THURSDAY, DECEMBER 21, 1871

THE COPLEY MEDALIST OF 1870

THIRTY years ago Electro-magnetism was looked to as a motive power which might possibly compete with steam. In centres of industry, such as Manchester, attempts to investigate and apply this power were numerous, as shown by the scientific literature of the time. Among others Mr. James Prescot Joule, a resident of Manchester, took up the subject, and in a series of papers published in Sturgeon's "Annals of Electricity" between 1839 and 1841, described various attempts at the construction and perfection of electro-magnetic engines. The spirit in which Mr. Joule pursued these inquiries is revealed in the following extract: "I am particularly anxious," he says, "to communicate any new arrangement in order, if possible, to forestal the monopolising designs of those who seem to regard this most interesting subject merely in the light of pecuniary speculation." He was naturally led to investigate the laws of electro-magnetic attractions, and in 1840 he announced the important principle that the attractive force exerted by two electro-magnets, or by an electro-magnet and a mass of annealed iron, is directly proportional to the square of the strength of the magnetising current; while the attraction exerted between an electro-magnet and the pole of a permanent steel magnet varies simply as the strength of the current. These investigations were conducted independently of, though a little subsequently to, the celebrated inquiries of Henry, Jacobi, and Lenz and Jacobi on the same subject.

On the 17th of December, 1840, Mr. Joule communicated to the Royal Society a paper on the production of heat by Voltaic electricity; in which he announced the law that the calorific effects of equal quantities of transmitted electricity are proportional to the resistance overcome by the current, whatever may be the length, thickness, shape, or character of the metal which closes the circuit; and also proportional to the square of the quantity of transmitted electricity. This is a law of primary importance. In another paper, presented to but declined by the Royal Society, he confirmed this law by new experiments, and materially extended it. He also executed experiments on the heat consequent on the passage of Voltaic electricity through electrolytes, and found in all cases that the heat evolved by the proper action of any Voltaic current is proportional to the square of the intensity of that current multiplied by the resistance to conduction which it experiences. From this law he deduced a number of conclusions of the highest importance to electro-chemistry.

It was during these inquiries, which are marked throughout by rare sagacity and originality, that the great idea of establishing quantitative relations between Mechanical Energy and Heat arose and assumed definite form in his mind. In 1843 Mr. Joule read before the meeting of the British Association at Cork a paper "On the Calorific Effects of Magneto-Electricity and on the Mechanical Value of Heat." Even at the present day this memoir is tough reading, and at the time it was written it must have appeared hopelessly entangled. This I should think was the reason why Prof. Faraday advised Mr. Joule not to submit the paper to the Royal Society. But its drift and results are summed up in these memorable words by its author, written some time subsequently: "In that paper it was demonstrated experimentally that the mechanical power exerted in turning a magneto-electric machine is converted into the heat evolved by the passage of the currents of induction through its coils, and on the other hand, that the motive power of the electro-magnetic engine is obtained at the expense of the heat due to the chemical reaction of the battery by which it is worked."* It is needless to dwell upon the weight and importance of this statement.

Considering the imperfections incidental to a first determination, it is not surprising that the "mechanical values of heat," deduced from the different series of experiments published in 1843, varied somewhat widely from each other. The lowest limit was 587, and the highest 1,026 foot-pounds for 1° F. of temperature.

One noteworthy result of his inquiries, which was pointed out at the time by Mr. Joule, had reference to the exceedingly small fraction of the heat which is actually converted into useful effect in the steam-engine. The thoughts of the celebrated Julius Robert Mayer, who was then engaged in Germany upon the same question, had moved independently in the same groove; but to his labours due reference will doubtless be made on a future occasion. In the memoir now referred to Mr. Joule also announced that he had proved heat to be evolved during the passage of water through narrow tubes; and he deduced from these experiments an equivalent of 770 foot-pounds, a figure remarkably near to the one now accepted. A detached statement regarding the origin and convertibility of animal heat strikingly illustrates the penetration of Mr. Joule and his mastery of principles at the period now referred to. A friend had mentioned to him Haller's hypothesis, that animal heat might arise from the friction of the blood in the veins and arteries. "It is unquestionable," writes Mr. Joule, "that heat is produced by such friction, but it must be understood that the mechanical force expended in the friction is a part of the force of affinity which causes the venous blood to unite with oxygen, so that the whole heat of the system must still be referred to the chemical changes. But if the animal were engaged in turning a piece of machinery, or in ascending a mountain, I apprehend that in proportion to the muscular effort put forth for the purpose, a *diminution* of the heat evolved in the system by a given chemical action would be experienced." The italics in this memorable passage, written it is to be remembered in 1843, are Mr. Joule's own.

The concluding paragraph of this British Association paper equally illustrates his insight and precision regarding the nature of chemical and latent heat. "I had," he writes, "endeavoured to prove that when two atoms combine together, the heat evolved is exactly that which would have been evolved by the electrical current due to the chemical action taking place, and is therefore proportional to the intensity of the chemical force causing the atoms to combine. I now venture to state more explicitly, that it is not precisely the attraction of affinity, but rather the

* Phil. Mag. May 1845.

mechanical force expended by the atoms in falling towards one another, which determines the intensity of the current, and, consequently, the quantity of heat evolved; so that we have a simple hypothesis by which we may explain why heat is evolved so freely in the combination of gases, and by which indeed we may account 'latent heat' as a mechanical power prepared for action as a watch-spring is when wound up. Suppose, for the sake of illustration, that 8 lbs. of oxygen and 1 lb. of hydrogen were presented to one another in the gaseous state, and then exploded; the heat evolved would be about 1° F. in 60,000 lbs. of water, indicating a mechanical force expended in the combination equal to a weight of about 50,000,000 lbs. raised to the height of one foot. Now if the oxygen and hydrogen could be presented to each other in a liquid state, the heat of combination would be less than before, because the atoms in combining would fall through less space." No words of mine are needed to point out the commanding grasp of molecular physics, in their relation to the mechanical theory of heat, implied by this statement.

Perfectly assured of the importance of the principle which his experiments aimed at establishing, Mr. Joule did not rest content with results presenting such discrepancies as those above referred to. He resorted in 1844 to entirely new methods, and made elaborate experiments on the thermal changes produced in air during its expansion: firstly, against a pressure, and therefore performing work; secondly, against no pressure, and therefore performing no work. He thus established anew the relation between the heat consumed and the work done. From five different series of experiments he deduced five different mechanical equivalents; the agreement between them being far greater than that attained in his first experiments. The mean of them was 802 foot-pounds. From experiments with water agitated by a paddle-wheel, he deduced, in 1845, an equivalent of 890 foot-pounds. In 1847 he again operated upon water and sperm-oil, agitated them by a paddle-wheel, determined their elevation of temperature, and the mechanical power which produced it. From the one he derived an equivalent of 781·5 foot-pounds; from the other an equivalent of 782·1 foot-pounds. The mean of these two very close determinations is 781·8 foot-pounds.

At this time the labours of the previous ten years had made Mr. Joule completely master of the conditions essential to accuracy and success. Bringing his ripened experience to bear upon the subject, he executed in 1849 a series of 40 experiments on the friction of water, 50 experiments on the friction of mercury, and 20 experiments on the friction of plates of cast-iron. He deduced from these experiments our present mechanical equivalent of heat, justly recognised all over the world as "Joule's equivalent."

There are labours so great and so pregnant in consequences, that they are most highly praised when they are most simply stated. Such are the labours of Mr. Joule. They constitute the experimental foundation of a principle of incalculable moment, not only to the practice, but still more to the philosophy of Science. Since the days of Newton, nothing more important than the theory of which Mr. Joule is the experimental demonstrator has been enunciated.

I have omitted all reference to the numerous minor papers with which Mr. Joule has enriched scientific literature. Nor have I alluded to the important investigations which he has conducted jointly with Sir William Thomson. But sufficient, I think, has been here said to show that, in conferring upon Mr. Joule the highest honour of the Royal Society, the Council paid to genius not only a well-won tribute, but one which had been fairly earned twenty years previously.*

Comparing this brief history with that of the Copley Medalist of 1871, the differentiating influence of "environment" on two minds of similar natural cast and endowment comes up in an instructive manner. Withdrawn from mechanical appliances, Mayer fell back upon reflection, selecting with marvellous sagacity from existing physical data the single result on which could be founded a calculation of the mechanical equivalent of heat. In the midst of mechanical appliances, Joule resorted to experiment, and laid the broad and firm foundation which has secured for the mechanical theory the acceptance it now enjoys. A great portion of Joule's time was occupied in actual manipulation; freed from this, Mayer had time to follow the theory into its most abstruse and impressive applications. With their places reversed, however, Joule might have become Mayer, and Mayer might have become Joule.

JOHN TYNDALL

THE BROWN INSTITUTION

IN 1852 a large sum of money was bequeathed by the late Mr. Thomas Brown to the University of London for the purpose of "founding and upholding" an Institution for "investigating, studying, and if possible endeavouring to cure" the diseases and injuries of animals useful to man. The sum was to be allowed to accumulate for a limited period, at the end of which the principal and interest were to be applied in the manner directed. And it was provided that in case the University should fail to carry out the trust imposed upon it within nineteen years after the testator's death, the whole sum with the accumulations should be transferred to the University of Dublin, to be applied for the endowment of certain philological professorships. The will contains various directions for the administration of the proposed Institution. The most important are those which relate to the appointment of a Committee of Management and of a Professor. The committee must be appointed by the governing body of the University, and must consist either of members of the Senate or of other persons, members of the medical profession. As regards the qualifications of the professor nothing is said. He must be appointed by the University, must give a course of lectures annually, and must have a residence adjacent to the Institution.

The nineteen years have now almost expired. In pursuance of the testator's directions, the "Brown Institution" has just been opened. Last summer a large plot of freehold land was acquired by the University in the Wandsworth Road, close to the goods station of the South-

* Had I found it in time, this notice should have preceded that of the Copley Medalist of 1871.

Western Railway. On this ground a Hospital for Animals has been built, consisting of stables for the reception of the larger quadrupeds, and of houses of various descriptions for those of a smaller size. All of these buildings are constructed in the best style, with a view to the well-being of the creatures they are destined to contain, being thoroughly drained, paved, and ventilated, and warmed with hot-water pipes. Adjoining them there is a spacious exercise ground.

As many of our readers already know, the Senate have placed the Institution under the management of Dr. Burdon Sanderson, of University College, London, who, as Professor, will, in future, deliver the annual course of lectures.

If the scope and purpose of the Brown Institution were limited to the care and cure of diseased animals, its establishment would scarcely be worthy of record in the pages of NATURE, for, however desirable it may be that the animals that serve us should be kindly and skilfully treated when they are sick, the object has so remote a relation to the promotion of physical science that our readers could not be expected to take any special interest in it. But, happily alike for humanity and for science, the late Mr. Brown showed by his selection of persons to be entrusted with the carrying out of his intentions, by the instructions contained in his will for their guidance, and by the terms in which he defined the purposes of the proposed Institution — placing study and investigation first, cure afterwards—that he was not actuated by a mere sentimental sympathy for the lower animals as such, but that he desired, by promoting the scientific study of their diseases, to benefit mankind.

As might have been expected, the Senate of the University of London have not only fulfilled the letter of the testator's dispositions, but have proved by the manner in which they have done so, that they are actuated by the same noble purpose. They have shown this first of all in their selection of a Committee of Directors. What could be a better guarantee for the future good administration of the Institution than the fact that among its directors are to be found such men as Busk, Carpenter, Gull, Paget, Quain, Sharpey, Sibson, and Simon, men eminent as physiologists, pathologists, or clinical teachers; of each of whom it may be said that he has contributed a large proportion to the total amount of work done in his own branch of science in England during the past thirty years. We do not think that it would have been possible, even if their choice had been perfectly unlimited, to have selected persons more fitted for the purpose, whether as regards personal character or scientific attainments.

Under the direction of Dr. Sanderson, a laboratory intended, to quote the terms of the will, "for the study and investigation of disease," has been built on the ground already referred to at Vauxhall, adjoining the hospital for animals. The laboratory consists of four admirably-lighted and spacious working rooms, connected by a corridor. Underneath these are four other rooms, which, although not so lofty, are also well adapted for many kinds of research. In the same building is included a stable for the reception of animals intended to be the subjects of special observation.

In the work of the laboratory the Committee of Direction have most wisely associated with Dr. Sanderson, under the title of Assistant Professor, Dr. E. Klein, whose name is well known as the contributor of valuable articles to Stricker's "Histology," and of several important embryological researches. Well trained as a pupil of Brücke and Stricker in the methods of research, whether physical, chemical, or microscopical, young in years though old in accomplished work, Dr. Klein is singularly fitted for the post. Dr. Sanderson is much to be congratulated in having so able a coadjutor.

It may not be out of place if we attempt to give our readers an idea of the work which we suppose will be done or attempted in the laboratory of the Brown Institution.

The facts on which the science of disease, so far as it may as yet be called a science, is founded, are gathered from two sources, the bedside and the laboratory. In clinical studies the same, or even greater, exactitude is required as in those of the physicist or chemist; but even when they are conducted in the wards of a hospital, the Harveian method of "searching out the secrets of nature by way of experiment," can only be applied under limitations which very materially embarrass the inquiry. The pathologist at the bedside is not in the position of an experimenter, but only in that of a student, who stands by at a greater or less distance, while another, over whom he has no control, performs experiments in his presence, without deigning to explain to him their nature or purpose. The true physician fears to meddle with the processes of which he is the attentive and anxious spectator. Although the more ignorant members of the medical craft—the so-called "practical" men—may sometimes, with the best intentions, experiment on their patients with harmful drugs, such experimentation is repudiated by the man of science.

There are, however, many questions relating to disease, of the most profound importance to the human race, which cannot be solved, and never will be solved, by thus, as it were, standing on one side and watching what goes on at a distance; such questions, for example, as the nature of contagion, and those which relate to the origin and proximate causes of our most common diseases, such as inflammation, fever, and tubercle. The knowledge which has been acquired on these subjects during the last few years has been gained by work done in laboratories. The advantages of this mode of inquiry, as compared with the indirect clinical method, are of two kinds—the one relating to the objects of observation, the other to the means which are at the disposal of the inquirer. In dealing with animals, he is embarrassed by scarcely any of the limitations which render clinical observation so difficult. The very considerations, indeed, which in the case of man, absolutely forbid his entertaining any other purpose excepting that of prolonging life and alleviating pain, not only allow, but encourage him, in the case of animals, to disregard altogether the present suffering for the future benefit. We are clearly justified in profiting by the sufferings of the lower animals for man's sake. We may subject them experimentally to the action of remedies without any immediate view to their being thereby benefited. We may place them under conditions which we know will produce disease, for the purpose of studying the mode of action of those conditions. We have at least as good a right to kill sick animals for the purpose of investigating the anatomical changes produced by disease, as to slaughter healthy animals for food. And even if in the pursuit of our inquiries we are compelled to inflict pain, we are perfectly right in doing so—provided that truths valuable to humanity are to be learnt by it.

