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THE ALPS FROM END TO END.

The Alps from End to End. By Sir William Martin Conway. With 100 full-page illustrations by A. D. M'Cormick. Pp. xii + 397. (Westminster: Constable and Co., 1895.)

SIR WILLIAM CONWAY has given us in this new work a racy account of a three months' summer journey through the Alps from west to east. To a climber of Himalayan fame it was not much that the "Playground of Europe" had to offer in the way of enterprise or difficulty. Neither was it Sir William Conway's intention to combine scientific aims with his mountaineering feats in the Alps, as he had done in the Himalayas. The purpose he had in view was

"to devise a route . . . so that a climber might begin at one extremity of the snowy range, and walk up and down through its midst to the other extremity over a continuous series of peaks and passes" (p. 3).

The tour began at Turin, on June 1, 1894, and ended on August 26, at Gastein. Seven were of the party—Sir William Conway and Mr. E. A. FitzGerald, accompanied by three well-known "high tour" guides, Carrel, Zurbriggen, Aymonod, and two Gurkhas, Amar Sing and Karbir, who had been with Sir William Conway in his Himalayan journeys.

The first part of the route lay along the French-Italian frontier, traversing the Maritime, Cottian, and Graian Alps. The climbs here were not so successful as they might have been, owing to late snow on the mountains, general bad weather, and the occasional interference of the frontier gendarmerie. The narrative gives many amusing accounts of custom-house colloquies. We read of the "inquisitive Jack-in-office" and "suspicious lieutenants," and we sympathise with the would-be climbers waiting hours at Colle di Tenda for a colonel "who was either asleep or out," and at last sent a "ghostly captain" to assure the party of his distinguished consideration, but to announce that *this fortified circle of the hills was closed to all the world!* The author's keen sense of humour is seen to full advantage in those early chapters.

The first successful climb was that of Pelvo d'Elva, on June 8, followed on June 12 by the ascent of Monte Viso, both in the Cottian Alps. The cold must have been very intense.

"Some cold cream in FitzGerald's pocket was turned into a stony lump. Our knitted gloves were as stiff as boards. Icicles hung in rattling plenty from beards and moustachios . . ." (p. 53).

Unfriendly weather pursued them into the Graian Alps, but on June 22 the Aig. de la Grande Sassière was climbed under favourable skies, and a glorious panorama rewarded them. They could see from the Maritimes in the south to Mont Blanc and the Swiss giants in the north. The photograph of the Sassière peak is a good representation of the broad slabs of slaty rock so characteristic of this region of the western Alps.

Very pleasantly written pages relate the journey from the Graians into the *Valais*, over the Col de St. Grat and down the Ruitor glacier to La Thuille. Sir William Conway writes:

"Assuredly nowhere else is Mont Blanc better seen than from this Ruitor névé. No foreground more admirably serves to set off its blue shadowing buttresses and cream-coloured domes than the flat white area of this magnificent snow-field" (p. 94).

The party was now in full training, and the glaciers and peaks of the Mont Blanc massif were attacked with extraordinary vigour. The reader is hurried through the grand natural gateway of Courmayeur, torn along the lovely Allée Blanche—where he is allowed to hear nothing but Aymonod's chatter, and to see little except two cowherds fighting and a background of glacier—then, after rapid climbing up the Miage glacier, he is given time for reflection in the Dôme Club-hut. The next day, too, when the summit is reached, the reader cannot but be disappointed that Sir William Conway so summarily dismisses the expected description of the view. Any one who has read Mr. Leslie Stephen's "Sunset on Mont Blanc," will feel the wide difference between the essayist who faces the difficulties of Mont Blanc because he is first and foremost a lover of nature, and the author who is steeped in professional climbing, and includes Mont Blanc in a record tour! We are glad that the continuation of the route was made over Col d'Emaney and the Salanfe Alp, for this region deserves to be better known amongst English travellers.

The most effectively written chapters in the book are those which follow, on the "Western Bernese Oberland." There the author strikes many wise notes of experience. For instance:

"All high mountains command fine views, but there are differences between them, both in kind and degree. I hold that on one side at least there should be green and fertile land. If a lake is visible, so much the better. The eye, too, should somewhere plunge into a profound valley. The great ranges should not spread themselves out like a procession, but should be grouped into masses. The Diablerets' view conforms to all those conditions" (p. 138).

Every one knows how the pleasant châlet-life helps to enhance and soften the scenery of the mountain pasturages between Lake Geneva and Lake Thun. Sir Willam Conway treats this part charmingly, and, thanks to the garrulous guide Aymonod, he acquaints the reader with many secrets of Swiss cheese-making.

A much sterner aspect of Swiss experience is presented in the chapter on Monte Rosa. The ascent was undertaken on this occasion by Sir William Conway entirely for the sake of the "reading public." A fearful gale blew on the mountain, and we question if charitable England will agree that the amount of enlightenment gained entirely justified the foolhardiness of the party of climbers and the frost-bitten toes of Amar Sing! The author writes in his enthusiasm:

"Such struggles with nature produce a moral invigoration of enduring value . . . They bring a man in contact with cold, stony reality, and call forth all that is best in his nature. They act as moral tonics. Of all the time I have spent in the mountains, such days as these have possessed upon the whole the most enduring value" (p. 174).

There is an unmistakable Cromwellian spirit about this.

The description of the journey through the eastern Bernese Oberland is inclined to be scrappy. It is here that the words "couloir" and "bergschrund" are explained for the first time in the book, although they have been in constant use by the author in the preceding 200 pages. Such flaws of orderly treatment frequently occur, and remind us that the narrative was originally written for a serial journal. The Alps of Uri and Glarus repeat the more pastoral scenes of the western Oberland, but the inhabitants are less kindly criticised.

The Austrian frontier was reached on August 1. The chief peaks ascended in Austria were the Scesa Plana in the Rhaetikon, the Weisskogl in the Oetzthal Mountains, the Hochpfeiler in the Zillerthal Mountains, the Gross Venediger, the Gross Glockner, the Klein Glockner, and the Ankogl Mountain. During the latter part of the journey, east of the Brenner Pass, Sir William Conway was accompanied only by the two Gurkhas. The departure of the others, especially the loss of Aymonod's talk, was not without its effect on the "Journal." Side-incident distributed itself around fellow-travellers, and the German and Austrian Club-huts received a large share of attention at the author's hands.

Taken as a whole, the "Alps from End to End" is a notable example of the fact that occasional papers published in a "serial" seldom cohere satisfactorily into an independent volume. As a literary production, the book is not likely to add to the author's fame. It is one which will live only in the form of extracts. Fine descriptive passages occur in plenty, but they are loosely padded together. Ideas are fragmentary, and repetitions tiresome. The state of the weather and the digestion of the party play naturally enough a most important part in a faithful journal of Alpine climbing, and if the journal be read by weekly instalments, those items may act pleasantly as a foil to the main interest of climbing. But it will try the patience of most readers to go through the variations of this theme, chapter after chapter, in a one-volume book. Still, the book contains a mass of useful hints and information for ardent Alpine climbers. The author conveniently condenses the details of each part of the route, and gives the time record, at the beginning of each chapter. These small summaries are admirable storehouses of reference for the practical walking tourist.

Every one, tourist or stay-at-home, will feel bound to admire the *sangfroid*, pluck, and determination which carried the author through an extremely difficult programme of climbs in all weathers. But the Alpine devotee will regret the business-like way in which the tour was accomplished, as well as the unsympathetic attitude often shown by the author towards the hard-wrought inhabitants of these high regions. Sir William Conway is at his best when he discusses views, peaks, cloudland, and the elements constituting a good panorama. Nothing could be neater, for example, than the following aphorism: "That panorama is the most perfect which the eye follows round with the greatest luxury of movement" (p. 348).

There is one feature in the book which cannot entirely be passed over. It is the author's touch of impatience with the familiar figure in the Tyrol—the German "Rücksack" tourist who carries his belongings on his back, and is his own guide. The same feeling is extended to many of the well-meant efforts of the German and Austrian Alpine Club for the aid and better enjoyment of such tourists.

"The Tirol is cursed with wire-ropes. Wherever a good scramble was offered by nature, it has been ruined in this fashion by man, with the result that any bumpkin can get conveyed almost anywhere in this mountain area" (p. 275).

Sir William Conway seems to forget what an enormous saving it is for hundreds of the less wealthy tourists to be able to climb mountains and cross cols safely without paying the tax levied by the guides. And that is what the step-cutting and path-making of the German and Austrian Club have made possible in their Alpine territory. On the other hand, an economical tour in the *Valais* is almost impossible. Care seems to be taken by the people there not to put up a single signboard for the guidance of the walking tourist! This certainly leaves nature's beauty unimpaired; but we fear it is the commercial instinct, and not the love of beauty, which rules the Swiss.

Little can be said of the numerous illustrations contributed by Mr. A. D. M'Cormick to Sir William Conway's book. They are drawn from photographs; but, although interesting, they lack the artistic freedom and cleverness which made the charm of Mr. M'Cormick's original sketches for "Climbing in the Himalayas."

THE ARCHEGONIATE SERIES OF CRYPTOGAMS.

The Structure and Development of the Mosses and Ferns. By Douglas Houghton Campbell, Ph.D., Professor of Botany in the Leland Stanford Junior University. Pp. vi + 544. (London: Macmillan and Co., 1895.)

BOTANISTS have been waiting with considerable expectation for the appearance of Prof. Campbell's book on the Archegoniate Series of Cryptogams. Neither the mosses nor the ferns and their allies have been comprehensively treated for some years, and thus, whilst much individual work has been accomplished in this department within recent times, it has become exceedingly difficult for those whose special interests happen to lie in other directions, to keep abreast of the ever-flowing tide of information respecting these most important families of plants.

Thus, had Dr. Campbell merely given us a text-book, in the sense in which that word is too often used—or misused—his claims to the gratitude of the morphologist might have been by no means inconsiderable. But in reality his treatise can scarcely be termed, with propriety, a text-book; and herein lies at once its strong and, we may venture also to add, its weak points. It is the outcome of a very extensive series of researches on the development and structure of the plants denoted as archegoniate cryptogams; and there is a peculiar freshness in the treatment, which can only be attained as the result of personal and intimate knowledge of the objects under discussion. This quality at once raises it far above the level of the ordinary book, in which the character of the compiler is often but too painfully apparent.

A very noticeable feature of the volume is the large space devoted to the consideration of the Liverworts. It is quite surprising how little attention botanists have been accustomed to pay to this group; many are quite content with acquiring a knowledge of *Marchantia*, which, as it happens, is a very specialised form, and not in the least degree typical of Liverworts taken as a whole. This neglect becomes the less intelligible when one considers the extreme diversity of outward form exhibited even within the limits of a single genus, a diversity only perhaps paralleled by some genera of lichens, or of algæ. And again, their position at the base of the archegoniate series should have sufficed to rescue them from a condition of such unmerited oblivion. Thus the author has done good service by devoting 140 pages to the Hepaticæ alone. It is, perhaps, to be regretted that he did not lay more emphasis on the extent of morphological specialisation which may be observed amongst the species of individual genera; thus, for example, all gradations may be traced in forms included in *Symphyogyna*, from a creeping flat thallus to a complicated branch system, recalling that of the sporophyte of many Hymenophyllaceæ. No doubt this would have entailed an increased number of pages, but, we think, the addition would have been very welcome.

We notice one or two slips here and there. Thus it is stated on page 97 that all the acrogynous Jungermanniaceæ possess a three-sided apical cell, whereas at least one species of *Physiotium* is known in which the cell is lenticular in transverse section. But these are but trivial errors, and the general summing-up of the Hepaticæ strikes us as extremely good, even though we may hesitate before accepting all the author's conclusions.

The Vascular Cryptogams are treated very fully, as one would have expected from a writer who has himself contributed so much to our knowledge of the group. But occasionally, if we may venture to say so, the investigator is perhaps a little too much to the front; and whilst individual plants are fully described, and very complete and minute accounts are given of the development of their various organs, one loses, to some extent, the feeling of a wide synthetic grasp of the plants as a whole. It is true, however, that this is atoned for by the suggestive summaries and discussions on phylogeny at the end of the chapters.

Many readers will probably wish that Prof. Campbell had devoted a little more space to a comparative treatment of the external forms, and had also given some account of the numerous biological adaptations which are so abundantly exhibited by many ferns and mosses; and we cannot help feeling that this would have been acceptable, even if it had necessitated the sacrifice of space now devoted to details of development.

The only point about the book which really strikes us unfavourably is the scanty recognition accorded to Palæontology. It is just amongst the vascular cryptogams that the palæophytologist is at his best, and we venture to express the hope that in future editions this most important branch of the subject may be far more fully treated.

If we have seemed to have indulged in a few adverse criticisms, this has not been done with any hostile intention, but merely by way of attempting to point out how, in our judgment, a really fine work may perhaps be improved. Enough, however, has, at any rate, been said to show that Prof. Campbell has rendered a most important contribution to the literature of botany; and we cannot, finally, suppress an expression of gratification that it should have appeared in the English language.

OUR BOOK SHELF.

A Laboratory Manual of Organic Chemistry. By Dr. Lassar-Cohn. Translated by Alexander Smith, B.Sc., Ph.D. Pp. vii + 403. (London: Macmillan and Co., 1895.)

IT will be readily understood in these days of rapid research that the appearance of a compendium of the latest reagents, processes and apparatus, intended to curtail the labour of the laboratory, would prove acceptable to organic chemists, and it is not surprising that the second German edition of this work should have appeared within a brief period. Its success in Germany may be taken as a guarantee that the English translation will be well received. The original will already have found its way, without doubt, into some of our laboratories. There are many useful laboratory methods scattered through the literature, which are frequently difficult to lay one's hand on at once. The present volume, which has been compiled with much discrimination, gives an account of all the important laboratory operations used in organic research, carefully described and illustrated. In these descriptions either the original account is reproduced, or a sufficiently detailed description is supplied by the author, so that a final appeal to the reference may generally be dispensed with—an important point, since herein lies the essence of the book's utility. The less important methods are briefly mentioned with a reference to the original paper. To try and strike a mean between these two extremes is obviously useless, and occasionally the author falls into this error. For example, under "aluminium chloride" (p. 105), it is stated that this substance is prepared from aluminium and chlorine. Then follows a fairly long account of a method by Gattermann, from aluminium and hydrochloric acid. The description is unaccompanied by any diagram, and is so incomplete that I am confident that any one who attempted to prepare it for the first time from this description would fail. In such a case the author would render a greater service by simply giving the reference.

The book does not aim at teaching the principles of organic chemistry, and it is perhaps not fair to find fault with some of the expressions and definitions used, which are apart from its main purpose. One brief reference may be permitted.

Chapter xii. (p. 10) contains a general account of "condensation." The writer has never met with any good definition of this term, and it is perhaps, as Gmelin said of the relation of organic to inorganic chemistry, more easily felt than defined; but surely we have here a needless confusion of ideas or, at least, of language. In the first paragraph we read: "By condensation we mean the formation of a substance from two others with loss of water, alcohol, hydrochloric acid, ammonia, or a halogen from both components." That the *two others* are not necessarily organic substances we learn from the next paragraph, where an example of condensation is given in the case of hydroxylamine and an aldehyde. Consequently we may include in this term the formation of ethereal salts, nitro-compounds, sulphonic acids, &c. Further down we read: "We include under this heading also the phenomenon of internal condensation, in which a body loses water and forms a new substance." Is the formation of ethylene from alcohol *condensation?* In the next paragraph we are told that loss of water is not essential; but that polymerisation is a form of condensation. In the next we read: "By means of condensation (*i.e.* the formation of a substance from two others) chemists have been able to prepare far more new bodies and entire classes of bodies than by any other process."

With the exception of a few mistakes in spelling, especially of proper names, and a few omissions in the index, the work of the translator leaves nothing to be desired. J. B. COHEN.

Physikalisch-chemische Propaedeutik, Erste Hälfte. Von H. Griesbach. (Leipzig: Wilhelm Engelmann, 1895.)

THIS work is designed mainly for the use of the chemist and the doctor; for, according to its author, the former, when engaged on certain legal inquiries or on questions relating to hygiene, must know something of medical science, and the latter, in order to follow his calling to advantage, must be familiar with much that is chemical and physical.

The present volume is the first half of the work, and deals with physico-chemical science and logic, the origin, nature, methods, and aim of physico-chemical science, measurement and systems of measurement, time, space, matter, energy, motion, velocity, the divisibility and constitution of matter, hypotheses regarding the ether, the atomic hypothesis, living and dead matter, organised matter as producing fermentation and disease, &c.

The reader requires no special scientific knowledge to follow the information supplied, which differs essentially from that given by most of the text-books, as a detailed historical account, containing short biographical sketches of leading investigators, is given in the case of each of the subjects dealt with. References are also given to original papers, and although the material discussed is mainly theoretical, apparatus and methods are also treated to some extent.

For a book which deals with subjects so widely apart as, say, the genesis of the elements and the karyokinesis of a living cell, the information is remarkably accurate, up to date, and well arranged; and the historical method adopted in the case of subjects which are but seldom handled in this way, makes the book specially interesting and valuable. J. W. R.

The Pterophorina of Britain. By J. W. Tutt, F.E.S. Pp. 161. (Hartlepool: John E. Robson.)

ALTHOUGH dignified with the title of a monograph, this work, reprinted from the *British Naturalist*, is a carelessly compiled reproduction of almost every statement which has ever been published upon the British species of Plume-moths. Mr. Tutt has not attempted to condense into a useful or readable form this mass of crude material, which, however, may prove attractive to a certain type of collector. The generic diagnoses, unaccompanied by synoptic tables or figures, are taken mainly from Jordan's abstract of Wallengren's "Scandinaviens Fjädermott," and the specific characters are given mostly in the words of other writers, two or three descriptions being sometimes quoted for a single species. The book is roughly printed, and contains several misspellings of names; it will bring little credit to author or publisher, though as a compilation it may prove useful to those who care to search its pages.

Submarine Telegraphy and other papers. By James Bell, A.Inst.E.E., and S. Wilson. Pp. 63. (London: *Electricity* Office, 1895.)

A COLLECTION of papers, originally published in the columns of *Electricity*, dealing with matters belonging to technical telegraphy. Will be especially serviceable to persons engaged in the postal telegraph service, but appeals to all practical electricians.

LETTERS TO THE EDITOR.

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

The Astronomical Theory of the Glacial Period.

TWO letters have recently appeared in NATURE (October 17, p. 594, and November 29, 1895, p. 29), in which Sir Henry Howorth attacks Sir Robert Ball as the author of a work entitled "The Ice Age," on the ground that the supposed astronomical cause of glaciation is totally inadequate to produce the alleged effect.

I do not now write because I have any new contribution to make to the discussion, but because the author of the review in NATURE (January 28, 1892) of "The Ice Age" might perhaps be expected to express an opinion on the subject in the columns of NATURE.

I still think that the book has the merit of laying down the simpler issue as to the direct effect of the variation in the eccentricity of the earth's orbit on climate, and of setting aside the many collateral causes with which Croll has somewhat clouded the subject.

I wish, however, to reiterate that Sir Robert Ball has, as I think, emphasised the wrong numbers, when he lays so much stress on the ratio 63 to 37, which expresses the ratio of the heat received by a whole hemisphere in its summer to that received in its winter. The really important point to consider is what change that ratio undergoes when the eccentricity of the orbit varies.

In my review it was shown that, with maximum eccentricity of the earth's orbit, and with summer in perihelion, the ratio of the daily supply of heat in summer to that in winter must be augmented by the factor $\frac{199}{166}$; whilst with summer in aphelion the same ratio must be reduced by the factor $\frac{166}{199}$. Thus the contrast between the two configurations is best represented by the ratio of 199^2 to 166^2, or of nearly 6^2 to 5^2, or say as 3 to 2. These are the numbers which deserve emphasis.

