THURSDAY, OCTOBER 16, 1884

HANDBOOK OF BOTANY

Handbuch der Botanik. I. and II. Herausgegeben von Dr. A. Schenk. (Breslau: Verlag von Eduard Trewendt, 1879–1882.)

As early as 1861 it had become apparent to certain leading German botanists that the limits of their science had been so far extended as to make it impossible for one writer to treat the whole subject with such uniformity and thoroughness as is required in the composition of a standard text-book, including the substance of the facts well ascertained up to the date of its issue. Accordingly Hofmeister, with the assistance of others, and especially of Sachs and De Bary, planned a joint "Handbook of Physiological Botany," and though, owing to the difficulties which are always liable to attend joint authorship, the parts written by the several contributors were issued at various dates from 1865 to 1877; and though some of the parts included in the original scheme never appeared at all, those published are together the result of the most considerable attempt hitherto made to issue a comprehensive and standard Text-book of Physiological Botany. During the twenty years which have followed the adoption of this plan by Hofmeister and his colleagues unprecedented advance has been made in the science, and it is thus still more necessary than before that the task of authorship of a comprehensive handbook should be divided. The "Handbook of Botany," which is in course of issue by Prof. Schenk, and of which two volumes are already complete, is a second attempt, somewhat similar in idea to that of Hofmeister, though differing from it in many points. The staff of authors is larger, and since the space allotted to the several authors is less, greater uniformity in date of issue has been attained; there is, however, in Schenk's "Handbook" no prearranged and well-balanced plan of the ground to be covered, or at least the first two volumes give no clue to any such plan. Each article appears to be independent of its neighbours, and must be regarded as a separate essay on a definitely circumscribed, and in some cases a very limited, branch of the science. Since this is the case, it is clear that the "Handbook" cannot be used by beginners as a text-book of the science; it is suited rather to specialists, and others who may desire special information on the subjects which are treated. To these the book will be of the greatest use and interest, since the articles are written by well-known men, who have made the subjects of their essays their special study.

It will be impossible here to discuss each of the articles in detail, nor indeed will it be necessary to do so, since in more cases than one the articles are in the main useful epitomes of more extended works already well known to the public; in other cases, however, the articles are the result of fresh constructive work. While those of the former category will be merely named, those of the latter order demand more careful attention.

The first article, entitled "Die Wechselbeziehungen zwischen den Blumen und ihre Kreuzung vermittelnden Insekten," is by Dr. Hermann Müller, whose name will be sufficient guarantee of its excellence; while it is written in such a style as to interest those who have not made botany their special study. It is followed by a short article by Prof. Drude on Insectivorous Plants. The essay on the Vascular Cryptogams, by Dr. Sadebeck, is one of the most important of the whole series: the author gives a concise account of the chief facts hitherto ascertained, and has arranged them on a plan which is well suited to their comparative treatment. After a short general description of the cycle of life as found in these plants, he treats in the first section of the spore, germination, the prothallus with the sexual organs, and the embryo; while the second section is devoted to the vegetative organs and the sporangia. Each organ is described successively, as far as it is known, in the various forms of vascular Cryptogams, and thus the comparison of details of structure and development of each organ in various groups is made more easy than is usually the case in other works. Then follows an article by Prof. Frank under the heading "Die Pflanzenkrankheiten"; this may be regarded as a useful abstract of his more extended work on the same subject, which is already well known to botanists. The first volume is brought to a conclusion by an article on the Morphology of Phanerogams, by Prof. Drude. It has been the object of the author to furnish a compendium of the external conformation of flowering plants, and their sexual organs, reference being made to their comparative anatomy and development. At the present day this object is in itself unsatisfactory: to gain a true insight into the morphology of Phanerogams reference must necessarily be made to the lower forms, and the want of such reference and comparison is apparent throughout this article, especially in that part of it which is devoted to the morphology of the flower.

The second volume includes, in the first place, a treatise on Vegetable Physiology, by Detmer, which has laboured under the disadvantage of being published almost simultaneously with the excellent lectures of Prof. Sachs on the same subject. It is followed by Falkenberg's essay, headed "Die Algen im weitesten Sinne," which is one of the most important of the whole series. In the introduction he shows how the classification of the Thallophytes proposed by Cohn in 1872, and adopted by Sachs, led the way to the system of classification proposed by De Bary for the Fungi; this writer, after excluding the Myxomycetes and Schizomycetes, recognises that the various remaining groups of Fungi may be regarded according to their morphological characters as a natural series. Falkenberg treats the Algæ in a similar way; he strips off from the whole series of chlorophyll-containing Thallophytes (to which the term Algæ in its widest sense may be applied) certain outlying groups, viz. the Diatomaceæ, Schizophyceæ, and Florideæ; the remaining Chlorophyceæ and Melanophyceæ together form that series which he terms the *Algæ in the narrower sense*. Adopting this general method, the author has constructed a compendious description of the whole series of Algæ, which will well repay those who read it. The essay on the Muscineæ, by Goebel, is written in a similar spirit to that of the article which precedes it, and can be well recommended as giving the best concise account of the morphology and development of that class hitherto published. The article by Prof. Pfitzer on the Diatomaceæ will be welcomed, as giving in an accessible form a detailed account of the structure of these plants from the pen of one who has already distinguished himself in this field. The second volume is brought to a close by a treatise by Haberlandt named "Die physiologischen Leistungen der Pflanzengewebe," a subject well suited to one of the Schwendenerian school, to which its author belongs. Anatomical facts, many of which are already well known, are here placed before the reader in the light of the anatomico-physiological method, which the pupils of Schwendener claim as having been initiated by him in 1874.

From what has been already said, it is clear that this "Handbook" will, by the individual worth of many of its articles, take a prominent place among standard botanical works, and will undoubtedly be of great service to advanced students. Further volumes are still in progress, and the appearance of their successive numbers will be looked forward to with interest. F. O. B.

OUR BOOK SHELF

A Synopsis of the British Mosses. By C. P. Hobkirk, F.L.S. Second Edition. 8vo, 240 pages. (London: L. Reeve and Co., 1884.)

THIS is a new edition of a work that appeared originally in 1873. There is no other recent handbook of British mosses, so that it has had the field entirely to itself, and has had a large circulation amongst our home collectors. It is a cheap working handbook, something on the scale of Babington's "Manual of the British Flowering Plants and Ferns," without any figures, but with full diagnostic characters of all the indigenous genera and species. Britain is exceptionally rich in mosses, and in this new edition 129 genera and 576 species are enumerated and described, with a short notice of locality.

Mr. Hobkirk is well known as an excellent practical bryologist of many years' experience. He has not attempted either in this or the previous edition to introduce any novelty in classification. In the first edition he followed Wilson closely, and Wilson in his turn deviated but little in arrangement, nomenclature, and the circumscription of genera and species from the great standard work on the mosses of the whole of Europe, the magnificent "Bryologia Europea" of Bruch and Schimper, which contains elaborate figures of every known species. In this second edition Mr. Hobkirk has altered his classification to correspond with that of Jaeger's "Adumbratio Muscorum," a change which we consider of very doubtful utility, as it has the effect of making the preliminary synopsis much more elaborate and more difficult for a beginner to understand and use.

An illustrated work on British mosses brought up to the standard of Bruch and Schimper has been greatly wanted. Now, Dr. Braithwaite is bringing out in parts a work of this character, with admirable original drawings and detailed descriptions. At the present time this is about one-third completed, and it is greatly to be hoped he may have health and strength to finish it. For any one needing a cheap working handbook we can cordially recommend the present book. It contains a brief glossary of terms. Only the names that are adopted are given, without any synonyms. One fault in the preliminary key that will puzzle a beginner is the want of definitions for the two primary divisions—Saccomitria and Stegomitria. Another point that without explanation will likely puzzle students is that the authorities cited for each species are those of the author who first used the specific name, taken quite independently of the genus under which it is now placed, so that, for instance, Linnæus is cited as the authority for *Eucladium verticillatum*, when the genus Eucladium was first characterised by Bruch and Schimper half a century after Linnæus died. The orthodox plan is to cite the authority for genus and species combined.

J. G. BAKER

Our Insect Allies. By Theodore Wood. 8vo, pp. 1-238. (London: Society for Promoting Christian Knowledge, 1884.)

WRITERS on popular entomology are hard driven nowaday to find titles for their works, or subjects that have not already been worn to shreds by previous authors. To be successful they must possess the same talent that enables a *chef-de-cuisine* to contrive an *entrée* from the same materials, so disguised by name and sauces as to lead his patrons to consider they are partaking of a new dish. The author of this nicely got up little book has evidently felt himself in such a position, but on the whole he has succeeded very well, the more so because there are fewer errors than ordinarily exist in popular entomological works. He takes as his standpoint the fact that very many insects are indisputably serviceable: some by ridding the world of putrid or unhealthy organic matters, both animal and vegetable; some by destroying other insects undoubtedly noxious. The result is that we get here a series of histories of individuals or groups detailed in popular language, often from personal observation, and for the most part well illustrated by woodcuts. The author evidently feels himself most at home in dealing with the *Coleoptera*, and, as we think, judiciously takes up the position that bark-beetles and wood-borers are scavengers, seeking to devour what is already morbid, and are not the *cause* of decay in the trees in which they are found. We fail to follow his account of the mechanism by which the click-beetles (p. 207) perform that acrobatic movement so familiar to our childhood in the shape of the "jumping frog"; to our mind the "mucro" that is the chief agent in this action is not "elastic." Why are the *Aphis*-parasites known as *Aphidius* stated to be *Chalcididæ* (p. 168)? Why is a *Syrphus* larva figured (p. 160) as that of a "Golden-Eye," or "Lace-Wing"? The introductory remarks and the concluding notes contain some very judicious reasoning on the aim and purpose of entomological studies, and we sincerely wish we could agree with the author (p. 236) that collectors, as opposed to students, are "now in a very small minority"; a vast improvement towards this end has undoubtedly taken place latterly, but the time for congratulation has not yet arrived. The Society under whose auspices this little book is published has done much towards popularising natural history in this country; this work may be classed amongst the best of the series, and no doubt in a second edition the author will revise it and rectify a few palpable errors.

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.*]

Shifting of the Earth's Axis

HAVING quoted the Greenwich observations so often, and with all respect, during the last twenty years, in my several Great-Pyramid publications, as showing that there is a slow shifting of the earth's axis going on, with the effect of altering the latitude of places minutely from age to age (see more especially p. 81 of fourth edition of "Our Inheritance in the Great Pyramid"), I cannot remain unconcerned when the present energetic Astronomer-Royal comes out so very positively with the statement that the Greenwich observations of the last forty-seven years (which he confines himself to) show nothing of the kind, and that there is no such movement of the earth's axis going on.

At least so I understand his letter in NATURE, vol. xxx. p. 536. If that statement or conclusion is perfectly correct, I must of course allude to it in the next edition of my Pyramid book, and adopt its corrections, whatever they may lead to; so that it is well at once to ask any further questions which seem demanded for full trust and credibility.

That the observations made at Greenwich during the last forty-seven years, when computed as the said Astronomer-Royal has computed them, do not show any change of latitude during that space of time, no one is more ready to allow most honourably than myself. But before we can admit that that result, pure and simple, absolutely establishes the non-shifting of the earth's axis of rotation, two more things at least must come about, viz.:—

First, the Astronomer-Royal must attack and demolish the observations and calculations made at the great Russian Observatory of Pulkowa, which show that such a change, at the rate of about one foot *per annum*, has been going on through the last quarter of a century, and are even believed in America to be more accurate than the Greenwich observations. And

Second, he must take up, and similarly destroy, the testimony of the earlier Greenwich observations themselves, before these last forty-seven years of his own computation began.

Now those earlier Greenwich observations were so remarkable for what they did indicate in their own time, that I may freely mention now, seeing that all the parties are dead, that somewhere about 1836, Sir Thomas, then Mr. Maclear, at the Royal Observatory, Cape of Good Hope, received a private letter from Thomas Glanville Taylor, Honourable East India Company's Astronomer at Madras—and earlier an assistant at the Royal Observatory, Greenwich—stating his belief that the latitude of the British National Observatory was continually decreasing; and he gave a list of latitudes, as determined by Greenwich observations, so far back as they went, but condensed into three epochs, to prove the point.

The matter was kindly communicated to me by my then chief, Sir Thomas Maclear, and was of course deemed interesting and curious at the time; but had quite gone to rest in my mind, until twenty-nine years afterwards, when I fell across the same effect, in the same direction and at nearly the same rate, but through a longer period of time and to a much larger accumulated quantity, at the Great Pyramid of Geezeh in Egypt. The datum for the latitude of 4000 years ago, to compare with the present observed latitude of the same spot—though exactly that which the learned Dr. Hook desired so much, but in vain, to find anywhere 200 years ago—is not perhaps so purely and perfectly scientific as the high-class practical astronomers of our times will always condescend to notice. But in the accompanying feature of change of azimuth, so creditably brought to the front by Mr. Flinders Petrie, there is a testimony of modern observation to ancient accuracy of so respectable a character—if I may be allowed so to say—that it ought not to be entirely ignored; and it was first mentioned thus.

While I was at the Great Pyramid in 1865, and just after I had there measured the azimuths of the entrance-passages of the Great and Second Pyramids on successive evenings by reference to the six-hour elongations of Polaris, there came a letter from a retired civil engineer in Edinburgh, a man of long Batavian experience in his day, and gifted with remarkable powers of modern science and originality of mind,—in which letter he was pleased to run down all the presumed object of my work out there on the Geezeh Hill. Especially too was he pungent on the point that even the best of the ancients had not that triumph of modern civilisation, "the manufacturing principle," in them; "for," said he, "they could not make two things alike."

Whereupon I sent him the azimuths of the entrance-passages of those two grand pyramids, so many hundred feet apart, and pointed out, that, though they showed an error of azimuth for modern date of nearly five minutes, yet the one pyramid exhibited so very nearly the same identical quantity as the other,—that they were, angularly, nearer together, or more exactly alike, than were the two halves of one and the same azimuth circle I was observing with. And yet that circle was by so celebrated a maker of modern times as Troughton, and the instrument a choice one specially made by him to be presented to the celebrated Prof. Playfair, by his admiring students in the University of Edinburgh; and by Playfair's executors, again, presented after his death to the Royal Observatory, Edinburgh, where it is still preserved in honour. C. PIAZZI-SMYTH

15, Royal Terrace, Edinburgh, October 10

The Sky-Glows

IN reply to Mr. Backhouse's question (p. 511) as to where the context of Mr. Neison's remarks can be seen, I can only say that I do not know. I came across the portion quoted relating to these phenomena at the end of the Astronomer-Royal's Report upon the Weather of 1883. Like Mr. Backhouse, I have been on the look-out for solar halos, or big rings round the sun as we call them, for the last thirty years, more with a view to be prepared for squalls, &c., when boating, than anything else. But for some years there has been so much haze about the sun, and the weather has so often cried "Wolf," so to speak, with no responding gale or squall, that of late I have ceased to take much note of such warnings. I may here mention that the rosy corona, when visible, as it so often is now, can be well seen by looking towards the place of the sun, but standing in the shadow of some high building; or at times by totally eclipsing the sun with a hat held between him and the eye.

As far back as January 1, 1884, in a letter to the *St. James's Gazette*, I ventured to predict that we had not seen the last of what were then spoken of as the "Recent Sunsets"; there was a very remarkable after-glow ten days after this, reaching the zenith, seen even in London. In the same letter of January 1, I suggested some increase in solar energy as the cause of these phenomena.

For some time, though feebler repetitions of the glows continued to be seen up to the end of March, there was nothing strong enough in the way of colour worth noting. But from what I continued to see in the shape of vapour, together with that strange warm colour by day about the sun, I felt sure that whatever might be the cause of these phenomena must still be going on, and in a short note dated April 12, I again spoke of their probable early reappearance. A graphic account of these after-glows, written by an observer at Smyrna, appeared in the *St. James's Gazette* of February 25, in which he pointed out what I have often since noticed, viz. that with excess of moisture all colour disappears.

This was at times very remarkable in the early part of July this year, when we had some of the strangest white sunsets I have ever seen. The sky around and above where the sun had set, looking almost ceiling-like in its opacity, upon which soon appeared numbers of weird small cloud forms, at times very regular, like ripple-marks in sand, or the bones of some great fish or saurian embedded on a slab of stone.

Against these pale sunsets all buildings and trees told like black velvet, while the clouds would rest almost stationary for a long time. Years back such a sky would have betokened a hurricane; but evening after evening they were repeated, and no storm of any importance followed. For many years past, but notably during the summer of 1883, I had observed a steady increase in a white luminous glare about the sun, so much so that I wrote about it in the year 1882 to my brother in India. I was not therefore surprised altogether when as the sunsets increased in colour, which they mostly do in autumn, that this glare last winter was followed by something more than usual in the way of colour; and here I should like to say that, as far as I have seen, and I have missed very few chances of watching them, that though last winter twilights often increased up to a certain time in strength, yet they did not exceed in duration the time allotted to twilight in the almanacs. This letter is already too long, but I cannot help asking, in conclusion, whether it may not be possible that we have been all along muddling up cause and effect, and that the eruption at Krakatoa, the recent earthquakes and waves, as well as the strange atmospheric phenomena, which are still about us, cannot all be traced to one cause, viz. actual increase of sun power?

Southampton ROBERT LESLIE

THIS evening after sunset I noticed a column of yellowish light over where the sun had set, and moving with the sun. I have seen the same before. Can it be the zodiacal light? I have frequently noticed during the present year, while the sun was much too high for any sunset colours, a pinkish colour in the sky. This has been observed by others, but I do not know whether it has been seen outside the British Islands. It must be connected with the sunset-glows which several of your correspondents have described. OSEPH JOHN MURPHY

Belfast, October 12

Circular Rainbow

In the notice given in Nature (Sept. 11, p. 465) of the beautiful circular rainbow that is seen in the spray of the Montmorenci Falls near Quebec, the expression in the heading, "seen from a hill-top" will convey an erroneous idea without some explanation. The complete circle is only seen by getting down in the spray to the edge of the low rocks, within a few inches of the level of the water, and the circular bow then passes down to the feet on each side; it is indeed most perfectly seen by turning round and stooping down to look back between the legs, when the complete circle is seen without interruption from the feet. The bow is small in diameter, and is a narrow band, appearing nearer to the eye than an ordinary spray rainbow. I had the pleasure of seeing it on August 25, on the occasion of the British Association Canadian visit. William P. Marshall

15, Augustus Road, Birmingham, October 13

To Find the Cube of any Number by Construction

Prof. Karl Pearson has kindly referred me for a *simple* graphical construction for any positive or negative power of any rational quantity whatever to Egger's "Grundzüge einer graphischen Arithmetik." This method, he informs me, is reproduced in Cremona's "Il calcolo graphico." I was of course aware that there were several simple constructions. I was induced to write upon the subject owing to the unexpected discovery that there was a line in the geometry of the triangle which enabled one to obtain the cube of a number.