The other respect in which the comparative pathologist has an advantage over the clinicist, lies in the choice of means. It is true that during the last few years much progress has been made in the application of instruments of precision to the investigation even of human diseases; but, after all, there are few of those instruments which are really valuable. In the case of animals it is entirely different. The microscope may be applied to the investigation of tissues unaltered by those changes which speedily follow the extinction of life. The measurement of the temperature of the body, whether with relation to the changes which it undergoes in disease, or to the differences between diseased and healthy parts, can be performed in animals with all the exactitude which such investigations require—in man such exactitude is impossible, because the conditions of observation cannot be controlled. Instruments of precision may be used for the investigation of the changes which disease produces in the mechanical functions of respiration and circulation, which, for reasons already adverted to, could not be applied in the sick room, or in the wards of a hospital—and if they were applied, would yield no satisfactory results.

Again, in animals it is possible to apply the ordinary methods of chemistry to investigate the modifications produced by disease in the process of nutrition; whereas in man this is attended with such insuperable difficulties, that it may be regarded as impossible.

Many other similar examples might be mentioned; but these may serve to explain the way in which we hope to see the new laboratory at Vauxhall brought into relation with the hospital for sick animals. Believing that the study of pathology, like that of physiology, of which it forms part, can only be successfully prosecuted by observing the operation of chemical and physical laws in the living diseased body, and applying the same methods as are used by the chemist and physicist to their investigation, and that the more this principle is acted on, the more rapid and solid will be the progress made, we regard the establishment of the Brown Institution as an important step in the right direction. We should have been still better pleased if it had been a laboratory of physiology, for this ought to have preceded the other. We think it, however, not unlikely that it may, by setting an example of good work, exercise a considerable indirect influence in the promotion of physiological studies in this country.

We must not omit to mention that although the laboratory is intended for research rather than for instruction, it will be open to those who may wish to engage on their own account in scientific inquiries. The only condition imposed by the directors on those who desire admission to the laboratory as workers, is the possession of "previous scientific training." Each worker will have to defray the expenses of material, but no other payment will be required of him. It is understood that the laboratory will be opened on the 1st of January, 1872.

FOREIGN YEAR-BOOKS

Jahrbuch der Erfindungen. Herausgegeben von H. Hirzel und H. Gretschel. Sechster Jahrgang. (Leipzig: Quandt und Händel; London: Williams and Norgate, 1870; pp. 472.)

THE sixth volume of this series fully sustains the high character achieved by its predecessors. Astronomy, physics and meteorology, mechanics and mechanical technology, and chemistry and chemical technology form the subjects of the respective chapters.

We cannot open any part of the work without observing the care with which it is edited. We shall select for special notice the latter part of the chapter on chemistry, which treats of organic compounds, beginning with the following paragraph upon the products of oxidation of paraffin. After describing the recent improvements introduced by Hübner in the preparation of this substance from coal-tar, and in its mode of purification, and noticing its remarkable stability (it being unaffected by concentrated hydrochloric or sulphuric acids, and by the alkalies), the reporters state that there are certain oxidising agents, and especially chromic and nitric acids, which it is unable to resist. Gill and Meusel have studied the action of these reagents on paraffin, and have arrived at the following results:—

"The paraffin in common use fuses at 56° C., and by repeated crystallisation from sulphide of carbon the fusing point may be raised to 60° and upwards. If we boil from 300 to 500 grammes of pure paraffin with 120 grs. of bichromate of potash, and 180 grs. with sulphuric acid diluted with twice its volume of water for three or four days in a glass retort till the chromic acid is completely reduced to chrome-oxide, acetic acid and other acids of the same series, and principally cerotic acid, are formed; the latter being a white solid substance that does not fuse at a lower point than 78° C., and also occurs as a main constituent of bees'-wax. If we boil paraffin continuously with five or six times its volume of nitric acid of 1·3 sp. gr., which has been previously diluted with $1\frac{1}{2}$ times its volume of water, we likewise obtain cerotic acid, in addition to acetic, butyric, valerianic, and succinic acids, and other products" (p. 261).

Passing over a section on "Fats, fatty oils, and allied substances, and the products of their decomposition," in which is a notice of the explosive compounds derived from glycerine, we come to one treating of "Resins," in which there is a notice of Puscher's interesting and highly-practical communication on shellac-ammonia solutions. Perhaps the most valuable of the applications of these solutions is their property of dissolving certain of the aniline colours, as aniline green, aniline yellow, and fuchsine.

The organic non-nitrogenous acids, the carbo-hydrates, alcohol and its products, the albuminous bodies and their allies, newly-discovered organic bases, pigments and pigment-yielding bodies, both natural and artificial, nutritious matters, and disinfectants, are all duly considered. The report on artificial pigments is especially deserving of commendation. It consists of nearly fifty pages full of practical matter, and, taken in conjunction with a previous report that appeared in the second volume (for 1866), forms the most complete summary of this important department of practical chemistry, that, taking its limits into consideration, we are acquainted with.

As usual the volume concludes with a necrology for the past year.

OUR BOOK SHELF

Marvels of Pond-Life: or a Year's Microscopic Recreations among the Polyps, Infusoria, Rotifers, Water-Bears, and Polyzoa. By Henry J. Slack, F.G.S., &c. Second Edition. (London: Groombridge and Sons.)

THIS little volume is already so well and favourably known to microscopists that any formal notice or commendation is scarcely necessary. Professing only to be a first book on "Pond-Life," it does not attempt more than to guide the young student in searching after, collecting, and examining the various animal organisms which inhabit fresh water. The division into months indicates that it is also popular rather than abstruse, and the number of species mentioned or figured is very limited. There appears to be no good reason why the present edition should not have made an advance beyond its predecessor, and given us an additional chapter or two on the construction and management of small aquaria at home, adapted especially and entirely to minute pond-life, by means of which metropolitan students might continue the study when unable to go to the ponds; and also on those artificial ponds for the evolution of Infusoria, so much alluded to of late, infusions of organic substances. Keeping in view the simple pretensions and elementary character of this volume, it fully answers the design of its author, and we are glad to announce the appearance of a second edition.

Physikalisches Repetitorium, &c., &c. Von Dr. Ferdinand Bothe. Second Edition, revised and enlarged. (Brunswick: Vieweg, 1871.)

A BRIEF enumeration of the more prominent facts and formulæ of physics; carefully divided into subjects, and with occasional dates and names of inventors or discoverers. We conceive that to make an excellent work of this kind (if such a thing be at all desirable), all that is necessary, is to take a really good treatise on natural philosophy and construct something between an Index to, and an Abstract of, its contents. It seems probable that some such process has been employed by Dr. Bothe; but either he cannot have used a trustworthy book for analysis, or his analysis is not a faithful one. In fact, if we look on it seriously, a more painful volume we have not often met with; nor a more amusing one, if we could fancy its blunders intended to amuse. We simply open its pages at hazard, and make a few pickings:—

"64. The density and resilience (*Spannkraft*) increase in proportion to the pressure, the volume is inversely as the pressure, and *vice versâ*—Boyle's or Mariotte's Law, 1679." James Bernoulli was a contemporary of these men, and says in his work, "De Gravitate Aetheris," "Veritas utriusque hujus regulæ manifesta fit duobus curiosis experimentis ab Illustr. Dn. Boylio hanc in rem factis, quæ videfis [*sic*] in Tractatu ejus contra Linum." The date of this tract of Boyle's is 1662, and it is to be observed that Bernoulli does not mention Mariotte at all. We notice, in passing, that Young's name is not mentioned under Capillarity, and we arrive at the following curiosity:—"140. Unit of momentum or of work (*Arbeit*) is the force (*Kraft*) which can in one second communicate to unit of weight a velocity of unit of length. (Its) metrical measure is the kilogramme-mètre; in Prussia, England, &c., the foot-pound." But we beg Dr. Bothe's pardon. We had no right to render *Arbeit* by "work," which is its usual equivalent in scientific books; for looking back we find:—"129. The product of the weight of a body into its velocity is called Momentum, and also *Arbeit*"! It is scarcely possible to conceive a more hopeless jumble of essentially different things than these sentences exhibit. The Heliotrope is (468) ascribed to Gauss, 1830 (?). Did not Drummond use it in 1826? 471 gives Bunsen and Kirchhoff the credit of the spectroscope, with its collimator, &c. What of Swan? As to the equality of absorption and radiation, Angström is given without date, Stokes and Balfour Stewart not mentioned. "472. The planets and comets (!) send back only the rays which the sun has sent to them." 484. In the enumeration of the earliest attempts to produce photographic impressions, there is no mention of Wedgwood, &c. 558. No mention is made of Northmore, whose long priority in the liquefaction of gases was insisted on by Faraday. 592. The old story of Mayer and the dynamical theory of heat. His date is given as 1842; Davy and Rumford (who did all that is referred to in the text more than forty years before) are not mentioned. Joule is coupled with Clausius, and the date 1853 is assigned to them! Of Carnot, Colding, Rankine, Thomson, &c., not a word. 598-600. The experimental laws of heat of combination are very imperfectly given, and, without any mention of Andrews and Hess, handed to Thomsen and Favre and Silbermann, with the date 1853! 666. The similarity of the order of bodies considered separately as conductors of heat and electricity is given to Wiedemann and Franz in the same prolific year. Surely Forbes pointed it out twenty years earlier! So far as we have seen, Sir W. Thomson, Clerk-Maxwell, &c, are not even named in the book.

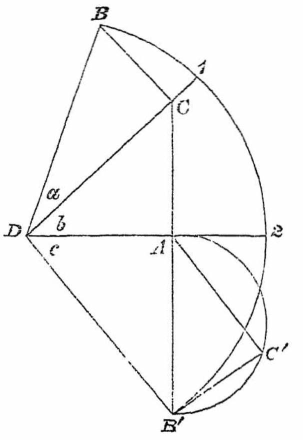
If the reader remember that these are merely the things which have caught our eye in turning over the pages at random, he will not blame us for absolutely declining to examine the work more closely. A series of working tables is appended, but without very close examination we should hesitate to trust them, after what we have seen of the character of the book. That we have noticed it at all is due to the circumstance that some consolation is to be derived from the mere fact of its existence. We are all (in consequence, perhaps, of recent events) more or less imbued with the notion that Germany (Prussia especially) is rapidly taking the lead in matters of scientific education and investigation; and no doubt there is some truth in this. But the game is not lost, we are not yet passed in the race, and our old supremacy is quite within our reach even now, provided we make speedy and sufficient exertions to regain and maintain it. It will not drop into our mouths for a mere wish; but is it reasonable to wonder at the state of science in this country, where so few statesmen pay the least attention to it, when we find that even in enlightened Prussia, such a book as the above can be written by a recognised teacher, and published in a second edition by one of the highest firms in the world?

LETTERS TO THE EDITOR

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

Proof of Napier's Rules

I AM greatly obliged to "J. J. W." for pointing out the objection of a want of generality in the construction of the figure contained in my former letter (in NATURE, No. 106), for the proof of Napier's Rules; which the more general construction now described by "J. J. W." most simply and most effectually removes. To illustrate his more perfect general construction with a figure—D is the centre, and B12B′ a part of the circumference of a circular piece of cardboard, upon which the arcs B1, 12 are taken equal to the sides of the right-angled spherical triangle which it is required to represent. If we join DB, D1, D2, and draw BC, CA perpendicular to D1, D2, the latter perpendicular prolonged meeting the circle of the circumference in B′, and join DB′; and on AB′ as diameter describe the semicircle AC′B′; and with the centre A, and radius AC, another circle, meeting the semicircle in C′, so that the straight line AC′ is equal to AC; and join B′C′. Then it is easily shown that if AC CB are the two sides, AB′ is the hypothenuse of a right-angled triangle, which, when the four triangles are closed together so as to form a solid figure, will coincide with the triangle AC′B′. As BC (or B′C′) will then be perpendicular both to CD and to CA (or C′A), it will be perpendicular to the plane DCA; and the arc B1, which is in the same plane with it, will be at right angles to the arc 12. The third arc 2B′ will therefore be the hypothenuse of a right-angled spherical triangle, of which B1, 12, are the two sides. Calling these arcs or the angles of the faces resented by them, a, b, c, and the angles opposite to them in the spherical triangle, A, B, C, the proof of Napier's Rules, with this solid figure, proceeds by the same direct steps as those already described, with a special example of the figure in my former letter. As the construction there described is confined to the representation of a particular kind of right-angled spherical triangle, and is therefore inapplicable to illustrate the proof of



Napier's Rules experimentally in every given case, the general construction supplied by "J. J. W.," which is limited by no such restrictions, and which is at least equally convenient, will evidently serve more effectively the same practically useful and instructive purpose.

Instead of "accessible," as applied to the difficulties of the geometrical proofs produced by Mr. Cooley in his letter on "Elementary Geometry" (in NATURE, No. 103), which are indeed there obviously overcome, I would have used the word "surmountable" as more descriptive of geometrical difficulties, properly treated and discussed, had the word immediately presented itself to me; but having often found an easily executed model extremely useful and convenient in practical applications of Napier's Rules, with whose design, as a general resource to facilitate their study, I was not, however, so fully satisfied, I applied, perhaps unconsciously, to Mr. Cooley's demonstrations a term expressing strictly only the diffidence with which I ventured to present to readers of NATURE my own very imperfect geometrical contrivance. In thus making my difficulties accessible to "J. J. W.," I very gratefully acknowledge the assistance which I have derived from his remarks on my letter in NATURE, No. iii., and I cheerfully admit the merit and superiority of the general rule for constructing a proper model in cardboard, to illustrate the proofs of Napier's Rules, and to facilitate their study, which he has kindly consented to describe.

Newcastle-on-Tyne, Dec. 16 A. S. HERSCHEL

Alternation of Generations in Fungi

I AM sure that the Rev. M. J. Berkeley will exonerate me from any deliberate intention to misrepresent him; nor do I think that there is, after all, much difference of opinion between us regarding the present subject, unless, perhaps, that I am more sceptical. I alluded to the paper cited by him from the "Journal of the Horticultural Society," on propagation of bunt spores, and not to his communications on the hop or vine mildew. I was under the impression that he regarded the "four consecutive forms of reproductive cells in the bunt" as an instance of alternation of generations. On reference to the original paper, I find that he did not go so far then as to indicate four consecutive forms of reproductive cells; but that Tulasne followed on his track in 1854, and in 1857 Mr. Berkeley seemed to have accepted the results of Tulasne's observations, since, in his "Introduction," he gives figures at page 318, in the description of which the following phrases occur:—"spores of the second order," "spores of the third order," "spores of the fourth order." Here are the "four consecutive forms of reproductive cells" to which I alluded. At page 321 he writes concerning the bunt:—"The spores, however, are not immediate means of propagation; they are, in fact, only a sort of prothallus, from which the mycelium grows, producing at the tips, or on lateral branchlets, bodies of various forms, which are themselves capable of germination, and immediately reproduce the species." The real issue between us seems to lie in the phrase, "alternation of generations." If the bunt spores, on germination, produce fusiform bodies, which, after conjugation, produce short cylindrical spores, and thus intermediate reproductive cells unlike the parent cell come between that and the ultimate reproduction of the species, I am induced to call it an "alternation of generations." It would be waste of time to discuss phrases, or I might take exception to the application of this phrase to the *Erysiphei*. The conidia and pycnidia of the hop mildew may be developed without sporangial conceptacles, and the parasite reproduced without sporangial fruit, but I cannot recognise alternation of generations in the reproduction of a species by means of conidia, stylospores, or sporidia, or by one of these alone. If such may be construed into an alternation of generations, it must be by permitting greater elasticity to the phrase. Conidia germinating and producing pycnidia, the stylospores of the pycnidia germinating and producing sporangial conceptacles, containing the sporidia which, upon germination, will produce the mycelium and conidia again, returning to the original form after two or three consecutive departures from it, appears to me a perfect type of alternation of generations. I fully admit that "if it is once established that a Puccinia produces an Æcidium, or an Æcidium a Puccinia, we should have a clear case, especially when the third form reverts to the first again." Without the slightest desire to "depreciate the labours of Oersted and De Bary," I cannot admit that they have established facts until their observations are confirmed, especially when there is an evident possibility of their having been deceived. I shall have no hesitation in accepting the facts when they are confirmed by independent and equally trustworthy observers, although I may be unable to account for some of the phenomena. At present I must confess that I am not so sanguine as Mr. Berkeley appears to be.