The astronomical theory has, however, been recently subjected to a powerful criticism by Mr. Culverwell in some papers in the *Geological* and *Philosophical Magazines*,[1] and the criticism is, I understand, adopted by Sir Henry Howorth. A concrete case (using only round numbers) will express very shortly Mr. Culverwell's argument. At present, with practically zero eccentricity of the earth's orbit, in latitude 51° the ratio of the daily supply of heat in summer to that in winter has a certain magnitude, say A. Then the corresponding ratio for latitude 55° is $\frac{5}{6}$ A; and for latitude 47° is $\frac{6}{5}$ A. Now this difference is found to have nearly the same value, viz. 4°, for all the middle latitudes, so that it may be concluded that the alleged cause for glaciation would give London a climate something like that of Yorkshire; and the converse would produce a climate something like that of mid-France. The parallelism of the two cases is by no means perfect; but with allowance of the widest margin of uncertainty, it seems that neither a polar nor a tropical climate could be produced by the astronomical cause.

Is there any great flaw in Mr. Culverwell's argument? I do not at present see one; and great as are the uncertainties of the case, they seem insignificant as compared with those involved in calculations founded on the temperature of space, as used by Croll and Ball. Mr. Culverwell has independently carried to its logical conclusion the same line of argument as that of my review, and I can now only confess with regret that I did not perceive whither it tended.

The astronomical theory of the great changes of climate of which geology affords evidence is so alluring, that I cannot sur-

1 *Phil. Mag.*, December 1894, p. 541; *Geolog. Mag.*, decade iv. vol. ii. No. 367, p. 3, January 1895, and No. 368, p. 55, February 1895. Since this letter has been in type, I have read a valuable paper by Mr. G. F. Becker (*Amer. Journ. Sci.*, vol. xlviii. August 1894), in which he concludes that zero eccentricity of the earth's orbit will present the condition most favourable to glaciation. I have to thank Sir H. Howorth for reminding me of this paper.

render it without regret, and should gladly welcome a destructive criticism of Mr. Culverwell's argument.

I have had some conversation with Sir Robert Ball on this subject, and I find that he is not as yet disposed to change his opinion. He contends that, when we bear in mind that it is in the tropics that the great oceanic currents get their warmth, we should admit that the change in the daily supply of heat by one-fifth part is competent to produce a great change in northern climates. Whilst I think that he would not now lay much stress on the quantitative results derived from the supposed temperature of space, he would still maintain that the cause is adequate to the effect. But does not this bring us nearly back to Croll's point of view, and demand a discussion of the effect of diminished or increased sun-heat on oceanic circulation?

December 16, 1895. G. H. DARWIN.

[At the request of the Editor, one sentence has been erased from the original letter.]

Barisal Guns.

WITH reference to Prof. Darwin's letter in NATURE of October 31, 1895, relative to Barisal Guns, I enclose a communication, which I received from an observer familiar with the phenomenon.

Medical College, Lahore. D. G. F. GRANT.

I first heard the Barisal Guns in December 1871, on my way to Assam from Calcutta through the Sunderbans. The weather was clear and calm, no sign of any storms. All day the noises on board the steamer prevented other sounds from being heard; but when all was silent at night, and we were moored in one or other of the narrow channels in the neighbourhood of Barisal, Morelgunge and upwards, far from any villages or other habitations, with miles and miles of long grass jungle on every side, the only sounds the lap of the water or the splash of earth, falling into the water along the banks, then at intervals, irregularly, would be heard the dull muffled boom as of distant cannon. Sometimes a single report, at others two, three, or more in succession; never near, always distant, but not always equally distant. Sometimes the reports would resemble cannon from two rather widely separated opposing forces, at others from different directions but apparently always from the southward, that is seaward. We were not very far from the sea when I first heard them, and on mentioning to an old lady on board that I heard distant cannon, she first told me of the mysterious sounds known as the "Barisal Guns." For the next two years I was in Upper Assam, above Goalpara, and do not remember ever hearing them there; but in 1874 I was working in the Goalpara district in the tract south of Dhubri, between the Brahmaputra and the Garo Hills; sometimes near the river, sometimes near the foot of the hills, at others between the two. I gradually worked down as far as Chilmari Ghât (I think it is called), the landing-place for Tura, the headquarters of the Garo Hills district, and distant quite 300 miles from the mouths of the Brahmaputra and Ganges. The villages are few and far between and very small, firearms were scarce, and certainly there were no cannon in the neighbourhood, and fireworks were not known to the people. I think I am right in saying I heard the reports every night while south of Dhubri, and often during the day. The weather on the whole was fine. Short, sharp "nor'westers" occasionally burst on us of an evening, with much thunder and lightning; but the days were clear, and, as a rule, the sounds were heard more distinctly on clear days and nights.

I specially remember spending a quiet Sunday, in the month of May, with a friend at Chilmari, near the river-bank. We had both remarked the reports the night before and when near the hills previously. About 10 a.m. in the day, weather clear and calm, we were walking quietly up and down near the river-bank, discussing the sounds, when we heard the booming distinctly, about as loud as heavy cannon would sound on a quiet day about ten miles off, down the river. Shortly after we heard a heavy boom very much nearer, still south. Suddenly we heard two quick successive reports, more like horse-pistol or musket (not rifle) shots close by. I thought they sounded in the air about 150 yards due west of us over the water. My friend thought they sounded north of us. We ran to the bank, and asked our boatmen, moored below, if they heard them, and if so in what direction. They pointed south!

As we often did with boatmen, we asked these their opinion of the sounds. They said they heard them at all seasons and in every direction all up the river from Serajgunge to Dhubri—their beat; that they were in the air, and came from the gods (Deota) celebrating the continuous marriage of the Ganges (the goddess Ganga) with the Brahmaputra (son of Brahma); that they were heard in their fathers' time, and long before. We could elicit nothing further from them or others.

The year previous I had discussed the sounds with Captain Stewart, of the Survey of India Department, who had some years previously been employed on the Survey of the Sunderbans tracts. He said the reports were heard all over the Sunderbans; that several experts had failed to account for them. He once had a theory that they were caused by submarine eruptions in the Bay of Bengal; but this would hardly account for them 300 miles distant, and I believe they are never heard out at sea in the bay.

I have heard planters (who have heard them near the hills where bamboo jungles abound) say that they were merely reports caused by bamboos bursting in jungle fires. But they are heard far from all bamboo jungles, and in the absence of jungle fires.

Strange to say, I next heard the reports when crossing the Mahanadi River, between Purnea and Kishengunge in the old road from Sahebgunge to Darjeeling.

The time was about ten at night, the evening close, hot, and very cloudy, but no thunder anywhere. The booming sounded some miles away. There are no cannon anywhere in the neighbourhood, nor any large cities where possibly fireworks might be in progress. The year after I heard them again at the same place; on this occasion the sky was clear and starry, the time between 3 and 4 a.m., the booming distant but very distinct.

I have no theory. I fancy the sounds must be purely electric, but certainly have nothing to do with clouds, nor with cannon, nor fireworks, nor jungle fires.

If I can answer any other questions on the subject, I will be pleased to do so; but I think I have told you all I know.

G. B. SCOTT.

Remarkable Sounds.

I HAVE this day received from the Rev. W. S. Smith, Congational Minister of Antrim, Ireland, the following account of natural sounds connected in some way with Lough Neagh. The details are so interesting that I send them as a contribution to your present correspondence on natural sounds.

Highgate, N., December 19, 1895. C. TOMLINSON.

Lough Neagh is a sheet of water covering an area of upwards of 150 square miles, with very gradually receding shores, excepting at one or two spots. For many years after my settlement as minister here from England, I heard at intervals, when near the lake, cannon-like sounds; but not being acquainted with the geography of the distant shores, or the location of towns, or possible employments carried on, I passively concluded that the reports proceeded from quarrying operations, or, on fine summer days, from festive gatherings in Co. Derry, or Co. Tyrone. In time I came to understand that it was not from the opposite shores, but from the lake itself that the sounds proceeded. After questioning many of the local residents, I extended my inquiries to the fishermen, but they could assign no cause. A strange thing about the matter is that the people generally knew nothing of the phenomenon, and that it is shrouded in mystery. I have heard the sounds during the whole year. . . . I have heard the reports probably twenty times during the present year, the last being on a Sunday afternoon a month since, when I heard two explosions; but with two exceptions they have all seemed to come from many miles away, from different directions at different times. They have come apparently from Toome Bay, from the middle of the lake, and from Langford Lodge Point, about nine miles distant. A fisherman thought they must be the result of confined air that reached the lake by means of springs that are believed to rise here and there in the bottom. But the lake is shallow, seldom more than 45 feet deep. The depression now covered by the lake having been caused, it is believed, by volcanic action when the trap-rock of Co. Antrim was erupted, there may possibly be subterranean passages, though I confess their occurrence does not seem very probable; while the sounds emanate, as stated, from various parts of the lake. I have as yet spoken to no one who observed any movement of the waters when explosions took place, nor have I spoken to any one who was close to the spot at the time. Rather every one seems to have heard them only in the distance, which is strange, as fishermen are on the lake during many months in the year, at all hours of the day and night.

Last winter the whole of the lake was frozen over, for the first time since 1814. One fine afternoon, when the air was still, I was skating in the neighbourhood of Shande's Castle, when these mystical guns boomed forth their reports every five or six minutes. On the last day of the skating, when thousands of people from Belfast and elsewhere were assembled in Antrim Bay, there were two fearful boomings, that startled every one near me. They seemed to think some dreadful catastrophe had occurred, as the sounds appeared to proceed from not more than half a mile away. I never before heard them so near. The ice in Antrim Bay remained as it was, but I afterwards learned that it was then breaking up six miles away, but with no alarming sounds. Last February, when the ice of Lough Neagh was breaking up, a strange occurrence took place at Ardmore. A great ridge of ice, a mile and a half long, and 10 feet high, and 15 feet at the base, was formed along the shore during three days. There was a dead calm at the time, so that the ice was not thrown up by the waves. The ice along this part of the shore, for a third of a mile out, was intact; the ridge must have consisted of ice brought from a considerable distance, and forced under the shore ice, which was raised every few yards into small archways, and then shot out from beneath to the height previously specified. The pieces of ice were from half a yard square to bits of an ounce in weight, all mingled in the huge mass. Such a sight had not occurred since 1814, when, as I learned from a member of my congregation, who had seen it in that year, the water of the lake could not be seen from the shore-road on account of this icy obstacle. There are currents in Lough Neagh, but I am not aware of any strong enough to produce such an effect. W. S. SMITH.

Postcript.—In my former letter, an extract was given from Major Head's work, published some fifty years ago, and on again referring to it, I find the following passage:—"The cold increased to a very low temperature, the effect of which upon the extended sheet of ice that covered the bay, was remarkable. It cracked and split from one end to the other with a noise that might have been mistaken for distant artillery." This explains the sound to some extent in winter, but Mr. Smith says that the cannon-like sounds may be heard at any time of the year. This requires explanation. C. T.

IN your issue of November 14 last, Prof. McKenny Hughes appears to favour the idea that the curious sounds heard near the shores of Morecambe Bay, are due to the waves breaking "on the long, flat shore" thereof. I heard these sounds on Saturday, December 21, and could trace them to blasting operations near Carnforth. I heard them between 9.15 and 9.30 a.m., and the *tide was quite out*. So the "very suggestive coincidence" between the Bays of Bengal and Morecambe loses point! Further, it is seldom that the waves break at all, or even "curl over" on the shores of Morecambe Bay, where it is flat; but the water simply overflows the banks of the channel of the river Kent, whose course is followed by the tidal wave. There are limestone quarries at or near Arnside, Silverdale, Warton, Carnforth and Borwick. O. FIRTH.

Hawthorn House, Baildon, Yorks.

CHARLES DARWIN, in his "Naturalist's Voyage round the World" (new edition, 1890, p. 346), describes some curious earth-sounds heard in Northern Chile; he also gives references to Seetzen and Ehrenberg as authorities for the occurrence of similar sounds on Mount Sinai, near the Red Sea.

It is stated that the phenomenon is caused by sand in motion. WILLIAM STONEY.

Civil Service Club, Capetown, December 4, 1895.

The Merjelen Lake.

THE annexed illustration is reproduced from a photograph taken by me on August 16, 1890, when the lake was empty. How long it had been so, or how long it continued, I cannot say. So far as my memory serves me, there was no water whatever in it, and I distinctly recollect noticing the icebergs lying high and dry on the bottom. I walked along what had been the margin of the lake on my way to the Aletsch glacier, which I ascended to the Concordia Hut. I hope the photograph may be of interest to Dr. Preller, and others who know the lake.

GREENWOOD PIM.

Co. Dublin, December 19, 1895.

The Metric System.

I NOTE a call from Mr. John W. Evans, in NATURE for December 5, 1895, for the use of the metric system in meteorology. If this means the substitution of the metre for the yard, there can be no serious objection except this. In meteorological studies ·01″ of air pressure is an extremely convenient limit, and in most inquiries only two figures are needed. On the other hand a millimetre (·04 inch) is altogether too large a limit, and one-tenth m.m. is too small. The labour of writing, averaging, and studying with the metric scale will be at least one-third greater than with the common inch scale to the same degree of accuracy. It is a very great pity that the French, in looking for a convenient length to divide into a thousand parts, did not take up the already existing yard, which is nearly the same as the metre. Yard ÷ 1000 = (metre ÷ 1000) − ·004 inch. The amount of confusion that would have been saved is almost incalculable.

The metric system usually carries with it the Centigrade scale on the thermometer, and here the whole English-speaking world should give no uncertain sound. I am not contending for any scale in chemistry or geology, but in meteorology it would be difficult to find a worse scale than the Centigrade. The plea that we must have just 100° between the freezing and boiling points does not hold; any convenient number of degrees would do. The Centigrade degree (1°·8 F.) is just twice too large for ordinary studies. The worst difficulty, however, is in the use of the Centigrade scale below freezing. Any one who has had to study figures, half of which have minus signs before them, knows the amount of labour involved. To average a column of thirty figures, half of which are minus, takes nearly double time that figures all on one side would take, and the liability to error is more than twice as great I have found scores of errors in foreign publications where the Centigrade scale was employed, all due to this most inconvenient minus sign. If any one ever gets a "bee in his bonnet" on this subject, and desires to make the change on general principles, it is very much to be hoped that he will write down a column of thirty figures half below 32° F., then convert them to the Centigrade scale, and try to average them. I am sure no English meteorologist who has ever used the Centigrade scale, will ever desire to touch it. It is much easier and safer to convert the Centigrade scale to the Fahrenheit before any studies are made. As a matter of fact, European temperatures are a sealed book to active workers in meteorology on account of this unfortunate scale.

England and the United States may congratulate themselves, however, in the fact that European meteorology is almost flat. There are no low area or high area systems moving with regularity at any definite speed or in any definite direction, such as are experienced in the United States three or four times in a month. The rains in Europe are mostly sporadic, and do not accompany any well-defined low areas.

If any change is to be made, cannot the meteorological world come together upon a thermometer having its zero at −40° both Fahrenheit and Centigrade. This would be a most convenient scale, and would eliminate nearly all minus signs. The conversions from the old to the new would be very convenient by simply adding 40° to each. The conversion from the new Fahrenheit to the new Centigrade would also be extremely convenient by dividing by 1·8 instead of the present very puzzling system, and *vice versa*, Centigrade degrees + 1·8 would give the Fahrenheit value. This would obviate the difficulty of the minus sign, but there would still remain the fact that the Fahrenheit degree is by far better than the Centigrade.

Washington, D.C., December 18, 1895. H. A. HAZEN.

Apparatus for determining the Specific Gravity of Minute Fragments of Minerals.

THIS is a contrivance designed to facilitate the measurement of the specific gravities of minute fragments of minerals by means of a diffusion column of methylene iodide, as described by Prof. Sollas in NATURE, vol. xliii. p. 404, and vol. xlix. p. 211. There is no new principle involved in the construction of the instrument. It is merely the adaptation to this particular purpose of a well-known device for getting rid of the error due to parallax, such as is employed in Prof. Joly's spring balance for determining specific gravities, and the best form of tangent galvanometer. The index, which in this case is a fine wire, or fibre of any kind, is reflected in a mirror placed behind it, the coincidence of the fibre and its reflection showing that the eye of the observer is exactly on a level with the index.

In the drawing, *a* (Fig. 1) is a test tube containing the methylene iodide column in which the fragments of mineral under observation are immersed, together with two or more beads or fragments of mineral whose specific gravity is known. This is held by a clip, *b*, in a slot cut through the upper end of a thin piece of wood, some 12 inches high, fixed vertically to a firm base, so as to bring the test tube and its contents to a convenient height above the table. A slip of mirror, *d*, carrying a scale, *e*, divided in millimetres or any even graduations, is fixed vertically at the side of the slot, and the fibre is carried by a slider, *c*, which is shown separately in Fig. 2. This may be made of a strip of thin sheet copper, and has a "window" cut through it at *b*, so that the contents of the test tube may be seen.

The readings are taken by bringing the fibre successively on a level with the approximate centre of gravity of each fragment,

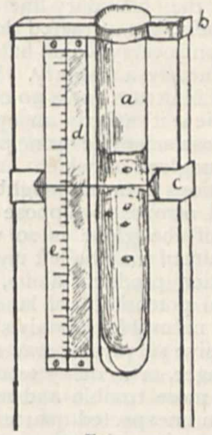

FIG. 1.

and noting the division of the scale cut by it, taking care that it coincides with its reflection in the mirror at each reading. The instrument might be improved by making the scale movable through a short distance vertically, so that one of the larger divisions of the scale might always be brought on a level with

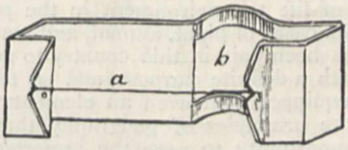

FIG. 2.

the uppermost of the beads or fragments used as indicators. The whole thing may be made at a cost of less than a shilling. It has been tried in the geological laboratory of the Royal College of Science for Ireland with satisfactory results.

T. D. LA TOUCHE.

Cactaceæ in the Galapagos Islands.

MR. HEMSLEY is mistaken in stating, as he does on p. 623 of NATURE for October 24, 1895, that Dr. G. Baur was attached to the U.S. Fish Commission Steamer *Albatross*. Dr. Baur had no connection with the *Albatross* Expedition of 1891, the object of which was deep-sea dredging, and only included an incidental visit of a few days to the Galapagos. Nor did Dr. Baur write the general sketch of that expedition, or have anything to do with the photographs which accompany it, as one might infer from a subsequent note by Mr. Hemsley on the Cactaceæ of the Galapagos (NATURE, November 14, 1895, p. 31).

As regards the Cactaceæ of those islands, I collected branches of *Opuntia* and of *Cereus* from Chatham and from Charles Islands; what became of those pieces I do not know, as they do not seem to have reached the hands of Dr. J. N. Rose, who described the plants collected at the Galapagos by the *Albatross* Expedition.

Mr. Hemsley will find that both Dr. Wolf and myself state that *Cereus* grows to 20 feet in height. Dr. Wolf also called attention to some striking differences he had noticed in the specimens of *Opuntia* and of *Cereus* he found on the different islands.

In stating his position, botanically speaking, regarding Dr. Baur's theory of the origin of the Galapagos, Mr. Hemsley would surely not give us to understand that *Opuntia* and *Cereus* are limited to Chili on the west coast of South America, as his second note (p. 31) seems to imply. ALEXANDER AGASSIZ.

Museum of Comparative Zoology, Cambridge, Mass., December 2, 1895.

[MR. HEMSLEY admitted, in NATURE of November 28, 1895, that he was in error in thinking Dr. Baur was attached to the U.S. Fish Commission Steamer *Albatross*.—ED. NATURE.]

THE VENEZUELA AND BRITISH GUIANA BOUNDARY.