October 14 R. Tucker

EXPLORATIONS IN ICELAND[1]

The Lava Desert of Ódáðahraun

On July 25 we set out for the southern Dyngjufjöll, in order to examine Askja. All previous explorers of that volcanic locality have taken the northern route from Svartárkot, but no one has hitherto approached it from the east, from Herðubreið, any advance from that side having been deemed impracticable. This I wanted to test for myself, and shaped my course from the tent (pitched, as before said, to the south of Herðubreið) in a direct line on the wide gap that opens in Askja to the east. The whole intervening country was one continuous succession of lavas, so effectively covered with pumice and scoriæ from the great explosion of 1875, fortunately for us, that the whole was really one scoriac plain, the pumice boulders measuring generally one to two cubic feet, some more, some less. If it had not been for this scoriaceous cover, these lavas would have proved pretty certainly utterly impassable for horses. We took good care to keep to the crests of the thickest pumice-drifts, and though such travelling is rough enough for horses, yet they sustain no great harm, because the pumice is so light and brittle. Under the south-eastern spurs of Dyngjufjöll we came upon a lake, shallow, but of considerable magnitude, of the existence of which there was no previous knowledge. About midway between Herðubreið and Dyngjufjöll the country begins to rise up towards the aforementioned gap in Askja. Askja is a cauldron-shaped valley in the centre of Dyngjufjöll, which is an enormous complex of mountains 4500 feet high. This valley contains innumerable craters which have erupted at various periods; the sides of this valley rise to between 700 and 800 feet, but out of the aforementioned gap lavas have flowed over the lower country outside all the way down to Ódáðahraun, forming an enormous oval of an average incline of 4° 33′. When we came close up to the gap, the scoriæ ceased, and at once the lava became exceedingly difficult to pass. But by aid of frozen snowdrifts filling dips and dints in the slopes, we managed to thread our way along, and thus actually to get into the valley; only one single tongue of lava we had to cross without the aid of snowdrifts—one which, though very narrow, we had the greatest difficulty in getting our ponies over. Having at last succeeded in this, we rode along frozen snowdrifts under the southern slopes of the Askja valley, and thus reached actually on horseback the craters which exploded here in 1875. Previous visitors to Askja have entered the valley through a pass in the mountains inclosing the valley from the north, outside which pass they have had to abandon their horses and to reach the craters on foot over an almost impassable lava-stretch in the bottom of the valley, taking four to five hours in passing the distance from the pass to the craters. From our tent by Lindaá it took us nine hours to reach the craters, but the return route we accomplished in seven. We now left our ponies provided with their fodder beside the large eruptor of 1875, and set off on foot to examine the locality in every direction, spending for that purpose the whole of the bright night and a portion of the next day. So over-covered was Askja with snow that journeying along here was like journeying in the heart of winter. The whole mass of Dyngjufjöll is made up of palagonite breccia interspersed with layers of basalt. Into this mass Askja sinks in the shape of a shallow basin, and may derive its present form partly from certain stretches of it having sunk down in consequence of eruptions, partly from that natural dint or basin-formation of valleys which is so strikingly common to tufa mountains in Iceland. But the supposition that the whole of this valley, about sixteen square miles English, is one crater, the result of one great volcanic explosion, is unwarranted. In the great eruption of 1875 a very considerable extent of ground "fell in" in the south-eastern corner of the Askja valley round the craters, and the vertical precipice of the fractured crust of the earth on the side of the Askja valley measures, according to Prof. Johnstrup's survey, 740 Danish feet; that at the opposite side in the mountains is at least double in thickness. The vertical walls of the precipices exhibit in a clear manner the successive layers of lava which fill the bottom of the Askja valley. In the earth-slip thus created there was, in 1876, a small lake of dull-green colour, circular, and measuring about 4000 feet in diameter. This lake now fills the whole bottom of the slip and measures 10,000 feet in length. In 1876 the temperature of the water was 22° Celsius (71°·6 F.), but has now fallen to 14° C. (57°·2 F.). The crater, which by its explosion covered the east country with pumice and scoriæ in 1875, is situated in the north-eastern brim of the fissure, and is 300 feet in diameter and 150 feet deep; its outer circumference flat, and built up of scoriac ashes, its inside cylindric and perpendicular. In 1876 this crater only emitted steam, now it has turned into a boiling cauldron of clay, the clay mud at the bottom being gray with an admixture of bluish green tint, boiling and wallopping incessantly; through the south-eastern part of its bottom there issues by a subterranean vent a thick column of steam with loud roars and reports, and all around this column smaller fissures issue thinner jets of steam and fumes. Interspersed with the scoriæ in Askja and on the eastern side of the surrounding mountains are found small glazed grayish-blue pieces of trachyte thus formed by the last eruption: among these there are some found of which one-half, or a portion, is reduced to pumice, while the remainder retains its trachytic constituency. In the south-eastern corner of the dip right up from the water are also found a number of craters from which radiate rents and gulfs honeycombed with innumerable fumaroles and crater-tubes from which clouds of steam roll up high above the crests of the mountains, the roar and boom from which are heard to a great distance, resembling the rumbling sound of steam let off from many boilers at once. Deposits of sulphur are already visible round a number of the fumaroles, and yellow-green patches of sulphur show all about the precipices, where every chink and rupture lets off sulphurous fumes. In the eastern part of the slip the scoriaceous layers

[1] Continued from p. 565.

have recently been rent asunder by a rift 150 to 200 feet deep, reaching from the summit of the mountain all the way down to the water. Across this rift there is no way of passing, and, in order to reach the south-eastern corner of the slip, it is necessary to scramble up to the top of the mountain, so as to get round the crevasse. It is difficult to form any adequate conception of the titanic grandeur of Nature at this spot. He who has once had the opportunity of viewing it from the precipice of the earth-slip will never forget the impression. Having finished my survey here, we returned to our tent the same way we had come, glad of rest, exhausted with fatigue and want of sleep as we were, after thirty-six hours' continuous travel.

On July 28 I set out on my return journey to Mývatn, taking a direct course across the northern part of Ódáðahraun to the farmstead of Grænavatn, on the southern side of the lake (Mývatn). This I did with a view to rediscovering the whole of the old highway, the eastern end of which I had already traced. First we shaped our course directly for the northern end of Herðubreiðarfjöll, guided by the beacons to which I have alluded already. We crossed a pass, dividing the easternmost neck of the mountains from the main range, in the eastern approach to which an excessively rough lava, split by innumerable rifts, had to be traversed, in which we succeeded by the mode of scrambling. On the verge of one of the rifts in this lava we came upon a dilapidated beacon, and again upon another on the western defile from the pass, from where we threaded our way along the skirts of a recent and very rough lava, directing our course for the central neck of Herðubreiðarfjöll. Here we were intercepted by two enormous rifts, 100 to 150 feet deep, divided by an earth-slip one mile broad, and twenty miles long. With the exception of Almannagjá and Hrafnagjá, near Thingvellir, these are the largest rifts in Iceland. Having succeeded in bringing our caravan over the eastern brim down alongside the spurs of an isolated "fell," we charged the western brim in vain for a long time until we came upon a sort of steep pass, up through which we brought our ponies, and found upon the verge three dilapidated beacons, which showed that we were still on the traces of the old highway. From this spot beacons may be still traced in a straight direction for Fremri Námur, but recent rifts and lavas have destroyed the road, which, though I now knew its direction, I could pursue no farther. Here, namely, we thought we had overcome all difficulties, but found soon to our cost that we were mistaken. Some distance to the east of Fremri Námur there is a quite recent-looking lava, very long, but narrow, which evidently has welled out of a lava fissure here in 1875, when, besides Askja, Mývatnsöræfi also were in a state of volcanic activity. This lava is not connected with the well-known more northerly lavas of 1875, wherefore its existence has been overlooked hitherto; and when Johnstrup constructed his map of the lava of Mývatnsöræfi, he was not aware of the fact that the same rift which gave birth to the northern lavas of 1875 had, further to the south, given existence to this, which measures fully one-half of the others. To the east of this lava the earth is all cut up by bottomless cracks, over which it was truly a breakneck business to pass. Across some we had to urge our ponies to jump, others we passed by means of natural bridges of loose boulders, which frequently gave way. This was travelling with one's life in one's hand, and to me it is the greatest wonder that no harm resulted to man or beast. To attempt crossing this new lava was entirely out of the question, so we had to bend our way southward along its eastern skirts until we might get round its southern spur. At this end of the lava I observed a peculiar rift not more than thirty to forty feet long and three to four inches broad, on which twelve craters were situated, in every way formed and shaped as large craters are generally, but of such miniature dimensions, that they looked as if they had been intended for toys for children; the aperture of most of them was only four to five inches in diameter, that of the largest two feet. These had, however, squirted forth dashes of lava to the distance of sixty feet. When at last we had reached the southern end of the lava, a new trouble intervened in the shape of what appeared to be an endless rift, and utterly impassable. We had therefore to make up our minds to spending the night, or whatever time might be required, in finding a passage across this barrier; and after five hours' weary struggle we at last managed to scramble across where the main crack split up into smaller ones. This was hard work for our ponies, languishing with thirst and with hardly anything to eat; and perhaps only a degree less arduous for us, who in the matter of food and drink were no better off. Having crossed this serious barrier, we came upon a much more even tract of lava, and presently, to our intense relief, struck a pool of water under a snowdrift in a dent in the lava, where, having watered our horses, we treated them to the last scanty remainder of their fodder, and then went on our way. In the early morning we reached the valley called Heilagsdalr in Bláfjöll, where we were obliged to pitch our tent in order to give the exhausted animals the benefit of the scanty pasture which a few plots of grass offered. After a few hours' welcome sleep we broke up hastily, a gale of wind with rain and sand-drift having burst upon us in the meanwhile. Our course now lay across the spurs radiating to the eastward from Bláfjöll, but such was the violence of the hurricane that it was well nigh impossible to sit on horseback without being blown away, and equally difficult to guard against the despairing animals being blown out of our hands into the howling wilderness. After some really considerable trouble and hardship, we managed to scramble down a precipitous gorge into the upland plateau on which the Lake of Mývatn has found its bed. After having more than once lost our bearings on these lower lava wilds, we succeeded at last in striking the homestead of Grænavatn, exhausted with our exertions, and were glad of a grateful rest in good beds, after having spent a fortnight in a tent, with our saddles for pillows.

TH. THORODDSEN

Reykjahlíð, near Mývatn, August 4

STORAGE BATTERIES

THE importance and desirability of an efficient and economical storage battery have been very widely recognised, but it is at the present time pretty generally felt that no existing form of storage battery is perfect, and that they are on the whole extravagant and wasteful to an extent sufficient to more than compensate for their undeniable convenience. It is perfectly certain that their employment has not become at all general, and that they have failed to realise the somewhat sanguine hopes of their early promoters.

It seems worth while to examine into the causes of this partial failure, and to inquire how far the evil opinion held by many practical men concerning our present method of storing electrical energy is justifiable.

One of the main objections is that storage involves a loss of some 50 per cent. of the whole. Now all methods of storing and transmitting energy involve some loss. To say that any particular method involves a loss of 50 or even 90 per cent. is not to condemn it utterly. There are many cases when the convenience of storage outweighs the evil of waste altogether; three principal ones may be specified.

(1) When the power of the source would be otherwise so completely wasted that every fraction of it stored is clear gain. This is the case of much terrestrial water power. The energy of the tides or of Niagara is enormous, and wholly wasted so far as human activity is con-

cerned; if 50 or even 10 per cent. could be stored in such a way as to be conveniently available, it would be of considerable value, and any arrangement capable of effecting this storage could only with injustice be stigmatised as wasteful. The solar energy of the Carboniferous epoch has most of it been wasted; but a small fraction—probably not a millionth per cent.—has been saved and stored in the Coal-measures. It is possible to abuse the coal for not having stored more, but we find it a useful modicum nevertheless.

(2) A second case when the advantage of storage overbalances the loss is when regularity and continuity of supply is needed, and when the source is irregular and fitful. Wind and wave power illustrate this kind of source; it is manifest that wind power has not been so largely used as it would have been, had it been steady and dependable. A practicable method of storing up its energy and giving it out as wanted would gradually cause it to be very largely employed. This case is also illustrated faintly by a gas-engine or jerky motor of any kind, and the regularity and dependableness of a storage cistern may very well make it desirable to put up with some waste provided it be not excessive. Mechanical devices for approximating to regularity, such as the use of slack driving belts, undoubtedly give rise to a waste of power, and so does any form of regulator. But in the utilisation of artificial forms of power like this, questions of economy become almost pre-eminent; and wastefulness is here a most serious objection, and, it may be, prohibitive defect. At the same time, if the engine is liable to stop, or if it is not always working, some mode of storing energy may be absolutely necessary, whether wasteful or not.

(3) Another case, and to some extent the converse of the last, is when the available source is weak, though continuous, while the power is only needed for a short time, but during that time is required to be great. This is exemplified in the operation of pile-driving, where energy is stored in the slowly-raised weight to be suddenly expended on the head of the pile, also in the operation of drawing a bow; or again when a small waterfall or steam-engine, running continuously, is to be utilised for lighting during five or six hours each day: the obviously right plan in such circumstances as these is to store the energy during the hours it is not wanted, and thus virtually to double or treble the power of the source while it is actually in use. Unless, however, the loss occasioned by storage were reasonably small, there would be but small gain in attempting the process in this third case.

It is plainly advantageous to devise a method of storing that shall give out the greater part of what is put in; but we see by these examples that a reasonable loss may be more than compensated by convenience, regularity, availability, and dependableness. Again, when energy has to be transmitted over great distances, it is in practice difficult or impossible to make the expenditure of energy at one end depend upon and be regulated by its consumption at the other; and so, without some system of storage, great waste will ensue during intervals of small consumption. Looking to the immense development which the transmission of energy may be expected to undergo in the course of the next few decades, a convenient and manageable method of receiving large quantities of transmitted energy, and of holding it in readiness until wanted, must be of prime importance.

It was in view of such applications as these that the invention of the storage battery by Faure was hailed with enthusiasm by the highest scientific authority in Great Britain; while the public, jumping to the conclusion that a thing for which so many uses could be instantly found must needs be a profitable investment, hastened to provide money, not for commencing careful experiments and perfecting the arrangement, which would have been wise, but for manufacturing tons of apparatus in its first crude, immature, and untried form. Some day it may perhaps be recognised that because it can be shown that a thing will be extremely useful when perfect it does not follow that it has already attained that perfection, that indeed probabilities based on historical developments are enormously against such abnormal and instantaneous maturity, and that the careful nursing and rearing necessary to healthy maturity are better given in the seclusion of laboratory and study than in the excited and heated atmosphere of the Stock Exchange. It is doubtless recognised already that all preliminary operations are better conducted on a scale smaller than the wholesale manufacturing one. In developing a new industry there are scientific difficulties to be overcome, and there are manufacturing difficulties. By scientific difficulties we mean such as the determination of weak points, the best ways of strengthening them, and generally the discovery of theoretically the best modes of effecting the object in view: manufacturing difficulties begin with questions of expediency and economy—how most cheaply and satisfactorily to carry out the indications of theory, to obtain this or that material—and include the organisation of a system of manufacture, of division of labour, of machine tools, which shall enable the work to be done with economy, security, and despatch. Over-haste in the preliminary stages causes both these sets of difficulties to be tackled together, and so throws a grievous burden on both adviser and manager. All these untoward conditions have storage batteries experienced; and to say they have not fulfilled the hopes of their early promoters is no more than to say that those hopes were untimely and unreasonable. The period of maturity has been undoubtedly delayed by injudicious treatment, but its ultimate attainment seems to us inevitable; and it is at present a matter of opinion how nearly it has already been reached: certainly great steps towards it have been made. Let us inquire what some of the difficulties encountered have been, and it will be seen that, formidable as some of them are, they belong essentially to an infantile stage, and are not suggestive of constitutional debility.

The first form of manufacture consisted in rolling up sheets of lead and composition, with trousering to keep them separate. The difficulties found were that the coatings would not adhere, but became detached in large flakes; that the trousering got corroded through and permitted short circuiting; and that free circulation of fluid being impossible, the acid became exhausted in some places and concentrated at others, and thus every sort of irregularity began. Now regularity or uniformity is of the most vital and fundamental importance in any form of battery. If any part of a plate is inactive, that part is better away; if any plate in a cell is inactive, it is better away; and if any cells of a battery are inactive, they are infinitely better away. The rolling or coiling up of the sheets being found awkward in practice and liable to detach the coatings, flat plates came to be used, then perforated plates, and then cast grids; these last having such large hole space that they held enough composition, and held it securely enough, to enable the trousering or intermediate porous material to be dispensed with. This was an evident step in advance: free circulation of the liquid became possible, and could be assisted by stirring; there was nothing to corrode except the plates themselves, and the composition, being in the cells or holes of the grid, might be reasonably expected to adhere. So far expectation was not altogether belied. The adhesion was not perfect, it was true, and pieces of composition sometimes fell out of the holes, especially if too powerful currents were passed through the cell, but still it was much better than it had been; and if the plates were well filled, properly formed, and fairly treated, the composition adhered extremely well and securely. The circulation of the liquid was not automatically perfect either, but mechanical agitation could be readily applied; without it the acid near the bottom of the cells tended to become more concentrated than that near the top, not by reason of gravitation undoing diffusion, which is impossible, but because during each charging fresh acid is formed, and in great part falls to the bottom in visible streams. Another great advantage was that some amount of inspection of the plates became possible, and experience as to the actual behaviour and appearance of the plates, began to be accumulated. And painfully varied that experience was. Every variety of extraordinary behaviour which could be suggested as probable, and a good many which no one could possibly have imagined beforehand, made their appearance. The hundreds of tons of batteries made at this period doubtless enabled these unpleasant experiences to be more rapidly acquired than would have been done on a small scale, but it was a costly series of experiments. However, the experiments were made, the public involuntarily assisted in the acquisition of experience, and, caring less for knowledge than for marketable commodities, they expressed dissatisfaction at the result. Many of these incipient difficulties are now overcome by the manufacturers, but the great dislike of the public to involuntary experiments, and the shock which their confidence underwent on being unexpectedly called upon to participate in research, have not yet altogether abated.

The main difficulty now experienced was how to keep the plates from touching. They might be put in wooden frames, or elastic bands might be stretched round each of them, and if they would only keep flat it was impossible they should touch unless the composition should drop out of the holes. Sometimes the composition did drop out of a hole, and bridge across the interval between two plates, but the more common and more fatal experience was that the plates would not keep straight. In a few months the positives were found to swell, and as they swelled to buckle—to buckle and twist into every variety of form, so that elastic bands, wooden frames, and every other contrivance failed altogether to prevent short-circuiting. The cause of the buckling is of course irregular and one-sided swelling, and the cause of the swelling is apparently the gradual peroxidation and sulphating of the material of the bars of the lead grid, which occupy less room as metallic lead than as oxide or salt. As the bars swell, they press on the inclosed composition, occasionally driving it out, but more frequently, and with properly made and treated plates universally, distending themselves and stretching the whole medial portion of the plate. The edge or frame of the grid is stronger than the middle bars, and is not so easily stretched; in a good and uniformly worked plate it does stretch, and an old positive plate is some quarter of an inch bigger every way than a new one, but if one face of the plate is a trifle more active than the other, it is very plain that the most active side will tend to become convex; and buckling once begun very easily goes on. To cure it two opposite plans have been tried: one is to leave the plates as free and unconstrained as possible, hanging free it may be from two points, thin, and with crinkled or crimped margins to allow for expansion; the other is to make them thick and strong, with plentiful ribs for stiffness, and besides to clamp them up one to another as tightly as may be, and thus in mechanical ways to resist buckling and distortion. I do not know that any one could say for certain beforehand which of these two plans would be likely to answer best, but practice is beginning to reply in favour of the latter, and well braced plates of fair thickness show no unmanageable tendency to buckle. It must be remembered that no material can buckle with a force greater than that necessary to restore it to flatness, and this force in the case of lead is very moderate. Hence it may be fairly hoped to overcome and restrain all exuberances by suitable clamps and guides arranged so as to permit flat and even growth, but to check all lateral warpings and excrescences.