The correspondent signing himself "Mycelium" wishes to know if "the liability to produce parasitic fungi is communicated from the seed to the mature plant." In some instances we know such to be the case, in others perhaps only suspect it. The "bunt" is an instance, or why the steeping of seed corn? or how did the Rev. M. J. Berkeley succeed in producing bunted wheat plants from seed corn inoculated with bunt spores? Two or three years since I published particulars of a similar instance of celery seed and *Puccinia Apii*. It would be as rash to affirm that this is always the case as to deny its occurring at all.

M. C. COOKE

In Re Fungi

THE letters in your last two numbers have reminded me how ill this subject is studied by some botanists in this country. I will give two recent instances: 1. In the last number of the *Journal of Botany*, p. 383, it is positively stated that *Agaricus cartilagineus* (a rare and very critical species by the way) was determined by a growth which is there described a mere *mass of mycelium*. He must have been a bold man who ventured to name an agaric (above all things) from a *mass of mycelium*. 2. In the first number, October 1871, of the *new edition* of "Paxton's Botanical Dictionary"—"enlarged and revised"—under the article *Agaricus* there is to be found such a collection of obsolete names and absurd errors as to make the article simply ridiculous. W. G. S.

Mr. Lowne and Darwinian Difficulties

MR. LOWNE (NATURE, December 7) sees no difficulty whatever in explaining by what natural process an insect with a suctorial mouth is developed from one having the mandibular type of mouth, but still he does not explain. He affirms there is no doubt that "the pupa state is a modification (!) of the ordinary process of skin shedding," and that this is "proved" by so many facts that he cannot understand how it could be "denied," &c., but he does not prove it.

For aught I can tell, every internal tissue and every external scale of the butterfly may be represented in the larva; but I do not know and cannot prove that this is so, nor do I believe any one can prove it. That the changes which take place during the pupa state are very different from those that occur during any portion of the larva period, will be admitted by every one who has kept silkworms or bred butterflies. The assertion that there is absolutely only a difference in the time at which the successive skins are formed in this and in ordinary ecdysis, is but assertion on the part of Mr. Lowne. Indeed, controversy becomes profitless if authority is to be substituted for fact, and an attempt made to silence opponents and stop inquiry by such positive assertions as the above and the following :—"The imaginal skin is likewise derived from cells laid down in contact with the imaginal discs." If Mr. Lowne will be so good as to explain what no books tell me, and I fail to make out myself, I will study what he says with great attention, and thank him heartily. He knows me well enough to feel assured that I would do so ; but it is useless, and he must permit me to say that it is not in good taste, for him to comment about the "return of darkness," and to use expressions more positive and arbitrary than are called for.

Let us, if we can, get at the facts concerning some of these marvellous changes. For this there is nothing like discussion, carried on with care and consideration, even for an opponent ; and though the fittest may be certain that he will survive, don't let any one be in too great haste to proclaim himself either survivor or fittest, or call himself strong and others weak, as has been done once already by one distinguished evolutionist. Evolution is a much quieter and far more complex process than some enthusiasts would have us believe.

Mr. Lowne appeals to the fly. By all means let the fly be the subject of our inquiries. Of this creature he says, the nervous system undergoes *modification* but not *degeneration*. Now I ask, what part of the nervous system that is present in the maggot can Mr. Lowne find in the fly? I have studied both fly and maggot carefully, have worked at the matter long, and have utterly failed to find a trace of the nerve tissue of the maggot in the fly. Not only so, but I find the nerves of the fly as different as are the muscles from those of the maggot. The latter are altogether distinct in structure and in action. They contract at a very different rate, and are very different in many particulars.

Again, I must ask Mr. Lowne if he has seen any vestige of the mouth organs in the larva, for he says :—"It is the mouth organs of the larva which are new formations, not those of the imago." I have failed in my attempts to find any traces. There are other assertions about the alimentary canal and the sexual organs which are not proved. Does Mr. Lowne mean to say, for instance, that he or anyone else can adduce any reliable observations to prove that "the sexual organs are gradually developed, even from the time when the embryo is enclosed in the egg"? On p. 112 of his book on this very matter he says that he has not been able to verify Dr. Weissmann's assertion as to their presence, even in the larva; and now he suggests they exist in the egg!

But I must ask Mr. Lowne to explain what he means by saying in his letter, that it is an "utter mistake to suppose that any insect is re-developed during the pupa state," and that the nervous system "never undergoes degeneration;" because on p. 116 of his own book, published only last year, I find the following passage: "All the tissues of the larva *undergo degeneration*, and the imaginal tissues are *re-developed* . . . under conditions similar to those appertaining to the formation of the embryonic tissues from the yolk"! LIONEL S. BEALE

The Auditory Nerves of Gasteropoda

IN your issue for October 26, I notice an account of Leydig's recent paper on the auditory organ of the Gasteropoda, which, though excellent in other respects, has an error of omission which I should like to see rectified. When so important a discovery for morphology is discussed as that of the innervation of the otolithic sac from the supra-œsophageal in place of the sub-œsophageal ganglion which is its apparent connection in all Gasteropoda (excepting the Heteropodous forms), the credit of it should be given to the right man. That man is the most eminent and accurate of French comparative anatomists—M. Lacaze-Duthiers. Prof. Leydig states in the beginning of his own paper that Lacaze-Duthiers' statements on this subject (published in the *Comptes Rendus* about three years ago, if my memory serves me, and curiously mistranslated, *sus-œsophagien* being rendered *sub*-œsophageal in one of the first numbers of the Monthly Microscopical Journal), caused him to direct his attention again to this subject, and he has, as a result, confirmed the observations of the French *savant*, which were in opposition to the previously-received views of all observers, himself and Leydig included. Germany has a host of indefatigable anatomists, and the services of Franz Leydig, of Tubingen, are brilliant enough to eclipse most zootomical reputations; but let us not, at this moment above all others, forget to do justice, when the opportunity occurs, to a naturalist whose comprehensive, accurate, and beautiful zootomical monographs, rich in discoveries, have done more than those of any other Frenchman to sustain the great name of Cuvier's school.

Naples, Dec. 8 E. R. LANKESTER

DR. CARPENTER AND DR. MAYER

AT the Anniversary Dinner of the Royal Society on November 30, I was honoured by a request from the President to say a few words in acknowledgment of the toast to the Copley Medalist. I did so, stating briefly the origin of my acquaintance with Dr. Mayer's writings. Though Dr. Carpenter at the time was within sight of me, it did not occur to me to introduce his name into my remarks. A few days afterwards I was favoured by a letter from Dr. Carpenter, in which he reminds me somewhat sharply of this and other lapses as regards himself, and requests me to rectify the omission by a brief communication to the *Athenæum* or to NATURE. It will be fairer to Dr. Carpenter, and more agreeable to me, if he would state his own case *in extenso*. Here is his letter :—

"University of London, Burlington Gardens, W.,
"December 5th, 1871.

"MY DEAR TYNDALL,—If I correctly apprehended what you said at the Dinner of the Royal Society in regard to Dr. Mayer, you repeated what you had previously stated in your Lecture at the Royal Institution in 1863, as to the entire ignorance of Mayer's work which prevailed in this country until you brought it into notice on that occasion.

"Now, I very distinctly remember that a few days previously to that Lecture, I mentioned to you that as far back as 1851 I had become acquainted, through the late Dr. Baly, with one of Dr. Mayer's earlier publications ; and that, in bringing before the readers of the *British and Foreign Medical Review* (of which I was then the Editor) the 'Correlation' doctrine, as developed in Physics by Grove, and in Physiology by myself, I had stated that we had both been to a great extent anticipated by Mayer—as I should have shown much more fully if the pamphlet had earlier come into my hands.

"I also most distinctly remember that, as you stated in that Lecture, no one in this country—'not even Sir Henry Holland, who knows everything'—had ever heard of Mayer, I spoke to you again on the subject a few days afterwards ; and that you then expressed your regret at having entirely forgotten what had previously passed between us on the subject.

"As it would seem that this second mention of the matter has also passed from your mind, I shall be obliged by your looking at the passages I have marked in pp. 227 and 237 of the accompanying volume, from which I think that you will be satisfied that I had at that date correctly apprehended Mayer's fundamental idea, and that I have done the best to put it before the public that I could under the circumstances—the article having been in type and ready for press before his pamphlet came into my hands.

"Since, in thus bringing forward Mayer, I spontaneously abdicated the position to which I had previously believed myself entitled, of having been the first to put forward the idea that all the manifestations of Force exhibited by a living organism have their source *ab extra*, and not —as taught by physiologists up to that time—*ab intra*, I venture to hope that you will do me the justice of stating the real facts of the case in a short communication either to the *Athenæum* or to NATURE.—I remain, my dear Tyndall, yours faithfully, "WILLIAM B. CARPENTER

"Prof. Tyndall."

This letter was accompanied by a volume of the *Medico-Chirurgical Review*, containing an article headed, "Grove, Carpenter, &c., on the Correlation of Forces, Physical and Vital." As I am very anxious that my *amende* to Dr. Carpenter should be all that he could desire, I shall deem it a favour to be permitted to publish in NATURE the passages to which, by marginal pencil marks, he has directed my attention. The first of them is this :—

"We now come to the memoir 'On the Mutual Relations of the Vital and Physical Forces,' communicated to the Royal Society by Dr. Carpenter, which bears date June 20, 1850, and which is published in the 'Philosophical Transactions' for last year. This, we believe, is the first *systematic* attempt that has been made, in this country at least, to work out the subject, and, as it is mainly an expansion of the ideas which had been put forth in our own pages at the beginning of 1848, the author may claim priority as regards the enunciation and development of the idea, both of Dr. Fowler and Dr. Radcliffe, although to a certain degree anticipated by Mr. Newport. We shall presently find, however, that both these gentlemen were themselves anticipated in a quarter they little guessed, and the whole case is obviously one of a kind of which the history of physiology as well as of other sciences furnishes many examples, in which a connecting idea, developed in another department of inquiry, struck many individuals at once as applicable to the same class of facts, and was wrought out by them in different modes, and with various degrees of success, according to their previous habits of thought."

The impersonal way in which this and other passages of the article distribute merit among scientific authors caused me to ask Dr. Carpenter who wrote it. His reply to me was "I thought I had made it sufficiently plain to you that the article was written by myself."

Here follow the other marked passages quoted in full :—

"We must not omit, however, to give our readers some account of the remarkable production of Dr. Mayer, who seems to have arrived at conclusions in all essential respects similar to those of Prof. Grove and Dr. Carpenter previously to the publication of the first edition of the 'Correlation of the Physical Forces,' though subsequently to the delivery of the lectures in which Prof. Grove first announced his views and to the publication of the abstract of them. Of the existence of this treatise we have only recently been made aware, and we venture to affirm that Prof. Grove and Dr. Carpenter were alike ignorant of it. We bring it before the public now, both as an act of justice to its author, and also because it affords additional evidence in favour of the Correlation doctrine, that it should have been independently worked out by a clear and intelligent thinker.

"The first part of Dr. Mayer's treatise is concerned entirely with physical forces. He starts with the two axioms, 'Ex nihilo nil fit,' and 'Nil fit ad nihilum,' and founds upon abstract considerations his first argument for the unity of force, and for the convertibility of those which are commonly accounted distinct forces. Of this convertibility he then proceeds to adduce experimental proof, in very much the same mode with Prof. Grove, and he at last arrives at the following scheme expressive of their relations.

1. Force of Gravity. } Mechanical Force.
2. Motion. } Mechanical Effect.
A. Simple.
B. Undulating, vibratory.

Imponderables. { 3. Heat. 4. Magnetism, Electricity, Galvanic current.

5. Chemical decomposition of certain elements. Chemical combination of certain other elements. } Chemical Force.

"He then passes on to the study of vital phenomena, and he finds, like Dr. Carpenter, the source of all change in the living organism, as well animal as vegetable, in the forces acting upon it *ab externo;* whilst the changes in its own composition he considers to be the immediate source of the forces which are generated in it. He does not enter, like Dr. Carpenter, into an analysis of the phenomena of growth and development, but fixes his attention rather upon the production of heat, light, electricity, and (above all) motion by living bodies, and aims to show that all these forces are developed in the course of material changes in the organism, and hold a certain definite relation to them. On these points his exposition is very full and complete, and the perusal of his essay will amply repay any who desire to see how much may be done in imparting precision and clearness to physiological reasoning by minds trained in the school of exact science."

To these passages I would add one other brief quotation regarding the conversion of heat into electricity :—

"Of the production of electricity by heat, the phenomena first brought into view by Seebeck, and known under the name of 'thermo-electricity,' afford the most characteristic example. When dissimilar metals are made to touch, or are soldered together, and are heated at the point of contact, a current of electricity is set in motion, which has a definite direction according to the metal employed, and which continues as long as an increasing temperature is pervading them, ceasing when the temperature is stationary, and flowing in the contrary direction whilst it is decreasing" (pp. 213-14).

Having thus, it may be tardily, done justice to Dr. Carpenter, a very few words regarding his letter will complete the subject.

1. Dr. Carpenter has *not* correctly apprehended what I said at the dinner of the Royal Society in regard to Dr. Mayer. Neither at that dinner nor on any other occasion did I say that the ignorance of Mayer's labours in this country was "*entire.*"

2. I have not been altogether unmindful of Dr. Carpenter's desire to have his name mentioned in connection with this subject. In the printed report of the lecture referred to by Dr. Carpenter, delivered not in 1863 but in 1862, and published in the Proceedings of the Royal Institution for that year, these words appear—"Mayer's physiological writings have been referred to by physiologists—by Dr. Carpenter, for example—in terms of honouring recognition. We have hitherto, indeed, obtained fragmentary glimpses of the man, partly from physicists, partly from physiologists; but his total merit has never yet been recognised as it assuredly would have been had he chosen a happier mode of publication."

3. If this be not sufficient, my error was one of ignorance, not of will; for it is an entirely new idea to me that Dr. Carpenter regarded his relationship to Dr. Mayer in the light of a "spontaneous abdication," and it explains to me, what I could not previously understand, the importance attached by Dr. Carpenter to the passages above quoted.