THE sudden accession of an acute phase in the question of the boundary-line between British Guiana and Venezuela, has attracted the attention of the whole world to a controversy which has been proceeding intermittently for nearly a century. With its political aspect the pages of NATURE have no concern; but from another point of view it affords an opportunity for enforcing some of those scientific principles of geography, the ignorance or neglect of which has done much to embarrass the relations between neighbouring countries.

The fundamental error is to suppose that any piece of land on the face of the globe is so worthless that its ownership once claimed may be left undefined. So long as forest or plantation products alone, or mining alone, or any other partial potentiality of land is considered by itself, it is perfectly natural for colonists and governments to postpone expensive surveys of tracts that promise no immediate advantages, or to delay troublesome negotiations. In the end more trouble and more expense have to be faced, often in unexpected quarters.

Political geography is no longer a matter to be treated tentatively by politicians, any more than industrial chemistry is now a matter for the uninstructed experiments of tradesmen. It is the highest outcome of geographical theory, a theory which deals without a break with all terrestrial distributions, from the primary elements of geomorphology and climatology, to the adjustment of life to environment in the progressively more complex cases of plant, animal, and man. So little attention has been paid in this country to geography as a science with a definite purpose, and so few explorers have been equipped with even an elementary acquaintance with the principles of geography, that we gladly seize this opportunity to urge the importance of geographical theory as a guide to the prevision and prevention of frontier-disputes.

Two stages are necessary in arranging a frontier—drawing it on a map, and demarcating it on the ground. For the former purpose it is easiest to take a mathematical line, a meridian or parallel; for the latter some distinct physical feature, and of these there are only two which can be looked on as satisfactory—a watershed or a *Thalweg*. In practice the *Thalweg*, which is the line along which converging slopes meet, as the watershed is the line along which diverging slopes meet, means the central line of a river. The sea counts as neutral territory in all international affairs, and the coast-line requires no definition. The position of a parallel may be determined astronomically with great exactness, and, when marked on the ground by posts within sight of each other, is perfectly explicit; but it involves highly-skilled work and the agreement of two parties of expert surveyors. The case of a meridian may also be settled, but can rarely be free from the risk of rectifications being demanded, as more exact methods of determining longitude become available. Noteworthy exchanges of territory may thus be necessitated, perhaps involving hardships to individuals. A great mountain seems a peculiarly fitting corner-stone for the meeting of national frontiers, and is so used in the case of Mount Ararat; but Mount St. Elias has had to change its nationality by the rectification of the meridian of 141° W. Such a boundary-line as that between the southern part of Alaska and British Columbia, a line parallel to the coast, and ten leagues distant from it, represents perhaps the least scientific frontier on the face of the earth. It might be possible, when large scale maps are made of the fjord-riven coast, to draw this line on them; but only mutual goodwill and concessions could ever have allowed it to be even approximately marked on the ground.

In the particular instance before us, a map published by the Venezuelan Government in 1890, which it is impossible to reproduce on a small scale without the use of colour, shows the ten hypothetical western boundaries of British Guiana which have been put forward by one side or the other in the course of negotiations. Most of these lines are an outrage on geography, and it is difficult to believe that some of them were seriously put forward by the statesmen whose names they bear. We have been unable to find any British map showing these proposed boundaries, and it does not appear that our Foreign Office has published one. The boundaries in many cases cut natural features and mathematical lines at all angles and in irregular curves which it would be impossible either to describe verbally or to lay out accurately on the ground without a survey as minute as for a railway. These we do not require to dwell on, except in the way of pointing the moral that the results of geography should be officially recognised by Government departments more fully than has hitherto been the case. The Intelligence Division of the War Office is, in a sense, the Government Geographical Department, but it is concerned mainly with the practical work for the Army, and might well be supplemented by a more purely geographical office. It is true that the Royal Geographical Society is always ready to render help when called upon, and does not infrequently answer questions as to matters of fact. What is wanted, however, is rather an official geographer who may be consulted by the Government on matters of geographical theory as well as of fact, and who might be charged with the duty of preparing and keeping up to date an official geography of the British Empire.

The geographical conditions of the present boundary difficulty can be stated easily, so far as the main features are concerned; but the details are many and complicated. A boundary dispute in itself is a quite normal condition in South America. The boundaries of every republic in that continent are disputed; in several cases three neighbouring countries claim the same territory. It is impossible to draw a political map of South America, even on the smallest scale, that would be generally acceptable. In the case of Guiana, it is well to look at the physical condition of the land before tracing the contested boundaries. From the mouth of the Orinoko to the mouth of the Amazon the coast of South America runs nearly south-east, and the distance is 1000 miles. A line drawn from river to river, about 500 miles inland from the coast and parallel with the coast-line, contains the whole region known as Guiana. It consists of a gently rising plateau of Archæan rocks edged by very low plains of quaternary formation, bordering the Orinoko on the north, the Atlantic on the north-east, the Amazon on the south, and forming the llanos of the Rio Negro and Upper Orinoko on the south-west. The plateau bears a number of low mountain ranges, which have been made familiar by recent explorations; the Tumak-humak, Akarai, Pacaraima (including Roraima), Parime and Imataka, amongst others. The trade winds bring a heavy and regular rainfall to the whole eastern slope, causing it to be clothed with perhaps the richest and densest tropical forests of the world. On the top of the plateau, and on the western slopes, there is a reduced rainfall, and in accordance with this savannahs take the place of woods. Many large rivers carry off the surplus rainfall; of these we may name, on the north-eastern slope, from south to north, the Araguary, Oyapok, Maroni, Korentin, Essequibo (including the Mazaruni and Kuyuni), and, turning north-westward towards the sea, the Barama (or Guiana), Barima, and Amakuru. Every one of these rivers flows through dense primeval forests for the greater part of its course, and discharges on a muddy coast-line, which is rapidly growing seawards, thanks to the aid of mangroves.

On the south-western slopes the rivers are much longer, and flow to the Amazon and the Orinoko, the two great river-systems being connected, as is well known, by the natural canal of the Casiquiare. The more westerly rivers flow, for the most part, through open savannah country; so that the water-shed of the plateau is much more accessible from the Orinoko on the west and north than it is from the Atlantic on the north and east. From Mount Roraima south-eastward *Thalwegs* of the upper tributaries of the Rio Branca form a political boundary to near the source of the Essequibo, after which the water-shed is a political boundary, the whole southern slope (except a small portion of the Rio Branco basin included in British Guiana) constituting Brazilian Guiana. From Roraima westward the whole area draining to the Orinoko River incontestably belongs to Venezuela. The present frontier disputes concern the forest-covered Atlantic drainage area, which is shared by the only three colonies remaining in South America—British Guiana on the west, Dutch Guiana in the centre, and French Guiana to the east. Here most of the boundaries are *Thalwegs*, *i.e.* the central line of rivers. There is no question as to Dutch Guiana, which is demarcated by treaty from British Guiana by the Korentin, and from French Guiana by the Maroni. But here certainty ends. The French and Dutch differ as to which of the upper tributaries of the Maroni should be taken as the boundary between Dutch and French territory. The Brazilian Government recognises the Oyapok as the French boundary toward Brazil; the French claim the Araguary, the space between the two rivers which flow nearly at right angles to each other being a triangle with 250 miles of coast as a base. The uncertainty of boundaries at the east of Guiana is simpler to understand, and easier to explain than that at the west. The claim of Venezuela is that British Guiana is bounded by the *Thalweg* of the Essequibo, and it is so represented on Venezuelan maps. The extreme British claim, on the other hand, is that the whole drainage area of the Essequibo belongs to the colony, *i.e.* that the boundary is the main watershed of the plateau as far as the Imataka range on the north, which runs parallel to and very near the Orinoko. In addition, there is a claim for the basins of the small rivers which occupy the triangular area between the drainage areas of the Orinoko and the Essequibo. If the western boundary of British Guiana were to be of the same type as those of the other colonies, it would require to be the *Thalweg* of a river flowing into the Atlantic, *i.e.* either the Essequibo, the Mazaruni, the Kuyuni (these three unite to enter the sea at a common mouth), or the Barama (Guiana), Barima, or Amakura.

It is now too late to suggest the solution of the boundary problem by geographical principles. Were it not so, a very interesting argument could be held as to how far the physical unity of a drainage basin is impaired by the obstacles to movement along the *Thalweg* due to cataracts interrupting navigation on the rivers, and forests obstructing progress on land. It is, in fact, very much easier to reach the upper basin of the Kuyuni branch of the Essequibo over the savannahs from the Orinoko than through the forests from the Atlantic coast.

All modern maps of Guiana—except the Venezuelan—follow what is known as Schomburgk's boundary, either in its original or in a modified form.

In 1840 the Schomburgk line first appeared on a sketch map, the topography of which was very inexact. From the Amakuru River in the north it ran along the watershed southwards, thus leaving the whole basin of the Barima in British Guiana. It so happened that the line ran nearly on the meridian of 60° W. as far as the Kuyuni River; and when the Barima was found to rise far to the west of that meridian, the line was often still drawn along it, instead of following the watershed as was intended. In 1886 the British Government modified the line by carrying it along the Kuyuni River to its source, and then for a short distance along the watershed, to Roraima.

The whole area within the Schomburgk line has been taken into effective possession by the Government of British Guiana so far as a tropical forest of such magnitude can be occupied. The Barima River was recently explored to its source by Mr. G. G. Dixon, and the account of his journey in the *Geographical Journal*, for April 1895, gives some idea of the difficulty of forcing a way through the woods. Much of the land is auriferous, and the real point of the present frontier difficulty lies in the value of Yuruari mines in the upper basin of the Kuyuni, at present occupied politically by Venezuela, and commercially by the nondescript cosmopolitan population always attracted to gold-fields.

It is this fact that makes it hopeless to expect the dispute to be settled by the geographical principles which forty years ago could have easily prevented it. The only alternatives are to base the rival claims on actual effective possession, or on the original rights which were recognised between the Dutch settlers in Guiana and the Spanish colonists of the Orinoko at a time when the geography of the district was practically unknown. The romantic story of British enterprise in Guiana is admirably told in Lucas' "Historical Geography of the British Colonies," vol. ii., a work of admirable clearness and brevity.

D'Anville's atlas of 1772 shows practically the whole of the disputed area as Dutch Guiana, but contemporary and later maps are very conflicting, and all of them being unofficial are of small value as evidence. The chart of Captain Edward Thompson, who took part in the first capture of the "Wild Coast" from the Dutch in 1781, marks the Barima as "the western boundary of the Dutch according to their claim," but does not suggest any boundary in the interior. The rights and wrongs of historic evidence will doubtless be fully investigated by those responsible for a decision, and the present dispute will probably be settled, as similar difficulties have been settled before, by some judicious compromise which will give both parties the inestimable benefit of a fixed and definite frontier. But similar disputes will continue to arise in other places, and their solution will be protracted and rendered difficult as long as unsurveyed territory is claimed by rival powers, spurred on by rival concessionaires and interested company promoters.

The recent International Geographical Congress decided that the time had come when all governments should be urged to make a map of their possessions on the uniform scale of 1 : 1,000,000, or about sixteen miles to an inch. If the governments of all countries were jointly to take this matter up, survey all unsurveyed lands which they claim, and submit the uncertain boundaries, which are not yet complicated by gold-mines, to an International Commission of Geographers, to be decided on the basis of the new map on purely geographical principles, the expense would be many times saved by the security which defined frontiers give, and a magnificent contribution to science would be effected.

HUGH ROBERT MILL.

DR. JOHN RUSSELL HIND, F.R.S.

IT is with deep regret that we announce the death of Dr. J. R. Hind, whose name and whose work were possibly more familiar to astronomical students of the last generation than they are to those of to-day. By this we do not mean to imply that Dr. Hind had outlived his reputation, but that circumstances forced him to the front early in life and in connection with subjects that have long since ceased to attract or to interest. His claim to scientific reputation and remembrance will mainly rest on his long connection with the *Nautical Almanac*, and the steady character for accuracy and efficiency that it maintained under his direction. But to the production of the *National Ephemeris*, while one is responsible, many contribute, and no one would admit more readily than the late chief of that department, how much he was indebted to the invaluable aid he received from such assistants as Messrs. Richard Farley, Godward, and others, or more willingly share the credit with those less well-known authorities. For these and other reasons, it is not a little difficult to assign to Dr. Hind his proper place among astronomers. He never devoted himself in any way to the higher branches of physical astronomy; the mathematical training that is sufficient for an engineer is not of that character that is required to advance our knowledge of planetary theories, or to assist their development by new functions. He will rank rather with the school of Argelander, to whom he was deeply attached, than with that of Bessel or Le Verrier. It is equally true that he never had occasion to employ the newer methods of observation that spectroscopy and photography demand, or to discuss the results obtained by their means, since the habits of his life and the direction of his work were settled before these methods of investigation were generally employed. Looked at, therefore, from the broader ground that astronomy now occupies, his scientific life seems somewhat cramped; but to conclude that his career was misspent, would be to read the history of astronomy for the last half-century very incorrectly. He was emphatically a practical astronomer, and whether as an observer or in making the mathematical work of others available for practical ends, he had few equals. He knew his capacity very well; he attempted nothing beyond his powers, and few men have made fewer mistakes.

As already intimated, Dr. Hind was originally intended for the profession of engineering, a science for which he had little taste, and it was fortunate for his subsequent career that circumstances permitted him to join the staff of the Royal Observatory. He was attached to the magnetical and meteorological department, at that time not fully organised nor even confidently regarded as a permanent part of the establishment. In those early days (1840), self-recording instruments were practically unknown, and meteorological readings and general attention to details required all-night sittings from the assistants. It was in these long watches that he acquired the habit of calculating comet and planetary orbits, undertaken at first with the view to keep himself awake, but which grew into a confirmed habit, and laid the foundation of his reputation as a computer. In 1844 he left Greenwich to take charge of Mr. Bishop's private observatory at Regent's Park. At that time Neptune was not discovered, and the first work that he began at that observatory was the formation of ecliptical charts of stars, three degrees each side of the ecliptic, with the view of detecting the object that disturbed the motion of Uranus. The comparison of these charts with the heavens led to the discovery of a number of small planets, which then were objects of interest and importance. Some variable stars, and a few comets of which he was the fortunate discoverer, extended his reputation and attested his zeal as an observer. Meanwhile the habits of calculation that he had acquired at Greenwich, were never allowed to lay dormant, and every *Nachrichten* as it came to England contained the orbit of a comet or a new planet which he had contributed to its columns. The history of ancient comets, the unravelling of the tedious descriptions of old or of Chinese astronomers, was his constant occupation, so that he acquired a masterly knowledge of the history of that portion of astronomy, and at one time we believe he had the intention of publishing an annotated *Pingré*. That such a work would have enhanced his reputation, and shown him to be the possessor of much curious information gleaned from many authorities, cannot be doubted, but the pleasure he derived from clearing up doubtful points, and adding to his own stock of information, were his only reward.

His facility as a computer led to his selection for the post of Superintendent of the *Nautical Almanac* when a vacancy occurred in 1853, through the death of Lieut. Stratford, and it will generally be admitted that he filled the post with credit to himself, and to the extended reputation of the important work of which he had charge. Official duties to some extent interfered with his private researches, but his industry was always very great. The list of papers to which Dr. Hind's name is attached in the Royal Society's "Index," though considerable, does not exhibit the full measure of his computational activity, because he did not often publish his results. An examination of this "Index" will, however, show the large variety of astronomical topics to which he turned his attention. And to this list we can only refer, without mentioning any particulars. But, in this place it would be ungrateful not to recall the fact that the deceased astronomer was the first contributor to our "Astronomical Column," and for some years the whole of the "Notices" were written by him. Neither can we undertake to give a complete list of the honours and awards that were showered upon him. He received the medals of both the Royal and the Royal Astronomical Society, the Danish Medal for cometary discovery, and the Lalande Medal on more than one occasion. He was made a Corresponding Member of the Institute of France, and many other foreign societies placed his name among their honoured members. The writer of this brief notice trusts that it may not be out of place for him to record his own sense of indebtedness for many acts of kindness and much valuable information that he has received at the hands of Dr. Hind. WILLIAM E. PLUMMER.

NOTES.

THE list of New Year Honours includes the names of two well-known men of science—Sir Joseph Fayrer, K.C.S.I., who has been made a baronet, and Prof. Prestwich, who has been knighted. If long and distinguished services to the cause of science count for anything, both Sir Joseph Fayrer and Prof. Prestwich have well earned the honours conferred upon them. Mr. H. H. Johnston, C.B., has been promoted to the Knighthood of the Bath; but this is probably more on account of his administrative work in recent years than for his explorations in Africa.

THE late Baron Larrey has left a bequest to the Paris Academy of Sciences for an annual prize of 1000 francs for the best treatise by an army doctor on any question of medicine, surgery, or sanitation.

PROF. A. ARCIMIS, writing from Madrid, informs us that at 6 p.m. on December 25, 1895, an earthquake was felt in some villages of the province of Orenne, Galicia, north-west of Spain. Some walls were cracked, the clocks stopped, and two small houses were thrown down.

THE commercial prospects of Hudson Bay are receiving much attention. Notice has been filed of application to the Canadian Parliament for a charter to construct a railroad from Calgary, on the Canadian Pacific Railroad, to Fort Churchill.

THE inhabitants of Zürich have rejected, by 39,476 votes to 17,297, a proposal submitted to them for the absolute prohibition of vivisection. On the other hand, a counter proposal of the Grand Council in favour of the protection of animals with due satisfaction to the demands of science was adopted by 35,191 votes to 19,551.

IT is announced in *Science* that Miss Helen Culver has signed papers giving 1,000,000 dollars to the University of Chicago, to be used for the biological departments. This gift carries with it 1,000,000 dollars conditionally pledged by Mr. John D. Rockfeller on November 2. It is probable that a school of medicine will be established.

THE death has just occurred of Dr. George H. Kidd, distinguished for his researches and discoveries in surgery. We have also to record the death of Prof. A. von Brunn, Professor of Anatomy in Rostock University, and of Dr. Sickenberger, Professor of Botany and Chemistry in the Medical School at Cairo.

A MONUMENT to Dr. John Rae, the Arctic explorer, was unveiled on Monday, in St. Magnus's Cathedral, Kirkwall. The base of the monument is of Aberdeen granite, the pedestal of Peterhead granite, and the figure of Portland stone. The sculptor is Mr. Joseph Whitehead. On the pedestal is an inscription setting forth the dates of Dr. Rae's different geographical expeditions.

DR. W. HUGGINS, F.R.S., has been elected a corresponding member of the Berlin Academy of Sciences, and also of the American Philosophical Society, Philadelphia.

THE twenty-third annual dinner of the Old Students of the Royal School of Mines will take place on Friday, January 24. The chair will be taken by Mr. A. G. Charleton, and a number of well known men of science are expected to be present.

THE annual meeting of the Association for the Improvement of Geometrical Teaching will be held at University College, Gower Street, on Saturday, January 11. The morning meeting (at 11) will be devoted to the ordinary business of the Association. At the afternoon meeting (at 2), Dr. Larmor will read a paper on "Geometrical Methods," and visitors interested in the subject will be cordially welcomed.

WE learn through *Science* that the Astronomischen Gesellschaft has decided, because of the expense connected therewith, no longer to maintain a library. The announcement is made that the Society does not desire to receive any publications in the future, and that, with the completion of the thirtieth year of the *Vierteljahresschrift*, no exchange with other scientific bodies will be continued. Our contemporary also publishes the news that the Astronomical Observatory of the University of Berlin will probably be removed to Dahlem, to which suburb it is proposed to remove the Botanical Garden.