Uniformity of action is still essential, especially if all the plates in a cell are clamped together. Plates mechanically treated alike ought to be electrically so treated also, and it is impossible to keep a set of plates working satisfactorily together unless the contact of each is thoroughly and equally good, so that each may receive its fair share of current. Defects of contact have been a fruitful source of breakdown and irregularity. Clamps and screws of every variety have been tried, but the insidious corroding action of nascent oxygen exerted through the film of acid which by spray and creeping forms and concentrates on the lugs—this corroding action crawls between the clamped surfaces, gradually destroys all perfect contact, and sometimes produces almost complete insulation. Contacts on the negative plates give but little trouble; contacts on the positives have taxed a great amount of patience. Lead contacts "burned," *i.e.* melted, not soldered on, are evidently less liable to corrosion than brass or copper fittings, or than any form of clamp, but they are apt to be somewhat clumsy if of sufficient conductivity, and moreover they are awkward to undo again, and somewhat troublesome to do. However they have proved themselves so decidedly the best that now no other contacts will be used, and their re-introduction has been followed by a marked improvement in the behaviour of the cells. So long as contact with one plate was better than with another, a thing quite possible to happen without any difference being perceptible to the eye, so long was it possible for one or two plates to remain almost wholly inactive while another one or two received far more than their share of current, and became distended, warped, overcharged, and ultimately crumbled away. If one or two plates in a cell are black, and giving off torrents of gas, while the rest are brown and idle-looking, it is pretty fair evidence of irregular and insufficient contact, or else of some great discrepancy in the age or make of the plates. This point also is one that was not attended to in the early stages of manufacture; plates were made for stock, and cells were made up with plates of all ages selected at random from the store. Directly uniformity is perceived to be essential, this is recognised as obviously bad. Plates intended to work together should be of the same age and make, and inasmuch as keeping does not improve them, the best plan is not to make for stock, but to keep material ready, and then quickly make up as wanted. Plates in work deteriorate slowly, but they are wearing out in the fulfilment of their proper function; plates in idleness deteriorate as quickly, and they are rusting out in fulfilment of no function at all. Worn-out plates, however, are by no means valueless. Lead material has a well recognised price, and if attention were given to the subject, it is probable that decrepit and useless plates might be made to yield a very large percentage, if not the whole, of their original lead. For it must be remembered that plates deteriorate not by waste but by accretion: an old plate contains as much lead as a new one, but it contains it with the addition of oxygen and sulphion; no longer a tenacious coherent frame, but a crumbling mass of incoherent powder.

The age of plates is a point of vital interest, though but little is known as to the possibilities in this direction at present. A year may be regarded as a fair average age at the present time; but this is a low rather than a high estimate. Thick plates are found to last far longer than thin, which is only natural when it is remembered that the wearing out is due to corrosion, that corrosion proceeds mainly from the surface inwards, and that the internal portions of a thick plate are to a great extent protected by the mass of superincumbent material. If it can be shown, as we understand it can, (1) that the cost of materials is far more than the cost of manufacture; (2) that the worn-out material has a market value not incomparably less than the original; and (3) that the frequency with which plates have to be renewed is not such as to cause much inconvenience; then we hold that the first stage of the durability difficulty has been overcome. Much more may be hoped for in this direction as experience increases, and it is not extravagant to hope that a well-ribbed, properly-clamped, and fairly-treated thick plate may last as long as five years before it becomes disintegrated.

It is evident, however, that in a region where pure experiment is pre-eminent, and where the units of time are months and years, instead of hours and days, the accumulation of experience is a slow and tedious process. It is no use making statements involving periods of five years when no one has had the present improved form in use for so much as six months. Nevertheless it is possible to see that the present cells are better than their predecessors; and as their predecessors have lasted in good condition for a year and more it is not presumptuous to indulge in well-founded hopes. Many of the difficulties connected with the early forms of battery were aggravated by Utopian notions concerning internal resistance and compactness. The internal resistance of a cell was so beautifully small, that the manufacturers were tempted to diminish it still further by putting the plates far too close together. An eighth or tenth of an inch interval is well enough if the plates had been hard rigid slabs of perfect flatness; but it was madness to pack flexible lead plates full of composition certain to swell and liable to drop out so near together as this. Security and dependableness were sacrificed to a natural desire for sudden and Utopian perfection. We may hope that these lessons have been profited by, and that the manufacturers perceive that confidence and security are the first conditions of success, and that minutiæ as to the number of noughts before the significant figures in the specification of resistance begin, though those also are of importance in their turn, are yet of quite secondary consideration. Moreover, this packing of the plates so closely did not really do much to secure the result desired; the greater part of the resistance of half run-down cells is not in the liquid between the plates, but in the surface or scum separating each plate, and especially each negative plate, from the liquid, and hence putting the plates a safe distance, say a quarter or one-third of an inch, apart exerts an effect on the total resistance which is certainly far more than compensated by the ready opportunity thus afforded for access by both sight and touch. The old opaque boxes chock full of plates, with slight india-rubber bands between them, were started and left to Providence. No one could see what went on, nor could one readily get at anything to rectify what was wrong. In the present glass boxes properly arranged on accessible shelves with only plugs or studs between the plates, clear vision through the cell in any direction is easy, and accidental obstruction not only very seldom occurs but if it does it can without difficulty be seen and removed. But it must be granted that these boxes are less compact than their predecessors, and for some purposes, such as locomotion, compactness is of the first importance. Most true, for some purposes. It is not to be supposed that one type of cell will answer every possible demand. A dynamo to be highly efficient must have a large and massive field magnet, but in some places bulk and weight are fatal objections, and in these places smaller and more compact dynamos may be more suitable: something, however, must be given up to secure the required lightness and compactness, some sort of compromise must be effected. Just so with cells: we can point out what is theoretically the best form, and this form may, for large stationary electric light or power installation, be actually the most suitable; but we may also see that for boats, for tramcars, and for fish torpedoes, some very different and far more compact form may be quite essential.

Efficiency, Durability, Economy, Compactness: it may not be possible to attain all these at once—if it were, there would be small room for discussion—but sometimes one and sometimes another will be the pressing necessity, and manufacturers of storage batteries, like manufacturers of dynamos, must be prepared with forms suited to various needs.

We have spoken mainly of difficulties connected with the positive plates, and have said nothing concerning the negatives. It is not that these are not susceptible of improvement, but their faults have been of a less imperious and obtrusive nature. They are not perfect, but they do fairly well, and there has been little need to worry much about them, until the extraordinary behaviour of positives had been taken in hand and checked. The time is coming to attend to these also. They fail not from exuberance, but from inertness. As they grow old, they do not swell, and warp, and burst, and crumble, like the positives, but they grow quietly hoary, and serenely decay. The composition in a worn-out negative consists of white sulphate through and through, but the frame remains intact, and it consequently never falls to pieces, nor does it swell. Impurities in the acid used tell upon a negative plate—nitric acid is fatal. Acid much too weak or very much too strong is also deleterious, and idleness is bad. The difficulties connected with negatives mostly depend on their aggravating property of always requiring a quite opposite treatment to positives. The less a positive is formed and overcharged the better. A negative delights in complete formation and frequent overcharge. In recognition of this it is now customary to form them separately, and to give the negative a thorough dose of hydrogen without commencing the corrosion of the positive by an overdose of oxygen. When the discharge from a cell begins to flag, it is the resisting scum of sulphate that has formed over the negative plate which is responsible for the flagging. The true E.M.F. of a cell is wonderfully constant throughout the whole discharge; but the internal resistance is all the time increasing, at first very slowly, ultimately, towards the end, with a rush. One such run-down cell in the midst of a lot of others therefore obstructs the current terribly. If only a series of cells could with certainty be made to work together uniformly, if a series could behave as well as some of the cells in it, no one would have cause to complain.

Through the whole history of the manufacture, from the very beginning, a few cells here and there have always exhibited astonishing efficiency;—the aim of manufacturers may be said to be to bring all cells up to the level of a few. Much progress in this direction has been made, and it may be very fairly expected that, as uniformity is gradually attained, a series of cells subjected to the same treatment may behave in the same manner. Whenever this is certainly accomplished, there will have been reached a high stage of efficiency, beyond which further progress need be only in the improvement of comparably insignificant minutiæ.

The subject of the electrical storage of energy is really one of national importance;—it is comparatively a small matter whether this or that form of storage, or this or that company of manufacturers, succeeds in bringing out the permanent form. It sometimes unfortunately happens that enterprising pioneers only clear the way, and retire just in time for other men to come in and reap the fruits of their labours. So much capital and so much labour have been already expended in the effort to bring storage batteries to perfection, so great progress has been made, and so apparently small are the steps which yet remain to be accomplished, that we may surely fairly hope that some of the original believers in their great, and as it seems to us inevitable, future may yet live to see their faith justified and their patience rewarded, and may even taste some of that so-called "substantial" reward without the hope of which great commercial enterprises would never be undertaken, and modern civilisation would have scarcely yet begun.

O. J. L.

METEOROLOGY OF THE LOWER CONGO[1]

IN this work Dr. Danckelman has made a valuable contribution to the meteorology of Africa. The observations, which are printed *in extenso*, were made at Vivi, lat. 5° 35′ S., long. 13° 52′ E., at a height of 374 feet, from May 1882 to August 1883. The hours of observation were 7 a.m., 2 p.m., and 9 p.m., and to these were added for the six months ending May 1883 observations at 6 a.m. and 8 a.m., which thus furnish important data regarding the march of diurnal phenomena for the first three hours after sunrise. The full details which are given of the instruments employed, their exposure, and the methods of observing are particularly satisfactory. While the instrumental observations are very complete, no less care has been taken to make the non-instrumental observations of weather equally complete, and these have been planned and carried out to aid in discussions affecting both local and general meteorology.

In resuming and discussing these sixteen months' observations, Dr. Danckelman has conjoined with the results obtained for Vivi the results of observations made at St. Thomas, Gaboon, Chinchoxo, Loanda, and Melange, these places roughly representing the west of Africa from near the equator to about lat. 10° S. At all these places the annual minimum pressure occurs in February or March and the maximum in July, with a small secondary maximum in January. At Vivi atmospheric pressure at 32° and sea-level is 29·932 inches in February, 30·117 inches in July, and 29·997 inches for the year.

At Vivi the mean annual temperature is 76°·4, the lowest monthly mean being 70°·5 in August, and the highest 79°·5 in February. The highest observed temperature was 97°·2 on November 5, 1882, and the lowest 53°·8 on July 29 of the same year. A noteworthy feature of the climate of Vivi is the relatively low temperature from June to September; during the other eight months the means vary only from 77°·4 to 79°·5. This feature is common to the whole of this region of West Africa; and it corresponds to the dry season of the year, which, as regards the Lower Congo, is characterised by Dr. Danckelman as undoubtedly the most agreeable and the finest, as well as the healthiest, season of the year. On the other hand, on the elevated plateaux of the interior, the heat is very great during the day, and many maladies prevail among the natives, numbers of the ill-clad blacks succumbing to the diseases caused by exposure to the low temperature of the nights. The temperature of the Congo was observed at intervals during the year, the observations being made at a part of the river where the current was rapid. The results give a mean annual temperature of 81°·8, being thus 5°·4 higher than that of the air.

Of the winds observed at Vivi the percentages are south-west, 39; west, 15; west-south-west, 9; north, 8; and calms, 18; winds from any other direction being of rare occurrence. Thus of the 82 per cent. of wind-directions observed, 63 per cent were from the south-west quadrant. During the whole year south-west winds predominate in the afternoon, but during the dry season the wind frequently veers to west towards evening, so that at 9 p.m. west and west-south-west winds are more frequent than south-west, whilst south and south-south-west winds very rarely occur. This striking predominance of south-westerly winds has important bearings on the climatology of the whole of the Lower Congo as respects humidity, cloud, and rainfall. A striking peculiarity of the climate are the strong winds which often set in late in the afternoon and evening, and which are carefully recorded in the journal of the observations. The following are the number of times such winds have occurred each hour from 3 p.m. to 11 p.m.,—6, 11, 24, 28, 30, 32, 22, and 4, the hours of greatest frequency thus being from 5 p.m. to 10 p.m. During the other hours of the day they seldom occur. The degree to which this prevails is seen from the fact that the mean force of the wind is greater at 9 p.m. than at 2 p.m., the latter hour being the time about which the wind generally attains its maximum diurnal velocity over the land.

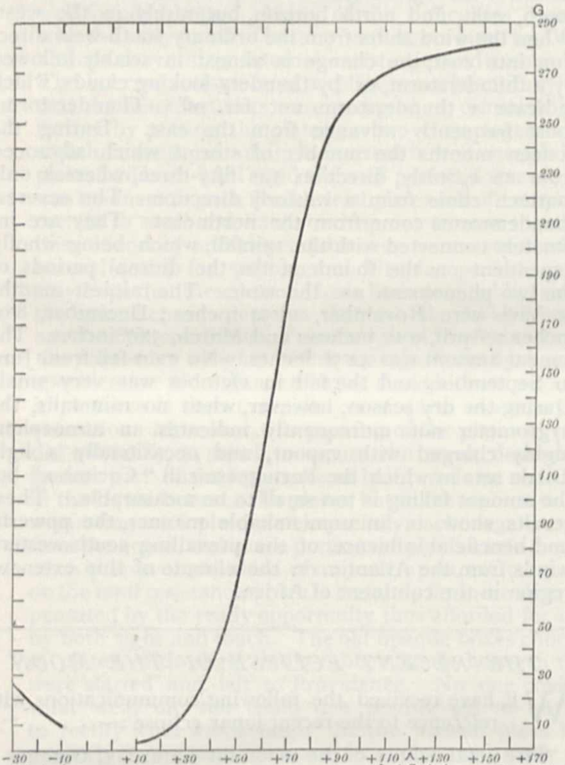
Thunderstorms are of frequent occurrence from November to May, but none were recorded from June to October. As regards their distribution during the day, scarcely any occurred from 11 p.m. to noon, the period of maximum frequency being from 1 p.m. to 8 p.m.; but particularly from 4 p.m. to 6 p.m., 29 per cent. of the whole having occurred during these two hours. During the rainy season heat lightning is of frequent occurrence in the south, east, and north horizon, but rarely in the west. When the wind shifts from the ordinary south-west direction into east, the change is almost invariably followed by a thunderstorm, or by thundery-looking clouds, which indicate a thunderstorm not far off. Thunderstorms most frequently advance from the east. During the sixteen months the number of storms which advanced from an easterly direction was fifty-three, whereas only fourteen came from a westerly direction. The severest thunderstorms come from the north-east. They are intimately connected with the rainfall, which being wholly dependent on the thunderstorm, the diurnal periods of the two phenomena are the same. The rainiest months at Vivi were November, 11·34 inches; December, 8·94 inches; April, 9·10 inches; and March, 5·67 inches. The annual amount was 42·56 inches. No rain fell from June to September, and the fall in October was very small. During the dry season, however, when no rain falls, the hygrometer not unfrequently indicates an atmosphere highly charged with vapour, and occasionally a light drizzle sets in which the Portuguese call "Cacimbo," but the amount falling is too small to be measurable. These results show, in an unmistakable manner, the powerful and beneficial influence of the prevailing south-westerly winds from the Atlantic on the climate of this extensive region in the continent of Africa.

[1] "Mémoire sur les Observations Météorologiques faites à Vivi (Congo Inférieure), et sur la Climatologie de la Côte sud-ouest d'Afrique en général." Par Dr. A von Danckelman. (Berlin, 1884.)

THE RECENT ECLIPSE OF THE MOON

WE have received the following communications with reference to the recent lunar eclipse:—

THE total eclipse of the moon on Saturday, October 4, was of particular importance as it was—since the one in 1877, which was practically lost through bad weather—the first opportunity of measuring the changes which the heat radiated by the moon undergoes with the proceeding eclipse. And indeed on Saturday evening at six o'clock it appeared highly probable that the fate of this eclipse would be the same as that of its predecessor, the sky being thickly covered with misty clouds, with hardly any motion whatever. Suddenly, however, as if by magic, thirty-two minutes before the beginning of the total phase, they all disappeared, and left a most perfect and exceptional clearness behind them, which not only lasted during the remainder of the eclipse, but also through the two following days and nights. Thus I was enabled to carry out the greater part of my programme in every detail. The apparatus used was the same which the Earl of Rosse described in his communications to the Royal Society in 1869, 1870, and 1873; it condenses by means of two small concave mirrors alternately on two thermopiles the rays of the moon collected by the 3-foot telescope, and the currents thus created are measured by the aid of a galvanometer with mirror and scale. The time of exposure was 1 min. sid. time for each pile, and the observations were throughout the eclipse carried on with only two interruptions of eight and nine minutes, the former caused by my examining the condensing mirrors for dew, the latter by the re-winding of the driving-clock of the telescope. Thus I took altogether 211 readings of the galvanometer, which were taken together in groups of ten for the formation of reliable mean values. It is these means—without the application of the still necessary reductions—which form the ordinates of the preliminary curve inclosed, the abscissæ of which are the times before the beginning and after the end of the total phase. It will be seen that no observations were made during the total phase, as I could not make certain that the moon's image was actually on the condensing mirrors; besides the readings of the galvanometer on near approach to the total phase became so unsteady and irregular that it was plain that the effect, if any, fell below the sensitive-



Curve showing the change of the lunar radiant heat as measured by thermopiles and galvanometer during the total eclipse of the moon on October 4, 1884, at Birr Castle Observatory, by Otto Boeddicker.

ness of the instrument and the unavoidable errors of observation. This is much to be regretted, as the few observations obtained before the total phase began show plainly that the minimum of the radiated heat takes place later than that of the moon's light. Indeed, so slowly did the readings of the galvanometer increase again that about twenty minutes after the total phase was over the almost entire absence of any effect led me to think that the small condensing mirrors must be covered with dew, an apprehension which was happily not verified. I should add that the galvanometer readings here given are not comparable with those published in 1873.

OTTO BOEDDICKER

The Earl of Rosse's Observatory, Birr Castle,
Parsonstown

THIS eclipse was seen from here (height above sea 530 feet) to great advantage in a cloudless sky, with the stars very brilliant and the air calm.

For some minutes before the actual contact a faint smoky look was visible, and this had the effect of flattening the edge of the moon on the side towards which the shadow was approaching. At 8h. 16m. there was a slight but very perceptible shadow, about a fourth of the moon's diameter in advance of the dark shadow, and in four minutes later the eclipse just entered the moon. At 8h. 44m. the margin of the dark shadow, which had had all along an edge of a woolly and irregular appearance, had now decided streaks in advance of it. The edge of the moon on the opposite side from the base to within 30° of the apex was marked by a rim of intense bright *blue*. Ten minutes later the blue margin was less brilliant and narrower, and the woolly appearance before mentioned was more decided, but less streaky. At 8h. 58m. the *blue* had contracted in length both at the base and apex, and gradually assumed a greenish shade. The cusps of the moon were elongated and more drop-like than continuous. At 9h. 18m. the shadow had almost covered the moon, which showed a faint glow beneath, though the circular appearance was not visible to the naked eye. A minute later, as the last point of light disappeared, a dense blackness of irregular form appeared on the opposite portion of the moon, extending over nearly a third of the surface. At 9h. 39m. the moon was scarcely perceptible to the naked eye. Through the telescope a faint luminosity at the apex could be discerned. At 9h. 47m. to the naked eye the moon seemed like a blurred star, very indistinct and considerably reduced in apparent size, and of no definite form. At 10h. 2m. the faint luminosity at the apex had almost gone, and the outline of the moon was more apparent through the telescope. The blurred, star-like appearance to the naked eye was still unchanged. At 10h. 9m. there was a thin luminosity on both horizontal sides of the moon. The apex of the moon was very indistinct. At 10h. 20m. a broader belt of light appeared on the northern side, but that on the opposite side was indistinct. There was still the same indistinctness to the naked eye, though every now and then a faint crescent-like appearance could be seen. At 10h. 38m. a somewhat broader crescent. At 10h. 43m. a bright light like that of the moon was visible to the naked eye at the apex, and four minutes later the moon reappeared. At 10h. 57m. the tip of a mountain was lit up near the moon's apex. At 11 p.m. the upper cusp extended further than the lower one. At 11h. 15m. a slight haze formed a burr about the moon, which was somewhat kidney-shaped. At 11h. 22m. the burr still visible, but much rounder in form. The eclipse ended at 11h. 45m.