4. I have looked at p. 227, and, indeed, throughout the entire article in the *Medico-Chirurgical Review* (and elsewhere), for evidence to prove that "at that date" (or at any other date), Dr. Carpenter had "correctly apprehended Mayer's fundamental idea," which is that of quantitative or numerical equivalence. Had I found such evidence, it would give me sincere pleasure to reproduce it here, but my search for it has not been successful.

5. This however entirely depends on my ability to appreciate such evidence. Holding the opinion that he does regarding the claims of his work to public recognition, Dr. Carpenter is perfectly consistent in demanding that even in an after-dinner speech those claims shall not be ignored.

JOHN TYNDALL

THE GEOLOGY OF OXFORD *

PROFESSOR PHILLIPS'S new work on the Geology of Oxford and of the Thames Valley is a most important contribution to the knowledge of the ancient history of the earth, and supplies a need which happens just at this time to be keenly felt. The Palæozoic rocks had been described and the forms of life which they contain unfolded, in "Siluria." The second, or Mesozoic chapter, is written with remarkable ability in the present work.

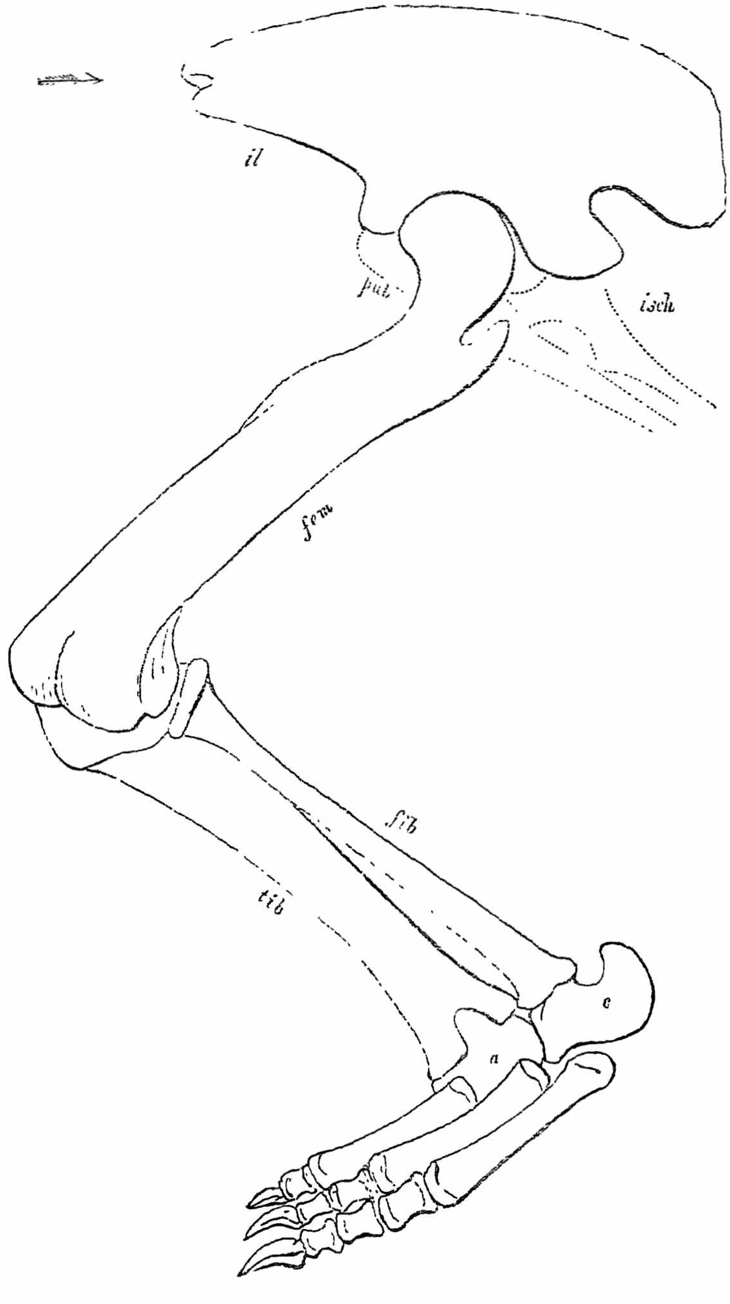


FIG. 1.—Megalosaurus—hind leg. Scale, one-tenth of nature.

This restoration in outline of the left hind limb of Megalosaurus is drawn from specimens, with the exception of the fibula, calcaneum, and ordinary phalangal bones—the claw-bone is known. Dotted lines represent the probable position of the pubic and ischial bones (according to the view of Professor Huxley); these being preserved in the British Museum and in the collections of the University of Oxford.

The principal bones are marked:—il. = ilium, pub. = pubis, isch. = ischium, fem. = femur, tib = tibia, fib. = fibula, c. = calcaneum, a. = astragalus. Cuvier supposed the calcaneum to be smaller than here represented.

The position of Oxford relatively to the formations which traverse Britain diagonally from the north-east to the south-west, equidistant on the one hand from the Malvern Hills which overlook the low-lying vale of Tewkesbury, and on the other from the basin of the Lower Thames, renders it a convenient centre around which to group observations which are primarily local, but which also affect the general question of Mesozoic Geology. In its latter aspect the book demands a most careful attention. The large number of plates and the carefully prepared lists

* "Geology of Oxford and the Valley of the Thames." By John Phillips, M.A., F.R.S., F.G.S. (Oxford Clarendon Press: 1871).

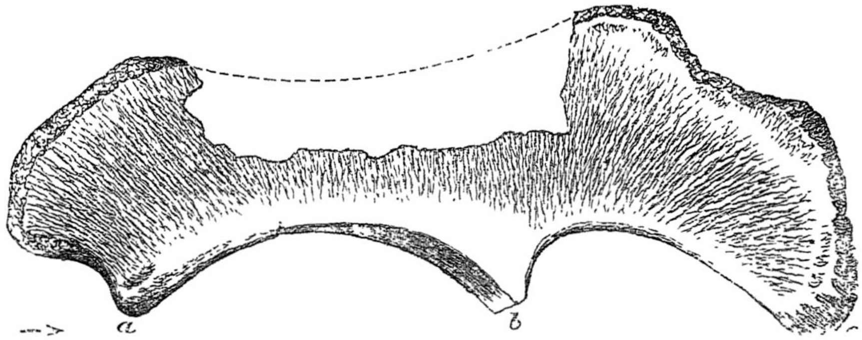


Fig. 2.—Ilium of Ceteosaurus, seen on the external face. Scale, one-tenth of nature. *a b*. The acetabulum.

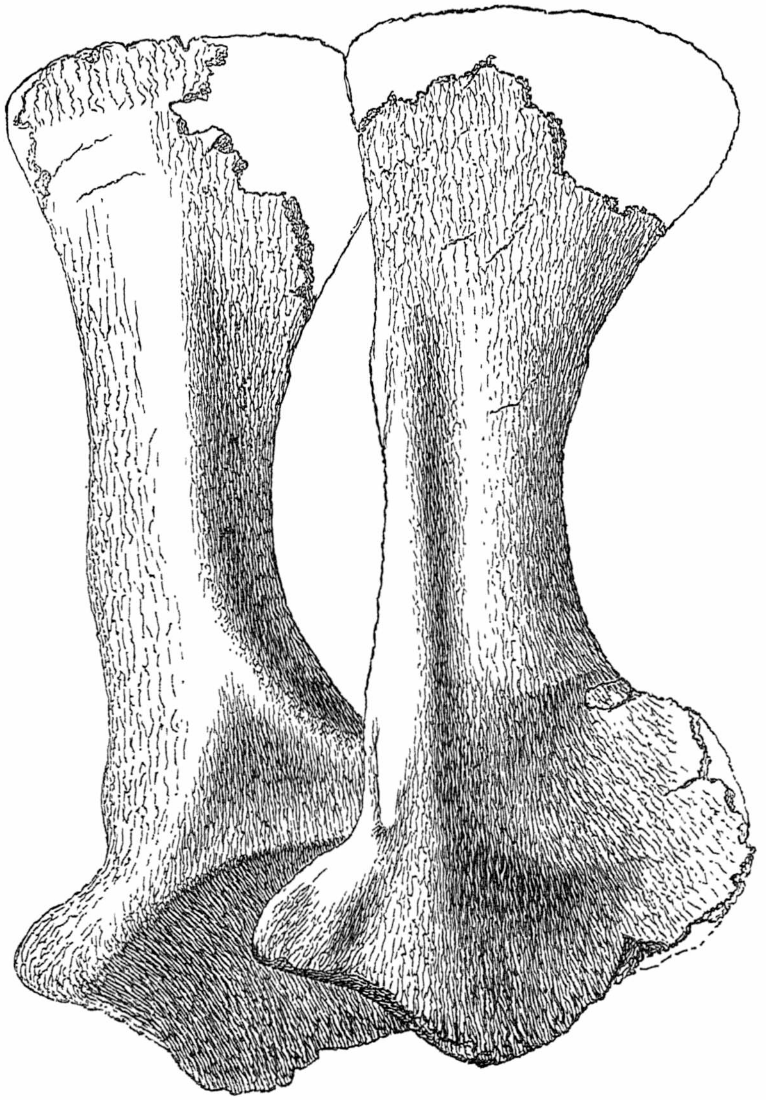


Fig. 3.—Scapulæ of Ceteosaurus. Scale, one-tenth of nature.

of fossils will be welcomed by all palæontologists; and those who enjoyed the advantage of studying geology at Oxford will find in this book the subject-matter of many of the lectures, and will have recalled to their minds the many pleasant associations connected with the expeditions of the Professor.

The work, as might be expected from the great and varied knowledge of the writer, is many-sided. In it the physical geographer will find the delicate questions of denudation, and of the excavation of hill and valley, discussed; the meteorologist will find the rain-fall tabulated; the hydraulic engineer the amount of water which is available for the use of Oxford and of London; the physicist the temperature and the prevailing winds; and the surveyor the position and thickness of the various strata from the Malverns eastward to London.

Prof. Phillips has, however, devoted his main strength to the description of the wondrous forms of reptilian life which have been furnished by the neighbourhood of Oxford, and which are preserved in a museum which is worthy of an old and wealthy University. The description of the Megalosaurus, and especially of the Ceteosaurus, is a most valuable addition to Palæozoology.

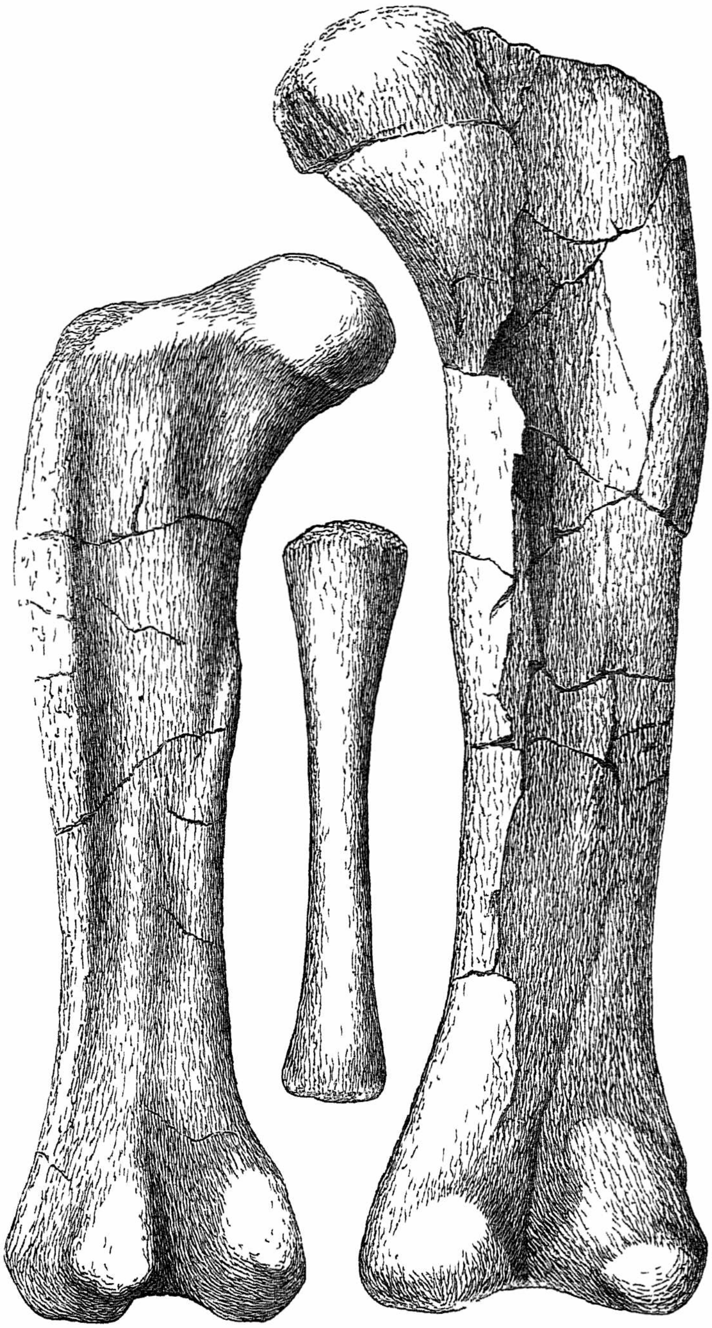


FIG. 4.—Femora and Fibula of Ceteosaurus. Scale, one-tenth.
The left-hand figure represents the specimen found in 1848 the right-hand figure that found in 1868, in the middle a small fibula found in 1848 is shown.

We owe to Prof. Huxley the clue to the right interpretation of the bones of both these animals, and the right definition of the whole group of Deinosauria, or Ornithoskelida, to which they belong, as being intermediate in character between the struthious birds and the reptiles. To this conclusion, however, he was largely aided by Prof. Phillips, and that it is true is rendered almost certain by the independent observations of Prof. Cope on the fossil reptiles of America. When the large pelvic bone from Stonesfield had been assigned to its true position in the skeleton of its possessor, and the so-called "clavicle" shown to be in all probability a long, stiliform, bird-like ischium, there could no longer be any doubt as to the

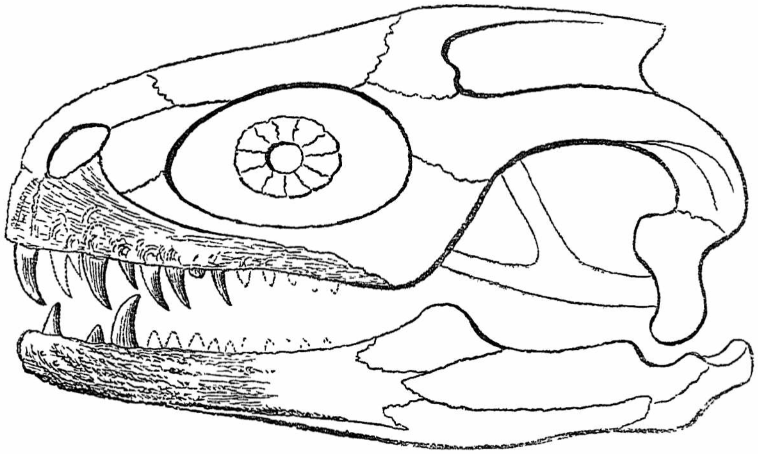


FIG. 5.—Head of Megalosaurus. Scale, one-tenth of nature.

