THURSDAY, DECEMBER 7, 1893.

ELEMENTARY PRACTICAL SCIENCE.

Elementary Course of Practical Science. Part I. By Hugh Gordon, M.A. (London: Macmillan and Co. 1893.)

IN the teaching of science, as of any other subject, the importance of method is most apparent in dealing with the elements. Of late years, since laboratory instruction has been generally introduced into our colleges, the teaching of science to advanced students may be said to be based on correct methods. But so much cannot yet be said for the teaching of the elements of science to young children. If, however, science is to obtain a recognised place in the curriculum of our primary and secondary schools, it is most important that the means adopted for the teaching of science should be educational in character. Very rarely is science so taught to young children and junior pupils in schools as to bring into active exercise the very faculties of the mind which it is supposed to develop. The teaching of science follows too closely the older education, by appealing to the memory, and storing the mind with facts and information of more or less value; and the methods employed involve a mental discipline too similar in kind to that of ordinary mathematics.

Mr. Hugh Gordon, in Part I. of his "Elementary Course of Practical Science," recently published, has broken comparatively new ground. His little book gives a very satisfactory answer to the question: How can the elements of science be so taught as to become a means of *educating* young children? In arranging a course of practical instruction in the rudiments of science, *two* principles have to be observed: first, the instruction must be introductory to science as a whole, and not to any branch of it; and secondly, the aims of the teaching must be strictly educational. In other words, the information imparted must be such as is equally applicable to physics, chemistry, and biology, and the methods must be those by which the student, at every stage of his progress, is enabled to learn by himself. Indeed, the real end of science teaching should be kept in view from the commencement of the study, and the pupil should be exercised, through his science lessons, in accurate observation, and in interpreting the results of experiments.

The book under review is the practical outcome of the experience gained by the author in superintending a course of science teaching in fifteen schools under the London School Board, and is based on a scheme drawn up by Prof. Armstrong for a committee of the British Association. Mr. Gordon has had the advantage of working for some time in the laboratory of Prof. Armstrong, to whom he very readily acknowledges his indebtedness for many valuable suggestions. The book is an endeavour to show how the most elementary science teaching may be made scientific. The author truly says: "Science had much better be left alone altogether than be taught unscientifically"; and it is only too evident that science is often so taught as to be of little or no value in the real work of education. Matthew Arnold said somewhere, "that all learning is scientific which is systematically laid out and followed up to its original sources, and that a genuine humanism is scientific." This is true, and it is because the humanistic studies have been for so many years systematically pursued, that both here and in Germany they have proved more serviceable in teaching scientific method than science itself.

The book before us is a collection of suggestions rather than a text-book or a science primer. It consists of 76 small pages, and contains a few exercises to be worked by the pupils, and for the guidance of teachers. Although an instalment only of a complete course, it indicates very fully the methods to be followed and the objects to be aimed at in teaching science to beginners. The key to the system is supplied in the question, addressed to the pupil, and constantly repeated in the text: "What is it you see? What would you expect to find if the conditions were varied?" and by the reiterated instruction: "Try it for yourself; try experiments to see if this is the case; record your results." In the method of instruction suggested by the author, the pupil is never passive; he is always doing something, and is consequently interested in his work. He is observing, recording, anticipating results, or experimenting. In dealing with the simplest matters, the methods of inductive inquiry are illustrated and practised. The teacher who follows Mr. Gordon does not instruct; he guides and assists his pupils in questioning and interpreting Nature. And although the immediate aim of this instruction is education rather than information—the development and strengthening, by suitable exercises, of certain faculties of the mind, rather than the acquisition of knowledge, the pupil nevertheless gains, in the short course here sketched out, much actual knowledge which cannot fail to prove useful in any kind of practical work. Through the experiments he is made to perform, he learns the metric system, the use of the balance, the mechanical principle of the lever, methods of determining specific gravities, the action of thermometers and of the barometer, facts concerning the expansion of solids and liquids, and applications to every-day phenomena of the principles of solubility and evaporation. Moreover, the child who has gone through this course will have learnt to be observant and accurate, and will have acquired a certain skill in the use of some of the simpler instruments of science. This is no small result of such a short course of lessons.

Between Mr. Gordon's method of teaching the elements of science, and the lectures or lessons illustrated by experiments which answer for science teaching in so many of our schools, there is the widest difference; and by showing in detail how this method may be applied, Mr. Gordon has made a very useful contribution to pedagogic literature. There is very little fault to be found in the subjects selected by the author to illustrate his method; they are nearly all such as are familiar to the pupil in his every-day life. The early and constant use of squared paper is rightly insisted on in most of the exercises. It may be thought that there is too great an advance in the difficulty of some of the exercises towards the end of the book. But possibly this may have been intentional on the part of the author, to encourage the teacher to fill in the breaks in the reasoning, and to prevent the book from being used by teacher or pupils as an ordinary text-book.

The book is clearly printed and illustrated, but would be improved if the numbers of the diagrams were given and referred to in the text. Where more than one diagram is found on the same page, it is not always evident to which diagram the lettering refers. The phrase "addition of interest" might also, with advantage, be changed to some other, less suggestive of commercial arithmetic. These, however, are small defects which can easily be corrected in a subsequent edition. The merit of the book is not in *what* it teaches, but in *how* it teaches; and not the least valuable part of it will be found in the introductory remarks addressed to the teachers.

PHILIP MAGNUS.

THE PYRENEES.

Les Pyrénées. Par Eugène Trutat. (Bibliothèque Scientifique Contemporaine.) (Paris: J. B. Baillière et Fils, 1894.)

IN this volume Dr. E. Trutat gives a sketch, as the full title states, of the mountains, glaciers, mineral springs, atmospheric phenomena, flora, fauna, and man in the Pyrenees, illustrated by woodcuts and diagrams, together with two small maps. The mountains differ from the Alps in their greater simplicity of structure, for they form "the most perfect type of a regular chain." Like the Alps, this consists of an axis of crystalline rocks, granites, gneisses, and schists, flanked on both sides by deposits comparatively unaltered. But there is one important difference: in the Alps, systems anterior to the Carboniferous are only recognised in the extreme east; in the larger part of the chain, rocks of that or of a later age rest on crystalline schists, which must be very ancient. But in the Pyrenees schists truly crystalline are succeeded by great stratified masses which have been much less markedly changed. The most ancient of these are assigned to the Cambrian, though as yet fossils either have not been found, or are too ill-preserved to afford any certain evidences of age. It seems, however, clear that they are older than the Silurian system, for the different members of this can be identified in several places by their characteristic fossils. The Devonian system is well developed, and followed by limestones (marine), conglomerates, and slaty rocks of the Carboniferous period. The occurrence of Permian rocks is considered by Dr. Trutat to be doubtful. Trias, of the Lorraine type, is found, followed by representatives of the various systems in orderly succession up to the Neocomian. Between this and the Cretaceous is a break, then the sequence continues till after the Nummulitic age. Then, as in the Alps, began the great series of movement, of what the present chain is the outcome. Masses of eruptive rock are connected with these disturbances. The enormous beds of conglomerate, called the *Poudingues de Palassou*, which sometimes surpass 1000 metres in thickness, recall the Alpine *nagelfluhe*. Strata partly marine, partly freshwater, represent the Miocene and the Pliocene; the Quaternary deposits presenting a general resemblance to those of the Alps.

The glaciers of the Pyrenees at the present day are comparatively small, the length of the largest not exceeding about 4300 metres, while that of the Great Aletsch is 32 kilometres, but their former extent, as in the Alps, was much greater. They filled the valleys, and even debouched on the lowland; that of the valley of Ariège must have been about 70 kilometres long. The glacial deposits have been assigned to two epochs, and Dr. Trutat claims for the earlier a considerable antiquity. In the Ariège he states that they underlie Pliocene marls, and near the plateau of Lannemezan pass under the Miocene deposits of Sansan. The sections which he gives are very rough, and further proofs of these statements, which involve obvious difficulties, are likely to be demanded. The Pyrenees owe their existence, as has been said, to post-Nummulitic disturbances, but they also afford evidence of great movements, both anterior to the Carboniferous and after the Neocomian, the latter apparently being less marked. Movements occurred after the great post-Eocene elevation, but of much less importance than they were in the Alps. The other topics, mentioned on the title-page, receive due notice, and the volume will be found useful, as it gives, in a concise and convenient form, much information about one of the most important mountain chains in Europe.

T. G. B.

OUR BOOK SHELF.

Les Courants Polyphases. Par J. Rodet et Busquet. (Paris: Gauthier-Villars et Fils, 1893.)

TO those desirous of obtaining a general knowledge of the principles used in the calculation of the efficiency and of the dimensions of polyphase motors, &c., this book will be of considerable use. In the first part, the calculation of the dimensions of, and losses in, the conductors conveying the currents are worked out at some considerable length, numerical examples being given. In the other sections the generation of polyphase currents, motors with rotating fields, and transformers are dealt with; in each case the general principles of the machines now in use being described, though no account is given of the details of their construction. There is also a short account of some of the plants for the transmission of energy by polyphase currents which have been installed, with a table summarising the tests and measurements made during the Frankfort Exhibition.

Solutions of the Examples in the Elements of Statics and Dynamics. By S. L. Loney, M.A. (Cambridge: Camb. Univ. Press, 1893.)

MR LONEY is indebted to a friend for these solutions, and also for the revision of the whole of the proof-sheets. We have glanced through many of the examples, and they seem to be fully and clearly worked out on the whole, very little being taken for granted. Students who cannot depend on the presence of a teacher, will find that with a judicious use of this key much may be self-taught.

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.*]

Sir Henry Howorth and "Geology in Nubibus."

SIR HENRY HOWORTH, in his reply to Dr. Wallace and Mr. LaTouche, concerning the excavating power of ice, remarks that he is "speaking to every man of science, geologist or otherwise." Indeed, from the tone of his letter he would appear to be defending modern science against the attacks of certain unscientific persons who hold extreme views on glacial questions. As one who has taken a great interest in this subject for a number of years, I trust that I may be allowed to add a few words to the discussion.

We are required by Sir Henry Howorth to establish two postulates. "(1) That ice can convey thrust for more than a moderate distance. (2) That glaciers, such as we can examine and report upon, are anywhere at this moment doing the excavating work . . ." Dr. Wallace postulates.

In reply to the first, we have the undoubted fact that in hundreds and thousands of instances striated rock surfaces do occur hundreds of miles from existing glaciers. On this point he remarks: "If glaciers travelled further in former days, it was doubtless because glaciers were larger in former days, because they descended longer slopes, and had larger gathering grounds; that is to say, because the country where they grew was more elevated." So the glacial period resulted from elevation, and all glaciated regions conveniently rose together to produce it, and as conveniently sank down again. I was quite unaware that this was the accepted view. We have no proof whatever that the striated slopes down which the old glaciers moved were steeper in glacial times than they are now. Indeed, the proof is all the other way, and we may consider it as proved that at long distances from their sources, and on comparatively level plains, glaciers have moved, and have polished, ground, scratched and *grooved* the rocks over which they passed. The only point about which there may be legitimate discussion concerns the possible extent of the abrasion.

In his mechanics Sir Henry Howorth is, I am afraid, rather unsound. There are really two factors upon which the possibility of motion in a viscous body depends. One is, of course, the slope of the surface over which it passes, and the other is the slope of the upper surface of the viscous body. Fracture and regelation have little to do with the question, for fracture only occurs near the surface, and fracture must not be confounded with *shear*. Sir Henry Howorth makes one statement which seems to account for the conclusions he has come to. It is "a viscous body, unless the viscosity approaches that of a liquid, cannot move by mere hydrostatic pressure." In fact he assumes, without adducing a particle of evidence in support of the assumption, that there is an inferior limit to the stress required to deform *glacier* ice. I always regarded viscosity as something which retarded motion, but did not in any way interfere with the ultimate result. I have personally made mechanical tests of ice, and also of many thousands of samples of steel, iron, copper, brass, tin, &c. All these substances yield elastically and permanently under stress, some of them under very small stresses, but ice is the only one of them that yields continuously from the moment the stress is applied until it is again removed.

It is not, properly speaking, pressure from behind that forces the ice forward. Ice being viscous, every individual particle moves in the direction of least resistance at a rate depending upon the stress and the viscosity. Sir Henry Howorth may term this "Geology in Nubibus," and call it unmechanical; but I would point out to him that I regard the question from the point of view of a *mechanical engineer*, which I am afraid he does not.

During the past summer I had the pleasure of seeing some of the Norway glaciers, and also of crossing the Folgefond snow field. It was interesting to note that although the streams coming from the hills and uplands free from ice were quite clear, those escaping from the glaciers were charged with sediment. In this connection I would call attention to a calculation made by Prof. G. F. Wright, giving the rate of erosion of its bed by the Muir Glacier. From the volume and turbidity of the water he makes the figure one-third of an inch per annum over the whole of the 1200 square miles of area occupied by the glacier. In fact, erosion goes on much more rapidly when the rocks are covered by moving ice than when they are not. Although we may feel absolutely certain, both by fact and reason, that the erosion beneath glaciers when they are moving with relative rapidity is very great, and be as sure as we reasonably can be of most things that such erosion must result in the formation of lake basins, I am afraid that we shall be unable to satisfy Sir Henry Howorth on the point. We cannot remove a glacier, and if there should prove to be a rock basin below measure of its depth, then replace the ice, and measure again, say, in a thousand years. This is the kind of proof the second postulate seems to demand. R. M. Deeley.

The "Zoological Record."

In reference to the letter of Messrs. Pocock and Bather, on the subject of the *Zoological Record*, in Nature of November 16, I desire to state that the council of this Society (to which the *Zoological Record* at present belongs) quite agree with the above-named gentlemen in their wish to render the *Zoological Record* more complete by combining palæozoology with it. With this view the council some time since addressed the Geological Society, and suggested what in their opinion would be an equitable arrangement for carrying out the plan. This arrangement, however, as will be seen by the copy of the correspondence enclosed herewith, was rejected by the council of the Geological Society. It remains, therefore, for Mr. Bather and such members of the Geological Society as may share his sentiments, to do their best to induce the council of the Geological Society to alter their views upon this question.

I have good authority for stating that the editor of the *Zoological Record* is not really of a different opinion to Messrs. Bather and Pocock on this subject, as would seem to be inferred in their letter. I believe that he only suggested that the palæontologists should start a record for themselves because of the refusal of the Geological Society to co-operate with us in our work. P. L. Sclater.

Zoological Society of London,
3, Hanover Square, London, W.

(Copy.)

To the Secretary, the Geological Society, Burlington House, W.

Dear Sir,—I am instructed by the council of this Society to apply to the council of the Geological Society under the following circumstances:

When the *Zoological Record* (which is now carried on by this Society) was established twenty-eight years ago, it was not considered that palæozoology came within its scope, and the recorders were instructed to notice only such palæontological works as appeared to be "of interest to the student of living forms" in their records. This part of the subject has, however, received a continually increasing amount of attention from the recorders, and the council of this Society, being desirous that palæozoology should in future be treated of in the *Record* as completely as recent zoology, asks the assistance of the Geological Society in carrying out this object.

The Zoological Society bears at present a loss of about £350 per annum, arising from the publication of the *Zoological Record*, and, as the inclusion of palæozoology in an exhaustive manner would materially increase the work of the recorders, and necessitate an addition to their remuneration (which is even at present too small), the council of this Society asks the council of the Geological Society to make a grant of £100 annually towards the expenditure thus incurred.

It is thought by some members of the Council that the Zoological Society, bearing, as it does at present, the whole loss arising from the publication of the *Record*, should not increase its expenditure thereon, and the sum mentioned above, £100, would, it is estimated, be sufficient to relieve this society from the additional expense that the inclusion of palæozoology, in its record of zoological literature, would involve. In return for this assistance, the council of this Society will make every effort to treat the subject of palæozoology exhaustively, and will add to each *Record* a reference to palæozoological memoirs stratigraphically arranged, besides dealing with those memoirs in detailed analysis in the systematic records.

In order that the interests of palæozoology may be more carefully attended to, the council of this Society will undertake to place a nominee of the Geological Society upon the committee of their body appointed every year to supervise the publication of the *Zoological Record*.

Should these proposals meet with acceptance from the Geological Society, the council will further undertake to place one hundred copies of the *Zoological Record* at the disposal of the Geological Society, and, if it be wished, will alter the title of the *Record* to *The Record of Zoological and Palæozoological Literature*, from *The Record of Zoological Literature.*

Trusting that these proposals will meet with the approval of your council, I am, dear sir,
Yours faithfully and obediently,
(Signed) P. L. Sclater,
January 21, 189 Secretary.

(Copy.)

From the Geological Society, Burlington House, W.

Dear Sir.—Your communication, dated January 21, 1893, was this day submitted to the council of the Geological Society, and I was asked by the council to inform you that they regretted that they were unable, in the present state of the Society's income, to recommend to the Fellows of the Geological Society an increase of expenditure such as would be necessitated by acceding to your request that a grant of one hundred pounds should be made to aid the publication of the *Zoological Record*.

Whilst regretting their inability to comply with your request, the council thank you for the conditional offer which accompanied it. I am, dear sir,
Yours faithfully and obediently,
(Signed) John E. Marr,
February 22, 1893. Secretary.

The Proposed Continuous Polar Exploration.

Your excellent summary of the proposed continuous Polar exploration (November 2, p. 18) conveys a wrong impression in its closing sentence. The system may in the future assume large proportions; but the *beginning*, to be made next year, *will be very small.* It will consist merely in the establishment of the principal station at the south-east angle of Ellesmere Land, and 80 days' exploration along the west coast of that land. At most, an advanced depôt, erected some 100 miles farther west, may be so fitted out as to serve at once as a secondary station. It is not easy to see why this work should be postponed till Peary and Nansen have returned. Their fields are far from ours, and their results can shed no light on the area west of Ellesmere Land. As well might you say that the exploration of the Mediterranean should not be begun until that of the Baltic was completed.

As you say, the possibility of continuous Polar exploration is not demonstrated. There can be no doubt, however, of the value of a permanent station at the entrance of Jones Sound, nor of the practicability of its maintenance, so long as the whalers continue to visit that region. How far exploration may be carried with that station as a base, it is impossible to foretell, but at any rate the existence of a secure base will be an advantage possessed by no previous expedition in that direction, and, in the words of the "Encyclopædia Britannica," will "make disaster on a large scale, humanly speaking, impossible."

U.S. Geological Survey. Robert Stein.

On the Classification of the Tracheate Arthropoda.—A Correction.

In No. 423 of the *Zoologische Anzeiger* (1893) I ventured to propose a new classification of the Tracheata, including under this heading those Arthropoda that are usually known as myriopods and insects. The principal changes suggested were the abolition of the name Myriopoda as indicating an unnatural assemblage of beings and the union of the *Chilopoda*, *Symphyla*, and *Hexapoda* in a division (Opisthogoneata), which was based upon the situation of the generative apertures at the hinder end of the body. But in referring the *Symphyla* to this category by adopting the assertions of Menge and Latzel respecting the position of the orifices in question, it appears that I fell into error; for Dr. Erich Haase has kindly written to me from Bangkok, with the information that by means of a series of transverse sections he was able, although with considerable difficulty, to confirm Grassi's statement to the effect that the generative apertures in *Scolopendrella* are situated upon the fourth body-segment. This genus is therefore progoneate, like the Diplopoda and Pauropoda; but whether it should be ranged with these two classes, or occupy an independent position between the Progoneata and Opisthogoneata, is a question for future discussion.

R. I. Pocock.

THE LOSS OF H.M.S. "VICTORIA."[1]

II.

We dealt last week with the circumstances relating to the loss of H.M.S. *Victoria*, and the causes of her sinking with such startling rapidity after she was rammed. The facts, so far as they are known, are fully and, in our opinion, fairly summarised by Mr. W. H. White, in No. 3 of the Admiralty Minutes, just issued; and Mr. White demonstrates clearly, from the results of calculations made in the Construction Department of the Admiralty, that the movements and behaviour of the ship after the accident, and the observed effects upon her line of flotation and her stability, are precisely what would be caused by the entry of water into the compartments at the fore end of the ship, which are known, or believed, to have been filled before she foundered. These calculations serve, therefore, the useful purpose of showing that the water known to have entered those forward compartments that were proved, by evidence given before the Court Martial, to be filled, was quite sufficient to account for the subsequent capsizing and sinking of the ship; and for the capsizing and sinking to happen exactly in the manner that was observed. This is so, as already stated, whether Mr. White be absolutely right or not with regard to the precise state of each separate compartment after the damage; as the evidence is sufficiently conclusive, upon the whole, respecting the various compartments, to reduce the probability of error to a very small amount, such as would not materially affect the practical results of the demonstration.