Having previously observed a number of lunar eclipses, I was specially struck with that of to-night; the density and blackness of the shadow was far greater than any previous one that I had seen, especially throughout the period of totality. In all previous eclipses I have been able to trace the outline; in the present case this was quite impossible. Usually the outline is more or less plainly seen, and not unfrequently there is a strong reddish light. The last total eclipse of the moon was seen from this place, and it had a copper-coloured appearance, and there was no difficulty in tracing the outline. In the present eclipse the moon's outline was invisible for some time to the unaided eye, and the apparent size so much diminished that it had more the appearance of a large star whose light was just able to pierce through a dense haze. Some of the valleys of the moon were left for a time in great darkness after the light had travelled beyond them. I may mention that all the above appearances were also seen by Major A. E. L. Lowe and Mr. H. L. P. Lowe, who were using other telescopes.

E. J. LOWE

Shirenewton Hall, near Chepstow, October 4

IT is to be hoped that the unusual phenomena attending the late eclipse of the moon may lead to a discussion which will throw some light upon the great differences observed on different occasions in the visibility of the eclipsed moon—differences which have not hitherto, so far as I know, been adequately explained.

In the late eclipse, as viewed from this station (where the sky was not only perfectly cloudless, but free from the least suspicion of haze), the obscuration of the moon was carried to a degree far beyond anything witnessed in the eclipses of recent times. For some time before and after the middle of the eclipse (that is, about 10 p.m.) the only trace of our satellite that remained in the sky was a faint dingy-brown nebulous spot, to which it was impossible to assign any definite form or dimensions, but which certainly did not approach the moon in point of apparent size. So inconspicuous was it that it was quite invisible through the window of a room in which lights were burning; and in the open air, if one had not known exactly where to look for it, one might have searched for some little time without discovering it. I speak of course of the naked-eye appearances. With an opera-glass the nebulous spot was resolved into a well-defined disk of the proper dimensions, but still very faint and dingy, the hue being a kind of reddish-brown. It was further remarked that the illumination was uniformly distributed over the disk, at least so far as this, that there was no preponderance of light in the direction of any one part of the moon's edge. This is what should naturally have followed from the central character of the eclipse, but it seems desirable to note the circumstance with reference to a theory presently to be mentioned.

The most obvious explanation of the unusual obscurity of the moon would be its unusually deep immersion in the earth's shadow, but this view seems to be clearly disproved by a comparison with the phenomena observed in a former eclipse. Referring to a note which I made at the time of the eclipse of August 23, 1877, I find the following remark:—"The moon, even in the middle of the total phase, was a conspicuous object in the sky, and the ruddy colour was well marked. In the very middle of the eclipse the degree of illumination was as nearly as possible equal all round the edge of the moon, the central parts being darker than those near the edge." Now the duration of totality in that eclipse was 1h. 44m. In the late eclipse it was 1h. 32m. The immersion of the moon in the earth's shadow must therefore, I presume, have been at least as deep on the former occasion as on the recent one. It may be mentioned as an additional argument against this explanation that in the late eclipse the visibility of the eclipsed portion was observed to be much less than usual even before the eclipse was complete. In fact it was not until within a few minutes of the total phase that the eclipsed portion could be certainly distinguished with the naked eye.

Another obvious suggestion in the way of explanation has reference to variations in the condition of that portion of the earth's atmosphere through which the sun's rays would pass to reach the moon. This explanation is not without interest in connection with the remarkable sunset effects of last winter, and, in a less degree, of the present autumn. But there are serious difficulties in the way of accepting it, for, in order to account for the observed phenomena, it would be necessary to suppose that an entire ring of the earth's atmosphere was uniformly affected. A want of uniformity in this respect would not cause merely an uneven illumination of the moon's disk, which some observers seem to have noted, and which may very well be set down to the actual differences on the surface of the moon; the effect would be specially marked upon some part of the moon's edge, and would be similar to what is observed soon after totality has commenced and shortly before it ends. Nothing approaching to this appearance was to be seen in the late eclipse at the time when the obscuration was greatest.

Is it possible that the surface of the moon may be in some small degree self-luminous, and that a variation (from unknown causes) in the degree of this self-luminosity may account for the difference observed in the visibility of the moon in two eclipses, in both of which the solar light was equally at its minimum?

Clifton, October 7 GEORGE F. BURDER

ON the occasion of the total eclipse of the moon on Saturday, October 4, the Director of the Pulkowa Observatory, near St. Petersburg, issued a circular to a number of other observatories suggesting the use of the opportunity to fix the exact diameter of the moon, the mean value of the true diameter not being known to a second. Even as regards a probably existing depression of the surface of the moon, we know only that it cannot be very great. The circular also requested observers to watch attentively all stars, even those of the tenth magnitude, eclipsed by the moon, and their egress on the other side, which is only possible during a total eclipse. In order to make these observations exact, Prof. Döllen, of the Pulkowa Observatory, calculated the number of such stars covered by the moon on that date, which he finds were 116, most of which are of the ninth and tenth magnitude and only one of the sixth. He has, moreover, for the use of observers, calculated the exact position of these during the eclipse for no less than sixty observatories from Pulkowa to the Cape, which is expected to give the desired result.

NOTES

OUR readers will hear with sincere regret that Prof. Huxley, under the orders of his medical advisers, left England yesterday for some months of absolute rest. When it is remembered how many functions Prof. Huxley has to fulfil, we need hardly say that the cause of his enforced retirement for a time is overwork. His presence in England will certainly be missed during the coming winter, and he may feel assured that he carries with him the sympathies of many friends, known and unknown. Prof. Huxley goes in the first instance to Venice.

WE heartily support the suggestion which has been made in the *Gardeners' Chronicle* that it would be most appropriate that some memorial of the late Mr. Bentham should, subject to the consent of the authorities, be placed in the Royal Herbarium at Kew, to which he was such a benefactor, and in connection with which his life-work was for very many years carried on. Official etiquette would probably preclude any steps being taken by the authorities at Kew in the matter; and, indeed, it is a subject that would more gracefully and appropriately be dealt with by outsiders.

SIR WILLIAM HARCOURT is always happy when he touches on science, either to point a political shaft, or, as on Tuesday at Derby, when descanting directly on its progress and bearings. He showed himself well versed—as one bearing the honoured name of Harcourt ought to be—in the history of scientific progress, and in the high importance of science apart from its utilitarian uses. Scientific study, in his conception, is above all others "the most useful and the most ennobling." "Depend upon it," he concluded, "if I may turn for a moment to the utilitarian view, these are not days in which we, as a people, can afford to be idle or to be ignorant. There is an immense competition going on in the world in all departments of trade. Remember you have the competition of countries where education of this character is of a more complete character, far more complete than anything we have in this country. If you go into Germany you will find in every small town that there are institutions where the severest education is given in all departments of technical knowledge. The old days when people could afford to go on in an easy, happy-go-lucky sort of manner are gone by. You may depend upon it, in the race which we have to run in the world, a training of the severest description is requisite, so that we may hold our own." We trust Sir William will bear his admirable Derby utterances in mind when as a legislator he has to consider the relation of science and of scientific workers to the Government and the country.

PROF. HUGO GYLDÉN, Director of the Observatory at Stockholm, and well known for his studies during recent years of the eight great planets, has been offered and has accepted the Professorship of Astronomy at the Göttingen University.

HIS MAJESTY the King of Italy has conferred the decoration of Knight of the Crown of Italy upon Deputy-Surgeon-General Francis Day, formerly Inspector-General of Fisheries in India, and lately Commissioner of the Indian Section in the great International Fisheries Exhibition.

DR. SOPHUS TROMHOLT having returned to Bergen after a lengthened sojourn in Iceland, whence we have from time to time received accounts of his researches, intends for the next few years to devote his time to the production of a great catalogue of all the auroræ seen in Northern Europe from the earliest times. The Norwegian Government have granted a not inconsiderable sum towards this gigantic undertaking. As this labour and the i mminent production of his new work, "Under the Rays of the Aurora Borealis," will occupy all his time for some while, the distinguished *savant* announces his inability to issue for the coming winter such sheets for the recording of auroræ as he has for some years been in the habit of distributing over all parts of Northern Europe. He trusts, however, that observers may continue to note the phenomena as heretofore on the lines laid down by him, and forward the same to him with as little delay as possible. His address is Bergen.

CHANDA SINGH, a blind student of St. Stephen's College, Delhi, is, according to the account given in *Allen's Indian Mail*, a prodigy. He cannot read or write, but possesses such a strong memory as to be able to repeat all his text-books, English, Persian, or Urdu, by rote, and to work out sums in arithmetic with remarkable rapidity. The unusual intensity of his mental powers is shown by his ability to multiply any number of figures by another equally large. At the last University examination he was examined *vivâ voce* by order of the Director of Public Instruction of the Punjaub, and he stood twenty-seventh in the list of successful candidates. On the recommendation of the same official, the judges of the local court have allowed him to appear at its law examination. Memory, as is well known, is wonderfully developed in Orientals, owing to the system of education which has obtained amongst them; but cases like Chanda Singh must be very rare even in the East.

DURING the last week of September the Thüringo-Saxon Verein für Erdkunde held its annual meeting at Kösen under the presidency of Herr Dunker (Halle). Amongst a number of interesting papers read we note the following:—On Baku and its naphtha and petroleum wells, by Herr Eberius (Döllnitz); on the scientific and economical importance of Cameroon, by Prof. Kirchhoff; on the limits between the High and Low German on the eastern side of the Elbe, by Dr. Haushalter (Rudolstadt); on the salt and fresh-water lakes between Halle and Eisleben, by Prof. Kirchhoff; on ancient places of worship in Northern Thüringia, by Dr. Rackwitz (Nordhausen).

THE thirty-second annual meeting of the German Geological Society took place at Hanover in the last week of September under the presidency of Herr von der Decken. Among the papers read we note the following:—On the limits of the Dyas formation, by Prof. Geinitz (Dresden); on the geology of North-Western America, by Prof. vom Rath (Bonn); on the Brachiosaurus occurring in the Hessian limestone, by Prof. Credner Leipzig); on the geology of the Harz Mountains, by Herr Langsdorff (Klausthal); on the occurrence of dolerite in the Vogelsberg, by Prof. Streng (Giessen); on Tasmanian tin ores, by Herr Gordaek (Klausthal); on new Devonian Bryozoa, by Dr. Bornemann (Eisenach). The next place of meeting will be Darmstadt.

WRITING on a subject of some interest at the present time, viz. the orthography of the names of the better known Chinese places, a correspondent of the *Tablettes des deux Charentes* says that *Tonquin* is more correct than *Tonkin* (and we presume also than *Tongking*, *Tonking*, &c.), for this is how the name is pronounced. He thinks that French pronunciation generally approaches that of the Chinese more than the English. Thus the Chinese have the nasal sound of *n* and a sound of *u* which we in England do not possess. We cannot always reproduce these Chinese sounds, while the French can do so easily. To represent the nasal sound the English add a *g* to the *n*, but they do not always pronounce it. The French borrow the English orthography, but they pronounce the *g* which the former have added, and thus completely disfigure the names. Thus (the correspondent goes on) the French pronounce *Shangai* instead of *Shanhaï*, and *Hongue-Kongue* in place of *Hon-Kon*. The most curious instance is that of *Canton*. The English call the province *Kwang-toung;* all the French journals follow this orthography, and yet it is Canton pure and simple. If the *k* is to be employed at all it should be written *Ton-Kien*, for we write *Fo-Kien* as the name of the province in which Foochow is situated, and the *Kien* is the same in each instance. The truth appears to be that all Far Eastern names have been transliterated haphazard, and almost in every case by people who knew nothing of the native languages. The older orthographies, such as Canton, Whampoa, &c., we owe to masters of ships, supercargoes, and the like, who visited the place in the last or beginning of the present century. In some cases they are not good attempts at reproduction, but little practical inconvenience has ever been found in adhering to them. According to the writer whom we have quoted the French are in a worse plight than ourselves; our orthography was at least an honest attempt to reproduce the names as they sounded to Englishmen. The French adopt this orthography, and then give the letters their French pronunciation; in other words the transmutation is (1) Chinese as it sounded to an Englishman; (2) that Englishman's Chinese as it sounded to a Frenchman! Little wonder then that there are hopelessly irreconcilable methods of spelling Chinese names of places.

THE Japanese Commissioner to the Health Exhibition writes that the omission of the names of the foreign authors of certain scientific works in the Japanese Section was quite unintentional. He adds that three of these works are by Japanese assistant professors. The omission, on which we commented last week in noticing the Catalogue, coupled with the statement that the English volumes were translations, was certainly calculated to leave an erroneous impression as to the authorship of the works in question.

THE second document just issued (Brockhaus, Leipzig) in connection with the "Riebeck'sche Niger Expedition," is like the first, which dealt with the Fulah language, mainly philological. The chief contents are: "Specimens of the Language of Ghât in the Sahara, with Haussa and German Translations," by the learned leader of the expedition, Herr Gottlob Adolf Krause. The Ghât being merely a variety of the Mashagh (Berber, or Western Hamitic), no special grammar was needed of a language which has already been somewhat fully elucidated by Barth, Hanoteau, Prof. Newman, and Stanhope Freeman. But these texts in the Arabic and Roman characters with interlinear German translations, and free Haussa and German versions, will be accepted as a boon by students of the languages of Sudan and North Africa. The accompanying "Anmerkungen" throw considerable light on many obscure grammatical points, besides bringing into still closer relationship the Semitic and Hamitic groups, whose fundamental affinity is daily becoming more and more obvious. The widespread relations of what Herr Krause calls the "Haussa-Musukanische Sprach-Stamm" (Haussa-Musgu linguistic family) are also elucidated, and the curious principle of vowel harmony prevalent in this group for the first time clearly explained. It differs entirely from the Bantu, which is initial, and also from the Finno-Tatar, inasmuch as in the latter the vowels of the agglutinated postfixes conform to that of the root, which is never modified, whereas in Haussa-Musgu the root-vowel conforms to that of the postfix, and is consequently subject to constant change. Thus: *a-dara* = he loves; *e-diri-kini* = he loves you, where the preceding pronominal and root-vowel *a* has been throughout modified to *e* and *i* by the influence of the following *i* of the pronominal suffix *kini*. The introduction contains many interesting details on the history of the Ghāt oasis, which most of our readers will learn for the first time was incorporated in the Turkish Empire some ten years ago. Here also some fresh light is thrown on the origin of the national or imposed names Mashagh (Imoshagh), Tuarek, Berber, Moor, Tibu, Fezzan, and Ghāt. For his munificence in undertaking the publication of these "Mittheilungen" without the remotest prospect of any pecuniary returns, Dr. Emil Riebeck has earned the lasting gratitude of the scientific world.

IN the June number of *Timehri*, a journal steadily growing in scientific importance and general usefulness, the accomplished editor, Mr. E. F. Im Thurn, continues his valuable "Notes on West Indian Stone Implements." The title of these papers is now enlarged by the additional words, "and other Indian Relics," so as to include all objects, whether of stone, shell, bone, or clay, which are often found associated together in such a way as to render their separate treatment almost impossible. This enlargement of the subject cannot fail to be appreciated by ethnologists, who will here find much instructive matter lucidly arranged, and illustrated by eleven plates containing thirty-five fac-similes of stone and shell implements, and twenty-one of Carib pottery. The first group of objects belong to Sir Thomas Graham Briggs, of Barbados, who has placed his fine collection at the disposal of Mr. Im Thurn, at the same time generously providing the means for the due illustration of the series. The second group forms part of a quantity of native (Carib) pottery recently discovered on the Enmore Plantation, west coast of Demerara. The urgent necessity of encouraging collections of this sort before all have disappeared, like the natives themselves, before the advance of civilised man, is made evident by the statement that a large heap of shell implements lately found in the parish of St. James, Barbados, were carted away "to macadamise a road." Other attractive papers in this number are: "Bush-Notes of a Huntsman," by M. McTurk; "The Mountains of the West Indies," by T. P. Poter; and "Essequibo, Berbice, and Demerara under the Dutch" (continued), by the Editor.

THE following announcements are made by Mr. Edward Stanford:—"A Parliamentary County Atlas of England and Wales," containing maps of all the counties engraved on a uniform scale. This atlas includes as a distinctive feature a series of physical, statistical, and administrative maps of England and Wales and of London. The rainfall, barometric pressure, temperature of the air and of the seas around our coasts are shown for every month of the year; a river basin map, with an accompanying table giving the area of each river basin and the length of the chief water channel in each basin; an orographic map, indicating by colours the plains, hills, and mountains, with much other information of interest. A short description of each county accompanies the maps. "A Trigonometrical Survey of the Island of Cyprus," executed by command of His Excellency Major-General Sir R. Biddulph, K.C.M.G., C.B., R.A., High Commissioner, under the direction of Major H. H. Kitchener (Captain R.E.), Director of Survey, assisted by Lieut. S. C. N. Grant, R.E. The map is drawn to a scale of one inch to one statute mile = 1 : 63,360, the scale of the Ordnance Survey of the United Kingdom; it has been engraved on fifteen copper plates, and will be printed on imperial sheets, forming, when bound, an atlas measuring 15 inches by 22 inches, or, when mounted together, one map measuring 12 feet 6 inches by 7 feet. "A School Map of British Colonies and Possessions," drawn on a uniform scale, and agreeing in style, size, and price with the other maps of Stanford's Series. Also a second and much enlarged edition of the "Geology of Weymouth, Portland, and the Coast of Dorset," with coloured geological map, section, and photographic frontispiece; "The Countries of the World," the fifth and concluding book of the Geographical Readers, by Charlotte M. Mason, containing Asia, Africa, America, and Australasia; the concluding volume of Stanford's "Compendium of Geography and Travel—Europe," by F. W. Rudler, F.G.S., and G. G. Chisholm, B.Sc., edited by Sir Andrew C. Ramsay, LL.D., F.R.S., with ethnological appendix by A. H. Keane, M.A.I., illustrated with fifteen maps and numerous cuts; "The Monuments of Athens: a Historical and Archæological Description," by Pomagistes G. Kastromenos, translated from the Greek by Agnes Smith, author of "Glimpses of Greek Life and Scenery"; "The Visitor's Guide to Orvieto," by J. L. Bevir, M.A., Assistant Master at Wellington College.

E. AND F. N. SPON have in the press "Candles, Soap, and Glycerine," a practical treatise on the materials used and processes involved, by Mr. W. Lant Carpenter, B.Sc.; "A Text-Book of Tanning," embracing the theory and practice of preparing and dyeing all kinds of leather, by H. R. Procter, of Lowlights Tanneries, Examiner in Tanning to the Guilds Institute; "On Portable Railways," by Mr. Paul Decanville, M.I.M.E.; "History and Description of the Manchester Water-Works," by J. F. La Trobe Bateman, F.R.SS. Lond. & Edin., Past President of the Institution of Civil Engineers, F.G.S., &c.; "An Electrical Supplement to the Pocket-Book of Engineering Formulæ," by G. L. Molesworth, M.I.C.E., M.I.M.E., Consulting Engineer to the Government of India for State Railways; "On the Analysis of Iron and Steel," by T. Bayley, author of "The Chemist's Pocket-Book"; a new edition of "The Modern Practice of Sinking and Boring Wells," by Mr. Ernest Spon, Assoc. Mem. Inst. C.E.; Spon's "Mechanics' Own Book," a manual for handicraftsmen and amateurs; "Sanitary Protection," a course of lectures delivered in the Theatre of the Royal Dublin Society, 1884, by W. Kaye Parry, M.A.

PHYLLOXERA is making steady progress in the Rhenish vineyards it seems. The pernicious insect has now been found on the right bank of the river, in the vineyards of Castle Ockenfels near Linz, where over 100 acres are infected. State aid has been asked for at Berlin, as the occurrence of the pest near Linz is far more serious than that in the Ahr Valley.