Restoration of the head and lower jaw, of which, however, only the anterior portions are known. These are shaded. The type of Varanus is followed in general, but the postorbital arrangement is different, the bony circle there being completed from considering iguana and other lizards with some eye to crocodile. The length of head as thus drawn (thirty-nine inches) is less than that usually allowed (five feet).

The posterior part of the maxillary bone is separated from the orbit, notwithstanding its smooth, apparently free edge, by an intervening continuation of the jugal. This may be objected to. The nasal cavity is supposed to be divided by a median ridge (the single nasal continuous with the intermaxillary bone) into two openings, as in some of the monitors. The intermaxillary bones, which originally included four teeth each, appear united to the maxillary in this adult specimen.

kind of animal to which it belongs. The massive anchylosed sacrum of five vertebræ, and the whole arrangement of the pelvic arch, as well as the peculiar form of

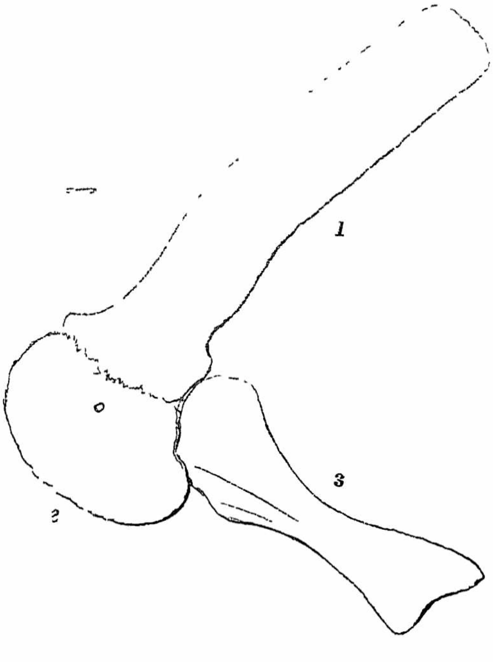


FIG. 6.—Megalosaurus. Scale, one-tenth of nature.

The left aspect of the shoulder girdle is here restored in outline from specimens in the Oxford Museum, which are complete except in regard to the lower end of the humerus. It will be remarked how bird like in the general arrangement and the forms of the bones is the humero-scapular structure, and specially how closely it resembles Apteryx.

1. Scapula. 2. Caracoid. 3. Humerus.

the astragalus and the shape of the coracoid and scapula, indicate a close alliance with the birds; while, on the other hand, the rest of the structure is mainly reptilian. The specimens which are preserved in the Oxford Museum, and which have been figured by Prof. Phillips, afford a very complete idea of the creature. The magnificent upper maxillary described by Prof. Huxley in the "Geological Journal," enables the front portion of the cranium to be restored with considerable certainty, and the accompanying woodcut (Fig. 5) may be taken to represent the entire head.

The premaxillaries of the Megalosaurus from the Oxford clay, in the collection of Mr. James Parker, are traversed by foramina which may indicate the presence of a small horny beak, or snout.

The arrangement of the shoulder girdle may be seen in Fig. 6, in which 1 = Scapula; 2 = Coracoid; and 3 = Humerus, as well as that of the pelvic arch and hind leg (Fig. 1), and the comparison of the two diagrams, will show the enormous disproportion of the hind to the fore limb in respect of size. All these three figures are drawn to one-tenth of natural size, and enable us to realise the form of one of the most remarkable of the fossil reptiles. The recent discovery of a nearly perfect skeleton by Mr. James Parker establishes the fact that some, at least, of the opistho-cœlian vertebræ, on which the genus Streptospondylus has been based by Prof. Owen, belong really to this animal. In point of time, the Megalosaurus lived from the Liassic to the Wealden age, and was one of the most formidable inhabitants of the great Mesozoic continent. The pains and labour which Prof. Phillips has bestowed in collecting and putting together the fragments and *disjecta membra* of the animal, and the careful criticism to which he has subjected each bone, render this portion of the work peculiarly valuable.

Nor is the chapter on the most gigantic of the fossil reptiles, the Ceteosaurus, inferior in interest to that which relates to Megalosaurus. The bones discovered in the Great Oolite at Enslow Bridge, near Oxford, in 1870, settled for ever all doubt as to the animal having been aquatic or terrestrial. The scapula (Fig. 3) and the ilium (Fig. 2) resemble in general outline those of Megalosaurus, and show that the animal belongs to the same Deinosaurian class, although "its fore limbs are more crocodilian," and "its pelvic girdle more lacertian." And the evidence offered by the articular ends of the bones of the extremities being adapted for movement in particular directions, the possession of large claws, and the hollowness of the long bones, indicate that it was of terrestrial, and not, as its name seems to imply, of marine habit. It may, however, have been, as Prof. Phillips suggests, "a marsh-loving or river-side animal." Its gigantic size may be gathered from the fact that one of the femora measures no less than 64, and a humerus 51·5 inches (Fig. 4).

Nor is there evidence wanting as to its diet. From the mutilated fragment of a tooth in the Oxford Museum, Prof. Phillips infers that its possessor lived on vegetables, since it resembled "that of an iguanodon in general shape (as far as can be known, one edge being broken), with a similar sweep of the concave surface seen in the diagram, and corresponding alternation towards the edge. The edge is not serrated, but the striæ of accretion are so arranged as to suggest that it may have been." The truth of this conclusion is proved by the subsequent discovery of a nearly perfect crown by Mr. Burrows, one of my students, in the Enslow Quarry, which has very much the appearance of a young tooth. It presents the serrations which have been worn away in the specimen above described, and bears out completely Prof. Phillips's description.

I have chosen merely these two animals as illustrating the subject-matter of the book, which is in every sense worthy of the high reputation of its author. W. B. D.

PARTHENOGENESIS AMONG THE LEPIDOPTERA

THE part of the *Archives Néerlandaises*, published by the Societé Hollandaise des Sciences à Harlem, for 1870, contains the results of some very interesting experiments undertaken by M. H. Weijenbergh, jun., on the above subject, one fraught with considerable interest to others besides entomologists. By Parthenogensis is meant the power that is possessed by females of producing eggs endowed with vitality, and from which young ones are produced, without impregnation taking place on each occasion. This subject has been extensively treated by von Siebold in his "Wahre Parthenogenesis bei der Schmetterlinge und Bienen," Leipzig, 1856, but confirmatory and new investigations were much needed. Those of M. Weijenbergh were conducted with every possible care and precaution, so that they can be relied upon. In the autumn of 1866 he saw a male and female of the species *Liparis dispar* together, and some days afterwards he saw in the same place a great quantity of the eggs, about 500 in number. In order to leave the rearing of these to natural processes, as far as possible, he left them exposed all the winter in the open air, and in April 1867, he removed them into his house. Before the end of the month the caterpillars had successively made their appearance. These were regularly fed, and by the middle of July each of the chrysalides which had been formed during June gave birth to a perfect butterfly. It was easy, with a little practice, to distinguish the sexes whilst in the caterpillar state, and all the males were removed as far as possible, and the females were placed in a box closed to all access from without. So successfully was this separation of the sexes effected, that only one male butterfly made its appearance among the females; and, as these had been successively removed to a third closed box as soon as they escaped from the chrysalis state, it was only necessary to sacrifice the three or four females which were in the box at the time. In all, about sixty females were obtained, to which there was absolute certainty that no male could by any possible chance have had access. Of these, two-thirds laid eggs in the autumn,—some, one, two, or three eggs only; others as many as ten or twenty, but yet even at the most not one-twentieth of the eggs of their mother. The other one-third laid no eggs at all In all about 400 eggs were collected, which were removed and carefully packed up till April 1868, when a large number of little caterpillars were seen. These were immediately placed on leaves in a large glass vase and watched carefully. It was easily to be seen that this batch of caterpillars possessed far less vitality than those of the previous year. A large number of the eggs dried up and were worthless, some fifty caterpillars alone appearing, and of these only about forty survived to become chrysalides. From these, by the end of July, twenty-seven butterflies made their appearance. The same precautions having been taken as before, the number of females was found to be fourteen. Of these, when again there had been no possibility of male access, one half laid no eggs, the remaining half, however, laying in all a fair number. As in previous years, these were removed and left all the winter carefully packed up, till, in April 1869, three years after the commencement of the experiments, young caterpillars again made their appearance. From these, strange to say, the number of butterflies obtained was in excess of those obtained in the previous year. The number of females as compared with males, was almost the same, in contradiction to the results of other investigators, which had indicated the probability of the ratio of the males to the females greatly increasing with each additional year. The eggs laid by the females of this year, carefully isolated as before, were packed up during the winter, but when examined in the spring of last year, 1870, no caterpillars made their appearance, the eggs became shrivelled up, and the experiment was at an end. There is every reason to believe that it was most carefully conducted, and that every regard was paid to strict accuracy during the whole three years or more that the experiment was being carried on. The results amount to these:—

(1.) Aug. 1866, eggs laid by impregnated female; April 1867, caterpillars appear; and, in July, perfect butterflies.

(2.) Aug. 1867, eggs laid by females of this year without impregnation; April 1868, caterpillars appear, and, in July, perfect butterflies.

(3.) Aug. 1868, eggs laid by females of this year without impregnation; April 1869, caterpillars appear, and, in July, perfect butterflies.

(4.) Aug. 1869, eggs laid by females of this year without impregnation; April 1870, no results—the eggs all dried up.

Thus, after the first impregnation of the female in the autumn of 1866, three successive broods of caterpillars and, ultimately, of butterflies made their appearance; and four successive times were eggs laid without further impregnation, in three of which they proved endowed with vitality. It would take a long series of experiments, each conducted with the same care as this, before an average could be drawn to determine the limit of this strange reproductive power. These experiments are so easily performed, and yet so valuable when accurately made, that a wide field is opened to those who do not care to undertake long and elaborate scientific investigations, and to such we most cordially commend them. Their value, as bearing on the theories of spontaneous generation, is very great, as there is much apparent probability that this power of Parthenogenesis will increase as we descend in the scale of life just as it decreases as we ascend. By its aid many phenomena, now apparently very strange and perplexing, will be found to be but obeying one great and universal law of nature, which becomes less visible the higher we ascend in the scale of life, but yet never ceases.

In conclusion, it may be stated that this power of Parthenogenesis has been found in many species of butterflies, and also among bees; and M. Weijenbergh, at the end of his interesting paper, gives a list of the seventeen or eighteen species which are known to him, or which are recorded as possessing this power. It is extremely probable that the more the subject is investigated, the more commonly will it be found to exist. J. P. E.

RESULTS OF SANITARY IMPROVEMENT IN CALCUTTA

WHEN a great public work is being done, it is a duty to call attention to it. In March 1862, Prof. Longmore, of Netley, who had acted as Sanitary Officer during the Mutiny at Calcutta, gave the following evidence before the Royal Commission on the sanitary state of the Indian Army :—"As regards the chief part of this extensive city (Calcutta)—that inhabited by the native population—the pestilential condition of the surface-drains and yards, and many of the tanks among the huts and houses, would not be credited by any one who had not been among them." In the "Report on Sanitary Improvements in India up to June 1871," recently printed by the India Office, is given a table showing that the cholera mortality in Calcutta had, for twenty years preceding 1861, averaged nearly 5,000 deaths per annum. In 1860 the cholera deaths were 6,553, and in 1866 they were 6,823. About this latter date works of drainage and water supply were commenced and have been gradually extended. Water is taken from the Hooghly and thoroughly filtered—it is then conveyed in pipes 12¾ miles in length to a reservoir in Calcutta and thence distributed. The whole population had this benefit conferred on them in the beginning of 1870, from which date the use of foul tank and river water was discontinued.

The drainage works are as yet confined to the southern districts, the sewage from which is conveyed to an outfall at the Salt Lake, and will be passed over a square mile of reclaimed land there, for irrigation of crops. The mortality from cholera in 1870 was 1,563, and the general mortality has fallen year by year with the extension of the works. Last year (1870) the death-rate was 23·4 per 1,000, considerably less than half what it was in 1865.

At a Social Science meeting held in Calcutta last March, a native physician, Dr. Chuckerbutty, gave his experience of the sanitary results as follows :—"I am in the habit of visiting, in the pursuit of my profession, the houses of the rich, as well as of the poor, in both divisions of the town, and I frankly confess that in the southern division, wherever the drainage works have been brought into play, the dwellings even of the humblest cottagers are in an infinitely better sanitary state than the mansions of the richest millionaires in the northern division where the drainage operations have not been extended. Before the completion of the water-works and the partial operation of the new drainage works, the mortality in Calcutta from dysentery, cholera, and fever, was most appalling. In 1865 dysentery was so common and fatal that sloughing cases of it were of daily occurrence. Such cases are now rarely to be seen. My annual share of cases of cholera in the Medical College Hospital before the completion of the new water-works was about 700, and I declare to you that, during the last eight months, I have scarcely had a dozen cases of that disease. Fever, too, has decreased during the same period in a like manner." The actual deaths from cholera in April, May, and June, of the present year were 85, 29, and 26, respectively.

After such results as these, we need not feel surprised that the Justices of Calcutta, a large proportion of whom are enlightened native gentlemen, decided unanimously last August to extend the drainage works all over the city, notwithstanding the opposition on purely theoretical grounds of certain British medical officers who ought to have known better, to the use of ordinary house drainage for Indian houses.

The opinion of the Army Sanitary Commission on this subject is quoted as follows in the India Office report :—"The municipal authorities of Calcutta and their officers have set an example of enlightened administration and effective expenditure to other Indian municipalities, which it is hoped will be followed. There are indeed few cities anywhere which can show so much good work done in so short a time and with such promising results for the future."

The laws of nature are the same everywhere. Calcutta has in times past suffered as London used to do from fatal fevers and bowel diseases, and there is now every prospect that a few years of active work will remove this stigma from the capital of the East, as it has been removed from the metropolis of the British Empire.

NOTES

THE following telegrams respecting the Total Eclipse of Dec. 12 have been received since our last :—"From the Governor of Ceylon to the Earl of Kimberley, dated, Colombo, Dec. 12, 10.45 A.M. :—'A telegram from Jaffna states that splendid weather prevailed during the eclipse. Most satisfactory and interesting observations have been made.'" "Mangalore, Dec. 16.—The eclipse observations have been very successful. The extension of the corona above hydrogen apparently small. Five admirable photographs have been taken." From Mr. Davis, photographer to the English Eclipse Expedition, through Lord Lindsay :—"Mangalore, Baikul.—Five totality negatives ; extensive corona ; persistent rifts ; slight external changes." The French Academy of Sciences has received from M. Janssen the following telegraphic despatch, dated Octacamund, December 12, 5h. 20m. :—"Spectre de la Couronne attestant matière plus loin qu'atmosphère du Soleil."