AN ingenious system of purifying atmosphere and regulating temperature is in operation at Chicago, for the switchboard room of the Telephone Company; where dust formerly interfered seriously with the connections on the switchboard. The air for the room is forced through a chamber, where it is thoroughly sprayed; then passed through rapidly rotating spiral coils, which strip it of superfluous moisture, and afterwards through a chamber kept at nearly uniform temperature by the use of ice or of heating apparatus, as may be required. Access to the switchboard room is through an ante-chamber; and the temperature of the room itself shows a variation of not more than two degrees in a month.

UPON application by the East Suffolk County Council, the Home Secretary has made the following order: The taking or destroying of wild birds' eggs is prohibited in the years 1896, 1897, and 1898, in the following places within the administrative county of East Suffolk—namely, the sea coast, beach, foreshore, sandhills, saltings, or salt marshes, situate between the sea or estuaries and the land side of the sea or estuarial wall, embankment, ditch, fence, or other artificial or natural boundary separating the same from the cultivated land, from the north side of the river Blyth to Landguard Point (excluding the estuary of the Alde above the ferry at Slaughden Quay, Aldeburgh).

THE Duke of Devonshire has not had to wait long for the further information which he expressed a desire to have, when the deputation waited on him on November 28 to support the appointment of a Statutory Commission for the establishment of a teaching University for London. It will be remembered that the Duke, in the course of the interview, expressed a desire for information with reference to the attitude of the graduates towards the scheme contained in the Report of Lord Cowper's Commission, and upon other points. A long memorial, signed by sixty representatives of colleges, medical schools, and other institutions, has now been forwarded to the Duke of Devonshire, as President of the Council. The memorial deals seriatim with the attitude of the graduates, the amendments proposed in the interest of external students, the suggested procedure by way of charter, and the rights of veto under the existing charter. It should assist the Duke to form an opinion as to the unreasonable and unconstitutional nature of the opposition to the scheme.

MR. LEON CLERC, Secretary of the Chambre de Commerce Française de Londres, writes as follows:—"The town of Dôle (Jura), the birthplace of Pasteur, has decided to erect a monument to that great man. A Committee, at the head of which is M. Félix Faure, President of the Republic, and many of the present Cabinet, Senators, and Deputies, has been formed, and a list of subscriptions opened throughout the civilised world. The French Chamber of Commerce in London has been requested to take the necessary steps to bring this subscription to the notice of the British public. The work of Pasteur has been fully recognised in England, where his admirers are very numerous, and many will cherish his memory to the end of their days. We feel certain that, in this universal manifestation of gratefulness towards a benefactor of mankind, Englishmen will respond liberally. Our President, M. Marius Duché, Monument House, E.C., in conjunction with the whole of our Committee, will be pleased to receive all donations, which will be acknowledged in the Press; and should any representative men desire to join our Committee, we shall be pleased to accept their aid."

DURING this month the first number of *The Journal of Experimental Medicine*, a periodical devoted to original investigations in physiology, pathology, bacteriology, pharmacology, physiological chemistry, hygiene and medicine, will be published by Messrs. D. Appleton and Co., New York. The journal will be devoted exclusively to the publication of original work in the experimental medical sciences, with special references to work done in America. It will doubtless stimulate scientific investigation, and should extend the influence of scientific medicine. The practitioner who wishes to keep abreast of the times will appreciate the value of such a publication. That the journal will be of high character, and truly representative of scientific medicine in America, is assured by the character of those whose co-operation has been secured. Dr. William H. Welch, Professor of Pathology in the Johns Hopkins University, is to be the editor of the new journal, and with him will co-operate a board of twelve associate editors, all of whom are eminent workers in scientific medicine. The journal will appear in, at least, four numbers during the year, and oftener when necessary.

ANOTHER periodical, the first number of which will appear in America this month, is entitled *Terrestrial Magnetism* an international quarterly journal to be published under the auspices of the Ryerson Physical Laboratory, University of Chicago, and edited by Dr. L. A. Bauer, with the co-operation of numerous eminent workers in terrestrial physics in many parts of the world. The journal will be devoted exclusively to terrestrial magnetism, and its allied subjects, such as earth currents, auroras, atmospheric electricity, &c. The magnetic needle has become such a promising instrument of research, not only in terrestrial but also in cosmical physics, that the journal which is to be devoted to phenomena connected with it will appeal to a large class of investigators. To quote from the circular heralding the new publication: "No other mechanical means is so surely and so completely recording the physical history of terrestrial and cosmical changes as the self-registering magnetographs of our magnetic observatories, whereby the fitful tremors of the delicately suspended magnetic needle are being indelibly fixed on the sensitised sheet. On that paper, as Maxwell eloquently expressed it, the never resting heart of the earth is now tracing in telegraphic symbols, which will one day be interpreted, a record of its pulsations and its flutterings, as well as of that slow but mighty working [the secular variation] which warns us that we must not suppose that the inner history of our planet is ended."

The *British Medical Journal* publishes the following list of prizes awarded by the Paris Académie de Médecine. The Barbier prize of £80, offered every year for the discovery of a remedy for an incurable disease, such as hydrophobia, cancer, epilepsy, cholera, &c., was not awarded to any of the ten competitors, but an "encouragement" of £20 was granted to M. E. Legrain for his work on the sero-therapeutic treatment of typhoid, and smaller sums were awarded to five others. The Henri Buigniet prize of £60, for the best work on the application of physics or chemistry to medicine, was awarded to Dr. Chabrié for his memoir on the "Chemical Transformation of the Fundamental Substance of Cartilage during Ossification." The Adrien Buisson triennial prize of £420, offered for the discovery of a remedy for a disease hitherto looked upon as incurable, was divided among the following: £240 to Dr. Jarre for his work on the "Cure of Tic Douloureux of the Face by a New Surgical Method"; £80 to Dr. Chervin for his memoir on "Stammering and other Defects of Pronunciation"; £40 to MM. Wurtz and Marcano for their essay on "Leprosy: its Prophylaxis and Treatment"; £20 to Dr. Galliard for his work on "Pneumo-thorax"; £20 to Dr. Christiani, of Geneva, for his researches on the "Thryoid Body"; and £20 to Dr. Calvin, of the Medical Department of the French Army, for his work on "Chronic Paludism." The Chevillon prize of £60, for the best work on the treatment of cancer, was awarded to Dr. Répin for his work entitled "Contribution to the Study of a New Method of Treatment of the Malignant Inoperable Tumours: Toxitherapy." The Dauvel prize of £40 for the best work on myxœdema was divided between Dr. Combe, of Lausanne, and the Drs. Cristiani (M. and Madame), of Geneva. The Desportes prize of £52 for the best work on practical therapeutics was divided between Dr. Thibierge ("Therapeusis of Diseases of the Skin") and Prof. Delorme ("Disappearance of Neuritic Disorders, &c., by Localised and Forcible Compression"). The Huguier prize of £120 (triennial) for the best work on diseases of women, especially their surgical treatment, was awarded to Drs. S. Bonnet and P. Petit for their "Practical Treatise on Gynæcology. The Laborde prize of £200 was divided, Drs. Gouguenheim and Glover being awarded £100 for their "Atlas of Laryngology and Rhinology"; Dr. Chipault £60 for his "Operative Surgery of the Nervous System"; Prof. Reverdin, of Geneva, £20 for his essay on "Surgical Antisepsis and Asepsis," and Dr. Delbet £20 for his "Surgical Anatomy of the Bladder." A prize of £40 (Prix Adolphe Monbinne) was awarded to Dr. L. Petit for his book on "The Consumptive in the principal Countries of Europe." The Perron prize (£152) was divided among six candidates, Dr. Sabouraud getting the lion's share (£72) for his work on "Human Trichophytoses."

Mr. Dinshah A. Taleyarkhan, who was President of the Tropical Section of the International Congress of Hygiene and Demography, Budapest, has sent us, from Baroda, two short notes referring to the discovery of the anti-toxin of snake-poison. The first of these notes was published in January 1890; but as we find no mention of a publisher or society upon it, we conclude it was issued privately, though it appears to have been brought before the medical faculty at Baroda. In the course of his paper, the author suggested that "the blood of a weasel must itself be an antidote to snake-poison," and remarked: "An alternative process may also be tried in first inoculating animal-blood with the virus of a serpent, and then preparing an extract for inoculation into the blood of a human being bit by a serpent." No experiments are described by Mr. Taleyarkhan, the object of the notes he has sent us being to call attention to his suggestions, made five years ago, and to compare them with the actions taken by Prof. Fraser.

The question of the destruction of undersized fish and its prevention was by no means exhausted by the evidence laid before the Select Committee of the House of Commons, which was appointed to consider the subject in 1893. The evidence supplied to that Committee by the Marine Biological Association proved that the plaice of the English Channel belong to a smaller race, and reached maturity at a smaller size than those of the North Sea. Mr. J. T. Cunningham, one of the naturalists of the Association, has during the last year paid special attention to the further investigation of this subject in the North Sea, and has proved that the limits determined at Grimsby do not apply to the whole of that region. On the one hand, his evidence does not support the contention of the Germans that the plaice on their coast are a smaller race than those on the English side, but on the other hand he has found that the plaice landed at Lowestoft from the Dutch coast south of Texel are no larger than those of the English Channel, the extreme length of immature females being fourteen inches in the latter case, as against eighteen inches in the case of fish generally obtained in the North Sea. The difficulty of devising beneficial regulations to be applied to the fishing industry, while such important facts were still unknown, is sufficiently obvious.

Recent attempts to improve the existing methods for the isolation of argon from the atmosphere have led to a closer examination of reactions in which nitrogen is directly absorbed by metals. The number of these known to be capable of readily combining with nitrogen at a red heat now includes magnesium, lithium, barium, aluminium, zinc, iron, and copper. Magnesium nitride, the properties of which were first pointed out by Brigled and Geuther, and which played an important part in the discovery of argon, is now well known, but our knowledge of other metallic nitrides is still incomplete. Metallic barium, which is readily prepared by the action of sodium at a moderately high temperature upon the double fluoride of barium and sodium, has just been shown by M. Limb (*Comptes rendus*, December 9), to absorb nitrogen energetically, and its use as a cheap means of preparing argon from air is suggested. Other nitrides have been prepared by a new method by MM. Rossel and Frank (*ibid.*). Calcium carbide, well powdered and mixed with finely-divided magnesium (aluminium, zinc, iron or copper), on heating over a Bunsen burner, with free access of air, gives calcium oxide, together with a nearly quantitative yield of the corresponding nitride. But the most remarkable results have been given by lithium. This metal was recently shown by M. Guntz to absorb nitrogen with incandescence at temperatures below a red heat. It has now been shown by M. Deslandres (*ibid.*), and M. Guntz (*Comptes rendus*, December 16), that this absorption takes place slowly in the cold. The latter exposed about ten grams of lithium over sticks of caustic soda to a slow current of air; the product after four months consisted of seventy-six parts of nitride, twenty parts of hydroxide, and only four parts of metallic lithium. M. Deslandres proved the same fact under somewhat different conditions by allowing a confined volume of nitrogen to act on metallic lithium. The absorption was slow, but was so complete that the characteristic bands of the nitrogen spectrum entirely disappeared. M. Deslandres compares the reaction to the slow absorption of oxygen by phosphorus, and points out that as a reaction of nitrogen this is unique.

AN interesting contribution, by M. Osmond, to the existing knowledge on the molecular structure of hardened steel appears in the *Bulletin de la Société d'Encouragement* for November. He states that in highly carburised steels, containing more than 1·3 per cent. of carbon, quenched at temperatures above 1000°, there are two constituents, A and B, which differ widely in their properties. The constituent A is the ordinary, hard, strongly magnetic substance of which hardened steel containing 1 per cent. of carbon is almost exclusively composed. Its hardness is greater than that of orthoclase. B, on the other hand, is only about as hard as fluorspar, and, as far as can be judged by a study of its properties while mixed with A, is non-magnetic. M. Osmond, however, has been hitherto unable to prepare B free from A, although by quenching steel containing 1·6 per cent. of carbon at a temperature of from 1000° to 1100° in ice-cold water, he has obtained a mixture of A and B in about equal proportions. This mixture is comparatively very feebly magnetic. The constituents both contain carbon, and exist side by side in separate polyhedra. The author concludes that B is the allotropic form of iron (denoted γ), which is especially stable above 860°, and is present to the exclusion of the other forms of iron in steels containing 25 per cent. of nickel, or 12 to 13 per cent. of manganese, these steels being non-magnetic. The hard constituent A would then be the allotropic form β. The research will be hailed by the allotropists as affording most important evidence in favour of their theory.

A PAPER on the temperature variation of the thermal conductivities of marble and slate is contributed to the *American Journal of Science* by B. O. Peirce and R. W. Willson. The net result of their investigation is that such a temperature variation does not exist; in other words, that marble and slate conduct heat equally well at all temperatures. This result is of some importance to the physics of the earth's crust, and the manner in which it was arrived at displays some ingenuity. Two faces of a slab of marble or slate were kept at different temperatures, and the fall of temperature between one surface and the other was determined by means of thermopiles. Now it is notoriously difficult to determine the temperature accurately at a certain point, and borings lead to errors in estimated depth. So the expedient was adopted of slicing the slab into a series of layers pressed together, between every two of which a thermo-couple was introduced. The interstices were only a few tenths of a millimetre, and experiments with different intervals proved that the error in the temperatures observed did not exceed one or two degrees Centigrade. On plotting the temperatures and distances, the temperature was found to have fallen uniformly throughout the slab. If the conductivity had been higher at higher temperatures, the fall on the hot side would have been more decided. The temperatures ranged from 350° C. to zero.

ATTENTION may be drawn to a translation, in handy form, of Prof. Hering's well-known addresses on "Memory as a General Function of Organised Matter," and on "The Specific Energies of the Nervous System." The little volume is issued by the Open Court Publishing Company of Chicago, as part of the Religion of Science Library, at a price of 15 cents.

A CONCISE account of the work of Priestley, Scheele, Cavendish, and Lavoisier, in connection with "The Discovery of Oxygen, and its Immediate Results," which appeared recently in the *Pharmaceutical Journal*, has been reprinted and published in the form of a pamphlet, obtainable at the office of our contemporary.

A "MUSEUM REPORT," containing a descriptive list of the donations to the Museum and Herbarium of the Pharmaceutical Society during 1893–94, has been prepared by Mr. E. M. Holmes, Curator of the Museum, and published by the Society. The catalogue contains interesting notes concerning the more important donations, and the complete list of donations to the Herbarium, in which the plants and plant products are arranged in alphabetical order, should prove of practical value.

THREE new volumes have lately appeared in the comprehensive Aide-Mémoire series published jointly by MM. Gauthier-Villars and G. Masson, Paris. One is "Applications Scientifiques de la Photographie," by M. G. H. Niewenglowski. We have long expected the publication of a book on this subject, but the one now before us is satisfactory. No account is given of the remarkable results obtained in astronomical photography, and the photography of solar and stellar spectra is only mentioned in a few words. In fact, the author has not treated his subject in the broad scientific manner it deserves, and there is yet room for some one to write a really good treatise on it. The volume by M. X. Rocques entitled "Analyse des Alcools et des Eaux-de-vie," just published in the same series, should prove of service to analytical chemists; while the third of those lately received—"La Topographie," by Lieut.-Colonel P. Moëssard, is an excellent manual on maps and their construction, containing concise descriptions of triangulation, levelling, and cartography.

THE last number of the *Bollettino* of the Italian Seismological Society contains an interesting paper by Dr. G. Agamennone, on the earthquake of Paramythia (Epirus) during the night of May 13–14, 1895. The official accounts state that 269 houses were destroyed, and 262 more were rendered uninhabitable; that seventy persons were killed and fifty wounded; but Dr. Agamennone believes that these figures refer only to the district of Paramythia. The area including all the villages where houses were damaged is elliptical in form and contains about 400 sq. km., the longer axis being directed N.N.E. and S.S.W. The centre of this area lies near the villages of Dragani and Carvounari, where the intensity of the shock was between ix. and x. of the Rossi-Forel scale. In this district, the ground was fissured in several places. The shock was felt, though slightly, at Zante, which is 180 km. from the epicentre; and the pulsations were registered by several of the Italian seismographs, and also by the horizontal pendulum at Nicolaiew, about 1250 km. from Paramythia.

THE additions to the Zoological Society's Gardens during the past fortnight include a Burchell's Zebra (*Equus burchelli*, ♀) from South Africa, presented by the Hon. W. Rothschild; a Common Badger (*Meles taxus*), British, presented by Mr. Thomas B. Place; a Rough Fox (*Canis rudis*) from Demerara, presented by Captain J. Ernst; a Blue and Yellow Macaw (*Ara ararauna*) from Brazil, presented by Mrs. Alec. Tweedie; two Hoary Snakes (*Coronella cana*), two Puff Adders (*Vipera arietans*) from South Africa, presented by Mr. J. E. Matcham; an Anamolous Snake (*Coronella anamola*) from Brazil, presented by Mr. Frank Summers; a Nilotic Monitor (*Varanus niloticus*) from North Africa, deposited; two Ornamental Lorikeets (*Trichoglossus ornatus*) from Moluccas, two Forsten's Lorikeets (*Trichoglossus forsteni*) from Sumbawa, purchased; a Cactus Conure (*Conurus cactorum*) from Brazil, received in exchange; a Southern River-Hog (*Potamochærus africanus*) from East Africa, presented by Mr. Henry M. C. Festing; a Golden Eagle (*Aquila chrysaëtus*) from Scotland, presented by Mr. Osgood H. Mackenzie; a White-crowned Mangabey (*Cercocebus æthiops*, ♂), a Green Monkey (*Cercopithecus callitrichus*, ♂) from West Africa, deposited; two Red-sided Tits (*Parus varius*) from Japan, purchased.

OUR ASTRONOMICAL COLUMN.

ROTATION OF JUPITER.—Most of the determinations of the rotation period of Jupiter have been made by observations of surface markings between latitudes 45° N. and 35° S., and little has been known as to the conditions of rotation near the poles. This is due to the fact that conspicuous and sufficiently definite spots are chiefly confined to the equatorial regions of the planet, and partly to the unfavourable conditions under which the poles are presented to us. Some important observations, however, bearing on the rotation in high latitudes, have been secured by Mr. Stanley Williams with the aid of a 6½-inch Calver reflector (*Ast. Nach.*, 3325). On October 10, 1892, a short dusky streak, almost oblong in appearance, was observed quite close to the north limb of Jupiter, and reaching at least as far as 85° N. Other streaks of similar appearance were subsequently observed, and frequent observations of the times of mid-transit were made. Confirmation of the results has been obtained by an examination of several photographs of the planet taken at the Lick Observatory about the same period, the markings being sufficiently distinct for measurement. Generally speaking, the visual agree very closely with the photographic results, the mean rotation period derived by the two processes only differing by about two seconds. The mean result for the rotation period of the surface material of Jupiter, between north latitudes 40° and 85°, is 9h. 55m. 38·9s. ± 1·20s., this being the length of a sidereal rotation expressed in mean solar time. The following statement illustrates the degree of accuracy obtained :—

	h.	m.	s.		s.	
Spot *a* ...	9	55	33·7	±	1·32	(77 rotations)
Spot *b* ...	9	55	43·8	±	2·25	(41 ,,)
Spot *c* ...	9	55	39·7	±	0·60	(252 ,,

Mr. Denning's value for a spot in latitude 35° N., namely, 9h. 55m. 39s., agrees very closely with the foregoing, and differs by only a few seconds from the period deduced from observations of the red spot.

Supplementary details, which are given by Mr. Williams, indicate that the positions of markings on Jupiter may be determined with quite as much accuracy from photographs as by the best micrometrical measurements in the telescope.