A TUNNEL, measuring about 5000 feet in length, and constructed at least nine centuries before the Christian era, has just been discovered by the Governor of the island of Samos. Herodotus mentions this tunnel, which served for providing the old seaport with drinking water. It is completely preserved, and contains water tubes of about 25 centimetres in diameter, each one provided with a lateral aperture for cleansing purposes. The tunnel is not quite straight, but bent in the middle; this is hardly to be wondered at, as the ancient engineers hardly possessed measuring instruments of such precision as those constructed nowadays.

THE *Globus* reports the discovery of the ruins of an ancient city near Samarkand. They are situated upon a hill, which was doubtless a fortress formerly. Remains of utensils and human bones have also been found. According to Arabian sources the large city of Aphrosiab existed there in the time of Moses; it was the royal residence, and the king's castle stood on the hill, and was provided with subterranean corridors. The result of the excavations show that the ruins are indeed those of a very ancient city. The various depths, however, differ widely; in the lower ones fine glass objects are found, which are quite absent from the upper ones; the lowest layers contain remains of a very primitive nature, *i.e.* coarse implements of clay and flint. The excavations are being continued. News from Turkestan announces the discovery of another ancient city, Achsy, on the right bank of the Amu Darya. Remains of brick walls and other buildings are said to be visible in considerable numbers.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♂) from India, presented by Mr. A. F. M. Smith; a Brown Capuchin (*Cebus fatuellus*) from Guiana, presented by Mr. G. S. Malet Barrow; a White-backed Piping Crow (*Gymnorhina leuconota*) from Australia, presented by Mr. F. Larkworthy; two Loggerhead Turtles (*Thalassochelys caouana*) from the Mediterranean, presented by Mr. Allan McGregor; a Common Chameleon (*Chameleon vulgaris*) from North Africa, presented by Mr. A. R. Rogers; a Horned Lizard (*Phrynosoma cornutum*) from Texas, presented by Capt. H. Mends; a Brown Mud Frog (*Pelobates fuscus*), European, presented by Mr. Claude Russell; a Sulphur-breasted Toucan (*Ramphastos carinatus*) from Mexico, a Macaque Monkey (*Macacus cynomolgus* ♂) from India, a Robben Island Snake (*Coronella phocarum*) from South Africa, deposited; a Collared Fruit Bat (*Cynonycteris collaris*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE APPROACHING APPEARANCE OF ENCKE'S COMET.—It may be hoped that, as at the last return of this comet in 1881, an accurate ephemeris for its reappearance, which is now at hand, may be issued from the Imperial Observatory at Pulkowa. According to the mean motion, assigned by the calculations of Dr. Backlund at perihelion passage in 1881, the comet would be again in perihelion (perturbations neglected) about March 7·5 G.M.T., 1885, so that as the effect of planetary attraction will be small during the actual revolution, the comet's track in the heavens will not greatly differ from that it followed in 1852, when the perihelion passage occurred on March 14. It was first observed in that year by Dr. Vogel, at Mr. Bishop's Observatory in the Regent's Park on the evening of January 9, a refractor of 7 inches aperture being employed; at this time its distance from the sun was 1·35, and that from the earth 1·55, so that the intensity of light expressed in the usual manner was 0·23. At the return in 1875, when the perihelion passage took place on April 13, the comet was detected at the Observatory of Marseilles by M. Stephan, on the evening of January 27, distant from the sun 1·50, and from the earth 1·98, the theoretical intensity of light being therefore 0·113, or only half that at the comet's discovery in 1852. There appears to be a probability that with the large instruments now so comparatively common in observatories, the comet may be observed at a greater distance from the sun than in that year, and possibly during the absence of moonlight in November. If we assume March 7·5 for the date of perihelion passage, and bring up the longitudes in Dr. Backlund's orbit of 1881 to the beginning of 1885, we shall have the following positions of the comet at Greenwich midnight:—

1884	R.A. h. m.	N.P.D. ° ′	Distance from Earth	Distance from Sun	Intensity of Light
Nov. 5	23 2·1	82 24	1·310	2·043	0·140
9	22 57·4	83 4	1·318	2·000	0·144
13	22 53·4	83 40	1·328	1·957	0·148
17	22 50·1	84 13	1·340	1·913	0·152
21	22 47·4	84 42	1·352	1·867	0·157
25	22 45·4	85 7	1·365	1·821	0·162

Encke did not continue the ephemeris in 1852 beyond the date of perihelion passage, but if we calculate from his elements for April 19·5 (the day of the new moon), we find the comet's place to have been in R.A. 342° 55′, N.P.D. 109° 22′, its distance from the sun 0·89, and from the earth 0·95, or the intensity of light 1·41; it would rise at the Cape about 14h. 1m., and must therefore have been readily observable. We may expect that in 1885 observations will be made in the southern hemisphere after perihelion passage.

VARIABLE STARS.—Mira Ceti is now close upon a minimum, a phase of which there are not too many observations: its magnitude is usually about 8·5 on Bessel's scale. χ Cygni will probably be at minimum about November 15: the mean of the last five periods, according to the observations of the late Prof. Julius Schmidt, is 408·2 days, and 1880 May 31·2 may be taken as a mean maximum epoch. A maximum of the fiery-looking variable R Leonis may be expected about December 10. R Leporis will probably be at minimum at the beginning of January.

SOUTHERN BINARIES.—There are two southern double-stars which appear to deserve much closer observation than they have yet received on the score of their probable binary character and rapid motion. They are:—

	R.A. h. m. s.	N.P.D. ° ′
h. 5014	17 59 22	133 24·2
h. 5114	19 18 32	144 33·2

The positions are brought up to 1885·0 from the Paramatta Catalogue.

ON THE SEAT OF THE ELECTROMOTIVE FORCES IN THE VOLTAIC CELL

AT the Montreal meeting of the British Association a discussion on the above subject was opened by Prof. O. J. Lodge. Copies of the following notes were distributed to the members present by the opener of the discussion, together with the accompanying letter. As it has been suggested that their reproduction here would be of service, we willingly give them a place.

University College, Liverpool, July 29th, 1884

The following set of statements are privately issued by me solely with the object of securing attention to definite points in the discussion on Contact Electricity, at Montreal, which I have been instructed by the Sub-Committee to open. They are numbered for convenience of reference. I have no authority whatever for appending the names I have appended to some of the statements; and in general the whole thing is merely a statement of my own personal belief. At the same time the wording is carefully chosen and is intended to be correct in detail, and the views indicated I have held with greater or less clearness for some seven years. I should have small hesitation in believing these views to be true, were it not that I fear they are at variance with those of Sir Wm. Thomson. It is in no spirit of presumption, but simply in order more easily and distinctly to elicit the truth, that I have ventured thus to record them, and I am very willing to modify all or any of them on ground shown. It may be hoped that the discussion at Montreal will result in a substantial basis of agreement with regard to this elementary and long-debated matter. OLIVER J. LODGE

I.—ORTHODOX STATEMENTS BELIEVED BY O. J. L. TO BE TRUE IN THE FORM HERE SET DOWN

A.—Volta

1. Two metals in contact ordinarily acquire opposite charges; for instance, clean zinc receives a positive charge by contact with copper, of such a magnitude as would be otherwise produced under the same circumstances by an E.M.F. of ·8 volt.

2. This apparent contact E.M.F. or "Volta force" is independent of all other *metallic* contacts wheresoever arranged, hence the metals can be arranged in a numerical series such that the "contact force" of any two is equal to the difference of the numbers attached to them, whether the contact be direct or through intermediate metals. But whether this series changes when the atmosphere, or medium surrounding the metal, changes is an open question: on the one side are experiments of De la Rive, Brown, Schultze-Berge; on the other side, of Pfaff, Pellat, von Zahn. It certainly changes when the free metallic surfaces are oxidised or otherwise dirty. And in general

this "Volta force" is very dependent on all non-metallic contacts.

3. In a closed chain of any substances, the resultant E.M.F. is the algebraic sum of the Volta forces measured electrostatically in air for every junction in the chain; neglecting magnetic or impressed E.M.F. [Verified most completely by Ayrton and Perry.]

B.—Thomson

4. The E.M.F. in any closed circuit is equal to the energy conferred on unit electricity as it flows round it.

[Neglect magnetic or impressed E.M.F. in what follows.]

5. At the junction of two metals any energy conferred on, or withdrawn from, the current, must be in the form of heat. At the junction of any substance with an electrolyte, energy may be conveyed to or from the current at the expense of chemical action as well as of heat.

6. In a circuit of uniform temperature, if metallic, the sum of the E.M.F. is zero by the second law of thermodynamics; if partly electrolytic, the sum of the E.M.F. is equal to the sum of the energies of chemical action going on per unit current per second.

7. In any closed conducting circuit the total intrinsic E.M F. is equal to the sum of the chemical actions going on per unit electricity conveyed ($\Sigma . J\theta e$), diminished by the energy expended in algebraically generating reversible heat.

8. The locality of any E.M.F. may be detected, and its amount measured, by observing the reversible heat or other form of energy there produced or absorbed per unit current per second. [This is held by Maxwell, but possibly not by Thomson, though its establishment is due to him.]

II.—Statements believed by O. J. L. to be false though orthodox

9. Two metals in air or water or dilute acid, but not in contact, are practically at the same potential. [Sir Wm. Thomson, Clifton, Pellat.]

10. Two metals in contact are at seriously different potentials (*i.e.* differences of potential greater than such milli-volts as are concerned in thermo-electricity). [This is held by nearly everybody.]

11. The contact force between a metal and a dielectric, or between a metal and an electrolyte, is small. [Ayrton and Perry, Clifton, Pellat, and probably Sir Wm. Thomson].

III.—Statements believed by O. J. L. to be true though not orthodox

12. A substance immersed in any medium tending to act upon it chemically will (unless it is actually attacked) be at a different potential to the medium in contact with it, positive if the active element in the medium is electro-positive, negative if the active element is electro-negative.

13. The above difference of potential can be calculated approximately from the potential energy of combination between the substance and the medium, the energy being measured by compelling the combination to occur and observing the heat produced per amount of substance corresponding to one unit of electricity.

14. In addition to this contact force, due to potential chemical action or chemical strain, there is another which is independent of chemical properties, but which seems to be greatest for badly-conducting solids, and which is in every case superposed upon the former contact force, the two being observed together and called the Volta effect. Very little is known about this latter force except in the case of metals; and in these it varies with temperature, and is small. In the case of non-metals it is often much larger than the chemical contact force.

15. The total contact force at any junction can be experimentally determined by measuring the reversible energy developed or absorbed there per unit quantity of electricity conveyed across the junction. [Practical difficulties, caused by irreversible disturbances, being supposed overcome.]

16. In a chain of any substances whatever, the resultant E.M.F. between any two points is equal to the sum of the true contact forces acting across every section of the chain between the given points (neglecting magnetic or impressed forces).

17. In a closed chain the sum of the "Volta forces," measured electrostatically in any (the same) medium, is equal to the sum of the true contact forces, whether each individual Volta force be equal to each individual true force or not.

18. Wherever a current flows across a seat of E.M.F., there it must gain or lose energy at a rate numerically equal to the E.M.F. multiplied by the strength of the current.

Development of the above and Special Application to Metals.

19. A metal is not at the potential of the air touching it, but is always slightly below that potential by an amount roughly proportional to its heat of combustion, and calculable, at any rate approximately, from it. For instance, clean zinc is probably about 1·8 volts below the air, copper about ·8 volts below, and so on. If an ordinary oxidising medium be substituted for "air" in the above statement it makes but little difference.

20. Two metals put into contact reduce each other instantly to practically the same potential, and consequently the most oxidisable one receives from the other a positive charge, the effect of which can be observed electrostatically.

21. There is a slight true contact force at the junction of two metals which prevents their reduction to *exactly* the same potential, but the outstanding difference is small, and varies with temperature. It can be measured thermo-electrically by the Peltier effect, but in no other known way. It is probably entirely independent of surrounding media, metallic or otherwise.

22. If two metals are in contact the potential of the medium surrounding them is no longer uniform: if a dielectric it is in a state of strain, if an electrolyte it conveys a current.

23. In the former case the major part of the total difference of potential is related closely to the difference of the potential energies of combination, and is approximately calculable therefrom. In the latter case the total E.M.F. is calculable accurately from the energy of the chemical process going on, minus or plus the energies concerned in reversible heat effects.

24. There are two distinct and independent kinds of series in which the metals (and possibly all solids) can be placed; one kind depends on the dielectric or electrolytic medium in which the bodies are immersed; the other kind depends on temperature. The one is something like the Volta series, but it is really the Volta series minus the Peltier; the other is the Peltier. To reckon up the total E.M.F. of a circuit we may take differences of numbers from each series and add them together.

IV.—Brief Summary of the Argument

25. Wherever a current gains or loses energy, *there* must be a seat of E.M.F.; and conversely, wherever there is a seat of E.M.F., a current must lose or gain energy (that is, must generate or destroy some other form of energy, chemical, thermal, or other) in passing it.

26. A current gains no energy (*i.e.* destroys no heat) in crossing from copper to zinc, hence there is no appreciable E.M.F. there.

27. When a current flows from zinc to acid, the energy of the combination which occurs is by no means accounted for by the heat there generated, and the balance is gained by the current; hence at a zinc-acid junction there must be a considerable E.M.F. (say at a maximum 2·3 volts).

28. A piece of zinc immersed in acid is therefore at a lower potential than the acid, though how much lower it is impossible to say, because no actual chemical action occurs. [If chemical action does occur, it is due to impurities, or at any rate to local currents, and it is of the nature of a disturbance.]

29. A piece of zinc, half in air and half in water or acid, causes no great difference of potential between the air and the water (Thomson, Clifton, Ayrton and Perry, &c.), consequently air must behave much like water.

30. If the air were slightly positive to the water, as it is (Hankel), it might mean that the potential energy of combination of air with zinc is slightly greater than that of water, or it might represent a difference in the thermo-electric contact forces between zinc and air, and zinc and water, or it might depend on a contact force between air and water. [If such a contact force between air and water exists, it is obviously of great importance in the theory of atmospheric electricity, for the slow sinking of mist globules through the air would render them electrical.]

31. Condenser methods of investigating contact force no more avoid the necessity for unknown contacts than do straightforward electrometer or galvanometer methods; the circuit is completed by air in the one case and by metal in the other, and the E.M.F. of an air contact is more hopelessly unknown than that of a metal contact.

32. All electrostatic determinations of contact force are really determinations of the sum of at least three such forces, none of which are knowable separately by this means.

33. The only direct way of investigating contact force is by the Peltier effect or its analogues. [Maxwell.]

34. Zinc and copper in contact are oppositely charged, but are not at very different potentials: they were at different potentials before contact, but the contact has nearly equalised them.

[Certain portions of these statements which may appear wildly hypothetical, such as 13, are to be justified by figures. The justification is not complete, for lead and iron are untractable, but it does not affect the main position.]

THE AMERICAN ASSOCIATION

We are indebted to the courtesy of the Editor of *Science* for the following reports of the Sectional proceedings of the American Association.

In the Section of Physics a paper was read on "The Relation of the Yard to the Metre," by Prof. William A. Rogers, who has given his life to perfecting the construction and the testing of standards of length, and the result of this his latest investigation is that the metre is 39·37027 inches in length. One of the most important physical measurements is that of the wave-length of light of any given degree of refrangibility, and this determination is best made by means of the diffraction grating. On account of the extensive use of the magnificent gratings constructed by Prof. Rowland for this purpose, Prof. Rogers instituted an investigation to determine the coefficient of expansion of the speculum-metal used in the construction of these gratings. He also noted that from its homogeneity, fineness of grain, and non-liability to tarnish, this speculum-metal is peculiarly suitable for constructing fine scales, though its extreme brittleness is an objection to its use for large scales.

Prof. Rowland stated that he proposed to construct scales on his ruling-engine which would enable the physicist at any time, by purely optical means, and without knowing the coefficient of expansion of the metal or its temperature, to obtain the value of the length of the scale in terms of the wave-length of any given ray of light. These scales were simply to be straight pieces of speculum-metal ruled with lines just as an ordinary grating, except that the length of the lines is to be only about one centimetre, every one-hundreth line being somewhat longer than its neighbours: the whole ruled slip is to be one decimetre in length. From the manner of ruling, it will be easy to count the whole number of lines in the length of the strip, and then by a simple use of the scale as a grating, in a suitable spectrometer, the whole length may be immediately found at any time in terms of any specified wave-length of light. In some forms of telephones and in the microphone the action depends on the change in resistance of a small carbon button on being subjected to pressure. There has been much discussion as to whether this diminution of the resistance with pressure is due to a change in the resistance of the carbon itself, or simply to the better contact made between the carbon and the metallic conductor when the pressure is applied.

Prof. Mendenhall has carried out some experiments to determine the question; and one of his methods of experimenting—that with the hard carbons—appears to point conclusively in favour of the theory that the resistance of the carbon itself is altered by pressure. The experiments made by him on soft carbon are open to criticism, though they also point to the change taking place *in* the carbon. Prof. Mendenhall finds that the resistance is not simply proportional to the pressure, and thinks that by increasing the pressure a point of maximum conductivity would be reached where there would be no change in resistance for a small change in pressure.

Prof. A. Graham Bell, the inventor of the telephone, read a paper giving a possible method of communication between ships at sea. The simple experiment that illustrates the method which he proposed is as follows:—Take a basin of water, introduce into it, at two widely-separated points, the two terminals of a battery circuit which contains an interruptor, making and breaking the circuit very rapidly. Now at two other points touch the water with the terminals of a circuit containing a telephone. A sound will be heard, except when the two telephone terminals touch the water at points where the potential is the same. In this way the equipotential lines can easily be picked out. Now, to apply this to the case of a ship at sea: Suppose one ship to be provided with a dynamo-machine generating a powerful current, and let one terminal enter the water at the prow of the ship, and the other be carefully insulated, except at its end, and be trailed behind the ship, making connection with the sea at a considerable distance from the vessel; and suppose the current be rapidly made and broken by an interruptor; then the observer on a second vessel provided with similar terminal conductors to the first, but having a telephone instead of a dynamo, will be able to detect the presence of the other vessel even at a considerable distance; and by suitable modifications the direction of the other vessel may be found. This conception Prof. Bell has actually tried on the Potomac River with two small boats, and found that at a mile and a quarter, the farthest distance experimented upon, the sound due to the action of the interruptor in one boat was distinctly audible in the other. The experiment did not succeed quite so well in salt water.

Prof. Trowbridge then mentioned a method which he had suggested some years ago for telegraphing across the ocean without a cable; the method having been suggested more for its interest than with any idea of its ever being put in practice. A conductor is supposed to be laid from Labrador to Patagonia, ending in the ocean at those points, and passing through New York, where a dynamo-machine is supposed to be included in the circuit. In Europe a line is to extend from the north of Scotland to the south of Spain, making connection with the ocean at those points; and in this circuit is to be included a sensitive galvanometer. Then any change in the current in the American line would produce a corresponding change in current in the European line; and thus signals could be transmitted.

Mr. W. H. Preece then gave an account of how such a system had actually been put into practice in telegraphing between the Isle of Wight and Southampton during a suspension in the action of the regular cable communication. The instruments used were a telephone in one circuit, and in the other about twenty-five Leclanché cells and an interruptor. The sound could then be heard distinctly; and so communication was kept up until the cable was again in working order. Of the two lines used in this case, one extended from the sea at the end of the island near Hurst Castle, through the length of the island, and entered the sea again at Ryde; while the line on the mainland ran from Hurst Castle, where it was connected with the sea, through Southampton to Portsmouth, where it again entered the sea. The distance between the two terminals at Hurst Castle was about one mile, while that between the terminals at Portsmouth and Ryde amounted to six miles.