The Admiralty calculations thus remove all reasonable doubt as to whether the compartments known to have been filled were sufficient in themselves to account for the final disaster; and they make it unnecessary, in order to explain what happened, to speculate as to the probability of the collision having been more far-reaching in its effects upon the structure, or internal arrangements, of the ship than the evidence indicates. The evidence, as it stands, is shown to completely account for the facts; and to furnish a solid basis for investigation, or argument, as to the lessons that may now be learned from the loss of the *Victoria*.

The Lords Commissioners of the Admiralty, in the first of the three Minutes lately issued, dated October 28 last, on the finding of the Court Martial, stated that the question of closing the water-tight doors of the *Victoria*, and the construction and stability of the ship, would be dealt with separately. Their lordships accordingly issued the second Minute, dated October 30. This Minute states that, in consequence of the Court Martial finding "that it does not feel itself called upon, nor does it feel itself competent, to express an opinion as to the causes of the capsizing of the *Victoria*," their lordships instructed the Director of Naval Construction to make a thorough examination and analysis of those parts of the evidence which throw light on these points. The report prepared by Mr. White, in accordance with these instructions—No. 3 of the present Minutes—was dealt with in our article of last week; but we then left over for subsequent consideration the references made in the Minutes to the lessons taught by the various circumstances of the case.

These points being dealt with authoritatively in the second Admiralty Minute, dated October 30, we shall deal principally with that Minute in the following remarks. It commences by adopting the figures and the conclusions stated in Mr. White's report with regard to the nature of the blow received by the *Victoria*, the after movements and behaviour of the ship, the extent to which water found access into her, and the effect of such water upon her flotation and stability. We have nothing

[1] Continued from p. 104.

further to say upon the subjects dealt with in this portion of the Minute, which appears to accord with the evidence, and also with the known effects that would be produced by filling the compartments that were opened up directly to the sea, or into which water could pass freely through open doors, hatches, &c.

The Admiralty Minute next expresses the opinions of the Board upon the following points; and we will take these in the order named in the concluding paragraph of our former article: (1) The effect of longitudinal bulkheads upon the capsizing of the ship; (2) what would probably have happened if the doors and ports in the upper-deck battery had been closed; (3) what would probably have happened if all doors, hatches, &c. had been closed before the collision took place; (4) the efficiency of the water-tight doors to the bulkheads, and the means of closing them quickly; (5) the value of an armour belt at the ends for resisting damage; (6) the sufficiency of the stability possessed by the ship; and (7) the steps that should be taken "to prevent the recurrence, under similar circumstances, of the conditions which, after the collision, resulted in the loss of the ship."

1. *The effect of longitudinal bulkheads upon the capsizing of the ship.*—Mr. White points out that there was no continuous central longitudinal bulkhead in the *Victoria*. In the stokeholds and engine-room there were two such bulkheads on opposite sides of, and each several feet from, the centre line; but these were far abaft the damaged portion of the hull, and do not appear to have been reached by water that entered the ship up to the moment of sinking. There were a few longitudinal partitions in the fore part of the ship; but some of these were inoperative because of damage or open doors. The effect of filling the compartments formed by these longitudinal partitions has been calculated, and it is stated that this would only cause an inclination of about 5° in the intact condition of the ship. This result does not, however, bear directly upon the actual effect produced in such circumstances as are being considered, because the damage caused by collision not only admitted water into the ship, but it reduced, at the same time, her power to withstand the heeling effect of the excentric compartments that were thereby filled. The ship would only have heeled about 5° with these compartments filled, if the hull had been uninjured; but if the hull had been uninjured, the compartments would, of course, not have been filled. Mr. White goes on to say: "It appears on investigation that in the damaged condition and at the extreme position which the *Victoria* occupied before the lurch began, the flooding of the compartments enumerated, and the accumulation of water on the starboard side, account for the observed angle of heel, 18 to 20 degrees." This inclination—which is what was really due, in the circumstances, to water being held over to starboard by the longitudinal partition above referred to; as the accumulation of other water to starboard was merely the consequence of the heeling thus caused—must have allowed the sea to enter the ports and door of the upper deck battery sooner than it otherwise would have done, and thus have hastened the capsizing of the ship. The Admiralty Minute states that "the evidence clearly shows that the existence of longitudinal water-tight bulkheads in the *Victoria* was not the cause of her capsizing. There were only a few minor longitudinal partitions in the fore part of the ship. Many of these were inoperative because of damage or open doors."

This conclusion is doubtless correct so far as it relates to the continuous longitudinal central bulkhead to which the capsizing of the ship was prematurely, though confidently, attributed by certain hasty critics, because such a bulkhead did not exist in the forward part of the ship that was affected by the collision. It clearly does not apply, however, to the minor longitudinal partitions above referred to, because these must have been contributory to the disaster according to the extent by which the water they held over towards one side caused the heel of the ship to increase as the bow became immersed, and the stability diminished. It is a question that could only be settled by further investigation, whether the reduction of stability and the heeling effect thus caused was greater or less in this particular case than would have occurred if the water had been free to pass from side to side of the ship within the fore-and-aft limits of the compartments it entered.

2. *What would probably have happened if the doors and ports in the upper-deck battery had been closed.*—Mr. White says: "It is not possible to state absolutely that the *Victoria*, with turret and battery closed, could have been kept afloat permanently under the actual circumstances of the collision"; and he points out that there are many compartments into which water might have found its way eventually, through doors and hatches that were probably open. He considers, however, that "her capsizing would have been improbable even if she had eventually foundered." The Admiralty endorse this opinion in their minute, which states: "The great weight of water thus gradually admitted into the forward part of the ship might eventually have caused the ship to founder by the head."

We see no reason to believe that the ship could possibly have been saved by the closing of these doors and ports. By the time the sea had reached them the fore end of the ship was so deeply submerged, and there were so many openings by which water could then find its way into compartments not already filled, that it is difficult to conceive how even the rate at which she was so rapidly sinking could be checked. When the sea had reached the height of the turret ports and the upper-deck battery ports and doors, the ship was inevitably doomed. She might possibly have sunk by the head without capsizing, although this appears improbable. With her stability reduced to the extent described in the Admiralty Minutes, when the bow was under water, and the heel to starboard was great, it would appear that the effect of the increasing quantities of water in the ship would certainly be to capsize her very soon. But whatever might have been the precise manner in which she would have gone down, there appears no doubt that the vessel would have gone to the bottom almost immediately after she did, even if the turret and upper-deck battery ports and doors had been closed.

3. *What would probably have happened if all doors, hatches, &c., had been closed before the collision took place.*—We agree upon this point with the opinion, based upon the calculations of the Construction Department, which is expressed in the Admiralty Minute as follows: "While the loss of buoyancy must in that case have been considerable, yet, making all due allowances for probable damage, the ship would have remained afloat and under control, and able to make port under her own steam. Her bow would have been depressed about to the water level, her heel to starboard would have been about one-half of that observed before the lurch began (*i.e.* 9 or 10 degrees), her battery ports would have been several feet above water, and she would have retained ample stability."

4. *The efficiency of the water-tight doors to the bulkheads, and of the means of closing them quickly.*—This question is one of the greatest importance in the present case, because, as we have seen, the *Victoria* might apparently have been navigated safely into port if all the water-tight doors, hatches, &c. had been closed soon enough to prevent water passing from compartments directly opened up by the collision into others from which they were separated by water-tight bulkheads or decks. The Admiralty expresses strong and unqualified opinions upon this point. Their lordships say "the detailed evi-

dence establishes the fact that water-tight doors, hatches, &c. in the *Victoria* were in good order. It contains nothing which suggests a doubt of the efficiency of the system of water-tight subdivision existing in the *Victoria*. At the parts affected by the collision the subdivision was minute, but doors were left open. According to the established practice of the Admiralty in all classes of ships, the number of water-tight doors is made as small as possible consistently with the essential conditions for working and fighting the ship. . . . In conclusion, their lordships are of opinion that . . . . the arrangements of water-tight doors . . . . did not by any fault of principle contribute to the loss of the ship; but that, on the contrary, had the water-tight doors, hatches, and ports[1] been closed, the ship would have been saved." Mr. White says, in his Minute:-" No orders were given to close doors until one minute before collision. It is established by the evidence that the doors, &c. were in good order. The failure to close doors, therefore, was due entirely to the insufficiency of the time available, especially in compartments breached by the collision."

The statement that the water-tight doors, hatches, &c. were in good order at the time of the collision appears justified by the evidence; except, perhaps, with regard to the door at the after end of submerged torpedo room, which slides horizontally, and could only be moved six or eight inches when the attempt was made to close it after the collision. Their lordships go on to say that the detailed evidence contains nothing which suggests a doubt of the efficiency of the system of water-tight subdivision. We cannot discover, however, that this question was investigated by the Court Martial. Very complete evidence was obtained as to the exact state of each compartment, and of each opening into the compartments, at the time of the collision; but the general question of the efficiency of the system of water-tight subdivision, which involves that of the water-tight doors and hatches to the various compartments, was not gone into. It would appear, indeed, to have been expressly excluded from the investigations of the Court Martial, since it can only be judged in relation to the buoyancy and stability of the ship; and the Court confined itself, as already stated in the quotation given from the Admiralty Minute, to placing upon the Minutes all evidence obtainable with regard to the closing or otherwise of water-tight doors, &c., but did not feel itself called upon, nor feel competent, to express an opinion as to the causes of the capsizing. While it may therefore be true that the evidence contains nothing which suggests a doubt upon these points, it is, on the other hand, equally true that it contains nothing which proves the assertion that the system of water-tight subdivision was efficient.

One of the weak points in the water-tight subdivision appears to have been the doors and hatches to openings in the bulkheads and decks; and especially the impossibility of closing a sufficient number of them after the collision to keep the ship afloat. The doors upon the mess deck were all closed; but this deck was about 3 feet above the water-line, and there was time to attend to the doors upon it before the inrush of water drove the men away. On the protective deck below, however, and on the platform in the hold, there was not time to get at all the doors and hatches before the water reached them; while most of those that were got at and closed appear to have been only partially, and very imperfectly, secured. The plans of H.M.S. *Victoria*, appended to the Admiralty Minutes, show ten water-tight doors in the bulkheads on the protective deck, at the fore side of the armour belt. This deck is about 100 feet in length, and includes the whole of the area directly affected by the collision; and there is only one important bulkhead in this space which does not contain a door, viz. that which divides the cable locker from the fresh-water tanks. On the platform in the hold, immediately under the protective deck, there are eight water-tight doors in the bulkheads, while there is in addition a water-tight door in the bulkhead at Frame Station 35, which forms the after boundary to the space. This was the door which could not be closed when the attempt was made to do it. There is no bulkhead upon this deck in the space referred to which does not contain a door. Besides these doors there are numerous openings, fitted with water-tight hatches, in the decks over the various compartments.

The Admiralty Minute states that the number of water-tight doors was made as small as possible, in accordance with the established practice of the Admiralty. It would be difficult, however, to fit more doors than are shown upon the plans of the two decks that are below the water-line in the *Victoria*—the protective deck and the deck below it in the hold.

Judging by the Admiralty plans, it was only a certain number of these water-tight doors that were fitted so as to slide horizontally; and some were merely hinged doors, which could only be closed by entering the compartment in which they were situated, and were secured by a number of clips round the edge of the door. Some of these were upon the most important transverse bulkheads, such as the two bulkheads which enclosed the submarine mining flat on the platform in hold. We have always considered that arrangements should be made for closing all doors in bulkheads that are essential to the efficient water-tight subdivision of the ship from a deck that is at a safe height above water, as well as in the compartments where the doors are; and we believe, also, that this is the Admiralty rule—as it obviously ought to be. If doors are fitted below the water-line so as only to be opened or closed in the compartments where they are, they should seldom require to be opened, and never to be left open, unless the bulkheads to which they are fitted are not considered essential to the efficiency of the water-tight subdivision. It does not appear by the evidence, or by the Admiralty Minutes, that a single one of the many doors in the fore part of the ship on and below the protective deck could be closed from a deck at a safe height above water; because the sliding doors could only be closed, we believe, from the main deck, which does not appear to have been more than 3 feet above water at the time of the accident, and was almost instantly immersed. In view of these circumstances we cannot agree with the opinion of the Admiralty that there is "nothing which suggests a doubt of the efficiency of the system of water-tight subdivision existing in the *Victoria*. It appears, upon the other hand, quite practicable to improve the efficiency of this system by dispensing with some of the doors, and by arranging with reference to the others that every one which requires to be left open for even an instant, without the certainty of some one being in constant attendance upon it till it is closed, should be capable of being worked from a deck at a safe height above water.

Mr. White says that the failure to close the water-tight doors in the forward part of the *Victoria* has caused suggestions to be made that automatic or self-closing doors should be adopted instead of existing arrangements. This suggestion was, he adds, carefully considered long ago, after certain experimental doors had been tried. He is satisfied that the existing arrangements are the best, and that their safety is only a question of good time being allowed for closing the doors. It must be remembered, however, that when doors can only be closed in the compartments where they are situated, and these are below the water-level, the inrush of water would often effectually prevent the closing of the doors in bulkheads that separate the compartment that is breached from those

[1] It has already been pointed out that the closing of the ports would apparently have had but little effect, and the Admiralty admit that the ship might still have foundered.

adjacent to it. Also, with such arrangements below as those of the *Victoria*, it is impossible to ensure that an unforeseen accident would always allow of sufficient time to close the water-tight doors in the manner required.

The efficiency of the water-tight hatches, and the chances of their being properly secured in an emergency when they are fastened by a number of clips round the edge, as at present, is also a question that appears to require consideration; while it is to be observed that the sliding horizontal door in the protective deck of the *Victoria*, which opened into a shoot through which coal was trimmed from the reserve bunkers at after end of protective deck, into the side bunkers in the stokehold, could not be closed from the shoot in which the men worked who were trimming the coal; but had to be worked from the submerged torpedo room, a compartment below the protective deck. This open door had an important effect upon the capsizing, for Mr. White states that "one of the chief causes of inclination to starboard is to be found in the fact that, owing to open doors, water was able to find its way from bunkers above the protective deck, down through the coal-shoot, and so to fill No. 7 bunker just before the forward starboard stokehold."

It appears to us that one of the chief lessons taught by the circumstances of this disaster, is the necessity of reducing the number of water-tight doors and hatches, and of arranging that all of them which are essential to the efficiency of the water-tight subdivision, and are ever likely to be left without attendance while open, should be capable of being closed, either by a thoroughly satisfactory self-acting arrangement, or by appliances for working them from a deck at a safe height above water.

The points still remaining to be considered will be reserved for our next article.

FRANCIS ELGAR.

REAPPEARANCE OF THE FRESHWATER MEDUSA (LIMNOCODIUM SOWERBII).

FOR three years nothing has been seen of the freshwater medusa in the Regent's Park, and naturalists had given up hope of carrying on any further investigation into its life-history. It seemed as though this beautiful little organism—brought we know not how or whence into the midst of London—had, like some mysterious comet, unexpectedly burst on the zoological world, and as unexpectedly disappeared.

I was, therefore, greatly astonished to hear in September, from my friend the Director of Kew, that the curator of the Sheffield Botanic Gardens (Mr. Harrow) had discovered it in quantity in the Victoria Regia tank under his care during the present summer, and I was soon after delighted by the safe arrival from Sheffield of a bottle containing living well-grown specimens of the familiar jelly-fish. Mr. Harrow informs me that he observed it in the tank at Sheffield for the first time in the beginning of June of this year (1893). Specimens were still observed as late as the middle of October—giving a duration of some fourteen weeks—an unusually long period. Mr. Harrow estimates the total number seen as at least 300.

The last seen in the Botanic Gardens, Regent's Park, London, were taken from the *new* Victoria Regia tank on July 30, 1890. The question as to how the jelly-fish got to Sheffield is easily answered. Water plants (Nymphæaceæ and Pontederia) were sent (as I am informed by Mr. Sowerby and by Mr. Harrow) from Regent's Park to Sheffield to re-stock the tank there on April 4, 1892, and on April 7, 1893. Hence there was the probability of some of whatever reproductive germs of Limnocodium existed in Regent's Park being transferred to Sheffield. The curious thing is that in 1892 and in 1891 no Limnocodium were seen in the original source—viz. the Regent's Park tank—nor in 1893, excepting a few sent from Sheffield and placed in that tank by Mr. Sowerby.

This is the first instance recorded in which another Victoria Regia tank has been "infected" with Limnocodium from the original Regent's Park tank, excepting when the new tank in Regent's Park was in 1890 infected from the old one—by the transference to it of weeds and roots containing the germs of the jelly-fish.

The tank at Kew has never been properly infected, for it is, I regret to say, the anti-zoological custom at the Royal Gardens to thoroughly cleanse, wash, and furbish up the Victoria Regia tank every year so thoroughly that the winter germs of the jelly-fish are removed or destroyed. Hence Limnocodium has flourished at Kew when sent there from Regent's Park, but has never "carried over" from one season to another. It is, fortunately, the custom in other botanical gardens to leave a quantity of "sludge" (including some old leaves and stems) at the bottom of the tank, when the water is drawn off and the soil prepared for a new season, and hence Limnocodium has been preserved from destruction for so many years.

As to what is the precise nature of the process by which Limnocodium has been carried over from one season to another in the Regent's Park, we are still uncertain. The facts at first ascertained were these, viz. that the jelly-fish suddenly appear each year as early as April or as late as August, and remain for from five to twelve weeks, when they die down and absolutely disappear. During the first few weeks of their appearance the water is found to contain an immense number of minute young forms ($\frac{1}{30}$ of an inch in diameter), which I described and figured in the *Quart. Journ. Micros. Science*, vol. xxi. p. 194. Evidently these young were being produced in quantity in the tank, and gradually developed to the full size of half an inch diameter. The form and appearance of these young were such as to lead me to the conclusion (subsequently found to be erroneous) that they had been developed from eggs. At the same time the remarkable fact was established by the examination in successive years of many hundred specimens that the adult *Limnocodia* were every one, without exception, males. They produced abundant motile spermatozoa, but not a trace of an egg-cell was ever found in any one of them!

The hypothesis which I entertained in 1884 as an explanation of this curious state of things was—that the female was a non-motile, perhaps a fixed hydriform organism, and I accordingly searched for such a form in the mud and *débris* from the bottom of the tank. At last, in a large quantity of such material which I obtained when the tank was cleared out in the winter of 1884, my assistant, Dr. A. G. Bourne, found a very strange diminutive polyp adhering in numbers to the root-filaments of Pontederia. This polyp he carefully described in the same year in a communication to the Royal Society. There was very great probability that this little polyp, devoid of tentacles, and not more than $\frac{1}{8}$th of an inch long, was the "trophosome" of the Limnocodium medusa. That this was a true inference was subsequently proved by Dr. G. H. Fowler, who in 1890 (*Quart. Journ. Micros. Science*, vol. xxx.), the last year in which the jelly-fish were seen in London, showed that the little spherical young found floating in the water of the tank are nipped off by a process of transverse fixion from the free ends of the minute polyps described by Bourne.

Fowler (whose observations were made in my laboratory in 1888) found the polyps very abundantly upon floating water-plants widely scattered in the tank; they were also detected by Mr. Parsons, of the Quecket Club, in water which was the overflow of the tank, and accumulated in an outside reservoir.

The immediate question then became "How do the polyps originate?" The polyps account for the medusæ, but whence do they themselves originate? And this question still remains unsolved. The polyps are observed to increase by budding, but they never form clusters of more than four "persons." How do they become distributed over the under surface of nearly all the floating leaves in the tank? How do they get carried to an outside reservoir? Is it not improbable that they would continue year after year to propagate themselves by budding as polyps, and in the summer to throw off successive crops of *male* medusæ? It is possible that this is the whole history, but not quite probable.

In any case, however, the existence of the minute polyps attached to water-plants is sufficient to explain the introduction of the jelly-fish to Sheffield. It also is sufficient to explain the original introduction of the jelly-fish to the Regent's Park, since in 1878 (two years before the first discovery of the jelly-fish) specimens of a remarkable water-plant (*Pontederia*) were brought from Brazil by a lady and presented to the Botanical Society, and placed in the Victoria Regia tank.