THE PARALLAX OF α CENTAURI.—As part of a discussion of the meridian observations of α Centauri, made at the Cape Observatory during the years 1879–1881, Mr. A. W. Roberts has deduced a new value for the parallax of this interesting system (*Ast. Nach.*, 3324). Mean places for the two components have been computed by applying corrections for proper motion and orbital motion, and assuming a parallax of 0″·75. The errors of a systematic nature, which cannot be certainly accounted for, are believed to be due to an erroneous value of the refraction depending upon the temperature. Disregarding these, and adopting the aberration constant determined by Chandler, 20″·50, the parallax of α Centauri, from declination measures alone, was found to be 0″·81 ± 0″·05. From the right ascension measures alone, the value 0″·66 was calculated. Solving for both coordinates, the resulting value for parallax is 0″·71 ± 0″·05. This corresponds to a little over 4½ light-years and shows a marked agreement with the parallax 0″·75 found by Drs. Gill and Elkin in 1882 from measures made with the heliometer.

NEW VARIABLE STAR OF THE ALGOL TYPE.—*Harvard Observatory Circular*, No. 3, announces that the star B.D. + 17°·4367, magnitude 9·1, and approximate position for 1900, R.A. 20° 33′·1, Decl. + 17° 56′, is a variable of the Algol type. The change in brightness appears to be rapid, and the range of variation to exceed two magnitudes.

THE NATURE OF THE PHYSIOLOGICAL ELEMENT IN EMOTION.

PROF. A. C. WRIGHT contributes an interesting paper "on the nature of the physiological element in emotion" to *Brain* (parts 70 and 71), the object of which is to apply the results obtained by Gaskell's observations on somatic and splanchnic nerves to the study of the emotions. Prof. Wright begins his paper by taking as an example the phenomena observed in a kitten confronted with a strange dog, and shows that such an emotional stimulus would call forth in the kitten a regular series of reflex responses : first of all, through the involuntary visceral efferent nerves ; then the semi-involuntary muscles, such as those of the face, would be called into action ; and, lastly, there would be reflex response of certain parts of the voluntary muscular system. The essential features to be recognised in this example and in every emotional reaction are—the origination of the emotion in a violent sensory stimulus, a condition of extreme neural tension in the reflex centre, and an overflow of neural energy into different paths. This overflow takes place first into channels associated with involuntary muscle, then into those associated with semi-voluntary muscle, and lastly into those associated with voluntary muscle. The physiological essence of the emotion is to be found not in the visceral reflex actions, but in the high neural tension of the reflex centre which gives rise to these actions. In childhood sensory stimuli call forth in each case responses both of involuntary and voluntary muscle, while with increasing age the outflow of neural energy from the reflex centre becomes more and more restricted to paths associated with voluntary muscle. As a result of such transformation we get purposive voluntary action. The author notices the *à priori* necessity for some system of control of the reflexes, since "if each minimal stimulus were to evoke a separate reflex movement in an organism which was endowed with a sensitiveness at all approaching that of the human organism, life would be a mere chaos of muscular movement." Voluntary muscles react to the slightest stimuli, but involuntary muscular actions are only called out by intense stimuli, or by a summation of slighter ones. High neural tension in the reflex centre is therefore necessary for these reactions of involuntary muscles, and all such high neural tension is attended with a sense of distress. The replacing of the "generalised somatico-visceral reflexes of inexperience and childhood by the specialised purposive reflexes of experience and adult life" . . . "is not so much a question of substituting one variety of reflex for another, as it is a question of substituting a condition of low neural tension for a condition of high neural tension."

PHOTOGRAPHY AND CHRONOGRAPHIC MEASUREMENTS.[1]

IN chronographic measurements in physiological experiments, photography has been in constant use for several years, and the methods are well known. I have extended recently the method of what may be called photographic chronography to measuring the velocity of projectiles. On former occasions I have shown that to obtain the best chronographic results, magnetic and solenoidal arrangements should be avoided, since by their use a time lag is introduced. The following chronographic method depends entirely on light. Two sources of light at a suitable distance apart throw two beams of light on to a sensitive plate, carried on the carriage of a tram chronograph. By means of lenses, the beams of light are caused to form two sharp images on the plate in a vertical line, one above the other ; a tuning-fork trace is also made on the plate ; if the plate traverses, when the beams of light are not interrupted, on development, two black parallel lines appear on the plate ; but if, during the passage of the plate, the beams of light are cut by any solid object which shuts off the light, then on development two gaps are seen to exist. The distance between these markings when interpreted in terms of the fork trace, give the velocity of the object which cuts through the beam of light. The method was illustrated by allowing a projectile to pass through the focus where the convergent beams of light from two sources of light cross.

Another method was also shown in which the projectile cut through two thin screens placed in the paths of the beams of light, and so opened a passage for the light. In this case two parallel lines are found on the plate, one longer than the other ; the difference of their lengths, when duly interpreted, gives the velocity of the projectile ; when the distance between the screens is considerable, the beams of light have to be reflected on to the chronograph by mirrors. FREDERICK J. SMITH.

[1] A Note on a Lecture given at Oxford, October Term, 1895.

ATMOSPHERIC ELECTRICITY.[1]

IT is hardly possible to imagine that the first experimenter who obtained an electric spark sufficiently strong to produce a sensible sound should not at once have been struck by the fact that he was in the presence of thunder and lightning on a small scale. We find, indeed, in various writings from the early days of electrical machines a number of suggestions that the thunderstorm is an electrical phenomenon; but to Benjamin Franklin belongs the merit of having perceived that a direct experiment was needed to prove what so far was only a guess. In an article entitled "Opinions and Conjectures concerning the Properties and Effects of the Electrical Matter arising from Experiments and Observations made at Philadelphia, 1749," the following passage occurs:—

"To determine the question whether the clouds that contain lightning are electrified or not, I would propose an experiment to be tried where it can be done conveniently. On the top of some high tower or steeple place a kind of sentry-box, big enough to contain a man and an electrical stand. From the middle of the stand let an iron rod rise and pass, bending out of the door, and then upright 20 feet or 30 feet, pointed very sharp at the end. If the electrical stand be kept clean and dry, a man standing on it, when such clouds are passing low, might be electrified and afford sparks, the rod drawing fire to him from a cloud.

"If any danger to the man should be apprehended (though I think there would be none), let him stand on the floor of his box, and now and then bring near to the rod the loop of a wire that has one end fastened to the leads, he holding it by a wax handle; so the sparks, if the rod is electrified, will strike from the rod to the wire, and not affect him."[2]

The experiment suggested by Franklin was successfully performed in Marly (France), by D'Alibard, on May 10, 1752,[3] in London by Canton, in Spital-square, on July 20, 1752, and by Wilson, in Chelmsford, Essex, on August 12 of the same year. Franklin himself describes having used a kite in Philadelphia in a letter dated October 19, without giving the date of his observations. But this must be supplied in some passage which I have not been able to find, for Rosenberger ("Geschichte der Physik," vol. ii. p. 316) mentions that it was done in June.

Franklin's disbelief in the dangerous character of the experiment must have received a severe shock when he heard of the death of G. W. Richmann, who, in the year 1753, was killed by an electric discharge drawn from the clouds by means of a kite.

The thunderstorm is the most impressive effect of atmospheric electricity, though it is rivalled in beauty by the aurora, and in interest by the many phenomena of daily occurrence, which are only made perceptible to us by proper instruments. In a lecture delivered before this Institution on May 18, 1860, Lord Kelvin described the delicate electrical appliances constructed by him for the more accurate observation of atmospheric electricity. The problems then for the first time clearly stated, gave a powerful and still lasting impulse to the investigation of atmospheric electricity, and though no decisive answer can be given to all the questions raised in that lecture, recent researches have brought us somewhat nearer to their solution.

Observations which may be made every day and at every place have shown that the earth is electrified, whatever the weather may be. In the language of the older theories, which we cannot as yet altogether abandon, we say that the earth is covered with negative electricity, or, in modern phraseology, we express the same idea by the statement that we move about in an electrified field, that electric lines of force stretch through the air from the ground, from our bodies, and from everything which is exposed to the sky overhead. The strength of this electric field is not at all insignificant. If we wish to produce it artificially between two parallel plates kept at a distance of one foot, we should have to apply an electromotive-force sufficient—and sometimes more than sufficient—to light up the incandescent lamps which illuminate our dwellings. The electric force is comparatively weak in our country, but 50 volts per foot are constantly observed, and 100 volts are not uncommon; but in dryer climates the amount of the force may be considerably in excess of these figures.

[1] Discourse delivered before the Royal Institution of Great Britain, by Prof. Arthur Schuster, F.R.S.

[2] "Experiments and Observations on Electricity made at Philadelphia, in America," by Benjamin Franklin, LL.D. and F.R.S. (London, printed for David and Henry, and sold by Francis Newbery, 1769, p. 66.)

[3] *Ibid.*, p. 107.

If we fix our minds on the lines of force starting from the surface of the earth, we are at once led to ask, Where is their other end? Do they curve round and back again to earth? Do they end in the dust which everywhere surrounds us, or do they reach up to the clouds? Do they pass through the clouds and end where invisible particles separate the sunset red from the midday blue? Or, finally, do they leave the earth altogether, and form intangible bonds between us and the sun, the stars, the infinity of space? These are not idle questions, and we cannot be said to have solved our problem unless some definite answer is given to them. The last-mentioned view, propounded originally by Peltier, and latterly supported by Exner, is the simplest. If we could allow that the earth, once electrified negatively, could remain electrified for ever, the corresponding positive electrification being outside our atmosphere altogether, the chief difficulty of atmospheric electricity would be removed, and the normal fall of potential at the surface would be explained by the permanent negative electrification of the surface.

Unfortunately this view, to be tenable, has to assume that the atmosphere is a complete non-conductor to the normal electric stress, and this is known not to be the case. We know of several causes which break down the insulating properties of air. If two pith balls are electrified and repel each other, and a match be lit in their neighbourhood, the pith balls come together, showing that they have lost their charge, and consequently that the flame of the match has destroyed the insulating power of air. It is not only the flame itself which conducts, but also the gases rising from the flame.[1] The following experiment will prove this. In Fig. 1, A represents a metallic tube bent round at the upper end, and containing at its lower end a Bunsen burner in metallic contact with the tube, which is also connected to an electroscope. The tripod, T, which supports the tube, is insulated by blocks of paraffin. A Leyden jar, L, on a separate support, is placed so that the knob stands at about the level of the upper part of the tube, which acts as chimney to the flame. The knob of the jar may be a few inches away from the opening of the chimney, and not necessarily in a line with it. The experiment succeeds, although the gases rising from the burner may not come into contact with any part of the jar. The jar is charged, and care must be taken that no fibres of dust attach themselves either to the jar or chimney. I have found it convenient to join a piece of amalgamated zinc to the end of the chimney. Under these circumstances the charge of the jar will be found to leak across to the tube, and the leaves of it will diverge. If, as in Exner's form of electroscope, the leaves, on reaching a certain divergence, discharge by forming a

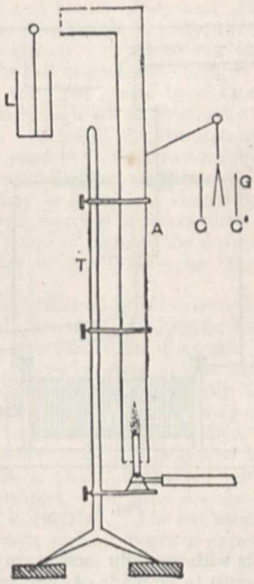

FIG. 1.

[1] The most complete investigation of the conduction of gases rising from flames is contained in a series of papers by Giese (*Wiedemann's Annalen*, vol. xvii.).

contact with earth-connected plates, C C′, the charging and discharging can be watched for a long time. It will be noticed that the flame, being altogether surrounded by a tube of the same potential, cannot be active in this case, but the conductivity must be due to the gas as it escapes from the chimney.

It follows from these experiments that every fire burnt on the surface of the earth, and every chimney through which products of combustion pass, act like very effective lightning conductors, and would consequently discharge, slowly but surely, any electrification of the surface of the earth. The peculiar immunity of factory chimneys against damage by lightning appears from statistics collected by Hellmann in Schleswig-Holstein,[1] for while 6·3 churches per thousand were struck, and 8·5 windmills, the number per thousand of factory chimneys was only 0·3.

Franklin was acquainted with the action of flames; he also discovered that no charge can be given to a red-hot iron ball, a fact which seems to have been forgotten until re-discovered in our own times by Guthrie. Franklin also tried the action of sunlight, but obtained no result. Had he performed the experiment with carefully-cleaned zinc, he would have anticipated one of the most striking of Hertz's discoveries. We now know that a negatively-charged surface will discharge into air when illuminated by strong violet light, and sunlight will be sufficient with specially sensitive materials. This action has been investigated in detail by Elster and Geitel, who have not, however, succeeded

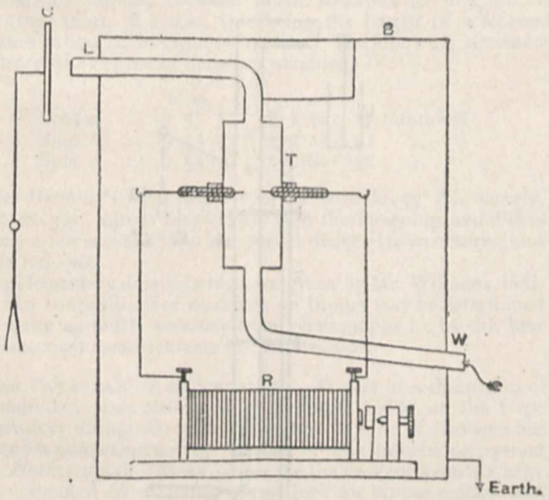

FIG. 2.

in obtaining results with sunlight acting on such bodies as we know the earth's crust to be made of. So far, then, we have no experimental evidence to include light as an active agent in the phenomenon of atmospheric electricity.

We possess in the electric discharge itself a very powerful, and probably very generally active means of breaking down the insulating power of air. Some of the experiments (*Proc. Roy. Soc.*, vol. xlii.) which I described some years ago to prove this, were objected to on the ground that it might not be the discharge itself, but the ultra-violet light sent out by the luminosity of the discharge, which was active. The following form of the experiment conclusively shows that the discharge acts independently of light.

In Fig. 2, R represents a Rhumkorff coil entirely surrounded by a metallic box, B, which is connected to earth. The terminals of the coil lead to two electrodes inside a metallic tube, T, which is also kept at zero potential. This tube is arranged so that a current of air can be blown through it. The air, on escaping through the tube, is made either to impinge on or to pass near a metallic plate connected to a charged electroscope. Under these circumstances, the electroscope is not discharged either by a current of air alone, or by the coil alone. But as soon as the air is blown through the apparatus while the sparks are passing and then made to impinge on the plate C, the electroscope is instantaneously discharged. The experiment succeeds when a plug of cotton-wool is inserted at W, to stop the action of the dust; but a plug of cotton-wool at the other end diminishes the action so much, that I am doubtful whether the effect then really exists there. I am, so far, not inclined to believe that the action is due to dust, but rather that the cotton-wool acts in increasing very considerably the interval which elapses between the time at which the spark acts and the time at which the sparked air passes out of the tube. The effect may be observed even though the tube L is lengthened by an addition of another piece 3 feet or 4 feet long.

Several phenomena, one of which had been known for a long time, can be explained by the fact that the electric discharge changes the condition of the gas into a state similar to that of gases rising from flames. It is mentioned, for instance, by Faraday that electric sparks are liable to succeed each other along the same path, and it is known that the same holds for lightning flashes, facts which themselves point to a higher conductivity of air along the path of the previous discharge. A curious instance of a similar effect is afforded by lightning conductors, which are sometimes put up to protect overhead leads used for conveying a high tension current. Owing to the obvious impossibility of connecting the leads directly to earth, a small air gap is interposed, the idea being that the air gap will act as an insulator for the current the leads are intended to carry, but that if during a thunderstorm the potential rises sufficiently high to be dangerous, equalisation may take place through the air gap to earth by means of a small spark. So far, the air gap answers its purpose, but as soon as a spark passes through the gap, it destroys the insulating power of the air, and the main current consequently takes a short-cut through the gap. At Pontresina, in the Engadine, lightning conductors put up in this way are so sensitive that a flash of lightning several miles away causes a small spark by induction, and instantaneously puts out every electric lamp in the town.

If we accept the view that an electric discharge destroys the insulating power of the gas, it follows that the outer regions of the atmosphere must conduct, for we have ample reason to suppose that electric currents are passing continuously through those regions. The aurora borealis in the arctic regions is, according to Nordenskiöld's observations, a permanent phenomenon, and the diurnal changes of terrestrial magnetism show that in our latitudes electric currents traverse the air above us. However small a conductivity we may assign to the atmosphere, the earth could not remain electrified inside such a shell of partially conducting gases. Lord Kelvin drew the same conclusion in the Royal Institution lecture, on the assumption that gases at much reduced pressures cease to insulate. We may leave it an open question whether the normal electric stress could in itself cause a discharge in the outer regions; but we cannot deny that under existing conditions these regions do not insulate, and Lord Kelvin's argument still holds good.

But the question of the ending of the lines of force—in other words, the location of the positive charge corresponding to the negative electrification of the surface of the earth—can only be solved by balloon or kite experiment, and we may briefly mention the more important results which have so far been obtained.

Observations made up to heights of about 1000 feet seem to indicate a strengthening of the electric field—*i.e.* the fall of potential per metre is greater at a height of, say, 200 metres than on the surface of the earth. The observations of Dr. Leonhard Weber (*Elektrotechnische Zeitschrift*, April 1888) bring out this point clearly. In one case the fall of potential at a height of 350 metres was found to be six times that at the earth's level. This increase is in itself not surprising, if we remember that every particle of dust raised from the ground must itself be negatively electrified, and probably the observed increase in the electric force is sufficiently accounted for by the presence of electrified dust.

Observations made at greater heights in balloons, on the other hand, seem clearly to indicate that this increase soon ceases, and that a diminution already takes place at moderate heights. Thus the observations of Dr. O. Baschin (*Meteorologische Zeitschrift*, September 1894) gave for the fall of potential in volts per metre the numbers 49, 28, 13 at heights of 760, 2400, 2800 metres respectively, and at a height of 3000 metres no measurable fall at all could be obtained. These observations were made in clear weather. The balloon afterwards passed over a layer of clouds, and strong electric effects were noticed. Similar observations had been previously made by

[1] "Veroffentl. des Kgl. Preuss. Stat. Bureaus," 1886, p. 177, quoted by Bebber, "Meteorologie," p. 245.

others (Andrée, Le Cadet, and Bornstein), and though the subject is by no means exhausted, we may take it as provisionally established that the lines of force of the normal electric field of the earth end within the first 10,000 feet or 15,000 feet. This result is of great importance, for it shows that in fine weather there must be a layer of positively electrified air permanently above us. Currents of air in this layer must affect the field as we observe it, and possibly the daily period may be due to changes in the currents of air at a moderate height. A fact discovered by Exner is of importance in connection with this subject. Observing at three different places (in a field close to Vienna; in St. Gilgen, on the Wolfgangsee; and on the hills near Venice), he found that whenever there was a strong south wind, with a clear sky, the normal electric force was always increased, and sometimes considerably (*Wiener Akad. Sitzungsberichte*, vol. xcvi., 1887).

The daily changes show, with few exceptions, a remarkable uniformity at different places. There are in general two maxima of potential—one at 8 or 9 o'clock in the morning, and one in the evening. The evening maximum is the most marked, while at some places, and especially near towns, the morning maximum disappears. The same general features of the daily variation have been found to hold at a number of European stations, at Cape Horn, Melbourne, and in the Northern Arctic regions. If the variation is separated into two—one having a period of 24 hours, and the other of 12 hours—the latter is found to agree in phase at widely different places on the earth's surface, while the former is found to vary to a much greater extent, and hence to be probably more affected by local circumstances. The remarkable researches of Hann have given a similar result for the diurnal variations of the barometer, and we may reasonably conclude that the semidiurnal variation of atmospheric electricity is connected with the same circulation in the upper regions of the atmosphere which shows itself in the corresponding changes of pressure.