A few years ago Mr. E. H. Hall, then a student at the Johns Hopkins University, taking a thin strip of gold-leaf through which a current of electricity was passing, and joining the two terminals of a very sensitive galvanometer to two points in the gold-leaf, one on one edge, and the other on the other, choosing the points so exactly opposite that there was no current through the galvanometer, found that on placing the poles of a powerful electro-magnet, one above and the other below the strip of gold-leaf, he obtained a current through the galvanometer, thus indicating that there was a change in the electric potential, due to the action of the magnet. Mr. Hall explains this change by supposing the rotation of the equipotential lines in the conductor about the lines of magnetic force. This explanation has been brought into question by Mr. Shelford Bidwell, who attempts to explain the action thus: the magnetic force acting on the conductor carrying the current would cause the conductor to be moved sideways, were it free to move; but, since it is held by clamps at the ends, the magnetic force acting upon it brings it into a state of strain, one edge being compressed and the other stretched; and Mr. Bidwell supposes the whole Hall effect to be due to thermal actions taking place in consequence of this unsymmetrical state of strain. Prof. Hall, who is now at Harvard, has made some careful experiments to test this explanation of Mr. Bidwell. He used not only gold-leaf, but strips of steel, tinfoil, and other metals, and clamped them sometimes at both ends, sometimes in the middle, and sometimes only at one end; and in all cases the action was the same, with the same metal, irrespective of the manner of clamping. This was strong evidence against Mr. Bidwell's position.

Sir William Thomson suggested, as a further test to bring about the state of strain, which Mr. Bidwell supposes to be the primary cause of the action, by purely mechanical means, bringing pressure to bear on one side or the other, and seeing whether the action obtained is at all commensurate with the action found by Mr. Hall.

Prof. Hall then discussed an experiment by which Mr. Bidwell had obtained a reversal of the effect, and showed that the reversal was only apparent, and that when carefully examined the results of Mr. Bidwell's experiments were best satisfied by the theory of the rotation of the equipotential surfaces about the lines of magnetic force.

Sir William Thomson spoke of the discovery of Mr. Hall as being the most important made since the time of Faraday. He favoured Mr. Hall's explanation, though he considers Mr. Bidwell's suggestion as very important, and thinks that it will very likely be found that both the Hall effect and thermal effects have a common cause, rather than that one is to be taken to explain the other. He showed also that the mathematical examination of the subject indicates three relations to be investigated,—first, the relation of thermal force to the surfaces of equal rate of variation of temperature; second, the relation of electric current to the equipotential surfaces; third, the relation of the thermal flow to isothermal surfaces. The second of these is that investigated by Mr. Hall, who has found that under the conditions mentioned the lines of flow are *not* perpendicular to the equipotential surfaces. There remains, therefore, "work for two more Halls," in either proving or disproving the existence of the analogous actions in these other two cases. Sir William Thomson also suggested the following exceedingly interesting mechanical illustration or analogue of Hall's effect. Let us be living upon a table which rotates uniformly for ever. A narrow circular canal is upon this table, concentric with the axis of rotation of the table, and nearly full of water. After a while the water will acquire the same velocity of rotation as the table, and will come to a state of equilibrium. The outer edge of the water in the canal will then stand a little higher than the inner edge. Let us now apply a little *motive* force to the water, and by means of a pump cause it to flow in the canal in the same direction in which the table is already rotating: it is evident that it will stand higher on the outer edge, and lower on the inner edge of the canal, than before. But, should we cause it to flow in the opposite direction to the motion of the table, it will stand lower on the outer edge, and higher on the inner edge, than in its position of equilibrium. The experiment made by Mr. Shelford Bidwell may also be illustrated by putting a partition in the canal so as to divide it into two circular concentric troughs, and make a little opening in the partition at some point; then taking two points near the opening in the partition, one in one trough and one in the other, if they are very close to the partition, the point in the outer trough will be at a *lower* level than that in the inner one; but if they are not close to the partition, but one is taken close to the outer edge of the outer trough and the other close to the inner edge of the inner trough, then the point in the outer trough will be at a *higher* level than that in the inner trough, though the difference in level will be only about half of what it would have been had there been no partition separating the canal into two troughs.

Prof. Forbes called attention to the fact that the classification of the metals according to their thermo-electric qualities gives not only exactly the same division into positive and negative, but that the very *order* obtained in that way corresponds to that obtained by classifying according to the Hall effect, except *possibly* in the case of aluminium.

In the Section of Mathematics and Astronomy the first paper read was by Prof. E. C. Pickering, upon the colours of the stars. The need of exact photometric measurement of different parts of their spectra was first pointed out, and the author then described a very ingenious method of accomplishing this. In the telescope tube, a little beyond the focal plane, is a direct-vision prism, so set as to give a spectrum extended in declination; and on the preceding side of this prism is placed a piece of plane glass, whose edges are so ground that, when a small portion of the following side of the cone of rays falls upon it, it gives a small white ghost, just preceding the spectrum and always opposite the same wave-length. In the focal plane is one of Prof. Pritchard's neutral-tint wedge photometers, and behind it a thin metal diaphragm with four long, narrow slits parallel to the equatorial motion; so that, when the spectrum transits behind them, four little stars—a red, yellow, blue, and a violet—shine through these slits, and the time of the disappearance of each, as they move towards the thicker edge of the wedge, measures its brightness. From these times may be deduced the magnitude and colour curve of the star. To fix the same wave-lengths for each observation, the little white ghost is adjusted upon one of two parallel wires which project out beyond the preceding side of the diaphragm. For a succeeding transit, the ghost is adjusted upon the other wire, half a slit-interval distant, and thus eight points of the spectrum are photometrically measured.

Prof. Young of Princeton spoke very highly of the ingenuity and effectiveness of the device, especially for the systematic measurement of a large number of stars. He pointed out, however, what might be a source of error, viz. the different sensitiveness of different observers' eyes to different colours, so that they would probably observe the times of disappearance of the four coloured stars slightly relatively different.

The next paper, by Prof. Daniel Kirkwood, discussed the question whether the so-called "temporary stars" may be variables of long period, referring to the sometimes-claimed identity of the temporary stars 945 and 1264, with the well-known Tycho Brahe's star, which blazed forth in Cassiopeia in 1572, and whose position is pretty closely known from his measures. The conclusion reached was, that on account of the sudden apparition of the temporary stars, the short duration of their brightness, and the extraordinary length of their supposed periods, they should be considered as distinct from variables.

Prof. Mansfield Merriman, the author of the well-known treatise on "Least Squares," proposed a criterion for the rejection of doubtful observations, founded upon Wagen's demonstration of the law of frequency of error, which was simpler than Pierce's or Chauvenet's. It involves, however, a determination of what is the unit of increment between errors of different sizes, a thing difficult to determine in very many cases.

Prof. Pickering then read another paper upon systematic errors in stellar magnitudes, showing, without any question, that the magnitudes of all the star-catalogues, from that of Ptolemy down to the great work of Argelander in the *Durchmusterung*—all depending upon eye-estimates—are systematically affected by being in, or close to, the Milky Way; they all being estimated too faint, and the error amounting to about half a magnitude in the Milky Way itself. This arises from the brightness of the background upon which the star is viewed. In the Harvard photometry measures, this source of error is avoided, since, in the comparison of each star with the Pole-Star, the two fields are superposed, and their added brightness affects both stars alike.

Prof. M. W. Harrington, Director of the Ann Arbor Observatory, read a paper upon the asteroid ring. He showed that the representative average orbit would be an ellipse of small eccentricity, with semi major axis equal to about 2·7 times that of the earth, and inclined to the plane of the ecliptic about 1°; and that, in the progressive discovery of these small bodies, the average mean distance had gradually increased, but now seemed to have reached its limit. On the assumption that the surfaces of all the asteroids have the same reflecting power as Vesta, Prof. Harrington reaches the conclusion that the volume of Vesta is about 5/17 that of all these 230 bodies put together, and that Vesta and Ceres together form almost one-half the total volume.

Prof. Rogers, of the Harvard College Observatory, read two papers. The first, upon the magnitude of the errors which may be introduced in the reduction of an observed system of stellar co-ordinates to an assumed normal system by graphic methods, showed a great amount of laborious research, and was a good illustration of the vast amount of monotonous work necessary in the present stage of astronomical observation in order to reach the highest degree of accuracy attainable by the search for and elimination of minute systematic errors. His next paper was upon the original graduation of the Harvard College meridian circle *in situ*. This described a method of turning a meridian circle through any desired constant arc up to about 30° without any dependence upon the circle and reading microscopes, effected by means of an arm swinging between fixed stops, and clamping to a circular ring on the axis by an electro-magnetic clamp. With this Prof. Rogers claimed to be able to set off a constant arc through as many as five thousand successive movements of the clamping arm. The ingenious method suggested and carried out by Mr. George B. Clark, of the firm of Alvan Clark and Sons, of grinding the clamping circle to a perfect circular form while the telescope was swung round in its Y's was fully described, and also Prof. Rogers' method of arresting the momentum of the telescope at the stops by water-buffer plungers. The great advantage of thus being able to set off a constant arc independent of the circle and microscopes was pointed out, with especial reference to the investigation of division errors and flexure of circle, and also to the division of the circle itself *in situ ; i.e.* mounted on its axis and turning on its pivots.

Prof. Young called attention to the necessity of guarding against expansion and contraction of the bar holding the stops, due to radiation from the observer's body.

Mr. S. C. Chandler, Jun., of the Harvard College Observatory, gave the results of observations and experiments with an "almucantar" of four inches' aperture, a new instrument devised by Mr. Chandler, which seems to be of remarkable accuracy, and promises to furnish an entirely new and independent method of attacking some of the most important problems in exact observational astronomy. The instrument consists of a telescope and vertical setting-circle, which can be clamped at any zenith-distance, and is supported on a rectangular base which floats in a rectangular trough of mercury, the whole turning round a vertical axis so as to observe in any azimuth ; these observations being simply the times of transit of any heavenly body over a system of horizontal wires in the field. The observations thus far have been entirely upon stars, and all at the apparent zenith-distance of the pole. After some very small periodic variations in the zenith-distance pointing had been traced to changes of temperature, and had been removed by sawing through the wooden bottom of the mercury trough, the instrument showed an astonishing constancy in this zenith-distance pointing, extending over weeks at a time, and far exceeding the constancy of the corrections to the best fundamental instruments of our observatories.

A paper was read by Mr. Chandler, upon the colours of variable stars. Showing, first, that most of the variables were *red*, he described some fairly satisfactory methods which he had used to measure the *degree* of redness of all the periodic variables ; and then, plotting a series of points whose abscissæ represented the *length* of the periods, and ordinates the *degree of redness*, their agreement with a curve making a very decided angle with the axis of abscissæ brought out without question the remarkable law that *the redder the star the longer is its period of variability*. In discussing any theory of variable stars, Mr. Chandler pointed out that Zöllner was the only one who had thus far taken into account two laws already known, viz. (1) that they are generally *red ;* (2) that they *increase* in brightness *much more rapidly* than they *decrease ;* and now, in any further theory, this new third law must have a place, viz. that the *redder* they are the *longer* is their period.

Dr. R. S. Ball, Astronomer-Royal for Ireland, read a paper upon the ruled cubic surface known as the cylindroid, whose equation is

$$z(x^2 + y^2) - 2mxy = 0.$$

Mr. W. S. Auchencloss of Philadelphia exhibited a balancing machine for finding the centre of gravity of any number of different weights distributed along a line, which seemed to be of excellent construction, extremely easy and rapid in manipulation, and quite sensitive. In connection with a time-scale of 365 days at one side, it was shown how rapidly a complicated system of business accounts could be settled, and how it could be applied to various engineering problems.

Prof. J. H. Gore, of the U.S. Geological Survey, read a paper upon the geodetic work of the U.S. Coast and Geodetic Survey.

The next paper was by Mr. J. N. Stockwell of Cleveland, upon an analysis of the formula for the moon's latitude as affected by the figure of the earth. In this Mr. Stockwell claimed that Laplace's formula for expressing this was wrong ; the question turning upon an approximate integration of a differential equation, which he claimed to show was wrong by separating into two terms a single one which expressed the difference of two effects, which, thus evaluated separately, became either indeterminate or of an impossible amount.

Prof. J. C. Adams of Cambridge, England, made some comments upon Mr. Stockwell's paper, speaking in high terms of the general work which Mr. Stockwell had done in the difficult subject of the lunar theory ; but from such conclusions and methods as those brought forward in this particular case he said he must express his total dissent. He then pointed out that this equation was, to begin with, only an approximation ; that, before it could be treated at all as a rigorous one, many other small terms must be included ; that, further, its integration was only an approximation ; and that in this case any separation into terms, which, on a certain approximate assumption, became either indeterminate or very large, was of no value as a test of the equation.

Prof. Ormond Stone, Director of the Leander McCormick Observatory of the University of Virginia, gave an elaborate description of that Observatory, now approaching completion, and to be devoted entirely to original research. The telescope, which will soon be mounted, is the twin in size of the Washington 26-inch, and like it in most of its details, except that the driving clock is like that of the Princeton 23-inch, with an auxiliary control by an outside clock, and that it has Burnham's micrometer illumination. The Observatory has a permanent fund of 76,000 dollars as a beginning, and 18,000 dollars have been expended in Observatory buildings, and 8000 dollars for the house of the Director. Situated 850 feet above the sea, and on a hill 300 feet above surroundings, the main building, circular in shape, is surmounted by a hemispherical dome 45 feet in diameter. The brick walls have a hollow air-space, with inward ventilation at bottom and outward at top.

Mr. Warner, the builder of the dome, gave an interesting description of the ingenious method of adjusting the conical surfaces of the bearing-wheels, so that they would, without guidance, follow the exact circumference of the tracks ; and then of the adjustment of the guide-wheels, so that the axis of this cone should be exactly normal to the circular track. The framework of the dome consists of thirty-six light steel girders, the two central parallel ones allowing an opening six feet wide. The covering is of galvanised iron, each piece fitted *in situ*, and the strength of the frame is designed to stand a wind-pressure of a hundred pounds per square foot. There are three equal openings with independent shutters, the first extending to the horizon, the second beyond the zenith, and the third so far that its centre is opposite the division between the first and second. The shutters are in double-halves, opening on horizontal tracks, and connected by endless chain with compulsory parallel motion of the ends. The dome weighs twelve tons and a half, and the livering one ton and a half, and a tangential pressure of about forty pounds, or eight pounds on the endless rope, suffices to start it. If this ease of motion continues as the dome grows old, it is certainly a remarkable piece of engineering work.

In the discussion following, Prof. Hough thought he should prefer the old style of single opening extending beyond the zenith.

Prof. Stone could not agree with him, the greater extent of opening making it less probable that the dome would have to be moved so far in turning from star to star, and at the same time furnishing better ventilation, and the opportunity for cross-bracing adding strength to the dome. He stated that he should first take up the remeasurement of all the double stars of less than 2″ distance between 0° and − 30°.

The Rev. Father Perry, the Director of the Observatory at Stonyhurst, England, gave the result of late researches on the solar surface, with special reference to evanescent spots.

Mr. Lewis Swift, Director of the Warner Observatory at Rochester, N.Y., read a paper upon the nebulæ, in which he described his method of search for new nebulæ. One very interesting statement of Mr. Swift, to the effect that there had not been a first-rate clear sky since the red glows appeared a year ago following the Krakatoa explosions, bears out the general experience of workers in other observatories, especially those who try to see stars near the sun in the daytime.

An interesting discussion arose as to the much-disputed existence of the nebula round the star Merope in the Pleiades ; the general drift of it being that the nebula no doubt existed, but in order to see it a clear sky was necessary, and a very low power and large field, so that the nebula might be contrasted with darker portions of the same field ; that a large telescope was not necessary, in fact the smaller the better, provided the optical qualities were relatively as good. Mr. Swift said he could always see it under favourable conditions ; and Mr. E. E. Barnard, of Nashville, Tenn., the discoverer of the latest comet, said that before he knew of its existence at all, he picked it up as a supposed comet.

Prof. Adams of Cambridge, England, read a paper upon the general expression for the value of the obliquity of the ecliptic at any given time, taking into account terms of the second order. The difficulties of obtaining a formula for this quantity, on account of the many varying elements upon which it depends, were clearly explained by a diagram, and the results given of an approximation carried much further than ever attempted heretofore.

Prof. Harkness, in paying a high compliment to the celebrated mathematician and astronomer for these laborious and valuable researches, also expressed a wish that some of the *n*-dimensional-space mathematicians would follow the example of Prof. Adams, and apply some of their superfluous energy to the unsolved problems in the solar system, which have some direct practical bearing.

Prof. Newcomb, in remarking upon the mass of the moon used in this problem, expressed the opinion that this could be obtained most accurately by observations of the sun, in determining the angular value of the radius of the small circle described by the earth about the common centre of gravity of earth and moon, since this, in his opinion, seemed to be the only constant which could be determined by observation absolutely free from systematic errors, and hence was capable of an indefinite degree of accuracy by accumulated observations; and he asked Prof. Adams's opinion on this point.

The latter replied that he thought the quantity too small for certain accurate determination, almost beyond what could be actually seen by the eye in the instruments used.

Prof. Newcomb admitted, in the case of absolute determinations, the general impossibility of attempting to measure what cannot be seen, but, in the case of differential or relative determinations in which there was no supposed possibility of constant or systematic errors, he advanced the theory, which he had some thought of elaborating more fully at some time, that such determinations might be carried by accumulated observations to a sure degree of accuracy far beyond what can be seen or measured by the eye absolutely.

Prof. Adams hoped he would more fully elaborate and publish this idea, since there was in it an element well worth careful consideration.

Prof. Harkness doubted the sufficient accuracy of meridian observations of the sun, on account of the distortions produced by letting the sun shine full into the instrument; and spoke of the difficulties in the transit of Venus observations from this cause.

Prof. Newcomb replied that he would have to show that this would be periodic with reference to the moon's quarters in order to affect this constant systematically.

Prof. Adams then presented another note upon Newton's theory of atmospheric refraction, and on his method of finding the motion of the moon's apogee.

In the Section of Chemistry papers were read by C. F. Mabery of Cleveland, O., on chloropropiolic acid and some derivatives of acrylic and propionic acids; and by C. W. Dabney on anhydrobenzamidosalicylic acid.

Dr. Springer of Cincinnati exhibited some improved torsion scales and balances. One of these had been used by Prof. F. W. Clarke, and its action was spoken of as being very satisfactory.

Messrs. L. M. Norton and C. F. Presscott read a paper on continuous etherification; and Prof. Monroe detailed results of analyses of an efflorescence on the bricks of some new buildings. It was chiefly sodium sulphate.

Mr. Clifford Richardson, in a paper on the chemistry of roller-milling of wheat, stated that the dark colour of northwestern hard winter wheat could be overcome by using steel rollers run at different speeds.

Prof. Atwater has examined the nutritive value of different fish. He finds flounder to be the least value, and salmon the highest, and in invertebrates the oyster takes the lowest place as a food-stuff.

A lengthened discussion took place on the subject of valence in chemistry. Prof. Clarke remarked that it was especially useful in organic chemistry in explaining isomerism and in synthesis. It was also used in mineralogy; and he mentioned as examples of isomerism the three minerals kyanite, andalusite, and fibrolite, giving the structural formula for each. He then took up the questions of variable valence, invariable valence, and maximum valence as points that might be discussed. He remarked, further, that valence was an attempt to explain the arrangement of the atoms in a molecule, and spoke of the drawback of being obliged to represent them on a plane surface, space of three dimensions being much nearer the true state of affairs.

Prof. B. Silliman remarked that the last statement of Prof. Clarke was the key to the whole difficulty about valence. A plane surface is insufficient to explain the facts. He testified to the great utility of valence, and spoke of the chaotic condition of organic chemistry before this question of valence was appreciated. It was a working hypothesis, a scaffold without a building, but not the building. Hypothesis is not always the truth.

Prof. W. Ramsay said that the difficulties about valence could be traced to Lavoisier, who worked upon stable compounds, as oxides, chlorides. He also thought that a study of the heat of formation of many compounds would be a key to the valence of the elements; and said that the difficulties of conceiving of the motions of the atoms was well illustrated in Sir William Thomson's effort to explain them in complicated vortex evolutions.

