THURSDAY, SEPTEMBER 7, 1871

THE IRON AND STEEL INSTITUTE

AFTER having on so many different occasions dwelt upon the importance and advantages to be derived from the cultivation of Science by those engaged in the industrial undertakings of this country, we cannot do otherwise than refer in terms of deep interest to a meeting, which took place during the past week at Dudley, under the presidency of Mr. Bessemer, in the heart of the oldest iron-making district of Great Britain.

About three or four years ago a few gentlemen in Cleveland, the youngest seat of the iron trade in the world, propounded the idea that it would be beneficial to all concerned to organise an association of those interested in the manufacture of iron and steel, to meet and discuss all matters connected with these branches of metallurgical science, but from which all questions of a merely trade character should be carefully excluded.

To dispel any idea of this Iron and Steel Institute, as it is designated, being intended by its original promoters to be confined to their own locality, they solicited and obtained the consent of the Duke of Devonshire to act as their first President. Looking at his Grace's position as one largely interested, but in an entirely different district, in the manufacture this body was intended to foster, and having regard to the literary and scientific attainments of this distinguished nobleman, a more judicious selection could not have been made.

From the day of the first introduction of this association to the public to the present, we find an unflagging interest has been maintained in the papers submitted at the gatherings, and in the discussions which have followed their reading. As a natural consequence we are glad to find that among its 450 members is included almost every name of note in this very important branch of our national industry.

We know of no manufacturing operation requiring for successful enterprise a more extensive acquaintance with scientific truth than that of the iron-smelter and its associated branches. His work is conducted in apparatus of a very costly and gigantic character, and under circumstances which render experimental research very difficult; while, on the other hand, its prosecution upon a commensurate footing is attended with so much expense as to render failure almost ruin.

It unfortunately happens also, that the pursuit of pure scientific inquiry connected with the subject is impeded by obstacles of no ordinary character. A blast furnace containing twenty or thirty thousand cubic feet of space and filled with nearly 1,000 tons of materials, chiefly in a state of intense ignition, is not a field to which the chemical philosopher can, without considerable preparation, transfer his labours. From the crucible of the laboratory to such an enormous and almost inaccessible mass, the focus of very intricate and violent chemical action, is too great a step to be made by the chemist for a few hours with any chance of success; for the very questions in which the iron-master would desire his assistance are the results of anterior circumstances, which themselves must be well known to him who attempts to explain their consequences.

On the Continent—in France, in Belgium, in Germany, in Sweden—there are to be found men of great reputation who have identified themselves with this union of science with art, because in these countries are to be found educational establishments so located as to afford the professors who fill the respective chairs abundant opportunity of making themselves personally acquainted with the action of the iron furnace, and, indeed, with every step in this and in other branches of manufacture.

We can adduce no better proof of the real value of the labours of the Iron and Steel Institute than the estimation in which they are held by some of the Continental professors, two of whom we noticed were present at the meeting to which we have alluded.

In our own country, without saying to whom the blame, if any, belongs, men of science and men of industrial occupation have not been brought sufficiently together. As a rule our schools of science are remote from the scenes where science is practically applied. In consequence our professors are, perhaps, less familiar with and less interested in, pursuits, which, in the eyes of a Leoben or Louvain teacher of metallurgy, possess sufficient attraction to induce him to undertake a long journey to be present at a meeting, or to study the operations of our own iron makers, rarely or never visited by the learned of their own nation.

It is this reflection, perhaps, more than any other, which has induced us to notice so favourably the proposition to found in the centre of a great mining and manufacturing district the proposed College of Physical Science at Newcastle-on-Tyne. We regard it as a desideratum no less important to the philosopher than to those who may seek for instruction within its walls, for it is one which will afford to him who has to instruct ample opportunity of studying the application of those great and important truths which it is his office to teach.

We cannot conclude this brief notice without heartily commending the spirit in which the members of the Iron and Steel Institute, throwing aside all narrow-minded prejudice and jealousy, communicate to each other the result of their own individual research, and make known for the use of all the progress each has effected in his own sphere. It seems to us that everyone is acting under the feeling that, on giving information, he is in reality promoting his own advancement. However this may be, society at large, not the least interested in their progress, cannot fail to profit by assistance thus rendered and received, and therefore we most cordially wish all prosperity to the Iron and Steel Institute.

INSTRUCTION TO SCIENCE TEACHERS AT SOUTH KENSINGTON

DURING the months of June and July, a number of science teachers from various parts of England, Scotland, and Ireland, were assembled in London, for the purpose of attending special classes, arranged for their instruction under the auspices of the Science and Art Department. We propose to give some account of the course of instruction in the principles of Biology, which was directed by Prof. Huxley, to whose suggestion, we believe, liberally accepted by Mr. Forster and acted upon by the Government, this important scheme for raising the character of science teaching in the various schools and classes at present in relation with South Kensington is due. It had long been felt by those who annually examined teachers and pupils for certificates in various branches of science, under the Science and Art Department, that the candidates displayed a sad want of practical acquaintance with the subjects in which they presented themselves for examination; many showed considerable ability and great book knowledge, but a knowledge of the things themselves with which science deals, a proof of personal intercourse with Nature, which after all is the only foundation of scientific knowledge, and without which all the 'ologies are so much book-wormery, was to a very great extent wanting. Under the existing state of things it seemed almost impossible to get out of this vicious condition, for the scholars who were in their turn destined to become teachers were for the most part taught by men who were deficient in practical knowledge; and with the increasing demand for science teaching there appeared to be a probability of the evil being increased by the rapid accession of these book-taught students to the position of instructors. The only way to meet this difficulty was to find teachers who had the requisite familiarity with "the solid ground of Nature," and set them to work to leaven the mass. The readiest means of doing this was undoubtedly that adopted by the authorities—namely, to summon to a central class the ablest of the teachers at present distributed throughout the kingdom, and to impart to them as much practical acquaintance and personal familiarity with the *things* of which they had read in books, as was possible in a given time. By annual repetition of this plan there can be little doubt that the body of science teachers throughout the country would be materially affected. Being already acquainted with the outlines and much of the detail of their subjects by hearsay, they would readily understand and appreciate the facts and methods of investigating facts placed before them, and after passing through such a course of instruction would be prepared to proceed further in the same direction by their own individual efforts, and what is more important, to teach, not at second-hand, but from experience, not as fluent repeating machines, but as thoughtful students of phenomena.

Thirty-nine students, of whom one was a lady, attended the course of instruction in the principles of Biology, their expenses (involved in coming to London) being defrayed by the Government. The course occupied six weeks; the students attended every day, with the exception of Sundays, from ten in the morning until half-past four in the afternoon (Saturdays until two). Each morning at ten o'clock a lecture, occupying from an hour to an hour and a half, was given by Prof. Huxley, and the remainder of the day was employed in dissection, microscopic work, and demonstrations, in carrying out which Prof. Huxley was assisted by Prof. Michael Foster, Prof. Rutherford, and Mr. Ray Lankester. The students were placed in pairs at large working tables, and each table was provided with a microscope (with inch and one-eighth inch objectives, and two eye-pieces furnished with micrometric square-ruling), with four scalpels, two pairs of scissors, two pairs of forceps, pins, thread, dissecting needles, watch-glasses, beakers, pie-dishes, glass tubing, and camel's-hair brush.

The practical instruction proceeded *pari passu* with the lectures, the students at once taking their places at the tables after the lecture, and setting to work at materials provided for them to dissect or examine with the microscope in illustration of, or rather as the sequel to, the lecture which they had just heard. Each student was required to send in full reports and drawings as the result of his day's work, many of which proved very excellent; an abstract of the lecture was also given in by each student, with the report of his practical work, and the lot were returned at the end of the course (after due examination by the lecturers) to the students for their future reference. Two prizes—which were two microscopes similar to those used by the members of the class, and provided like them with inch and one-eighth inch objectives—were offered to the students who should be considered to have done best during the course, especial weight being given to excellence in the practical work, as judged both by observation of the student when at work, and by the reports sent in. The names of the students were placed in two classes of merit at the termination of the course, arranged in alphabetical order.

Now as to the subjects which were gone over in the time, which, though limited to six weeks, yet, by dint of hard work, was made to take in more than many a six months' course. The yeast plant occupied the first lecture, and each student was provided with some yeast, which was carefully examined and drawn under the microscope. Each student sowed some in Pasteur's solution which he had himself prepared, and on the following day studied its germination. In like manner the Penicillium mould was studied, sections being cut through the crusts, and careful drawings made of mycelium, hyphæ, conidia, &c. The latter were sown, and their development accurately observed and drawn by each student. A solution of hay was given to each, and the formation of a Bacterium film was studied, the form and movements of Bacteria were compared with the Brownian movements of gamboge rubbed up in water. The structure of the higher Fungi was then studied in specimens of a common toad-stool, and thus a general notion of the morphology and life-history of the Fungi was obtained. Protococcus in its various stages, Palmella, and Volvox next formed the subjects of lectures and practical work, and from these simpler forms the students passed on to Spirogyra and Chara. In Chara the advance in cellular differentiation was noted by each student on specimens supplied to him, and the male and female reproductive bodies examined in detail, and the Antherozooids were obtained in active movement. The phenomenon of cyclosis was also very carefully gone over, each student comparing that of Chara with that seen in Valisneria, and in the hair of the nettle and of Tradescantia; drawings and descriptions being made and the specimens prepared by every student for himself. During this time a certain amount of familiarity had been obtained by all with the use of the microscope—not half a dozen of the class, be it remembered, having previously ever used the instrument at all, still fewer one of adequate power—and as well as the instrument itself, the use of various reagents had been learnt, such as iodine-solution for demonstrating starch, and for delineating protoplasm, acetic acid, magenta-solution, &c. From Chara the class proceeded to the study of the Fern—the sori and sporangia were examined in the first place, and the general form of the fern-frond; then each student was provided with spores which had been previously allowed to germinate, of two stages of development, the one set with the quite young pro-embryo-like prothallium, the other more advanced exhibiting numerous archegonia and pistillidia, the structure of all of which were examined and drawn; and in many cases active antherozooids were obtained. The structure of the fern stem followed, exhibiting typical scalariform, dotted and spiral ducts, and other forms of tissue; also the leaf of sphagnum; the methods of recognising starch and cellulose being here again used. From the fern the class passed on to the study of a bean plant as typical of a phanerogam. Its general morphology, the microscopic structure of its tissues, the minute structure of the flower and the histology of the essential reproductive organs were examined during three consecutive days, and finally the development of the seed and the growth of the young bean plant were studied.

In this work each student used a razor for making sections of the parts to be studied, and portions of turnip were made use of for embedding delicate pieces of tissue, such as leaves, in order to facilitate the cutting of thin sections. A few typical flowers (*e.g.*, Campanula, Rosa, Viola, various Orchids) were next studied as examples of the kind of modification of parts exhibited by phanerogamous plants, and also the female flowers of a small Conifer. Before proceeding to the animal kingdom, a lecture was devoted to a retrospect of the steps through which the class had passed from the simple to the more complex forms, a comparison of the various methods of reproduction, and an outline of the physiology of vegetable life.

Amœbæ, the colourless corpuscles of the Triton's blood, and the amœboid particles of Spongilla, were the first examples of animal life studied, each member of the class making drawings of the various forms due to protoplasmic movement presented by an individual example of each of these cases of simple organism whilst in the field of his microscope. The Gregarinæ of the earthworm next occupied a day, and every student was able to observe and draw the actively moving nucleated Gregarina, its simple encysted condition, and its various stages of breaking up into pseudonaviculæ.

The structure of Infusoria was next examined, as exemplified in Vorticella and Vaginicola, the nucleus, contractile vacuole, mouth, &c., being fairly observed and drawn by all the students. Specimens of Hydra were provided on the following day, and the endoderm and ectoderm, thread-cells and reproductive organs studied. To this followed a copious supply of *Cordylophora lacustris* (from the Victoria Docks), in which the class were able to study a typical compound Cœlenterate, and to recognise not only the male and female gonophors, but the larval "planula-form" as it escaped from the reproductive capsules. Plumatella as a typical Bryozoon succeeded this, and then two days were given to the dissection and histology of Anodon, of which each student was provided with two or three specimens. The lobster as a typical Arthropod was then examined, a fresh specimen being supplied to each table; the heart and vessels were first studied, then the alimentary canal, liver, reproductive organs, and green glands. A large piece of mill-board covered with paper was used by each pair of students for placing out in order, numbering, naming, and comparing the twenty somites and their appendages, an instructive preparation being thus made. The corresponding parts were again examined, and the microscopic structure of the muscular tissue, blood, liver, and gills, in specimens of the river cray-fish. The careful dissection of the frog next occupied some days, and to this succeeded the rabbit.

Simultaneously with the dissection of these vertebrata, the study of the microscopic structure of the various tissues and organs was commenced, so that whilst one student was using the microscope, his companion at the table was dissecting, and *vice versa*. The blood of the frog and of man, the movements of the colourless corpuscles in both cases, and the action of acids on them, the varieties of epithelium, the various forms of connective tissue and its corpuscles, cartilage, bone, muscular tissue smooth and striped, nerve fibres and cells, the termination of nerve in muscle, and the structure of the more important organs, were examined by the class, *not* in already prepared and mounted "slides," but in specimens which each student took for himself, usually from the animal under dissection, and treated with various reagents, the methods of cutting thin sections and embedding tissues in wax or paraffin being learnt at the same time.

A simple injecting apparatus (formed by two Wolff's bottles and a large vessel of water) was put up, and the method of injecting a frog shown to each student. The best part of a day was spent in a thorough dissection of a sheep's heart, and another in the dissection of the sheep's larynx. Vertical antero-posterior sections of the sheep's head were supplied to the various tables, and in these the parts of the brain and cranial nerves (already made out in the rabbit), the tongue, the relations of the cavities of the mouth, nose, and ear, the ducts of the salivary glands, and the muscles of the eye, were studied. The structure of the eye was again examined by each student, in specimens of those of the bullock, suppli l in quantity, and the internal ear and auditory ossicles were demonstrated in rough preparations of the sheep and rabbit.

But little time could be afforded to Physiology; and, indeed, it was hardly possible that each member of the class should perform many physiological experiments for himself. The movements of the heart in the frog after excision, and the localisation of the nerve-centre, was made out by each student for himself; also the phenomena of reflex action in the frog after the destruction of the cranial portion of the cerebro-spinal nervous system. Again, each table was supplied with simple galvanic forceps, and the irritation of nerve and of muscle examined, also the action of chemical and mechanical stimuli on the nerve. The action of curare poison on the frog (Bernard's experiment) was examined by every student, and the condition of the poisoned and the unpoisoned leg compared. Every member of the class was made familiar with the simplest way of demonstrating the circulation in the frog's foot, tongue, and mesentery, under the microscope, and repeatedly examined the phenomenon for himself. Rigor mortis and the artificial rigor produced by warm water were examined. The conversion of starch into sugar by the saliva, and the methods of proving the presence of starch and grape sugar, were made the subject of experiment by every individual of the class. The peristaltic movements of the intestine and the absorption of the chyle by the lacteals were exhibited and closely examined. A model of the circulation, consisting of india-rubber tubes and pump, was used for demonstrating the nature of the pulse, the pressure (by means of manometers placed in connection) in the arteries and veins, and the effect of dilatation and contraction of the capillaries and of rate of pulsation on this pressure. Finally, the thorax was opened in a narcotised rabbit, and the heart exposed, and each student satisfactorily witnessed the pulsations of that organ and the inhibitory effect of irritation of the vagus nerve; the blood-pressure was exhibited to each member of the class in a similarly narcotised dog by means of the hæmodynamometer, a tube being placed in the animal's carotid artery; and as a concluding demonstration the important fact of the influence of nerves upon gland secretion was demonstrated by the beautiful experiment of Bernard, the chorda-tympani being irritated, whilst a canula was placed in the duct of the submaxillary gland. Great care was taken that none of the experiments exhibited to or performed by the members of the class should be open to the charge of cruelty, the animals used being either completely narcotised, or (as in the case of the frogs) having the cerebral portion of the nervous system destroyed in the proper manner.