WE can hardly credit the report which has just reached us that the Treasury has, at the last moment, declined to sanction the expenditure of public money on the publication of the Eclipse Reports of 1860 and 1870. We understand the combined report is now nearly ready, and both Parliament and the nation are entitled to receive a statement of the manner in which the public money has been expended. There are innumerable cases which may be cited as precedents for the publication of similar documents by the Government ; as, for example, the Survey of Sinai, and the annual Greenwich Reports of Observations. After the Government has so generously granted money for recent scientific observations, we can hardly believe that the spirit of parsimony will so far prevail at the last moment as to mar, in this manner, the services it has performed towards Science.

THE death is announced on October 10, in Nicaragua, of fever, of Dr. Berthold Seemann, one of our most enterprising travellers and naturalists. Born at Hanover in 1825, Dr. Seemann was, in 1846, appointed naturalist to H.M.S. *Herald*, in its survey of the Pacific, during which voyage he had the opportunity of exploring, more thoroughly than almost any other European, the Pacific countries of South America and the Isthmus of Panama. In the same vessel he subsequently visited the Arctic regions, and the "Narrative of the Voyage of H.M.S. *Herald*," by Sir John Richardson and Dr. Seemann, is an important contribution to the natural history of previously little-known regions, the portion contributed by the latter comprising an account of the flora of Western Eskimo-land, north-western Mexico, the Isthmus of Panama, and the island of Hong-Kong. In 1860 he was sent by the English Government to the Fiji Islands, then lately acquired, and on his return published two works, one containing a narrative of his mission, the other, under the title of "Flora Vitiensis," a history of the vegetable productions of the islands. Since 1864, he has been greatly interested in the mining capabilities and other resources of the various states of Central America, and has spent much of his time there in the interest of different trading communities, and in promoting the route across the Isthmus. Dr. Seemann is the author of several popular botanical works in German and English, and has been since its foundation, Editor of the *Journal of Botany, British and Foreign.*

PROF. SEDGWICK's appeal for subscriptions from members of the University of Cambridge, to enable him to purchase the valuable collection of fossils belonging to Mr. Leckenby, has resulted in the collection of the sum required, 800*l.* Arrangements have been made for the completion of the purchase, and it is expected that in a few weeks Mr. Leckenby's valuable collections will be deposited in the Cambridge Geological Museum. This prompt and liberal response to the touching appeal of the venerable Professor demonstrates the regard in which he is universally and deservedly held by the members of the University.

THE following is the result of the examination for the Natural Science Tripos at Cambridge :—First Class—Garrod, John's; Lydekker, Trinity; Lewis, Downing; Warrington, Caius. Second Class—W. Edmunds, John's; Fox, Peter's; Read, John's; Owen, Downing; Everard, Trinity; Maudslay, Trinity-hall; Brewer, John's; Buddon, John's; Wigan, Trinity; Blunt, John's. The following acquitted themselves so as to deserve ordinary degrees :—Burrows, Caius; Murphy, John's; Phelps, Sydney; Pittman, Corpus; Wakefield, Caius. In the second class Fox and Reed are bracketed, also Brewer, Buddon, and Wigan.

NEXT term, Mr. Ruskin, Slade Professor of the Fine Arts at Oxford, will deliver a course of lectures on "The Relation of Natural Science to Art."

THE Government is advertising the appointment, by open competition, of a clerk to the Curator of the Royal Gardens at Kew, and of a second assistant in the Herbarium. The salaries commence at 100*l.* and 60*l.* respectively, and the specified age is in one case from 20 to 30, and in the other from 18 to 30. The examinations will take place on January 16.

THE following lectures have already been delivered this winter at Manchester, as Science Lectures for the People :—The first on November 3 on "Yeast," by Prof. Huxley; November 10 "Coal Colours," by Prof. Roscoe; November 16, "The Origin of the English People," by Prof. A. S. Wilkins; November 24, "The Food of Plants," by Prof. Odling; December 1, "The Unconscious Action of the Brain," by Dr. Carpenter. These lectures are always well attended, but since they are all reported and printed at the low price of a penny each, they appeal to a much wider circle than most of a similar character. This is the third year of these Science Lectures. The lectures for this session and those of past years are published by John Heywood, Deansgate, Manchester.

THE *Pall Mall Gazette* states that the approaching 400th anniversary of the birth of Copernicus has revived a contest of long standing between Poland and Germany, each of which claims the great astronomer as a son. The Germans argue that he was a German because he was born in Thorn, which at the time of his birth was under German rule; to which the Poles reply that Thorn was then really a Polish town, having been separated from Poland only seven years before; that his father and mother were Poles; that when he studied at Padua he enrolled himself among the students of the Polish nationality; and that throughout his life he gave constant proofs of his attachment to Poland and her King. Poland has always honoured Copernicus as one of her greatest men. A statue of him was erected by national subscription many years ago at Warsaw, and there are two others at Cracow, besides which numerous Polish medals and books have been issued in celebration of his memory. The anniversary above mentioned will be celebrated on the 19th of February, 1873, and great preparations are already being made at Posen for the occasion. The "Society of the Friends of Learning" in the old Polish city held a meeting the other day, at which it was decided, on the motion of a Polish clergyman, Canon Polkowski, to offer a prize for the best life of Copernicus, comprising the results of the latest investigations on the subject, and to publish it in the Polish, French, and German languages.

WITH a view towards the completion of the collection of water colour paintings illustrating the history of that art, Mr. William Smith, Vice-President of the National Portrait Gallery Trustees, has allowed Mr. Redgrave, R.A., the Inspector-General for Art, to select from his choice and valuable collection as many rare specimens as, in Mr. Redgrave's judgment, would illustrate the *early period* of the art. The works selected by Mr. Redgrave have been presented by Mr. Smith to the nation.

IT has been arranged that the new machines for printing, composing, and distributing type, which have been recently perfected at the *Times* printing office, shall be completely exhibited in working at the London International Exhibition of 1872. The power of rapid production by these several means is probably threefold in advance of any existing modes of printing. The *Mail* newspaper will be printed three times a week, and if possible the daily supplement of the *Times.*

THE third part of Mr. W. H. Baily's "Figures of Characteristic British Fossils, with Descriptive Remarks," has just been published. Part 4, which will complete the first volume, is in progress; each part consists of ten beautifully-executed plates, and the text is interspersed with many woodcuts. These latter are chiefly of recent forms. The figures are for the most part original, and this little work most worthily fills up a blank in biological literature.

FROM the commencement of November till December 12, a period of six weeks, the temperature at London was below the average, with the break of only a single day. The tables forwarded weekly by Mr. Glaisher to the *Gardener's Chronicle* show the average depression during the whole of that period to have amounted to as much as 6·5 F. below the mean of the last fifty years, the minimum being on December 8, when the thermometer fell to 18°·6, and the temperature of the twenty-four hours was 19°·3 below the mean. Throughout France the month of November was very severe, the mean temperature of the month having been lower only four times during the last century. According to statistics presented to the Academy of Sciences by M. Ch. Sainte-Claire Deville, the thermometer fell as low as −11°·3 C. (11°·7 F.) at Montargis on December 3, while even at Marseilles the remarkably low temperature (for that latitude) of −2°·5 C. (27°·5 F.) is recorded on November 23. During the present month the frost is stated to have been still more severe in France and Italy, where much snow has fallen at Rome; and the unusual depression appears to have extended to North America.

THE Smithsonian Report, 1869, contains an account of the eruption of the Volcano of Colima in June 1869, by Dr. Charles Sartorius. The height of the volcano is 11,745 feet, and it had remained in repose since the last eruption in 1818. On June 12, 1869, dense smoke issued from the crater, and violent detonations were heard. On the 13th smoke and stones were ejected from the crater, and a "glowing upheaval" of the surface was seen. It was visited on June 15, when it was found that an upheaval of some 114 feet by 754 feet had taken place, forming a flattened arch. The appearance was that of a wild mass of volcanic red-hot rocks heaped one upon another, and constantly in motion, not unlike freshly-burned lime when sprinkled with water. The rocks which rolled down were, on cooling, of a grey colour. A piece broken off rang like glass, and was vitreous and porous, In the middle of the upheaved mass the movement was strongest; three large clefts and intense light were displayed, while engulphed stones, which were swallowed up in great masses, were followed by a noise as of violent wind, and by clouds of smoke sometimes blue, sometimes yellow. The temperature of the air in the vicinity was 126° F. The stones in the midst of the heaving mass seemed to be softened, though not melted, and no flow of lava took place. This upheaval had taken place on a small, flat plain upon the north-east side of the mountain, it ascended to the scarp of the cone, and stretched in the direction of the snow peak, which was some $2\frac{3}{4}$ miles distant. On reaching this summit the temperature was found to be 41° F. From here the whole of the new upheaval could be surveyed. In the middle of it the most vehement movement was in progress, attended by the constant upheaving and descent of rocky masses, fire, and blue and yellow columns of smoke. The upper ancient crater has a diameter of 492 feet, and from it arose dense sulphurous vapour. Later explorers found a fissure from the new upheaval to the upper peak, 1—3 feet wide and about 3 feet in depth, but neither heat nor vapour issuing from it. Such volumes of fetid gases issued from the fissure that the inhabitants of the district were forced to leave their abodes. Cows and sheep were killed by it, so that it was found necessary to drive away the herds from the neighbourhood of the volcano.

PROF. VERRILL has lately given, in the *American Journal of Science*, an account of the researches in marine zoology prosecuted by him during the past summer at Wood's Hole, Massachusetts, in connection with investigations of Prof. Baird respecting the food fishes of the coast of the United States; and in this he calls the attention of zoologists to some of the more important features of these examinations, promising a fuller account hereafter. One of these results consisted in ascertaining that, while the shores and shallow waters of the bays and sounds, as far as Cape Cod, are occupied chiefly by southern forms belonging to the Virginian fauna, the deeper channels and central parts of Long Island Sound, as far as Stonington, Connecticut, are inhabited almost exclusively by northern forms, or an extension of the Acadian fauna. Both the temperature observations at the surface and the deep-sea dredgings prove that there must be an offshoot of the arctic current settling into the middle of Vineyard Sound. Quite a number of interesting ascidians, both simple and compound, were met with by Prof. Verrill, several of them entirely new to science. Several new sponges were collected, and also a large number of crustaceans and molluscs previously unrecorded in that region. We would refer our readers to Prof. Verrill's article in the November number of the *American Journal of Science* for these interesting facts.

Harper's Weekly furnishes the following additional information of the great exploring expedition upon which Prof. Agassiz has been expecting to engage during the voyage of the Coast Survey steamer *Hassler*, from Boston to San Francisco, by way of the Straits of Magellan. The expedition was originally to start as early as July or August, and in that event the exploration in question would have commenced off the coast of the United States. Owing, however, to unexpected delays, the vessel has but recently fitted out and reported at Boston, where she has been detained, undergoing alterations of her machinery. We have already noticed the general plan and objects of the expedition. The scientific corps, as will be remembered, consists of Prof. and Mrs. Agassiz, Count Pourtales, ex-President Hill, o Cambridge, Dr. White, Mr. James Blake, and Dr. Steindachner, each gentleman having special charge of a particular department of the work, and interested in its successful accomplishment. The vessel itself is under the command of Captain P. C. Johnson, with Messrs. Kennedy and Day as lieutenants. Owing to the lateness of the season, the original plan of making extended explorations in the West Indies and off the eastern coast of South America has necessarily been modified, and the vessel will probably proceed almost directly to the Falkland Islands and the Straits of Magellan, there to commence the comprehensive investigations proposed, as otherwise a sufficient share of the summer season of the Straits could not be secured. The Atlantic Ocean work thus given up will, in all probability, partly at least, be performed by the *A. D. Bache*, a consort of the *Hassler*, next year.

THE American Museum of Natural History, established at Central Park, New York, has, we learn from *Harper's Weekly*, had a most liberal offer made to it. The collection of shells of Dr. John C. Jay, formerly of New York, but now of Rye, is well known as one of the largest in the world; indeed, some years ago it was decidedly the finest in the United States; and although, with the lapse of years, the doctor has been less energetic in keeping it up to the present date, yet it forms a cabinet of magnificent extent, embracing, it is said, 14,000 species, 20,000 varieties, and 50,000 specimens, and costing many years of labour, and over 25,000 dols. in money. In addition to this, there is a library of 850 bound volumes, almost approaching completeness in its extent upon the subject of conchology. This has cost the doctor 10,000 dols., many of the works having been purchased at a time, too, when they were cheaper than at present. The doctor now offers to sell this library to the Museum of Natural History for the sum of 10,000 dols., and with it to present the entire collection of shells just referred to, so that the whole may go together, and form a complete section of the museum.

ADVICES from Portland, Oregon, under date of November 17, announce the arrival of Prof. O. C. Marsh, with his party of Yale College students, from an extended geological and palæontological exploration in the Blue Mountains and the John Day Valley. As might have been anticipated from the previous discoveries of the Rev. Thomas Condon, of Portland, in the same region, under much less favourable auspices, very extensive collections of fossil animals were made, which, when placed, as intended, in the museum of Yale College with those previously gathered by Prof. Marsh, will make a series of the extinct vertebrates of North America unequalled in any other cabinet.

AT the meeting of the Norfolk and Norwich Naturalists' Society, held Nov. 18, Mr. Barrett read some further notes on the coast insects found at Brandon, which he considered confirmatory of the opinion expressed by him in a former paper, that these species have occupied this district, now far inland, from the time when it was part of the sea-coast. Amongst other coast species mentioned by Mr. Barrett was *Agrostis Tritici*, and of this species he remarked that, although it occurs sparingly on inland heaths, all the specimens are of a dull brown colour, whilst those found on the sea-coast are generally distinctly marked and richly coloured; all those taken by him at Brandon had precisely the deep style of colour and markings which characterise it on the sea-coast. *Agrostis cursoria*, although very abundant on the sea-coast, is not to be found at Brandon; and this Mr. Barrett considers a very strong proof hat the other strictly littoral species enumerated have not reached their present situation by migrating across the intervening land from the present sea-coast. This species he thinks it not improbable was an immigrant from the eastward at a comparatively recent date, and that it has attained its greatest abundance on the spot where it first obtained a footing. It would not, therefore, have been an inhabitant of this portion of the post-glacial coast.

AN earthquake shock was felt in New Jersey, Delaware, and Pennsylvania in the United States, on October 9. At Delaware it was noticed at 9.40 A.M, and at Philadelphia at the same time.

THE MONOCOTYLEDON THE UNIVERSAL TYPE OF SEEDS*

IT must be evident to those who heard my paper on "Adnation in Coniferæ" at the Chicago meeting of the Association that the observations there detailed could scarcely be accounted for, if the belief be true which is generally held by botanists, that the leaf originates at the node from which it seems to spring. It is not, however, an object with me to attack existing theories, or establish new ones, but simply to present facts as I see them. The origin of the leaf will no doubt prove a question which will in time take care of itself. But this generalisation cannot be avoided by the readers of that paper, that the whole plant is originally a unity; and that the subsequent formation of elementary organs, and their complete development, or absorption into one another, is the result of varying phases of nutrition. The leaves in Coniferæ were found to be free or united with the stem in proportion to the vigour of the central axis. Following up the subject, I now offer some facts which will show that all seeds are primarily monocotyledonous; and that division is a subsequent act, depending on circumstances which do not exist at the first commencement of the seed growth.

