands in the section A—*b* of the solar spectrum; 126 bands, distributed in equal proportion and identically in the groups A, B, and *a*, depend exclusively on oxygen, while the others belong to the vapour of water.—On the characteristic equation of carbonic acid, by M. E. Sarrau.—On the preparation of hypophosphoric acid, by M. A. Joly.—Note on some properties of zinc, by M. L. L'Hôte. Pure zinc, alloyed with a very small quantity of arsenic or antimony, conducts itself in water like zinc alloyed with iron. Hence all the zincs of commerce decompose water at the boiling-point.—Heat of combustion of some ethers of organic acids: ethylic ether of lactic acid, citrate of ethyle, normal ethylbutyric ether, ethylisobutyric ether, by M. Louguinine.—On the pyrogenous decomposition of the polyatomic acids of the fatty series, by M. Hauriot.—On the normal and primary monochloruretted butyric compounds, by M. Louis Henry.—Action of chlorine on anhydrous chloral, by M. Henri Gautier.—Analysis of the deposits formed by the mineral waters of Chabetout, Puy-de-Dôme, by M. Fr. Thabuis. The chief constituents of the deposits from these ferruginous waters are sesquioxide of iron, nearly 50 per cent.; organic matter, 9·4; lime, 2·2; gelatinous silica, 11·1; carbonic acid, 1·8.—Optical examination of some little-known minerals: kirwanite, a silicate of the protoxide of iron, lime, and alumina, with about 4 per cent. of water; hullite, consisting mainly of amorphous matter, and found in the Irish basaltic rocks; harringtonite, a zeolithe of lime and soda; bowlingite, a hydrosilicate of alumina, iron, and magnesia; botryolite, identical with datholite, by M. A. Lacroix.—On experimental denutrition, by M. Ch. E. Quinquand. This process, which consists in *starving* an organ, or part of an organ, supplies a new and useful method of investigation, enabling physiologists to advance the study of elementary nutrition and of the fundamental action of medicines.—On the effects produced by the ingestion and intra-venous injection of some colouring substances derived from coal, and much used in colouring drinks and aliments, by MM. P. Cazeneuve and R. Lépine. One of these (binitronaphthol, or Manchester yellow), is shown to be distinctly injurious; the other two derived from it quite harmless.—On the anatomy, digestive, and nervous systems of the genus Discina, by M. L. Joubin.—Account of a young megaptera recently stranded in the maritime district of La Seyne (Mediterranean), where this species of whale is extremely rare, by M. G. Pouchet.—On the respiration of plants, continued, by MM. G. Bonnier and L. Mangin.—On the desiccation of plants immersed in aqueous solutions, by M. Albert Levallois.—On the processes of fructification of the various genera of sigillaria, by M. B. Renault.—On the underlying rocks of the Tertiary formations in the neighbourhood of Issoire, Auvergne, by MM. Michel Lévy and Munier-Chalmas.—Geological observations on the kingdom of Shoa and Galla country, south of Abyssinia, by M. Aubry.—Note on the discovery of phosphates of lime made in the spring of the present year in the lowest Tertiary strata in the south of Tunis, by M. Philippe Thomas.—On the Jebel Zaghwan range, Tunis, and on the great fault in this orographic system running north-east and south-west, and indicating the line of upheaval of the lower chalk formations now in contact with the Upper Eocene rocks, by M. G. Rolland.—Note on the discovery of a human station dating from the Stone Age in the woods of Clemert, by M. Emile Rivière.—On the advantages to be derived from a thorough knowledge of the displacements of the Gulf Stream in weather forecastings, by M. de Tastes.—Various communications on the shooting stars of November 27, from M. Stephan, of the Marseilles Observatory; M. Hirn, of Colmar; M. Colladon, of Geneva; M. Perrotin, of Nice; M. Quenin, of Pelonne (Drôme); MM. Hildebrandsson and Charlier, of Upsala; M. Phipson, of London; and others, with remarks by M. Faye.

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