Throughout the course the morning's lecture was made preparatory to or an extension of what was afterwards brought under actual observation. The concluding lecture was devoted to a retrospect of the work which had been gone through, and an exposition of the idea which had guided the scheme of study pursued, the object having been not to make botanists, nor zoologists, nor anatomists, of the members of the class, but to give them a practical insight into the structure and activities of living things, in such a way as to enable them to observe for themselves the relations and connections of the various forms of life, and to follow from actual examples the characteristics and increasing complexity of different plans of structure.

The reports of work and lectures daily sent in by the members of the class were entirely satisfactory, and the spirit and enthusiasm displayed throughout proved how greatly the value of the course was appreciated. When it is remembered that, with scarcely an exception, these teachers had hitherto never used the microscope, never dissected a single organ or organism for themselves, nor seen one properly dissected, the advantage gained by the experience they have now obtained, even if only a portion of what was condensed into six weeks' work remains with them, is something very considerable, for it is something of a *new kind*, a form of knowledge which they had entirely failed to obtain before.

It is exceedingly interesting to find that no difficulty was experienced in going over all these matters in a class which was not confined to men alone, and most heartily do we hope to see in the future a larger proportion of women engaged in this and other branches of scientific study. Those who imagine that women have some innate incapacity, and assert that if admitted to classes now limited to men they would be unable to profit by them, or would hinder the progress of the class by the greater attention they would require in order to keep them to the level of male students, may take this fact to heart—one of the microscopes offered as a prize for the best work done, and the best record of the lectures and the day's work, was adjudged simply upon the merits of her reports and work to the one lady among the thirty-nine students who formed the class. On the other hand, this fact will probably stimulate that unavowed feeling, akin to the trades-unions' hostility to competition, which is the cause of the arbitrary exclusion of one half of the community from our greatest educational institutions. E. R. L.

MAGNUS'S BONES OF THE HEAD OF BIRDS

Untersuchungen über den Bau des Knöchernen Vogelkopfes. Von Dr. Hugo Magnus, Assistentarzt an der Klinik des Herren Prof. Dr. Förster, zu Breslau. Mit sechs Tafeln. (Leipzig: Engelmann, 1870; pp. 108. London: Williams and Norgate.)

THIS work is a systematic description of the form, structure, and relations of the various bones of the head of birds. Each bone is taken separately, and the chief varieties it presents in the several sub-classes are described, and are illustrated by carefully drawn plates. In here giving a brief notice of the work, we need scarcely say that the details of the several bones in the adult state are very well and clearly given, and the author has had opportunities, of which he appears to have thoroughly availed himself, of studying and comparing the skulls of a large number of birds. The mode in which the variations from typical structure are given is instructive and accurate. We will give an extract to show the mode in which he deals with the subject, and select a part of his account of the squama of the Temporal:—

"The squama of the Temporal Bone (Squama, Sheitelbein, Geoffroy) closely resembles that of Mammals in its form and position. It is an elongated, scale-like bone situated upon the lateral wall of the skull above the tympanic cavity, and is posteriorly in contact with the occipital, above with the parietal or temporal, and anteriorly with the great wing of the sphenoid, with which, as we have already seen, it frequently unites to form the posterior orbital process. The external convex surface of the squama for the most part enters into the formation, sometimes more and sometimes less, of the temporal fossa, especially in the long-billed marsh and aquatic birds. From the processus orbitalis posterior a semicircular line runs upon or around the squama, separating it with its striæ or ribs from the planum temporale. Near the anterior border of the squama, and usually below the posterior orbital process, a process shoots forth from its surface, which is the Processus Zygomaticus of Carus and the temporal process of Köstlin. This in some birds, as in the Larks, Parrots, and Fowls, is tolerably well marked, and fuses with the processus orbitalis posterior. In the singing birds, this process is very variable in size; in Thrushes, Sylviadæ, Motacillidæ, and Hirundinidæ, it is rather feebly developed, resembling a small blunt head. In the Fringillidæ it is developed into a slender rod, as it is also in Edolius, and (though to a less degree) in Lanius. In the Paridæ it forms a broad lamina. In the Corvini, it is considerably developed; while in the Falcons it appears to be entirely absent. In Owls it is slender and acicular. In the Woodpeckers it is very large, and occupies a special groove of the Quadrate bone. In the marsh and aquatic birds it approximates very closely to the articular surface of the os quadratum, and is for the most part of very small size." He then proceeds to describe the internal surface of the squama, and its junction with other bones to form the tympanic cavity.

The principal defects of the book are obviously that the author has little acquaintance with the history of the embryological development of the class of birds generally, and does not appear to have studied the serial homologies of the several bones in other classes.

As an instance of the former defect, we may note that Herr Magnus maintains with Nitzsch that the os dentale, or median portion of the lower jaw, is developed from a single point of ossification. "I have never," he says, "been able to discover the presence of two nuclei, even in the youngest animals I have examined, nor any trace of a suture." We would refer to Mr. Parker's paper on the head of the fowl, as clearly showing the double nature of this bone, though no doubt the two parts early coalesce. Taking the ethmoid bone again, we find Dr. Magnus describing it rightly as a cranial bone, or rather as one belonging to his animal sphere. He notices the singular modification in form it undergoes throughout the whole class, and observes that it is a thin bony plate lying between and in front of the globes of the eye. The anterior portion, he goes on to say, situated in front of the eyes, and provided with lateral processes that shut off the nasal from the orbital cavities, may be regarded as the labyrinth of the ethmoid. And then comes the remarkable statement that "the plate extending backwards from this and separating the two orbital plates from each other is the *crista galli*, whilst the short plate extending forwards into the cavum narium represents the lamina perpendicularis." This scarcely appears to us to be an accurate representation. The crista galli is to all intents and purposes an intracranial projection of bone, we might even consider it to be a portion of the falx cerebri which has undergone ossification, and such result must be above the plane of the canals for the nasal branch of the fifth; in point of fact, it may be seen in all birds in a rudimentary form between the two passages for the olfactory nerves. (See note to Mr. Parker's paper "On the Structure of the Skull of the Common Fowl," Phil. Trans. 1869, p. 762.) The septum between the two orbits is chiefly and not to a small extent only, formed by the os perpendiculare or lamina papyracea. Putting aside the references elsewhere made to this point in the course of the descriptions of the bone, we must praise the very full account that is given of its relations in the different classes of birds, proving not only that the means of investigation at Dr. Magnus's disposal are extensive, but that he has made excellent use of them.

As an example of the second point, on which we have ventured to criticise Dr. Magnus's work, we may refer to the entire section on the bone to which he has applied the term Paukenbein, or Tympanic, bone, which, in part at least, corresponds to Mr. Parker's Basi-temporal, and the relations of which the latter writer has worked out so well. Its nature is essentially misunderstood by Dr. Magnus, who appears to have drawn his conclusions from heads examined at too late a period of development, whilst he scarcely makes any reference to its homologies, so important in determining a difficult and disputed relation of this kind.

H. P.

OUR BOOK SHELF

The Elements of Plane and Solid Geometry. By H. W. Watson, M.A. (Longmans, Green, and Co.)

THIS is one more Text-book of Geometry. It adopts completely the general principles of the geometrical reformers in England, in the classification of theorems according to their subjects, the free use of super-position, the separation of problems from theorems, the art from the science, and the avowedly arithmetical treatment of proportion. It is distinguished from most that have preceded it by its greater length, especially in its treatment of ratios, by its somewhat wider range of illustration, and its comprehending the elements of solid geometry. But the book is disappointing. A well-trained and well-read mathematician, with plenty of experience in teaching, and we imagine plenty of leisure for writing, ought to turn out a better book. In a text-book which does not profess to be original in its matter, the arrangement and manner are of the first importance; and in both these respects the book in our judgment fails, and fails openly. The large number of miscellaneous propositions with which several of the books open give a real confusion to the whole volume. And it would be easy, if space permitted, to show that the arrangement is unnatural in some important points. Moreover, some of the demonstrations are very inelegant, such as Book I., pp. 11, 17, and Book II., pp. 12, 13; indeed the latter pair are more than inelegant.

On the whole, therefore, we believe that the book before us, though not without merit, is not a very valuable addition to geometrical reform. It seems to show very clearly what the reformers must aim at, and take infinite pains to achieve: the establishment and recognition of a standard syllabus of geometry. When this is agreed upon, we shall see better text-books than have yet been written.

Victoria. (1) *Mineral Statistics of Victoria for the year* 1870. Presented to both Houses of Parliament by his Excellency's command. (Melbourne: By authority: John Ferres, Government Printer.)—(2) *Reports of the Mining Surveyors and Registrars.* Quarter ending March 31, 1871. (Melbourne: By authority: John Ferres, Government Printer.)

THESE reports are models of what such statistical reports should be; the tables are methodically arranged, easy of reference, and apparently exhaustive; the printing would be creditable even to a London printer. In the former, besides the interesting summary and the appendices, there are fifty-three admirably constructed tables, setting forth the statistics, from every possible point of view, of the mining operations in all the districts, divisions, and subdivisions of Victoria for the year 1870. Of course the statistics relate mainly to gold, the metal most sought after; but all obtainable information is likewise given with reference to whatever other mineral produces are found in the province—silver, tin, copper, antimony, lead, cobalt, manganese, coal, &c. Every means has been taken to make the statistics reliable, and the result, with regard to gold, is that there has been a falling off of the produce in 1870, as compared with 1869, to the extent of upwards of 40,000 oz., which decrease is largely accounted for by the heavy and unprecedented floods of 1870 interrupting the mining operations, the decrease in the number of mines, and the falling off in the yield of gold from several of the deeper alluvial mines. It is stated that during 1870 several scientific gentlemen volunteered to deliver to the miners gratuitously lectures on subjects connected with mining, but received no encouragement from the district authorities, who seem not to have thought it worth their while to provide a room. The interests of science are, however, by no means neglected. We learn from these reports that during last year more than 800 groups of minerals, rocks, and fossils, were added to the collection of the mining department. Efforts have also been made to obtain specimens of the mineral products of other countries in exchange for native products. Another colony is now likely to reap a rich reward, as already many specimens have been sent both from Europe and America. We are glad to learn that Dr. Von Mueller is preparing a report on the large collection of native fossils which has been made. The second report, for the quarter ending March 31, 1871, is considerably more interesting than the former, in a scientific point of view. Besides full and valuable mining statistics, there are two appendices: (A) "Notes on the Rocks and Minerals of the Owen's District," with a sketch map, by Mr. E. J. Dunn, containing much valuable information on the geology of the district; (B) an interesting paper containing succinct observations on what the author, Ferd. Von Mueller, Director of the Melbourne Botanic Garden, considers a new genus of Fossil Coniferæ, to which he has given the name *Spondylostrobus*. It is allied to *Cupressinites* of Bowerbank. We are sorry we have not space to copy the author's description. The validity of the genus, Mr. Mueller declares, rests chiefly on the extraordinary development of the columella, if so it may be called; this columellar portion forming indeed the main body of the fruit, the so-called new genus differing in this respect from all other cupressineous genera living as well as extinct. The paper is illustrated by a beautifully executed lithograph, containing several coloured figures, natural size, of the fossil, and also by a plan of the field, and sections of the strata in which it is found. We have much pleasure in commending these interesting, and on the whole, encouraging reports, to the notice both of statisticians and geologists.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Pendulum Autographs

HAVING read with much interest Mr. Hubert Airy's communication to NATURE (No. 94), on "Pendulum Autographs," I wish to say a word on the compound pendulum long ago devised, I believe for the first time, by Prof. Blackburn, of Glasgow.

I construct the pendulum as follows:—A piece of soft iron wire, about $\frac{1}{40}$th of an inch diameter, is fastened by its ends to two points in the ceiling, and a heavy bob is hung from its middle point. A second wire of the same length is similarly attached to the ceiling and to the bob, so that the wires form two superposed isosceles triangles with the line between the suspension points for their common base. A light deal rod about the same length, more or less, as the distance of the suspension points, has a fine saw cut about $\frac{1}{4}$th of an inch deep, made lengthwise at both ends. The wires which form the sides of the first triangle are put into one cut, and those of the second triangle into the other; and the rod may then be slid up or down along the wires, to different heights, so that when the pendulum is at rest the rod is horizontal, forming the base of one isosceles triangle, with sides of double wire, and its vertex down at the bob, and also of other two isosceles triangles, equal and similar to one another whose sides are of single wire, and whose vertices are the points of suspension in the ceiling respectively.

It is now evident that the rod is rigorously constrained to oscillate in a plane perpendicular to the line joining the suspension points, while the vertex of the triangle below the rod, which is the point of suspension of the bob, is free to move, at any instant, only in a plane at right angles to the plane of motion of the rod. As the amplitudes of the oscillations are practically made small compared with the lengths of the component pendulums, we thus obtain, with almost any desired degree of exactness, the composition of two simple harmonic motions of different periods of adjustable ratio, and in rectangular directions. It is easy also to see how, by making the wires of unequal length, and dividing them proportionally at the point of suspension of the bob, the simple component motions may be adjusted to different inclinations. In order absolutely to prevent the bob from creating indeterminate motions about its point of suspension, it would be needful to substitute, for the wires below the suspended rod, stiff pieces rigidly attached to the bob. But with due care in swinging the pendulum no very sensible motion of the bob, relatively to its suspending wires, need occur.

To record the motions of the pendulum, I have most frequently adopted the old plan of sand running out at a fine hole at the bottom of the pendulum bob. But for class experiments at the University of St. Andrews, I have also made the following arrangement:—A heavy bob of lead, in metallic connection with its suspending wires, has a metal point projecting from its lower end. Wires from an induction coil are connected, one with either of the suspension points in the ceiling, the other with a sheet of a tin foil which rests on a table, and over which is placed a sheet of paper, all but touching the point projecting from the bob. The pendulum having been first got to swing steadily, the induction coil is put in action, and the sparks, passing from the pendulum point to the tin foil, trace on the paper, if it be suitably prepared, a record of the pendulum motion. I used one of Ruhmkorff's original coils, which, with a single Grove's cell, was quite sufficient. The rheotome acted automatically, and with considerable regularity. The dots on the paper made by the sparks showed distances varying from one element of the pendulum track to another, and thus exhibited in a very interesting manner the variation in the velocity of the pendulum bob.

WILLIAM SWAN

Ardchapel, Dumbartonshire, Aug. 24

PERMIT me to state that the diagrams in No. 94 of the 17th June to Mr. Hubert Airy's "Pendulum Autographs," are identical with the "Kinematic Curves" by Mr. Perigal, drawn by him upwards of thirty years ago, and discovered by Mr. Sang of Edinburgh two years previously (On the Vibration of an Elastic Spring, Ed. Ph. Tr.), autographic copies being in the possession of the Royal Society, Royal Institution, and Royal Astronomical Society. *Vide* my application of the Binomial Theorem to Perigal's Bicircloids (Lond. Phil. Mag. 1849-1850). Mr. Perigal calls these curves, Lemnoids, Paraboloids, &c.

August 28 S. M. DRACH

Thickness of the Earth's Crust

THE question in debate is not a mathematical one. Accepting Archdeacon Pratt's calculations as correct, they would show that certain facts in the earth's motion are what they would be if the earth were a rigid mass, or nearly so. But this at present is not disputed. What is disputed is the soundness of the inference drawn from these facts respecting the fluid or solid state of the earth's interior, for it is contended that in either case the movements in question might be practically the same, provided only they were slow enough. I do not think this is replied to by Archdeacon Pratt in his letter in NATURE, August 31.

Whatever the disturbing forces may be, they amount to a motive impulse given to some portion of the mass of the earth. This impulse may have two effects: either it may alter the shape of the mass by causing part of it to move in some direction faster than the rest can follow, or it may alter the position of the mass by causing the whole to move together. If the portion which receives the impulse is able to move the rest as quickly as it moves itself, the whole will move together; and where there is any cohesion at all, there must be a degree of slowness at which this condition is attained.