In addition to the more regular periodic changes, the electric stress observed in fine weather shows marked differences on different days and at different seasons. With respect to these, the researches of Prof. Franz Exner (*ibid.*) have led to the important result that there is a close connection, direct or indirect, between the amount of aqueous vapour present in the atmosphere and the fall of potential observed at the surface of the earth. If p_0 be the pressure of aqueous vapour present in centimetres, Exner deduces the equation for electric force P

$$P = \frac{A}{1 + kp_0},$$

where $A = 1300$, $k = 13{\cdot}1$.

The formula agrees very well with observations in which the vapour pressure varied between 0·23 and 0·95, and it is especially to be remarked that it is the amount of vapour and not the humidity which determines the electric force. Observations made by Mr. E. Drory during a journey round the world fit in very well with Exner's formula, and observations made at such widely different places as Suez, Albany, Sydney, Colombo, and Penang showed a fall of potential practically identical with that calculated from the above formula, though the same constants were taken and the vapour pressure varied between 0·8 cm. and 2·2 cm.

Messrs. Julius Elster and Hans Geitel (*Wiener Akad. Sitzungsberichte*, vol. ci., 1892) have followed up this research. Their investigations have shown a satisfactory agreement with Exner's formula, if the mean values of a number of observations in which the vapour pressure is approximately the same, is considered. But individual numbers differ very widely from the mean, so that the formula cannot be used to predict the normal fall of potential on any particular day. There is, perhaps, nothing surprising in the great divergence of such individual results if it is considered that we only observe the moisture near the surface of the earth, but are ignorant of the total amount of water in the column of air over the district in which the observations are carried out. The same authors have shown that an equally good agreement can be obtained if, instead of the amount of aqueous vapour, we take the intensity of active radiation as the determining circumstance. The light might be supposed to act on the general surface of the earth, as it does according to Hallwachs' observations on a metallic body, dissipating a regular charge. There are some difficulties in the way of this explanation, the most serious being the absence of experimental evidence that sunlight actually does act in the manner indicated on any substance forming part of the earth's surface. It is impossible at the present time to enter more fully into this subject, but attention must be drawn to the very important indirect result, that there seems to be a connection between ultra-violet radiations and the amount of aqueous vapour present in the air.

The phenomena of atmospheric electricity have been studied at the mountain observatory established on the "Sonnblick," in Salzburg, at a height of 3100 metres.

The important result has been established that the electric force is singularly constant. The great differences observed at low level between the electric field in summer and winter, or on dry and wet days, seems to be completely absent, and these facts tend to support the conclusion derived from balloon observation, that the positive ends of the lines of force are situated at a height of something like 10,000 feet.

Brief allusion must be made to some of the causes which alter to a marked extent the normal fall of potential. As the surface of the earth is negatively electrified, it follows that dust carried up by the wind must be electrified, and it is found, indeed, that in violent dust storms the laws of force near the surfaces are altogether distorted and reversed in direction. Werner Siemens (*Pogg. Ann.* cix., 1860; *Meteorologische Zeitschrift*, 1890, p. 252) could, while standing on the top of one of the pyramids during a strong wind, charge an improvised Leyden jar sufficiently to obtain strong sparks. A casual observation of Elster and Geitel (*Ziele und Methoden*, p. 11) may prove significant. On March 7, 1889, the temperature in Wolfenbüttel was rising from − 10° C. to + 2° C., a cirrus layer covering the sky. The fall of potential changed in the course of four hours from 1302 volts per metre to − 1200 volts, that is, from a very exceptionally high fall to an equally strong gradient in the other direction. Although the atmospheric circumstances were anomalous, they seem in themselves not sufficient to account for the anomalous electrical effects, and the authors suggest that a possible explanation may be found in a violent dust storm which on the previous day was observed in Alexandria.

Fogs are generally found to increase the normal fall considerably, so that the drops of water must be taken as positively electrified.

Waterfalls considerably disturb the electric condition of the air in their neighbourhood, the air surrounding the fall being charged negatively, sometimes to considerable distances.

Whether clouds in themselves are always electrified is very doubtful; they no doubt disturb and generally weaken the fall of potential at the earth's surface, but this may only be due to a displacement of the positively-electrified layer which balloon observations have shown to exist at a height of from 10,000 feet to 20,000 feet. While a cloud discharges rain, the electrical effects in the neighbourhood of the place are the same as that in the neighbourhood of a waterfall. The explanation is probably the same in the two cases, and by means of experiments, alluded to further on, we may reproduce the negative electrification of air under similar circumstances.

Measurements of the electrification of falling rain or snow, simple as they appear at first sight, are beset with very serious difficulty. We owe the most complete investigation on the point to Messrs. Elster and Geitel (*Wiener Sitzungsberichte*, vol. xcix., 1890). They find no regularity in the electrification, though positive signs slightly preponderate with snow and negative signs with rain.

The approach of a thunderstorm announces itself by characteristic cumuli clouds, and the general atmospheric condition favourable to their formation is felt by many persons of nervous temperament. Many of us are accustomed to hear that "there is thunder in the air." Whatever the special feeling of "thunder" may be due to, it cannot be an electrical effect, for electrical instruments delicate enough to detect a small fraction of the normal force, give no indications of the approach of a thunderstorm, and it is only when the cloud has begun to discharge rain or hail that strong electrical effects are noticed. During the thunderstorm the electroscope is, of course, much disturbed, and there are frequent and violent reversals of its indications.[1] The fact that no effects are observed at the surface of the earth during the approach of a thunder-cloud does not prove that there is no electrical separation, for we may imagine two oppositely electric layers at different levels producing a strong

[1] Weber, *Elektrotechnische Zeitschrift*, vol. x.; Elster and Geitel, "Ueber einige Ziele und Methoden Luftelektrischer Untersuchungen," Wolfenbüttel, 1891.

electric field between them, but only weak effects outside. That some such things may possibly occur is indicated by observations made in mountain districts, where violent electrical disturbances are observed previous to the formation of clouds (Trabert, *Meteorologische Zeitschrift*, 1889, p. 342). The cumulus cloud, from which the lightning strikes out, is nearly always associated with a cirrus layer above it, and the flash occurs more frequently upwards or sidewards between the clouds than down to earth. Under such circumstances it is clear that instruments on the surface of the earth can only very partially indicate the nature and distribution of electrical stress in the neighbourhood of the cloud.

Thunderstorms seem always to be connected with a vortex motion, and meteorologists distinguish two kinds of thunder-storms. The first kind forms in the outlying portions of a large cyclonic system. The storms which occur in winter are mostly of this nature, and the vortex necessary for its formation is of the nature of a secondary disturbance. The thunderstorm which forms in summer, on the other hand, makes its own vortex, and is of a much more local character than that which is produced round a previously established barometric depression. The summer storm is much influenced by the character of a district. There are certain configurations apparently favourable to its formation, as is clearly brought out by the charts which have been made representing their frequency.

The route travelled over by the storm is affected by mountain ridges, and rivers also seem to offer a peculiar impediment. Many of them are brought to an end either along their whole front, or only part of it, when they reach the banks of a large river.[1]

Some curious problems are presented by the detailed structure of lightning flashes. Although these lie outside the range of the present lecture, reference must be made to the very beautiful photographs of lightning flashes taken both in this country and abroad. The ordinary forms which lightning takes are familiar to all; but a good deal of mystery still surrounds the so-called globular lightning. The manner in which this form appears is best described in the words of eye-witnesses.

Dr. A. Wartmann gives to the Physical Society of Geneva the following account of what he saw:[2]—"At half-past six o'clock in the evening I drove from Versoix to Genthoud. On the Malagny road I heard the coachman say he did not know where he was. His eyes were so much fatigued by the frequent and intense lightning discharges, that he was blinded, and could not, even in the intervals, see the road, in spite of the good lanterns alongside. I stepped on to the box and took the reins. We had barely passed the principal gate of the grounds of Dr. Marcet, when I became conscious of a bright and lasting luminosity behind me. Thinking it was a fire, I turned round, and saw, at a distance of, roughly, 300 metres, a ball of fire of about 40 cm. diameter. It travelled in our direction with a velocity about equal to that of a bird of prey, and left no luminous trail behind. Just as the ball had overtaken us, about 24 metres to our right, it burst with a terrific noise, and it seemed to me as if lines of fire started from it. We felt a violent shaking, and remained blinded a few seconds. As soon as I regained power of distinguishing objects, I saw that the horses had turned at a right angle to the carriage, with their chests in the hedge, with drooping ears and all signs of great terror. I returned on the following day to the place where I had seen the ball explode, but could find no sign of any damage. At a distance of 100 metres I found that a group of three trees, bordering a wood, had their upper branches singed, but it is not possible to say whether this was due to the discharge which I had seen."

The following is a translation of an account given by Mr. H. W. Roth (*Meteorologische Zeitschrift*, 1889, p. 231):—"During the thunderstorm of May 19, 1888, at about 6 p.m., a flash of lightning took effect which seems to me remarkable from a physiological point of view. The dealer Werner, from Ellerbruch, and his son (sixteen years old), with a one-horse conveyance containing rags, were on the road which leads from here to the village of Ottensen, about three miles away in a south-westerly direction. The father had been left a little behind, and the son was occupied in giving bread to the horses, when he found himself suddenly surrounded by light, and noticed a fiery ball, about the size of his fist, moving towards him along the back of the horse. Then he lost consciousness. He felt no concussion. The father, on approaching, saw the horse's limbs still contracting, and at first he thought his son was dead, but succeeded, after considerable efforts, in bringing him back to life in about three-quarters of an hour. The horse was dead."

Some curious statistics have been collected, especially in Germany, as to the damage done by lightning flashes. That damage seems to have increased to an enormous extent within the last fifty years, and although in cases of this kind statistics may easily be at fault, there seems no doubt about the reality of the fact, which may find an explanation in the partial cutting down of forests in those parts where thunderstorms chiefly occur. When lightning strikes into forests, it selects certain trees by preference. Thus, in the principality of Lippe, taking the percentage of beeches struck by lightning as unity, that for other trees is as follows:—Oak 48, spruce fir 5, Scotch fir 33.

The St. Elmo's fire, a continuous discharge from points and sharp angles, is often observed on board ship and in mountain districts during a storm. Its appearance was considered a sign of the approaching end of the lightning, and was looked upon with favour by the ancient sailors in the Mediterranean Sea, who gave to it the name of Castor and Pollux. There was another appearance called Helena, a bad omen, which by many is believed to have been another form of the St. Elmo's fire, and the present name has been stated to be a corruption of the word Helena. Some support is given to this view by the fact that the Emperor Constantine built a castle in the Pyrenees, which he named after his mother, Helena, and this castle seems to be referred to occasionally as St. Elne or St. Elme. But it is much more probable, as argued by Dr. F. Piper (*Pogg. Ann.*, vol. lxxxii. p. 317), that the word is derived from St. Erasmo, a bishop who came from Antiochia, and suffered a martyr's death at the beginning of the fourth century. He seems to have been specially considered the patron of Italian sailors. Churches and castles in Naples and Malta were called St. Erasmo and St. Ermo, and Ariosto describes St. Elmo's fires as St. Ermo's fires. The electric discharge which goes under this name has a different appearance according as it is the positive or negative electricity which escapes, and both kinds occur with about equal frequency.

Although we have not yet arrived at any satisfactory theory of atmospheric electricity, some progress has been made, and this account would not be complete without a short account of the views taken by men of science on the subject. The number of theories proposed is very considerable. Dr. Suchsland,[1] in a pamphlet published in 1886, gives an account of twenty-four, to which he adds one—his own. The year 1884 alone has given birth to four theories.

We may group the theories according to the origin they assign to the source of energy which is involved in the formation of the electric field. All the work we can perform is either derived from the sun or from the earth's rotation. There is, as far as I know, only one theory—that of Edlund—which makes the earth's rotation in space responsible for the separation of electricities in the atmosphere. But Edlund's views are not tenable in theory, and, even granting his deductions, the normal fall of potential should, according to the views of the author, have a different sign in the polar and equatorial regions, which is contrary to the observed fact. This theory does not, however, exhaust the possibility of explaining atmospheric electricity as a phenomenon of electromagnetic induction, and it is not disproved that in some form or other the rotation of the earth's magnetic field may play a part in the origin of the electric field. The theories which take solar radiation as the source of the energy divide themselves into several groups. We may think of a direct thermo-electric or actinic action, but there is, so far, no experimental support to such views. One of the earliest and most natural suppositions is the belief in evaporation as a source of electrification. This was Volta's theory, and experiments have at various times been produced in its support; but, so far, no one has been able to invalidate Faraday's conclusion that whenever electrification seemed to appear as a consequence of evaporation, it was really due to secondary causes, such as the friction of the liquid spray against the sides of the containing vessel. Rejecting Volta's theory, there is nothing left but the belief in some form of contact or frictional electricity either between drops of water and air, or water and ice, or any two of

[1] Bebber, "Meteorologie," p. 255; Bornstein, *Archiv der Seewart*, viii., 1885.

[2] *Arch. des Sci. Phys. et Nat.* (3) vol. xxi., 1889. The above account is translated from the *Meteorologische Zeitschrift*, 1889.

[1] "Die Gemeinschaftliche Ursache der Elektrischen Meteore und des Hagels," H. W. Schmidt, Halle-a.-S.

the various bodies present in the atmosphere. The possibility of contact electricity between a solid or liquid and a gas, is not quite easy to submit to the test of experiment. If we rub two solid bodies together, we may, by separating them, investigate the electric field produced; but, supposing we have a drop of water surrounded on all sides by air, the water may be covered with an electric layer of, say, positive electricity, the air in contact with the water with the opposite kind, and it is not at all clear how we could experimentally demonstrate the difference of potential between the air and the drop which is thus produced. A current of air flowing past the drop might carry away some of the negative layer, and in this way an electric field may be established while clouds are forming, but the conditions necessary for an experimental demonstration would be very difficulty to realise. Two methods have been devised which practically demonstrate some form of contact electricity between gases and water.

Lenard, wishing to imitate the electric field observed in the neighbourhood of waterfalls, has established by careful experiment a number of important facts, which are all consistent with the following explanation. If we imagine two oppositely electrified layers at the surface of a drop of water such as has been referred to, and if the drop falls on to a layer of the same liquid, or if similar drops impinge on each other, the difference of potential produced by the fusion of the surface layers becomes greater than is consistent with equilibrium. For, taking the case of drops falling into a mass of water contained in a cylindrical vessel, the extent of surface between air and water is not increased by the falling drops, and we must imagine that surface to be already covered with a sufficient electrical sheet to establish the required difference of potential, The electrification of the drops is, therefore, not wanted, and a change in the distribution takes place. The natural supposition would be, that this equilibrium would be restored very quickly through the surface of the water, but a certain time seems to be required for this. Meanwhile, the strong current of air which in Lenard's experiments is brought down with the water drops carry some of the electricity away, the water remaining positive. More recent experiments of Lord Kelvin's, with air bubbling through water, point similarly to contact forces between gases and liquids, and in these experiments also it appears that a considerable time is required to establish electric equilibrium between a gas and a solid. Lenard finds very important differences caused by small impurities in the water, the water acting much more strongly when it is pure. If it contains as much salt as is contained in the sea, the effect is reversed, and the air becomes positively electrified. The explanation which is given above is practically that of Lenard, whose observations have been confirmed and further extended by Prof. J. J. Thomson. These experiments, no doubt, account for the behaviour of air in the neighbourhood of waterfalls, and they probably also explain the negative electrification of air in the neighbourhood of districts in which rain is falling. The strong positive electrification of mist may also be due to the same cause.

There seems to be no doubt that the formation of a cloud is often accompanied by electrical effects. A few years ago, descending from the Dent Blanche, I found myself, after sunset, at a height of about 12,000 feet. A current of air was apparently blowing up the valley which stretches from Evolena towards Ferpecle, and I could observe a cloud condensing below me at a height a little below the snow-line. As night came on and we continued our descent over the glacier and down the valley, a series of electric discharges were noticed between the cloud, which was lying in a deep-cut valley, the sides of the mountain, and the blue sky overhead. Here the moist air was evidently streaming through the cloud, depositing its moisture in the form of drops, and it seemed the most natural explanation at the time that the air left the cloud in an electrified state.

But while by means of experiments we have been able to produce some of the phenomena of atmospheric electricity, we have other important effects which cannot be accounted for in so simple a way. The electric discharges during a thunderstorm give evidence of electric fields, which could hardly be explained by contact electricity between drops of water and air alone. The fact that thunderstorms are nearly always connected with the formation of hail, and Faraday's experiments showing that water rubbing against ice becomes negatively electrified, is made use of in the theories of Sohnke and Luvini. It is quite likely that there is some truth in these theories. Their weak point lies in the difficulty of seeing how particles of ice and water can be first sufficiently mixed to allow of friction, and then become sufficiently separated to produce an electric field of such magnitude as we know must exist in a thunder cloud.

It is to be remarked, however, that the laws of contact electricity must be applicable to gases as well as to solids, and that if water becomes positive when rubbing against air, and negative when rubbing against ice, there must be a strong contact difference between ice and air. In other words, it does not matter whether there is direct friction between ice and water, or whether the air forms an intermediate body. We may imagine air rising through a cloud containing drops of water negatively electrified, and then passing through an ice cloud having its negative electricity increased, thus leaving the ice and water particles at a difference of potential which may, by a fusion of the drops, increase sufficiently to produce a lightning discharge. This seems to me the most plausible theory which, in the present state of our knowledge, can be formed. As regards the permanent negative charge of the earth's surface, the time has not yet arrived for forming a definite opinion. Although we know that the earth, once electrified, would gradually lose its charge into the atmosphere, yet we can express no opinion as to the rate at which the loss is going on. That loss may be exceedingly slow, and consequently equilibrium might be attained by a very small preponderance of negative electricity brought back to its surface through some cause or other. Rain, as has already been mentioned, is more frequently electrified negatively than positively in our own climate, and though we do not know how far this holds in the tropical belt, it is at any rate possible that the surface of the earth may in this way alone make up for the loss. We may also reasonably think that Lenard's observation on salt water may account for the permanent charge. Every wave that breaks into spray under the action of a strong wind would leave the water negatively electrified, the air carrying away the positive charge. It would be of great interest to possess observations on atmospheric electricity on board ship while waves are breaking in the neighbourhood. So far we have only Exner's observations to guide us, who found, while observing at Lavinia, in Ceylon, that the spray from breaking waves affected the indications of the electrometer, proving its positive electrification (*Wiener Akad. Sitzungsberichte*, vol. xcviii.).

But although the loss of electricity from the earth's surface may be very slow, it is equally possible that it is considerable. We shall not be able to treat this question satisfactorily until we have some clearer notion of the causes of the aurora. We know that the aurora implies electric currents, and the circuit of these currents may lie completely within the earth's atmosphere, and have nothing to do with the observed fall of potential near the ground. It is also possible that the body of the earth forms part of the electric circuit, and if that is the case, there must be across different parts of the surface an outward and inward flow of positive electricity. Such a discharge could not fail to influence the phenomena we have discussed, and it seems probable that we should have some evidence derived from observation if the aurora was always accompanied by discharges through the earth's surface. Except in the polar regions, these auroræ do not seem to affect the normal fall of potential. There is a third view we may take as to the circulation of electric currents indicated by the aurora: the return current may take place in space outside the earth's atmosphere. A good deal might be said in favour of this view, and the rotation of the earth's magnetic field in space might be a sufficient cause for the production of these currents; but this is not the place to enter further into this question.

Calculations made from observation on the height of the aurora have generally resulted in an altitude of from 100 to 200 miles, except in the polar regions, where the aurora seems occasionally to descend to a much lower level. It has also been noticed that auroræ are associated with certain bands of cirrus clouds, and this seems to indicate that although the luminous phenomenon is sufficiently intense to be observed at only great heights, yet the electric phenomena may descend to the level of the cirrus.