Mr. A. H. Allen called attention to the failure of chemists to recognise the value of the work of John Newlands, in the periodic classification of the elements usually ascribed to Mendeléeff.

Prof. Greene remarked that it was best to consider the cause of valence.

Prof. Ira Remsen testified to the utility of valence. He remarked that there were two ways of teaching: one by giving all the principal theories first, and the other giving the facts and then the theories—which latter he considered the best method. He had come to the conclusion that valence should never be mentioned until all the important properties of a compound are known. In regard to its value to young students, he thought its use was dangerous until they fully understood its meaning. He believed that the value of valence had been magnified, and that it was better to study the reactions of compounds, and the methods for their synthesis, and the manner of breaking up.

Mr. A. H. Allen said that many formulæ that showed the structure of compounds according to the valence of the elements do not give any idea of the true constitution of these compounds as ascertained from a knowledge of their properties. He gave, as examples of his meaning, potassic dichromate and fuming sulphuric acid.

Prof. Dewar, of Cambridge, England, maintained that the graphical method and structural formulæ were most useful, but they are often presented in a way that shows an incomplete knowledge of the ideas of the person who devised the formula. He remarked that the text-books contained too many pictures of graphical formulæ, and that he considered it better to follow the historical method for developing theory.

Prof. Atwater thought that some idea of valence should be given at the beginning, as it assisted the student's memory.

Prof. W. Ramsay said that he was satisfied of the utility of making the student perform experiments that brought out facts to illustrate the theory of valence, so that he could thus understand its meaning from his own work.

Prof. Caldwell said that he could not get along with students in chemical analysis who had not obtained some idea of the theory of valence.

Prof. Remsen thought that the theory of valence might be some good as an assistance to the memory; but such assistance was of doubtful value, and too empirical.

Prof. J. W. Langley, Vice-President, said that valence, or chemism, may be a force emanating from the atom, or it may be a force outside the atom; it is static, or dynamic, and a knowledge of it was more a physical than a chemical problem. From the educational view he thought it better to use the theory of valence in connection with the history of the theories concerning atoms and molecules. As a further step, the language and figures of magnetism might be used.

Prof. Stewart described a process of making leather by treating hides with sulphurous acid under pressure.

Prof. Atwater has grown pease in washed sea sand, and found them to gain from 35 to 50 per cent. more nitrogen than they contained originally, and refers this increase to nitrogen directly absorbed from the air.

Dr. Springer, in a paper on fermentation, showed that a ferment exists in the stems of tobacco plants which decomposes nitrates and forms butyric acid from sugar solutions.

Prof. Dewar spoke of the density of solid carbonic acid, which he finds to be 1·58 to 1·60.

Other short communications were made by Prof. Munroe on deliquescence; on human milk, by Prof. Leeds; on gas analysis, by Dr. Elliott; and on fish oils, by Mr. Allen.

The most interesting discussion was on educational methods in laboratories and chemical lectures, by Prof. Remsen, who remarked that in Germany the student does not go into the laboratory until he understands reactions, while in England and the United States he is placed there at the beginning of the course. Prof. Remsen follows an order of instruction in which the student becomes first acquainted with apparatus and methods of manipulation. He next makes gases, and repeats lecture experiments. He then experiments on oxidation and reduction. Next follows the quantitative analysis of air. Then come alkalimetry and acidimetry, with success. This practical work and the lectures occur simultaneously, and by the time the lecturer has reached the metallic elements, the students are ready to take up test-tube reactions with profit. During the first year the student should only just begin analysis. After the general properties of the metals are known, let the student devise methods of separation. The course of instruction in our colleges Prof. Remsen regards as too short and superficial. Lecture-experiments should never be made for show. Æsthetics and chemistry are entirely distinct.

Prof. Atwater said that chemistry is taught now, as a rule, after the student has acquired the methods of the classics and has never been taught to observe facts. Chemists must show that their science will give what is called "liberal culture," or it will not find a place in our educational institutions. Present methods are not doing this, as they fail to make the student think for himself.

In the Section of Geology and Geography no paper was greeted with more interest or closer attention than that by Prof. J. E. Hilgard, Director of the U.S. Coast Survey, on the relative level of the Gulf of Mexico and the Atlantic Ocean, with remarks on the Gulf Stream and deep-sea temperatures. Its two most essential points are:—(1) The discovery by a most careful series of levels, run from Sandy Hook and the mouth of the Mississippi River to St. Louis, that the Atlantic Ocean at the former point is 40 inches lower than the Gulf of Mexico at the latter point; and (2) that ocean-water at all depths exceeding 1000 fathoms possesses a temperature of nearly 35° F., because this is the temperature consistent with its greatest density. Should the water become either cooler or warmer, it must expand; which it cannot do on account of the superincumbent pressure.

Prof Henry S. Williams, of Cornell University, in a paper on the influence of geographical and physical conditions in modifying fossil faunas, introduced the exceedingly important subject of the extent to which palæontological evidence is to be regarded as an absolute guide in correlating strata in different regions. Prof. Williams explained a series of sections, principally in Chemung and Catskill rocks, taken from a number of localities across New York State, and adduced from them abundance of proof that faunas in Devonian times, as at present, changed not only geologically in sequence of time, but also geographically according to the areas of their distribution. The influences which brought about a change in the character of the sediment deposited also manifested themselves in altering the forms of the organisms inhabiting these sediments.

Prof. Alexis Julien of New York communicated the results of a very extended study of the *Eozoon canadense* from nearly all the localities where it has thus far been found, adding other localities of his own discovery. The result of his investigations led him to decide in favour of the inorganic nature of the so-called fossil, although his ideas in regard to the mode of its formation differ considerably from any heretofore advanced. He noticed as universal in all localities, that the calcium and magnesium carbonates were very unequally distributed in the Eozoonal limestones, and that there was a large development of pyroxene where the dolomite was least abundant. He moreover observed the constant tendency on the part of pyroxene to be arranged in layers alternating with either calcite or apatite, as well as abundant evidence that pyroxene passed by hydration into serpentine, a process which could be seen in every stage at any of the localities visited. From these data it was assumed that siliceous waters, permeating limestones originally evenly dolomitic, would cause the local development of pyroxene by the change of the magnesium carbonate into the corresponding silicate. Were it the case, as so often occurs, that this pyroxene was developed in layers, its subsequent alteration to serpentine or loganite would readily account for all the appearances exhibited by the Eozoon, without the necessity of appealing to organic agencies.

A large number of papers (forty-three in all) were presented before the Section of Biology; but we regret that in our limited space we can give merely the briefest outlines. The first we may mention was a paper by Mr. D. C. Beyer, on the influence of oxygenated and unoxygenated blood, as well as of blood in various degrees of dilution, on the heart of the frog and terrapin. The paper aimed to prove that it is not concentrated mammalian blood which produces the greatest amount of work either in the heart of the frog or that of the terrapin, but that a certain degree of dilution is necessary.

Dr. C. S. Minot read a paper on biological problems. The author opposed the trinomial system, and considered the present mode of determining species entirely unscientific, and thought that the species should be based on a statistical study of all the variations that are known to occur. Individuals are not always homologous. The only fixed units are (1) cells; (2) the whole series of generations of cells from a single ovum—a cell-cycle. An individual may be almost any fractional part of a cell-cycle. Roughly speaking, the higher the organism the fewer the number of individuals it comprises. The author considered the ovum to be homologous with the encysted protozoon, the radial zone being equivalent to the capsule or cyst of the protozoon, and the contents also homologous.

Prof. E. D. Cope in a paper on the phyllogeny of the Artiodactyle mammals derived from American fossils, considered the derivation of the selenodont dentition from the bunodont as established from a mechanical point of view. The oldest American Artiodactyl (Pantolestes) is bunodont. The modification proceeded as in other ruminants on the lines of the co-ossification of the bones of the legs and feet. The peculiar structure of the carpus in the Oreodontidæ shows them to be, without doubt, the ancestors of the Tragulina. The following table represents the present views of the author on the subject:—

Tritubercular bunodontia (Pantolestidæ).

Selenodontia. Quadritubercular bunodontia.

Hyopotamidæ. Oreodontidæ. Poëbrotheriidæ.

(?) Pecora. Tragulina. Tylopoda.

Mr. H. F. Osborne presented observations on the amphibian brain, containing results of microscopic study upon the frog, Menobranchus, Menopoma, and Amphiuma. His method of study was by making series of sections, in their different planes. The relative position of gray and white matter was the same as that found in the spinal cord of these and other vertebrates. The courses of the principal nerve-fibres in their course from the medulla forward to the hemispheres was described, showing the course of the transverse commissures, and pointing out a commissure in the roof of the third ventricle hitherto overlooked. This demonstrated that each brain-segment had its own dorsal commissure. The differences of the cerebellum in the Anura and Urodela were pointed out, and the resemblances of the latter to the mammalian brain were dwelt upon. The pia blood-vessels are all sent in upon the anterior face of the pituitary body. The pineal elements were shown to consist of certain very inconspicuous foldings of the epithelium of the roof of the third ventricle, which have been generally overlooked. These foldings represent what remains of the stalk of the pineal gland.

Mr. S. Garman's paper on Chlamydoselachus, the frilled shark, treated of the internal anatomy of this peculiar shark. The nearest forms are Notanidæ, Hexanchus, and Heptanchus. Hind and fore brain resemble that of fœtal sharks; the cartilage is soft: the lateral line is open as in fœtal sharks, and continued to the end of the tail. The pelvis is twice as long as broad: the nearest resemblance to this is seen in the fœtal Heptanchus. According to the author, the Chlamydoselachus may be a sub order of the Galei.

The next paper was by Prof. E. D. Cope, on the mammalian affinities of Saurians of the Permian epoch, and referred to the detection of mammalian resemblances between Thesomorphus and reptiles of the Permian epoch. Resemblances in the pelvic and scapular arch were pointed out. The quadrate bone was discussed, referring to the theory of Albrecht. The genus

Clepsydrops shows that it has the mammalian number of bones in its tarsus, and the resemblance was nearest to that found in *Platypus anatinus*.

Dr. C. H. Merriam read a paper on the hood of the hooded seal (*Cystophora cristata*), describing it as an inflatable proboscis overhanging the mouth, and extending posteriorly to a point behind the two eyes, lined with nasal mucous membrane, and divided longitudinally by two cartilages. It is not noticeable until the male has reached its fourth year.

In a paper on some points in the development of pelagic teleostean eggs, Mr. G. Brook, Jun., first considered non-pelagic eggs, instancing those of trout, in which the hypoblast originates as an involution of the lower layer upon itself, the space between the layers being quite distinct. In pelagic eggs the process is quite different. Sections of the eggs of *Trachinus vipara* at this stage show that the parablast of Klein, the intermediate layer of American authors, is made up of a large number of free cells, and nuclei are absorbed from the yolk, which contribute to a very great extent to build up the hypoblast. In this case there is no true invagination. In *Motella mustela* the origin of the hypoblast is similar to that of Trachinus; but the resulting cells, instead of being quite similar to the original ones, as usual in teleostean eggs, are very much larger, and hexagonal, so that they cannot be derived directly from the lower layer of cells. The author sustained the views of Ryder as regards the segmentation cavity in pelagic eggs. He also holds that there is no circulation in pelagic embryos before hatching.

Mr. G. Macloskie, in a paper on the dynamics of the insect's test, commenced with a general description of the chitinous skeleton with its in- and out-growths, &c. The tracheæ have spinal crenulations, which have been hitherto misunderstood and supposed to be threads; these tracheæ transmit gases directly to the tissues, and the blood is not used for this purpose. The tracheæ are not directly controlled by muscles, their action depending on the successive production of a partial vacuum, and condensation of air around them.

Prof. A. Hyatt read a paper on the larval theory of the origin of tissue, stating that the building-up of the tissues of the Metazoa is due to a quick and rapid division of cells. Minot's theory that the origin of the sexes is due to the difference in cell elements was supported. The author considered the Planula a more primitive form than the Gastrula. In another paper Prof. Hyatt presented objections to some commonly-accepted views of heredity, asserting that heredity has no need of the gemmule hypothesis or pangenesis, but that it can be equally well understood upon the supposition that the nuclei of cells are the immediate agents of the transmission of characteristics. The author presented the case of a man in Maine, who resembled his mother on one side of his body and his father on the other side, as an illustration of his theory, and he contested the position of Prof. Brooks as regards heredity. In a paper on the structure and affinities of Beatricea, the same author stated that this fossil has had many positions assigned to it in almost all the groups of the Invertebrata, though he himself now thought it a Foraminifer. Thin sections were examined, the structure being found to consist of cells joined by a stolon.

Dr. C. S. Minot presented a paper on the skin of insects. The skin consists of three layers—externally the cuticula, overlying an epithelium, which lies in turn on a sheet of connective tissue; the epithelium is homologous with the epithelium of other animals, and should be so called instead of hypodermis; and dermis, which should be applied to the connective tissue, as it is the homologue of that of vertebrates. The cuticle of caterpillars has not yet been fully described: it consists of two layers, a thick one and a thin one.

In a communication on the development of Limulus, Mr. J. S. Kingsley stated that the account begins after the formation of the blastoderm. At this time there is a single layer of cells surrounding the yolk, in which are scattered nuclei. The mesoblast arises as a single sheet on the ventral surface. Its cells come largely from the blastoderm, but some arise from the yolk nuclei. The mesoblast soon forms two longitudinal layers, one on each side in the neighbourhood of the limbs. The cœlom is formed by a splitting of the mesoblast, and at first consists of a series of metameric cavities extending into the limbs. The supra-œsophageal ganglion arises by an invagination of the epiblast. The heart arises as two tubes in the somatophore, which later unite. The mesenteron does not appear until after hatching. The amnion of Packard is the first larval cuticle, and bears no resemblance to the amnion of the tracheata. A second cuticle is formed and moulted before hatching. The eyes appear on the dorsal surface at the same time that the limbs appear on the ventral. In these characters Limulus agrees essentially with the Tracheata, and has nothing in common with Crustacea.

Prof. B. G. Wilder, in a paper entitled, "Do the cerebellum and oblongata represent two encephalic segments, or only one?" remarked that most writers had considered two segments to exist. The cephalad of these segments is held to include the cerebellum, together with the portion of the "brain-stem" immediately connected herewith and the latter part of the oblongata. The only writers that have admitted of a single segment caudal of the mesen are Balfour, A. M. Marshall, Owen, and Spitzka. The views of Spitzka were then discussed, concluding with the opinion that sufficient evidence to settle either side was insufficient, and that the question was still open.

Dr. J. A. Ryder presented a paper on the morphology and evolution of the tail of osseous fishes. The caudal fin of fishes is developed in the same way as the median or unpaired fins, from a median fin-fold. After the protocercal stage of the larva is passed, a lower caudal lobe grows out, which is probably the homologue of a second anal fin. The hypotheses which grow out of a consideration of the facts of the development of the tails of fishes are the following:—(1) Whenever heterocercality manifests itself, there is a more or less extensive degeneration of the caudal end of the chordal axis, which began to be somewhat manifest far back in the phylum in such forms as Holocephali, Dipnoi, and Chondropterygians. (2) With the outgrowth of the lower lobes (second anal) the energy of growth tended to push the tip of the chorda upward; the lobe itself arising, probably in consequence of the localisation of the energy of growth and the deposit of organic material at the point, according to the demands of use and effort. (3) Local use and effort, acting as constant stimuli of local growth, carried the heterocercal condition and its accompanying modification of degeneration and reduction still further, as is shown by a study of the homologous elements in the tails of fishes; while use and effort would also continue to augment heterocercality, until the inferior and superior lobes were of about the same length and area, when the morphological characters of the caudal fin would become approximately stable for any one species, as may be shown by measurements of a simple mechanical illustration, in which the interaction and composition of the faces which are brought into action are demonstrated. (4) The mechanical demonstration alluded to above, taken together with the fact that the primitive or ancestral form of the tail, which is typified by a temporary condition in fish larvæ, when the myocomata are rudimentary, but still symmetrical, amounts almost to a demonstration of the principles first laid down by Lamarck, then elaborated by Spencer, and more recently applied to special cases by the author and Prof. Cope.

In a communication on growth and death, Dr. C. S. Minot gave the results of 10,000 measurements of weight of growing guinea-pigs and other animals from birth to maturity. The rate of growth was found to steadily diminish from birth onward; so that the loss of power begins at once, and continues until death. The common views of death were discussed, and the current conceptions of animal individuality were attacked. The author then referred to the bearing of our present knowledge of senescence upon the theory of life, and the relation of life to a material substratum.

A paper on the osteology of Oveodon was read by Mr. W. B, Scott, in which this genus was said to belong to the Artiodactyla, although there are some strong resemblances to the Suidæ. The vertebræ are ruminant, markedly in the case of the axis. The thoracic vertebræ have long prominent spines, and small bodies slightly amphicœlus. The lumbars, probably five in number, are heavy, with short spines and broad flat transverse processes. The sacrum contains two vertebræ which touch the ileum. The tail is long and slender, and the legs proportionally long. There are a short head and short metapodials, giving the animal a wolf-like appearance. The radius and ulna are distinct. The carpus consists of eight bones, including the pisciform. There are short unanchylosed metacarpals. The ungual phalanges are long and pointed, as in Hyopotamus. A rudimentary pollex is present, this being the only Artiodactyl with one.

Mr. J. Struthers, in a paper on finger muscles in *Megaptera longimana*, and in other whales, records rudimentary flexor and extensor muscles in these animals, and shows that they are more or less used, as the muscular fibres are red and not degenerated.

Dr. G. M. Stemberg described his experimental research relating to the etiology of tuberculosis. The author repeated the inoculation experiments of Koch, with similar results. The experiments of Formad to induce tuberculosis in rabbits by introducing into the cavity of the abdomen finely powdered inorganic material, have also been repeated with entirely negative results. The author held that Koch's bacillus was an essential factor in the etiology of tuberculosis.

Dr. C. E. Bessey, in a paper on the adventitious inflorescence of *Cuscuta glomerata*, stated that the examination of young plants shows that the inflorescence is developed from numerous crowded adventitious buds, and not by the repeated branching of axillary flowering branches as commonly stated.

In a paper on the hitherto unknown mode of oviposition in the Carabidæ, Prof. C. V. Riley records habits of *Chlænius impunctifrons*, traced from the eggs up. The eggs are laid singly, in cells made of mud or clay, on the under surface of leaves.

Mrs. A. B. Blackwell read a paper on the comparative longevity of the sexes. The study was exhaustive, and made on statistics from all parts of the world; and the greater longevity of woman over man was established. In old countries the females preponderate, while males lead in newly settled ones. Up to eighteen years the males are in excess of the females: later the females predominate in numbers.

THE PRIME MERIDIAN CONFERENCE

THE Prime Meridian Conference at Washington on Monday adopted the Greenwich line as the universal prime meridian. Only one vote—that of St. Domingo—was given against its adoption; but the representatives of France and Brazil declined to vote.

The following details of the session are from the *Times* Correspondent:—

To the American resolution for adopting the Greenwich line, Mr. Fleming (Canada) moved an amendment to the effect that the Conference should adopt the 180th degree of longitude east from Greenwich as the prime meridian; but the other British delegates opposing the proposition it was lost. Señor Valera, the Spanish Minister, said that he had been instructed by his Government, in voting for the meridian of Greenwich, to say that it hoped the metric system of weights and measures would be adopted by England, the United States, and the other nationalities there represented, as recommended by the Conference at Rome. Gen. Strachey (Great Britain) said that he was authorised to state that his country had asked to be allowed to join the Metrical Convention, and that the metric system was already recognised by the laws of Great Britain, and was in use for scientific purposes. He could not, however, say that it would be adopted in any circumstances as a popular system of weights and measures throughout England. M. Lefaivre (France) said the Greenwich was not a scientific meridian, and that it implied no progress in any science, but was merely a commercial standard. Since, therefore, nothing would be gained to science by adopting Greenwich, France could not make a sacrifice of her own meridian, and incur the vast expense consequent upon the adoption of a new one, because she would thereby gain no advantage whatever. Sir William Thomson, who was present as a guest, by the invitation of the Conference, spoke in favour of the choice of Greenwich. He said that it was purely a matter of convenience, and that Greenwich answered the world's convenience better than any other standard meridian. Sir Frederick Evans (Great Britain) presented a statement showing that the shipping tonnage controlled by the Greenwich standard of longitude was about 14,000,000 tons, and that controlled by the Paris one only 1,735,000 tons. From the statement of chart sales, &c., to nations outside England, he showed how largely the Greenwich measure was used.