Mr. Pratt's rope of sand, if dealt with here, is a system of particles between which there is no cohesion. They are not able, by attractive power, to move each other at all. But if hung out in free space, they would certainly assume a definite shape as a whole, and would retain it with complete "rigidity" in spite of any applied force which was not able to move any of them faster than they could move each other.

Suppose the earth were projected bodily along the line of its axis towards the pole star, what would happen to a loose stone lying on the surface at the south pole? If the earth moved northward ten feet in a second, the stone would, at the end of the first second, be still upon the surface. If the earth moved twenty feet in a second, the stone, at the end of the first second, would be a yard behind it, but before the end of the next second it would be on the surface again. Are not the relations between the rigid, the fluid, and the elastic states all illustrated here? What would be the real cohesive force in a molten earth, as compared with a congealed one, is another matter. "Molten" does not necessarily mean "limp," and the question, if determinable, has not, I imagine, been determined. The molten earth would no doubt be less compressible; and this, in some cases, may be equivalent to an increased cohesion. Let me add that I have no theory as to the earth's interior. A. J. M.

Sept. 5

Spectrum of the Aurora

MAY I call your attention to an error which has occurred in the engraving of the Spectrum of the Aurora which I sent you last week. The lines are marked in strength exactly the reverse of what they should be. Thus: No. 1 is the strongest, and is a sharp line easily seen, and in the drawing it is the weakest; and so with the others. No. 1 is the brightest, No. 5 is the faintest.

47, Brook Street LINDSAY

Transparent Compass

I BEG leave to draw your attention to a contrivance that I think very suggestive, of improvements in getting up compasses for iron and wooden vessels. This I propose to effect by using glass globes with transparent needle-cards, and thus making a transparent mariner's compass, visible in all directions, that may be either supported or suspended by very simple and compact fittings wherever most convenient.

In iron vessels this transparent compass can be readily placed beyond the local attraction of the iron. In appearance like a pearl, and in good taste.

Please draw attention to this very simple remedy for so many real or alleged complaints of the deviation of the compass on board of iron vessels. GEORGE FAWCUS

North Shields, Sept. 4

A Substitute for Euclid

SINCE Prof. Tait has given the weight of his authority to the attack for some time past directed against Euclid, I, and perhaps some others who like me have sons whom they wish to educate as mathematicians, would be much obliged to Mr. Wilson, or any other of your correspondents, who would recommend a book which is suited to lay the foundation of geometry in the future. A FATHER

Monolithic Towers of Cement Rubble for Beacons and Lighthouses

IT occurs to me to suggest the trial of common rubble set in Portland or other equally good cement in the construction of beacons and seamarks, as also for lighthouses. The advantages of employing cement rubble, not in prepared blocks but by continuous building, are the following:—

1. The dispensing with all squaring or dressing of materials.
2. The suitableness for such work of any stone of hard quality, thus rendering it unnecessary to bring large materials from a distance, or to open quarries for ashlar.

3. No powerful machinery is needed as for moving or raising heavy materials.

4. A saving in the levelling of the rock for a foundation for the tower.

5. The ease of landing on exposed rocks small fragments of stone, as compared with the landing of heavy and finely-dressed materials.

The erection of an experimental beacon on the plan I have suggested would be attended by comparatively small expense.

Edinburgh, Aug. 29 THOMAS STEVENSON

Neologisms

IT may be something of a bull, but I wish to consult your correspondents about a neologism which does not exist. It must exist very soon, however, because it is urgently wanted, as will be seen from the following considerations :—

A straight line has a certain *direction* ; we all know what is meant ; it is an inherent property of a straight line. The answer to the question what property two parallel straight lines have (independently of their being produced) is that they have *the same direction*. I do not invite discussion on the propriety of this definition, but only call attention to the fact that the words exist by which this conception of parallels can be expressed.

Now to speak of planes. A plane has a certain —— ; two parallel planes have the same ——. The same *what?*

Again, here are two theorems, which are, in fact, reciprocal :— (1) planes parallel to given straight lines are themselves parallel ; and (2) if two intersecting planes are respectively parallel to two other intersecting planes, the lines of intersection are parallel ; which may be better enunciated as follows :—(1) two directions determine one —— ; and (2) two —— determine one direction.

The German geometers have no difficulty about it. They say, "Durch 2 Richtungen ist eine Stellung, durch 2 Stellungen eine Richtung bestimmt." J. M. WILSON

Rugby, Sept. 1

The Nucleus of Adipose Tissue

WILL you allow me to make one or two observations upon the remarks made by the writer of the article on the last part of the "Physiological Anatomy and Physiology of Man?" In few words the reviewer has drawn attention to several of the most important points advanced in this part, and for this I feel much indebted to him. But with reference to adipose tissue, he observes that I convey the impression that the adult fat cell "consists merely of an envelope containing oily matter—no mention being made of the fact that by proper treatment a nucleus also can be almost always demonstrated." This is strange, for I believe I was the first to demonstrate the "nucleus" in the fat cells of adult adipose tissue. In my lectures at the Royal College of Physicians in 1861, I showed specimens illustrating the fact, and in the work reviewed I have endeavoured to show that the so-called "nucleus" (germinal matter or bioplasm) is concerned in the formation of the fatty matter, and in its removal from the fully formed fat vesicle (p. 305) whenever we get thin from the absorption of fat. In fig. 198, plate xx., fat vesicles in various stages of development are represented, the "nucleus" being given in every one, while in fig. 132, plate xv., are shown some fat vesicles in cartilage, the nucleus being seen in every instance. If I have not made this point sufficiently clear in my description, it arises from the circumstance that I desired to leave as much as possible of the general description given by my predecessors in the first edition. In the early part of the chapter the nucleus has not been mentioned, which is to be regretted, but in the latter part, containing the new matter, very frequent allusion to it has been made.

It is this *constant presence* of the "nucleus" (bioplasm) in all tissues, at every period of life, at least as long as their activity lasts, to which I have long attached such great importance. I have endeavoured to show that this nucleus matter (bioplasm or germinal matter) is alone instrumental in the formation and removal of all textures, and at every period of life. This alone, of all the matter of the body, is in a living state, and capable of formation. Contrary to the generally received opinion, I have proved its presence even in all forms of yellow elastic tissue, and have shown that this texture is formed upon the same principles as other tissues.

61, Grosvenor Street LIONEL S. BEALE

Eclipse Photography

FROM letters I have received, it appears that the table of exposures given in my "Notes" is not correctly understood. It is necessary to explain again that the reason why the plate exposed 8 secs. gave a better result than the one exposed 30 secs. was because the eclipsed sun was nearly covered by cloud during the long exposure, and was quite clear during the short. The 30 secs. plate would have been greatly over-exposed for certain details, but the outer corona would probably have been more clearly defined. By giving some plates short and others long exposures, it was intended to show different effects, as would certainly have been the case if we had been favoured with a cloudless sky.

I am informed that it is proposed to attempt to obtain uniformity of results by using the same kinds of instruments and chemicals at all the stations. So far, good. But where is the certainty that the hands that will use the chemicals and instruments will produce equality of results? It is about the same as giving to a dozen men pens, ink, and paper, and expecting from them twelve specimens of caligraphy all alike. It would be preferable to decide beforehand whether negatives, or positives, or both are to be taken, and then to allow the operators to choose their own methods. A. BROTHERS

The Museums of the Country

IT must be obvious to any scientific person visiting the provincial museums of this country, how inefficient they are for the purpose of preserving Geological and Natural History collections, which are being formed more or less throughout the land.

Whilst the British Association directs so much attention towards the advancement of science by means of investigation, and grants money for the purpose, it is short-sighted on its part to neglect the subject of Local Museums as means for preserving collections for the benefit of science and of posterity.

To give an illustration of the way in which such museums are too often conducted. In a west-country museum there has lately been an addition, consisting of a valuable collection of cave bones, and that is well-preserved and arranged, but why? In a great measure because one of the members of the local society happens to take an interest in that department. But in what condition is the local geological collection? In a state of neglect and disorder, because in that department no one takes an interest. In other museums, where there is nobody to take an interest in the subject, the state of the collections may be imagined.

It is much to be regretted that museums should remain in such a condition. The formation and preservation of local collections ought not to depend upon impulse, or the chance enthusiasm of individuals, but should be the result of a generally recognised business-like system ; and it should be the interest of the various local societies to provide competent curators. It should also be the duty of these societies to preserve for the museum of the district the collections which have been formed, by local geologists or collectors, and not to permit them to be scattered or added to those in the British Museum and to that in Jermyn Street, where they may be said to become buried, and where the geological collections are already of an unmanageable size.

F. G. S.

Thunderstorms

THE prevalence of thunderstorms accompanied by serious accidents during the last two months has led me and many others to consider whether the phenomena of electrical discharges are thoroughly understood. We have heard of several instances in which the electric fluid "came down the chimney, filled a room with sulphurous vapour," and terrified or injured persons sitting near the fire-places. One fatal accident took place within a few hundred yards of my own house. A gentleman's coachman driving along the turnpike road was instantaneously killed on his box, "the lightning," it is said, "having struck him on the head and passed through his body to the iron work of the carriage, and thence to the ground." From the appearance of the body there is no doubt that the fluid did pass through the poor fellow and caused his death, but my opinion is that he was killed by an ascending current which was attracted to the wheels of the carriage, passed upwards through his body, issued at his head, and shivering his hat (made of felt, and therefore a bad conductor) to fragments, passed to the cloud above. During the same storm I was watching the lightning playing on the hill which is separated by a valley from my house. Every flash I observed was double, composed I imagine of an ascending and descending current. In every instance one of the two flashes was brighter than the other; but I could detect no difference of time; as far as the eye could judge they were simultaneous. The inference I am disposed to draw from these facts is this, that during thunderstorms ascending currents are to be guarded against no less than descending ones; that when chimney-pieces are shivered and people sitting by the fire side are killed, the electric fluid has not come down the chimney at all, but has proceeded from the earth, and, having found a good conductor in the fender and grate, has passed through them harmlessly, and has then overflowed, so to say, into the room, or shattered the non-conducting masonry. Continuous lightning-conductors, on Sir Snow Harris's principle, afford sufficient protection to public buildings, but metal pinnacles terminating below in masonry or woodwork are likely to cause mischief, and iron pillars, unless insulated below by some non-conducting substance, must be equally objectionable. C. A. JOHNS

SIR WILLIAM THOMSON ON THE LAW OF BIOGENESIS AND THE LAW OF GRAVITATION

A PASSAGE in the address of the President of the British Association appears to me so remarkable, and so much at variance with the notions entertained by biologists of various shades of opinion, that I am surprised that no observations were made upon it during the sectional meetings, and beg now to draw attention to it. I may mention that in the discussion on spontaneous generation in Section D on the last day of the meeting, I did say substantially what I now write to you, but no one present defended Sir William Thomson's position. The passage in question is as follows: " But science brings a vast mass of inductive evidence against this hypothesis of spontaneous generation, as you have heard from my predecessor in the Presidential chair. Careful enough scrutiny has, in every case up to the present day, discovered life as antecedent to life. Dead matter cannot become living without coming under the influence of matter previously alive. This seems to me *as sure a teaching of science as the law of gravitation*. . . . I confess to being deeply impressed by the evidence put before us by Prof. Huxley, and I am ready to adopt as an article of scientific faith, true through all space and through all time, that life proceeds from life, and from nothing but life." * In the first place it is to be remarked upon this passage, that the reference to his "predecessor in the Presidential chair," and to ' the evidence put before us by Prof. Huxley," is made in such a way as would lead an uninformed person to suppose that not only was the speaker simply availing himself of that evidence, but also merely following or re-enunciating a belief previously expressed by Prof. Huxley. This I do not for a moment suppose was in any way the meaning of Sir W. Thomson, who unintentionally has made it appear that Prof. Huxley comes to the same conclusion from the consideration of certain facts, as he does. So far from this re-assuring concord having an existence, I doubt if any single biologist of name (of whatever philosophic tendencies) would venture to assert that it is as sure a teaching of science as the law of gravitation that dead matter cannot become living without coming under the influence of matter previously alive, and conclusions derived from a consideration of a vast series of facts prohibit an evolutionist from accepting such a doctrine without the most complete and widely-reaching evidence in its favour. Sir William Thomson's authority must be accepted as unquestionable as to the amount of sureness which may be attributed to the law of gravitation; but with great deference to him, I should like to ask if he would definitely maintain that it is no greater than that which may be attributed to the dogma "no life except from life." It is the fact that within human observation the law of gravitation is a true statement; it is also the fact that within human observation the dogma "no life except from life" is a true statement; but how can it be for a moment supposed that this places the two statements in the same position of sureness? Does not all depend on that term "within human observation?" Will not the sureness depend on the extent and thoroughness of the observation? And is it not the case that whilst human observation of bodies in relation to the law of gravitation is of the most vast character—embracing not only all varieties of terrestrial matter, but innumerable extra-terrestrial bodies—the human observation of the way in which living matter originates or grows, is a mere trifle so insignificant in extent that it is as a drop in the ocean? Sir William Thomson speaks of being "deeply impressed by the evidence put before us by Prof. Huxley," and is thereupon ready to adopt an article of scientific faith "true through all space and time." What was the evidence in question? The merest fragment, as Prof. Huxley would himself acknowledge (though associated with much more evidence upon allied matters)—simply this by no means astonishing though much controverted fact, that when out of the unspeakably many kinds of mineral matter which you might take, you take one or two and boil them and seal them up and submit them to a variety of processes, the object of which is not to produce favourable conditions for the evolution of life, but to prevent the access of already living matter—you don't get life produced. The whole of this kind of experiment, and of the evidence which so much impressed Sir William Thomson, cannot—attended as it is with negative results—have anything to do with the general question of the *de novo* origin of living matter. Such evidence merely relates to a particular supposed case of such origin, one out of thousands conceivable. Yet this is what it seems to me—I write with diffidence—Sir William Thomson has taken as evidence of the same value as that on which rests the law of gravitation. Because it seems rather more probable than not that organisms do not arise *de novo* in boiled and sealed solutions of tartrate of ammonia, in hay-decoctions and turnip-juice, *therefore* it is true through all space and all time that dead matter never becomes living without the action of living matter; therefore nowhere to-day on the whole earth—in the sea charged with gases, open to sunlight and atmosphere, holding salts and complex semi-organic compounds, suspended and in solution—is this process going on; in no pond; under no moss; and not only to-day, but we are to conclude that never at any time did Nature in her great laboratory produce life from mineral matter, because in certain arbitrary, crude, and utterly artificial conditions of isolation she refuses to do so. Is it true that the law of gravitation is no surer a teaching of science than the dogma about the origin of life which rests on such logic?

That I have not misrepresented the utter poverty of observation upon the origin of life will, I believe, be admitted by all naturalists—possibly individuals unacquainted with biological phenomena may have conceived it to have been relatively more extensive. We have been able to trace the commencement of so many of the various living forms to eggs, that it becomes waste of time to examine into cases of alleged spontaneous origin of *complex* forms from mineral matter; and biologists have now to look for the formation of simple organic material. Observations therefore which merely tend to disprove the spontaneous productions of maggots, worms, ciliated infusoria, and fungi, are not to be reckoned as "evidence on the origin of life," they do not bear on the question as it now presents itself, the working hypothesis of science being, not that *animals* or *plants* originate *de novo*, but that *organic matter* has at one time done so, and is doing so still. It is, I believe, just to assert that observation bearing on this hypothesis is almost entirely wanting, and

* NATURE, Vol. iv., p. 269

indeed, the few experiments of the French observers, and of Gilbert Child and Bastian in this country are the only ones at present made.