As regards the connection between the aurora and the sun-spot period, further observations in the polar regions are needed. On the one hand, we have Paulsen's[1] statement, derived from observations in Greenland, to the effect that the greatest number

[1] Paulsen, "Danske Videnskab. Selskabs Forhand.," 1889. (I have not seen the original memoir, but only an abstract in the *Jahrbuch der Astronomie und Geophysik*, 1890.)

of auroræ are seen when sun-spots are at their minimum, that is, at a time when in our own latitudes the number is smallest; and, on the other hand, we have Nordenskiöld's observations, which seem to point in the opposite direction. In a publication which contains much important matter on the geographical distribution and form of the aurora borealis, Nordenskiöld contrasts the appearances he has observed in the *Vega* during the winter of 1878-79, passed in the Behring Straits, with that previously observed in 1872-73 to the north of Spitzbergen. According to this author, the auroræ, during the minimum sun-spot period in 1878-79, were "hardly worthy of his notice by the side of those observed in 1872-73." But although only faintly luminous, the auroræ of 1879 were persistent and regular in shape. They did not affect the magnetic field, and seem to show a regular and continuous, though weak, electric discharge. The arc and streamers in 1872 were much more brilliant and much more irregular. Some objection may be raised against these observations, in so far as they refer to different places, and local circumstances may have affected the phenomenon; but in the face of the very careful description he gives us, we cannot as yet accept Paulsen's results without further confirmation.

The problem of atmospheric electricity, like that of terrestrial magnetism, presents special features in the arctic regions, and until we possess a greater number of observations in those little accessible parts of the earth's surface, many important problems cannot be satisfactorily solved. Arctic and antarctic expeditions are of interest to scientific men, not because they care much whether we get a few miles nearer the pole, but because a well-conducted party collects invaluable information on its journey. Although much remains to be done in the regions surrounding the north magnetic pole, our knowledge in the southern hemisphere is almost disgracefully inadequate, and it is to be hoped that before long a well-equipped expedition may fill up to a certain extent the large gaps in our electrical and magnetical knowledge which at present stop so many of our researches.

But although investigations to be conducted in the arctic regions are of primary importance, we may do much nearer home in extending and completing existing information. Instrumental appliances and methods of observation, originally put into a satisfactory state by Lord Kelvin, have been improved, especially by Mascart, Exner, Elster, and Geitel. One of our most crying wants at present is a series of continuous observations by means of self-registering instruments in places where the neighbourhood of a town, or other local circumstances, do not interfere with the normal changes. The Greenwich Observatory, to which we look for help in such matters, is placed in the difficulty that the daily variations there observed are markedly different from those in the majority of places, and it is probable that the nearness of London is fatal to any generally useful series of observations of atmospheric electricity being conducted in our national Observatory.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

THE annual general meeting of the Association of Technical Institutions will be held at the Goldsmiths' Hall on Friday, the 24th inst.

THE Senate of University College, Liverpool, on the joint recommendation of Profs. Lodge and Hele Shaw, have appointed Mr. Alfred Hay, B.Sc., of University College, Nottingham, to the Lectureship on Electrotechnics, vacant by reason of the election of Mr. F. G. Baily to the chair of Electrical Engineering at the Heriot-Watt College, Edinburgh.

MR. L. F. GOLDSTAND has presented the Royal Agricultural College, Cirencester, with the sum of £200 for the institution of three silver medals annually, to be awarded according to results of the final examination for the diploma, at the discretion and decision of the Principal. The donor has requested that the medals be styled the "McClellan," the "Harker," and the "Goldstand," respectively; and the Principal, on behalf of the Governing Body, has accepted and ratified the donation.

THE eighth annual report, just published by the National Association for the Promotion of Technical and Secondary Education, is a mine of statistical and other information referring to the development of educational organisation in Great Britain. Substantial progress is recorded in the work of technical education; and it appears that of the £744,000 annually available in England alone, £600,000 is being spent on education. In the year covered by the report, 7252 scholarships and exhibitions, of the total yearly value of £40,598, were offered by thirty-seven counties. A large section of the report is devoted to summarising the recommendations of the Royal Commission on Secondary Education.

AMONG recent appointments and nominations abroad, we notice the following:—Dr. Hürthle to be Extraordinary Professor of Physiology at Breslau; Dr. N. Busch to be Director of the Botanic Garden of the University of Dorpat; Dr. K. G. Huefner, Professor of Organic and Physiological Chemistry at Tübingen, to succeed the late Prof. Hoppe-Seyler at Strassburg; Dr. Bauschinger, of Munich, to succeed the late Prof. Tietjen as Extraordinary Professor of Astronomy at Berlin; Dr. Anton F. v. Eiselsberg, Professor of Surgery at Utrecht, to be Prof. Braun's successor at Königsberg; Dr. H. Nichols to be Lecturer in Psychology in the Johns Hopkins University, Baltimore.

SCIENTIFIC SERIALS.

The Quarterly Journal of Microscopical Science for August 1895 contains: On the variation of *Haliclystus octoradiatus*, by Edward T. Brown (plate 1). Some 154 specimens were examined, 120 of these were perfectly normal but 34 afforded either cases of congenital variation, or showed regeneration of organs after destruction or injury. Most of the abnormal forms are figured.—On the collar-cells of Heterocœla, by George Bidder (plate 2). Observations were made on *Leucandra aspera*, *Sycon raphanus*, *S. compressum*; this last was found best suited for examination under high powers during life, its collar-cells are among the largest, if not as large, as any known. The protoplasm of these cells is in life greenish, and they have nearly the form and relation to each other of full corn-sacks standing side by side in a granary. The living collar is invariably an almost perfect cylinder, very little constricted at its base. As to Sollas's membrane, the statements of Vosmaer and Pekelharing, which the author once thought erroneous, he now confirms, there is no normal union of the collars, the membrane is only to be met with in "paraffin sections."—The metamorphosis of Echinoderms, by Henry Bury (plates 3-9). With the view of clearing up some of the differences in observation and opinion of the more recent observers of the metamorphosis of this group, the author has worked out as far as possible the metamorphic changes of at least one form of larva in each of the five classes of Echinoderms; for reasons given, the metamorphosis of Synapta is written in greater detail than that of the rest. As to the relation of the Echinodermata to the Enteropneusta, "there seems to be a chain of evidence of their connection, which though not indeed conclusive—that embryological evidence alone can never be—is at least as strong as that which binds together any two of the great subdivisions of the animal kingdom."—A criticism of the cell-theory; being an answer to Mr. Sedgwick's article on the inadequacy of the cellular theory of development, by Gilbert C. Bourne. The article of Prof. Sedgwick here criticised appeared in the *Q.J.M.S.* for November 1894.

The number for November 1895 contains:—On the distribution of assimilated iron compounds, other than Hæmoglobin and Hæmatins, in animal and vegetable cells, by Dr. A. B. Macallum (plates 10-12). After some preliminary remarks on the special literature of the subject and references thereto, the author details his methods of study. This portion of the memoir is very instructive, not only for the facts recorded, but for the hints given; chlorophyll yields no evidence that it contains iron, and it is mentioned incidentally that species of Monotropa remain colourless when fixed in solutions of corrosive sublimate. The greater part and sometimes the whole of the assimilated iron in the cells of the higher forms of animal life is held in the nucleus, in the chromatin of which it is chiefly found, and the same is true of the nuclei of all the higher vegetable organisms; it is rarely found in the cytoplasm of the cells, but full details of such occurrences are given. An important section is devoted to the occurrence of assimilated iron in special forms of life, such as in protozoa, fungi, bacteria, and the Cyanophyceæ.—On the structural changes in the reproductive cells during the spermatogenesis of Elasmobranchs, by J. E. S. Moore (plates 13-16). The author establishes a long series of structural homologies found before, during, and after the synaptic phase in the reproductive cycles of both animals and plants, and so close is their correspondence, amid a host of complex structural details, that it is in the highest degree improbable that the two series of phenomena can have been independently evolved; and whatever the synapsis may eventually turn out to be, it is evidently a cellular metamorphosis of a profoundly fundamental character, which would appear to have been acquired before the animal and vegetable ancestry went apart, and to have existed ever since.—Notes on the fecundation of the egg of *Sphærechinus granularis*, and on the maturation and fertilisation of the egg of *Phallusia mammillata*, by M. D. Hill (plate 17). In these forms there is no egg astrosphere or egg centrosome; both these structures are brought into the ovum by the spermatozoon, and they give rise by division to all the subsequent astrospheres and centrosomes throughout ontogeny. There is consequently no such thing as a "quadrille."—Further remarks on the cell-theory, with a reply to Mr. Bourne, by Adam Sedgwick, F.R.S.

Symons's Monthly Magazine for December contains a climatological table and summary for various selected stations of the British Empire, for the year 1894. Australia records the highest shade temperature, viz. 107°·0 at Adelaide, on November 26, and it was the dryest station. In the twelve years for which the annual summaries have appeared, this station has yielded the highest maximum in ten years, Melbourne in one, and Calcutta in one. The lowest temperature in the shade was recorded at Winnipeg, −46°·1 on January 24. This station has never been equalled for lowness of absolute shade temperature, and has only twice failed to record the greatest mean daily range; the variation during the year amounted to 141°·9. The dampest station was Esquimalt, where the mean humidity was 88 per cent.; London comes next, being 81 per cent., and both these places were the most cloudy, the average amount being 6·3. The least cloudy stations were Bombay and Grenada, where the average amount was 4·0. The greatest annual rainfall, 77·5 inches, occurred at Colombo, and the least, 18·1 inches, at Winnipeg. The Cape of Good Hope observations were unfortunately missing.

L'Anthropologie, 1895, Tome vi. No. 4.—Quatenary deer of Bagnères-de-Bigorre (Hautes-Pyrénées), by Édouard Harlé.—A careful examination of the mandible has led the author to the conclusion that the animal to which it belonged was neither a reindeer nor a stag, but that it must be considered a variety of the fallow-deer; and its presence in conjunction with *Elephas primigenius*, *Rhinoceros tichorinus*, and the reindeer, at the foot of the Pyrenees, is a fact of some interest.—Note on the age of metals in the Ukraine, by Baron de Baye. The progress of civilisation was not uniform in the north and south of Russia in Europe. In the district south of a line which corresponds very closely with the 50th parallel of latitude, it has been found that the use of metals was known at a very early date, whilst the Stone Age continued for a much longer period in countries to the north of this line. Baron de Baye is careful to explain that the term "Scythian," which he uses freely in connection with the mounds and the various bronze articles found in them, does not express an anthropological unit, but is used in a purely geographical and ethnological sense. The numerous tribes, however, comprehended under this name had the same civilisation, practised the same arts and the same funeral rites, and left behind them similar archæological remains.—Anthropological observations on the tumuli and worked flints of the Somali and the Danakil, by Dr. Jousseaume. The tombs are constructed of rough stones, more or less spherical or ovoid in shape, and of various sizes. The flints are arranged by the author in four groups, the first of which is represented by a single specimen in the form of a wedge. The second group is spatulate, rather long, very thick, and always larger at one end than at the other. The flints included in the third group are discoidal, and of various sizes; while the fourth group includes all those that are lance-shaped.—Infantilism, feminism, and the hermaphrodites of the ancients, by Henry Meige. In this section of his paper the author treats of feminism, of which a very beautiful example came under the observation of Prof. Charcot at La Salpêtrière.

Bollettino della Società Sismologica Italiana, vol. i., 1895, No. 6.—Earthquake of Paramythia (Epirus) during the night of May 13–14, 1895, by G. Agamennone (see p. 205).—The Guzzanti microseismoscope, by G. Guzzanti.—Notices of Italian earthquakes (April–May 1895), referring chiefly to the Florentine earthquake of May 18, and to the pulsations of the earthquake of Paramythia of May 13–14.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, November 28, 1895.—"The Expansion of Argon and of Helium as compared with that of Air and Hydrogen." By Dr. J. P. Kuenen, Professor of Physics in University College, Dundee, and Dr. W. W. Randall, Lecturer in John Hopkins University, Baltimore, U.S.A.

The gas-thermometer used for these experiments was such as could be easily heated by means of a "Ramsay and Young" vapour-jacket. It consisted of a bulb sealed on to a capillary tube, which in turn was sealed on to a wider tube, provided with a mark. This mark is situated just outside the heating arrangement, immediately below the capillary tube. The inner mercury-surface is made to coincide with this mark, and both this one and the outer surface are read on a scale with a telescope. The readings were duly corrected for capillary action, expansion of glass and mercury, and the part of the gas that is not heated. The results show that up to 240° C., the highest temperature that was employed, the expansion of both argon and helium is very nearly the same as that of air and hydrogen.

December 5, 1895.—"Studies in the Morphology of Spore-producing Members. Part II. Ophioglossaceæ." By F. O. Bower, F.R.S. Preliminary Statement on the Sorus of *Danæa*, by F. O. Bower, F.R.S.

In Part I. of these studies it had been shown on comparative grounds to be probable that septation of sporangia, previously in the race simple, had taken place among the Lycopodineæ. It appeared, however, important to show that such a process of septation has taken place elsewhere; examples of it are found in the anthers of many Angiosperms of the orders Mimoseæ, Onagraceæ, Loranthaceæ, Rhizophoreæ, &c. The development has been studied in certain cases, and it is seen that a partial sterilisation of sporogenous cells results in the formation of sterile septa, which may vary greatly in thickness from a broad band of tissue to a narrow one; sometimes the septum may be represented by a single layer of cells of the nature of a tapetum, or the septum may be incomplete. A comparison of these cases with plants of Pteridophytic affinity shows that similar structural and developmental details are found: the most conspicuous case is that of *Danæa*, in which large synangia are found on the under-surface of the leaf; these, though attached along the leaf surface, have a structural similarity to the spike of *Ophioglossum*. It is not uncommon to find in them, as in the Angiosperms quoted, great variety of size of the loculi, and of thickness of the septa, while incomplete septa are also common: the close parallel as to these characters is a very striking feature, and raises the probability of their having resulted from a similar mode of evolutionary progress, *i.e.* by septation.

The second part of the "studies" refers to the Ophioglossaceæ, and the suggestion made by various writers (Mettenius, Strasburger, Celakovsky, and others), that they are related to the Lycopods is upheld; it is supported on grounds of comparison of external form, of anatomy, of the characters of the Gametophyte and embryology, as far as known. From these various sources a general support of the relationship has been traced, the nearest point of comparison appearing to be between *O. Bergianum*, and *Phylloglossum Drummondii*. It is contended that the external similarity of these plants, long since recognised, is not a case of mere mimicry, but of real relationship, though this probably dates from an unknown common ancestry.

Such a relationship involves the idea of septation; but it has been shown that septation of a very similar nature has taken place in the anthers of Angiosperms. *Danæa* among Pteridophytes shows very similar characters, and, finally, a minute study of development in *Ophioglossum* has elicited facts which are compatible with such a view. From simple types of *Ophioglossum* a progression may be traced to the larger and more complex species, *e.g.*, *O. palmatum*; while a somewhat parallel sequence would lead from such a plant as *Botrychium simplex* onwards to the larger, elaborate species of the genus. *Helminthostachys* appears to hold a somewhat independent position.

December 12, 1895.—"On the Formation and Structure of Dental Enamel." By J. Leon Williams, D.D.S., L.D.S.

The special points in the formation and structure of ename which I have attempted to elucidate in this paper, may be summarised as follows :—

(1) The existence of a very thin membrane, or a structure of membrane-like appearance, lying between the ameloblasts and the forming enamel, and also between these cells and those of the stratum intermedium. I have also, in many specimens, seen a similar membrane covering the odontoblasts.

(2) The formation of enamel by deposit, and not by cell calcification. This deposit probably consists of two distinct cell products—a granular plasm and spherules of calcoglobulin.

(3) The relation of the cells of the stratum intermedium to true secreting tissue ; this relation being especially marked in the enamel organs of the rat and mouse.

(4) An intricate vascular network in the stratum intermedium. I should also mention that I have seen a free distribution of blood vessels in the odontoblastic layer of cells in the mouse. rat, and calf, as well as in human embryos, thus conclusively proving that these cells are not calcified.

(5) The fibrous character of enamel in many of the lower animals, and the change of these fibres into more or less regularly arranged granules in the monkey and in man.

(6) That the varicosities of the enamel rods are not caused by acids (although often rendered more clear to view by acid treatment), but represent a true structural peculiarity of this tissue. That these varicosities, which often continue in an uninterrupted line across large fields of view, correspond with the course of one set of fibres. The varicosities may, therefore, be caused by the presence of this set of cross fibres. The only alternative explanation which has occurred to me is that there may be a rhythmic, simultaneous action of all the ameloblasts concerned in the deposit of the material for enamel building. The last theory seems to be less reasonable than the first.[1]

(7) The Retzius bands are often as distinctly marked in forming as in mature teeth, and in teeth which have been kept constantly moist, as they are in dried specimens. The enamel rods are often seen to pass without break across several of these bands. The bands are principally due to a deposit of pigment, and not to imprisoned air or gas, as claimed by von Ebner.

Chemical Society, December, 5, 1895.—Mr. A. Vernon Harcourt, President, in the chair.—The following papers were read :—Researches on the terpenes, VI. Products of the oxidation of camphene ; camphoic acid and its derivatives, by J. E. Marsh and J. A. Gardner. Camphoic acid, $C_{10}H_{14}O_6$, is the chief oxidation product of camphene ; a number of new derivatives of the camphopyric acids, $C_9H_{14}O_4$, are described.—New derivatives from α-dibromocamphor, by M. O. Forster. On treating α-dibromocamphor with nitric acid, a lactone, dibromocampholidid, $C_{10}H_{14}Br_2O_2$, is obtained ; on reduction it yields an unsaturated acid, bromocamphorenic acid, $C_{10}H_{15}BrO_2$. Camphorenic acid, $C_{10}H_{16}O_2$, and campholidid, $C_{10}H_{16}O_2$, have been prepared.—Isomeric π-bromo-α-nitrocamphors, by A. Lapworth and F. S. Kipping. π-bromo-α-nitrocamphor is obtained, together with a bromocamphoric acid, by heating π-dibromocamphor with nitric acid ; it is converted into an isomeride by crystallisation from hydrochloric acid. These two substances are probably cis- and trans-isomerides.—Derivatives of π-bromocamphoric acid, by F. S. Kipping. π-bromocamphoric acid, when treated with alkalis, yields first a lactonic acid, $C_{10}H_{14}O_4$, and then π-hydroxycamphoric acid, $C_{10}H_{16}O_5$; it probably contains the group CH_2Br.—π-dibromocamphoric acid and its derivatives, by F. S. Kipping. π-dibromocamphoric anhydride, $C_{10}H_{12}Br_2O_3$, is prepared by the action of bromine and red phosphorus on π-bromocamphoric acid ; it yields π-dibromocamphoric acid, $C_{10}H_{14}Br_2O_4$, when heated with nitric acid, and π-bromocamphanic acid, $C_{10}H_{13}BrO_4$ on boiling with water.—w-bromocamphoric acid, by F.S. Kipping. w-bromocamphoric acid, isomeric with π-bromocamphoric acid, is obtained by hydrolysing Wreden's bromocamphoric anhydride.—π-chlorocamphoric acid, by F. S. Kipping and W. J. Pope. π-chlorocamphoric acid, $C_{10}H_{15}ClO_4$, is prepared by oxidising π-dibromocamphor with nitric acid.—Derivatives of α-hydrindone, by C. Revis and F. S. Kipping. Dibromohydrindone is obtained by the action of a soda solution of bromine on α-hydrindone at ordinary temperatures, whilst at 100° a condensation product of the composition $C_{18}H_{12}O_3$ is obtained. When monobromohydrindone is dissolved in cold alcoholic potash, a condensation product of the composition $C_{18}H_{13}BrO_2$ is deposited.—The alkaline reduction of metanitraniline, by R. Meldola and E. R. Andrews. Alkaline reducing agents convert metanitraniline into an azoxy-compound,

$$\begin{matrix} NH_2.C_6H_4.N \\ NH_2.C_6H_4.N \end{matrix}\Big\rangle O$$

; the corresponding azo-compound has also been prepared.—The chemistry of dibromopropylthiocarbimide ; and the action of bromine and iodine upon allylthiourea, by A. E. Dixon.