It is well known that in some species of Coniferous plants the number of cotyledons varies. I have noticed in addition to this that whether the cotyledons are few or many, there is no increase in the whole cotyledonous mass. In the Norway spruce, *Abies excelsa*, there are sometimes as many as ten cotyledons, in others only two. In the latter case they are broad and ovate, while in the former they are narrow and hair-like; in short, when in the two cotyledoned state it is not possible to note any difference between a seedling Norway spruce and a Chinese arbor vitæ, *Biota orientalis*, except by the lighter shade of green. The two-leaved condition is not common, but specimens of threes and others I exhibited to Drs. Torrey and Gray at the Troy meeting. Any one who will examine sprouting seeds of the Norway spruce will agree to the proposition that the cotyledons are not original and separate creations, but a divided unity. My next observations were on some acorns of *Quercus agrifolia*, the division into cotyledons were numerous and irregular. Cut across vertically, some represented the letter C, others the letter N, and again, with four cotyledons the letter M. Here again it was clear that whatever the form and number of the cotyledons, there was no increase of the original cotyledon mass. Examining sprouting peach kernels, the variations in form and number were of the most remarkable character. I need not repeat them in detail here, as they are reported in the April and May "Proceedings of the Academy of Natural Sciences of Philadelphia." In addition to the fact of no increase in the whole cotyledon mass, it was here clear that when the cotyledons were duplicated, the duplications at least were subsequent to the original ones. Still so far nothing had been seen to indicate when the first pair of cotyledons were formed. *Quercus macrocarpa* and *Quercus palustris* were silent to my questions. In a large number I found no variations whatever. Each mass was divided smoothly and exactly into two cotyledons. *Quercus robur*, the English oak, however, gave some curious evidence. Two germs under one seed coat were numerous, and often three, and the cotyledons took on a variety of forms. But there was never any more increase in the cotyledonous mass than if but two lobes had been formed, and there was no more rule in the division than there would be in the sudden breakage of a piece of glass. A detailed account of these will also be found in the "Proceedings of the Academy of Natural Sciences of Philadelphia" for May. *Quercus rubra*, the American red oak, furnished the one link wanting to connect the first division into lobes with the other phenomena. All the acorns examined had three or four sutures in the cotyledon mass, and extending all along the longitudinal surface externally, without any reference to cotyledonal divisions. These sutures extended sometimes but a line in depth, at others almost to the centre of the mass, always accompanied by the inner membrane, as is the case in ruminated seeds. The whole mass was divided only in two parts in any that I examined of this species, but the division was always in the direction of the sutures. Hence each cotyledon was very irregular. Sometimes one-third the mass only went to one while the other had two-thirds of the whole mass. It was easier to burst in the weaker line of resistance. But the interest for us is to note that ordinarily the cotyledonous mass was a unit—then the sutures or fissures were formed, and ultimately the two divisions of the lobes followed in their direction. The division was the last condition, not the first. I know how much we should guard against generalising on a limited supply of facts, but it requires an effort to believe that oaks, pines, and peaches, as we have seen primordially monocotyledons, are in this respect different from other so-called dicotyledonous plants; and if we grant that all seeds are primarily monocotyledonous, may we not ask why in any case they are divided? We have seen that there is no increase of mass in the division, the same amount is furnished in one as in many. Would it in any way injure the Indian corn to have its mass divided into two lobes? or would not the plantlet be as well provided for if the acorn were in one solid mass? Division would seem to be a necessity occurring subsequent to organisation, and existing from the position of the plumule alone. In monocotyledons, as we know, the plumule is directed parallel to, or away from, the cotyledonous mass, when, of course, on this theory, it remains an undivided mass. But in the dicotyledonous section, the plumule is directed towards the apex of the mass; and as we know in the case of roots against stone walls, or mushrooms under paving-stones, the disposition in the growing force of plants is to go right forward, turning neither to the right nor the left; so in this mass of matter the development of the germ would make easy work of the division; and no doubt often at so early a stage as to give the impression we have been under hitherto, that the division is a primary and essential process.

* Abstract of a Paper read at the Indianopolis Meeting of the American Association for the Advancement of Science, August 1871, reprinted from the *American Naturalist*. By Thomas Meehan.

SCIENTIFIC SERIALS

THE *Monthly Microscopical Journal*, No. 35, November 1871. "On the Form and Use of the Facial Arches," by W. Parker, F.R.S., is chiefly occupied by observations on embryo salmon. "Another Hint on Selecting and Mounting Diatoms," by Capt. Fred. H. Lang, details the method employed by the author for remounting diatoms, either previously badly mounted, or from which it is desirable to select certain forms.—"The Monad's Place in Nature," by Metcalfe Johnson, M.R.C.S.E., has for its object to show a connection between the earlier forms called Monads, and those higher and more complicated organisms at present recognised under the name of Infusoria, Mucedinæ, Confervæ, Oscillatoriæ, &c. The conclusions deduced from some of the experiments are that the author looks upon Monas in its earliest forms to be the starting point whence several products may result, and among the number are Infusoria, Mucedinæ, Englenæ, Oscillatoriæ. He is induced to believe that the Pin-point Monad, when developed under absence of light and only a limited quantity of air, gives rise to the class of plants known as Mucedinæ. Again, he maintains that during the watching of the liquids under experiment the Monads presented various forms, evidently transitional, from the round Pin-head Monad to oval young Paramœcia, until we come to sufficient size to give it a name such as *Kolpoda Cucullus*, &c.—"Infusorial Circuit of Generations," by Theod. C. Hilgard, deals with a similar subject, but in a very different style. It is often very difficult to gather the author's meaning from language such as the following:—"And from each little dot in these 'clouds of life' a separate vorticella can be seen to develop! It is here, indeed, at this first *visible* advent or exordium of animate life, and the resurrection of millions of germs through the spontaneous dissolution of a single one, that the last nubecular *microscopic* perceptions closely resemble the last nebular telescopic as well as the *theoretic* ones of Laplace's cosmogony." The concluding portion of this paper, which is reprinted from *Silliman's Journal*, appears in the succeeding number, and is interesting as a contribution to the "curiosities of scientific literature."

THE *Monthly Microscopical Journal*, No. 36, December 1871.—"Notes of Prof. James Clark's Flagellate Infusoria, with Descriptions of New Species," by W. Saville Kent, F.Z.S. An entirely technical paper, consisting of the diagnostic characters of new species, with those of previously-described ones amended. Eleven forms are figured and described, all of which were found in fresh water at Stoke Newington.—"On Bog Mosses," by R. Braithwaite, M.D., F.L.S., Part II., is occupied chiefly with the anatomy of the leaf and development of the plant.—"On the Conjugation of Amœba," by J. G. Tatem, is a note serving to strengthen the supposition previously advanced by this author, "that these large Amœbæ so frequently met with in the autumn months are actually the incorporation of two individuals in a copulative act," from which free-swimming ciliated germs might eventually issue. "On the Connection of Nerves and Chromoblasts," by M. Georges Pouchet. The inference drawn from an examination of the pectoral fin of a young flat-fish is that there is a reality of connection between the nervous and sarcodic elements, but that the nature of this connection is unknown.

THE *Revue Scientifique*, Nos. 19—25, contains, among others, the following articles, translations, and reprints: — General Morin's eulogy on Piobert and his inventions in artillery; Dr. Carpenter's lectures at the Royal Institution; the continuation of Grehant's course of lectures on Experimental Physiology; M. Lorain on primary and secondary instruction in France; Berthelot on the union of alcohols with bases, and on the history of carbon; Moleschott on the regulators of human life; Saussure on the life and works of Claparède; Valentin on the electric properties of nerves during embryonic life, and during putrid decomposition; a summary of the most important papers read at the Bologna International Congress of Anthropology and Prehistoric Archæology; Contejean on the origin of sedimentary deposits; Mr. Bentham's last anniversary address to the Linnean Society; Fonvielle on aërial navigation; Prof. Huxley's article in the *Contemporary Review* on English Critics of Darwin.

THE twentieth volume (1870) of the *Verhandlungen der k.k. zoologisch-botanischen Gesellschaft in Wien*, although a stout octavo, is hardly equal in bulk or in the variety of its contents to some of its predecessors; nevertheless its readers will find in it an abundant supply of valuable papers on zoological and botanical subjects. As usual, entomological articles are in the majority under the former head, and here Dr. Winnertz leads off with two papers on Diptera, containing descriptions of species belonging to the *Lestreminæ*, a sub-family of Cecidomyidæ, and of the species of *Heteropeza* and *Miastor*—two genera of the same family. Singularly enough these, and a short notice by M. von Bergenstamm on the metamorphoses of *Platypeza holosericea*, are the only papers on Diptera in the volume.—The Lepidoptera also receive but little notice, but on the Rhynchota we have some important papers:—M. P. M. V. Gredler furnishes a list, with notes, of the Heteropterous Rhynchota of the Tyrol, and Dr. F. X. Fieber the characters of twelve new genera and twelve new species of the same group. The forms described by the latter are from various parts of Southern Europe.—M. C. Tschek describes a number of Austrian Ichneumonidæ belonging to the group of the Cryptoides, Dr. G. Mayr a number of new species of ants, and Dr. J. Kriechbaumer four new South European species of humble bees.—A paper on the Orthoptera of the Syrnian valley in Hungary by M. V. Graber, which includes an interesting description of the district, is the only other entomological paper to which we shall refer.—The malacologist will find a list of the land and freshwater mollusca of Galicia by Dr J. Jachno, a monograph of the genera *Emmerida* and *Fossarulus* by M. Brusina, and an important paper on the anatomy of *Tribonophorus* and *Philomycus*—two forms of naked Pulmonata; whilst for the ichthyologist we have the first part of a descriptive synopsis of the fishes of the Red Sea from Dr. C. B. Klunzinger, who also notices the animals observed upon a coral reef in the Red Sea.—M. D. Dybowski describes a new form of Salamander from Siberia under the name of *Salamandrella Keyserlingii*, and Dr. Burmeister gives a description of the pelvis of *Megatherium*. The botanical papers are to a considerable extent of the nature of local lists, but some of these contain a good deal of descriptive matter. Thus in M. Schulzer von Müggenburg's "Mycological Observations in North Hungary" we find many descriptions of fungi; Glowacki and Arnold's "Lichens from Carniolia" contains descriptions of species, as does also the latter's "Lichenological Excursion into the Tyrol," and the contribution to the moss-flora of East by MM. Juratzka and Milde. M. F. Hazslinsky describes the *Sphæriæ* which are parasitic upon the rose; M. Julius Klein's mycological communications contain a description of a new genus of Mucorine fungi, and of some other forms which grew with its representative; and M. Schulzer von Müggenburg, above-mentioned, has also his mycological contributions, which consist almost entirely of descriptive matter. The papers which treat of the higher forms of plants, and those describing the natural history journeys of their authors, are not numerous. We may mention especially a long paper by M. F. Krasan on the periodical phenomena of vegetable life, and an article by Dr. A. Unterhuber on the position of the scales of the fruit in *Ceratozamia mexicana*. This list of papers will be sufficient to show how much there is in the proceedings of the Vienna Zoologico-Botanical Society to interest both the zoologist and the botanist.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, December 6.—Mr. J. Prestwich, president, in the chair. Prof. Giovanni Capellini, of Bologna, was elected a Foreign Correspondent of the Society. 1. "On the presence of a raised beach on Portsdown Hill, near Portsmouth, and on the occurrence of a Flint Implement at Downton." By Mr. Joseph Prestwich, F.R.S., President. The author noticed a section observed by him in a pit ten miles westward of Bourne Common and five miles inland in a line on the north side of East Cams Wood. It is situated at an elevation of 300 feet above the sea level, and shows some laminated sands with seams of shingle, overlying coarse flint-shingle with a few whole flints, which the author regarded as a westward continuation of the old sea-beach which has been traced from Brighton, past Chichester, to Bourne Common. A flint flake was found by the author at the bottom of the superficial soil in this pit. The author also noticed the occurrence of a flint implement of the type of those of St. Acheul in a gravel near Downton in Hampshire. This gravel capped a small chalk-pit, and its elevation above the River Avon was about 150 feet. Two gravel terraces occur between this pit and the river, one 40 by 60 the other 80 by 110 feet above the level of the latter. Mr. Codrington stated that, according to the Ordnance Survey, the level of the pit at Cams Wood was not more than 100 feet above the sea, so that it was at about the same level as the gravels of Titchfield and elsewhere. Mr. Evans remarked that the flint flake from Cams Wood presented no characters such as would prove it to be of Palæolithic age. He was, on the contrary, inclined to regard it as having been derived from the surface. He commented on the height at which the Downton implement had been discovered, which was, however, not so great but that the containing gravels might be of fluviatile origin. Mr. Gwyn Jeffreys thought that if the beds at Cams Wood were marine, some testaceous remains might be found in them. If these were absent, he should rather be inclined to regard them as fluviatile. Mr. J. W. Flower contended that the gravel at Downton could not be of fluviatile origin. He thought, indeed, that the gravel was actually at a higher level than the present source of the river. If this were so, he maintained that he transport of the gravel by fluviatile action was impossible. He further observed that gravels precisely similar, also containing implements, had now been found, as well in the Hampshire area as elsewhere, the transport of which, in his view, could not possibly be attributed to any existing rivers. At Southampton they occur 150 feet above the River Itchen and the sea, and considerably inland; at Bournemouth, on a sea cliff 120 feet in height; and at the Foreland (at the eastern extremity of the Isle of Wight), on a cliff 82 feet above the sea, and far remote from any river. If, therefore, these deposits were effected by fluviatile agency, it was evident that all traces of the rivers were afterwards effaced by some great geological changes, or, in the alternative, some great geological change, not fluviatile, must have caused the deposit. Upon the whole he was disposed to conclude with the French geologists as well as with many eminent English authors that the accumulation of all these superficial drifts was, as the late Sir Roderick Murchison had said, sudden and tumultuous, not of long continuance; and thus it was such as would result from some kind of diluvial action, rather than from the ordinary long-continued action of water. Mr. Judd pointed out, in contravention to Mr. Jeffreys' views, that in the Fen district, over large tracts of deposits of undoubtedly marine origin, not a trace of marine shells could be found. Mr. Prestwich, while willing to concede that the implement-bearing gravel-beds had been deposited under more tumultuous action than that due to rivers of the present day, was still forced to attribute the excavation of the existing valleys and the formation of terraces along their slopes to river-action. He showed that Mr. Flower's argument as to the present level of the source of the river was of no weight, as the country in which it had its source was formerly, as now, at a much higher level than the gravel at Downton. As to the absence of marine shells at Cams Wood, he cited a raised beach in Cornwall which, in company with Mr. Jeffreys, he had examined for a mile without finding a trace of a shell, though for the next half-mile they abounded. There was the same difference between the raised beach at Brighton and at Chichester. He was obliged to Mr. Codrington for his correction as to the level at Cams Wood, though the pit was at a higher elevation than the one to which Mr. Codrington had alluded.—2. "On some undescribed Fossils from the 'Menevian Group of Wales.'" By Mr. H. Hicks. In this communication the author gave descriptions of all the fossils hitherto undescribed from the Menevian rocks of Wales. The additions made to the fauna of the Lower Cambrian rocks (Longmynd and Menevian groups) by the author's researches in Wales during the last few years now number about fifty species, belonging to twenty-two genera, as follows:—Trilobites, 10 genera and 30 species; Bivalved and other Crustaceans, 3 genera and 4 species; Brachiopods, 4 genera and 6 species; Pteropods, 3 genera and 6 species; Sponges, 1 genus and 4 species; Cystideans, 1 genus and 1 species. By adding to these the Annelids, which are plentiful also in these rocks, we get seven great groups represented in this fauna, the earliest known at present in this country. By referring to the Tables published in M. Barrande's excellent new work on Trilobites, it will be seen that this country also has produced a greater variety, or, rather, representatives of a greater number of groups from these early rocks than any other country. The species described included *Agnostus*, 5 species; *Arionellus*, 1 species; *Erinnys*, 1 species; *Holocephalina*, 1 species; *Conocoryphe*, 2 species; *Anopolenus*, 2 species; *Cyrtotheca*, 1 species; *Stenotheca*, 1 species; *Theca*, 2 species; *Protocystites*, 1 species, &c. The author also entered into a consideration of the range of the genera and species in these early rocks, and showed that, with the exception of the Brachiopods, Sponges, and the smaller Crustacea, the range was very limited. A description of the various beds forming the Cambrian rocks of St. David's was also given, and proofs adduced to show that frequent oscillations of the sea-bottom took place at this early period, and that the barrenness of some portions of the strata, and the richness of other parts, were mainly attributable to these frequent changes. Mr. Gwyn Jeffreys suggested that the term Polyzoa might be adopted in preference to that of Bryozoa, as being the more ancient term, and that the name *Proserpina* should not be applied to the new genus of Trilobites, as it had already been appropriated to a tropical form of land-shell.