The reason is obvious, the conditions of the experiment and observation required are so difficult that we have not yet mastered them. They are first, to ensure all the favouring circumstances in the laboratory experiment which natural stations afford, and, of course, to ensure them it is necessary to know or have some idea (which biologists have not) as to what they may be; second, to exclude simultaneously all living matter; third, to make the observations *throughout* with the greatest minuteness which the microscope permits—a necessary condition, on account of the possible smallness of particles of living matter. When we have had experiments performed in this way with a vast variety in the first-named set of conditions, so as to obtain and study the action of various natural circumstances which might possibly be present in the *de novo* origination of living from mineral matter—then we may speak of evidence on the question. As it is, we have but a very incomplete and discordant series of observations on one class of conditions in which there is a presumption of spontaneous generation (the case of Bacterium), and which have been selected for experiments on account of a supposed facility for isolation, without interference with the conditions, but of which very little is understood at all. I venture to submit that this single case, in which there has been some little investigation with, be it granted, negative result, so far from warranting the enunciation of a dogma, which is declared to be as sure as a great law expressing the concurrence of almost infinitely numerous, varied, and reiterated observations, does not even justify an opinion; it has no possible bearing upon the source of the minute protoplasmic particles which the microscopist finds abundantly in sea-water, nor upon the origin of the atmospheric germs which are so largely invoked by some persons. It leaves us necessarily to *a priori* considerations in regard to the origin of life on the earth, and until direct researches are made, the hypothesis developed by *a priori* argument must have far more claim on the adhesion of an unbiassed mind, than a pseudo-law, though the latter bear an authority so great in some departments of science as is that of Sir William Thomson.

E. RAY LANKESTER

RECENT FRENCH ZOOLOGICAL DISCOVERIES

TWO naturalists, who have been more than usually successful in their investigations of the faunas of distant and little-known countries, have recently returned to France, and are now engaged in working out the results of their arduous expeditions. These are M. le Père Armand David, and M. Alfred Grandidier.

M. le Père Armand David is a missionary priest of the Order of Lazarists, who was for many years resident at Pekin. Here he devoted much time and attention to the fauna of the surrounding country, which was at that period little known, and entering into communication with the authorities of the Jardin des Plantes of Paris, supplied that establishment with many interesting novelties. Amongst these one of the most remarkable was a new deer with very peculiar horns and a long tail, which was named by M. Alphonse Milne-Edwards *Elaphurus davidianus*, after its indefatigable discoverer. But about two years ago Father David moved the seat of his investigations into still more promising quarters. It was, we believe, the magnificent new species of Pheasants transmitted by Bishop Chauveau from Ta-tsien-leou—a town in Western Szechuen upon the frontiers of Tibet—that first called his attention to the probable richness of this district in other departments of zoology. Nor have his expectations been in any way disappointed. The collections of Mammals, Birds, and Reptiles, obtained by Father David during the recent exploration of *Mopin*, as this portion of the Celestial Empire is termed by the French writers, have of late years seldom been equalled in any part of the world for extent or variety. The fauna of these mountains seems to be a sort of *pendant* to that of the Himalayas, which, some years ago, was so successfully investigated by Mr. Hodgson. The singular *Ælurus* or Wah, of Nepal, is replaced by a larger and even stranger form, the *Æluropus* of M. Milne-Edwards, a large bear-like mammal, quite distinct from anything previously known. A long-haired monkey inhabits the pine forests, remarkable for the development of its nose, which the same naturalist has proposed to name *Rhinopithecus*. The *Takin* of the Mishnees of Upper Assam (*Budorcas taxicolor*) is represented by a second species of this most singular genus of Ruminants. A new form of Cervidæ is remarkable for its small horns and well-developed canines; and there are a host of interesting novelties belonging to the insectivorous and rodent orders in Père David's series. In birds, M. Jules Verreaux, to whom the working out of this part of M. David's collections has been assigned, has already discriminated upwards of thirty new species. Amongst these many belong to the remarkable genera discovered by Mr. Hodgson in the hill-forests of Nepaul, and hitherto unknown to occur elsewhere. Perhaps the most noteworthy of them is a small Passerine form allied to *Paradoxornis*, which has only three toes, a phenomenon hitherto unknown in that typical order of birds. The Reptiles and Batrachians obtained by Father David in Moupin are also said to contain many novelties. Since the lamented death of Prof. Duméril, their investigation has, we believe, been undertaken by Prof. Blanchard, who has within these few last days brought before the French Academy a notice of one of the most extraordinary animals of the latter group. This is no other than a gigantic aquatic Salamander allied to, but distinct from, the now well-known *Sieboldia maxima*, of Japan. The discovery of this form of life in continental Asia is a fact of the highest significance as regards geographical distribution, as it was previously believed to be in the present epoch confined to the Japanese Islands, though remains of a closely allied animal (*Andrias scheuchzeri*) are found in the tertiary freshwater deposits of Central Europe.

We have mentioned only a few of the principal discoveries of M. David, but enough has been said to show the importance of the additions he has made to zoological science, and to heighten the interest attaching to the complete investigation of the fauna of the Chinese frontier of Tibet, which this distinguished naturalist has thus inaugurated.

While Father David has been labouring among the snows of Central Asia, another not less arduous devotee of science has been risking his life in the tropical forests of Madagascar, and has likewise made many brilliant discoveries. M. Alfred Grandidier, who has now returned from, we believe, his *third* voyage of discovery in that strange island, has shown that the riches and eccentricities of its fauna have not yet been exhausted. His collections, which have only reached the Jardin des Plantes very recently, although brought to France before the political storm of last autumn commenced, have not yet been fully examined. But they are said to contain very full series of several species of Lemuridæ, the comparison of which is likely to lead to important results, besides examples of a new genus of Rodentia, and many other Mammals of high interest. M. Grandidier has also paid much attention to the fossil deposits of Southern Madagascar, which contain the remains of the extinct gigantic bird, *Æpyornis maxima*, and has arrived at some important results (such as the former presence of *Hippopotamus* in Madagascar) which may ultimately tend to modify some of the views generally held concerning the true nature of the fauna of this island and its origin.

P. L. S.

PENDULUM AUTOGRAPHS

II.

IT naturally occurred to try what would be the effect of introducing a third joint in the suspension of the pendulum, and altering the angles between the different joints. First I tried three joints at angles of 60°, by means of two intermediate pieces attached to the rod, to the cross-bar, and to one another, by string-hinges. The result did not differ from that obtained with two joints. This surprised me, for I had expected that each joint in taking its share of the swing would somehow assert its claim to its own proper period distinct from the periods of the other two. But on close examination, attaching long slender splints of wood to the cross-bar, the two intermediaries, and the rod, and bringing their depending ends near together, by

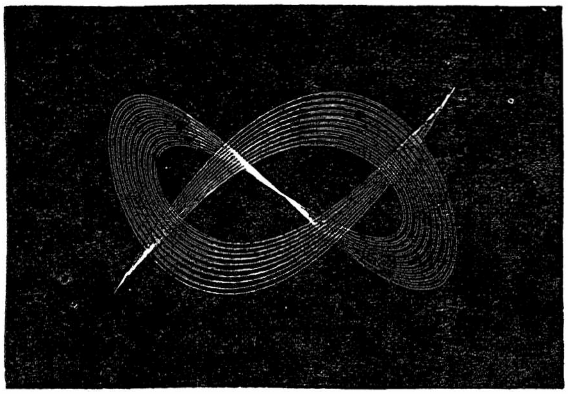

FIG. 7.—Proportion 3 : 5.—Cusped type.

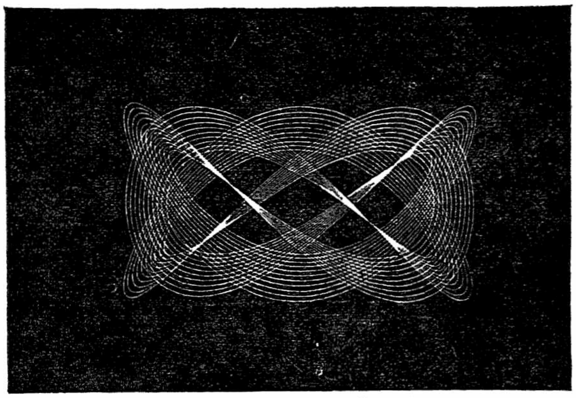

FIG 8 —Proportion 3 : 5 —Looped type.

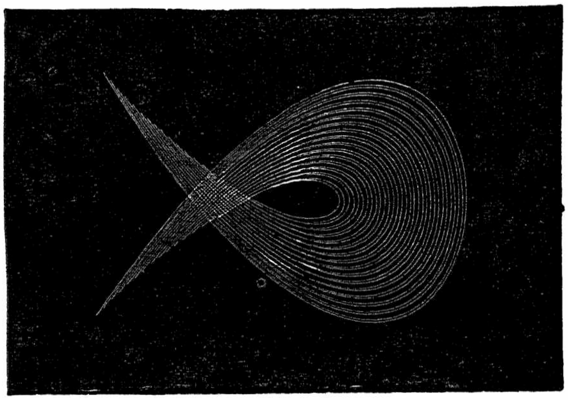

FIG. 9.—Proportion 2 : 3.—Cusped type.

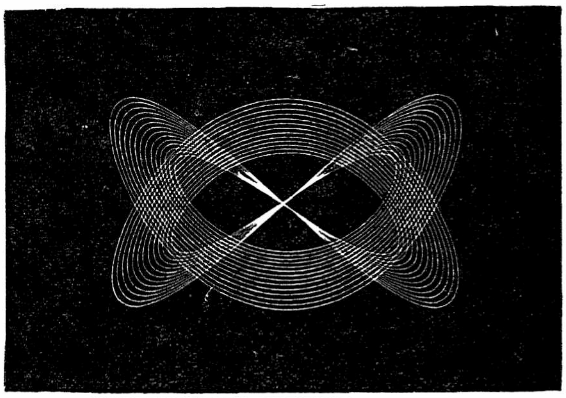

FIG. 10.—Proportion 2 : 3.—Looped type.

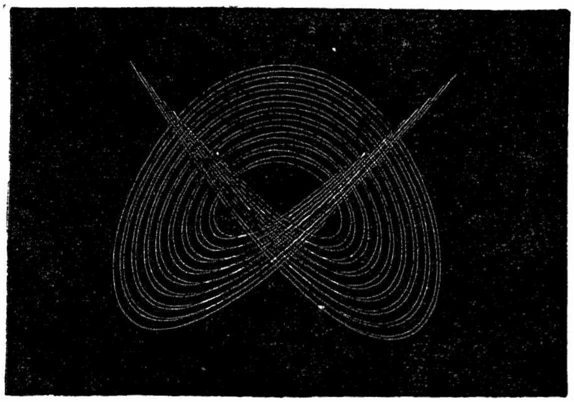

FIG. 11.—Proportion 3 : 4.—Cusped type.

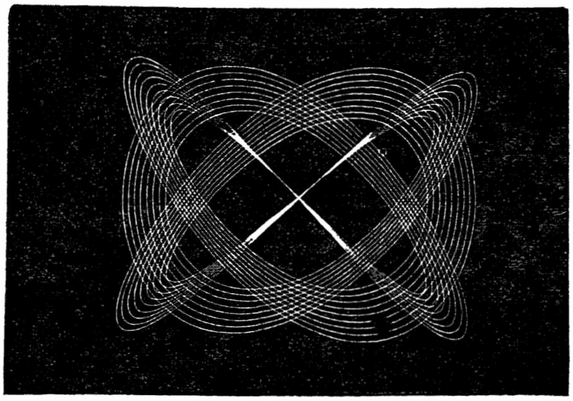

FIG. 12.—Proportion 3 : 4.—Looped type.

pairs, for comparison of relative motions, I found that the two upper joints alone were able to make good their title to independent periods of swing: the third joint acted merely as subsidiary to the second, merely serving to facilitate the bend at the second joint, and always synchronous with that bend, never able to establish a proper period of its own. All three joints conspired to allow undeviating vibration in a plane at right angles to that allowed by the topmost joint alone, just as though there had been only two joints at right angles. I also tried three joints at angles of 90° and 30°, and two joints at an angle of 60°. They all gave similar results. It is clear that the planes of slow and quick vibration are determined solely by the position of the topmost joint, the former being that in which the pendulum can swing by the topmost joint alone, and the latter that in which the topmost joint takes no share at all, or the least possible share, at right angles to the first. However the joints may be arranged, they conspire to establish a virtual second joint, at right angles to the topmost joint, and at some point below it not necessarily coinciding with the position of any one of the actual joints, and apparently varying for different positions of the plane of vibration.

At this stage I showed some of my pendulum's tracery to my brother-in-law, Mr. E. J. Routh, M.A., of St. Peter's College, Cambridge, and received some hints from him which led me to adopt an arrangement whereby the position of the lower joint could be varied, so as to bring the two periods of vibration into any simple proportion, as 1 : 2, 2 : 3, &c. This was done by a very simple mode of suspension,—two cords hanging from two fixed points some distance apart, and passing through a small ring that could slide up and down the concurrent cords and be fixed at any height. In this arrangement I found the germs of an infinite variety of curious and elegant curves, that gave a new lease of life to my course of experiment. Before attempting anything, I drew in my pocket-book the skeletons of the curves that would be described corresponding to the proportions, 1 : 2, 2 : 3, 3 : 4, 4 : 5, 5 : 6, 6 : 7, 7 : 8, 8 : 9, in the periods of quick and slow vibration. Then I hung a bullet by two threads from two points about ten inches apart on the circumference of a child's wooden hoop, fixing the hoop upright with the bullet swinging inside the circle. Another bit of thread furnished the sliding-ring to nip the two convergent threads at any desired point, and a few trials enabled me to fix it pretty accurately for the proportion 1 : 2, so that the bullet would swing in the plane of the hoop in half the time it took to swing at right angles to the plane of the hoop, making two vibrations in the plane of the hoop to one vibration athwart. (The length of the pendulum varies as the square of the period of vibration, so the slider was at a point one quarter of the distance from the bullet to the horizontal line between the two points of suspension.) It was with great interest that I watched the motion of my pensile bullet, and greeted the verification of my pocket book sketch. Pull the bullet aside and start it obliquely, and it describes a crescent-moon, the two horns being formed by the double swing in the plane of the hoop, while the length of the crescent measured from tip to tip is given by a single swing athwart the plane of the hoop. Start the bullet from rest at the centre by a sudden blow obliquely given, and it describes a figure of eight with its length athwart the plane of the hoop. (See Figs. 5 and 6.) Other proportions gave still more curious results in accordance with my skeleton sketches, and made me impatient to try them on a larger scale. The lead at my command I packed into a long-shaped zinc box, with a tubular orifice at the top, which in my service became the bottom. To the lead I added a stone jar containing some 10 lbs. of mercury, and made everything secure with cordage. The dependent tubular orifice seemed made on purpose to accommodate a few inches of wooden roller which carried the glass pen, and a diagonal beam in the ceiling of my bedroom offered a capital fixture for two rings about four feet apart, giving suspension to two iron chains by which my incongruous pendulum-bob was doomed to swing. To nip the chains together at the requisite height, I used a loose link which hooked into the corresponding links in the two convergents, and made a very rude and coarse adjustment, which left all accuracy to chance. It chanced, however, that the adjustment for the proportion 2 : 3 was beautifully accurate, and I shall never forget the feeling of delight which I experienced while watching the marvellous fidelity with which the pen-point traced the curve appropriate to that proportion. The pendulum was drawn aside and started obliquely on one side of the plane of slow vibration, and having to make three vibrations across that plane to two vibrations to and fro, it compounds these into a curve like a capital Q with two tails, one on each side, looking like a swallow-tailed balloon. (See Fig. 9.) At the end of the second to-and-fro vibration the pen returns to the point whence it started, except that friction compels it to fall short little by little at every stroke; but if the adjustment is accurate, as it was in this particular case, the shape of the curve remains the same from first to last, and the figure is filled up to the very centre by the orderly description of curve within curve conspiring to produce a web of lines of astonishing regularity. If the adjustment of the connecting link is very slightly inaccurate, the curve begins to change its shape little by little at every stroke, in one way or another, according as the link is too high or too low; and wonderfully intricate is the result, for after a due series of intermediate stages, the original figure reappears, but reversed; and after another series of changes it presents itself again to view in its original posture, but much diminished by the friction that has been in operation throughout all these changeful phases. It may be imagined how intricate the web becomes, though the limits of illustration do not allow me to give a specimen here. It was easy to eliminate all the transition-curves from the tracery, by depressing the paper for the proper interval, and allowing it to return to contact with the pen only at those distinctive phases when the original figure was reproduced either erect or reversed. I obtained a very curious specimen by applying this selective method to the case of the proportion 1 : 2, allowing the pen to mark only the crescents and the figures of 8 in alternate series, converging orderly to the centre.