Linnean Society, December 5, 1895.—C. B. Clarke, F.R.S., President, in the chair.—Messrs. Bernard Arnold and Rupert Vallentin were admitted, and the following were elected Fellows of the Society : W. M. Christy, Rev. H. P. Fitzgerald, A. W. Geffeken, Rev. E. A. Peacock, Rev. T. R. Stebbing, and W. O. Stentiford.—The President called attention to a portrait of the late Prof. Babington, of Cambridge, which had been lately presented by his widow to the Society. On the motion of Dr. Murie, seconded by Mr. A. W. Bennett, a vote of thanks to Mrs. Babington was unanimously accorded.—Prof. Stewart offered some remarks on the types of the axes of certain Gorgonaceæ, in which he referred chiefly to the importance or otherwise of the presence of spicules in the axes, and exhibited the following species in illustration of his remarks : *Paragorgia arborea*, *Melitodes ochracea*, *Suberogorgia suberosa*, *Corallium rubrum*, *Caligorgia verticillata*, *Verrucella guadalupensis*, *Isis hippuris*, *Plexaurella crassa*, and *Eunicella verrucosa*. Some criticism was offered by Dr. Murie, chiefly in relation to the structure of *Gorgonia flabellum* and *Gorgonia setosa*.—Mr. Martin Woodward exhibited and made remarks on a living specimen of *Ouramœba*, which he thought should be regarded as a common Amœba attacked by a parasitic fungus.—Mr. G. C. Druce communicated a paper on a new species of *Bromus* in Britain, which was said to differ from others of the genus in its inflorescence, having single, short, stiff pedicels arising alternately right and left of the main rachis, each bearing at its extremity 3–5 sessile, or in some cases shortly stalked spikelets, giving an interrupted and compact appearance to the whole inflorescence, which is made up of two rows of clustered groups of 3–5 spikelets. This peculiar feature being absent in its nearest allies, the name *interruptus* was proposed to distinguish it ; another feature being that the palea was split to the base, and not merely bifid. It appeared to have been described or referred to by Prof. Hackel as *Bromus mollis*, var. *interruptus*, but Mr. Druce considered that it was sufficiently distinct to be entitled to specific rank. He had found it growing abundantly in a field of vetches near Upton, Berkshire, and specimens had been examined from Headington, Oxford, and Dartford, Kent. In a discussion which followed, Dr. O. Stapf reviewed the literature of the subject, and gave reasons for regarding the so-called new species as merely an abnormal growth of *Bromus mollis*. Critical remarks were made also by Mr. H. Groves and Mr. A. B. Rendle, who were inclined to share the opinion of the last speaker.—A paper was then read by Mr. W. F. Kirby, on some new or little-known *Phasmidæ* in the collection of the British Museum, with illustrative specimens.

Mathematical Society, December 12, 1895.—Major P. A. MacMahon, R.A., F.R.S., President, in the chair.—Prof. Hill, F.R.S., gave a sketch of a note on the convergency of series, by Dr. R. Bryant.—Lieut.-Colonel Cunningham, R.E., communicated at some length a paper on the criterion of 2 as a 16^{ic} residue. A discussion followed the reading, in which Messrs. Bickmore, Kempe and the President took part.—Dr. Hobson, F.R.S., read a short note on the distribution of electricity induced on an infinite disc with a circular hole in it, by Mr. H. M. Macdonald.—A paper by Dr. R. Lachlan, on the double foci of a bicircular quartic, and the nodal focal curves of a cyclide, was taken as read.

Zoological Society, December 17, 1895.—Sir W. H. Flower, K.C.B., F.R.S., President, in the chair.—Dr. Donaldson Smith offered some remarks on some of the animals observed by him during his recent journey to the Lakes Rudolph and Stephanie, and alluded especially to the species of zebras and antelopes encountered during his journey.—Mr. Sclater exhibited and made remarks on the head of an antelope obtained in Kavirondo, British East Africa, by Mr. E. Gedge. This antelope had been hitherto usually identified with the "kob" of Western Africa, but appeared to belong to a distinct species, to which the name *Cobus thomasi* had been given by Herr Neumann.—Mr. G. F. Hampson read a paper on the classification of two sub-families of the moths and of the family

[1] Since the above was written, I have demonstrated that there is a simultaneous deposit of the spherical bodies over the entire surface of forming enamel.—J. L. W.

Pyralidæ, the *Schœnobiinæ* and *Crambinæ*.—A communication was read from Mr. Oldfield Thomas, on *Cænolestes*, a still-existing survivor of the Epanorthidæ of Ameghino, and the representative of a new family of recent Marsupials.—Mr. Walter E. Collinge read a paper on the sensory and ampullary canals of *Chimæra*, and the innervation of the same.—Mr. F. A. Bather read a paper on the fossil crinoid *Uintacrinus*. The paper attempted a complete morphological description of *Uintacrinus socialis*, based on specimens from the Upper Cretaceous Beds of Western Kansas, now in the British Museum.—A communication from Dr. C. Brunner von Wattenwyl gave a list of the Orthoptera of the Hawaiian islands.

Royal Meteorological Society, December 18, 1895.—Mr. R. Inwards, President, in the chair.—Mr. R. H. Scott, F.R.S., read a paper on some of the differences between fogs, as related to the weather systems which accompany them. In this it was shown that there are at least two distinct classes of phenomena described under the generic name of "fog." In the case of anticyclonic fogs, no rainfall takes place; the temperature is low in the morning, and there is a considerable rise of temperature during the day; while in the case of cyclonic fogs, rainfall does take place, and the temperature is high in the morning, frequently approaching or even equalling the maximum for the day. Mr. Scott also investigated the cases of several well-marked fogs in London, and found that there was no direct relation traceable between the temperature accompanying them and the death rate.—Major H. E. Rawson described the results of his analysis of the Greenwich barometrical observations from 1879 to 1890, with special reference to the declination of the sun and moon.—A paper by Mr. S. C. Knott was read, giving the results of his meteorological observations taken at Mojanga, Madagascar.—Mr. Scott also exhibited some specimens of the illustrations in the "International Cloud Atlas," which is now being prepared for publication.

PARIS.

Academy of Sciences, December 23, 1895.—Annual public meeting.—M. Marey in the chair.—The President delivered an address, in which reference was made to the celebration, in October last, of the centenary of the Institute of France. The members and associates deceased during the year—MM. Pasteur, Verneuil, Larrey, Cayley, Dana, Vogt, Ludwig, Huxley, Lovén, Hellriegel—were referred to, and a brief *résumé* given of the life-work of each. The President then announced that a biennial prize (20,000 fr.), which is in rotation in the gift of each of the five Academies, and which this year fell to the Academy of Sciences, has been awarded to M. Raoult for his discovery of the numerical relations between the molecular weight of a substance and the lowering of the freezing point and vapour pressure of its solvent. The other prizes were awarded as follows: The Francœur prize to M. J. Andrade, the Poncelet prize to M. G. Robin, for his contributions to mathematical physics. In Mechanics, the extraordinary prize of 6000 fr. was divided between M. Mottez (2500 fr.), for his work on the correction of ships' compasses; M. Houette (1500 fr.), for his aids to navigation; M. Gosselin (1500 fr.), for his method of studying the velocity of projectiles; and M. Baucher (500 fr.), for his study of the action of sea-water upon metals. The Montyon prize was given to M. Galliot, for a new application of electric traction on canals; and the Plumey prize to MM. Pollard and Dudebout. The Fourneyron prize was divided between M. G. Marié and M. Lecornu, for their experimental and theoretical work on steam governors. In astronomy, the Lalande prize fell to M. M. Hamy; and the Valz prize to Mr. Denning (of Bristol), for his work on comets and shooting stars. In Physics, M. E. Bouty was accorded the La Caze prize for his numerous researches in electricity and magnetism; in Statistics, the Montyon prize was divided between M. A. Martin and M. C. Baltet, whilst honourable mention was accorded to MM. Hovelacque and Hervé. In Chemistry, the Jecker prize was divided between M. Tanret (6000 fr.), M. Renard (2000 fr.), and M. Burcker (2000 fr.); whilst the La Caze prize was given to M. Le Chatelier for his researches on the combustion of explosive mixtures, pyrometry, and thermodynamics of chemical processes. In Mineralogy and Geology, the Grand prize for the physical sciences was adjudged to M. C. Brongniart for his researches in palæontology; the Delesse prize to M. Delafond for his stratigraphical studies; whilst the Bordin prize is equally divided between M. de Pousargues and M. Barrat. In Botany, the Desmazières prize was awarded to M. Borzi, the Montagne prize to M. F. Renauld, and the De la Fons-Mélicocq prize (900 fr.) to M. G. de la Marlière. In Anatomy and Zoology, the Thore prize fell to M. P. Mégnin, the Savigny prize being not awarded. In Medicine and Surgery, three Montyon prizes were given, to MM. Gangolphe, Imbert, and Teisser; mentions and minor awards went to MM. Chipault, Gouguenheim and Glover, Polaillon, Bellini, and Parant. The Barbier prize was divided between M. Bœckel and M. Dupuy, the Bréant prize being left unawarded. M. E. Reymond was adjudged the Godard prize, M. Lancereaux the Chaussier prize, and M. Vaillard the Bellion prize, with honourable mention to M. Vincent and M. Rouget, M. Mauclaire and M. Detroye. The Mège prize was awarded to M. Baudron, and in connection with the Dusgate prize (not given) honourable mention was accorded to M. Icard. The Lallemand prize was divided between M. Toulouse and M. Halipré, with mention of MM. Chervin and Debierre. In Physiology, the Montyon prize was given to M. Artus (with mention of M. Tissot), the La Caze prize to M. Dastre, the Pourrat prize to M. Charrin, the Philipeaux prize to M. Chabrié, the Martin-Damourette prize being divided between M. Besson and M. Cristiani, with honourable mention of Dr. de Keating Hart. In Physical Geography, the Gay prize was awarded to M. Angot, a second prize (1000 fr.) being given to a paper by an anonymous author. General prizes: The Montyon prize (unhealthy industries) was given to M. Gérardin, the Trémont prize to M. B. Renault, the Gegner prize again to M. Paul Serret, the Petit D'Ormoy prize to M. Albert Ribaucour, for the mathematical sciences, and to M. Pomel for the natural sciences. The Leconte prize was awarded to Lord Rayleigh and Prof. Ramsay for their work on the constitution of atmospheric air. The Tchihatchef prize was adjudged to Dr. Radde, the Gaston Planté prize to MM. J. and P. Curie, the Saintour prize to M. Termier. The Cahours prize was divided between MM. Lebeau, Simon, and Varet, the Alberto Levi prize (50,000 fr.) between MM. Behring and Roux; to the former for his discovery of the antidiphtheric serum, and to the latter for the happy application which he has made in France of this discovery. The Kastner-Boursault prize was awarded to M. Baudot for his improvements in multiplex telegraphy, the Laplace prize to M. Bachellery, and the Felix Rivot prize to MM. Bachellery, de Ruffi de Pontevès Gevaudan, Delemer, and Labordère. Details of the prizes announced for 1896, 1897, 1898, and 1899 are given.

BERLIN.

Meteorological Society, November 5, 1895.—Prof. Hellmann, President, in the chair.—Dr. Zenker gave an abstract of a lengthy paper on the thermal constitution of climates. He explained how, from the solar radiation which he had previously calculated out for the outer boundary of the atmosphere, he had deduced that which takes place at the inner; and how from the latter, taking into account the radiation from the earth, and the existence of clouds, he had determined the annual and monthly temperatures for areas of each two degrees of the earth's surface. In part two of his paper, he had compared the temperatures calculated as above with those actually recorded for many places in India, Africa, America, and Australia.—Mr. Archenhold exhibited the negative of a photograph of a lightning-flash taken on August 24 last. The negative showed one narrow black flash on the dark field of the heavens above the brightly illuminated tree-tops, and a second much broader, bright flash, which was brighter than the tree-tops. The opinion was expressed that probably the great intensity of the possibly multiple flash may have led to the solarisation of its own image.

Physiological Society, November 8, 1895.—Prof. du Bois Reymond, President, in the chair.—Dr. Hausemann spoke on the large interstitial cells of the testis. He had found that they occur somewhat rarely in the connective tissue between the tubules in hibernators during their winter sleep, but are, on the other hand, extremely numerous after they awake. In man they are plentiful in the newly-born and children, less numerous at puberty and during manhood, and increase again largely in numbers in old-age.—Dr. Rawitz had repeated Loeb's experiments on the normal development of exovates, and the remains of impregnated sea-urchin's eggs after treatment with dilute sea-water. In most cases the exovates succumbed as well as the eggs; in only one case had he observed that while the exovate succumbed, the egg developed into a complete gastrula. From this it appeared that the exit of a portion of the contents of the egg had no influence on the development of the remaining part.—Prof. Zuntz criticised a recent paper by Filehne and Kiouka, in which they attempted to disprove his view that the increased respiratory frequency during muscular exertion is due to the action on the respiratory centre of some product formed during the activity of the muscles. He showed that their objections do not hold good, and that their experiments do not upset his conclusions.

November 29, 1895.—Prof. du Bois Reymond, President, in the chair.—Dr. René du Bois Reymond spoke on the opposition of the thumb, a point on which very scanty and insufficient information is contained in text-books of anatomy. He had studied in detail the theory of saddle-joints, and on the assumption that the movements take place about two axes at right angles to each other and passing through the point of contact of the two bones, he had arrived by construction at a mathematical formula corresponding to the ideal saddle-joint. This formula shows that a certain very limited amount of rotation is possible in this joint. He had further investigated, by the horopter and photographically, the actual movements of the thumb, the hand being firmly fixed, and gave the several phases of the movements which occur in the joint between the metacarpus and trapezium, and between the phalanx and the metacarpus during opposition. —Dr. Schultz demonstrated on the humerus of a duck the connection between the lungs and the bone cavities.

Physical Society, November 15, 1895.—Prof. von Bezold, President, in the chair.—Prof. Warburg gave a short account of experiments, by W. J. Wäggener, on the temperature of the flame of a Bunsen burner. The measurements were made with carefully-tested Le Chatellier's thermo-electric elements. The accuracy of the measurements was further tested in two directions. Firstly, with reference to the effect of high temperature on the E.M.F. of the element, it was found that prolonged heating makes the platino-iridium wire more markedly irregular than it does the platinum ; hence the element was exposed to the flame for a short time only. Secondly, with reference to the disturbing effect of heat conduction, it was found with wires of 0·5, 0·2, 0·1 and 0·05 mm. diameter, that when they are coiled up so that they can be almost completely enveloped in the zone of active combustion the three thicker wires recorded the same temperature, whereas, when not so coiled up, the thicker wires gave a lower temperature. The thinnest wire gave the highest values in the outer edge of the flame and in the zone of active combustion, in the inner cone a lower value than that given by the wire of 0·1 mm. The highest temperature recorded was 1704° C. Taking the highest records of the above four wires, and representing them graphically, a curve was obtained which gave the value 1750° C. for a wire of zero thickness, a temperature not much below the melting point of platinum, 1780° C.—Prof. Thiesen spoke on the formulæ which make it possible to obtain a perfect image with a simple lens.

AMSTERDAM.

Royal Academy of Sciences, October 26, 1895.—Prof. Van Sande Bakhuyzen in the chair.—Prof. Martin read a paper on Tertiary fossils from the Philippines. Basing his arguments upon a collection of fossils formed many years ago by Semper in the Philippines, the author showed that in Luzon, in the upper course of the Rio Grande de Cagayan, there appear neo-miocene strata, which must be considered equivalent to the typical neo-miocene of Java. Similar strata are also found in Cebú ; moreover, neo-tertiary (miocene or pliocene ?) fossils have been brought away by Semper from the hills of Aringay in Luzón, and finally pliocene ones from the Rio Agusan, Mindanáo. In the Philippines there occur tertiary and newer deposits, which correspond to the newer sediments in Java, both as regards the age and the petrographic structure and the fossil fauna of the said strata.—Mr. Jan de Vries presented a paper on a class of complete functions. Let W be a function of γ of the nth degree, then the general formula is determined for a function that satisfies the equation $W_{n}-\gamma W_{n-1} + W_{n-2} = 0$.—Prof. Kamerlingh Onnes communicated Dr. Zeeman's measurements on absorption of electrical vibrations in electrolytes, undertaken at the suggestion of Prof. Cohn of Strassburg, and carried out in the Leyden laboratory. The (as yet) preliminary results are : (1) the energy of the electric vibrations in pervading an electrolyte diminishes in the logarithmic ratio ; (2) if the wave-length is 6·5 m., the energy has decreased to one-third of its original value when the wave has passed through 6·5 c.m. of a solution of common salt, the resistance of which is $3200 \cdot 10^{-10}$ that of mercury.—Prof. Lorentz presented, on behalf of Mr. A. Smits, a paper, entitled "A Description of the Micromanometer." By means of the instrument described, a difference of pressure equal to $\frac{1}{1500}$ m.m. of water or $\frac{1}{20250}$ m.m. of mercury, may, if all precautions possible are taken, be measured under the most favourable conditions.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Gesammelte Abhandlungen über Entwickelungsmechanik der Organismen : Prof. W. Roux, Erster and Zweiter Bands (Leipzig, Engelmann).—Die Mikroskopische Thierwelt des Süsswassers : Dr. F. Blochmann. Abthg. 1. Protozoa (Hamburg, Gräfe).—Fauna der Gaskohle und der Kalksteine der Permformation Böhmens : Dr. A. Fritsch, Dritter Band (Prag, Rivnác).—Die Haustiere und Ihre Beziehungen zur Wirtschaft des Menschen : E. Hahn (Leipzig, Duncker).—The Story of the Solar System : G. F. Chambers (Newnes).—A Manual of Inorganic Chemistry : Dr. T. E. Thorpe, 2 Vols., new edition (Collins).—Die Spectralanalyse : Dr. J. Landauer (Braunschweig, Vieweg).—In Haunts of Wild Game : F. V. Kirby (Blackwood).—Dynamo-Electric Machinery : Prof. S. P. Thompson, 5th edition (Spon).

PAMPHLETS.—Dynamo Attendants and their Dynamos : A. H. Gibbings, 2nd edition (Rentell).—Submarine Telegraphy, &c. : J. Bell and S. Wilson (*Electricity* Office).—Pharmaceutical Society Museum Report for the Year 1893-4 (Bloomsbury Square).

SERIALS.—Proceedings of the Society for Psychical Research, December (Paul).—Popular Science Monthly, December (Paul).—American Naturalist, December (Philadelphia).—History of Mankind : F. Ratzel, Part 4 (Macmillan).—Bulletin of the Illinois State Laboratory of Natural History, Urbana, Ill., Vol. 4 (Springfield, Ill.).—Good Words, January (Isbister).—Sunday Magazine, January (Isbister).—English Illustrated Magazine, January (198 Strand).—Longman's Magazine, January (Longmans).—Economic Journal, December (Macmillan).—Astrophysical Journal, December (Wesley).—Contemporary Review, January (Isbister).—Century Magazine, January (Macmillan).—Natural Science, January (Rait).—Journal of the Chemical Society, December (Gurney).—Zeitschrift für Physikalische Chemie, xviii. Band, 4 Heft (Leipzig, Engelmann).—Fortnightly Review, January (Chapman).—The Humanitarian, January (Hutchinson).—Phonographic Quarterly Review, January (Pitman).—Journal of the Royal Microscopical Society, December (Williams).

CONTENTS.