The resolution recommending the choice of Greenwich was then adopted, the ayes being 21, and there being but one nay—San Domingo. France and Brazil abstained from voting.

Mr. Rutherford (United States) moved that from the Greenwich meridian the longitude be counted in two directions, up to 180°, the east longitude being "plus," and the west "minus." The Russian Minister advocated this proposal, but Count Lowenhaupt (Sweden) moved the adoption of the fourth resolution of the Roman Conference for counting longitude continuously through the whole 360°. Herr von Alvensleben (Germany) said that this was a matter of detail, and therefore he should not vote upon it. The British delegates agreed with the German Minister that this was a matter of detail, and held that it would not make any difference which method was adopted. Señor Juan Pastorian (Spain) stated that he favoured the plan of counting longitude westward continuously round the world.

The discussion was here adjourned.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The beginning of Michaelmas Term shows that the University and Colleges have not been idle in erecting new buildings for the accommodation of students. The new buildings of Magdalen—to be called the Waynflete Buildings—are ready for occupation, and will be used this term. No one can deny that the most beautiful of Oxford Colleges has added a new ornament to the city in the Waynflete Buildings. The new buildings of Trinity College are rapidly approaching completion. Stretching back from quaint old Kettle Hall in Broad Street, they extend to near the beginning of the Lime Walk in the College Garden. The open space in front of the College—known as Trinity Green—will now be bounded on the east by these new buildings. The Green will thus become one of the largest "quads" in Oxford. On the north side of the University Museum the new Physiological Laboratory is rising. Its situation is one of the pleasantest in Oxford. That Prof. Burdon-Sanderson is attracting pupils to physiology is a patent fact in Oxford, and one that will be received outside that city with the strongest feeling of satisfaction.

Since last Term we have to deplore the loss of Mark Pattison, Rector of Lincoln College. Mr. Merry, Public Orator in the University, and Fellow and Tutor of the College, has been elected his successor.

The following scheme of lectures and classes has been agreed on by the Board of the Natural Science Faculty:—

In the Department of Physics Prof. Clifton will lecture on the Galvanometer and Methods of Measuring Electric Currents, and on Thermo-Electricity. Prof. Price will lecture on Optics, Physical and Geometrical. Prof. Clifton and Mr. Walker give instruction in Practical Physics in the Clarendon Laboratory. Mr. Walker will give a course on Questions incidental to the Practical Study of Mechanics and Heat. Mr. Baynes will give a course of lectures at Christ Church on Thermodynamics, and form a class for practical instruction in Magnetic and Electrical Measurements. Mr. Dixon will give a course of experimental lectures, on Elementary Heat and Light, at Balliol College. Prof. Pritchard will lecture on Spherical Astronomy, and form a class for practical work in the University Observatory.

In the Department of Chemistry Prof. Odling will give a course of lectures on 3-carbon and 4-carbon compounds. Mr. Fisher will lecture on Inorganic, and Dr. Watts on Organic, Chemistry. At Christ Church Mr. Vernon Harcourt will form a class for Volumetric Analysis. Practical instruction in Chemistry is given daily in the Museum Laboratory, and in the Chemical Laboratories at Christ Church and Balliol College. Prof. Gilbert will complete his course on the Constitution of Plants, and will then lecture on the Effects of Manures, Exhaustion and Variations of Season on the Amounts of Produce and on the Composition of Wheat.

In the Morphological Department Prof. Moseley will begin his course of Comparative Anatomy; each lecture will be followed by special demonstrations on the subject of the lecture. Dr. Hickson gives a course on the Morphology of the Vertebrata, each lecture to be followed by special demonstrations. Mr. Barclay Thompson lectures on the Anatomy of the Mammalia; Mr. Jackson on the Fundamental Principles of Comparative Embryology; Mr. Poulton on the Distribution of Animals; Mr. Morgan on Odontography and on Human Osteology. Prof. Westwood will lecture on the Insect Skeleton.

In the Department of Physiology Prof. Burdon-Sanderson will lecture on the Physiology of Circulation and Respiration. Practical instruction will be given in the Laboratory by the Professor, Dr. Dixie, and Dr. Gotch.

Prof. Bayley Balfour will give a course of lectures in the Botanic Garden, on the General Morphology of Plants.

Prof. Prestwich will lecture at the Museum on the Principles and Elements of Geology. Dr. Tylor will lecture on the Intellectual Development of Mankind.

During the course of the present Michaelmas Term, Scholarships will be awarded in Natural Science at the following Colleges:—At Balliol College, without limitation of age; at

Christ Church, limited to persons under nineteen; at Magdalen College, limited to persons under nineteen years on July 2, 1885.

Mr. J. N. Dobie has been elected to a Natural Science Scholarship at Exeter College in Biology and Chemistry.

CAMBRIDGE.—Mr. G. F. Harmer, B.A., of King's College, has been appointed Demonstrator of Comparative Anatomy. Mr. Harmer was placed in the first class of the Natural Sciences Tripos in 1883, being distinguished in Zoology and Comparative Anatomy.

An examination for Entrance and Foundation Scholarships will be held at Gonville and Caius College, beginning on January 9 next. Six Entrance Scholarships, varying in value from 40*l.* to 80*l.*, may be awarded. The successful candidates must come into residence in October 1885. They may be awarded for Classics, Mathematics, or Natural Science, separately or combined. They are tenable for a year, when the holder will be eligible for a Foundation Scholarship. The scholarships may be increased or diminished each year, according to the scholar's performances in the College or University Examinations. Those who distinguish themselves in the Triposes may have their scholarships prolonged after their degree. A successful candidate, who after the examination enters for competition at another College, forfeits *ipso facto* any scholarship for which he may have been recommended. Candidates must be under nineteen years of age on the first day of examination. Undergraduates of the College may at the same examination compete for scholarships, in which case there is no restriction of age. Candidates in Natural Science will be examined in Physics, Chemistry, Biology, and Animal Physiology, and will be expected to show proficiency in at least two of these subjects, of which chemistry must be one. The Rev. A. W. W. Steel, Senior Tutor, will furnish fuller particulars.

Lectures and Demonstrations on Physics and Chemistry in Michaelmas Term: Prof. Stokes, short course on Double Refraction and Polarisation; Mr. Atkinson, Heat and Hydrostatics; Mr. Glazebrook, Electricity (el.), also Advanced Physics; Mr. Shaw, Physics, Elementary and Advanced; Mr. Hart, Mechanics and Heat (1st M.B.); Electricity (Nat. Sci. Tripos, Part 1); Practical Physics, Demonstrations and Practical Work at Cavendish Laboratory.

Chemistry: Prof. Liveing, General Course; Prof. Dewar, Dissociation and Thermal Chemistry; Mr. Main, Elementary Organic Chemistry; Mr. Pattison Muir, General Principles, and Elementary Chemistry, especially Metals; Mr. Heycock, General Principles (non-metals).

Demonstrations in Spectroscopic Analysis, Prof. Liveing; Special Demonstrations for Medical Students, Mr. Sell and Mr. Fenton; also Special Demonstrations for Nat. Sci. Tripos, Part 1; Mr. Robinson, Demonstrations in Analysis of Food and Water; Practical Work at the University, St. John's, Caius, and Sidney College Laboratories.

Prof. Lewis, Mineralogy and Crystallography; Demonstrations on Minerals, by Mr. Solby.

Prof. Stuart, Mechanism and Applied Mechanics; Mr. Lyon, Rigid Dynamics. The Mechanical Workshops are open from 8 to 1 and 2 to 6 daily.

Geology: Prof. Hughes, Economic Geology and Geological Surveying; Tides, Mr. E. Hill; Stratigraphy, Dr. R. D. Roberts; Palæontology, Echinoidea, Mr. T. Roberts; Constituents of Rocks, Mr. A. Harker. Field Lectures will be specially announced.

Botany, Elementary, with Practical Work, Dr. Vines; Physiology of Plants, Advanced, Dr. Vines.

Zoology and Comparative Anatomy: Prof. Newton, Evolution in the Animal Kingdom; Elementary Morphology (Invertebrata, Mr. Sedgwick; Advanced Invertebrata, Mr. Harmer; Mammalia, Living and Extinct, Osteology, Strickland Curator (Dr. Gadow).

Physiology, Elementary Course, with Practical Work, Prof. Foster; Chemical Physiology, Advanced, Mr. Lea; Advanced Course of Physiology and Histology, Mr. Langley; Preparation for 2nd M.B., Mr. Hill.

Human Anatomy, Muscular System, Prof. Macalister; also Demonstrations of Visceral Anatomy; General Pathology, Prof. Roy; also Practical Class in Morbid Histology.

Advanced Mathematical Lectures: Prof. Stokes, Optics; Prof. Cayley, Recent Developments in Analysis and Geometry; Prof. Darwin, Theoretical Astronomy; Mr. Forsyth, Higher Algebra, Binary Forms; Mr. Hobson, Higher Dynamics; Mr. Glazebrook, Higher Geometrical Optics; Mr. J. J. Thomson, Electrostatics; Mr. Macaulay, Thermodynamics; Dr. Besant, Theory of Equations, Differential and Integral Calculus; Mr. Lock, Fourier's Series, and Vibrations of Strings and Bars; Mr. Stearn, Hydrodynamics; Mr. Temperley, Laplace's Functions; Mr. Webb, Theory of Attractions and Elasticity, if a sufficient number apply.

THE formal inauguration of the University College of North Wales, Bangor, has been fixed for the 18th inst., and it is expected that the Duke of Westminster, the Earl of Powis, Lord Aberdare, Lord Penrhyn, Mr. Mundella, and other gentlemen, will take part in the proceedings. The fitting up of the new laboratories and lecture theatres for the chemical and physical departments is being rapidly pushed forward, and the buildings include a very complete suite of rooms for the use of each department. An excellent equipment of scientific apparatus has been secured for all branches of chemistry and physics. Mr. George Macgowan, F.R.S.E., formerly of Prof. Fresenius's Laboratory, Wiesbaden, and now of Prof. Kolbe's Laboratory, Leipzig, has been appointed Demonstrator in Chemistry under Prof. Dobbie, and Mr. D. M. Lewis, M.A., Trinity College, Cambridge, Demonstrator in Physics under Prof. Gray.

SOCIETIES AND ACADEMIES

SYDNEY

Linnean Society of New South Wales, July 30.—Dr. James C. Cox, F.L.S., Vice-President, in the chair.—The following papers were read:—Revision of the Lamellibranchiata of New Zealand, by Capt. F. W. Hutton, F.G.S. This is a carefully revised list of all the Lamellibranchiate mollusks of New Zealand, with the corrected synonyms and localities of each species. A list is also given of the names of each genus which had been wrongly included by previous authorities in the New Zealand fauna.—List of some New South Wales Zoophytes identified by Dr. Kirchenpauer, by Baron Sir F. von Mueller, K.C.M.G., F.R.S., &c. The list contains the exact localities of six species of Hydroida and fifteen of Bryozoa collected by Miss Bate on the south-east coast, and Miss Hodgkinson at the Richmond River. They were all detached from sea-weeds, and identified by Dr. Kirchenpauer, Burgomeister of Hamburg.—New Fishes in the Queensland Museum, part iii., by Charles W. De Vis, M.A. Mr. De Vis in this paper goes through the families *Berycidæ*, *Sciænidæ*, *Carangidæ*, *Scombridæ*, *Trachinidæ*, and *Triglidæ*, describing in all twenty-three new species, mostly from the coasts of Northern Queensland.—Census of Australian snakes, with descriptions of two new species, by William Macleay, F.L.S. The two new species are named *Dipsas boydii* and *Diemenia atra*, both from the Herbert River District, Queensland. The census gives the names, references, and localities of 108 species of snakes, thirty-five of these being innocuous, and seventy-three venomous. The paper concludes with some remarks on the immunity from snake-bite enjoyed by Australia, as compared with India.—On a new species of kangaroo (*Dorcopsis chalmersii*) from the south-east end of New Guinea, by N. de Miklouho-Maclay. A young kangaroo obtained by N. de Miklouho-Maclay in New Guinea, in 1880, has proved to be (on account of the great size of the præmolars, the general shape of the skull, and the direction of the hair on the neck) a new species of Dorcopsis, which he describes as *Dorcopsis chalmersii*, Mcl. The specific name, *Chalmersii*, is given in honour of the well-known and distinguished missionary of the south coast of New Guinea. The paper contains a full description of the animal and its dentition.—On a complete debouchement of the sulcus Rolando into the fissura Sylvii in some brains of Australian aboriginals, by N. de Miklouho-Maclay. A complete junction of the sulcus Rolando with the fissura Sylvii, which is very rare in brains of our race (a single case only having been described by Prof. Turner), has been found by the author in two out of four brains of Australian aboriginals. The junctions of the sulcus Rolando with other sulci are, according to Dr. Maclay, also not uncommon in brains of men of dark races, and occur more frequently than in the brains of men of the white race.—The Australian Hydromedusæ, part v. (conclusion), by R. von Lendenfeld, Ph.D. In this paper the monograph on the Australian Hydromedusæ is brought to a close. All known Australian species are enumerated, with the necessary references, and thirty new species discovered and described by the author are added. The total number of species is 231. The most interesting of the new species are illustrated. The classificatory system established by the author is used.—Muscular tissue of Hydroid Polyps, by R. von Lendenfeld, Ph.D. A Hyroid Polyp discovered by the author in Port Phillip possesses a singular apparatus for escaping its enemies. This animal was investigated by Dr. R. von Lendenfeld, and a remarkable muscular structure was discovered. The histological structure of this is described, and some general conclusions drawn from the observations on muscular tissue, by O. and R. Hertwig, Claus, and the author.—Notes on the fibres of certain Australian Hircinidæ, by R. von Lendenfeld, Ph.D. The author discusses the origin of the "filaments," and describes some new and interesting peculiarities of the Australian Hircinidæ.—On the Myrtaceæ of Australia, by the Rev. W. Woolls, Ph.D., F.L.S. In this paper the author gives tabular statements of the distribution of this large order throughout the globe, but with special reference to its development in Australia. From an examination of these tables, as well as from other considerations, it is clear that West Australia must be regarded as the metropolis of the essentially Australian flora, the plants of the eastern portion of the continent bearing evident relation to Asiatic and Oceanic forms, while those of the west find their nearest, though still very distant, kindred in the yet more distant continent of South Africa.—On marine Annelids of the order Serpulea: observations on their anatomy, with descriptions of the Australian species, by William A. Haswell, M.A., B.Sc. The points treated of are the pseudohæmal system, the segmental organs, the tubiparous glands, budding and hermaphroditism, and the characteristics of the Australian representatives of the order. The arrangement of the vessels in several of the genera is described. Segmental organs of a simple type are shown to exist in addition to tubiparous glands which had been previously regarded as representing the segmental sacs of other *Polychæta*. Details are given of the structure of the tubiparous glands in a variety of genera.—On a new Crustacea found inhabiting the tubes of *Vermilia*, by William A. Haswell, M.A., B.Sc. In the tube of a Port Jackson *Serpulid* the author found several specimens of a remarkable Isopod, each with a brood of young. It proved to be a form differing in various points from any of the known families, but most nearly related to the Anthuridæ. The young were free in the cavity of the tube, sheltered, however, by fasciculi of hairs fringing the pereion of the parent. Like the "normal" Isopoda, and unlike the Anthuridæ, the embryos are flexed in the egg towards the dorsal side; there is a pair of jointed larval appendages connected with the second larval cuticle.—Note on *Pristiophorus cirratus*, by William A. Haswell, M.A., B.Sc. This remarkable genus of sharks was shown to be viviparous, and to possess a rudimentary shell thrown off in the uterus as in *Mustelus*, *Carcharias*, *Galeus*, and *Sphyrna*.

PARIS

Academy of Sciences, October 29.—M. Rolland, President, in the chair.—Note on the total eclipse of the moon on October 4, by M. Mouchez. Owing to its long duration, this eclipse offered a favourable opportunity for more exactly determining the diameter of the moon through the occultation of numerous small stars near the lunar disk. Although the weather was far from favourable, a sufficient number of observations were taken by MM. Périgaud and Bigourdan to advance the solution of this problem considerably. A large number of photographs were also taken by MM. Paul and Prosper Henry. The duration of the eclipse appeared to be rather less than the period theoretically determined.—Note on the experiments recently made at Turin and Lanzo to distribute the electric light to great distances, by M. Tresca. These experiments, carried out by Messrs. Gaulard and Gibbs in connection with the International Electrical Exhibition now being held at Turin, are stated to have been attended by a large degree of success. A Siemens' dynamo-electric machine of 30 horse-power generated an alternate current, which was simultaneously utilised by the Swan, Siemens, and Bernstein systems distributed over a circuit of twenty-four miles between the Exhibition, Lanzo, and intermediate stations.—Note on the nitrates present in plants at the various periods of their vegetable development, by MM. Berthelot and André.—On the explicit solution of Hamilton's quadratic equation in quaternions or in matrices of the second order, by Prof. Sylvester.—A fresh communication on a method of extracting the colouring matter from straw, by M. E. Cadoret, was referred to the previously appointed Commission; and a memoir by M. Goyet, on a project for the construction of a marine canal between the Atlantic and the Mediterranean was referred to M. de Lesseps for examination.—Results of the observation of the recent lunar eclipse made at the Paris Observatory (equatorial *coudé*), by M. Périgaud.—Note on the same eclipse as observed at the Paris Observatory (equatorial of the West Tower), by M. G. Bigourdan.—Observation of the same eclipse, by M. Trépied.—Observations of Wolf's comet made at the Observatory of Algiers (0·50 m. telescope), by M. Rambaud.—Observations of the new comet, made on September 24, 25, and 26, by M. Perrotin. Under the spectroscope the nucleus yielded a continuous brilliant spectrum crossed by the three usual bands of comets, and by a fourth in the violet, like that observed some months ago in the spectrum of the Pons-Brooks comet.—Observations of the solar spots and faculæ during the third quarter of the present year, by M. Tacchini.—Remarks on the solar halos observed at Rome during the last four months (four illustrations), by M. P. Tacchini. This phenomenon, which has been constantly visible at Rome shortly before sunset since the first appearance of the after-glows in November 1883, is stated to resemble in every respect that already described elsewhere by M. Cornu.—Note on the action of water and of nitric acid on the basic acid of the bioxide of tellurium, by MM. Klein and J. Morel.—Note on the experimental study of infectious osteomyelitis, by M. A. Rodet.—On the elimination of phosphoric acid by the urine in cases of mental disorders and epilepsy, by M. A. Lailler.—Geological observations on the section of the Cordilleras traversing the Isthmus of Panama, by M. Ch. Mano. A careful survey of this region in connection with the works now in progress on the Interoceanic Canal has satisfied the author that the northern continuation of the Andes system following the double curve of the isthmus throughout its entire length, belongs to a far more recent geological epoch than that of the syenites and serpentines of Choco and Antioquia, whence they appear to branch off. They are also later than the diorites, volcanic and other porphyries of the Costa Rica coast range, which belong to the system of the Rocky Mountains, stretching thence northwards to the Polar Sea. The eruptive rocks of the isthmus seem to be contemporary with the volcanic formations of Auvergne, dating from the Quaternary or the dawn of the present epoch, and containing fossil human remains.

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