For the suspension of the paper, I fixed four pairs of upright rods at the four corners of a shallow tray, which could be slipped under the pendulum, and each rod gave support to an india-rubber band, which, with its fellow at right angles, was attached by a small hook to the corresponding angle of the paper. Each band could be slid up or down its rod, to allow of nice adjustment of the level of the paper, and the whole tray could be raised on a footstool or chair, to suit the elevation of the pendulum when the slider was run aloft in attaining the proportions nearer unity, such as 7 : 8, or 8 : 9.

The iron chain was soon exchanged for strong cords, passing through a narrow wire ring, which could be arrested at any point by a needle driven through both cords below the ring. This was a small improvement, allowing more accuracy in the adjustment of the slider, and therefore more accuracy in the proportion between the two periods of quick and slow vibration. But it was still very far from satisfactory. Meanwhile I had ordered a cylinder of lead, weighing half a cwt, to be cast, with a hole through the axis; for my zinc box full of "notions" was so tall that I could not bring the slider near enough to the centre of gravity to obtain any proportion lower than 1 : 3, and that only with great trouble. When the cylinder of lead appeared, I sawed it into two unequal portions, so that I could use either or both; and instead of simple cords, which twisted in a most troublesome manner below the sliding ring, I introduced a stiff rod of fir to carry the lead by a cross pin, and I used two pairs of cords passing through holes in a slider on either side of the central rod. This slider was a small block of wood pierced to fit the rod, and provided with a lateral screw to fix it at any required height. This arrangement ensured admirable steadiness and freedom from torsion, and a great many sheets were filled with the improved performances of the machine; but there still remained an important defect to remedy. The coarse cords, at the point where they entered the holes in the slider, made a very rough hinge for the cross vibration to rely upon, and it was manifest in the tell-tale records of the curves described that considerable change of period took place between the beginning and the end of the web; and the change was always such as to increase the disparity between the two periods, which could only mean that the level of the centre of quick vibration in the cords immediately above the slider was lowered when the range of oscillation diminished. It was easy to see that a large oscillation would strain the cords to a greater height above the slider than would be called for in a smaller oscillation. The truth of this surmise was proved by the success of the remedy applied. Instead of cords I used two pairs of broad tapes, and instead of a solid slider I made one in two halves, embracing the rod in the centre and nipping the concurrent tapes on either side between their opposed faces, being clamped together by thumb-screws beyond the tapes on either side. Here the slider had no firm hold on the rod beyond the accuracy of its fit, which served to prevent torsion, but had firm hold on the pairs of tapes, pinching them with especial accuracy at the upper edge of the slit in which they lay between the two halves, and reducing the hinge there to a narrow line no thicker than the pairs of tapes instead of the gross thickness of the cords which they superseded. The improvement of the pendulum's performance on paper was very striking. When well adjusted, it was scarcely possible from beginning to end to detect any change in the shape of the figure described; *scarcely* possible, I say, for even now our hinge is not a mathematical line, and we do not obtain perfect mathematical accuracy in our results. Further improvement might be obtained by refinement in tapes and slider, or by increasing the total height of the pendulum, or by substituting some other form of hinge; but the form which I have described is so simple, and its performance so good, that I am content to accept its one very small fault for the sake of its many excellent qualities.

Figures 1-12 are the produce of this pendulum thus improved. They are only a few of the most interesting out of an endless variety of interesting curves, and are chosen as characteristic specimens of a series too extensive to be fairly represented except by a much larger number of illustrations. Figures 1 and 2 represent the proportion 1 : 3, the lowest that is easily attainable without a loftier pendulum; and the following pairs of figures show successively the proportions 2 : 5, 1 : 2, 3 : 5, 2 : 3, 3 : 4. Each of these is illustrated by two figures exhibiting the two chief types of the curve proper to that proportion. They may be termed the cusped type and the looped type. It will be seen that the two cusps in the first figure of each pair are opened into loops in the second, and that each loop in the first is doubled in the second. Between these two typical forms we have an infinite series of intermediate forms possessing features of great interest, those nearest the cusped type especially being characterised by a peculiar "watered" appearance, due to the intersection of two sets of lines very slightly inclined to one another. This is seen, for example, in Fig. 3, which errs a little from the perfect type.

Accuracy of proportion between the two periods of vibration could only be arrived at by repeated trials. The sliding-clamp sufficed for coarse adjustment, but for fine adjustment it was found necessary to attach a subsidiary weight below the large one in some way admitting of considerable range of position, so as to alter minutely the position of the centre of gravity. A heavy iron nut travelling easily on a screw-thread cut on the depending shaft that carried the pen supplied this want, and greatly facilitated the attainment of the utmost accuracy at command.

With a pendulum only seven or eight feet high, there is great difficulty in obtaining the curves that correspond to any proportions lower than 1 : 3, because the slider cannot be brought within a certain distance of the centre of gravity, which lies somewhere in the middle of the lead. To obtain the proportion 1 : 3, that is, to make the pendulum swing three times across for every one swing to and fro, we must lower the slider within a foot of the centre of gravity (the length of the pendulum varying as the square of the period of oscillation), and to obtain the proportion 1 : 4, the distance between the slider and the centre of gravity must be 1-16th of the height of the pendulum, or only six inches in the present instance; but three or four of those six inches are taken up with the thickness of the lead and the attachments of the tapes, and the rest with the depth of the slider, and so the curve cannot be obtained without a more lofty suspension for the pendulum. This greater elevation I found in the great octagonal room which Sir Christopher Wren built as the chief room of the Royal Observatory in Greenwich Park. By means of two hooks fixed above opposite windows in this room, from which my tapes converged to the middle, I got a height of eighteen feet, and was able to reach such proportions as 1 : 4, 1 : 5, 1 : 6. At this extreme it was really amusing to watch the busy haste of the manifold cross-vibration over-riding the staid gravity that marked the slower oscillation to and fro. To obtain proportions lower yet than these, I should want a great increase in height of suspension; but there is no great inducement to attempt this, as the nature of the curve may be foreseen at a glance, and is marked by extreme simplicity—merely a zigzag or a string of beads.

Some of these experiments with lofty suspension were made on stormy days; and while watching the travels of the delicate pen-point, I could see that their regularity was slightly disturbed by every gust of unusual violence that beat against the high walls.

But this article would never end if I allowed myself to dwell on all the points that called for attention in the course of experiment which I have been describing. I fear I have exceeded due limits already, and feel that I owe an apology to the reader for so large a trespass on his patience. My apology must be the elegance and exquisite symmetry of these natural curves in their admirable obedience to a purely natural law, and the great pleasure I have enjoyed—the sense of high privilege I have felt—in their investigation. I understand that these curves, or some of them, have been demonstrated before, by means of a stream of sand flowing from a hole in the base of a vessel that was used as the weight of the pendulum, and I believe that steel springs of elliptic or oblong cross-section have been made to trace such curves as that which first attracted my attention in the vibration of my slender acacia-twig; but I am not aware that any specimens of the series have ever before been exhibited in a form that rendered them accessible to the public eye.

HUBERT AIRY

SOME SPECULATIONS ON THE AURORA

IN preparing a lecture on the Aurora Borealis some months ago, I was led to some speculations which may or may not be new, and may or may not be of some value. I will submit them to the readers of NATURE.

I assume of course that the auroral rays extend to great heights above the surface of the earth, that they are sensibly parallel, and that their apparent point of convergence is, generally speaking, that to which the freely-suspended magnet points. In the great aurora of October 24, 1870, this point was close to η Pegasi at 8.30 P.M., coinciding very well with the direction of the magnet.* Remembering that this aurora was witnessed over a large part of the northern hemisphere, and that there was a contemporaneous aurora in the southern hemisphere, and, assuming that at each place the direction of the auroral streamers is approximately parallel to the magnet, we must conceive the earth, during such an auroral display, as a globe with streamers of light radiating and diverging from its polar regions, and spreading far out into space. The general direction of these streamers at different spots on the earth will be got by placing a magnet below a sheet of paper and getting the magnetic curves with iron filings,

* To-night it is a few degrees below α Cygni (but not clearly defined) at 11 P.M.

and then describing a circle, to represent a section through the axis of the earth so that the magnet shall occupy the central part, about two-thirds of its diameter. The portion of the magnetic curves outside the circle will cut the circle at different angles, and fairly represent the directions of the auroral streamers.

Now, Arago, in his catalogue of auroras, shows that during the months of September, October, March, and April we are especially favoured with auroras; and that in these months they are both brighter and more frequent than at other times. This periodicity indicates an extra terrestrial origin for auroras. Does it not show that during those months we pass through an auroral region, just as in November and August we pass through meteoric regions, or, in other words, that we intersect a ring of some substance capable of being electrified by the earth in its passage, when there is any change in its magnetic power, and so rendered luminous? But it is impossible not to conjecture that this ring or disc is the very disc which is visible to us as the zodiacal light; for besides the fact of zodiacal light being specially visible during the same months, there is the positive evidence of spectrum analysis to the identity of the substances luminous in the aurora and the zodiacal light. We are led then to the hypothesis that there exists round the sun, and extending as far as our earth, an atmosphere, consisting of an unknown element, a gas of extreme lightness, and that this atmosphere is especially condensed in the form of a disc extending round the sun, but probably not concentric with it. The same element appears to exist in the solar corona, and was also detected in the vague phosphorescent luminosities of the sky on a particular evening, by, I think, Angström.

I wish to suggest, therefore, that catalogues of auroras may, like catalogues of meteors, determine auroral regions in the earth's orbit, and that two such regions are, in fact, already shown by Arago's catalogue, and that this periodicity, as well as the results of spectrum analysis, indicate a cosmical origin for auroras.

There is one more point which may be interesting. The luminous streamers have a lateral motion; they shift sideways, and in fact rotate round their pole. Is this motion of rotation always, or even generally, in the same direction? I have not observed it often enough to speak with confidence. But if so, it must have some definite cause, and will be analogous to that of rotation in a definite direction of an electrical current round the pole of a magnet. The earth must be looked upon as a delicate solar electroscope and magnetometer, and the electrical discharge round the earth is stratified, and is in lines and strata that have, perhaps, motions in definite directions.

It may be worth remarking that the 22nd, 23rd, 24th, and 25th of October are the most famous days in the year for auroras, at least in the present century, and that the greatest displays of all on those days have happened at intervals of multiples of eleven years. Last year we had splendid auroras on the 24th and 25th; there is, therefore, some ground for expecting fine auroras on the same nights this year, if the auroral cycle corresponds to the sunspot and magnetic cycles. J. M. WILSON

NOTES

WE learn from Indianapolis journals, received at the moment of going to press, that the American Association for the Advancement of Science commenced its sittings on the evening of August 21 by an opening address from the retiring president, Prof. T. Sterry Hunt, on the Iron Interests of Indiana, in which he completely sustained every claim that had been made for the State, showing conclusively that it has the elements within its borders from which to secure a manufacturing future that shall make Indiana the mediterranean workshop for the whole country. The sections commenced their sittings on the following day, and San Francisco was fixed on as the next place of meeting. An extra double number of the *American Naturalist* for September 15 will give a full report of both the opening address and the sectional proceedings In a future number we shall give an epitome of all matters of interest discussed at the meeting.

M. JANSSEN has been commissioned by the French Government to proceed to the East to observe the total Solar Eclipse of December next. He has, therefore, been compelled to decline the offer made to him by the British Association to take part in the British Expedition.

THE President of the Royal Society has received a telegram from the Government Astronomer, Melbourne, that the Eclipse Expedition will leave that port on November 20.

WE regret that owing to the omission of a sentence, the note respecting the distinguished visitors at Section A of the late meeting of the British Association, read incorrectly in a small proportion of the edition of our last number. We now supply the omission by giving the following probably unexampled list of Senior and Second Wranglers and Smiths' Prizemen who were present:—Adams, Cayley, Challis, Stokes, Hon. J. W. Strutt, Hopkinson, Kelland, Tait, Wilson, Thomson, Maxwell, Sylvester, Clifford, Jack, J. W. L. Glaisher; of these the first nine were Senior Wranglers.

WE learn from the *British Medical Journal* that in accordance with the will of the late Dr. Lacaze a prize of 10,000fr. is to be awarded by the Faculty of Medicine of Paris every second year to the best work on phthisis and on typhoid fever alternately. The first prize will be awarded at the end of the academical year 1871-2, for the best work on phthisis. Essays (with a distinguishing motto and the author's name in a sealed envelope) must be sent in before July 1, 1872. The prize is open to foreigners.

IN a paper read before the Natural History Society of Boston (U.S.), Mr. W. T. Brigham gives an account of several remarkable earthquakes that have occurred in New England, with a list of all such phenomena that have occurred in that region from 1638 to 1870. Some of these disturbances appear to have been violent and protracted.

WE understand from the *Geological Magazine* that there will shortly be published a Geological Atlas of England, by Mr. W. Stephen Mitchell. The Atlas will contain the following Maps:—1. Cambrian (of Survey); Lower Cambrian (of Sedgwick). 2. Lower Silurian (of Survey); Middle and Upper Cambrian (of Sedgwick). 3. Upper Silurian (of Survey); Silurian (of Sedgwick). 4. Old Red Sandstone; Devonian. 5. Carboniferous Limestone; Yoredale Beds. 6. Millstone Grit; Coal Measures. 7. Permian (of Survey); Pontefract Group (of Sedgwick). 8. New Red Sandstone; Rhætic (Penarth). 9. Lias. 10. Lower Oolite. 11. Middle Oolite. 12. Upper Oolite. 13. Wealden; Neocomian. 14. Gault; Upper Greensand; Chalk and Chalk Marl. 15. Eocene. 16. Crag. 17. Alluvium. 18. Bone Caves. 19. Metamorphic (?) 20. Igneous. The Maps will be printed in colours, each Map exhibiting only the range of one formation, and the names of places on the formation. In some few cases where it is requisite, as a clue to the locality, to introduce the names of places near, but not on, the formation, these will be printed in a different type. The Maps ($11\frac{3}{4}$in. by $9\frac{1}{4}$in.) are based on a photographic reduction of the last edition of the Greenough Map, which is published under the direction of a committee appointed by the Geological Society. In all cases where, through researches more recent than this last edition, any changes have been adopted in the grouping of the beds, this atlas conforms with the latest alterations. The revision of the proofs of particular maps has been kindly promised by Mr. W. Boyd Dawkins, F.R.S., Mr. W. Whitaker, Mr. H. Bauerman, Mr. J. W. Judd, Mr. Charles Moore, Mr. W. T. Aveline, and others. Letter-press will accompany each map, giving in a tabulated form the subdivisions of the formations, the origin of the names of the groups of beds, their lithological characters, thickness, range, &c., with a historical notice of the various classifications that have been at different times employed. The lists of fossils will be arranged on a new plan, showing in a tabulated form for each formation the genera that first appear, those that last appear, and those that are numerically abundant in that formation. Separate tables give the characteristic species. These lists are prepared expressly for this work by Mr. R. Etheridge, F.R.S., &c., Palæontologist to Her Majesty's Geological Survey of Great Britain.

THE continental scientific journals record the death of Dr. Milde, a well-known botanist, whose contributions to systematic cryptogamic botany are especially valuable.

WE have to notice the death, at a very advanced age, of James De Carle Sowerby, the first secretary of the Royal Botanic Gardens, Regent's Park, an office which he held till last year, when he resigned it in favour of his son. Mr. Sowerby belonged to a family, many members of which have distinguished themselves by their devotion to various branches of science, and to the pictorial illustration of natural objects.