Royal Geographical Society, December 11.—Major-Gen. Sir H. C. Rawlinson, president, in the chair.—A paper was read by Mr. Keith Johnston, "On the Rev. Thomas Wakefield's Map of Eastern Africa;" the subject being limited to the form of Speke's Lake Victoria Nyanza, which Wakefield's native travellers had decided to consist of at least two lakes.—Capt. R. F. Burton followed with a paper on "Lake Ukara or Ukarewe," in which he argued from the new information gleaned by Mr. Wakefield at Mombaz, and Captain Speke's own data, that Victoria Nyanza consisted of many separate lakes, and that it was a "Lake Region," and not a single lake.

Sunday Lecture Society, December 17.—"On the Optical Construction of the Eye," by Dr. Dudgeon. The early part of the lecture was occupied with a description of the optical construction of the eye. In order to ascertain the precise focal length of aqueous humour, the lecturer immersed his eyes in water, which, being of the same refractive power as the aqueous humour, extinguishes it as a lens. He then ascertained what power of lens was required to restore perfect vision under water, which he found to be affected by an artificial lens, whose focus was exactly $1\frac{1}{2}$ inch under water. He constructed a pair of spectacles fitted with air lenses, formed by very concave watch-glasses placed back to back, and united round their edges by a ring of wood or vulcanite. In this way he formed air lenses which had a focus of $1\frac{1}{2}$ inch in water, but which offered no obstruction to vision in the air. With these spectacles perfect vision both for near and distant objects below the water was obtained, and on coming to the surface these spectacles allowed of perfect vision in the air. He then explained the construction of the eyes of fishes and amphibia, which have no anterior aqueous lens, but only a nearly spherical crystalline lens. He next explained the mechanism of the accommodation of the eye from distant to near vision. He showed that this was not effected by any increase of the convexity of the anterior surface of the crystalline lens, as is generally supposed, but by a slight rotation of the crystalline lens from without inwards, whereby the focus of the crystalline lens was shortened to the degree necessary to throw the image of a near object accurately on the retina. Finally, he pointed out that some of the principal discoveries of modern physicists already existed in the eye. Thus, the principle of achromatic lenses by the combination of two lenses of different refractive power was seen in the eye when a water lens was combined with the crystalline lens; the discovery of Descartes, that an elliptical surface of a lens obviated spherical aberration, was also found in the eye; and Herschel's discovery that a combination of the meniscus with the double convex lens prevented spherical aberration also obtained in the eye.

Photographic Society, December 12.—A paper was read by Lieut. Abney, R.E., F.R.A.S., on albumen applied to photography. He first referred to the use of albumen as a substratum for collodion films. Taking different proportions of albumen and water, and iodising part of each, he found that with the best collodion process the iodised substratum as a whole gave neither increase nor diminution of sensitiveness, whilst with the uniodised substratum the sensitiveness was slightly diminished. He next pointed out the cause of blisters in developing dry plates, and traced them to the expansion of the albumen; the substratum rising from the glass at the smoother portions. He lastly touched upon the uncombined sulphur always present in albumen, as much as 1·2 grains being found in a whole sheet of paper, whilst but $\frac{3}{4}$ grain of metallic silver was found in prints of the same area. He argued from this that silver prints must fade, apart from the imperfect washing, unless the sulphur be removed. He recommended the makers of albumenised paper to try to do this, first forming albumenate of potash by the addition of potash to the albumen. The unprecipitated part contained the sulphur. This might be removed and the albumen once more dissolved by the addition of acid.—A paper on M. Dagrou's microphotographic despatches was also read, detailing the methods of preparation; as many as 50,000 messages were received in Paris during the Siege upon these films, conveyed to the capital by pigeons.

MANCHESTER

Literary and Philosophical Society, November 14.—E. W. Binney, F.R.S., president, in the chair. The president said that, on Friday the 10th inst., he observed at Douglas in the Isle of Man, a splendid display of the aurora borealis. At 8 P.M. it appeared as an arch of a greenish colour, extending from west to east, through the tail of the Great Bear. Afterwards, at ten o'clock, the same kind of arch was observed with another higher up, which ranged west and east through the Pole star. At this time numerous streamers and flashes of light of a green and yellowish-white colour flashed up from near the horizon to the zenith, from east, south, and west; those towards the west had a reddish hue. The sky was beautifully clear, and the light from the aurora was greater than ever previously observed by him.—"On the Origin of our Domestic Breeds of Cattle," by William Boyd Dawkins, F.R.S. There are at the present time three well-marked forms inhabiting Great Britain. 1. The hornless cattle, which have lost the horns which their ancestors possessed through the selection of the breeder. The polled Galloway cattle, for instance, are the result of the care taken by the grandfather of the present Earl of Selkirk, in only breeding from bulls with the shortest horns. The hornless is altogether an artificial form, and may be developed in any breed. 2. The *Bos longifrons*, or the small black or dark brown Welsh and Scotch cattle, which are remarkable for their short horns and the delicacy of their build. 3. The red and white variegated cattle, descended from the urus, and which have on the whole far larger horns. These two breed freely together, and consequently it is difficult to refer some strains to their exact parentage. The large domestic cattle of the urus type are represented in their ancient purity by the Chillingham wild oxen, as they are generally termed, but the exact agreement of their colour with that specified in the laws of Howel Dha proves that they are descended from an ancient cream-coloured domestic ox with red ears. The animal was introduced by the English invaders of Roman Britain, and was unknown in our country during the Roman occupation. The *Bos longifrons*, on the other hand, was the sole ox which was domestic in Britain during the Roman occupation, and in the remote times out of the reach of history it was kept in herds by the users of bronze, and before that by the users of polished stone. This is proved conclusively by the accumulations of bones in the dwelling-places and the tombs of those long forgotten races of men. The present distribution of the two breeds agrees almost exactly with the areas occupied by the Celtic population and the German or Teutonic invaders. The larger or domestic urus extends throughout the low and fertile country, and indeed through all the regions which were occupied by Angle, Jute, Saxon, or Dane; while the smaller *Bos longifrons* is to be found only in those broken and rugged regions in which the unhappy Roman provincials were able to make a stand against their ruthless enemies. The distribution, therefore, of the two animals corroborates the truth of the view taken by Mr. Freeman, that the conquest of Britain by the

English was not a mere invasion of one race by another, but as complete a dispossession as could possibly be imagined. The *Bos longifrons* lingers in Wales, after having once occupied the whole country, just as its Celtic owners still linger, while the urus is an invader just in the same sense as their English possessors. The *Bos longifrons* is of a stock foreign to Europe, and the urus was most probably domesticated in some other region by those Neolithic people. Both these animals have probably been derived from an area to the south and east of Europe, and were introduced by the Neolithic herdsmen and farmers at a very remote period.

Dublin

Royal Dublin Society, November 20.—Prof. R. Ball, M.A., in the chair. Mr. Maurice Cole exhibited and explained a working model of an improved seed sowing machine.—Prof. Edward Hull, F.R.S., read some notes of a recent visit to Vesuvius.—Dr Emerson Reynolds exhibited a new apparatus for gas analysis, and Mr. A. G. More exhibited some specimens of well-stuffed birds from the museum of the Society.

Royal Irish Academy, November 30.—Rev. J. H. Jellett, president, in the chair. The Secretary read a paper by M. Donovan on Earl Stanhope's alleged imperfections of the tuning fork; also for Dr. Whitley Stokes a paper on a fragment of Cormac's glossary.—Mr. G. H. Kinahan read a paper on and exhibited sketches of what appeared to him a new type of Clochân, observed in the county of Mayo, South of Louisburgh. The structure was composed of large flags inclining inwards to form sloping sides and roof, the very apex of which was covered by horizontal flags. He also exhibited a sketch of a form of cross observed in the same neighbourhood, and which was unlike anything he had ever seen.

Paris

Academy of Sciences, December 11.—M. J. Boussinesq read a paper on a remarkable property of the points where the lines of greatest slope of a surface have their osculatory planes vertical, and on the difference which generally exists at the surface of the earth between the lines of the ridge or the thalweg, and those along which the slope of the soil is a minimum.—M. Becquerel presented a third memoir on the discoloration of flowers by electricity, and on the cause of the phenomenon, in which he shows that electricity acts in this case by destroying the envelopes of the cells containing the coloured materials. Heat produces the same effect. The author remarked upon some general applications of these facts.—A paper on the diffusion and deleterious influence of mercurial vapours, by M. Merget, was read. The author disputed the conclusions of Faraday, founding his opposition upon experiments and observations which show that the vaporisation of mercury is a continuous phenomenon not even interrupted by the solidification of the metal, and that the vapours emitted by it are capable of great diffusion, nearly in accordance with the dynamic theory of gases. M. Dumas called attention to some observations on this subject by M. Boussingault.—M. C. A. Valson presented a note on the part played by space in the phenomena of solution, in which he discussed the contraction produced by the solution of various salts in water.—A note on different acoustic phenomena observed during balloon-ascents, by M. W. de Fonvielle, was read. The author remarked upon the fact that certain acute but very feeble sounds are often heard in balloon ascents, and accounts for the phenomenon by the reverberation of the balloon itself.—M. Serret presented a note by M. de Tastes on a new propeller, consisting of a plate or fan worked in the manner of the tail of a fish or whale. M. A. Barthélemy presented a memoir on the vibrations communicated to mercury and liquids in general, in which he described and figured the curious effects produced by these vibrations in vessels of various forms.—M. Delaunay read a note on the cold of the 9th December, containing some interesting observations on the range of this extreme cold over the Continent of Europe; and M. C. Sainte-Claire Deville presented a second note on the precocity of the cold in the present year.—M. P. P. Dehérain presented a memoir on the intervention of the nitrogen of the atmosphere in vegetation, in which he demonstrated by experiment the absorption of the atmospheric nitrogen by decomposing organic matters, and suggested that by this means nitrogen may be absorbed by the soil.—M. Wurtz presented a note by MM. C. Friedel and R. D. Sylva, on the action of chlorine upon chloride of isopropyl; and a note by M. E. Grimaux on derivatives of chloride of tollylene.—A note was read by M. Dubrunfaut on the combustibility of carbon, in which he maintains that carbon is combustible only in gases containing water; and another by M. F. Jean on the quantitative determination of glucose, recommending a process depending on the precipitation of metallic silver by protochloride of copper, prepared from the protoxide precipitated by glucose.—The deposits of phosphate of lime in France formed the subject of three papers, namely, a note on the composition of that recently worked in the Departments of Tarn-et-Garonne and of the Lot, by M. A. Bobière; an account of the deposits of Saint-Antonin and Caylux, in the former department, by M. Trutat; and a short note on the organic origin of the deposits in the Quercy, by M. Malinowski. M. Trutat described the structure of the deposits, and noticed the remains of certain mammalia found in them.—M. Daubrée communicated a note by M. P. Fischer on the existence of Lower Tertiary strata in Madagascar. These beds, belonging apparently to the great Nummulitic formation, occur on the west and south-west coast of the island. No nummulites have been found in them.—M. E. Blanchard presented a note by M. A. Milne-Edwards on the structure of the placenta in the Tamandua. The author describes this placenta as differing in various respects from those of other Edentata, and remarked that the diversity in the fœtal envelopes of those mammals would lead to the supposition that either the characters derived from them are not so important among the Edentata as in other groups, or the forms united in the Edentata are less nearly related than is generally supposed. He is inclined to the latter opinion.—M. Duchartre communicated a note by M. J. de Seynes on *Penicillium bicolor*, Fr.; and M. Robin presented a note by M. Rabuteau on the physiological properties of various chlorides.

BOOKS RECEIVED

English.—Nature; or, the Poetry of Earth and Sea: From the French of Madame Michelet (T. Nelson and Sons).—The Mountain: From the French of J. Michelet (T. Nelson and Sons).—Beautiful Birds in Far-off Lands: M. and E. Kirby (T. Nelson and Sons).—Text Books of Science; Theory of Heat: J. Clerk Maxwell (Longmans).—A Manual of Zoology: H. A. Nicholson; 2nd edition (Blackwood).—Comparative Metaphysics; Part II.: S. H. Hennell (Trübner).

Foreign.—(Through Williams and Norgate.)—Handbuch der vergleichenden Anatomie: E. O. Schmidt.—Mineralogische Mittheilungen, Jahrg. I., Heft 1: G. Tschermak.

DIARY

THURSDAY, December 21.

Royal Society, at 8.30.—Contributions to the History of Orcin. No. II. Chlorine and Bromine Substitution Compounds of the Orcins; Note on Fuedsol: Dr. Stenhouse, F.R.S.—On some recent Discoveries in Solar Physics; and on a Law regulating the Duration of the Sunspot Period: W. De La Rue, F.R.S., B. Stewart, F.R.S., and B. Loewy.

Linnean Society, at 8.—On the Anatomy of the American King-Crab (*Limulus polyphemus*, Latr.): Prof. Owen, F.R.S.

Chemical Society, at 8.

London Institution, at 4.—The Philosophy of Magic. 1. The Magic of Modern Conjurers: J. C. Brough, F.C.S.

FRIDAY, December 22.

Quekett Microscopical Club, at 8.

THURSDAY, December 28.

Royal Institution, at 3.—On Ice, Water, Vapour, and Air. No. I. Prof. John Tyndall, F.R.S.

London Institution, at 4.—The Philosophy of Magic. 2. The Magic of the Theatre: J. C. Brough, F.C.S.

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Errata.—P. 123, col. 2, line 36 from top, for "or D1 . . . or D2," read "on D1 . . . on D2."