A new interest has recently been added to that already attaching to Limnocodium by the description of another fresh-water jelly-fish, the *Limnocnida Tanganyisiæ*. This remarkable form was worked out in my laboratory in Oxford during last winter by Mr. R. T. Günther, who received the specimens from his father, Dr. Günther, F.R.S., of the British Museum. Dr. Günther had written to the Mission on Lake Tanganyika in order to procure the specimens. Individuals of three kinds are described by Mr. Günther, viz. males, females, and a-sexual individuals which produce crops of buds on the manubrium (see his papers in the *Ann. and Mag. Nat. Hist.*, 1893, and in the forthcoming number of the *Quart.-Journ. Micros. Science*). Whilst differing greatly from Limnocodium in most respects, Limnocnida agrees with it, in a most extraordinary way, in the minute structure of the marginal sense-organs. No light is thrown by Limnocnida on the problem of the life-history of Limnocodium.

I subjoin a list of dates in reference to the history of Limnocodium, and may add that the columns of NATURE already contain numerovs communications relative to it, viz. in vol. xxii. (1880), pp. 147, 177, 178, 190, 218, 241, 290, and in vol. xxxi. p. 107.

1880.—June 10, first observed in Regent's Park; remained six weeks.
1881.—June 12; reappeared; remained five weeks.
1882.—None observed.
1883.—April 28; twelve weeks.
1884.—April 27; twelve weeks (?).
1885.—April 5 (no record of duration).
1886.—August 7 (no record of duration).
1887.—End of May (no record of duration).
1888.—May 10 (no record; very few observed).
1889.—None.
1890.—New tank constructed and stocked; July 10 a few.
1891.—None.
1892.—None. Plants sent to Sheffield April 4.
1893.—None in London. Plants again sent, April 7, to Sheffield.
1893.—June 7 to mid-October, large numbers observed in tank at Sheffield.
Hydroid trophosome discovered by Bourne in winter of 1884.
Production of medusæ by hydroid, observed by Fowler, in May, 1888.

E. RAY LANKESTER.

DEATH OF PROF. TYNDALL.

ANOTHER of our "Scientific Worthies" has "crossed the bar," leaving behind an honoured name and works that will perpetuate his memory. On Monday evening Prof. Tyndall passed away at his residence, near Haslemere. For some time previous he had been suffering from insomnia and rheumatism, and very unfavourable symptoms set in on Monday morning. He quickly became unconscious, and except for a brief interval at midday, remained in this state until half-past six o'clock, when a peaceful change from life to death took place. It appears that the cause of death was an overdose of chloral, which Prof. Tyndall took as a sedative against insomnia. He had been in the habit of taking narcotics for several years past in order to overcome the sleeplessness from which he suffered. On Monday about the usual quantity was administered to him, but his greatly weakened condition was unable to bear so much. The inquest on the body, which was considered necessary by the doctors, was held yesterday.

A detailed account of Tyndall's life was given in these columns in August, 1874, so it is only necessary to trace now a brief outline of his career. He was born in 1820, at Leighlin Bridge, near Carlow, in Ireland. But it was not until 1847 that he began his career as a teacher of science, by accepting a post in Queenwood College, Hampshire, where Dr. Frankland was chemist. A year later the two friends did what every young man of science should do, if possible—they went together to a German University, the University of Murburg, Hesse Cassel, rendered celebrated by Bunsen and others; and to Bunsen, whose lectures he attended, and in whose laboratory he worked, Tyndall was never tired of expressing his obligations. He was at Murburg when Knoblauch, preceded by a distinguished reputation, and accompanied by a choice collection of instruments, went there as Extraordinary Professor. Subsequently, in conjunction with Knoblauch, Tyndall carried on his "classic" inquiries in connection with diamagnetism, afterwards prosecuting his research in the laboratory of Prof. Magnus at Berlin. In 1851 his life-long friendship with Prof. Huxley commenced, and in the following year he was elected a Fellow of the Royal Society. In February, 1853, he gave the first of his eloquent Friday evening lectures at the Royal Institution. Shortly afterwards, at the proposal of Faraday, he was appointed Professor of Physics in the Institution, a post from which he retired in 1887. The managers and members of the Institution marked their sense of the benefits he had conferred upon it by electing him as Honorary Professor, a title previously borne by Davy and Brande, and by calling one of the annual courses of lectures "The Tyndall Lectures." His bust was also placed in the Institution in memory of his relations with it.

A complimentary dinner was given to Tyndall on the occasion of his retirement from the Chair of Physics in the Royal Institution. The body of eminent men which met at the dinner was such as has seldom if ever been brought together to do honour to a man of science, and when the chairman, Sir George Stokes, the then President of the Royal Society, gave voice to the desire of the company that their guest should long enjoy the leisure which he had so well earned, it was not thought that after but six years of rest from labour he would be called away. The speeches made at the dinner are reported in NATURE, vol. xxxvi. p. 222, and they show the high regard in which Tyndall was held throughout the world of science, art, and letters. In responding to the toast of the evening, he gave an account of his life, including in his speech the following true remarks:—"To keep technical education from withering, and to preserve the applications of science from decay, the roots of both of them must be imbedded in the soil of original investigation. And here let it be emphatically added, that in such investigation practical results may enter as incidents, but must never usurp the place of aims. The true son of science will pursue his inquiries irrespective of practical considerations. He will ever regard the acquisition and expansion of natural knowledge—the unravelling of the complex web of nature by the disciplined intellect of man, as his noblest end, and not as a means to any other end." This was the kind of spirit that actuated Tyndall throughout his career. It was well shown in 1872, when he placed the balance of 13,000 dollars, that remained after his lecturing tour in the United States, in the hands of a committee who were authorised "to expend the interest in aid of students who devote themselves to original research."

It would be superfluous for us to enumerate Tyndall's explorations in the domain of science, or to expatiate upon his remarkable power of presenting a subject both in speech and in writing, for among men of science these facts are common knowledge. To such men as he—not only original discoverers, but also popular and powerful interpreters of scientific fact—we owe much of the advancement that has been made during the last forty years.

NOTES.

MR. H. H. TURNER, of Greenwich Observatory, has been elected to the Savilian Professorship of Astronomy at Oxford, in succession to the late Prof. Pritchard.

THE Russian traveller Potanin, who has spent more than a twelvemonth in a botanical exploration of Thibet, is expected in St. Petersburg in January next. M. Dobrotworsky has arrived at Jenisseisk on the Jenissei, on a botanical expedition.

PROF. BEN. K. EMERSON, of Amherst College, and of the U.S. Geological Survey, who met with a serious railroad accident last summer, and was reported killed, has so far recovered that he started in November on a trip round the world, for rest and recuperation. He visits Italy, Egypt, India, Java, and Japan. Prof. Emerson has been engaged for a long time in mapping the crystalline rocks of Central Massachusetts and Connecticut.

DR. NICOLE has been appointed Director of the Bacteriological Institute of Constantinople.

DR. SEUBERT has been appointed Professor of Analytical and Pharmaceutical Chemistry in the University of Tübingen.

MR. W. F. C. GURLEY has been appointed Director of the Geological Survey of Illinois.

WE learn that Prof. D. A. Gilchrist has accepted the Professorship of Agriculture at the University Extension College, Reading.

DR. K. VON DALLA TORRE has been appointed Professor of Botany in the University of Innsbrück, and Dr. H. Möller Professor of Botany in the University of Greifswald.

MR. W. T. MCGEE, known for his contributions to geology, has been appointed Director of the Bureau of Ethnology at Washington, U.S.

THE Chair of Comparative Anatomy and Zoology in the Biological School of the University of Pennsylvania has been accepted by Prof. E. D. Cope, and that of Geology and Mineralogy by Prof. A. P. Brown.

THE death of Dr. Webb, the well-known Principal of the Aspatria Agriculture College, is a severe loss to agricultural education. After a very brief illness, he passed away on November 28, in the prime of life. Through his exertions the College at Aspatria has been raised from a very low condition to its present high standing. He was greatly respected by his students, and his place as a teacher of agriculture will not be easily filled.

THE first step towards the introduction of the decimal system into Russia will be taken on January 13, 1894, when, by order of the Czar, the chemists of the Russian empire will begin to use decimal weights and measures.

A PRIZE of 1800 liras is offered by the Italian Geological Society for the best account of the state of knowledge of Palæozoic and Mesozoic formation in Italy, the work to be in continuation of D'Archiac's "Histoire des progrès de la Géologie," and to be presented before the end of March, 1896.

Die Natur announces that the Berlin Academy of Sciences has granted Drs. Richarz and Krygar-Menzel two thousand marks for the carrying on of their investigations of the constant of gravitation. A like sum has been granted to Dr. Franz Reinecke for the furtherance of his ethnological and anthropological studies.

THE ninth congress of Russian Naturalists will be opened at Moscow on January 15, 1894. The Mathematical and Physical Faculty of the Moscow University has undertaken its organisation. Reductions of railway fares are offered to persons who will apply for that purpose to the Dean of the Faculty before December 13. The first general meeting of the congress will take place on January 16, and the conference will close on the 23rd of that month.

MR. C. M. IRVINE informs us that at four o'clock on the afternoon of December 4 a brilliant meteor passed over Lesmahagow, N.B., travelling true south. The altitude was about 45°. The arc through which it was visible was about 10°, and the duration of visibility nearly 3 secs. Colour, pale greenish blue. The sky was overcast with detached clouds. The passage of the meteor was slightly zigzag, deviating from a straight line by about 1° on either side.

THE second series of lectures given by the Sunday Lecture Society begins on December 10, in St. George's Hall, when Sir Benjamin W. Richardson, F.R.S., will lecture on "The Mastery of Pain." Lectures will subsequently be given by Prof. A. A. Rambaut, Royal Astronomer of Ireland; Dr. R. D. Roberts, Prof. Percy Frankland, F.R.S., Mr. C. T. Dent, Mr. Arthur W. Clayden, and Prof. R. Meldola, F.R.S.

THE following are among the lecture arrangements at the Royal Institution before Easter:—Prof. Dewar, six lectures (adapted to a juvenile auditory) on air, gaseous and liquid; Prof. Charles Stewart, nine lectures on locomotion and fixation in plants and animals; Mr. W. Martin Conway, three lectures on the past and future of mountain exploration; Prof. Max Müller, three lectures on the Vedânta philosophy; the Right Hon. Lord Rayleigh, six lectures on light with special reference to the optical discoveries of Newton. The Friday evening meetings will begin on January 19, when a discourse will be given by Prof. Dewar, on scientific uses of liquid nitrogen and air. Succeeding discourses will probably be given by Mr. A. P. Graves, Mr. T. J. Cobden-Sanderson, Prof. W. F. R. Weldon, Prof. Silvanus P. Thompson, Prof. John G. McKendrick, Dr. W. H. White, the Right Hon. Lord Rayleigh, and others.

ACCORDING to the *Times* correspondent at Cairo, Messrs. Garstin and Willcocks have inspected the four sites proposed for reservoirs in which to store water for irrigation purposes during the summer when the Nile is low, and their reports will shortly be presented. The Government will then invite three European hydraulic engineers of the highest reputation to come to Egypt and make a technical examination of the proposed schemes. This will probably be in February next. Three of these schemes are for the construction of dams across the river at either Kalabsheh, Assouan, or Silsileh; the fourth proposes to utilise the natural depression of the Wady Raian, in the province of Fayoum, by conducting into it the flood-water of the Nile.

THE London County Council some time ago decided to establish a pathological laboratory and museum in connection with the London lunatic asylums. Last week the Council accepted the plans prepared by Mr. G. T. Hine, and we understand that they will shortly be put into execution at Claybury. A competent pathologist is now to be appointed, who will be supplied with material from the Claybury and other asylums under the supervision of the London County Council. The necessity for such a laboratory has long been felt, and although good work has been done in several asylums by enthusiastic workers, these investigations have hitherto been carried out at a great disadvantage, chiefly owing to the want of assistance on the part of the governing bodies. So great have been these difficulties that in many asylums pathological science has been totally neglected. The task of electing a pathologist will not be an easy one. It is to be hoped the choice will fall on one who has made his mark in all the various branches of neurological science; for the study of cerebral disease is so bound up with that of the spinal cord and nerves, that a knowledge of cerebral pathology must prove useless if not combined with a thorough mastery of the clinical phenomena of spinal and peripheral nervous diseases, of their lesions, and of the methods of clinical and experimental neurological investigation.

As might have been expected, the anti-vivisectionists, headed by the Lord Chief Justice of England, have memorialised the Viceroy of India and the members of the Executive and Legislative Councils. In this document the usual sentimental arguments against vivisection are stated. If with reference to the Indian Bill now under consideration for the regulation of vivisection experiments, it should be deemed advisable to legislate on the subject, the signatories suggest (*a*) that the higher animals, such as horses, asses, mules, dogs, and cats, for which special certificates are granted in England, and also monkeys, should be wholly exempted from experimentation; (*b*) that it should be made essential to keep the animals under an anæsthetic throughout the investigation; (*c*) that the use of curare should be entirely prohibited; (*d*) that it should be provided that one inspector at any rate shall be selected on account of his recognised humanity, not his scientific knowledge. The executive committee of the Society for the Protection of Animals from Vivisection have also recently transmitted to the Viceroy and the members of the Executive Council a protest against the establishment of a Pasteur Institute in India. They represent that similar institutes in Paris and elsewhere have so far failed to prevent deaths from the bites of dogs and other animals alleged to be rabid, and that 256 persons have died in spite of the preventive treatment invented by M. Pasteur. It is also remarked that the Pasteur system involves and depends upon the cultivation and perpetuation of the malady of rabies in series after series of sentient animals, to their great misery and suffering, but the benefits that mankind derives from it are naturally ignored.

DURING the week ending the 2nd inst. several depressions passed across these islands, causing gales on our northern coasts. In the rear of these disturbances northerly winds set in, with a great fall of temperature; on the 1st and 2nd inst. the thermometer fell to 20°, or less, in nearly all parts. In Scotland the lowest readings were between 12° and 15°. But by Sunday, the 3rd inst., the temperature rose rapidly in the north and west, and subsequently the rise extended to the southern parts of the country.

THE *Meteorologische Zeitschrift* for November contains a paper on the frequency of halo phenomena, by G. Hellmann. Few text-books have dealt with this subject, and those that have done so state that lunar halos are most frequent, an error which appears to date from the time of Aristotle. Certainly the moon offers less opportunity for such phenomena. Prof. Hellmann points out that only such observatories as record hourly observations afford the necessary materials for giving a satisfactory answer to the question. He has examined various records, and especially those of the Upsala observatory, the result being that the solar phenomena exceed the lunar by about five to one, by far the most frequent halos being those of 22° radius. The halos as well as mock-suns and mock-moons show a distinct yearly period. The solar phenomena are most frequent from April to June, and the lunar phenomena are most frequent in the winter half-year, being dependent on the length of the nights. These results are supported by observations made in the United States, and also in Japan.

THE *Pioneer Mail*, of November 9, contains an article on the past monsoon in India, based upon the official reports of rainfall between June 1 and October 15. These reports show a generally satisfactory state of affairs, about half the country having had excessive, and half deficient rainfall; some regions which generally receive only moderate rain had an excessive amount, while those which usually receive an excessive amount had a relatively light fall. The causes which bring about this half-yearly reversal of the winds are of especial interest, and offer a large field for study. Among the generally accepted theories, one attributes the origin of the rain-bearing current to the intense heating of the plains of Upper India, while another is that the chances of a good monsoon vary inversely with the amount of snow during the preceding winter. The writer thinks that these theories have failed in the present instance, while admitting that the distribution of heat and, under some circumstances, the snowfall exercise an influence on the monsoon. He sets up another theory, viz. that the monsoons are caused by the heated air of Asia rising up and overflowing at a great height to the southern hemisphere, where it settles down and is impelled northward by its own energy and by pressure in the rear. A reference to the "Memorandum on the Snowfall, &c." issued by the *Meteorological Reporter* on June 1 last, shows that the general forecast was to the effect that the rainfall might be deficient to a moderate extent in north-west India, and would very probably be at least normal in other parts. If any modification of the accepted theories be necessary, it will doubtless be shown by a study of the daily charts of the Indian monsoon area, to which we recently alluded, and the publication of which began with the present year. One of the special objects in preparing these charts is to elucidate the conditions which determine the advances, and variations in strength of the monsoon currents

SOME interesting observations on the velocity at which crystallisation proceeds in a super-cooled substance are communicated by Mr. Moore to the current number of the *Zeitschrift für Physikalische Chemie.* The method of experiment resembles that originally used by Gernez. The substance is contained in a carefully cleaned U-tube, made of thin glass, which is immersed in a bath of liquid maintained at constant temperature, and which during an observation is kept open at both ends. When crystallisation sets in, in such a tube, the line of demarcation between solid and liquid can readily be followed by eye, and the time can easily be noted which is taken by the crystallisation to travel a definite distance down a limb of the U tube. Satisfactory observations cannot be taken when the crystallisation is rising in a limb of the tube, owing to the disturbing effects of the thermal changes attending solidification. Experiments on acetic acid showed that at any temperature the velocity is uniform, and is independent of the diameter of the tube, and observations on acetic acid, phenol, and mixtures of phenol with water and with cresol, show that the velocity increases with the amount of super-cooling, and at a diminishing rate. For phenol it is ·6 c.m. per sec. with 4°·4 super-cooling, and 2·9 c.m. with 15°·8 super-cooling. The addition of water and of cresol to phenol largely reduces both the velocity of crystallisation and the rate at which it increases with the amount of super-cooling. Several of the curves indicate a maximum velocity as the extent of super-cooling increases. Attempts to observe this maximum were rendered fruitless, however, by the spontaneous crystallisation of the substances.

DIURNAL movements of the ground have been noticed at Santiago for some years, and have usually been attributed to the action of heat upon the Santa Lucia mountain. According to *La Nature*, the observatory has recently been removed to a plain at the south of the city, and Dr. Obrecht, the director, has investigated the movements. It appears from his observations that from noon until nine o'clock in the evening, the ground to the north-east is raised, and then gradually descends until seven o'clock on the following morning. These curious variations sometimes attain an amplitude of 3″ or 4″. There is also evidence that from July to September the ground to the north-east is continuously raised, while from September to November, the part to the east of the observatory is continuously elevated. The total amplitude of elevation is said to be about 35″.

MR. A. SIGSON, a professional photographer at Rybinsk, contributes an account of his method of obtaining photographs of snowflakes to the *Journal of the Russian Physico-Chemical Society*. He used a Zeiss microscope provided with an aplanatic lens and a long focus camera. This was placed near an attic window at a strong inclination to the horizon. The flakes were received on some rough cloth and transferred to a small net of cocoon fibres stuck on a card perforated in the middle. This card was placed on the stage of the microscope, and the illumination was so arranged that half the field was uniformly illuminated, and the other half shaded off. For an enlargement of fifteen times the exposure lasted two to five seconds, with plates supplied by M. Lumière. To avoid the melting of the flakes by the breath of the operator, the latter is obliged to breathe through a pipe bent backwards during the adjustment of the apparatus.

IN *Bulletin* No. 8 of the Geological and Natural History Survey of Minnesota, Dr. Andrew C. Lawson publishes two papers of great importance for the systematic grouping of volcanic rocks in North America. The first paper is on the "Anorthosytes of the Minnesota Coast of Lake Superior," and is prefaced by a long note, written by Prof. Winchell, on "The Norian of the North-West." In this note Prof. Winchell gives up many of his previously-formed ideas regarding the Minnesota rocks, in favour of the conclusions now obtained by Dr. Lawson. There occurs on the Minnesota coasts a rock almost wholly composed of a plagioclase felspar which had been included by Profs. Winchell and Irving in the Keweenian or Cupriferous series of volcanic lavas and sheets. For this rock, Dr. Lawson accepts the name of "Anorthosyte," given by Prof. Adams to similar rocks in the Norian series of Quebec; and he proves conclusively that it is a Plutonic formation, solidified under deep-seated conditions, and exposed later during the long period of pre-Palæozoic erosion. On its eroded surface the volcanic lavas of the Keweenian series were poured out, no rocks belonging to the Animikie series being present in this area. The thickness of the Keweenian series had been estimated by Prof. Irving at 20,000 feet. Dr. Lawson is of opinion that the series is comparatively thin, ranging from zero to a maximum of a few hundred feet. Special interest attaches to the hummocky—*roches moutonées*—aspect of the old surfaces of the Anorthosyte rock at Beaver Bay, Carlton Peak, &c., as this is such a marked feature of the ancient erosion planes of Archæan rocks in North America. Dr. Lawson compares the Anorthosytes of Minnesota with the Norian series of irruptive plagioclase rocks invading Archæan gneisses in Quebec, but until there is sufficient evidence in favour of this correlation, he suggests that a local name of "Carltonian" be given to the Minnesota Anorthosytes.