THE Essex Institute publishes an obituary notice of its late president, Mr. Francis Peabody, of Salem, who died October 31, 1867, and who was noted for his researches in mechanical physics.

THE trustees of the Manchester Grammar School are so satisfied with the excellent work done in the Physical Science Department, under the superintendence of Dr. W. M. Watts, that they have begun to fit up a second and larger laboratory, at the cost of from 700*l.* or 800*l.* It is only three or four years since this department of the school was opened, and already many valuable scholarships and other honours have been gained by the boys.

THE following eminent archæologists are announced as contributing papers for the next session of the Society of Biblical Archæology:—M. Heinrich Brugsch, F. C. Chabas, Clermont Ganneau, and the Chevalier de Sauley. The first part of the society's transactions will be ready early in the spring, and will contain articles by Dr. Birch, J. W. Bosanquet, M. Ganneau, Prof. Lowne, Lieut. Prideaux, G. Smith, and H. Fox Talbot.

THE Society of Arts have consented to give their co-operation to the Polytechnic Exhibition, to be held at Moscow next year, in celebration of the two hundredth anniversary of the birth of Czar Peter the Great.

ACCORDING to recently-published statistics of the University of Edinburgh Botanical Class, in the session of 1871 the number of pupils was 306. Of these, 241 (including 5 ladies) were medical students, 12 pharmaceutical students, and 53 general students.

THE Archæological Society, whose gathering at Weymouth we recorded last week, devoted Wednesday to an examination of objects of antiquarian interest in that town, including the Corporation regalia and muniments. On Thursday papers were read as follows:—By Mr. H. S. Cuming, F.S.A., "On the Patron Saint of Dorset, St. Edward, King and Martyr." By Mr. J. Drew, F.R.A.S., F.G.S., "On Art Treasures and their preservation." By Mr. G. Eliot, "On the Antiquities of Portland." There was afterwards an excursion to Corfe and Dorchester, visiting several objects of interest on the way. The papers read on Saturday and Friday evenings were as follow:—Mr. J. R. Planché, Somerset Herald, "On the Family of Robert Fitzgerald, the Domesday Tenant of Corfe." Mr. Edward Levien, M.A., F.S.A., Hon. Sec., "On Wareham and its Religious Houses." Mr. W. H. Black, F.S.A., "On Wareham and the Earliest Historic Monuments in Dorset." Rev. William Barnes, B.D., "On the origin of the name and people of Dorset." Mr. Joseph Stevens, M.D., "On newly-discovered Roman and Saxon remains at Finkley near Andover." The meeting was brought to a close on Saturday evening. Saturday's excursion was first by rail to Bindon Abbey, thence to Wareham, and afterwards by rail to Corfe Castle. The concluding meeting was held at the Royal Hotel on the return of the excursionists to Weymouth, at 8.30, when, after the reading of some papers, the usual formal resolutions and votes of thanks to the gentlemen who had assisted the Association in conducting the proceedings were passed and the congress was brought to a close.

THE Annual Meeting of the Devonshire Association for the Promotion of Literature, Science, and Art, recently held its sittings at the picturesque little town of Bideford, occupying three days, the retiring President, Mr. J. A. Froude, resigning the chair to the Rev. Canon Kingsley, who gave an eloquent and interesting address. Papers were read, mostly of an archæological and geographical character, by Mr. Pengelly, Mr. Spence Bate, and other distinguished Devonians.

IT is stated that Prof. Watson, of the University of Michigan, has discovered a new planet in the constellation Capricorn, of the tenth magnitude. This is the 115th of the series.

MR. J. R. HIND, F.R.S., has calculated the Ephemeris for Greenwich mean time of Futtle's Comet, which will be visible during this and next month. According to Prof. Luther, its next perihelion passage will occur about the 30th of November. The following are Mr. Hind's figures:—

1871	Right Ascension.	Declination.
Sept. 1	100° 13·2′	62° 22·7′
„ 7	106° 36·6′	60° 55·6′
„ 15	115° 12·8′	58° 20·7′
„ 23	122° 38·8′	54° 59·8′
Oct. 1	129° 15·7′	50° 49·5′
„ 7	133° 44·3′	47° 2·6′
„ 13	137° 51·5′	42° 35·5′

PROF. A. HALL sends us some careful Equatorial Observations made at the U.S. Naval Observatory, Washington, and Supplementary Notes on the observations for magnetism and position made in the U.S. Naval Observatory expedition to Siberia to observe the Solar Eclipse of August 7, 1869.

A RECENT number of the "Astronomische Nachrichten" contains an elaborate paper by Prof. E. Schönfeld, "On the Change of Light of Variable Stars."

THE *Journal of the Society of Arts* states that a memorial monument has been erected in New South Wales to the memory of Captain Cook, at the supposed place at which he landed from the *Endeavour* in April, 1770. On the monument are two brass plates, one bearing the following inscription:—"Captain Cook landed here 28th April, 1770. Victoria Regina. This monument was erected by the Hon. Thomas Holt, M.L.C., A.D. 1870. The Earl of Belmore, Governor." The other contains the following words from Captain Cook's journal:—"We discovered a bay and anchored under the south shore, about two miles within the entrance, in 6 fathom water, the south point bearing S.E., and he north point east. Latitude 34° S., longitude, 208·37°." The entrance to the bay where Cook landed has other memorials. On the north side is the column erected, on behalf of the French nation, to the French navigator, La Perouse. The enclosure around the column is planted with trees and flowers. The monument erected by Mr. Holt is on a place less elevated, but it can, nevertheless, be seen from several parts of distant suburbs. Public subscriptions are being made for a monument of a more costly kind, to be erected in one of the parks of the city of Sydney.

THE last number of the Bulletin of the Société d'Acclimatation of Paris contains an interesting and important report on the

International Fishery Exhibitions of Boulogne, Arcachon, and Havre.

AT the recent annual meeing of the Royal Cornwall Institution, a discussion arose on a paper read by Mr. Robert Blee "On the Comparative Health and Longevity of Cornish Miners," in the course of which the startling statement was made, that a death occurred every other day among the Cornish miners from the mode in which the men were raised from the pits.

PROF. DANIEL WILSON, of Toronto, publishes in the *Canadian Journal* an essay on "The Huron Race and its Head-form," illustrated with a lithograph and many outline drawings. Prof. Wilson's investigations lead him to believe that the comprehensive generalisations of earlier American ethnologists, under the guidance of Dr. Morton, which led to the doctrine of a homogeneous cranial type for the American aborigines, have everywhere failed when subjected to the crucial tests of detailed observation, and that we everywhere find transitions from one to another and essentially distinct ethnical group. There is, he concludes, no longer an assumed American man, as distinct from every type in the Eastern Hemisphere as the Catarhine Simiadæ of the Old World from the Platyrhine group of New World monkeys.

ON Monday, August 21, between three and four o'clock in the morning, a large waterspout burst over the village of Ollon and the adjacent mountains in Switzerland. Great damage was done to the roads and vineyards, but no loss of life is reported.

A VIOLENT hurricane and some earthquake shocks are reported from the Island of St. Thomas, in the West Indies, on the 21st of August. Hundreds of houses were destroyed, and over 150 persons killed or wounded.

FROM Indian sources we learn that the rainfall in Bombay this season is generally less than half the average of former years.

A VIOLENT typhoon raged at Kobe in Japan, on the 4th of July. Many vessels were wrecked, and about 400 lives were lost. Great damage was done to property on sea and on land.

THE news of most terrible earthquake shocks and volcanic disturbances comes to us from the Philippine Islands. In the small island named Camiguin, near to Misamis, for some months past a succession of most violent earthquakes has been experienced, causing crevices, &c., in the open country. On the 1st of May, about five o'clock in the evening, the earth burst asunder, and an opening was formed 1,500 feet long. Smoke and ashes, earth and stones, were thrown up and covered the ground far and near. At about seven o'clock, as darkness was coming on, this crater burst into activity with a loud explosion, followed by a shower of lava and ashes. About 150 persons were destroyed. The eruption of the new volcano has since been so tremendous that the inhabitants have forsaken the island, and of the 26,000 previously there, not 300 are left. Camiguin is only about thirty-six miles in circumference, and was very productive in abacá (the Manilla hemp) yielding annually from 30,000 to 40,000 piculs, or more than a tenth of the produce of the world. There is little hope of the island ever being again reoccupied or cultivated.

THE *American Journal of Microscopy* recommends, as the best plan of collecting diatoms in large quantities, to tie a thin, fine piece of linen over the faucet of the hydrant in the evening, and allow a small stream of water to pass through it all night. In the morning take off the cloth and rinse it in a little water in a goblet. When ready to examine, take a drop of water from the bottom of the goblet with a small pipette, or glass rod, and place it on a flat slide, or a slide with a concave depression, holding a few drops. Then, with a power of 100 or 350, sweep the field, and you will be rewarded with the sight of a wondrous collection of beautiful and unique forms.

THE BRITISH ASSOCIATION MEETING AT EDINBURGH

SECTION A.

Report of the Tidal Committee, by Sir W. Thomson.

HE stated that the work performed for the Tidal Committee since the last meeting of the British Association had consisted chiefly in the evaluation of tide components in a similar manner to that described in the previous reports. Mr. Parkes having again placed the tracings of the curves of the Kurrachee (Manora) self-regulating tide gauge at the disposal of the committee, a second year's observations had been read off and completely reduced. In addition to the tide components evaluated for Liverpool and Ramsgate, others had been introduced to correct the lunar diurnal (declinational) tides for parallax. Those components had been found to have sensible values for Kurrachee, where the diurnal tides are comparatively large. The solar elliptic semi-diurnal components had also been included, now that two complete years' observations were available. The comparison between the calculated and recorded heights from Liverpool not being considered as good as might have been expected from the labour bestowed on them, it was determined to continue the analysis of the Liverpool tides, with the view, if possible, of detecting the cause of the largeness of some of the differences. It would be seen on comparing the results contained in the previous report with the results arrived at, that the chief tides (the lunar and solar semi-diurnal) are now more retarded by about 4° than during the year previously analysed. The calculated heights in the comparison should therefore more nearly represent the heights about eight minutes after the hours assigned to them. An examination of the differences would show this to be the case. A fresh calculation and due allowance made for atmospheric pressure would doubtless very considerably reduce the discrepancies. The gradual increase in the height of the mean level of the water, probably arising from the filling in of the bed of the river, and consequent increase of friction, would account for some portion of this increased retardation. There was a very violent rise in the mean level for the year 1868-69, amounting to four-tenths of a foot. It, however, in the following year, had again subsided to almost its anticipated height. The uncertainty in the mean level of the water is an element which must at times seriously affect the differences between calculated and recorded heights, in any method of computation of heights from a *fixed* datum. It was very much to be regretted that the authorities at Liverpool had chosen the George's Landing-Stage for a tide float, affected as it must be (sometimes to a considerable extent) by the ever-varying weight it has to bear. This would affect the whole of the tide components evaluated, but more especially the solar components, and will account for the different values of the solar semi-diurnal tide, which, judging from the corresponding lunar component, should agree within much narrower limits. It was therefore thought that, should it be determined to again discuss the Liverpool tides, it would be better to take the tide curves as self-registered at Helbie Island, at the mouth of the Dee, in preference to those of the George's Pier. The Helbie Island tide curves, it was considered, would give much superior results. Through the kindness of the United States Coast Survey Office, two years' tide observations, taken at Port Point, San Francisco Bay, California, had been received. Here again there was an abrupt diminution in the height of mean level for the first two years. It having come to the knowledge of the Tide Committee that the United States Coast Survey Office were in possession of a series of hourly tidal observations, taken at Cat Island, in the Gulf of Mexico, and which were of a very remarkable and interesting character, it was thought a favourable opportunity of testing the value of the harmonic analysis for the evaluation of the components of the tides of this place, which appeared very complicated and peculiar. Application having been made, a series of about thirteen months had been received, and were now in course of reduction. It was extremely interesting to find that, although the lunar and solar semi-diurnal tides were very small in value, the series of means from which they were obtained were extremely regular and good, and the consequent determination of the phase of spring tides from their respective epochs was probably correct within a few minutes. The proportion between the amplitudes of the lunar and solar semi-diurnal tides was the nearest to equality yet obtained, being in the ratio of 11 to 6. The proportion between the lunar and solar diurnal (declinational) tides was about 4 to 1. After reading the report, Sir William said that one chief object which the originators of this investigation had in view was the determination of long period tides, and particularly the lunar declinational fortnightly tide, and the solar declinational semi-annual tide. The reason for desiring the determination of such tides with great accuracy was that this would give a means of estimating with absolute certainty the degree of elastic yielding which the solid earth experienced under the tide-generating influences of sun and moon. It was quite certain that the solid earth did yield to some degree, as it must do so unless they were infinitely rigid. It had long been a favourite assumption of geologists that the earth consisted of a thin shell of solid rock twenty to fifty miles thick, according to various estimates, inclosing an interior filled with melted material, lava, metals, &c. This hypothesis was, however, absolutely untenable, because, were it true, the solid crust would yield with almost as perfect freedom (on account of its thinness and great area) as if it were perfectly liquid. Thus the boundary of the solid earth would rise and fall under the tide-generating influences so much as to leave no sensible difference to be shown by the water rising and falling relatively to the solid, showing that if the earth, as a whole, had an average degree of rapidity, equal to that of glass, the tides would be very much diminished from the magnitude corresponding to a perfectly rigid globe with water like that of our seas upon it. This consideration, he had shown, rendered it probable that the earth had considerably more average rigidity than a globe of glass of the same size. The mathematical calculation showed a somewhat startling result, to the effect that a globe of glass of the same size as the earth, if throughout of exactly the same rigidity as glass on a similar scale, would yield, like an indiarubber ball, with remarkable freedom to the tide-generating influences, thus leaving a very much smaller difference to be shown by water if placed on the surface of such a globe, and estimated in its rise and fall relatively to the solid bottom on which it rested. The precise agreement of precession and nutation with dynamical estimates, founded on the supposition of the earth being perfectly rigid, made it probable that the earth was in reality vastly more rigid as a whole than any specimen of surface rock in the condition in which it was when experimented on in our laboratories. In speaking on this subject about ten years ago to Dr. Joule, that gentleman suggested that probably the great pressure in the interior produces in the material—which might be of the same substance as surface rocks—a greatly-increased rigidity in its actual position at any great depth below the surface; but the proposed tidal observation and calculation was the only method which gave directly, and without any possibly doubtful suppositions regarding interior arrangement of density on the earth, a measurement of its elastic yielding to the tide generating influences. Now that observations from so low a latitude as that of Cat Island were available for comparison with those of the tides on our own coast, the committee might advance hopefully to this part of their inquiry, which, accordingly, they proposed to make a primary object in the calculations to be next undertaken.

The other papers read were almost entirely confined to pure mathematical subjects. They were as follows :—

Report on Hyper-elliptic Functions by W. H. L. Russell, F.R.S.

Note on a Question in Partitions, by Prof. Sylvester, F.R.S.

On the Number of Invariants of a Binary Quantic, by Prof. Cayley, F.R S.

On Linear Differential Equations, and *On Focal Properties of Surfaces of the Second Order*, by W. H. L. Russell, F.R.S.

On Certain Families of Surfaces, by C. W. Merrifield, F.R.S. If $z = F(xy)$ be a surface, then writing

$$\alpha = \frac{d^3 z}{dx^3},\ \beta = \frac{d^3 z}{dx^2 dy},\ \&c.$$

If the surface be a cone $(\alpha\delta - \beta\gamma)^2 = (\alpha\gamma - \beta^2)(\beta\delta - \gamma^2)$; and if a cylinder $\alpha\delta - \beta\gamma = 0$, $\alpha\gamma - \beta^2 = 0$, $\beta\delta - \gamma^2 = 0$. In the present paper it is investigated whether solid surfaces, fulfilling these conditions, are necessarily cones or cylinders.

Description of a Model of a Ruled Cubic Surface, by Prof. Ball. The cubic surface was $z(x^2 + y^2) - 2axy = 0$.

On Vortex Rings, by Prof. Ball.

On the Mathematical Theory of Atmospheric Tides, by Prof. Challis, F.R.S.

Remarks on Napier's Original Method of Logarithms, by Prof. Purser.

On the Calculation of e (the base of the Napierian Logarithms) from a Continued Fraction, by W. L. Glaisher, F.R.A.S. The continued fraction from which e was calculated was

$$\frac{e-1}{2} = \frac{1}{1+}\ \frac{1}{6+}\ \frac{1}{10 + \&c.}$$

A formula far more convergent than the ordinary one for e in a series. The calculation gave e to 137 decimals, and confirmed the result given by Mr. Shanks, the value of e given in all the editions of Callet's logarithms being incorrect from the fortieth figure.

On Certain Definite Integrals, by J. W. L. Glaisher. The integrals were $\int_0^\infty \sin(x^n)\,dx$. $\int_0^\infty \cos(x^n)\,dx$.

On Lambert's Proof of the Irrationality of π, and on the Irrationality of certain other Quantities, by J. W. L. Glaisher. The quantities referred to were chiefly circular and exponential fractions.

On Doubly Diametral Quartan Curves, by Prof. F. W. Newman. A large number of drawings of quartic curves were exhibited to the Section.

On a Canonical Form of Spherical Harmonics, by Prof. W. K. Clifford. The canonical form in question is an expression of the general harmonic of order, n, as the sum of a certain number of sectorial harmonics, the number being when n is even, $\frac{5n - 10}{2}$, and when n is odd, $\frac{5n - 9}{2}$

SECTION C.

THE papers to be read on Tuesday numbered twenty-three, so that but little time could be allowed to each author, and then there was time for no more than half the papers to be brought forward. A report by Prof. Duncan, M B., F.R.S., on British Fossil Corals was read, wherein he pointed out the relations between the neozoic and palæozoic corals. Then Prof. Geikie read his report on the Progress of the Geological Survey of Scotland, a notice of which appeared in NATURE of August 10. Mr. Henry Woodward described a new and nearly perfect Arachnide from the Ironstone of the Dudley coal-field. The Pennystone Ironstone nodules of the coal measures have long been celebrated for their fossil contents, having yielded King Crabs, wings of Orthopterous insects, a supposed beetle, and numerous plant remains, both ferns and fruits of Lycopodiaceæ. The specimen described by Mr. Woodward is perhaps the most perfect form hitherto described. It is identical with one figured and described by Buckland as a Diamond beetle (*Curculio*) and named by him *Curculioides Prestvicii*. By means, however, of the specimen now obtained, the author clearly showed that it was not a Coleopterous insect, but a true Arachnide, closely related to the recent genus *Phrynus*. Mr. Woodward proposed, therefore, to name it *Eophrynus Prestvicii*, the genus *Curculioides* being retained for *C. Anstieii*, another specimen also figured by Buckland, which may be a true Rhynchophorous insect. Dr. Bryce called attention to some fossils from the Durine Limestone of Sutherland, Prof. Harkness exhibited one of the earliest forms of Trilobites, and Mr. John Miller furnished some remarks on the so-called Hyoid Plate of *Asterolepis*, and pointed out that it was really the dorsal plate.

Mr. Milne Home brought forward a notice of a scheme for the Conservation of Remarkable Boulders in Scotland, and for the Indication of their Position on Maps. Mr. Moggridge mentioned that in Switzerland a right of property in some of the boulders had been acquired by natural history societies and museums with a view to their preservation, and that on these a brass plate had been fixed with the word "Investable" marked upon it.

Prof. Traquair noticed some additions to the Fossil Vertebrate Fauna of Burdiehouse, near Edinburgh, and also called attention to a Labrynthodont skull, seven inches long, from the same limestone quarries (of Lower Carboniferous age), probably belonging to Huxley's genus *Pholidogaster*; this was the lowest geological horizon from which the remains of Labyrinthodont Amphibia had been as yet described.

At the meeting of the British Association at Liverpool, the Rev. John Gunn, F.G.S., &c., expressed the opinion that

Boulder Clays ought rather to be regarded as an evidence of a temperate climate in the districts where they are found, than of a glacial epoch; and in a communication now made he maintained that there is no occasion to invoke any additional causes of change of climature over and above those which are known to exist. He made some remarks on the agency of the sea in scooping out valleys and bays while clearing off or gathering over the surface of any area.

Mr. J. E. Taylor read an interesting paper *On the later Crag Deposits of Norfolk and Suffolk*. Mr. Prestwich remarked that the belief was gradually gaining ground, that the Red Crag was contemporaneous with the Norwich Crag. In regard to the fossil contents, he pointed out the difficulty there was in distinguishing the extraneous species.

Mr. P. W. Stuart Menteath read a very important paper *On the Origin of Volcanoes*, which, unfortunately, had to be hurried through in such a manner that but little could be gained from the hearing of it.

L'Abbé Richard read a paper (in French) *On Hydrogeology*.

Mr. W. S. Mitchell reported *On the Leaf-beds of the Lower Bagshot Series*.

Mr. C. W. Peach made some additions to the list of Fossils and Localities of the Carboniferous Formation in and around Edinburgh, and mentioned the occurrence of *Lituites giganteus*.

The Rev. W. S. Symonds exhibited a new *Onchus* spine from the Lower Old Red sandstone of Hay, Brecon.

A number of papers were held over until Wednesday, when it was arranged to read them; but as none of the authors of papers put in an appearance, Prof. Geikie adjourned the reading of the papers until the next meeting of the Association.

SECTION D

SUB-SECTION.—ZOOLOGY AND BOTANY

PROF. WYVILLE THOMSON made some observations on the palæontological relations of the fauna of the North Atlantic, as brought to light in his recent dredging explorations in the North Atlantic. In introducing his observations on these fauna, the professor called attention to the fact that, great as the results of the expedition in Her Majesty's ship *Porcupine* might fairly be held to be considered as an addition to scientific knowledge, still, the actual ground got over by dredging at any very considerable depth was of very small extent comparatively with what yet remained to be done. The field for these investigations, therefore, might be called in a sense unlimited.

Prof. Van Beneden read a paper, *On the Bats of the "Mammoth epoch as contrasted with those of the present day.*

MISCELLANEOUS

Among these we may particularly mention a paper by Mr. W. A. Lewis entitled *A Proposal to modify the strict Law of Priority in Zoological Nomenclature in certain cases*. The author insisted that it was perfect infatuation to serve blindly under word of the code drawn up under the sanction of the British Association now thirty years ago, and proposed that where there was now (August 1871) a universal agreement about a specific name, that name shall not be displaced on account of any prior name being discovered.

Dr. Sclater made some remarks on what he held to be an appropriate opportunity of establishing zoological observatories in connection with certain astronomical observatories which were to be established for the purpose of taking observations of the transit of Venus in 1874. On the occasion of the approaching transit, the Astronomer-Royal proposed to organise observing expeditions to the following five stations:—(1) Oohu, Sandwich Islands; (2) Kerguelen's Island; (3) Rodrigues; (4) Auckland, New Zealand; (5) Alexandria. At the first three of these stations it would be necessary to have a corps of scientific observers resident for twelve months previous to the transit, in order that the absolute longitude of these places, which was not now correctly known, might be obtained. Dr. Sclater pointed out how little was yet known of the terrestrial and marine zoology of these three islands, and specified various particulars in the case of each of their faunas, which it would be especially desirable to investigate. He then urged that the addition of one or more zoological collectors or observing naturalists to the corps of astronomical observers in each of these stations would occasion very slight additional expense, and suggested that application should be made to the Government to allow such naturalists to accompany the expedition, and to undertake the necessary explorations. He stated that there was a precedent for this course in what had been done in the case of the Abyssinian expedition.

The department unanimously concurred in the suggestion, and the desirability of such an application to Government being made.

Dr. Grierson read a paper *On the Importance of forming Provincial Museums, in which the Products of Districts might be Exhibited*. These museums could be connected with consulting and lending libraries, and from a central source there could be sent articles for exhibition at different times, and also persons who could give instructions on such subjects. Such institutions would not only tend to spread knowledge amongst the people, but they would be a means of preventing intemperance and improving their moral habits.

Miss Lydia Becker said she took an interest in this subject as one of those to whom a small share of responsibility had been given in enforcing the Education Act, being a member of the School Board of Manchester. That Board was now about to issue a scheme for a general course of instruction, and had appointed a committee for that purpose, of which she was a member. It had always seemed to her to be a matter of extreme importance to introduce such habits of observation as would follow from the introduction of natural science into elementary schools. She believed there was no portion of the population who were more likely to be interested in the matter than the children who attended these schools. They came there with their minds fresh and open to receive those impressions which were given in childhood, and they were very apt scholars. It had been said that the difficulty was in teaching boys; but she thought it was of as much consequence to teach girls natural science as boys. With regard to the principles of physiology and the laws of health, she thought that if any difference was to be made between the sexes, the girls should be first considered in the matter, as so much of the health of the population depended on the intelligence of women in these matters.

Sir Walter Elliot read a paper *On the Advantage of Systematic Co-operation among Provincial Natural History Societies*. It stated that a comparatively hurried inquiry had disclosed the existence of 115 such societies in Great Britain and Ireland. With reference to their publications, although the volumes of a few of the more important were found in several public libraries, the transactions of by far the greater number did not extend beyond their own localities. In this way not only were the great body of naturalists shut out from much useful information, but the isolation which existed must be detrimental to the societies themselves. Two modes of remedying the evil suggested themselves to his mind. One was, to have a central committee or single editor to collect and condense the most useful materials in all the local transactions; and the other, to form groups of societies, and publish the more original and valuable papers in each group under a joint editorship.

Mr. Symonds, who had been connected with the Cotswold Field Club for many years, said one of the great difficulties connected with these societies had been condensing the reports and publishing the papers that were worth publishing in one general volume of transactions. In Gloucester, paper after paper was published of the most valuable kind that would have done honour to the Royal Society if they had been read there, but which it was impossible for persons to obtain unless they were members of the club, or had friends who were members of it. He thought the difficulty could be met by having a council composed of the presidents, vice-presidents, and secretaries of field clubs throughout the length and breadth of the land, by whom the papers which were worth publishing could be selected. The paper which Sir Walter had now read would, he hoped, have the effect of producing some organisation among these clubs such as he had suggested.

A short discussion took place on the desirableness of some effort being made to utilise the information which was contained in many of the papers read before these clubs, and Sir Walter Elliot said he believed that before these meetings had closed a meeting would be held of those interested in this matter, to consider what should be done.

Three papers on Spontaneous Generation were read; the first of which, by Dr. Ferrier and Dr. Burdon Sanderson, F.R.S., was *On the Origin and Distribution of Bacteria in Water*, *and the Circumstances which determine their Existence in Animal Liquids and Fluids.* In this were detailed the results of a large number of experiments undertaken to throw light upon the phenomena of contagion. The authors employed Pasteur's solution, and also certain animal fluids, but they wished it to be understood that the conclusions at which they had arrived had reference merely to the different fluids employed, and had no distinct bearing upon the possibility or non-possibility of spontaneous generation occurring in other fluids. They did not find any evidence to show that organisms arose *de novo* in their fluids. On the contrary, they thought that the occurrence and number of organisms had to do with the extent of exposure to germs either in air or water. Some of the results arrived at were very important. Boiling the fluids employed was always found to destroy all *Bacteria* and their germs, and other experiments were recorded, tending to show that the air did not contain living *Bacteria*, as so many have assumed. They also ascertained that *Bacteria* were unable to resist the effects of desiccation even at the ordinary temperature of the air. Their examination of the fluids of the body tended to show that these, in their normal condition, did not contain the germs of *Bacteria* or other organisms. Blood and serum, when received in super-heated vessels and exposed only to super-heated air, did not undergo putrefaction—apparently because these fluids did not contain the germs of living organisms.

Dr. Dougal then read a paper *On the relative Powers of various Substances in preventing the Generation of Animalcules, or the Development of their Germs, with special reference to the Germ Theory of Putrefaction*, in which he detailed the results of his experiments upon the power which various poisons, antiseptic substances, and salts have in arresting the development of organisms, and in preventing the phenomena of putrefaction. His conclusions were wholly adverse to the germ theory of fermentation and putrefaction.

Dr. Charlton Bastian, F.R.S., followed with a communication *On some new Experiments relating to the Origin of Life.* After calling attention to the fact that not-living mineral materials were continually being converted into the substance of plants during their growth, and that no special "vital principle" was now believed by physiologists to exist in plants, he said that the question that had to be settled was, whether the elements of not-living matter could group themselves anew, so as to produce living matter under the influence of the same physical forces which were concerned in bringing about the growth of the plant; or whether such combination could only be effected in the presence of pre-existing living matter in which (as was generally admitted) no special forces were resident. This question could, he thought, only be settled by experiments. Fluids deemed suitable for the production or development of living things had to be enclosed within hermetically sealed vessels, and then such flasks and their contents had to be exposed to a degree of heat which could be proved to be destructive to any pre-existing living matter which they might contain. If, after the lapse of a certain period, the flasks still remaining hermetically sealed, the fluid showed evidence of the existence and multiplication of life, then it was argued such living things must have been evolved *de novo* from some new combination among the organic molecules contained in the solution. It was therefore obviously impossible to come to any conclusion on the subject until it had been definitely ascertained what amount of heat living matter (existing in the form of the lowest organisms) could withstand. The evidence on this subject was, Dr. Bastian thought, very clear and decisive. In the first place, he had taken water containing large quantities of *Amœbæ*, ciliated infusoria, and other organisms, and had ascertained that they were invariably killed by raising the temperature of the water in which they were contained to 140° F. When we have to do with organisms of this size there can be no difficulty in ascertaining what the effects of the heat have been. Some of the organisms were partially disorganised by this temperature, and none of them ever showed any signs of life after the exposure, although kept under observation for 24 hours or more. Dr. Bastian then referred to other experiments which he had elsewhere recorded, showing that *Bacteria*, *Torulæ*, and their germs, whether visible or invisible, were destroyed by exposure for ten minutes to 140° F.

A solution of tartrate of ammonia when inoculated with a drop of fluid containing living *Bacteria* and *Torulæ*, became quite turbid in the course of one or two days, owing to the presence and multiplication of myriads of *Bacteria*. But when a similarly inoculated solution was exposed to the temperature of 140° F. or upwards, it afterwards remained perfectly clear, even though freely exposed to the air, thus showing, not only that the organisms and their germs which had been inoculated were killed by exposure to this temperature, but that the air did not contain any such multitude of living *Bacteria* germs as had been alleged. Even had he been unable to fix the precise degree of heat which was fatal to all those lower organisms, it would be important to remember that the greatest unanimity of opinion prevailed among almost all experimenters (such as Pasteur, Huxley, Pouchet, Wyman, and others) as to the fact that the lower organisms were killed in fluids heated to 212° F. Knowledge as to the limits of "vital resistance" to heat being declared the necessary starting-point for further investigation, he had made twenty-four experiments at temperatures ranging from 266° to 302° F., and he called particular attention to the fact that in about one-half of these experiments no living things had been obtained from the sealed flasks. His conclusion, therefore, as to the possibility of the *de novo* origin of living matter could not be rebutted by other experimenters who hastily recorded one or two negative results with the view of showing that he had been in error. Three of the most successful of his more recent experiments in which he had resorted to these high temperatures were then recorded. In two of these strong turnip infusions, neutralised by *liquor potassæ*, were employed, one of which was exposed to 266° F. for twenty minutes, and the other to 293° F. for ten minutes. The hermetically sealed flasks and their contents were subsequently kept in a warm place for eight or nine weeks, and they were exposed for several hours daily during eight days to the direct influence of sunlight. Before opening the flasks the vacuum was ascertained to be still preserved. After breaking the necks of the flasks the fluids were found in both cases to have become slightly acid, and to present a somewhat sour odour. On microscopical examination of the fluid *Torulæ* in all stages of development were found in both, and in that which had been exposed to the temperature of 226° F. a considerable number of *Bacteria* were also present. In the third experiment a strong infusion of a common crucifer was made, and the sealed flask into which it was introduced, after having been exposed to the temperature of 266° F. for twenty minutes, was subsequently maintained at a warm temperature, and also subjected to the influence of direct sunlight for a time. The vacuum having been ascertained to be well preserved, the flask was opened at the end of eight weeks, and among the contents of the flask there were found three slowly moving, very minute *Protamœbæ*, and many extremely active tailed *Monads*, in addition to multitudes of *Bacteria* and *Torulæ*. The active tailed *Monads* obtained from this flask were almost immediately exposed in an experimental hotwater oven to a temperature of 140° F. for ten minutes, and the result was that all these *Monads* taken from the hermetically sealed flask which had been heated to 266° F. were killed by the much lower temperature of 140° F. This result was subsequently confirmed by other observations which tended to show that *Monads* were not only killed by a temperature short of that at which water boils, but that they were more or less disintegrated by such an exposure. The experiments, supported as they were by many others of like nature, were, Dr. Bastian contended, of so strict and crucial a nature as to entitle us to believe that living matter might be born *de novo* in solutions, owing to the occurrence of new combinations therein. He further contended that such new-born living matter might, as the experiments tended to show, more or less directly assume the forms of some of the lowest organisms, just as specks of crystalline matter assume those more or less complex shapes which characterise the crystals of various saline substances.