THE second paper in the same *Bulletin*, by Dr. Lawson, is entitled "The Laccolitic Sills of the North-West Coast of Lake Superior." Extensive trap-sheets are in this region associated with the Animikie and Nipigon groups of sedimentary rocks, and have up to this time been described as contemporaneous flows. Mr. Ingall had observed the intrusive nature of some of these so-called flows, but drew no farther conclusions. Dr. Lawson now advances the view that "there are no contemporaneous volcanic rocks in the Animikie group, and that the trap-sheets are all intrusive in their origin, and are of the nature of laccolitic sills." He supports this view by weighty evidence, such as the simplicity of the trap-sheets, their regularity and persistence over wide areas, the passage of thick sheets from the Animikie series into the higher horizons of Keweenian strata, the absence of pyroclastic rocks, the alteration of the rocks above and below the intruded sheets, and the direct continuity of the "trap-sheets" with dykes of the same intrusive rock. The "trap-sheets" occur as laccolitic sills both in the Animikie and Keweenian series, and are therefore later than these. Dr. Lawson thinks they may belong to the great series of trap-rocks intruded in the Silurian rocks of Quebec, but calls them for the present "Logan Sills," in honour of the late Sir William E. Logan.

IT is well known that electric currents may be produced by heating a single metal, if there be any variation in temper, or if the distribution of heat be very irregular, and the changes of temperature abrupt. Mr. W. H. Steele has made some experiments on these effects, in the Physical Laboratory of Melbourne University (*Science*, No. 562). A sensitive galvanometer put in circuit with a piece of iron wire showed a current when the wire was simply warmed with the fingers. This was the only metal which gave a current when at a temperature below 100° C. Altogether twelve different metals and four alloys were examined, and the effect noticed in each of them. In order to raise the wires to a high temperature without fusing them, they were passed through clay tubes (stems of tobacco-pipes), and, in the case of metals having low melting-points, the tube was completely filled with the metal. The highest electromotive force obtained from iron was 0·002 volt; 0·3 volt was observed with six different metals—lead, copper, gold, tin, zinc, and antimony; while with others, *e.g.* silver and aluminium, the effect was exceedingly small. In the case of lead, the effect showed no sign of ceasing after the metal had been heated for half a day. Gold gave the highest effect, as much as half a volt being observed. Mr. Steele remarks that these phenomena are generally quite sufficient to mask the ordinary thermo-electric effect at a red heat, and that thermo-electric tables are consequently not trustworthy for high temperatures.

THE current number of the *Comptes Rendus* contains a note, by M. Ch. André, on the variation of the electric state of the high regions of the atmosphere in fine weather. During a previous attempt to investigate this point, the author unfortunately met with an accident which has prevented him personally making any more observations; the measurements contained in this note have, however, been made under his direction. At opposite corners of the car of the balloon were fixed two cylindrical reservoirs, filled with distilled water, and insulated on plates of sulphur. To the base of each of these vessels an india-rubber tube, about 20 metres long, was attached, each tube having a small jet at its end. When the balloon had come to rest at any desired height, the difference of potential existing between two points, at a known vertical distance, was determined by means of an electrometer (Exner's pattern) connected metallically with the water reservoir. This difference of potential, the distance being kept constant, gave a measure of the strength of the electrical field. As a result of two series of observations, the author considers that in fine weather the strength of the electrical field does not increase with the altitude, but is the same at a given instant at any point along the same vertical.

In a paper communicated to the Reale Accademia delle Scienze, Torino, Signor Garbasso gives an account of his experiments on the reflection of electrical waves. The author allows the waves given out by a Hertz oscillator to fall upon a mirror consisting of a wooden plank 175 c.m. long and 125 c.m. broad, over which were stretched a large number (168) of parallel rectilinear resonators. These resonators were without spark-gaps, and consisted of wires 20 c.m. long with metal discs, 3·8 c.m. in diameter, fixed at either end. When another resonator, having a spark-gap, is placed so that the radiation reflected from this mirror falls upon it, bright sparks are produced, as has been shown by Trouton and others, when its length is parallel to the wires on the reflector, while no sparks are produced when it is at right angles to these wires. What seems curious, however, is that the radiation reflected, although it has such a large wave-length compared with the dimensions of the mirror, is not scattered but is regularly reflected.

In No. 5, vol. xii. of the *Zeitschrift für Physikalische Chemie*, Mr. Harry C. Jones gives an account of an additional series of observations on the freezing-points of dilute aqueous solutions. The most dilute solutions employed were in general about ·001—normal. Of the inorganic substances examined ammonia exhibited the most striking behaviour. Although the bases potash and soda like hydrochloric and nitric acid seem to be almost entirely dissociated into ions, ammonia is only dissociated to the extent of some twenty per cent. Phosphoric acid apparently dissociates into the two ions H and H_2PO_4, and in the most dilute solutions is less dissociated than sulphuric acid, which in turn is less dissociated than the monobasic acids. The extent of the dissociation thus obtained agreed, in the main, with that deduced from Kohlrausch's observations on the electric conductivity of the solutions. The organic substances examined gave quite unexpected results. Cane-sugar, dextrose, urea, phenol, and ethyl and propyl alcohols, which, according to the new theory, cannot undergo electrolytic dissociation, behaved in all cases in the most dilute solutions as if they were really dissociated, and gave molecular lowerings of the freezing-point which were much higher than the calculated value. Indeed, if one supposes for the moment that cane-sugar can dissociate into two ions, the observations on the freezing-point of its aqueous solutions, when treated as in the case of an electrolyte, would indicate that twenty-seven per cent. of the sugar is dissociated, or an amount greater than that found for ammonia. With rise in concentration the molecular lowering for all the organic substances diminishes, in some cases reaching a minimum and then increasing, or, as in the case of urea and the two alcohols, remaining constant. This constant minimum value of the molecular lowering agrees closely with the theoretical number. The explanation of these remarkable results from the standpoint of the new theory will be awaited with interest.

The marked increase in the vitality of the cholera bacillus in artificial culture media induced by adding larger than usual proportions of salt to the latter, was drawn attention to in these notes on August 24, in connection with the saline condition of the river Elbe at the intake of the Hamburg water-works during the great cholera epidemic. In a subsequent note, on September 28, it was pointed out how this property of the cholera organism had been taken advantage of by Koch and others in devising methods for the separate identification of this vibrio in water in the presence of other harmless saprophytic bacteria. Of extreme interest, therefore, are the experiments of Dr. M. N. Gamaleia, contained in a short paper, "Du cholèra virulent et épidémique," contributed to the *Comptes Rendus*, No. 5, 1893, p. 285. This investigator states that he was able to increase the *virulence* of the cholera organism by cultivating it in media containing from 3, 4, up to 5 per cent. of common salt. Nor were these results confined to one particular cultivation of the cholera bacillus, but were also derived with cholera cultures obtained from numerous different sources. On inoculating these salt-cultures of cholera vibrios into pigeons and guinea-pigs, symptoms of septicæmia developed, invading the blood and all the tissues. If one drop of the blood of these infected animals was taken and inoculated into others, the malady was transmitted. These observations support the theory that the unusual saline condition of the Elbe may have assisted in supplying the conditions which so greatly favoured the vitality and virulence of the cholera bacillus during the Hamburg epidemic.

The last two numbers of the *Botanische Zeitung*, published on November 1, are devoted to a memoir by E. Crato, "Morphologische und mikrochemische Untersuchungen ueber die Physoden." This memoir is stated to be an "Arbeit" carried on under the direct guidance of Prof. Dr. Reinke, at the University of Kiel, and the following is from the summary given by the author:—There lies at the basis of the vegetable cell a system of delicate lamellæ, arranged in such a way as to form a foam-like mass (Lamellensystem, Gerüstsubstanz). In those plants where the point has been carefully investigated, these lamellæ do not give the ordinary proteid reactions. The spaces enclosed by the lamellæ contain a clear, watery, slightly refractive fluid (Kammerflüssigkeit), whereto belong both cell-sap as well as enchylema. In these lamellæ there glide about, apparently at will, minute, refractive, bladder-like formations (physodes, to which the greater part of the microsomes belong), swelling out the lamellæ where they occur. These physodes certainly form readily transportable vehicles of chemical substances for the plant. In the brown Algæ these physodes contain substances similar to phenol. In all the Algæ which were investigated, the Laminaria excepted (their investigation is not complete), phloroglucin was found. Further, it would appear that these phenol-like substances are used up for the formation of the lamellar substance (plasma, &c).

Major J. W. Powell's eighth annual report, as Director of the U.S. Bureau of Ethnology, is a splendid addition to ethnological literature. In the first part of the volume the plans and operations of the Bureau are described, a brief account being given of the many investigations carried on during the fiscal year 1886–87 by the twenty-five assistants. The contributions contained in the volume are: "A Study of Pueblo Architecture, Tusayan and Cibola," by Mr. Victor Mindeleff, and "Ceremonial of Hasjelti Dailjis and Mythical Sand Painting of the Navajo Indians," by Mr. James Stevenson, this being his last official work before his death in 1888. In these papers "the prehistoric archæology of the Pueblos in the special department of architecture is the most prominent single subject presented and discussed; but the papers also include studies of the history, mythology, and sociology of that people, as well as of their neighbours and hereditary enemies, the Navajo." All these correlated studies are set forth in detail, and are profusely illustrated. Mr. Mindeleff's study relates to the ruins and inhabited towns found over a large territory in the interior south-western parts of the United States. His research leads him to conclude that there is no need for the hypothesis of an extinct race with dense population and high civilisation to account for the conditions actually existing in North America before the European discovery. Mr. Stevenson's paper is most interesting, and it has the advantage of being a statement of facts actually witnessed by the deceased author. Translations of six of the Navajo myths are also presented, some of which elucidate parts of the ceremony forming the main title of his paper. The whole work has been excellently done, and our only regret is that there should have been a delay of six years in its publication.

DURING the summers of 1891 and 1892 Mr. W. P. Hay took the opportunity, while visiting the caves of Southern Indiana, to observe the habits of the blind crayfish, *Cambarus pellucidus*. In some of the caverns, as at Shiloh Cave, the crayfish were extremely abundant. When observed in an undisturbed state, they were found resting quietly in some shallow part of the underground streams on the clay banks. They lay with all their legs extended, and their long antennæ gently waving about to and fro. They were easier caught by the hand suddenly seizing them than with a net. Noise did not seem to affect them. When first taken out of the water they were of a translucent pinkish white colour, with the alimentary track showing through as a blue body, but they soon lost these hues. The variation in the general spininess is very great. As a rule, the farther north the specimens were taken the smoother they were. At Mayfield's Cave, in Monroe County, a variety was found entirely without spines; this is described and figured as a sub-species. (Proc. U.S. Nat. Museum, No. 935, 1893.)

IN Wundt, *Philosophische Studien*, ix. Bd., 1 Heft., Herr Bruno Kämpfe brings together all the values of the integral for the probable error, *i.e*

$$\phi(\gamma) = \frac{2}{\sqrt{\pi}} \cdot \int_0^\gamma e^{-t^2} dt,$$

which gives the whole number of errors, both positive and negative, whose numerical magnitude falls between the given limits. The number of errors between any two given limits will be found by taking the difference between the tabular numbers corresponding to these limits. Since the total number of errors is taken as unity in the table, the required number of errors in any particular case is to be found by multiplying the tabular numbers of the actual number of observations. Thus, to take an example, if there were 1000 observations, and we wish to employ the limits 0·0 and 0·5, then looking in the column giving the values of γ, we find against them the numbers 0·0000 and 0·5205, which when subtracted from one another, and multiplied by 1000 give 520·5 or 520 errors. If the limits had been 1·5 and 2·0, then we should have found the corresponding values 0·9661 and 0·9953, which subtracted give 0·0292, and multiplied by 1000 give 29, *i.e.* 29 errors that lie between these limits out of 1000 observations. This table is published also as a *separatabdruck* by Wilhelm Engelmann, Leipzig, which is in a more useful form for computation. The values of γ can be read directly to three places of decimals.

WE have received a report of the meteorological observations made during 1892 at the Royal Alfred Observatory, Mauritius.

THE new issue of Mr. Edward Stanford's compendium of geography and travel includes a revised and partly rewritten edition of "Australasia." Under this title Dr. A. R. Wallace's excellent description of Australia and New Zealand has been published, and a second volume, embracing Malaysia and the Pacific Archipelagoes, by Dr. F. H. H. Guillemard, is in preparation.

MM. J. B. BAILLIÈRE ET FILS have added to their library of contemporary science a volume entitled "Pêches et Chasses Zoologiques," by the Marquis of Folin. The book is well illustrated, and, though much of the matter it contains is only of local interest, a large portion will be read with profit by students of natural history.

IT is very doubtful whether any useful purpose is served by the issue, from Mr. Edward Stanford's, of the series of maps edited by Captain A. Staggemeier, of Copenhagen. The maps show very little except the configuration of the land surfaces, the editor's idea being that they will be of service to physical geographers for placing observed facts of natural history, meteorology, &c., in their proper geographical position. There are five maps in the portfolio before us, two showing the Polar regions down to 30°, and three the zone between 45° of North and South latitude, on Mercator's projection; hence the zones between latitudes 30° and 45° are represented on both projections. It is intended to issue other maps on a larger scale, the whole series to comprise twenty-five plates, which will be published in six parts.

IT is encouraging to learn, from the forty-first annual report of the working of the Manchester Public Free Libraries, that during the year 1892–93, 77,878 volumes dealing with science and art were issued from the reference library, and 67,456 were referred to in the reading-room. The total number of books issued to borrowers by the nine branch libraries was 872,655, of which 45,526 are classified under science and art. Of the 100,123 volumes consulted in the reading-rooms of the branch libraries, 7869 were on science and art subjects. The record is a good one; but if the committee were to classify science separately from art, we should be better able to estimate from the figures the growth of interest in natural knowledge.

DR. ARTHUR GAMGEE has just completed the second volume of his text-book on the Physiological Chemistry of the Animal Body, upon which he has been engaged for some years. Like the first volume, it constitutes an independent and complete treatise, dealing with the physiological chemistry of the digestive processes. It has been the author's aim to give the reader a very full and, so far as possible, independent account of the state of knowledge on the subjects discussed. Messrs. Macmillan and Co. will publish the volume immediately.

MESSRS. MACMILLAN AND CO. are also about to publish a revised and enlarged edition of "Elementary Lessons in Steam Machinery and the Marine Steam Engine," by Messrs. Langmaid and Gaisford, Instructors on H.M.S. *Britannia*. It will be followed by other works constituting a Britannia Science Series. Among those already in hand may be mentioned "Physics for School Use," by Mr. F. R. Barrett, Mr. A. E. Gibson, Rev. J. C. P. Aldous, and others; a "Physics Note-Book," by Messrs. Gibson and Aldous; "Trigonometry for Practical Men," by Mr. W. W. Lane; and "Geometrical Drawing, Perspective, and Mechanical Drawing," by Mr. J. H. Spanton.

THE additions to the Zoological Society's Gardens during the past week include a Mozambique Monkey (*Cercopithecus pygerythrus*, ♂) a Sykes's Monkey (*Cercopithecus albigularis*, ♂) a Bell's Cinixys (*Cinixys belliana*) from East Africa, presented by Mr. T. E. C. Remington; a Red Tiger Cat (*Felis chrysothrix*) from the Gold Coast, West Africa, presented by Mr. William Adams; a Common Otter (*Lutra vulgaris*) from Yorkshire, presented by Mr. C. B. C. de Wit; a Herring Gull (*Larus argentatus*) British, presented by Mr. J. G. Goodchild; a Northern Mocking Bird (*Mimus polyglottus*) from North America, presented by Miss Dorothy Williams; a Viperine Snake (*Tropidonotus viperinus*) European, presented by Miss Ffennell; five Barbary Partridges (*Caccabis petrosa*) from North Africa, deposited.

OUR ASTRONOMICAL COLUMN.

THE VARIATION OF LATITUDE.—In the *Astronomical Journal*, No. 19 (November 14), Prof. S. C. Chandler gives the eighth of the important series of articles that he has been contributing on the variation of latitude. The special part of the subject which is referred to deals with the direction of the rotation of the pole and is accompanied by an explicit demonstration which includes all the data bearing upon it. Owing to the insufficient extent of series of observations in widely different longitudes to furnish independent values of the constants for both terms of the variation, Prof. Chandler has thought well to combine short series made in nearly the same longitudes, and so deduced fourteen determinations of the numerical equations for the latitude variation. Reducing the values so obtained to a common epoch, he found that the values of the observed Julian date when the latitude would be a minimum, or when the pole of figure would pass the meridian of the respective stations by virtue of the fourteen months' revolution alone, and of the sun's longitude on the observed date when the same phase would occur by virtue of the annual term alone, both decreased from Pulkowa *towards* Madison showing that the direction of the rotations in both the elements was from west to east. In the latter part of the article Prof. Chandler refers not only to our knowledge of the general law of latitude variation, but to the accuracy of the necessary constants which afford us a means of predicting the immediate future course. The minimum of the curve of April, 1893, will be followed by an interval of nearly two years, and will be marked by very slight fluctuations, so that from the maximum of October, 1893, to that of August, 1895, or from minimum April, 1893, to that near the beginning of 1895, "there will apparently be but a single decidedly marked period of, say 20-22 months," the total range amounting to 0″·10 as against 0″·56 which prevailed in 1889 and 1892. In May, 1896, the same dimensions as in 1889 will be again attained, and the variation from that time forward to 1898 it will be in full play with the range of 0″·5 or 0″·6, a period of nearly 390 days which prevailed between 1889 and 1892. In § 2 of the article Prof. Chandler adds a few words as to the reality of these movements of the earth's axis as against the motions being "merely misinterpretations of the observed phenomena" or an illusory effect of instrumental error due to the influence of temperature. Those of our readers who are still sceptical on the subject will learn that the observed law of latitude variation includes two terms, one with a period of fourteen months, and another with twelve months, making the phases come in very different relations to conditions of temperature dependent on season, an argument greatly against that brought forward by temperature-variation believers.

METEOR SHOWER FOR DECEMBER.—No news is yet to hand with regard to the Biela meteors, but we hope soon to receive accounts of the display which will give us some idea of the quantity and also of the date of reaching their maximum. The following meteor radiant-points are given by Mr. Denning for the ensuing month, that for the 10th lying approximately close to ρ Gemini in a prolongation of β and ρ Gemini, and being defined as a "most brilliant shower."

Date.	Radiant. α	δ	Meteors.
Dec. 8	145°	+ 7	Swift; streaks
8	208	+ 71	Rather swift
10	108	+ 33	Swift; short
24	218	+ 36	Swift; streaks
25	98	+ 31	Very slow

REFRACTION TABLES.—We have received a small pamphlet extracted from the *Mitheilungen aus der Deutschen Schutzgebieten*, Bd. vi., Heft 4, containing refraction tables computed by Dr. L. Ambronn, of the Göttingen observatory. These tables are not intended for such accurate values as are required in observatories with fixed instruments, but are intended to be used by those, who having made astronomical observations, wish to compute them on the spot, using approximate formulæ. Travellers, especially, will find these tables very useful for wide ranges, both as regards temperature and barometer arguments. The tables are based on Bessel's refraction-table formula, and by slightly combining the first two terms, which is no other than the mean refraction, and eliminating the term *log* T by reducing the height of the barometer to 0°C becomes, employing the usual notation:

$$\log \text{refraction} = \log \alpha \tan z + A \log B_0 + \lambda \log \gamma$$

or

$$\text{refraction} = \alpha \tan z \times B_0{}^A \times \gamma^\lambda \quad . \quad . \quad . \quad . \quad (1)$$

To make the correction for the mean refraction additive, the expression can be put in the form:

$$\text{refraction} = [\alpha \tan z + \alpha \tan z \, (\gamma^\lambda - 1)] \, B_0{}^A$$

Table II. gives the expression for the second term in the brackets using the mean refraction (α tan z) and the air temperature (γ) as arguments. For the barometer correction, if α tan z represent the mean refraction corrected for temperature then in equation (1) we may omit γ and write

$$\text{refraction} = (\alpha \tan z) \times B_0{}^A$$

$$\text{or, refraction } (\alpha \tan z) \times (\alpha \tan z) \, [B_0{}^A - 1]$$

The second term is taken direct from Table III. using the the mean refraction (corrected for temperature) and the height of the barometer as arguments.

To obtain the true refraction then, one simply (1) finds the mean refraction for the given zenith distance; (2) adds then the correction for temperature, and with this corrected mean refraction as argument; (3) adds the corresponding correction for the height of the barometer. Accuracy up to less than half a second of arc can be obtained.

GEOGRAPHICAL NOTES.

THE friends of the late Emin Pasha, at the suggestion of Dr. Schweinfurth, have resolved to collect subscriptions for a memorial to commemorate his long labours in Africa as a naturalist, traveller, and administrator. There must be many in this country anxious to have a share in such a tribute, and we shall shortly be able to intimate where subscriptions should be sent. The present proposal is to erect a monument in the Silesian town of Neisse.

BY the death of Mr. A. L. Bruce, at Edinburgh last week, the cause of geography and civilisation in Africa has lost a wealthy and judicious promoter. Mr. Bruce, who married as his second wife a daughter of Dr. Livingstone, was a director and one of the founders of the Imperial British East Africa Company. He was a devoted friend and warm supporter of Mr. H. M. Stanley, and took a leading position in organising and supporting the Emin Relief Expedition. Mr. Bruce was the originator of the Royal Scottish Geographical Society, of which he acted as treasurer, and in the prosperity of which he took the keenest interest to the last.

GUILIO GRABLOVITZ has published as a pamphlet a paper on tidal phenomena in the Mediterranean, read at the Italian Geographical Congress, and entitled "Sulla Osservazioni Mareografiche in Italia e specialmente su quelle fatte ad Ischia." The work done with recording mareographs is of considerable importance and several diagrams are given showing the tidal range and its fluctuations. The mean rise of the water was 11 centimetres at San Remo, 24 at Genoa, 12 in the North of Sardinia, from 15 to 22 along the west coast of Italy as far as Ischia, 30 in the Lipari Islands, but only from 2 to 13 round Sicily. In the Adriatic the range increased from 9 centimetres at Brindisi to 48 at Venice, which was the only station showing a range greater than one foot. The curves are recorded on a large scale, the ripples of the calm water in which the mareograph worked bearing a comparatively large ratio to the total tidal amplitude.

MONT ISERAN, in the eastern Alps, is, or rather was, one of the most remarkable mountains on the map of Europe, where it flourished long, although without any physical representative on the mountain-range itself. M. Henri Ferrand, in an entertaining little *brochure* relates its story, showing how it had come to be an accepted belief amongst cartographers that the river Isère had its source in a Mont Iseran. The mountain was fixed in latitude, longitude, and altitude by an Italian surveyor in 1809; but in the fifties, when Alpine climbing became fashionable, the discovery was made by climbers that no one in the neighbourhood could point out Mont Iseran. There was a col of that name, but no peak. An exhaustive French survey conclusively proved that the summit so long honoured on all maps had no real existence, and M. Ferrand tells the whole amusing history remarkably well as a lesson of the value of mountain-climbing, even to scientific topography.

THE telegraphic cable opened last month from Zanzibar to Mauritius and Seychelles is an important link in the cable network which is gradually encompassing the globe.

THE ANNIVERSARY MEETING OF THE ROYAL SOCIETY.

THE anniversary meeting of the Royal Society was held in the apartments of the Society at Burlington House, on St. Andrew's Day, November 30. The auditors of the treasurer's accounts having presented their report, the secretary read the list of Fellows elected and deceased since the last anniversary. The Society has lost eleven fellows on the home list, and two foreign members, as follows:—

Henry Tibbats Stainton, December 2, 1892, aged 70.
Sir Richard Owen, December 18, 1892, aged 89.
Dr. James Jago, January 18, 1893, aged 77.
Henry Francis Blanford, January 23, 1893, aged 58.
Thomas William Fletcher, February 1, 1893, aged 84.
Edward Walker, March 2, 1893, aged 73.
Alphonse de Candolle, March 28, 1893, aged 87.
Henry Edward Stanley, Earl of Derby, April 21, 1893, aged 67.
Ernest Edward Kummer, May 14, 1893, aged 84.
Rev. Charles Pritchard, May 28, 1893, aged 85.
Dr. John Rae, July 22, 1893, aged 80.
Thomas Hawksley, September 23, 1893, aged 86.
Sir Andrew Clark, Bart., November 6, 1893, aged 67.

The Society next proceeded to elect the officers and council for the ensuing year. A list of those selected for election was given in NATURE, November 9.

Lord Kelvin, the President, then delivered his address. After briefly referring to the work of the Standing Committees, he continued as follows:—

Not the least important of the scientific events of the year is the publication, in the original German and in an English translation by Prof. D. E. Jones, of a collection of Hertz's papers describing the researches by which he was led up to the experimental demonstration of magnetic waves. For this work the Rumford Medal of the Royal Society was delivered to Prof. Hertz three years ago by my predecessor, Sir George Stokes. To fully appreciate the book now given to the world, we must carry our minds back to the early days of the Royal Society, when Newton's ideas regarding the forces which he saw to be implied in Kepler's laws of the motions of the planets and of the moon were frequent subjects of discussion at its regular meetings, and at perhaps even more important non-official conferences among its Fellows.

In 1684 the senior secretary of the Royal Society, Dr. Halley, went to Cambridge to consult Mr. Newton on the subject of the production of the elliptic motion of the planets by a central force,[1] and on December 10 of that year he announced to the Royal Society that he "had seen Mr. Newton's book, 'De Motu Corporum.'" Some time later, Halley was requested to "remind Mr. Newton of his promise to enter an account of his discoveries in the register of the Society," with the result that the great work "Philosophiæ Naturalis Principia Mathematica" was dedicated to the Royal Society, was actually presented in manuscript, and was communicated at an ordinary meeting of the Society on April 28, 1686, by Dr. Vincent. In acknowledgment, it was ordered "that a letter of thanks be written to Mr. Newton, and that the printing of his book be referred to the consideration of the council; and that in the meantime the book be put into the hands of Mr. Halley, to make a report thereof to the council." On May 19 following, the Society resolved that "Mr. Newton's 'Philosophiæ Naturalis Principia Mathematica' be printed forthwith in quarto, in a fair letter; and that a letter be written to him to signify the Society's resolution, and to desire his opinion as to the volume, cuts, &c." An exceedingly interesting letter was accordingly written to Newton by Halley, dated London, May 22, 1686, which we find printed in full in Weld's "History of the Royal Society" (vol. i. pp. 308–309). But the council knew more than the Royal Society at large of its power to do what it wished to do. Biology was much to the front then, as now, and the publication of Willughby's book, "De Historia Piscium," had exhausted the Society's finances to such an extent that the salaries even of its officers were in arrears. Accordingly, at the council meeting of June 2, it was ordered that "Mr. Newton's book be printed, and that Mr. Halley undertake the business of looking after it, and printing it at his own charge, which he engaged to do."

It seems that at that time the office of treasurer must have been in abeyance; but with such a senior secretary as Dr. Halley there was no need for a treasurer.

Halley, having accepted copies of Willughby's book, which

1 Whewell's "History of the Inductive Sciences," vol. ii. p. 77.

had been offered to him in lieu of payment of arrears of salary[1] due to him, cheerfully undertook the printing of the "Principia" at his own expense, and entered instantly on the duty of editing it with admirable zeal and energy, involving, as it did, expostulations, arguments, and entreaties to Newton not to cut out large parts of the work which he wished to suppress[2] as being too slight and popular, and as being possibly liable to provoke questions of priority. It was well said by Rigaud, in his "Essay on the first publication of the Principia," that "under the circumstances it is hardly possible to form a sufficient estimate of the immense obligation which the world owes in this respect to Halley, without whose great zeal, able management, unwearied perseverance, scientific attainments, and disinterested generosity the 'Principia' might never have been published."[3] Those who know how much worse than "law's delays" are the troubles, cares, and labour involved in bringing through the press a book on any scientific subject at the present day will admire Halley's success in getting the "Principia" published within about a year after the task was committed to him by the Royal Society two hundred years ago.

When Newton's theory of universal gravitation was thus made known to the world Descartes' *Vortices*, an invention supposed to be a considerable improvement on the older invention of crystal cycles and epi-cycles from which it was evolved, was generally accepted, and seems to have been regarded as quite satisfactory by nearly all the philosophers of the day.

The idea that the sun pulls Jupiter, and Jupiter pulls back against the sun with equal force, and that the sun, earth, moon, and planets all act on one another with mutual attractions, seemed to violate the supposed philosophic principle that matter cannot act where it is not. Descartes' doctrine died hard among the mathematicians and philosophers of continental Europe; and for the first quarter of last century belief in universal gravitation was an insularity of our countrymen.

Voltaire, during a visit which he made to England in 1727, wrote: "A Frenchman who arrives in London finds a great alteration in philosophy, as in other things. He left the world full; he finds it empty. At Paris you see the universe composed of vortices of subtle matter; at London we see nothing of the kind. With you it is the pressure of the moon which causes the tides of the sea; in England it is the sea which gravitates towards the moon. . . . You will observe also that the sun, which in France has nothing to do with the business, here comes in for a quarter of it. Among you Cartesians all is done by impulsion: with the Newtonians it is done by an attraction of which we know the cause no better"[4] Indeed, the Newtonian opinions had scarcely any disciples in France till Voltaire asserted their claims on his return from England in 1728. Till then, as he himself says, there were not twenty Newtonians out of England.[5]

In the second quarter of the century sentiment and opinion in France, Germany, Switzerland, and Italy experienced a great change. "The mathematical prize questions proposed by the French Academy naturally brought the two sets of opinions into conflict." A Cartesian memoir of John Bernoulli was the one which gained the prize in 1730. It not infrequently happened that the Academy, as if desirous to show its impartiality,

1 It is recorded in the Minutes of Council that the arrears of salary due to Hooke and Halley were resolved to be paid by copies of Willughby's work. Halley appears to have assented to this unusual proposition, but Hooke wisely "desired six months' time to consider of the acceptance of such payment."

The publication of the "Historia Piscium," in an edition of 500 copies, cost the Society £400. It is worthy of remark, as illustrative of the small sale which scientific books met with in England at this period, that, a considerable time after the publication of Willughby's work, Halley was ordered by the Council to endeavour to effect a sale of several copies with a bookseller at Amsterdam, as appears in a letter from Halley requesting Boyle, then at Rotterdam, to do all in his power to give publicity to the book. When the Society resolved on Halley's undertaking to measure a degree of the earth, it was voted that "he be given £50 or fifty 'Books of Fishes.'" (Weld's "History of the Royal Society," vol. i. p. 310.)

2 "The third [book] I now design to suppress. Philosophy is such an impertinently litigious lady that a man had as good be engaged in lawsuits as have to do with her. I found it so formerly, and now I am no sooner come near her again but she gives me warning. The first two books without the third will not so well bear the title of 'Philosophiæ Naturalis Principia Mathematica,' and therefore I have altered it to this, 'De Motu Corporum Libri duo'; but, upon second thoughts, I retain the former title. 'Twill help the sale of the book, which I ought not to diminish now 'tis yours." (*Ibid.*, p. 311.)

3 *Ibid.*, p. 310.

4 Whewell's "History of the Inductive Sciences," vol. ii. pp. 202-203.

5 *Ibid.*, vol. ii. p. 201.

divided the prize between Cartesians and Newtonians. Thus, in 1734, the question being the cause of the inclination of the orbits of the planets, the prize was shared between John Bernoulli, whose memoir was founded on the system of vortices, and his son Daniel, who was a Newtonian. The last act of homage of this kind to the Cartesian system was performed in 1740, when the prize on the question of the tides was distributed between Daniel Bernoulli, Euler, Maclaurin, and Cavallieri; the last of whom had tried to amend and patch up the Cartesian hypothesis on this subject.[1]

On February 4, 1744, Daniel Bernoulli wrote as follows to Euler: "Uebrigens glaube ich, dass der Aether sowohl *gravis versus solem*, als die Luft versus terram sey, und kann Ihnen night bergen, dass ich über diese Puncte ein völliger Newtonianer bin, vnd verwundere ich mich, dass sie den Principiis Cartesianis so lang adhäriren; es möchte wohl einige Passion vielleicht mit unterlaufen. Hat Gott können eine *animam*, deren Natur uns unbegreiflich ist, erchaffen, so hat er auch können eine attractionem universalem materiae imprimiren, wen gleich solche attractio *supra captum* ist, da hingegen die Principia Cartesiana allzeit *contra captum* etwas involviren."

Here the writer, expressing wonder that Euler had so long adhered to the Cartesian principles, declares himself a thoroughgoing Newtonian, not merely in respect to gravitation *versus* vortices, but in believing that matter may have been created simply with the law of universal attraction without the aid of any gravific medium or mechanism. But in this he was more Newtonian than Newton himself.

Indeed Newton was not a Newtonian, according to Daniel Bernoulli's idea of Newtonianism, for in his letter to Bentley to date February 25, 1792,[2] he wrote: "That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a vacuum without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it." Thus Newton, in giving out his great law, did not abandon the idea that matter cannot act where it is not. In respect, however, of merely philosophic thought, we must feel that Daniel Bernoulli was right; we can conceive the sun attracting Jupiter, and Jupiter attracting the sun, without any intermediate medium, if they are ordered to do so. But the question remains, Are they so ordered? Nevertheless, I believe all, or nearly all, his scientific contemporaries agreed with Daniel Bernoulli in answering this question affirmatively. Very soon after the middle of the eighteenth century Father Boscovich[3] gave his brilliant doctrine (if infinitely improbable theory) that elastic rigidity of solids, the elasticity of compressible liquids and gases, the attractions of chemical affinity and cohesion, the forces of electricity and magnetism—in short, all the properties of matter except heat, which he attributed to a sulphurous fermenting essence—are to be explained by mutual attractions and repulsions, varying solely with distances, between mathematical points endowed also, each of them, with inertia. Before the end of the eighteenth century the idea of action-at-a-distance through absolute vacuum had become so firmly established, and Boscovich's theory so unqualifiedly accepted as a reality, that the idea of gravitational force or electric force or magnetic force being propagated through and by a medium seemed as wild to the naturalists and mathematicians of 100 years ago as action-at-a-distance had seemed to Newton and his contemporaries 100 years earlier. But a retrogression from the eighteenth century school of science set in early in the nineteenth century.

Faraday, with his curved lines of electric force, and his dielectric efficiency of air and of liquid and solid insulators, resuscitated the idea of a medium through which, and not only through which but *by* which, forces of attraction or repulsion, seemingly acting at a distance, are transmitted. The long struggle of the first half of the eighteenth century was not merely on the question of a medium to serve for gravific mechanism, but on the correctness of the Newtonian law of gravitation as a matter of fact however explained. The corresponding controversy in the nineteenth century was very short, and it soon became obvious that Faraday's idea of the transmission of electric force by a medium not only did not violate Coulomb's law o relation between force and distance, but that, if real, it must give a thorough explanation of that law.[1] Nevertheless, after Faraday's discovery[2] of the different specific inductive capacities of different insulators, twenty years passed before it was generally accepted in continental Europe. But before his death, in 1867, he had succeeded in inspiring the rising generation of the scientific world with something approaching to faith that electric force is transmitted by a medium called ether, of which, as had been believed by the whole scientific world for forty years, light and radiant heat are transverse vibrations. Faraday himself did not rest with this theory for electricity alone. The very last time I saw him at work in the Royal Institution was in an underground cellar, which he had chosen for freedom from disturbance; and he was arranging experiments to test the time of propagation of magnetic force from an induction coil through a distance of many yards to a fine steel needle polished to reflect light; but no result came from those experiments. About the same time, or soon after, certainly not long before the end of his working time, he was engaged (I believe at the shot tower near Waterloo Bridge on the Surrey side) in efforts to discover relations between gravity and magnetism, which also led to no result.

Absolutely nothing has hitherto been done for gravity either by experiment or observation towards deciding between Newton and Bernoulli as to the question of its propagation through a medium, and up to the present time we have no light, even so much as to point a way for investigation, in that direction. But for electricity and magnetism, Faraday's anticipations and Clerk-Maxwell's splendidly developed theory have been established on the sure basis of experiment by Hertz's work, of which his own most interesting account is this year presented to the world in the German and English volumes to which I have referred. It is interesting to know, as Hertz explains in his introduction, and it is very important in respect to the experimental demonstration of magnetic waves to which he was led, that he began his electric researches in a problem happily put before him thirteen years ago by Prof. von Helmholtz, of which the object was to find by experiment some relation between electromagnetic forces and dielectric polarisation of insulators, without, in the first place, any idea of discovering a progressive propagation of those forces through space.

It was by sheer perseverance in philosophical experimenting that Hertz was led to discover a finite velocity of propagation of electromagnetic action, and then to pass on to electromagnetic waves in air and their reflection, and to be able to say, as he says in a short reviewing sentence at the end of his eighth paper: "Certainly it is a fascinating idea that the processes in air which we have been investigating, represent to us on a million-fold larger scale the same processes which go on in the neighbourhood of a Fresnel mirror, or between the glass plates used for exhibiting Newton's rings."

Prof. Oliver Lodge has done well in connection with Hertz's work, to call attention[3] to old experiments, and ideas taken from them, by Joseph Henry, which came more nearly to an experimental demonstration of electromagnetic waves than anything that had been done previously. Indeed, Henry, after describing experiments showing powerful enough induction due to a single spark from the prime conductor of an electric machine to magnetise steel needles at a distance of thirty feet in a cellar beneath with two floors and ceilings intervening, says that he is "disposed to adopt the hypothesis of an electrical plenum," and concludes with a short reviewing sentence: "It may be further inferred that the diffusion of motion in this case is almost comparable with that of a spark from a flint and steel in the case of light."

Prof. Oliver Lodge himself did admirable work in his investigations with reference to lightning rods,[4] coming very near to experimental demonstrations of electromagnetic waves; and he drew important lessons regarding "electrical surgings" in an insulated bar of metal "induced by Maxwell's and Heaviside's electromagnetic waves," and many other corresponding phenomena manifested both in ingenious and excellent experiments devised by himself and in natural effects of lightning.

Of electrical surgings or waves in a short insulated wire, and

[1] Whewell's "History of the Inductive Sciences," vol. ii. pp. 198, 199.
[2] "The Correspondence of Richard Bentley, B.D." vol. i. p. 70.
[3] "Theoria Philosophiæ Naturalis redacta ad unicam legem virium in natura existentium auctore P. Rogerio Josepho Boscovich, Societatis Jesu," 1st edition, Vienna, 1758; 2nd edition, amended and extended by the author, Venice, 1763.

[1] "Electrostatics and Magnetism," Sir W. Thomson, Arts. I. (1842) and II. (1845), particularly § 25 of Art. II.
[2] 1837, "Experimental Researches," 1161–1306.
[3] "Modern Views of Electricity," pp. 369–372.
[4] "Lightning Conductors and Lightning Guards," Oliver J. Lodge, F.R.S. Whittaker and Co.

of interference between ordinary and reflected waves, and positive electricity appearing where negative might have been expected, we hear first, it seems, in Herr von Bezold's "Researches on the Electric Discharge" (1870), which Hertz gives as the third paper of his collection, with interesting and ample recognition of its importance in relation to his own work.

In connection with the practical development of magnetic waves, you will, I am sure, be pleased if I call your attention to two papers by Prof. G. F. Fitzgerald, which I heard myself at the meeting of the British Association at Southport in 1883. One of them is entitled "On a Method of Producing Electromagnetic Disturbances of comparatively Short Wave-lengths." The paper itself is not long, and I shall read it to you in full, from the "Report of the British Association," 1883: "This is by utilising the alternating currents produced when an accumulator is discharged through a small resistance. It is possible to produce waves of as little as two metres wave-length, or even less." This was a brilliant and useful suggestion. Hertz, not knowing of it, used the method; and, makingas little as possible of the "accumulator," got waves of as little as 10 cm. wave-length in many of his fundamental experiments. The title alone of Fitzgerald's other paper, "On the Energy Lost by Radiation from Alternating Currents," is in itself a valuable lesson in the electromagnetic theory of light, or the undulatory theory of magnetic disturbance. It is interesting to compare it with the title of Hertz's eleventh paper, "Electric Radiation"; but I cannot refer to this paper without expressing the admiration and delight with which I see the words "rectilinear propagation," "polarisation," "reflection," "refraction," appearing in it as sub-titles.

During the fifty-six years which have passed since Faraday first offended physical mathematicians with his curved lines of force, many workers and many thinkers have helped to build up the nineteenth century school of *plenum*; one ether for light, heat, electricity, magnetism; and the German and English volumes containing Hertz's electrical papers, given to the world in the last decade of the century, will be a permanent monument of the splendid consummation now realised.

But, splendid as this consummation is, we must not fold our hands and think or say there are no more worlds to conquer for electrical science. We do know something now of magnetic waves. We know that they exist in nature, and that they are in perfect accord with Maxwell's beautiful theory. But this theory teaches us nothing of the actual motions of matter constituting a magnetic wave. Some definite motion of matter perpendicular to the lines of alternating magnetic force in the waves and to the direction of propagation of the action through space, there must be; and it seems almost satisfactory as a hypothesis to suppose that it is chiefly a motion of ether with a comparatively small but not inconsiderable loading by fringes of ponderable molecules carried with it. This makes Maxwell's "electric displacement" simply a to-and-fro motion of ether across the line of propagation, that is to say, precisely the vibrations in the undulatory theory of light according to Fresnel. But we have as yet absolutely no guidance towards any understanding or imagining of the relation between this simple and definite alternating motion, or any other motion or displacement of the ether, and the earliest known phenomena of electricity and magnetism—the electrification of matter, and the attractions and repulsions of electrified bodies; the permanent magnetism of lodestone or steel, and the attractions and repulsions due to it; and certainly we are quite as far from the clue to explaining, by ether or otherwise, the enormously greater forces of attraction and repulsion now so well known after the modern discovery of electromagnetism.

Fifty years ago it became strongly impressed on my mind that the difference of quality between vitreous and resinous electricity, conventionally called positive and negative, essentially ignored as it is in the mathematical theories of electricity and magnetism with which I was then much occupied (and in the whole science of magnetic waves as we have it now), must be studied if we are to learn anything of the nature of electricity and its place among the properties of matter. This distinction, essential and fundamental as it is in frictional electricity, electrochemistry, thermo-electricity, pyro-electricity of crystals, and piezo-electricity of crystals, had been long observed in the old known beautiful appearances of electric glow and brushes and sparks from points and corners on the conductors of ordinary electric machines and in exhaustive receivers of air-pumps with electricity passed through them. It was also known, probably as many as fifty years ago, in the vast difference of behaviour of the positive and negative electrodes of the electric arc lamp. Faraday gave great attention to it[1] in experiments and observations regarding electric sparks, glows, and brushes, and particularly in his "dark discharge" and "dark space" in the neighbourhood of the negative electrode in partial vacuum. In [1523] of his 12th series, he says, "The results connected with the different conditions of positive and negative discharge will have a far greater influence on the philosophy of electrical science than we at present imagine." His "dark discharge" ([1544–1554]) through space around or in front of the negative electrode was a first instalment of modern knowledge in that splendid field of experimental research which, fifteen years later, and up to the present time, has been so fruitfully cultivated by many of the able scientific experimenters of all countries.

The Royal Society's Transactions and Proceedings of the last years contain, in the communications of Gassiot,[2] Andrews and Tait,[3] Cromwell Varley,[4] De La Rue and Müller,[5] Spottiswoode,[6] Moultan,[7] Plücker,[8] Crookes,[9] Grove,[10] Robinson,[11] Schuster,[12] J. J. Thomson,[13] and Fleming,[14] almost a complete history of the new province of electrical science which has grown up largely in virtue of the great modern improvements in practical methods for exhausting air from glass vessels, by which we now have "vacuum tubes" and bulbs containing less than 1/190,000 of the air which would be left in them by all that could be done in the way of exhausting (supposed to be down to 1 mm. of mercury) by the best air-pump of fifty years ago. A large part of the fresh discoveries in this province have been made by the authors of these communications, and their references to the discoveries of other workers very nearly complete the history of all that has been done in the way of investigating the transmission of electricity through highly rarefied air and gases since the time of Faraday.

Varley's short paper of 1871, which, strange to say, has lain almost or quite unperceived in the Proceedings during the twenty-two years since its publication, contains an admirable first instalment of discovery in a new field—the molecular torrent from the "negative pole," the control of its course by a magnet, its pressure against either end of a pivoted vane of mica according as it is directed by a magnet to one end or the other, the shadow produced by its interception by a mica screen. Quite independently of Varley, and not knowing what he had done, Crookes was led to the same primary discovery, not by accident, and not merely by experimental skill and acuteness of observation. He was led to it by carefully designed investigation, starting with an examination of the cause of irregularities which had troubled[15] him in his weighing of thallium; and, going on to trials for improving Cavendish's gravitational measurement, in the course of which he discovered that the seeming attraction by heat is only found in air of greater than 1/1000[16] of ordinary density; and that there is repulsion increasing to a maximum when the density is decreased from 1/1000 to 36/1,000,000, and thence diminishing towards zero as the rarefaction is farther extended to density 1/20,000,000. From this discovery Crookes came to his radiometer, first without and then with electrification, powerfully aided by Sir George Stokes.[17] As he went on he brought all his work more and more into touch with the kinetic theory of gases; so much so that when he discovered the molecular torrent he

1 "Experimental Researches," Series 12 and 13, Jan. and Feb. 1838.
2 Roy. Soc. Proc., vol. 10, 1860, pp. 36, 269, 274, 432.
3 Roy. Soc. Proc., vol. 10, 1860, p. 274; Phil. Trans.
4 Roy. Soc. Proc., vol. 19, 1871, p. 236
5 Roy. Soc. Proc., vol. 23, 1875, p. 356; vol. 26, 1877, p. 519; vol. 27, 1878, p. 374; vol. 29, 1879, p. 281; vol. 35, 1883, p. 292; vol. 36, 1884, pp. 151, 206; Phil. Trans., 1878, pp. 55, 155; 1880, p. 65; 1883, 477.
6 Roy. Soc. Proc., vol. 23, 1875, pp. 356, 455; vol. 25, 1875, pp. 73, 547; vol. 26, 1877, pp. 90, 323; vol. 27, 1878, p. 60; vol. 29, 1879, p. 21; vol. 30, 1880, p. 302; vol. 32, 1881, pp. 385, 388; vol. 33, 1882, p. 423; Phil. Trans., 1878, pp. 163, 210; 1879, 165; 1880, p. 561.
7 Roy. Soc. Proc., vol. 29, 1879, p. 21: vol. 30, 1880, p. 302; vol. 32, 1881, pp. 385, 388; vol. 33, 1882, p. 453; Phil. Trans., 1879, p. 165, 1880, p. 561.
8 Roy. Soc. Proc., vol. 10, 1860, p. 256.
9 Roy. Soc. Proc., vol. 28, 1879, pp. 347, 477; Phil. Trans., 1879, p. 641; 1880, p. 135; 1881, 387.
10 Roy. Soc. Proc., vol. 28, 1878, p. 181.
11 Roy. Soc. Proc., vol. 12, 1862, p. 202.
12 Roy. Soc. Proc., vol. 37, 1884, pp. 78, 317; vol. 42, 1887, p. 371; vol. 47, 1890; pp. 300, 506.
13 Roy. Soc. Proc., vol. 42, 1887, p. 343; vol. 49, 1891, p. 84.
14 Roy. Soc. Proc., vol. 47, 1890, p. 118.
15 Tribulation, not undisturbed progress, gives life and soul, and leads to success when success can be reached, in the struggle for natural knowledge.
16 Crookes, "On the Viscosity of Gases at High Exhaustions," § 655, Phil. Trans., February, 1881, p. 403.
17 Phil. Trans., vol. 172 (1881), pp. 387, 435.

immediately gave it its true explanation—molecules of residual air, or gas, or vapour projected at great velocities[1] by electric repulsion from the negative electrode. This explanation has been repeatedly and strenuously attacked by many other able investigators, but Crookes has defended[2] it, and thoroughly established it by what I believe is irrefragable evidence of experiment. Skilful investigation perseveringly continued brought out more and more of wonderful and valuable results: the non-importance of the position of the positive electrode; the projection of the torrent *perpendicularly* from the surface of the negative electrode; its convergence to a focus and divergence thenceforward when the surface is slightly convex; the slight but perceptible repulsion betweed two parallel torrents due, according to Crookes, to negative electrifications of their constituent molecules; the change of direction of the molecular torrent by a neighbouring magnet; the tremendous heating effect of the torrent from a concave electrode when glass, metal, or any ponderable substance is placed in the focus; the phosphorescence produced on a plate coated with sensitive paint by a molecular torrent skirting along it; the brilliant colours—turquoise-blue, emerald, orange, ruby-red—with which grey colourless objects and clear colourless crystals glow on their struck faces when lying separately or piled up in a heap in the course of a molecular torrent; "electrical evaporation" of negatively electrified liquids and solids;[3] the seemingly red-hot glow, but with no heat conducted inwards from the surface, of cool, solid silver kept negatively electrified in a vacuum of 1/1,000,000 of an atmosphere, and thereby caused to rapidly evaporate. This last-mentioned result is almost more surprising than the phosphorescent glow excited by molecular impacts in bodies not rendered perceptibly phosphorescent by light. Both phenomena will surely be found very telling in respect to the molecular constitution of matter and the origination of thermal radiation, whether visible as light or not. In the whole train of Crookes' investigations on the radiometer, the viscosity of gases at high exhaustions, and the electric phenomena of high vacuums, ether seems to have nothing to do except the humble function of showing to our eyes something of what the atoms and molecules are doing. The same confession of ignorance must be made with reference to the subject dealt with in the important researches of Schuster and J. J. Thomson on the passage of electricity through gases. Even in Thomson's beautiful experiments showing currents produced by circuital electromagnetic induction in complete poleless circuits, the presence of molecules of residual gas or vapour seems to be *the essential*. It seems certainly true that without the molecules there could be no current, and that without the molecules electricity has no meaning. But in obedience to logic I must withdraw one expression I have used. We must not imagine that "presence of molecules is *the* essential." It is certainly *an* essential. Ether also is certainly *an* essential, and certainly has more to do than merely to telegraph to our eyes to tell us of what the molecules and atoms are about. If a first step towards understanding the relations between ether and ponderable matter is to be made, it seems to me that the most hopeful foundation for it is knowledge derived from experiment on electricity in high vacuum; and if, as I believe is true, there is good reason for hoping to see this step made, we owe a debt of gratitude to the able and persevering workers of the last forty years who have given us the knowledge we have: and we may hope for more and more from some of themselves and from others encouraged by the fruitfulness of their labours to persevere in the work.

The President then presented the medals awarded by the Society as follows:—The Copley Medal to Sir George Gabriel Stokes, Bart., F.R.S., for his researches and discoveries in physical science; a Royal Medal to Prof. A. Schuster, F.R.S., for his spectroscopic inquiries, and his researches on disruptive discharge through gases and on terrestrial magnetism; a Royal Medal to Prof. H. Marshall Ward, F.R.S., for his researches into the life-history of fungi and schizomycetes; and the Davy Medal to Prof. J. H. van't Hoff and Dr. J. A. Le Bel, in recognition of their introduction of the theory of asymmetric carbon, and its use in explaining the constitution of optically active carbon compounds.

In the evening the Fellows and their friends dined together at the Whitehall Rooms, Hôtel Métropole.

[1] Probably, I believe, not greater in any case than two or three kilometres per second.
[2] Address to the Institute of Telegraphic Engineers, 189.
[3] Roy. Soc. Proc., June 11, 1891.

THE TEMPERATURE OF IGNITION OF EXPLOSIVE GASEOUS MIXTURES.

AN important contribution to our knowledge of this subject is communicated to the *Berichte* by Prof. Victor Meyer of Heidelberg, in conjunction with his assistant, Herr A. Münch. The interesting experiments which were carried out some eighteen months ago in the Heidelberg laboratory, concerning the conditions under which the explosion or silent combination of gaseous mixtures occurs, left the question of the precise temperatures of explosive combination undetermined, inasmuch as the necessary high temperatures were attained by the use of boiling salts whose temperatures of ebullition lay a considerable number of degrees apart. The researches have since been continued under conditions in which it has been found possible to determine the actual temperatures with precision. In these experiments any possibility of the occurrence of appreciable amounts of silent combination has been avoided, in order that the determinations of the temperature of explosive combination might be unaffected by errors due to that cause. The conspicuous novelty of the method adopted consists in placing the small bulb containing the mixture to be exploded inside the larger bulb of the air thermometer employed to determine the temperature, thus at once ensuring that the explosion bulb and the thermometer bulb shall be heated to precisely the same temperature. The objection which at first suggests itself, that the heat suddenly developed at the moment of explosion might exert a disturbing influence upon the indications of the air thermometer, was proved by direct and repeated experiment to be without validity, such disturbance being found to be too small to be measured. The bulb in which the explosion is brought about is not closed, for the explosion of such detonating mixtures of gases at rest, that is to say, confined to a closed space, is so violent that if the glass escapes pulverisation it is much distorted, owing to the temperature to which it requires to be heated being about its softening point. The distortion usually takes the form of a shrinking from two opposite points, where the glass is drawn in and distended to such an extent as to produce two internal spheres. Such deformation would of course alter considerably the volume of the air thermometer. This is avoided by attaching a long stem to the bulb, open at the free extremity, and of passing a slow current of the gaseous mixture through the apparatus. The bulb of the thermometer was heated by means of a bath of a fused alloy consisting of equal parts of tin and lead, and it was found immaterial whether the thermometer was directly immersed in the molten metal or protected by means of a closely-fitting refractory metal sheath. The estimation of the temperature was effected by displacing the air of the thermometer, whose volume was known, by means of a current of hydrochloric acid gas, and measuring its volume over distilled water which had recently been freed from air by boiling.

The first series of experiments were made with the detonating electrolytic mixture of hydrogen and oxygen. The gases were freed from ozone by passage through a solution of potassium iodide. They were then washed through water, with which a Woulfe's bottle was almost filled, after which they traversed a tube packed with numerous discs of brass gauze, which were found effectual in preventing the explosion from travelling back to the Woulfe's bottle. The mixed gases were then allowed to enter the explosion bulb by means of a capillary tube passing down the stem to the bottom of the bulb. The rapidity of the gaseous stream was found to exert no influence upon the temperature of explosion, within the limits imposed by the mode of experimenting. The bath was then gradually raised to the neighbourhood of the combining temperature, and the instant the explosion ensued the air contained in the thermometer was displaced by hydrogen chloride, collected over water in the measuring vessel, and its volume ascertained on the attainment of atmospheric temperature and pressure. By displacing the air the instant the detonation was heard, any appreciable augmentation of the temperature during the moment of explosion was prevented.

As the result of several series of experiments carried out with four distinct sets of apparatus, the temperature of explosion of electrolytic hydrogen and oxygen is found to vary from 612° to 686°. It would thus appear, conformable with the supposition of Prof. Van t'Hoff from theoretical considerations, that this mixture is incapable of exhibiting a sharply fixed temperature of explosion. Moreover, it makes no difference whether the mixture is dry or moist; for if dried a small amount of silent combination invariably renders it again moist before explosion occurs.

It has been currently supposed that the presence of sharp solid fragments, such as those of glass, exerts a lowering effect upon the temperature of explosion of hydrogen and oxygen. This supposition has been practically tested and found wanting in accuracy. Neither glass fragments nor sea-sand were found to reduce the temperature below the limits above stated. A remarkable result, however, was obtained when pieces of platinum foil and wire were introduced into the explosion bulb. It was found impossible in their presence to bring about an explosion, even when the temperature of the bath was raised to 715°. Quiet combination invariably ensued.

The size of the explosion vessel appears to be immaterial, except when reduced to very small dimensions, such as 4·5 mm. diameter, as in the case of the smallest bulb tested, when the range of molecular forces is approached. In six experiments with this small bulb no explosion occurred; in others the explosion did not occur in the vessel, but the quiet combustion there initiated was transmitted along the leading tube, through the tube containing the brass gauze discs, and eventually occasioned an explosion in the wash-bottle, disastrous to the latter.

In the cases of other explosive mixtures the admixture was effected, in the proper proportion, in a three litre flask, from which the gases were driven first through a wash-bottle, and subsequently through a test-tube, arranged likewise as a small safety wash-bottle, to prevent the explosion reaching the larger one.

Carbon monoxide and oxygen, in the proportion to form carbon dioxide, were found to suffer, for the most part, silent combination in the apparatus, and the wide limits of the observed temperatures of explosion, 636° to 814°, in those cases when explosion did ensue, were found to be due to more or less of such silent combination.

Gaseous mixtures of hydrocarbons and oxygen were found, however, with the exception perhaps of marsh gas and oxygen, to exhibit practically no quiet combination; and these mixtures have afforded most trustworthy and constant temperatures of explosion.

Marsh gas was found to explode, as a rule, with oxygen at temperatures varying from 656° to 678°, but occasionally quiet and complete combustion occurred. Other hydrocarbons never failed to yield an explosion.

Ethane detonated with oxygen in three experiments at 622°, 605°, and 622° respectively. A mixture of ethylene and oxygen exploded at 577°, 590°, and 577° in three consecutive experiments. Acetylene prepared by Gattermann's method, which in Prof. Meyer's experience yields it in a purer state than the more recent convenient method discovered by Maquenne, explodes with oxygen with exceptional violence, the wash-bottle being destroyed in every experiment. The temperature of this explosion was very constant, 510°, 515°, and 509° being successively observed. Propane mixed with five times its volume of oxygen likewise exhibits a very constant temperature of ignition, 548°, 545°, and 548° being indicated in three determinations. Propylene exploded with four and a half times its volume of oxygen at 497°, 511°, and 499°. Isobutane mixed with six and a half times its volume of oxygen detonated at 549°, 550°, and 545°; and isobutylene at 546°, 548°, and 537°. Finally, coal gas mixed with thrice its volume of oxygen was found to explode in three experiments at the remarkably constant temperatures of 649°, 647°, and 647°. It was found impossible, however, to induce a mixture of coal gas and air to explode under these experimental conditions.

It will be clearly seen from the above experiments with gaseous mixtures of hydrocarbon and oxygen, that the temperature of explosion falls as the content of carbon increases. Thus the mean temperatures for methane, ethane and propane are 667°, 616°, and 547° respectively. Further, the temperature also falls with the degree of saturation, or in other words, the less saturated the hydrocarbons become the more readily do they ignite in contact with oxygen. Thus ethane, ethylene and acetylene explode with oxygen at 616°, 580° and 511°; propane and propylene at 547° and 504°; and isobutane and isobutylene at 548° and 543°. It will also be observed, however, as would be expected, that these differences due to difference of saturation diminish as the series are ascended.

A. E. TUTTON.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Mr. Austen Leigh, Provost of King's, the Vice-Chancellor, has been appointed a member of the Geographical Committee, in the place of Dr. Ferrers, resigned. The award of the Geographical Studentship of £100 will be made towards the end of the Lent Term.

The first award of the Walsingham Medal, founded by the Lord High Steward for the encouragement of biological research, has been made to Mr. E. W. MacBride, Fellow of St. John's, for his monographs in zoology.

MR. ARTHUR WILLEY, at present giving a course of lectures in Columbia College, New York, has been elected to the vacant Balfour Studentship by the Special Board of Biological and Geological Studies of the University of Cambridge. It is understood that the investigation prescribed for him will be that of the embryology of *Nautilus pompilius*, for which purpose he will proceed to the South Seas.

SCIENTIFIC SERIALS.

The Quarterly Journal of Microscopical Science for September, 1893, contains studies on the comparative anatomy of sponges: V. Observations on the structure and classification of the *Calcarea Heterocœla*, by Dr. Arthur Dendy (plates 10–14). In this paper the author gives a general account of the anatomy, histology, and classification of the *Calcarea Heterocœla*, from the point of view of one who has for a long time past been engaged in an independent study of the group, and he brings together all that is known on the subject. While on the classification of the group he departs somewhat widely from the lines laid down by previous writers, yet the necessity of doing so was forced upon him by a study of nearly fifty Australian species. The author finds neither the canal system nor the skeleton affords a reliable guide for classification, and a compromise is the only satisfactory way out of the difficulty. The families adopted are: (1) Leucasidæ, (2) Sycettidæ, (3) Grantidæ, (4) Heteropidæ, (5) Amphoriscidæ.—On some points in the origin of the reproductive elements in Apus and Branchipus, by J. E. S. Moore (plates 15 and 16). Calls attention to some important details in the spermatogenesis of Branchipus and in the ovigenesis in Apus. In the former, the observations bear out the general law as to the similarity of the male and female cells, their specific peculiarities being physiological in origin, without morphological import. The divisional phenomena of these cells are intimately related to a protoplasmic structure, which might be fitly described as "Schaumplasma," and one of the initial impulses towards metamorphosis is a fusion of some of the intra-nuclear globules; while a considerable portion of the complicated karyokinetic figures, with their centrosomes, pseudosomes, and dictyosomes, appear to be the logical as well as the actual consequence of the continuance of this process. Some time before and always during the course of the chromatic changes bodies answering to the centrosomes in all details except in their numbers, which is much greater, make their appearance; these the author provisionally names "pseudosomes." The term "dictyosomes" is given to bodies which make their appearance connected one to another and to the inner group of chromosomes by fine strands, and which remain uncoloured by reagents, and are more or less related to the cell periphery. (In connection with these, Farmer's notes and figures of like bodies in the Pollen mother-cells is of interest. (See *Ann. of Bot.* September, 1893).—Notes on the Peripatus of Dominica, by E. C. Pollard (plate 17). Miss Pollard's species is apparently very nearly related to *P. edwardsii*, but differs in the number of ambulatory appendages, there being 29 to 34 pairs in *P. edwardsii*, while in *P. dominicæ*, sp. nov., there are from 25 to 30.—Studies on the Protochordata, by Arthur Wiley, B.Sc. (plates 18–20). II. The development of the neuro-hypophysial system in *Ciona intestinalis* and *Clavelina lepadiformis*, with an account of the origin of the sense-organs in *Ascidia mentula*. III. On the position of the mouth in the larvæ of the Ascidians and Amphioxus, and its relations to the Neuroporus.

Symons's Monthly Meteorological Magazine, November. Mr. Symons gives a summary of all the rainfall observations known to have been taken in Persia; the only places at which such appear to have been made are Ooromiah, in the north-west; Bushire, on the eastern shore of the Persian Gulf, and at Teheran. At Bushire the annual mean for 1878–90 is 12·96 inches. Recent observations at Teheran give a mean of about 10 inches, and the older observations, taken at the Russian Embassy, give a mean of about 11 inches, of which nearly the whole falls in the winter half of the year. To the north of the great mountain range, between Teheran and the Caspian, the fall is nearly four times as great as in Persia. The same number of the magazine contains a summary of the few meteorological papers read at the British Association at Nottingham.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, November 16.—"Experiments in Heliotropism." By G. J. Romanes, F.R.S.

I cannot find in the literature of heliotropism that any experiments have hitherto been made on the effects of interrupted illumination, when the periods of illumination are rendered as brief as possible—*i.e.*, instantaneous flashes of light. Accordingly I have conducted an extensive research on heliotropism, where the flashes have been caused either by means of electric sparks in a dark room, or by the opening of a photographic shutter placed before the plants in a camera obscura with an arc light or Swan burner, at a distance of several feet on the other side of the shutter. The electric sparks were made either with a Wimshurst machine, induction sparks, or by means of the following contrivance. From the binding screws of the condenser of a large induction coil copper wires were led to a cup of mercury, where, by means of an electro-magnet suitably actuated by clockwork, a current was closed and opened at any desired intervals: each break was therefore accompanied by a brilliant spark. A thick plate of glass was interposed between the seedlings and the electrical apparatus. In all the experiments here described the plants employed were mustard seedlings (*Sinapis nigra*), previously grown in the dark until they had reached a height of between one and two inches. Save when the contrary is stated, in all the experiments comparative estimates were formed by using the same pot of seedlings: during the first half of a comparative experiment half of the seedlings were protected from the light by a cap of cardboard covering half the pot; during the second half of the experiment this cap was removed, and the pot turned round so as to expose the previously protected seedlings to the influence of the light. The principal results thus obtained, and frequently corroborated, were as follows:—

I. Even having regard to the fact that for equal strengths of a stimulus excitable tissues are more responsive in proportion to the suddenness of the stimulus (or in a kind of inverse proportion to the duration of the stimulus), the heliotropic effects of such flashing stimulation as is above described proved to be much greater than might have been antecedently expected. This was shown to be the case whether the effects were estimated by the rapidity with which the seedlings began to bend after the flashing stimulation was begun, or by that with which they continued to bend until attaining a horizontal line of growth, *i.e.* bending to a right angle. Thus, at a temperature of 70° Fahr., and in a moist camera, vigorously growing seedlings begin to bend towards the electric sparks ten minutes after the latter begin to pass, and will bend through 45° in as many minutes; frequently they bend through another 45° in as many minutes more. This is a more rapid rate of bending than can be produced in the same pot of seedlings when the previously protected side is uncovered and exposed for similar durations of time, either to constant sunlight or to constant diffused daylight. This is the case even if the sparks (or flashes) succeed one another at intervals of only two seconds.

II. It would thus appear that the heliotropic influence of electric sparks (or flashes) is greater than can be produced by any other source of illumination. But in order to test this point more conclusively, I tried the experiment of exposing one half-pot of seedlings in one camera to the constant light of a Swan burner, and another half-pot of similar seedlings in another camera, placed at the same distance from the same source of light, but provided with a flash shutter working at the rate of two seconds intervals. The amount of bending in similar times having been noted, the pots were then exchanged and their previously protected halves exposed to the constant and the flashing light respectively. In both cases, the rapidity with which the bending commenced and the extent to which it proceeded in a given time after commencement, were considerably greater in the seedlings exposed to the flashing than to the constant source stimulation. The same is true if, instead of a Swan burner, the source of light is the sun.

III. Many experiments were tried in order to ascertain the smallest number of sparks in a given time which would produce any perceptible bending. Of course the results of such experiments varied to some extent with the condition of the seedlings. But in most cases, with vigorous young mustard seedlings and careful observation, bending could be proved to occur within fifteen to thirty minutes, if bright sparks were supplied at the rate of only one per minute. The most extreme sensitiveness that I have observed in these experiments was that of perceptible bending after half-an-hour's exposure to electrical sparks following one another at the rate of fifty in an hour. This result would appear to indicate that in heliotropism under flashing light there need be no summation or "staircase effect"; but that each flash or spark may produce its own effect independently of its predecessors or successors.

IV. It is noteworthy that, while the heliotropic effects of flashing light are thus so remarkable, they are unattended with the formation of any particle of chlorophyll. In the many hundred pots, and therefore many thousands of plants, which have passed under my observation in this research I have never seen the slightest shade of green tingeing the etiolated seedlings which had bent towards flashing light. On one occasion I kept a stream of 100 sparks per second illuminating some mustard seedlings continuously for forty-eight hours; and although this experiment was made for the express purpose of ascertaining whether any chlorophyll would be formed under the most suitable conditions by means of flashing light, no change of colour in any of the seedlings was produced.

With the exception of those mentioned in the last paragraph, all these results were obtained by using sparks from the coil condenser, as above explained. These sparks were very brilliant, and yielded the maximal results, which alone are here recorded.

"Experiments in Germination." By G. J. Romanes, F.R.S.

The primary object of these experiments was to ascertain whether the power of germination continues in dry seeds after the greatest possible precautions have been taken to prevent any ordinary processes of respiration for practically any length of time.

The method adopted was to seal various kinds of seeds in vacuum tubes of high exhaustion, and after they had been exposed to the vacuum for a period of fifteen months to remove them from the tubes and sow them in flower-pots buried in moist soil. In other cases, after the seeds had been *in vacuo* for a period of three months, they were transferred to sundry other tubes respectively charged with atmospheres of sundry pure gases or vapours (at the pressure of the air at time of sealing); after a further period of twelve months these sundry tubes were broken, and their contents sown as in previous case. In all cases, excepting that of clover, the seeds sown were weighed individually in chemical balances, and seeds of similar weights taken from the same original packets were similarly sown as controls.

The exhaustion of the tubes was kindly undertaken by Mr. Crookes, F.R.S., to whom I must express my best thanks for the assistance he has given. The kinds of seeds used were mustard, red beet, clover, peas, beans, spinach, cress, barley, and radish. In addition to vacuum tubes and control tubes containing air, others were charged with oxygen, hydrogen, nitrogen, carbon monoxide, sulphuretted hydrogen, aqueous vapour, ether, and chloroform.

With the exception of the beans, where only two were sown, ten weighed seeds were sown out of each of the tubes, and also out of each of the control packets which had been kept in ordinary air from the first. These results amply prove that neither a vacuum of one-millionth of an atmosphere, nor the atmospheres of any of the gases and vapours named, exercised much, if any, effect on the germinating power of any of these seeds. I may add that the same remark applies to an atmosphere of carbon dioxide, although in the particular series of experiments quoted this gas was accidentally omitted.

A subsidiary object of these experiments was to ascertain whether any appreciable variations would be caused in plants grown from seeds which, before germination, had been submitted to the conditions above explained. Hundreds of plants of the kinds named were grown from the seeds in the various tubes. But in no one instance was there the smallest deviation in any respect from the standard type grown from the corresponding control packet.

In the case of the beet-root, a larger number of plants were developed in many of the pots than the ten seeds which had been sown in each. This I found to be due to the fact that beet-root seeds very frequently throw up two seedlings apiece. Not so frequently, but still very often, they yield three, and sometimes even four.

Further experiments are in progress.

"On hepatic glycogenesis," by Dr. Noël Paton, Superintendent, Research Laboratory of the Royal College of Physicians of Edinburgh.

The object of the research is to determine the *mode* of conversion of glycogen to sugar in the liver. Is it due to a zymin, or to the metabolism of the liver protoplasm? A study of the *rate of conversion* of glycogen in the excised liver at the body temperature shows that there is an initial rapid and a subsequent slow stage in the process. The former occurs before visible morphological changes can be detected in the cells; the latter goes on after the cells are disorganised. The former is inhibited by *destroying the cells* (by pounding with sand), and by the presence of one per cent. of *fluoride of sodium;* the latter is not stopped thereby. The *product* of the former is glucose; of the latter, glucose with dextrin and, possibly, maltose. Agents, such as chloroform, ether, and pyrogallic acid, hasten the disintegrative changes in the cells, and accelerate the early rapid stage of conversion, but do not influence the later slower stage. During life the first may produce glycæmia by this action on glycogen conversion. They seem to act by hastening the katabolic changes immediately preceding cellular death. Drugs, such as curare, morphin, and nitrite of amyl, which cause glycæmia, do not do so by increasing the conversion of glycogen; they do not accelerate the morphological changes in the cells. These observations show that the *early rapid changes are due to the metabolism of the protoplasm.* The later slower changes are not due to the *acid* which develops, nor are they, to any marked extent, due to the action of *micro-organism;* they seem to be brought about by a *zymin* developed as a result of the disintegration of the cells.

November 23.—"Magnetic Observations in Senegambia." By T. E. Thorpe, F.R.S., and P. L. Gray.

On the occasion of the recent Eclipse Expedition to Senegambia we took with us a set of magnetic instruments of the Kew pattern, with a view of making observations in a district for which the magnetic elements have not hitherto been determined.

Observations were made at Fundium, Senegal, and at Bathurst, on the River Gambia.

The results are as follows:—

Fundium, Senegal, lat. 14° 7'·4 N., long. 16° 32' W. (approx.).

The observations were made on April 4, 5, and 14, 1893, in the vicinity of the Eclipse Camp and on a partially enclosed piece of ground between the Administrator's house and the River Salûm, about 80 yards from the shore. The temperature during the force observations was about 30° C.

The results are as follows:—

Declination	=	18° 44' W.
Horizontal force	=	0·30409 c.g.s.
Dip	=	Needle 1, 29° 9'·1
,, ...	=	,, 2, 29° 8'·2

Bathurst, River Gambia, lat. 13° 28' N., long. 16° 37' W.

The station was on a large piece of open ground and near the centre of McCarthy Square. All the observations taken were made on April 20, 1893.

Declination	=	18° 50' W.
Horizontal force	=	0·30514 c.g.s.
Dip	=	Needle 1, 28° 43'·4.
,, ...	=	,, 2, 28° 42'·1.

Physical Society, November 24.—Prof. A. W. Rücker, F.R.S., President, in the chair.—Colonel Maitland, C.B., was elected a member of the Society. Prof. S. P. Thompson then occupied the chair whilst the President read a paper on the magnetic shielding of concentric spherical shells. In this mathematical investigation the author considers cases in which the equipotential surfaces are surfaces of revolution about a line through the centre of the shells, and the permeability (μ) of each shell is constant. Taking the common centre as origin, the potential within any shell is expanded in terms of zonal spherical harmonics, and the ratio of the shielded to the unshielded field determined. The following important result is arrived at, viz. if the permeabilities of the enclosed and external space be the same, then the ratios of the shielded to the unshielded fields are the same *for each harmonic term*, whether the part shielded be external or internal. It is also shown that the shielding effect on external space when a small magnet is placed at the centre of the shell is the same as the shielding effect on the enclosed space when the shells are placed in a uniform magnetic field. The case of a single shell with a small magnet at the centre is next considered where the permeabilities of the internal and external spaces are taken as unity. Here the shielding depends on the ratio of the outer to the inner radius (a_1/a_0). When the thickness of the shell is 1/100 of a_1 the ratio of shielded to unshielded field (Ψ/ψ_0) is 3/13 when $\mu = 500$, and 3/23 when $\mu = 1000$. For $\mu = 1000$, increasing the thickness from *a* 1/10 to *a* 1/2 changes the shielding from 1/60 to 1/194, thus showing that after the shell is moderately thick, further increasing the thickness is not very effective. When the small magnet is displaced from the centre of the shell with its axis along a radius, then the shielding effect of the shell is greater on the side towards which the magnet is moved, and less on the opposite side. Thickening a single shell being inefficient, the effect of using two or three shells separated by air-gaps is investigated. Here, as in the case of a single shell, the shielding is improved by adding permeable material either within the inner or without the outer shell. If the inner and outer diameters are given then when the difference in these diameters is small, one continuous shell gives the best result. For a larger difference, two shells separated by an air-gap are much more efficient than a single one, and filling up the air-gap would appreciably diminish the screening effect. When the permeability of the substance is high the best shielding is obtained when the radii of the bounding surfaces of the shells are in geometrical progression. The great value of lamination is shown in the following table, where the volume of the permeable material is expressed in terms of that of the enclosed space, and the shielding in each case being the best.

	Volume of material used.	External field.
Single shell ...	1·0	0·018
Two shells	5·0	0·0006
Three shells ...	4·8	0·00016
Single shell ...	7·0	0·0102

The conditions for the best arrangement in each of the following cases are fully worked out in the paper, viz. Two shells when the largest and smallest radii and the volume of the material used are given; two contiguous shells of different permeablities; and three shells of different permeabilities. The main results of the investigation are that with thin shells lamination is useless, while with thick shells it is essential, if the best effect is desired. Experiments made on actual shells had fully confirmed the theoretical conclusions. Prof. Minchin said the mathematical results were very simply expressed. Although the work was apparently restricted to zonal spherical harmonics, some of the important formulæ apply equally to general spherical harmonics. Referring to the difficulty of shielding by single thick shells, he pointed out that the equation giving the relation between the shielded and unshielded fields with different thicknesses of shell represented a hyperbola with its asymptotes parallel to the axes; hence the shielding tended to a definite limit as the thickness increased indefinitely. Mr. Evershed said he had been engaged for the last two years on the subject of magnetic shielding, with a view to screening measuring instruments from external fields. In such cases it was not possible to use closed shells, and this introduced trouble. The best results he had yet obtained was to reduce the disturbance to about one-fifth. Another difficulty was introduced by the fact of the shields being magnetised by the current passing through the coil, and owing to hysteresis, the permeability was different according as the magnetisation increased or decreased. By using an outer iron shell a great improvement had been effected. To obtain the best results, it was important to have no joints in the shields. A coil frame with two shields of bent iron was exhibited. Mr. J. Swinburne remarked that the subject divided itself into two, shielding of instruments and shielding sources. If a dynamo itself be shielded, this did not prevent the currents in the leads producing magnetic disturbances. This was very important in ships. By using an alternator with resolving fields all disturbances could be avoided. Dr. C. V. Burton inquired whether by considering the hydrodynamical analogue of a porous material the case of perforated shells could be elucidated? Mr. A. P. Trotter wished to know if the homogeneity of the shield was of much consequence? At Oxford it had been found that a screen of four inches of scrap-iron was better than boiler-plate. Mr. Blakesley asked if the effect of moving a magnet sideways in a sphere had been observed. He thought the mathematics developed in the paper would be useful in working out the magnetic theory of the earth. Prof. S. P. Thompson thought that taking the permeability as constant would not be quite correct, for μ was a function of the magnetisation. Hence in the cases considered the outer shell would be the more permeable. In his reply, the President said scrap-iron in contact was not like clear space, for there were comparatively free paths for the induction at the points of contact. As regards the shielding of the dynamo at Greenwich, Mr. Christie had written to say that the credit was due to the makers of the machine and shields, Messrs. Johnson and Phillips.—Prof. G. M. Minchin read a paper on the action of electromagnetic radiation on films containing metallic powders. After noticing the resemblance of the phenomena exhibited by tubes containing metallic filings shown by Mr. Croft, on October 27, to those of photoelectric impulsion cells, he repeated some of the experiments with filings, and found the same effects when the filings were of ordinary fineness. He also noticed that the experiments did not succeed either when the filings were coarse or very fine. Coarse ones always conducted, whilst very fine filings or powders acting as insulators, except when strongly compressed. To establish a closer connection with the impulsion cells he tried films of gelatine and collodion containing metallic powders. Directions for preparing the films are given in the paper. On inserting such a film in circuit with a battery, key, and galvanometer, it acts as an insulator. To render a small portion conducting, the electrodes on the surface of the film are brought very close together, and one of the wires touched with an electrified body (an electric gas-lighter was often used). This caused a current to pass. The electrodes may then be separated a little further, and the process repeated until any desired portion is rendered conducting. The peculiarity of such a film is that if the circuit be broken at the film, the film becomes an insulator; whereas breaking the circuit at any other point leaves the film conducting. The action of the sparks or charges on the conductivity of the films is attributed to the influence of electric surgings in the wires by the electric discharges. The President read a written communication from Prof. O. J. Lodge, in which the writer suggested that the phenomena of the films, and also of Lord Rayleigh's water-jet experiment (in which water-drops are caused to coalesce by the presence of an electrified body), were due to the range of molecular attraction being increased by electric polarisation. Mr. Blakesley said he had tried Mr. Croft's experiments, and found that conductivity could be established in a tube of filings whilst the circuit was unclosed. Breaking the circuit of a transformer or electromagnet would produce conductivity; hence he concluded that electric surgings were not essential. Another curious experiment was to put the discharging knobs of an electric machine on a photographic plate at a distance of a few inches. On turning the machine a small spark travels slowly along the plate from the negative to the positive knob. On reversing the polarity of the machine the spark travels back along the same path, but if the polarity remains unchanged a second spark usually travels along a different path. Prof. C. V. Boys asked Prof. Minchin whether the films themselves, or the contacts between the electrode and film is made conducting by the sparks? Prof. S. P. Thompson wished to know if ordinary photographic dry-plates would serve the purpose? Mr. Evershed inquired whether the metal used as electrode made any difference? Prof. Minchin, in his reply, maintained that the phenomena were due to electric impulses. He had not tried photographic plates, and had always used platinum for his electrodes.

Chemical Society, November 16.—Dr. Armstrong, President, in the chair.—A letter has been addressed to Prof. Mendeléef, congratulating the Russian Chemical Society on the celebration of its twenty-fifth anniversary. The following papers were read:—The normal butylic, heptylic, and octylic ethereal salts of active glyceric acid, by P. Frankland and J. MacGregor. The authors have determined the rotatory powers of the homologous series of ethereal salts of active glyceric acid up to octylic glycerate; the molecular rotations of the normal and secondary butylic salts are greater than those of any others of the series. This kind of result has been predicted by Guye.—The ethereal salts of diacetylglyceric acid in relation to the connection between optical activity and chemical constitution, by P. Frankland and J. MacGregor. The authors have prepared the methylic, ethylic, propylic, isopropylic, and isobutylic salts of active diacetylglyceric acid. In the case of the first two of these salts, two of the atomic groups attached to the assymetric carbon atom are of equal mass; according to Guye's theory, these should be almost, or quite, optically inactive. This, the authors find, is not the case, and they therefore again urge that the qualitative nature of the groups attached to the assymetric carbon atom must be considered, as well as their masses.—The oxidation of paratoluidine, by A. G. Green. The red base obtained by Barsilowsky by oxidising paratoluidine with ferrocyanide is a diparatolylimide of the constitution

$$MeC\left\langle\begin{array}{l}C:(NC_7H_7)\cdot CH\\ CH\cdot C:(NC_7H_7)\end{array}\right\rangle CNH_2$$

On reduction it yields a stable leuco-base.—The action of benzoic chloride on urine in presence of alkali. Formation of benzoic derivatives of urochrome, by J. L. W. Thudichum. By the action of benzoic chloride on alkaline urine, a mixture of benzoic derivatives of urochrome is deposited.—The combination of hydrocarbons with picric acid and other nitro compounds, by W. A. Tilden and M. O. Forster. Picric acid combines with terpene, giving a compound which forms a peculiar potassium salt, yields picramide and borneol when treated with alcoholic ammonia, and gives borneol when treated with aqueous alkalis.—The formation of pyrrol derivatives from aconitic acid, by S. Ruhemann and F. E. Allhusen.—The conversion of α hydrindonoxime into hydrocarbostyril, by F. S. Kipping. α-hydrindonoxime yields hydrocarbostyril when treated with phosphorus pentachloride.—The constitution of lapachol and its derivatives. II. The azines of the lapachol group, by S. C. Hooker. The author describes methyllapazine, methyllapeurhodone, methylhydroxylapeurhodone, and several of their halogen derivatives.

Geological Society, November 22.—W. H. Hudleston, F.R.S., President, in the chair.—The following communications were read:—The basic eruptive rocks of Gran, by Prof. W. C. Brögger. In previous communications the author has maintained that the different masses of eruptive rock which occur within the sunken tract of country lying between Lake Mjösen and the Langesundsfjord are genetically connected, and have succeeded each other in regular order. The oldest rocks are the most basic, the youngest (except the unimportant dykes of diabase) are the most acid, and between the two extremes he has found a continuous series. He is now preparing a detailed monograph on this series of eruptive rocks, and in the present communication he gave an account of the results of his work on the oldest members. Several bosses of basic plutonic rock, now forming a series of dome-shaped hills, lie along a north-and-south fissure line. The most northerly is that of Brandberget in the parish of Gran, about 50 or 60 kilometres north-north-west of Christiania, and the most southerly occurs at Dignaes on Lake Tyrifjord, about 35 kilometres west-north-west of the same town. The prevailing rock in these bosses is a medium or coarse-grained olivine-gabbro-diabase; but pyroxenites, hornblendites, camptonites, labrador-porphyrites, and augite-diorites also occur. Analyses of the typical rocks from three localities on the north and south line were given, and the conclusion was reached that the average basicity of the rocks forming different bosses decreases from north to south. The contact-metamorphism was referred to; and the presence of hypersthene in the altered *Ogygia* shales, coupled with its absence from the same shales where they have been affected by quartz-syenite, led the author to the conclusion that the chemical nature of the intrusive rock does, in certain cases, produce an influence on the character of the metamorphism. Innumerable dykes and sheets of camptonite and bostonite are associated with the above-mentioned plutonic bosses. These are regarded by the author as having been produced by differentiation from a magma having the composition of the average olivine-gabbro-diabase. Analyses were given, and it was proved that a mixture of nine parts of the average camptonite and two of the average bostonite would produce a magma having the composition of the average olivine-gabbro-diabase. The petrographical variations, such as the occurrence of pyroxyenites and augite-diorites, in the plutonic masses themselves are described, and attributed to differentiation under physical conditions unlike those which gave rise to the camptonites and bostonites. In discussing the general laws of differentiation the author pointed out that it must have taken place before crystallisation to any extent had occurred, because there is a marked difference in mineralogical composition between the rocks occurring as bosses and those occurring as dykes; and, further, that it is dependent on the laws which determine the sequence of crystal-building, in so far as the compounds which, on given conditions, would first crystallise are those which have diffused to the cooling margin, and so produced a contact-stratum, of peculiar chemical composition, before any crystallisation had taken place. A discussion followed, in which the President, Prof. Judd, General McMahon, Prof. J. F. Blake, and Mr. W. W. Watts took part.—On the sequence of perlitic and spherulitic structures (a rejoinder to criticism), by Mr. Frank Rutley. This paper related to the order in which the perlitic and spherulitic structures have been developed in a felsitic lava of Ordovician age from Long Sleddale, Westmoreland. The author having described this rock in a paper published in the Quarterly Journal of the Society in 1884, and the accuracy of the views then expressed having been questioned, endeavoured to confirm his original statements, adducing in support fresh observations made upon this and other rocks of a similar kind. Mr. Marr and Dr. J. W. Gregory spoke on the subject of the paper, and the author briefly replied.—Enclosures of quartz in lava of Stromboli, &c., and the changes in composition produced by them, by Prof. H. J. Johnston-Lavis. The author described the existence of enclosures of quartz in a lava-stream at the Punta Petrazza on the east side of Stromboli, and also in the rock of the neck of Strombolicchio. He described the effects of the rocks upon the enclosures, concluding that the quartz has undergone fluxion but not fusion, and has supplied silica to the containing lavas, thus causing an increase in the amount of pyroxene and a diminution in the amount of magnetite in the portions of those lavas that surround the inclusions and raising the percentage of silica. He suggested that such a process at greater depths and higher temperature may, under certain conditions, convert a basic rock into a more acid one, so that possibly the andesite of Strombolicchio may have been of basaltic character at an earlier period of its progress towards the surface. He offered the suggestion that other rocks or minerals once associated with the quartz have been assimilated by the magma. The President and Prof. Judd made a few remarks upon the paper.

CAMBRIDGE.

Philosophical Society, October 30.—Prof. Hughes, President, in the chair.—The following officers were elected for the ensuing session:—President, Prof. Hughes; Vice-Presidents, Prof. Cayley, Prof. Darwin, Dr. A. Hill; Treasurer, Mr. Glazebrook; Secretaries, Mr. Larmor, Mr. Newall, Mr. Bateson; New Members of Council, Prof. Sir G. G. Stokes, Dr. Lea, Mr. Shipley, Mr. Seward.—The President (Prof. Hughes) read a paper on the geological evidence for the recurrence of ice ages. Prof. Hughes pointed out that the advocates of the astronomical explanation of glacial ages have urged that there has been a recurrence at regularly varied intervals of combinations, the result of which must have been circumpolar vicissitudes of climate; and, seeing that the secular recurrence of these conditions formed a necessary part of their theory, they gladly welcomed any confirmation of it, such as was offered by those geologists who saw in the character of the stones in certain conglomerates traces of ice-action in several successive geological periods. The value of this evidence he now criticised. He laid before the Society examples of the striated boulders and rock floors supposed to present glaciated surfaces, and with a view to the elimination of sources of error in the identification of the work of ice he exhibited a large series of specimens illustrating the various ways in which results were produced sometimes exactly the same as, and often closely resembling, the forms, markings, and other characters relied upon as proofs of ice action. By reference to these he showed that the facetted stones from which the extension of the glacial conditions over parts of Southern Germany was inferred, found their exact counterparts among those trimmed by blown sand into roof-like forms and ridges, and had no parallel among undoubtedly glacially-dressed stones. The scratched stones in the base of the New Red, or so-called Permian of England (with the exception of one single specimen, which he said must have got into the collection in Jermyn Street by mistake), he compared with those produced by earth movements, in which the included pebbles of the conglomerate were protruded through the softer matrix and scored and indented by harder fragments held in the mass. The supposed glaciation of the boulders in the basement beds of the carboniferous he explained in the same way, producing examples in which the matrix and included fragments were scored alike by movements along small fault faces. He exhibited a portion of the solid silurian floor on which these conglomerates rested, which was striated in a manner that might be easily mistaken for glacial action; but he explained that he had taken this from a thrust plane, and he pointed out the difference in the mineral condition of the surface between these slickensided surfaces and those produced by glacial action. He excluded from the present discussion cases in which ice agency was inferred only from the size and shape of the stones or their isolation in finest material. He admitted the probability of evidence of ice action being found along known axes of recurrent upheaval, such as those in the most ancient rocks along the Scandinavian range, or in the more recent deposits along the Alpine chain, or further south in the carboniferous boulder beds of India, Africa, and Australia; but he pointed out that these last, at any rate, could lend no support to the astronomer's contention, seeing that they surrounded a basin whose centre was in equatorial, not in circumpolar regions. He was willing to admit that in the astronomical combinations we might find a *vera causa* of vicissitudes of climate, but he urged that all the evidence from direct observation went to show that extreme glaciation does and did always bear a direct relation to earth movements.

PARIS.

Academy of Sciences, November 27.—M. de Lacaze-Duthiers in the chair.—On the registration of the variable elements of the sun, by M. H. Deslandres.—On equations and implicit functions, by M. Pellet.—On surfaces admitting of gauche cubics for asymptotic lines, by M. Blutel.—On ripples (*clapotis*), by M. E. Guyou. Equations are obtained in which elliptic functions are substituted for the circular functions employed by Boussinesq. According to these equations, each molecule oscillates along a straight line of fixed direction which itself oscillates vertically, and the resultant motion takes place along a parabola whose axis is vertical. For the surface molecules, the first movement is that of the projection upon the minor axis of an ellipse of a point describing the contour of the curve with a constant linear velocity. On examining the photographic tracing obtained by M. Marey it is found that the motion of the surface molecules takes place along a very flat closed curve. This divergence from the theory is easily explained by the oscillations of the cylinder producing in the experimental basin a vertical displacement of the layers which are theoretically at rest. The superposition of this motion upon that indicated by the theory has the effect of separating in a vertical sense the trajectories corresponding to the two inverse phases of an oscillation.—Mutual action of bodies vibrating in fluid media, by MM. Berson and Juppont. Two vertical discs were placed in air with their axes coincident. The one was made of steel, 0·033 cm. thick and 12 cm. in diameter, and was kept vibrating by two small electromagnets, excited by currents of intensities, varying according to the amplitudes required. The other was of mica, 0·012 cm. thick and 6 cm. in diameter, fixed normally to the bent end of a light bar of aluminium, which, supported by a long silver wire, formed the movable part of a torsion balance. The movement of this disc is due to the surrounding air, thus being analogous to electrostatic induction. The experiments were made inside a cage draped with soft and loose cloth, to prevent resonance. The torsion was measured with a Vernier micrometer to $\frac{1}{20}$ degree. The attractions exhibited between the two discs when vibrating ranged from half a dyne to about 600 dynes. At a distance of 1 mm. it was 602·3 dynes; at 2 mm., 98·0; at 4 mm., 14·5, and at 10 mm., 2·55. To produce the same forces electrostatically, a difference of potential of 600 volts would be required. The authors intend to study the effect of distance and of the medium in the case of pulsating spheres.—Calculation of the forces to which bodies placed in an electromagnetic field are subjected, by M. Vaschy.—On the variation of the electric state of the high atmospheric regions in good weather, by M. Ch. André.—On the preparation of metallic lithium, by M. Guntz.—Improvement of culinary and lubricating oils by an electric treatment, by M. L. A. Levat.—On chloralose, by MM. M. Hanriot and Ch. Richet.—On some facts relating to the effects of injections of organic liquids upon animals, by M. E. Meyer.—On absorption by the urinary ducts, by M. Bazy.—Transpiration and respiration as functions determining the habitat of the Batrachians, by M. A. Dissard.—On a ptomaïne extracted from urine in influenza, by MM. A. B. Griffiths and R. S. Ladell. This ptomaïne is a white substance crystallising in prismatic needles, soluble in water, and showing a feebly alkaline reaction. Its formula is $C_9H_9NO_4$. It is a poison producing strong fever and death in eight hours, and is not met with in normal urine.—On a new genus of fishes, related to Fierasfer, by M. Léon Vaillant.—On the male genital apparatus of the Hymenoptera, by M. Bordas.—Researches on the anatomy and development of the female genital apparatus of the Orthoptera, by M. Peytoureau.—On the localisation of the active principles in the Limnantheæ, by M. Guignard.—On the localisation of the active principles in the Cucurbitaceæ, by M. L. Braemer.—Experiments on the disinfection of mushroom beds, by M. I. Costantin.—On the exchanges of carbonic acid and oxygen between plants and the atmosphere, by M. Th. Schlœsing fils.—Subterranean grafting, applied to the preservation of ungrafted French vines, by M. Geneste.—On the requirements of direct or grafted vines, by M. Albert Renault.—Study of a variety of the cider apple in all its life periods, by M. A. Truelle.—Proofs and cause of the actual slow movements of Scandinavia, by M. A. Badoureau.—Observations on the oolitic limestone superior to the gypsum of Villejuif, near Paris, by M. Stanislas Meunier.

AMSTERDAM.

Royal Academy of Sciences, October 28.—Prof. van de Sande Bakhuysen in the chair.—Prof. Behrens treated (1) on the structure of native gold. Several samples of auriferous quartz were examined under the microscope, on the supposition that they would be found to contain agglomerated granules of metal. The gold was found to be crystallised in cubes, and in combinations of the cube with the octahedron, so perfectly as even to present good cleavage planes. It would appear, therefore, that such gold must have been precipitated and crystallised at the same time and under the same conditions as the surrounding quartz. Further evidence for this conclusion was afforded by the presence of microscopical cavities in small nuggets. Like the cavities in quartz, they occur in streaks and small heaps, being partly spherical or oval, and partly of a subangular shape. By the distribution of the alloyed silver in concentric layers of rich and poor alloy, the supposition of a molten state is excluded. All these peculiarities were also found in grains of gold washed from an auriferous ochre, enclosed by auriferous quartzite. (2) On the chemical constitution of alloys. (i.) Lead was found to be a good solvent for crystallising copper and its alloys, a small quantity of lead only being taken up by the latter. If a piece of copper is put into red-hot lead containing a little tin, the surface of the copper is changed to bronze, which does not melt, but will partly dissolve in the lead, and on cooling separate out in crystals. Bronze and common brass will not split up into definite alloys under this treatment. The alloy of copper with 10 per cent. aluminium, which is said to be homogeneous, behaves differently. It will yield red crystals in the upper part of the button, yellow ones in the middle, and white ones near the bottom of the crucible. (ii.) Is copper in bronze univalent or bivalent? With a view to solving this question, a series of silver-tin alloys was compared with corresponding bronzes. The following results were obtained: Regular crystals, Cu_6Sn and Ag_6Sn, Cu_3Sn and Ag_3Sn, CuSn and AgSn, $CuSn_2$ and $AgSn_2$; other systems (rhombohedric?) Cu_4Sn and Ag_4Sn (maximum of hardness), Cu_2Sn and Ag_2Sn (second maximum), Cu_2Sn_3 and Ag_2Sn_3. Now, for Ag_4Sn, no other structural formula can be admitted than $Ag_2 = Sn = Ag$, hence there is a great probability for the univalent character of copper in its alloys with tin. Several of the other formulæ will probably have to be doubled. The formula Ag_6Sn cannot be construed here, and the reasoning leads to the supposition that also in bronzes rich in copper the surplus of the predominant metal is simply dissolved in an isomorphous combination.—Prof. Schoute treated on regular sections and projections of the ikosatetrahedron. The author studied the central sections perpendicular to, and the orthogonal projections in, the direction of four lines that join the centrum to a vertex, or to the centre of an edge, of a triangular face, or of a bounding octahedral three-flat. As to the vertices, the four dimensional being L^{24}_{a} (24 = number of bounding three-flats, a = length of edge), proves to be the combination of a L^{8}_{a} (tessaract) and a $L^{16}_{a\sqrt{2}}$ or of three L^{8}_{a}'s; as to the bounding three-flats it may be considered as the combination of a L^{16}_{2a} and a $L^{8}_{a\sqrt{2}}$ or of three L^{16}_{2a}'s.—Prof. Kamerlingh Onnes gave a comparison made by Dr. Zeeman of his measures of polar reflection of light on magnets with the theories of Goldhammer and Drude. His experiments decide in favour of the first theory. Prof. Onnes communicated also an explanation by Dr. Kuenen of the abnormal phenomena observed in the neighbourhood of the critical temperature by the theory of mixtures. The experiments of Dr. Kuenen agree with the results of Gouy.

BOOKS and SERIALS RECEIVED.

BOOKS.—Julius Cæsar, with Introduction and Notes, &c.: W. Dent (Blackie).—Heat and the Principles of Thermodynamics: C. H. Draper (Blackie).—Hydrostatics and Pneumatics: R. H. Pinkerton (Blackie).—The Elements of Hypnotism: R. H. Vincent (K. Paul).—Handbook of British Hepaticæ: Dr. M. C. Cooke (Allen).—Helical Gears, a Foreman Pattern Maker (Whittaker).—Choix et Usage des Objectifs Photographiques: E. Wallon (Paris, Gauthier-Villars).—Geologic Atlas of the United States, Hawley Sheet, Massachusetts (Washington).—Jubilé de M. Pasteur (Paris, Gauthier Villars).—Marveilles de la Nature, La Terre avant l'Apparition de l'Homme: F. Priem (Paris, Baillière).—Specola Vaticana, Classificazione delle Nubi (Roma, Tipografia Vaticana).—Celestial Objects for Common Telescopes: Rev. T. W. Webb, 5th edition, vol. 1 (Longmans).

SERIALS.—Zeitschrift für Physikalische Chemie, xii. Band. 5 Heft (Leipzig, Engelmann).—Bulletin of the U.S. National Museum, No. 45, Monograph of the North American Proctotrypidæ: W. H. Ashmead (Washington).—Berichte der Naturforschende Gesellschaft zu Freiburg i. B., Band vii. Heft 1 and 2 (Williams and Norgate).—Botanical Gazette, November (Bloomington, Ind.)—Journal of the Anthropological Institute, November (K. Paul).—Natural Science, December (Macmillan).—Geological Magazine, December (K. Paul).—American Naturalist, November (Philadelphia).—Geographical Magazine, December (Stanford).—Mitteilungen des Vereins für Erdkunde zu Halle a S. (Halle a S.).—Bulletin of the New York Mathematical Society, November (New York, Macmillan).

CONTENTS.

PAGE

Elementary Practical Science. By Sir Philip Magnus . . . 121
The Pyrenees. By T. G. B. . . . 122
Our Book Shelf:—
Rodet et Busquet: "Les Courants Polyphases" . . . 122
Loney: "Solutions of the Examples in the Elements of Statics and Dynamics" . . . 122
Letters to the Editor:—
Sir Henry Howorth and "Geology in Nubibus."—**R. M. Deeley** . . . 122
The "Zoological Record."—**Dr. P. L. Sclater, F.R.S.; John E. Marr** . . . 123
The Proposed Continuous Polar Exploration.—**Robert Stein** . . . 124
On the Classification of the Tracheate Arthropoda.—A Correction.—**R. I. Pocock** . . . 124
The Loss of H.M.S. "Victoria." II. By Dr. Francis Elgar . . . 124
Reappearance of the Freshwater Medusa (*Limnocodium Sowerbii*).—**Prof. E. Ray Lankester, F.R.S.** . . . 127
Death of Prof. Tyndall . . . 128
Notes . . . 129
Our Astronomical Column:—
The Variation of Latitude . . . 133
Meteor Shower for December . . . 134
Refraction Tables . . . 134
Geographical Notes . . . 134
The Anniversary Meeting of the Royal Society . . . 134
The Temperature of Ignition of Explosive Gaseous Mixtures. By A. E. Tutton . . . 138
University and Educational Intelligence . . . 139
Scientific Serials . . . 139
Societies and Academies . . . 140
Books and Serials Received . . . 144