A general discussion then followed on the three papers, and perhaps the most practical contribution to it was furnished by Miss Becker, who said that the question had an important bearing on domestic economy, in relation to the making of preserves and the preservation of jam from mould. She advised the ladies present, when making preserves, to exclude the air before the preserve had cooled. The President afterwards took back the audience to the regions of pure science, and congratulated his hearers that this mort important subject was now attracting the attention of many earnest and philosophical workers.

SUB-SECTION.—ANTHROPOLOGY

On Tuesday, August 8th, the Anthropological section, in consequence of the crowded attendance, moved into the largest lecture hall in the Science and Art Museum. Mr. Kaines read the first paper *On the Anthropology of Auguste Comte*," in which he expounded the views of that author according to the principles laid down by Mr. Darwin He argued that man's intellectual and moral nature, as well as his body, were derived by natural causes, from the lower animals; and he maintained that Auguste Comte's worship of humanity would be the great doctrine of the future. He stated also that there were evidences of man's development furnished by the low condition of the human skulls of the palæolithic age.—Canon Tristram, in the discussion on this paper, granted what Comte and his followers had to say about the physical organism of man being like that of the lower animals, but was there not a metaphysical side to this question which ought to be heard.—Mr. Boyd Dawkins considered that Mr. Haines confounded two different propositions together in his essay. It was assumed, that because man's body was probably derived from that of the lower animals, his mind was equally thus derived. All naturalists were agreed, that man, so far as his body went, was descended from the lower animals; that he was the crown and front of the animal kingdom. But as regards man's mind, and his moral and intellectual faculties, he denied that any evidence whatever had been brought forward to show that their rudiments were to be found in any of the lower animals. The very least that can be done is to wait for more evidence, and the very worst to confound body with mind. He also denied that the palæolithic skulls afforded any trace of a lower state of intellect than at present. The skull of man found in the Dordogne was rather larger than usual; and that of Neanderthal, according to Prof. Huxley, might have enclosed the brain of a philosopher. We had no right to ascribe the actions of the lower animals to the same motives as our own, or to judge of their intellectual faculties by our own standard. On the evidence at present before us we must be content to confess our own ignorance.

On Wednesday, August 9, the following papers were read in the Anthropological department:—W. J. W. Flower commenced *On the Succession of the several Stone Implement Periods in England.* He argued that the two eras, Paleolithic and Neolithic, which had been given to these implements, were not now enough for England, the drift-period being separated from that of the cave, and that again from the tumuli and barrows.—Mr. Pengelly objected to the difference in time being made between the rough and polished flint implements, suggesting that it was probable that men who wished implements for rough and ready purposes, would break them off and form them, and would not go to the trouble of polishing them. He thought the fourfold arrangement of flint implements suggested by Mr. Flower might be convenient, but that at the same time the different kinds might be of contemporary formation.—Col. Lane Fox was inclined to think many of the types were accidental, arising from want of time for the worker to employ his talent. There were two great designs, however; one in which an end is rounded off so that the implement can be used in the hand, the other design being pointed at both ends, so that the implement might be inserted in a haft. As to the duration of the stone period, he thought we required a great deal more investigation and information.—Mr. Prestwich, assuming that the rivers of the flint period were frozen five months of the year, as they were now in Siberia, said some of the rough implements would have been used for cutting holes in the ice, while others would be used for digging roots. Another form, common to a later period, were the scrapers, used for scraping the skins of animals.

A paper was read from the Rev. W. Webster, *On certain Points concerning the Origin and Relations of the Basque Race.* It was in contravention of some ethnological theories propounded by Professor Huxley at a former meeting of the Association. A brief discussion took place, in which it was shown that the Basque language had similar inflections to those of the eastern languages.

Prof. Struthers gave a paper *On Sagittal Synostosis,* which was almost entirely anatomical. A controversy between design and evolution was introduced in the discussion of it.

Prof. McCann read a paper in opposition to *Mr. Darwin's Views on the Moral Sense in the Lower Animals,* maintaining that the moral sense was only implanted in man, and was the result of Divine intuition. A discussion took place on this, in which Prof. Struthers and Mr. W. Goodsir addressed the section.

The business was concluded by the President recapitulating generally the transactions of this department during the meeting. A cordial vote of thanks was passed to Prof. Turner for his services in the chair.

The sub-section was very well attended throughout the meetings, in spite of the desultory nature of the discussions, and the heterogeneous character of the papers, and of the absence of the usual debates.

SCIENTIFIC SERIALS

THE fifth number for the present year of the *Bulletin de l'Académie Royale des Sciences de Belgique* (May 1871) contains several important papers, among which we may particularly notice a Synopsis of the Cordulinæ, by M. E. de Selys Longchamps, and some investigations on the Evolution of the Gregarinæ, by M. E. Van Beneden. The former is a monographic revision of the extensive sub-family of Dragonflies, of which the genius *Cordulia* is the type; it contains a general sketch of the group and its subdivisions, and descriptions of all the known genera and species, with indications of the chief synonymy. This fresh instalment of the author's synopsis of the Dragonflies will be welcomed by entomologists. M. van Beneden's paper is a most valuable contribution to the history of those obscure parasites, the Gregarinæ; his observations were made on an unusually large species, measuring as much as 16 millim. in length, and found in the intestine of the lobster. This species, named by the author *Gregarina gigantea,* is figured in various stages in the plate accompanying the paper.—From M. J. J. d'Omalius d'Halloy we find a short note on the natural forces, in which he argues for the existence of a distinct vital force, and expresses the opinion that the vital force of man differs from that of other living beings—M. F. Duprez discusses the observations on atmospheric electricity made at Ghent, and compares them with those made at other places; and M. de Koninck gives a tabular list of the fossil corals of the Carboniferous formation, showing their distribution in various parts of the world.

THE first publication of the Anthropological Institute has just appeared in the form of a double number of the journal, to which is attached an appendix extending over 160 pages, and containing the proceedings of the Anthropological and Ethnological Societies prior to the date of their union. Amongst the most important papers in the appendix we may mention those on "Some of the Racial Aspects of Music," by Mr. Kaines; on "The Kinnerian and Atlantean Races," by Mr. M'Lean; on "The Concord and Origin of Pronouns, and the Formation of Classes or Genders of Nouns," by Dr. Bleek; on "Some Stone Implements from Africa and Syria," by Sir J. Lubbock; on "The Prehistoric Antiquities of Dartmoor," by Mr. Spence Bate; and on "East African Tribes and Languages," by Dr. Steere. The journal itself contains eight papers, all of considerable value, but amongst which we may especially refer to those by Sir J. Lubbock "On the Development of Relationships;" by Mr. C. Stanisland Wake on "The Mental Characteristics of Primative Man, as exemplified by the Australian Aborigines;" by Dr. Bleek on "The Position of the Australian Languages;" and by Mr. Boyd Dawkins on "The Results obtained by the Settle Cave Exploration Committee out of Victoria Cave in 1870."

No. 3, Vol. I. Ser. 2, of the *Proceedings of the Royal Irish Academy* has just been published. It contains, Science:—J. W. Dawson, LL.D., Addendum to paper on Eozöon; Professors King and Rowney on the Geological Age and Microscopic Structure of the Serpentine Marble or Ophite of Skye, Plate XIV.; also on the Mineral origin of the so-called Eozöon Canadense; Prof. Hennessy, F.R.S., on the Flotation of Sand by the rising tide in a tidal estuary; Dr. J. Purser, Report on the Researches of Prof. Cohnheim on Inflammation and Suppuration; C. R. C. Tichborne, report on the Molecular Dissociation by Heat of Compounds in Solution (abstract). Polite Literature and Antiquities:—H. Stokes, a List of the existing National Monuments of Ireland in the County of Kerry; A. G. More, F.L.S., on an Ancient Bronze Implement found near the Hill of Tara; R. R. Brash on an Ogham Stone at Kelbonane, County of Kerry, Plate XIII.; and an Appendix contains minutes of the proceedings of the Academy.

SOCIETIES AND ACADEMIES

LONDON

Hackney Scientific Association, June 6.—From the report read by the hon. secretary, Mr. H. W. Emons, it appeared that the society had made good progress during the past session, the number of members having more than doubled, and the papers communicated having been both numerous and important. Amongst the actual contributions by members to the progress of science during the session may be mentioned—A Method for ascertaining the Existence of Lunar Activity (Mr. W. R. Birt, F.R.A.S., vice-president), A new variable Star *e* Herculis (Mr. H. T. Vivian), Experiments to ascertain the Absorption of Atmospheric Nitrogen by Plants (Mr. G. E. Davis), A Determination of the Dimensions of the System of Algol (Mr. A. P. Holden), A List of the Fossil Mammalian Remains in the Lea Valley (Mr. R. E. Olliver), A new Section Cutting Apparatus (Mr. W. West), A new classified Catalogue of variable Stars, &c. Numerous original papers had also been read. Good progress had been made with the library, which had been enriched by contributions from several gentlemen, societies, and members. The officers for the fifth session having been elected, the meeting was brought to a close.

PARIS

Académie des Sciences, August 21.—M. Faye in the chair.—M. Chabris has calculated the quantity of nitrate of ammonia yearly carried down to the soil for the nutrition of plants by means of the rain to be two pounds of nitric acid, and consequently, three pounds of nitrate of ammonia per acre.—M. Dumas contributed to the Academy a piece of bread, the provision for the army, which had been infected by *Oidium aurantiacum*. Such facts are not exceptional, principally in very hot weather, and may be detrimental to the public health, as the fungus spreads very rapidly, and it is very difficult to get rid of it. A special committee has been appointed to prevent the infection if possible. MM. Dumas, Baron Larey, Tulasne the botanist, and Pasteur, the celebrated author of so many works on spontaneous generation, are members.—M. Berthelot has examined most carefully a piece of carbon from the Cranbourne meteorite, an Australian stone, and shows by many scientific arguments that the Cranbourne carbon is quite unlike the Orgueil meteorite carbon (a French specimen). The Cranbourne carbon must have been acted upon by a high temperature in ultra-terrestrian space, and no trace of organic origin is to be found on it.—M. W. de Fonvielle sent through M. Leverrier a note establishing that meteoric phenomena analogous to the Marseilles phenomenon are not exceptional cases.—M. Bert, the former Prefect of Lille during the war, described some most interesting experiments on the effects of pressure in suffocating animals living in a confined space. The rapidity of death is not the same for every kind of animal. If the pressure is very high the death is not due to any mechanical effect or to the want of oxygen, but to the presence of carbonic acid, resulting from respiration. It is poisoned by the produce of its own lungs.—The Academy held a secret committee for the nomination of a free member. The list of candidates long delayed was at last published, and M. Belgrand is at the head. But the nomination will be contested.

August 28.—M. Faye in the chair.—M. Saint Venant, a member of the Academy, sent a rather long paper "On the Motion of the Waves," and tried to express through several groups of equations the several motions, which he calls *houles* and *clapotis*, both of which become manifest when the sea is heavy. These new calculations are in some respects grounded on a work published by Gerstner in 1804, "Theorie der Wellen."—Dr. Wurtz, a member of the Academy, sent a paper "On the Action of Chlorine on Aldehyde."—M. Dumas presented, in the name of MM. Montefiore, Levy, and Kunzel, a work entitled "Experiments on Different Alloys, and principally on a Phosphoric Brass, which can be used for casting guns."—M. Jaussen sent to the Academy a complete report "On the Aëronautical Expedition with the *Volta*." The paper is printed in full.—MM. Troost and Hautefeuille sent a very long paper "On Subchlorides and Oxychlorides of Silicon." These chemical researches were executed in the laboratory of the Normal School, and have induced the learned experimenters to explain a new chemical paradox, and to show how it may happen that silicon appears to be volatilised under very curious and peculiar circumstances.—M. Leverrier read an account of several papers sent from Florence by M. D. Muller, relating to several important questions of terrestrial magnetism. In one of these papers the learned physicist explained how a very large perturbation was observed at the very moment when the sun and the moon came into contact on December 22, 1870, and ceased just when the two discs were separated. M. Muller was one of the Italian eclipse party sent to Terra Nova (Sicily). The view of the eclipse was lost, but a most interesting fact was witnessed. The Italian Government will very shortly issue a special publication on this unexpected phenomenon. It is to be noted, moreover, that the total amount of ecliptic perturbation was diminished in proportion to the distance from the central line of total obscurity.—At the secret committee which followed M. Belgrand was elected a free member.

NEW ZEALAND

Wellington Philosophical Society, July 1.—The president, W. T. Locke Travers, F.L.S., in his address, dwelt on the rapid extinction of the interesting subalpine vegetation of New Zealand, and stated that in a few years many plants that were not rare when he first botanised the Nelson Mountains would soon only be found in herbaria. Mr. Buchanan described the following addition to the flora: *Haloragis aggregata*, *Celmesia laterale*, *Acæna glabra*, *Rosthovia Novæ Zælandiæ*, *Danthonia monoica*, and subspecies of *Danthonia semi-annularis* and *Carex pyrania*. Dr. Knox gave the results of the dissection of the supposed Native Rat, and showed that it could not be distinguished from *Mus Rattus*. Mr. Skey announced the isolation of the bitter principle of the kernel of the Karaka berry (*Corynocarpus lævigata*) as a non-nitrogenous crystallisable resin similar to Digitaline. He proposed to name it Karakine. Dr. Hector exhibited the neck of a Moa with the skin, feathers, and tissues attached, and pointed out the similarity of the feathers to those of the Emu, while they differed from the Kiwi; remarking that *Apteryx Mantelli* has the feather shafts prolonged, giving the skin a harsh bristly feel, which distinguishes it from *A. Australis*. He showed a fine series of models of Moa's eggs he had prepared for comparison with the cast of the Æpyornis egg recently received. By a series of specimens which he had obtained alive from the natives and afterwards dissected, Dr. Hector showed that the difference between *Glaucopsis Wilsoni* and *G. olivascens* are merely sexual. Captain Hutton described the following additions to the birds of New Zealand: *Hydrochelidon leucoptera*, Temm, *Procellaria fuliginosa*, *Thalassidroma marina*, *Strepsilas interpres*, and a Totanus and Laurus that have not yet been determined. He also showed evidence of the existence in New Zealand of a goose allied to the Bean goose of Europe, and stated that it had been surmised by Dr. Finsch in a paper published last year that two of the above—Strepsilas and Totanus—would probably be found in New Zealand, and also an Actitis, which has not yet, however, been obtained.

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ERRATUM.—Vol. IV., p. 358, second column, line 20 from bottom, for "1870" read "1770."

NOTICE

We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception.