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THURSDAY, JANUARY 5, 1871

PROGRESS OF SCIENCE IN 1870

THE year which has just come to a close has neither been characterised by many new and striking scientific discoveries, nor have any novel applications of Science to ordinary industry and manufacture attracted special attention. The work done has been more a strengthening of that of past years, and a confirming or a disproving of theories and experiments, than the inventing of new ones. In one branch of Science only has any great advance been made, and that, as we shall presently show, we believe to have taken place in Geology. But this advance is one somewhat overlooked at present; but still of so important a character that, when once fully recognised in all its bearings, it may tend to disprove much of the geological teaching of the present day.

Taking the various Sciences as much as possible separately, we will begin with ASTRONOMY. Here attention has been chiefly directed, as has been the case for so many years past, to the Sun. Since it is now generally understood that when once the nature of this vast self-luminous body is accurately made out, much light will be thrown on many now perplexing and strange phenomena, the Eclipse of the 22nd of December last was anxiously watched for, and all possible observations were taken here by those who were unable to take part in the Government Expedition to Spain and Sicily. It is to be hoped that the labours of this Expedition, in spite of accident both on land and sea, and the unsatisfactory state of the weather at the time of observation, will yet yield results of great importance. At any rate we may fairly congratulate ourselves that at last we have a Government which has shown itself in other instances besides this special one, not unmindful of the claims of Science and of the value of accurate scientific investigation.

Mr. Lockyer and Mr. Huggins have continued their spectroscopic observations of the Sun, and Prof. Zöllner has published a very valuable paper on the solar prominences, theorising very boldly as to the temperature and pressure at the Sun's surface;* while in America Prof. Young has worked with good results at the same subject. Before leaving this branch of our subject, we would mention that Mr. Proctor has published some novel views as to the constitution of the stellar systems, which, under the somewhat fanciful titles of "star-drift" and "star-mist" must be familiar to most of our readers.

Whilst the vast domain of ORGANIC CHEMISTRY has been still further widened by the innumerable workers who plunge into this branch of the subject and neglect the many untrodden paths in Inorganic Chemistry, nevertheless no special or important discoveries are to be chronicled, unless we may mention the beautiful process by which Indigo has been synthetically constructed by M.M. Emmerling and Engler, following closely on the artificial manufacture of Alizarine by M.M. Liebermann and Graebe.

MOLECULAR PHYSICS has occupied a large share of attention, and the discussion before the Chemical Society on the existence, or non-existence, of Atoms and Molecules, has only too clearly shown how doctors differ amongst themselves, and that the very foundations of a Science, considered so essential by some, are utterly repudiated by others. A very remarkable paper on the Size of Atoms, originally published in these columns (NATURE, vol i. p. 551) by Sir William Thomson, in which he gives four distinct trains of reasoning by which he arrives at a proof of their absolute magnitude, has attracted much attention, and has been translated and copied into most of the continental and American scientific journals. Dr. Thomas Andrews has also pursued his remarkable investigations on the Continuity of the liquid and gaseous states of matter. The death of Prof. Wm. Allen Miller, F.R.S., and Dr. Matthiessen, F.R.S., have left sad voids in the ranks of our English experimental chemists.

In BIOLOGY, the investigations of Prof. Tyndall, "On Atmospheric Germs, and the Germ Theory of Disease,"* have contributed to a clearer knowledge of the nature of some of the most virulent of our infectious diseases, and have caused those diseases to be studied in a much more scientific manner than before.

The theory of Spontaneous Generation, which has been very prominently before the scientific world for the last ten years, has, during the past year, been very strongly attacked on the one hand by Prof. Huxley, and defended on the other by Dr. Bastian and Dr. Child. In his Inaugural Address to the British Association meeting at Liverpool, Prof. Huxley gave a long review of all the researches on the subject, from the time of Spallanzani and Needham to the present day, and declared his belief, after carefully weighing the evidence on both sides, that all life has its origin in some pre-existing life, and that Spontaneous Generation, or, as he termed it, Abiogenesis, is not now proved to take place. The investigations of Dr. Bastian, originally intended to have been read before the Royal Society, were published instead in these columns, in a series of three long articles (NATURE, vol. ii. pp. 170, 193, 219), in which he gave the reasons for his belief that Spontaneous Generation certainly does occur. Feeling himself attacked and his experiments somewhat underrated by Prof. Huxley in his Address, he criticised it at considerable length, and detailed the results of some new experiments (NATURE, vol. ii. pp. 410, 431, and 492) which confirmed his previous deductions.

The Darwinian theory of Natural Selection has been attacked by Mr. A. W. Bennett and Mr. Murray,† and defended by Mr. A. R. Wallace and others; Mr. Wallace having also vindicated his claims to priority in this question, since he published many of the now-recognised theories and speculations on the subject of Natural Selection, at a time when he was resident in the East Indies, and entirely unacquainted with what Mr. Darwin had written on the same subject.

As respects GEOLOGY, during the past year the Government has continued its grants of money for the purpose of Deep Sea Dredgings, and at present the report of the most recent Expedition is anxiously looked forward to. The results of the Expedition in the autumn of 1869, as given to the public by Dr. Carpenter, Prof. Wyville Thomson, and Mr. Gwyn Jeffreys during the past year, have been of the greatest possible interest and importance. They found that on the same level, at the

* Translation in full, NATURE, vol. ii. pp 522-526.

* See NATURE, vol. i. pp. 327, 351, 499, &c.
† NATURE, Vol iii. pp. 30, 49, 65, and 154.

bottom of the deep sea, two different deposits are in process of formation side by side, each characterised by a distinct Fauna, and yet apparently produced under perfectly similar conditions of land and sea, area, depth of water, &c. On investigating this curious result, however, it was found that the temperature of the water circulating over these two areas is very different, and that this mere difference of temperature is capable of entirely changing the character of the fauna of the simultaneously formed deposits. Thus an entirely new element is brought into geological speculations, since it is shown that at one and the same time strata may be accumulated containing widely different organic remains. In addition to this, they have shown that the calcareous deposit known to us as chalk is now being deposited all over the bed of the Atlantic Ocean, and there are many weighty reasons for believing that this deposit has gone on steadily ever since the time during which we imagined the cretaceous rocks of the world to have begun and ended. Many organisms formerly supposed entirely extinct have been re-discovered in these deep-sea dredgings; and, in short, much has been done to show that our past geological reasoning requires thorough and careful revision. Prof. Gümbel's discovery of the existence of Bathybius and similar organisms at all depths, and stretching over an indefinite period of geological time, is of the greatest importance in relation to this subject. Prof. Agassiz, on the other side of the Atlantic, has published reports of the deep-sea dredging off the Florida Coast, and has stated that the results of his researches, and those of others, both English and Scandinavian, have convinced him that there is life all over the sea bottom, and that where evidence of marine life cannot be found, we are justified in calling in the agency of the sea to explain certain obscure facts. These conclusions cannot be without their important bearing on many commonly received geological theories.*

In BOTANY many very careful series of observations have been made in the physiological department. Among the most important we may mention those of Prillieux and Duchartre in France, confirmed by Dr. M'Nab in this country, that, contrary to the previously accepted hypothesis, plants do not absorb any appreciable amount of aqueous vapour through their leaves; and those previously announced by M. Dehérain, that the evaporation of water from the leaves of plants is due to sunlight rather than to heat, and proceeds independently of the degree of saturation of the atmosphere. Much attention has also been paid in Germany, Italy, and England, to the fertile field of the phenomena of fertilisation, opened out by Mr. Darwin's observations.

In METEOROLOGY there is no great advance to chronicle. It still remains a Science without a head, a chaotic mass of facts with no definite order or arrangement; for though many are working at this subject, and some valuable papers on the Origin of Winds and Storms have been published, still no definite progress can be ascertained.

The splendid appearances of the Aurora Borealis, visible all over the British Isles in September and October, have directed public attention to those unmistakeably magnetic phenomena, and to the connection which exists between their appearance, great magnetical perturbations, and large solar spots. They have been examined very frequently during the past year by means of the spectroscope, and there is distinct evidence of lines in the green and red portion of the spectrum, the latter presumably due to hydrogen. We would direct attention to our desire to publish a complete tabular list of the more remarkable meteorological phenomena of the past year, so as to be serviceable to observers in all parts of the world. To render this as perfect as possible, we would invite the kind co-operation of all those interested in the subject who can forward us any data.

We cannot conclude without noticing how much Science has lost during the latter half of the year just ended by the fearful struggle that has taken place between France and Germany, where each nation has brought into requisition all the resources of Science only to inflict as much injury as possible on the other. For nearly six months we have witnessed the sad sight of workshops shut up, laboratories closed, universities and public schools wanting both professors and students, and the friendly emulation of similar tastes and pursuits turned to the fierce rivalry of the sword. Science will have to deplore the untimely loss of many of her most attached workers, and their country will have lost those who would in happier times have done her as much honour at home as they have shown bravery in the field. Whilst the French Academy, shut up in besieged Paris, has brought the art of ballooning to its present state of perfection, so that now it is used as a means of communication with the outside world, the result of the subtle strategy of the Germans, and the scientific education they so generally possess, has been to give them advantages which have, to the present time, baffled their adversaries. J. P. E.

* During the past year all the most important papers on Deep-Sea Dredging have appeared in these columns, and we would refer our readers to Vol. i. pp. 135, 166, 267, 612, 657; Vol. ii. pp. 257, 513, &c.

THE INTELLIGENCE AND PERFECTIBILITY OF ANIMALS

The Intelligence and Perfectibility of Animals from a Philosophic Point of View. With a few Letters on Man. By Charles Georges Leroy, partly under the pseudonym of "The Naturalist of Nuremberg." (London: Chapman and Hall, 1870.)

THESE Essays, written nearly a century ago, seem to have been intended chiefly as an answer to the doctrines of those French philosophers who maintained that animals were merely animated machines, or, as it was expressed by Buffon, that "the animal is a purely material being, which neither thinks nor reflects, but which nevertheless acts," and that "the determining principle of the animal's actions proceeds from a purely mechanical influence, absolutely dependent upon its organisation." Our author, on the contrary, maintains that the mental faculties of animals are strictly comparable with those of man; that they remember, combine, and reflect; that they are capable of self-improvement; and even that they possess a true language fully adapted to their needs. To support his views he gives what we may term a generalised life history of several animals, such as the wolf, fox, stag, fallow-deer, and roebuck, which his position of Ranger of Versailles and Marly gave him ample opportunities of studying. The chief fault of these interesting sketches is, that they detail hardly any of the special observation on which the generalised statements are founded. We are, therefore, unable to tell how much is fact and how much inference; and, what is probably the result of careful life-long observation fails to produce that effect of reality which a more direct narrative style would have given to it. In a few cases, however, he gives us actual observations; as when he proves that animals can count, by stating the fact that in order to destroy crows, which were destructive to game, a hut was made at the foot of a tree where there was a nest, in order to shoot the old birds when they returned to their young. It was found, however, that after the first time the man was always watched into the hut, and the crows would not return till he had left it or till night. To deceive them two men went to the door of the watch-house, one entering and the other passing on, but the crows would not come. The next day three went and two passed on, but still with no effect; and it was not till five or six went and all but one passed on, that they were deceived, being unable to count so many.

M. Leroy appears to reject altogether what is commonly termed Instinct, maintaining that the word should be applied only to those acts which are the direct consequences of organisation, such as the grazing of the stag, or the flesh-eating of the fox; but not to the expedients to which those animals resort in the gratification of their natural wants, which are due to sensation, observation, memory, and experience. To the objection that many animals perform complex operations perfectly well without experience, and always in the same manner, he replies that in many cases the fact is not so. He maintains, for instance, that there is a distinctly perceptible inferiority in the nests made by young birds, thus anticipating the observation of the American Wilson; and further remarks that the best constructed nests are formed by birds whose young remain a long time in them, and thus have more opportunity of seeing how they are made He says that the nests of young birds are ill-made and badly situated; and that the defects of these first constructions are remedied in time, when their builders have been instructed by their sense of the inconveniences they have endured. He maintains that nests of the same species of bird differ as much as human dwellings, and that of a hundred swallows' nests no two are exactly alike; and he imputes to want of long-continued observation our failure to discover improvement in them; a want which curiously enough, has been remedied by M. Pouchet, who has found a decided improvement in the nests of swallows at Rouen during his own lifetime. Our author has also some excellent remarks on hereditary habit, as strikingly shown in the case of many of our sporting dogs, and which, he believes, in wild animals is often mistaken for instinct; and he concludes that "It is possible that the actions which we see performed by some animals, independently of the teachings of experience, are the fruit of a knowledge of very ancient date, and that in former times a thousand trials, attended with more or less success, have finally led to the attainment of the degree of perfection which we see manifested in some of their works at the present day."

The migrations of birds, also, he maintains are the result of no blind instinct, but of instruction handed down from generation to generation. He says, "Let us take the swallows as an example which every one can observe. In the first place, their departure is always preceded by assemblages, the frequency and duration of which can leave no doubt that their object is to effect all the necessary preparations for a voyage undertaken by creatures who have the faculty of sensibility, and of understanding one another, and who are united for a common purpose. The incessant and varied twittering which reigns in these assemblies, clearly indicates communications and orders, indispensable for the numerous offspring of the year. They must stand in need of preliminary instruction, constantly repeated, to prepare them for the great event. Frequent trials of flight are no less indispensable, and are often followed by a repetition of previous lessons, which makes our roofs and chimneys ring again. Assemblies of men who should speak a foreign language could not give more evident signs of a similar project. But there is a more convincing proof than this analogy that these migrations are not the result of a blind and mechanical inclination. When, at the time fixed upon for the flight, which cannot, owing to weather, be retarded without compromising the welfare of the whole species, some, and even a large number of indviduals, are too young to follow the rest, they are left behind and remain in the country. But it is in vain that they reach maturity; the supposed *attraction towards a certain region* dees not affect them, or too slightly to enable them to gratify it. They perish, the victims of their ignorance, and of the tardy birth which made them unable to follow their parents."

The letters on Man, which are curiously mixed up with those on animals, are neither so interesting nor so well reasoned. Their object is mainly to deduce the complex phenomena of human existence from the two principles of "the love of ease" and "*ennui*," which being antagonistic, lead men to all kinds of expedients to secure the one or escape from the other. These, with sympathy, which he considers the pre-eminently human emotion, are made to explain most of the facts of man's mental nature. The work is written throughout in a pleasing and simple style, and exhibits to us a loving student of nature who observed and thought for himself, and who, in many of his conceptions, was far in advance of the great philosophers of the last century, among whom he lived.

ALFRED R. WALLACE

OUR BOOK SHELF

Use and Limit of the Imagination in Science. By Prof. Tyndall. (London: Longmans and Co.)

THIS is a second edition of Dr. Tyndall's Discourse at the meeting of the British Association at Liverpool. To it is now appended his Address as president of Section A of the British Association at the Norwich meeting. This was analysed by Dr. Clark Maxwell at a later meeting (see NATURE, Liverpool Meeting, Address to Section A). There is also added a short Essay from the *Saturday Review*, with the quaint title of "Earlier Thoughts," suggesting the irrepressible "Country Parson," A.K.H.B. Another curious addition is a selection of favourable, unfavourable, and often ridiculous critiques of various parts of his discourse; which reveal the existence of a strange state of things in the arcana of editorial dens.

One or two trenchant notes are appended to the Discourse. That regarding "microscopists" and spontaneous germ-formation is especially deserved and well laid on. This race of pseudo-experimenters, who do not know the simplest necessities of accurate experiment, but who by mere assertion endeavour to bear down genuine scientific men, is really the class which does mischief alike to science and to the cause of religion. Dr. Tyndall is certainly not fairly censured by the so-called "religious" press, but does it ever deal either praise or censure fairly? His remarks on the Materialists at the end of the Address are quite conclusive as to the absurdity and injustice of calling him by that name. There is a verse in Scripture that would suit the case exactly, but we leave the problem as a puzzle to the Editor of the *Record*.

The Student's Manual of Comparative Anatomy and Guide to Dissection. By G. Herbert Morrell, M.A., B.C.L. (Oxford: Shrimpton.)

WE have received the only part as yet published of this treatise, viz., that relating to the Birds. Mr. Morrell gives with great care, in a tabular form when possible, a condensation of all the information to be obtained in such works as Huxley's "Lectures on the Skull," and "Classification of Birds," Wagner's, Siebold's and other manuals, and the "Cyclopædia of Anatomy and Physiology," supplementing this by extracts from his own notes of dissections in the anatomical laboratories of the Oxford University Museum, and of Professor Rolleston's lectures given there. It is proposed to issue an atlas of woodcuts borrowed from various works to illustrate the letterpress. The book will be found very convenient by students at Oxford and elsewhere, who are carefully studying the comparative anatomy of the Vertebrata. We must decidedly object, however, to the omission of one group of organs *entirely*—the reproductive. It is a concession to a strange prejudice, and really renders a good work incomplete.

E. R. L.

LETTERS TO THE EDITOR

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

The Eclipse Expedition

I WRITE you a brief account of the doings of the English Eclipse Expedition up to the present time, thinking it may be interesting to your readers.

After leaving London we proceeded direct to Naples, staying on the road only a few hours at Cologne, half a day at Verona, and a day at Rome. Everywhere on the journey the most marked kindness was shown to the Expedition as the representatives of English Science. From Ostend to Verona we never changed carriages, and there only on account of a slight breakage in the carriage itself. Never once were we asked for passports, never once were our instrument cases overhauled, or anything beyond mere personal baggage, at the custom houses. At Naples we stayed a day, which was occupied in taking the lids off all the cases, repairing one or two slight accidents which had befallen the instruments, and making plans for our distribution in Sicily.

At four o'clock in the afternoon of Wednesday, the 13th, we were all on board the *Psyche*, a despatch boat which had been lent by the Government to convey ourselves and instruments to Sicily. A delicious sunny afternooon, a sea as smooth as molten glass, a ship's company receiving us with the utmost kindness and hospitality, how could the evening not pass as merry as a marriage bell? I cannot retail to you all the jokes which passed, the lively chats and quiet strolls by moonlight, the polariscopes and spectroscopes pointed to the sea and sky, ere long destined to address their momentous questions to the Sun himself, now having their merits and demerits freely discussed by the *savants*; but you can imagine it all. At last we turned in to enjoy that repose which sea air always induces in landsmen.

At about six we were all called and told we were approaching the Straits of Messina, the very Scylla and Charybdis of our classical lore. In moonlight, soft, cool, and delicious, we beheld those rocks of Scylla and steered through Charybdis, though we felt it not, and so onwards in the rising sun through the Straits. After breakfast we held our (as we then thought) final council only about an hour before we were to separate for our various Sicilian stations. Scarcely had the council broken up, when the sad event occurred which was destined to alter all our plans. We were steaming along about ten miles north of Catania, the gigantic cone of Etna, capped with snow, appearing to rise from the cliffs of lava under which we were passing, when the ship struck on a treacherous sunken rock, shown in none of the charts. I cannot pass over this sad catastrophe without referring to the noble behaviour of our gallant commander, Lieut. Fellowes, to whose coolness and energy we owe the safe disposal of ourselves, instruments, and baggage at Catania, without any serious loss. Mr. Lockyer, at the request of the captain, immediately telegraphed to Malta, whence the *Royal Oak* came to the aid of the *Psyche*. More lately the admiral of the Mediterranean fleet (Sir Hastings Yelverton) has arrived in the *Lord Warden*, and anchored beside her, and I am glad to be able to say that if the present calm weather continues there is every prospect that the ship may be saved.

For this expedition it was found necessary to make Catania the head quarters instead of Syracuse, as had been originally intended, as there would inevitably have been too great delay in removing the heavier instruments to Syracuse to carry out the original plan. Since our arrival, we have received all possible help from our own and the Italian Governments. From the latter we obtained the use of the garden of the Benedictine monastery, where we are working, as well as guards at some of our stations. Both here and elsewhere the American and Italian observers join heartily with us, and to their co-operation we owe much.

The arrangement of the English parties are now as follows. At Catania we are strong in the spectroscope. Mr. Lockyer and Mrs. Lockyer are working with a large reflecting mirror fitted with a single prism, but with special adaptation for giving plentiful illumination, as well as for placing the hydrogen spectrum side by side with the spectrum obtained from the corona. Mr. Seabrokes has a large refractor fitted with a spectroscope of six prisms for examining the chromosphere. Mr. Pedler works with a small direct-vision instrument. Prof. Thorpe has mounted his apparatus on the top of a portico in the garden, and has been engaged the last few days in making out daily curves of the chemical intensity. To Mr. Vignolles, sen., and Mr. Vignolles, jun., and myself are committed the time and general observations. In addition to the above, we hope to have the assistance of several of the officers from ships in the neighbourhood, who will make sketches of the appearances, and help the observers in various ways.

Our next detachment, under the charge of Prof. Roscoe, has left us, intending to find a stage of observation as high as possible on Etna. It is confidently expected that, by leaving some seven or eight thousand feet of the densest atmosphere behind, delicate but important observations may be made which would be impossible at a lower level. This party is piloted by Prof. Sylvestre, of the University of Catania, who, in common with all the authorities here, has shown the utmost courtesy toward the Expedition. The spectroscope observations will be conducted by Prof. Roscoe, with the assistance of Mr. Bowen; the polariscope is under the charge of Mr. Harris; photographic arrangements under Dr. Vogel; while Mr. Darwin will sketch the appearances presented.

At Agosta we are strongest in the polariscope, this instrument requiring the longest possible duration of totality. The party is under the charge of Prof. Adams (of King's College, London), who, in the polariscope, is assisted by Messrs. Ranyard and Clifford. The only spectrocopist stationed here is Mr. Burton, and the sketcher, Mr. Brett. This party have been living under canvas for the last few days, and, I need hardly add, received the utmost kindness from a military detachment stationed there with them.

At Syracuse we are only represented by one photographer, Mr. Brothers, assisted by Mr. Fryer and by Mr. Griffith, who will take observations with his polarimeter.

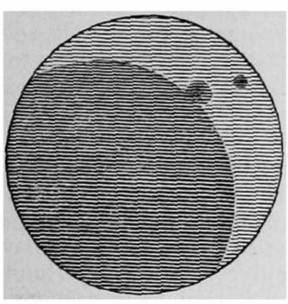
The authorities at Malta sent us here two sappers, thanks to whose exertions our observations have been erected with great rapidity, enabling us to station our instruments and make preliminary trials in the very positions they will occupy during the Eclipse. Since our arrival these preliminaries have kept us all extremely busy, and the ultimate success of our operations must in a great measure be attributed to the unremitting energy of Mr. Lockyer and Prof. Roscoe.

Up till to-day the weather has been superb, day after day just like the warm days we often have in England towards the end of June. The thermometer in the shade has reached from 75° to 80° F., while the barometer has been steady, but with just sufficient tendency downward, to fill with gloomy apprehension the less sanguine of our party. During this afternoon things do not look well, heavy clouds have been sailing over head, and have quite shrouded the upper five thousand feet of Etna; but we have yet hope, and all we can do is to wait patiently for about twenty hours, hoping then to get at least a bright gleam for the space of a minute and a half. If the sky so far favour us doubtless to-morrow will be an epoch in the history of astronomy.

L. CUMMING

Catania, Sicily, Dec. 21, 1870

The Eclipse



A REMARKABLE phase in the moon's passage across the sun was the perfect apparent contact of the limb of the moon with a sun-spot, of which the annexed figure is a fair representation. The noticeable thing was that the body of the moon itself and the sun spots were of so precisely the same tint that no trace of division was perceptible, one appearing to be merged in the other as long as the contact lasted.

Exeter, December 22, 1870 W. F.

Eozoön Canadense

THE letter of Mr. T. Mellard Reade on the subject of *Eozoön Canadense*, contained in your number for December 22, exhibits so complete a misapprehension of the state of our knowledge of that fossil, that I feel it necessary to break the silence which I have for some time imposed upon myself as regards this subject, in order that your readers may not be misled by the positiveness of his assertions.

1. Mr. Reade speaks of Eozoön, with the exception of the Tudor specimen, as having been obtained *only from metamorphosed rocks*. In reply to this, I have to state that the Eozoönal structure is *most* characteristically displayed in those portions of the Serpentine Limestone of the Laurentian formation which have undergone the *least* metamorphic change. In fact, the Calcareous lamellæ of the best specimens of Eozoön in my possession show less departure from the shelly texture with which I have become familiarised by the special study of the microscopic appearances of Shell, &c., for more than thirty years, than do the great majority of undoubted shells, corals, &c., contained in the least altered rocks of any geological period.

2. Mr. Reade assumes that the presence of the Serpentine lamellæ, which alternate with the Calcareous lamellæ, is itself an indication of metamorphic action. This position can only be sustained by those who are ignorant of the processes which can be shown to be at present going on upon the sea-bottom, and of which we have evidence in various geological periods; whereby the sarcodic substance of animals of various types of organisation, but especially of *Foraminifera*, undergoes replacement by siliceous compounds precipitated from sea-water during its decomposition. It was long since shown by Prof. Ehrenberg, that green sands of various ages, from the Silurian to the Cretaceous, are essentially formed of the *internal casts* of Foraminifera. The late Prof. Bailey (U.S.) first proved that the production of such internal casts is taking place at the present time. I have long had in my possession a set of beautiful internal casts of this kind, procured from the late Mr. J. Beete Jukes's dredgings on the coast of Australia. And quite recently I have obtained from Captain Spratt's dredgings in the Ægean a most remarkable series of such casts, which includes representations in green and ochreous Silicates, not merely of the sarcode-bodies of *Foraminifera*, but also of the sarcodic network that occupies the interspaces of the calcareous reticulation which I demonstrated twenty-three years ago (Brit. Assoc. Report for 1847) to be the basis of the skeleton throughout the class of *Echinodermata*. And Dr. Duncan has shown that a like process is taking place at the present time in the case of *Corals*; their animal substance being replaced by Silicates, whilst their Calcareous skeleton remains unchanged. No *mechanical* agency can account for this replacement. It is not effected by the percolation of Silicates in solution, under the "*hydrothermal*" action which Mr. Reade (following the lead of Messrs. King and Rowney) invokes as having been concerned in the production of the Canadian Eozoön. And I am justified by the opinion of several of our ablest Chemists and Mineralogists in the assertion that no agency save a progressive *chemical substitution* can account for the production of these wonderful models; the Silicates being precipitated from sea-water by the decomposition of the sarcodic substance which they replace and represent. Whether or not this doctrine be accepted, it may be confidently affirmed that whatever be the agency concerned in *their* production, the filling-up of the cavities of the Calcareous skeleton of Eozoön may be fairly accounted for in the same manner.

3. The most characteristic features of the best-preserved specimens of the Canadian Eozoön can thus be completely paralleled by those of analogous formations at present going on. Let us suppose that the North Atlantic sea-bed, instead of being covered by minute individualised *Globigerinæ*, were occupied by a shell-producing Rhizopod having the indefinite extension of *Bathybius*, and that its sarcodic substance came to be replaced (as in the instances just cited) by Silicates precipitated from sea-water; such a composite formation, elevated so as to become a terrestrial rock, *without any metamorphism whatever*, would be the precise parallel of the *Eozoön Canadense*. And just as, at the present time, the replacing minerals are not always the same, though always compounds of Silica, so the substituted material in Eozoön often consists of other minerals than Serpentine, always, however, being Silicates. In fact it was the *uniformity* of Morphological character, with *variety* of Mineral composition, that first led Sir William Logan—a geologist second to none in experience and judgment—to the suspicion of its organic origin.

4. Mr. Reade represents me as having made "the important admission" that "the several features in the structure of Eozoön (chamber-casts, canal-system, and proper walls) could be separately paralleled elsewhere," meaning, I presume, in undoubtedly Mineral structures. I have nowhere, that I can recollect, made any such admission: on the contrary, I have repeatedly argued that whilst the *combination* of structural characters in Eozoön affords the most unmistakeable evidence to those whose previously acquired knowledge enables them to appreciate their value, there are individual features which are inconsistent with any conceivable hypothesis of its purely Mineral character. Of these I may here state two, which I have always found to be most convincing to such as are familiar with the microscopic appearances of Minerals: *First*, the fact that the "canal-system" which traverses the Calcareous lamellæ passes *across* their cleavage-planes, instead of *between* them; and that this canal-system has precisely the same distribution, whatever may be the mineral which occupies its tubes, whilst its finest ramifications are frequently filled with calcite, as in the least-altered fossil Foraminifera. The idea that such arborescent expansions can have been produced by any kind of *infiltration* of one mineral into another, is thus, in the judgment of some of the most eminent Mineralogists of the day, *altogether untenable*; whilst the *precise parallelism* pointed out by Dr. Dawson, between the canal-system of Eozoön and that which I had shown to exist in the recent *Calcarina*, is no less satisfactory to Naturalists conversant with Foraminiferal structure. *Second*, the fact that the "Nummuline layer" or "proper wall" of the chambers consists of Calcareous lamella traversed by Siliceous aciculi, which sometimes lie straight and parallel, are sometimes curved, and sometimes penicillate; the precise equivalent to this being shown in the chamber-walls of recent Foraminifera, when the pseudopodia which occupied their tubuli during life have been replaced by Silicates. I assert again, on the authority of Mineralogists of the highest eminence, that such an arrangement cannot be shown in any undoubted mineral, and that it cannot be attributed to any physical agency. To liken this "Nummuline layer" to Chrysotile or any similar modification of Serpentine, shows a misapprehension of its essentially composite structure.

5. I cannot admit that the question of the Organic nature of the Canadian Eozoön (if question it be) is in the least degree affected by the occurrence of Metamorphic rocks presenting more or less morphological resemblance to it, in combination with undoubtedly Mineral characters. We should never think of deci-

ding the nature of Chalk, or of any more ancient Foraminiferal limestone, by the condition of its altered forms; the evidence of their Organic origin being supplied by the microscopic examination of specimens exhibiting the least evidence of change, and this evidence being not in any degree invalidated by the most complete mineralisation of particular examples. A large number of specimens of Ophicalcite have been submitted to me from various sources, as to some of which I have been able to say pretty confidently that they were originally Eozoic, but have been altered by subsequent metamorphism; whilst others do not present any feature whatever which would lead me to assign to them an Eozoic origin. To assume that these last (of which the Strath rock may be an example) are to be placed in the same category with the Canadian Eozoön, and thence to affirm that because they are purely Mineral productions, *it* cannot be Organic, involves a *petitio principii* by which it would be perfectly easy to prove the same thing of Chalk.—Let me illustrate my position by a parallel case. I have lately demonstrated* the existence of a Foraminiferal structure *departing much more widely than Eozoön does from any previously known type*, in a class of globular bodies from one to two and a half inches in diameter, occurring in the Upper Greensand; these having been previously regarded by experienced Geologists as mere Mineral concretions. Now, it so happens that the Magnesian Limestone of the North of England contains large masses of spherical concretionary bodies, bearing a strong *general* resemblance to *Parkeria* in internal structure as well as in outward form, but hitherto regarded, I believe universally, as Inorganic; and a reasoner like Mr. Reade would argue in this way:—"Because impartial geologists have pronounced the Permian concretions undoubtedly inorganic, the Greensand spheres are so likewise; and Dr. Carpenter's *Parkeria* becomes extinct as a fossil." But it likewise happens that the structural features which are most peculiar in *Parkeria* present themselves also in a remarkable living Foraminifer recently obtained from great depths in the *Porcupine* dredgings; so that the truly Foraminiferal nature of *Parkeria* cannot be a matter of the slightest doubt. And the only question now is, whether a careful microscopic examination of the minute structure of the Permian concretions may not afford, through its likeness to that of *Parkeria*, more or less definite indications of *their* Organic origin, obscured by subsequent metamorphism. The application is obvious.

6. I am equally unable to admit that if a rock presenting all the characteristic features of the Canadian Eozoön were to be found shading off into one containing characteristic Liassic fossils, this would afford the least tittle of evidence against the Organic character of the former. As the *Lingula* of the ancient Siluria has, in the judgment of our most eminent Brachiopodist, come down unchanged to the present time, and as even the same *varietal* modifications of Foraminiferal types were existing in the Triassic period as now in the Mediterranean, I see no reason why we should limit Eozoön to the Laurentian epoch. When this subject was last discussed at the Geological Society, I ventured to say that it would not in the least surprise me to find Eozoön, or something very like it, now existing on the deep-sea-bottom; and the notion was not treated by any of the eminent Geologists then present as having any *a priori* improbability. Since that time, the Coccoliths first discovered by Prof. Huxley, and the Coccospheres first observed by Dr. Wallich, in the Globigerina-mud, and afterwards recognised by Mr. Sorby in Chalk, have been detected by Prof. Gümbel in Silurian rocks; so that it is clear that the Biological condition of the deep sea has changed much less in vast periods of Geological time, than has that of shallower waters; whilst the probability has now almost reached a certainty that Rhizopodic life has been at least as largely concerned in the production of Calcareous deposits in earlier Geological periods, as we know it to have been in the later.

7. Though Mr. Reade "feels assured that whenever impartial Geologists take the question up, the fossil itself will become extinct," his assurance is not borne out by the judgment of the large number of impartial Zoolgists, Continental as well as British, who have satisfied themselves, by a careful examination of my series of microscopic preparations, of the Organic nature of the Canadian Eozoön, and have authorised me to express their entire accordance in my interpretation of its phenomena. An eminent Professor in one of our own Universities used this emphatic expression—"The matter seems to me not to admit of *hesitation*, much less of *doubt.*" My last Continental visitor, Prof. Carus, who is well known to possess a comprehensive and practical knowledge alike of Zoology, Palæontology, and Mineralogy—assured me that having come without any prepossession on the subject, he left me with a full conviction of the justice of my views. The respect paid by such Naturalists as Professors Milne-Edwards, Carus, Lovén, Van Beneden, and Escher von der Linth,—typical representatives of the Science of France, Germany, Sweden, Belgium, and Switzerland,—to my own judgment in a matter as to which *they* regard the special studies of a third of a century as giving me some claim to authority, may console me for the contemptuous repudiation of microscopic evidence in which Mr. T. Mellard Reade has thought it becoming to indulge. I am far from expecting, however, that anyone should pin his faith upon my own *ipse dixit*, supported though it be by the entire concurrence of my three fellow labourers in Foraminiferal investigation, Messrs. Parker, Rupert Jones, and H. B. Brady. And if it be thought that the decision of any tribunal of really "impartial geologists" is likely to carry more weight with the scientific public than that of the authorities I have cited, I am perfectly willing to go into the question with them; provided, however, that such tribunal consists of, or at any rate includes, men who are sufficiently conversant with the Microscopic appearances of undoubtedly Organic structures, to be able to recognise such appearances when they see them. One of the strongest opponents of the Organic origin of Eozoön designated as "an agatized mineral" a section of a *recent* Nummuline shell, that exhibited a minute tubulation corresponding with that of the nummuline layer of Eozoön, which he had just before characterised in the same manner. Another attributed the production of a perfectly mineralised internal cast of *Polystomella* in green silicate, from Capt. Spratt's Ægean dredgings, to the working-in of mud. And a third has abstained from even looking at my specimens, though I have repeatedly expressed my willingness to give him an opportunity of examining them. Such are *not* the judges before whom I would consent to plead the cause of *Eozoön.*

WILLIAM B. CARPENTER

* See my description of *Parkeria* in the Phil. Trans. for 1869.

Mimicry *versus* Hybridisation

ALLOW me space for a word or two in reply to Mr. Wallace and Mr. Butler's observations on my papers on Mimicry and Hybridisation.

There is only one point in my argument to which they have taken exception, and although, of course, I am not therefore entitled to assume that their silence on other points means assent, I may at least infer that in their view the point objected to is most open to assault, and that if it were established, the reader may regard the rest with increased confidence.

The objection is that the instances of hybridisation in plants which I have cited as parallel to the cases of mimicry between the Danaids and Nymphalids were merely cases of hybridisation between species of the same genus or allied genera, whereas these butterflies are more distantly related. The question, as thus put by these gentlemen, resolves itself into a question of comparative degrees of affinity, and Mr. Wallace, with his usual skill, tries to throw the onus of proof from his shoulders to mine. But with all submission we shall keep it where it naturally lies. He puts it that my argument rests on the assumption that hybridisation can take place between different orders or families, and quite logically (supposing me to have done so) objects to my making any such assumption in regard to insects, seeing that nothing of the kind has ever been observed in other animals or in plants. But I rest my argument on no such assumption. I ask no other measure for insects than is given to plants. It is Mr. Wallace who makes the assumption that the amount of difference between Lepidoptera has a different value from that attached to it in any other organic beings. It is he who claims for differences which in any other creatures would be regarded as no more than specific the importance of generic or ordinal. But however this may suit the artificial classification of the systematist, we cannot allow it when we come to deal with the actual workings of nature.

I am not surprised that either Mr. Wallace or Mr. Butler should take what appears to me an exaggerated view of the dignity and position of their favourite group. It is human nature that any subject to the study of which we have devoted ourselves should assume in our eyes larger proportions than it does in the eyes of those who take a wider but less detailed view of it. Hence we see Mr. Butler comparing the Lepidoptera to birds, as if it were a kingdom of equal magnitude, and seeking for equivalents for such groups as the hawks and doves within its limits. Whereas it seems to me that the truer parallel is between the whole class Insecta and Birds, and that the equivalent groups for hawks, doves, &c., are to be looked for, not in one of the sections, but in the whole of the class. He looks for both hawks and doves in the *Lepidoptera*. I find nothing but doves. If you want hawks you must go to the dragon-flies, which are their equivalent; and, of course, if we are only dealing with doves, there is nothing in the known phenomena of hybridisation opposed to such a cross having taken place.

It is impossible in the brief space that you would allow me, even to glance at the many arguments that I could adduce to show that this is the true position of the *Lepidoptera*. I hope to do so elsewhere. But I would only remind entomologists, especially lepidopterists, of the trifling characters on which their genera have been established, and how difficult it has been to find any generic characters at all. This is frankly acknowledged as the great difficulty attending the study of *Lepidoptera*, consequently characters which would never for a moment be looked on as generic in any other group of animals, are there allowed that value. If any specialist in another group objects, what is the answer? "We have no better characters, and we must do the best we can with the slight ones we possess." Quite right, in a systematic point of view. If the species of doves came to be reckoned by thousands, the ornithologist would just have to do the same thing; but that would not alter the position of doves in the animal kingdom—they would still bear the same relation that they do now to hawks, and be equally open to hybridisation among themselves, indeed, more so; for such great numbers of one type would be a presumption in favour of every mode by which species could be increased having been resorted to; and this by the way is an additional indirect argument in favour of hybridisation sometimes taking place among *Lepidoptera*.

Of course, I do not mean to say that there is nothing more than specific distinction between the Danaids and Nymphalids. I recognise them as good genera, but only as genera sufficiently nearly akin to allow of hybridisation taking place between them—and *ecce signum*—the mimics in question partaking of the characters of each in all respects as other hybrids do.

ANDREW MURRAY

67, Bedford Gardens, Kensington, Dec. 30, 1870

Measurement of Mass

THE favourite definition of *mass* in the text-books seems to be that the mass of a body is the *quantity of matter* it contains. If we had to do with but one kind of matter this would be intelligible, but I am at a loss to know what is meant when it is said that a piece of cork contains as much matter as a piece of lead. The only satisfactory method of explaining what is meant by the mass of a body, is to define it as a constant belonging to the body, which expresses the proportion between the force (measured statically) acting upon it and the acceleration produced; that every body has such a constant is the result of experiment. The mass of a body has no necessary connection with its weight. We employ weight to measure mass simply because gravity is a convenient constant force. If then we adopt a pound as our unit of *weight*, and use g to denote the force of gravity in reference to a foot and a second as the units of length and time, our unit of mass becomes the mass of g pounds, and this is not variable, although the unit of weight employed is variable; since if a true pound, as determined at London, were carried to the North Pole, it would weigh more than a pound, precisely in the proportion in which gravity at the Pole is greater than gravity at London.

THE REVIEWER OF EVERETT'S "DESCHANEL"

PHOTOGRAPHIC PROCESSES OF THE PRESENT DAY

THE last two or three years will certainly mark an era in Photography, for not only have several novel and important printing methods been discovered during that period, but other processes of less recent origin have of late been so elaborated and improved as to have become at the present moment practical and easy of manipulation. All of these are, without exception, based upon the action of light upon the bichromates of potash and ammonia; in no single case is the use of a silver salt involved—the agent employed for securing the photographic image in ordinary paper printing—and this is, in truth, a point whose value cannot be too greatly insisted on; for the silver print, be it washed and freed as thoroughly as possible from any deleterious bodies, will always suffer, more or less, from attacks of an impure atmosphere, the delicate metallic film of which the image consists being peculiarly liable to change, from the sulphur compounds and other impurities not unfrequently contained in the air we breathe. And even those silver pictures which do not at first show actual traces of fading or discoloration, will very soon be found, on careful examination, to have parted with some of their original brilliancy, and to lack the pristine freshness which always characterises newly-produced albumenised prints.

It is a great step onwards, then, to have at our disposal practical processes in which the employment of silver may be altogether dispensed with, by the substitution of another material of a more permanent character, either in the form of a chromium compound, or, what is better still, in the shape of gelatinous or greasy ink; and so clear and promising does the photographic horizon appear just now in this direction, as to leave little ground for doubting that before long the practice of printing in silver will be generally abandoned.

All recent printing processes rest, as we have before said, on the action of light upon the bichromates, and here we would parenthetically refer to a simple and familiar experiment which will help very materially to simplify our subsequent remarks. The well-known plan pursued by school-boys for printing fern-leaves and other objects by the aid of the sun, will readily be called to mind by many of us, and this simple manipulation it is that forms the groundwork of the whole series of inventions before us. A sheet of ordinary paper, which has of course been sized, or, in other words, received a thin coating of gelatine, is rubbed over with a solution of bichromate of potash; the latter, as we know, when mixed with any organic body renders the same sensitive to light, and the sizing or gelatine upon the paper becomes in this way endowed with excitable properties. Having been dried in the dark, our sheet of paper is next placed in the sun with the fern-leaf, or other object to be copied, pressed down upon it, and the light acting upon all such portions of the sheet as are not covered up, browns the gelatine there and renders it insoluble; the sizing underneath the leaf, and screened therefore from the light, escapes this reaction and remains soluble, and this, on the printing being completed and the paper washed in water, is at once dissolved away, there remaining a white image of the leaf upon a brown ground composed of bichromated gelatine rendered insoluble by the sun's rays. This experiment may be regarded as the key to the whole question of photographic printing, and by bearing it in mind the reader will have no difficulty in at once comprehending the various inventions of the kind just now being made public.

The first method claiming our attention is the so-called carbon process. Photographic printing of this nature in one form or another has been carried on probably for upwards of fifteen years; but in its experimental stage the mediocre character of the results furnished by it were such as to deprive the system of any material support from photographers, and until, in fact, Mr. J. W. Swan, of Newcastle, made known his method, no easy or reliable *modus operandi* can be said to have existed. The plan followed by Mr. Swan was to prepare a warm solution of gelatine and bichromate of potash mixed with some finely divided pigments, such, for instance, as Indian Ink, and apply this mixture in the form of a coating to a sheet of paper, so that when dry, the tissue, as it is called, assumed the form of a thin, black cake with a paper backing. This sensitive tissue was placed under a negative to print in the ordinary manner, the light penetrating in parts to a greater or less degree, and thereby rendering the surface partially insoluble. On removing the tissue from the printing frame, it might, if it were desired, be forthwith washed to remove the soluble portions (as in the case of the fern-leaf experiment), but by so doing the picture would be hard and deficient in detail, and therefore a slight modification is here instituted. Instead of washing away from the face of the tissue, the operation is pursued from the back, the film being in the first place cemented face downwards upon a sheet of india-rubber, and in this condition put into a tank of warm water. The original paper backing of the tissue is in this way at once washed off, as is also every part of the gelatine mixture not rendered insoluble, which latter, constituting the image itself, remains attached to the india-rubber sheet before mentioned. The picture is now sufficiently developed, and indeed quite perfect, except that it is reversed to our view, for we are looking at it, it must be remembered, from the back; this defect is, however, easily remedied by attaching to the image another sheet of paper by means of gum or gelatine, and then dissolving off the india-rubber facing by means of benzole or turpentine, when the finished image is obtained resting upon a support of white paper. The object of washing the carbon tissue from the reverse side and not from the front, or surface exposed to the sun, is to secure the finer details in the picture by fixing at once to a basis such portions of the tissue as may have been but very slightly acted upon, and thus prevent them from being ruthlessly washed away when placed in warm water.

The actual composition of pictures produced in this manner consists of gelatine, pigment, and a stable chromium compound, the gelatine being in a fixed or tanned condition, by a subsequent immersion of the prints in a solution of alum, and thus there is every reason to believe in the permanent character of such prints. A more simple and ready method of carbon printing has been lately invented by Mr. Johnson, and termed the Autotype process, but the principles involved therein are nearly the same.

Passing from printing in permanent pigments, in which, as in silver printing, the aid of light is necessary for the production of each separate picture, we come next to photo-mechanical methods. Of these there may be said to be two kinds partaking of the nature of lithographic and engraving methods. Of the first description we may mention three modes of working, all of which are capable of yielding very creditable specimens of printing: these are Albert-type, the Lichtdrück process, and Edwards's collographic method. The three inventions, which differ from each other and from minor plans of a similar nature only in a few details, are all based on the same principles. A sheet of patent plate glass is in the first place coated with a thick solution of bichromate of potash and gelatine; this film on drying is placed face downwards upon a sheet of black paper in the sun, and in this way the light rays penetrate the glass and act upon the sensitive compounds adherent to its under side. The bichromated gelatine becomes insoluble and firmly cemented to the glass, except on the exterior surface, for the black paper upon which this has rested absorbs the rays and leaves the outer film of gelatine still in a soluble condition. A second coating of the sensitive gelatine mixture is now applied to the former one, to which it adheres perfectly, from the fact of the first surface being unchanged, and upon the second coating an image is printed by means of a negative in the ordinary manner. After printing, the progress of which, by-the-bye, may be watched through the glass, instead of washing the surface and dissolving out all the soluble parts, a sponge dipped in cold water is simply rubbed over it, the moisture being absorbed by the gelatine where it has not been acted upon by light, and is capable therefore of swelling out; those portions of the film, on the other hand, which have been rendered quite insoluble and hard, are unable to take up any water whatever, and remain untouched therefore by the action of the sponge, while other parts again, slightly exposed to light, absorb water just to that degree to which they have remained soluble. In this condition an inked roller is passed over the surface, in the same manner precisely as in lithography, the greasy ink adhering to all the insoluble surfaces (where no water is), and to the other parts in a greater or less degree according to the amount of water present in those places. Thus the gelatinised glass is treated in every sense like a lithographic stone, being moistened, inked, and pressed in the same manner; the resulting print, however, is generally finer than that obtained in ordinary lithography, as the graining of a stone surface is always somewhat coarse, while in the present instance the breaking up of the ink by the minute pores of the gelatine impregnated with moisture is of an exceedingly fine character. Many thousand prints may be pulled off a printing block of this kind before it is destroyed, as the double layer of gelatine imparts a yielding nature to the plate which is not easily damaged; in Germany, in England, and also, we believe, in America, this process of photographic printing is extensively practised.

But by far the most important of all methods yet discovered is the Woodbury engraving process. So simple, and at the same time so perfect in its work, a casual observer cannot but fail at once to appreciate its value. A thin sheet of gelatine is sensitised by impregnation with bichromate solution, and exposed to light under a negative; subsequent immersion in warm water removes the soluble portions from the surface, and we have then a thin gelatine plate upon which the image is represented, more or less, in relief. This matrix, as it is called, is hardened by treatment with alum, and placed when dry in a hydraulic press, in contact with a plate of type metal. Subjected to considerable pressure the metal plate takes the impression of the relief, and thus becomes in every sense an engraved plate, in which the darkest shadows are represented by the deepest hollows, the half-tones by slight undulations, while in the high lights there is no depression at all. The printing off of copies from this engraved plate is very ingeniously contrived. A little pool of transparent gelatinous ink is poured upon a sheet of white paper, and the metal plate is brought down upon the same with some pressure; all superfluous ink is at once pressed out, and after a pause of a few seconds to allow the warm ink to cool and to become set, the plate is again raised, and a beautifully shaded print is the result, in which the shadows and half-tones are formed by layers of ink of different thicknesses. For inasmuch as the ink is of a transparent character, and there is more or less of it deposited upon the paper according to the depths of the hollows in the engraved plate, so the half-tones are rendered with perfect gradation and fidelity, while in the high lights almost all the ink having been pressed away and removed, there remains nothing but the white paper which forms the basis of the print.

By printing at once from many plates (for a gelatine matrix will yield several dozen of them), photographs may be printed at the rate of some thousands daily, without of course the assistance of light in any way. Moreover, the productions are of so perfect and delicate a nature as to be confounded actually with silver prints, being at the same time absolutely permanent. We are glad to say that this method is also being worked practically and extensively in this country, as also in France and America, and will, without doubt, be the process of the future; for it is indeed the only mechanical process by means of which photographs may be rapidly produced, possessing the same degree of excellence as the beautiful, but alas! too fleeting, albumenised pictures.

H. B. P.

PHYSIOLOGICAL LABORATORIES IN GREAT BRITAIN

THE introductory paragraph with which you bring under the notice of your readers the very excellent description of the Physiological Laboratory at Leipsic by Dr. H. P. Bowditch, of Boston, begins with the phrase: "In England we have absolutely no Physiological Laboratory open for students." As this statement appears to me to admit of misconstruction, as leading to the inference that the present neglect of physiology in England is entirely due to the want of opportunities, it seems desirable to place before those of your readers who are interested in the subject, the actual position of this country as regards facilities for this kind of research.

There is, at all events, one institution in London, viz. University College, in which, for many years past, it has been possible for any man desirous of conducting experimental inquiries in Physiology or Pathology to do so; in proof of which I may refer to the experiments of the scientific committees of the Medical and Chirurgical Society on Apnæa and on subcutaneous injections; to my own experiments on the transmission of cholera to the lower animals, and on the influence of the respiratory movements on the action of the heart—all of which inquiries were made in the Physiological Laboratory of University College by persons unconnected with the Institution. In this enumeration I make no reference to the more abundant similar work which has been done by professors and students of the College, because my only object is to show that, as regards London at all events, it is many years since it could be said with truth that there was no Physiological Laboratory open to students.

At the present moment there are laboratories connected with one or two of the principal medical schools in this country to which students are admissible. In Edinburgh the Physiological Laboratory is fitted with all the instruments and appliances for research which are to be found in the laboratories of Germany; and for some time the students have been superintended in their studies by practical teachers, thoroughly versed in those methods of exact research which have been lately introduced into vital physics. In addition to the Physiological Laboratory, which is under the direction of Professor Bennett, the Professor of Medical Jurisprudence (Dr. Maclagan), and the Professor of Materia Medica (Dr. Christison) severally open their laboratories without charge, only requiring those who profit by them to meet the current expenses of research. Further, Dr. Arthur Gamgee, Lecturer on Physiology at the Royal College of Surgeons, has opened a new laboratory in which several separate inquiries are now being carried on. In Edinburgh, therefore, little can be said of want of opportunities; and here again the best proof of the existence of the means is to be found in the results attained, *i.e.* in the laboratory work actually performed by Edinburgh students during the last few years, as, for example, the researches of Dr. Fraser on Calabar Bean, of Dr. McDougall on the action of phosphorus, of Dr. Paton on the active principles of Broom, of Dr. Brunton on Digitalis, of Dr. Keith Anderson on the excretion of urea in typhus, of Dr. Young on the quantity of iron in bile, of Dr. Rutherford (now of King's College) on the vagus nerve, and others which might be mentioned, all of which possess the essential characteristics which constitute scientific value, though differing very considerably from each other in completeness. That so much has been already accomplished affords encouragement for the hope that as soon as the obstacles which still exist in the way of the student have been removed, Edinburgh will stand behind very few of the German schools of medicine in scientific productiveness. Of these obstacles, the most serious is that of expense. The large fees which are demanded, particularly for the physiological laboratory, have restricted the number of workers, the best of whom, as your correspondent, Prof. Stricker, so well pointed out in one of his recent communications, are not to be found either in England or Germany among the well-to-do.

It must be admitted that at the present moment our great London Schools are behind those of Edinburgh, as regards means of physiological and pathological research. There are, however, good reasons for anticipating that in a very few years the aspect of things will be entirely changed. In King's College a physiological laboratory already exists, to which I understand students are admitted. I am not aware to what extent it contains the necessary accommodation, but it is certain that those who work in it have at all events the supervision and aid of a teacher thoroughly conversant with the art of investigation, At Guy's, Bartholomew's, and St. Thomas's I hear that similar improvements are at all events in contemplation. At University College, which, as has already been said, has long afforded opportunities not to be had elsewhere, these have been much extended during the present year. The Physiological Laboratory now consists of three rooms, one of which, of large size, is devoted to students, one is employed as a place of research and for the preparation of materials for demonstration, while the third is used for such special purposes as require a separate apartment.

The movement towards a more practical method of teaching the theory of medicine, of which the facts I have referred to afford evidence, is a new one. In the course of very few years it may be confidently anticipated that great progress will be made, and that although we cannot in so short a time hope to compete with the splendid institutions which exist at Leipzig or at Breslau, where spacious buildings, costly instruments, and abundant material, are freely placed at the disposal of the student without charge and without respect to his nationality, or any other consideration except his competency, we may hope to produce results which may be of equal importance for the advancement of Science.

At the present moment, the want which perhaps presses even more than that of laboratories, is that of *workers* in physiology—that is, of men already drilled in chemistry and physics, and prepared to devote a few years of their lives to continuous physiological or pathological research. The reason why such men are wanting is no doubt in great measure that hitherto the opportunities for work have been denied them. Another, and perhaps more efficient reason, is that the statement which is so often repeated in lectures, that medicine is based on physiology, is not really believed or accepted. Consequently, young physicians, instead of devoting their time and energies to research—whether conducted in the hospital wards or in the laboratory—spend the best years of their lives in the collection and exhibition of curiosities from the dead-house (miscalled pathology), in the compiling of masses of useless statistics, or in the performance of other drudgeries, as little conducive to their own improvement as to the advancement of medicine.

If it were not for the want of this scientific conviction, or, if I may venture to use the expression, scientific *faith*, the study of vital physics would make rapid progress in England, notwithstanding all the material obstacles which stand in their way. A dozen years of good work would place us again side by side with Germany, instead of being, as now seems possible, in danger of being overtaken by America.

This country still maintains its superiority over all other European countries in respect of medical and surgical skill, and has reason to be proud of it. But it is to be borne in mind that the men who exercise that skill were for the most part educated at a time when we could also compete with Germany in Science. As Science advances, its influence on practice, now so difficult to trace, will increase. If we continue to undervalue it as we have done, shall we not also eventually lose our practical preeminence?

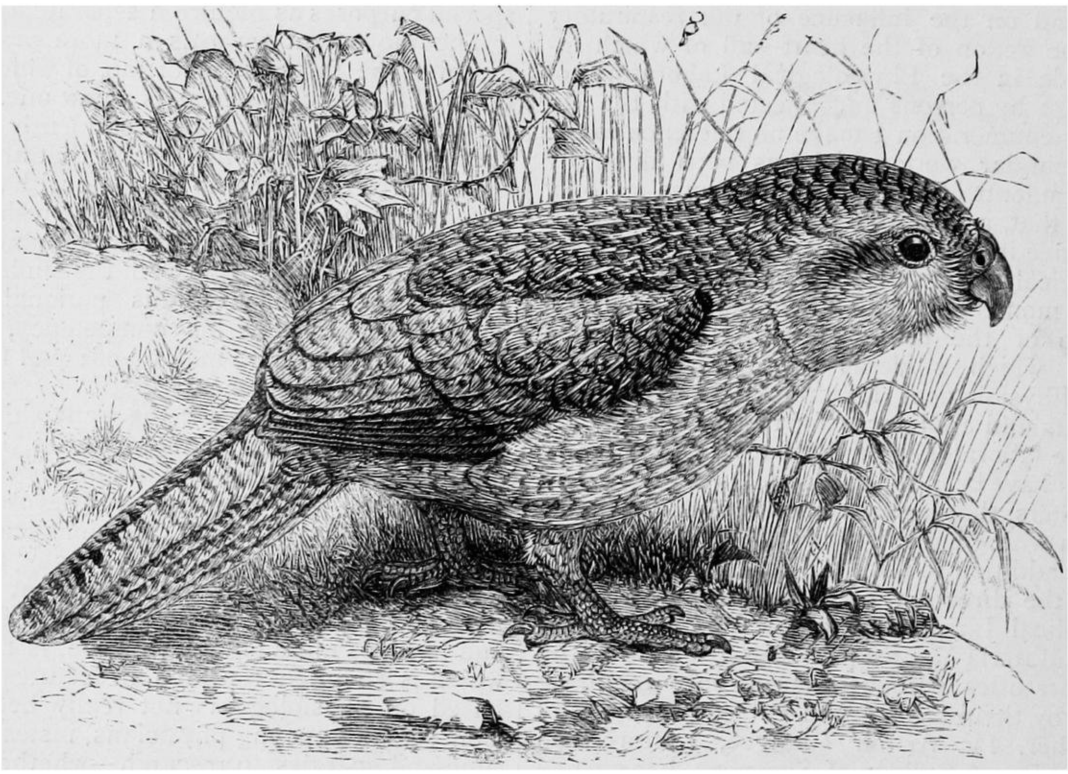
J. BURDON SANDERSON

NEW ZEALAND ANIMALS IN THE ZOOLOGICAL SOCIETY'S GARDENS

AT a recent examination for the Natural Science tripos at Cambridge, one of the pieces of information asked for in vain by the examiners was, I am told, "some account of the chief peculiarities of the Fauna of New Zealand." It so happens that the series of animals of our antipodean colony in the Zoological Society's Gardens is at the present moment unprecedentedly complete, and had the young gentlemen of Cambridge paid them an attentive visit, would have furnished ample materials for a proper answer to the examiners. I propose, therefore, to offer a few remarks upon them, and to make them the basis of some sort of answer to the question above mentioned.

Mammals in New Zealand there are none, except introduced species and bats, so it is not in this class of the animal kingdom that the visitor to the Society's gardens will find examples of New Zealand animals. There are, it is true, traditions and reports of some sort of "native rat"* having been occasionally met with, but no one has yet been able to produce a specimen of it, and after all, when captured, it may turn out to be only a stray individual of that cosmopolitan errant *Mus decumanus*. There are also marine mammals, both seals and whales, to be met with in the surrounding seas. But no terrestrial mammals, except bats, are indigenous to New Zealand, and for a country of such size, and situated in such latitudes, this is certainly a very remarkable and indeed unparalleled peculiarity.

With the next class of Vertebrates, however, the case is quite different. Bird-life, although according to the general evidence of the settlers not individually abundant, cannot be said to be badly represented in New Zealand. Dr. Otto Finsch, to whom we are indebted for the most recent summary of the birds of these islands,† gives 155 as the number of well-determined species hitherto met with, and there are, doubtless, a few more still to be made out, which will probably not long escape the grasp of



KAKAPO, OR GROUND PARROT

several excellent naturalists who are now at work on the fauna of their adopted country. Of these 150 species, one third, or perhaps rather more, are found only in New Zealand. But what makes its bird-life still more peculiar, is that the greater part of these 50 species belong to some 17 or 18 generic forms which are quite unknown elsewhere. And several of these forms (such as *Heteralocha*, *Strigops*, *Apteryx*, and *Anarhynchus*) are of the most bizarre and extraordinary character.

Of the Huia bird (*Heteralocha gouldi*) I have already given a notice and figure in the pages of NATURE.* I am not aware that there is any other instance in the class of birds in which the difference between the bill of the two sexes is so great, though something of the same sort is exhibited in the Humming-birds of the genera *Grypus* and *Androdon*. The Huia bird in the Zoological Society's Gardens has recently moulted off its worn and injured plumage, and is now in excellent health and condition.

* Vol ii. p. 146, June 23, 1870.

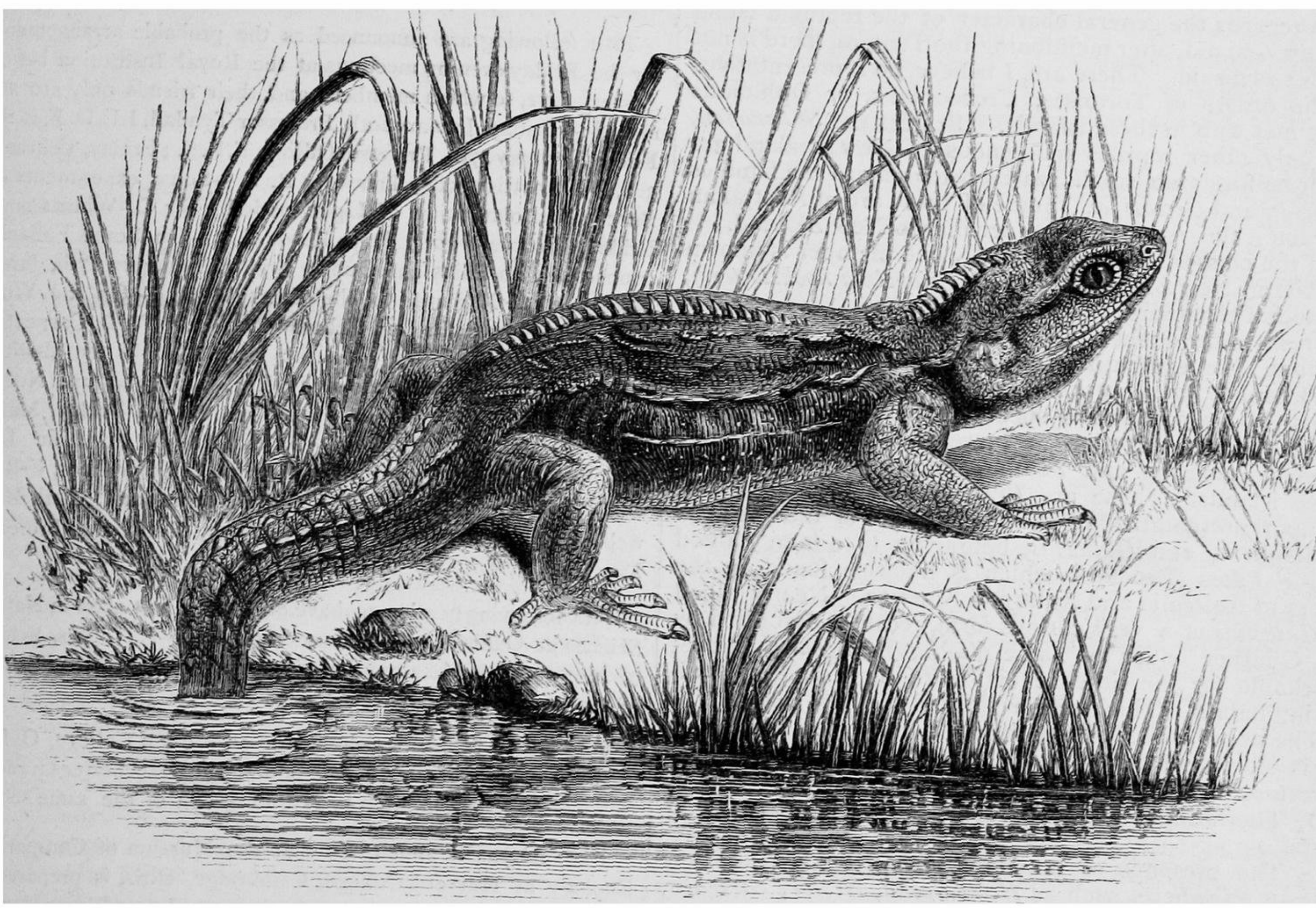
Of the very singular *Kakapo*, or Ground Parrot of New Zealand (*Strigops habroptilus*), I regret to say we have at present no example in the Society's Gardens. In the summer of last year one of these birds was successfully brought home from the Hokatika district, and was temporarily deposited by its owner in the Regent's Park Gardens, where it remained several months. But we were not able to come to terms as to its fair value, and the bird was consequently removed. The chief peculiarities of the *Kakapo* are its nocturnal habits, its abortive wings (which are nearly incapable of flight), the corresponding non-development of the crest of the sternum, and the possession of a facial disc, which gives it somewhat of an owl-like appearance. It is, however, a true parrot in the most essential part of its structure, and its food is strictly vegetable. During its sojourn in the Zoological Society's Gardens, it was fed principally upon corn and seeds of

* See "Dieffenbach's New Zealand," vol. ii., App. p. 185.

† Ueber die Vogel Neu-Seelands. Von Dr. O. Finsch in Bremen. Journ. f. Ornith., 1870, p. 241.

various sorts. The colour of this bird is of a bright green, freckled with black, which is said to assimilate it exactly to the mosses of the New Zealand forests where it is found. Although disappointed in not retaining possession of the example of this parrot which has been already in the Gardens, we are in great hopes (from intelligence lately received from New Zealand) of the approaching arrival of other individuals of this rare bird.

The Kiwi or Apteryx, which I mentioned above as a third remarkable form of New Zealandian bird-life, has long been represented in the Zoological Society's living collection. In December 1857, Mr. Eyre, then Lieut.-Governor of the Colony, presented to us the first example of this remarkable form that had ever been brought alive to Europe. This bird is a female, as has been evidenced by her producing enormous eggs for several years in succession. The first of these eggs was deposited in June 1859, since which time she has usually laid two in the spring of every year, at intervals of about a month between them. The egg of the Apteryx when first deposited weighs about $14\frac{1}{2}$oz.; it is smooth and of a dirty white colour, and measures $4\frac{3}{4}$in. in length by $2\frac{9}{10}$in. in breadth. As the weight of the parent bird is only about 60 oz., it will be seen that the weight of the egg is nearly equal to one-fourth of the bird, a fact, I suppose, quite without parallel in the animal kingdom. Since the acquisition of the female, two additional specimens of the Apteryx, both of the opposite sex, have been received: one in 1864, presented by Major Keane, and a second in 1865, presented by Mr. Henry Slade. The female continuing to produce eggs after the males had been placed in her company, we were in hopes of rearing young Kiwis in the Gardens, especially as on more than one occasion the male, as is the custom among Struthious birds, commenced to incubate. This operation he performed by squatting closely on the egg placed between his feet, so that its long axis



TUATERA LIZARD

was parallel to that of his body. Unfortunately, however, in no case has there been any result, and the eggs when examined have shown no appearance of having been impregnated. And at length our sole surviving male fell a victim to the exemplary zeal with which he performed the duties of incubation. After sitting upon one of these huge eggs for upwards of six weeks, he died, probably from exhaustion, so that the original female, received in 1851, is at present the only representative of the species in the Society's Gardens.

Besides the above-mentioned specimens of Mantell's *Apteryx* (which is probably not really different from the original *Apteryx australis* of Shaw) the Zoological Society have recently received a single living example of the undoubtedly distinct Owen's Apteryx (*Apteryx owenni*). This bird arrived in July 1869, having been forwarded as a present from the Acclimatisation Society of Otago. It is readily distinguishable from the ordinary species by its smaller size, spotted feathers, and the softer and more fluffy plumage, but is closely allied to it in general structure and in all other peculiarities.

The fourth bird that I mentioned as one of the more remarkable bird-forms inhabiting New Zealand, has not yet been received in this country in a living state, indeed it is only quite recently that naturalists have become perfectly acquainted with the eccentricity of its structure. It is a small wading bird, allied to the Shore-plovers, but possessing the extraordinary feature of having the end of its bill curved towards the right. By the examination of the chick of the *Anarhynchus frontalis*, as this singular bird is called, Professor Newton has recently shown that this abnormality commences from the egg.* Such instances of asymmetrical structure are very rare amongst the more

* Proc. Zool. Soc. Nov. 15 1870, p. 674.

highly organised animals, indeed I am not aware of any similar instance among birds.

Besides the birds above-mentioned, the Gardens contain the only examples ever received in Europe of the *Kaka* Parrot (*Nestor hypopolius*), another form of bird-life restricted to the forests of New Zealand.

I must now say a few words about the Reptilian life of New Zealand. As regards this branch of the animal kingdom, the Zoological Society's Gardens cannot be said to be very well supplied; at the same time we have now in them a living example of what is certainly the rarest and most singular species of the reptilian class that inhabits these islands. This is the Tuatera Lizard (*Sphenodon punctatum*), of which I have already given a notice in a former article in this journal.* Dr. Günther will, I hope, forgive me for calling it a lizard, for in spite of his elaborate proof that it is utterly different from every other known form of the Lacertian order, I cannot quite agree with him that it is entitled to form an order of itself.

As regards the general character of the reptilian fauna of New Zealand, after mentioning the Tuatera, there is not much to be said. There are, I believe, no representatives of the orders of Tortoises, Crocodilians, or Ophidians to be met with in these islands. Besides the *Sphenodon*, the only other reptiles indigenous to New Zealand are about half-a-dozen small lizards belonging to the families *Scincidæ* and *Geccotidæ*. Some of these are of Australian character, the remainder are peculiar to New Zealand.

Of the class of Batrachians, only one member is known to exist in New Zealand—a frog (*Leiopelma hochstetteri*), not found elsewhere.

Fresh-water Fishes, which must also be taken into account when estimating the peculiarities of a land-fauna, are likewise not very abundant in New Zealand. Two forms, however, deserve special notice, as indicating a former land connection at however remote an epoch, between Australia and South America passing through these islands. These are *Protrotoctes*,† belonging to a group representative of the Salmonoids of the northern hemisphere, and *Galaxias*, belonging to a family allied to the Pikes (Esocidæ) which is likewise found in the rivers of Australia and Antarctic America. Nearly allied to *Galaxias* is a very curious form, recently described by Dr. Günther under the name *Neochanna apoda*,‡ but remarkable for the absence of ventral fins, and its mud-loving habits. This fish is restricted to New Zealand.

The peculiarities of the fauna of New Zealand, so far as is shown by its terrestrial vertebrated animals, may therefore be summed up somewhat as follows:—

1. The absence of all Mammals, except two species of Bats.
2. The presence of numerous forms of Bird-life not known elsewhere, such as *Heteralocha*, *Nestor*, *Strigops*, *Apteryx*, and *Anarhynchus*.
3. The absence of Reptiles, except two genera of lizards and the *Sphenodon punctatum*, which, according to some of the best authorities, has claims to constitute an order of reptiles *per se*.
4. The absence of Batrachians, except one species of frog not known elsewhere.
5. The paucity of fresh-water Fishes—few genera only being known—which are allied partly to Australian and partly to Antarctic American forms.

But before closing our summary, it must not be forgotten to be mentioned that almost within the historical period New Zealand was tenanted by upwards of a dozen species of Struthious birds, constituting a family *per se*, but allied to the Cassowaries and Emus of Australia and the Papuan Islands. We must therefore add as a sixth item:

6. The recent presence of a peculiar family of gigantic Struthious Birds, now extinct—the Dinornithidæ—restricted to New Zealand.

P. L. SCLATER

* See NATURE, vol. ii p. 148
† Cf. Günther, Proc. Zool. Soc., 1870, p. 152.
‡ See Ann. Nat. Hist. ser. 3, vol xx. p. 305.

NOTES

IN other columns will be found the reports which have reached us up to the present time from the different sections of the Eclipse Expedition. Notwithstanding the unpropitious weather at nearly all the stations, we trust that some observations have been made which will throw light on still unsolved problems of Solar Physics. It is with great satisfaction that all English astronomers will hear that M. Janssen performed his perilous balloon voyage in safety, and reached his destination in Algeria; although he does not appear to have been rewarded with the opportunity of taking any satisfactory observations.

THE *Journal of Botany* states that the post of Curator of the Botanic Gardens, Calcutta, vacated by the death of Dr. Anderson, has been given to Dr. King, who has done some work in Indian botany, but is comparatively unknown in English botanical circles. This is the best-paid botanical appointment in England or any of its dependencies.

THE following are announced as the probable arrangements for the Friday evening meetings at the Royal Institution before Easter, 1871, to which members and their friends only are admitted:—Friday, January 20th, Professor Tyndall, LL.D. F.R.S.; Friday, January 27th, Professor Odling, F.R.S.; Friday, February 3rd, W. Spottiswoode, Treasurer R. S.—Some experiments on Successive Polarization of Light made by Sir C. Wheatstone; Friday, February 10th, E. J. Reed, C.B.—On Some Fallacies connected with Ships and Guns; Friday, February 17th, James N. Douglas, Engineer to the Trinity House—On the Wolf-Rock Lighthouse; Friday, February 24th, Dr. W. B. Carpenter, F.R.S. &c.—The latest Scientific Researches in the Mediterranean and Straits of Gibraltar; Friday, March 3rd, Capt. Noble, F.R.S.—On the Pressure of Fired Gunpowder; Friday, March 10th, W. Mattieu Williams—On Rumford's Scientific Discoveries; Friday, March 17th, J. Norman Lockyer, Esq., F.R.S.—On the Eclipse; Friday, March 24th, Professor J. Clerk Maxwell, M.A., F.R.S.—On Colour; Friday, March 31st, Professor Max Müller, LL.D.—On Solar Myths.

THE following gentlemen have obtained a first class in Natural Science at Oxford at the last examination:—C. Childs, Scholar of Merton, F. H. Champneys, Brazenose, F. W. Fison, Scholar of Christ Church, S. J. Sharkey, Scholar of Jesus.

THE Natural Science Scholarship at Merton College, Oxford, has been awarded to Mr. Richmond of the Manchester Grammar School, and an Exhibition to Mr. Ferguson of the same school.

PROF. AGASSIZ announces that the Museum of Comparative Zoology at Harvard College, Cambridge, U.S., is prepared to furnish extensive collections of all the rocks and loose deposits found upon and about the Key and reefs of Florida; also complete collections of the Corals in fresh and well-preserved specimens, in exchange for recent and fossil corals from other parts of the world.

THE *Pall Mall Gazette* states that a committee has been appointed, with Captain Beaumont, R.E., M.P., as president, and Lieutenant Grove, R.E., and Mr. Abel, F.R.S., as members, to carry out experiments on the utilisation of balloons for reconnoitring purposes. The former experiments on this subject, which were carried out at Woolwich and elsewhere a few years ago, were not attended with any useful results, and we believe the attempts which have been made during the present war to reconnoitre with balloons—an application not to be confused with the use of balloons for postal purposes—have not been more satisfactory. But the Americans on several occasions employed balloons to reconnoitre with fair success, and Captain Beaumont's committee may be able to throw some new light upon a subject which it certainly seems worth while to work out.

THE frost which has now lasted for a fortnight is the most severe that has been known in England since the memorable one of Christmas, 1860, that is, for exactly ten years. The lowest temperature at Blackheath was 15·3° F. on the night of the 24th December; but in the eastern counties the cold was more intense, being 8° at Hull, and nearly as low at Norwich, Nottingham, and Leicester. The highest minimum recorded by Mr. Glaisher in the *Gardener's Chronicle*, at any English station is 19·0°, at Leeds. In Scotland the minimum varied between 5·0° at Perth, and 19·2° at Aberdeen. The average was slightly higher in Scotland than in England. For the first fourteen days of the frost, the temperature scarcely rose above the freezing-point night or day, a very unusual circumstance in this country.

A SOCIETY has just been instituted under the designation of *The Society of British Archæology*. It originated in a meeting held on Dec. 9th in the rooms of the Royal Society of Literature, the proposed objects being "the investigation of the Arts, Archæology, History, and Chronology of Ancient and Modern Assyria, Palestine, Egypt, Arabia, and other Biblical Lands, the promotion of the study of the antiquities of those countries, and the preservation of a continuous record of discoveries now or hereafter to be in progress." Dr. Birch, of the British Museum, who occupied the chair on that occasion, explained that the proposed society would clash with none of the philological or exploration associations now in existence, but would have a distinct purpose—to concentrate and utilise the scattered materials connected with the geography, arts, and antiquities of the lands of the Bible, and to systematise the progress of Archæological research in England, America, and the Continent. The Society has already received the promise of the support of the most eminent living Biblical investigators, and another meeting will shortly be summoned for its complete establishment.

UNDER the title of "Science Education Abroad," Principal Dawson of M'Gill University, Montreal, republishes his Annual University Lecture, of the session 1870-71. After reviewing the state of scientific education in foreign countries from a Canadian point of view, as exhibited by the present condition of the various institutions for the spread of Science in Great Britain, the United States, Germany, and Switzerland, he contrasts with this the want of Science teaching in Canada. With the exception of two or three small and poorly supported agricultural schools, he states that the Dominion does not possess a school of practical Science, notwithstanding its mining resources, second to those of no country in the world. In the M'Gill University itself some part of Natural or Physical Science is studied in each year of the College course; but this falls far short of providing the full measure of the higher Science education required for the development of the resources of the country.

PROFESSOR M'NAB has been pursuing his investigations on the Transpiration of Watery Fluid by Leaves, to which reference was made a short time since in our columns.* The plant used in all the experiments was the common laurel (*Prunus laurocerasus*), and the fluid to test the rapidity of the ascent lithium citrate. The following are some of the more important results arrived at:—The total quantity of water in the leaves was found to be 63·4 per cent.; but of this, the proportion which could be received by calcium chloride, sulphuric acid, or by the action of the sun, was only from 5 to 6 per cent.; hence Dr. M'Nab calculates the amount of transpirable fluid in the stem and leaves to be between 6 and 7, the amount of fluid in relation to cell-sap to be between 56 and 57 per cent. The rapidity of transpiration he found to be in sunlight 3·03 per cent. in an hour; while in diffused daylight it was only ·59, and in darkness ·45 per cent. in the same time. These experiments were made when the plant had access to water by means of its stem. When the leaves were exposed without any means of supplying themselves again, the following results were obtained:—In a saturated atmosphere in the sun, 25·96 per cent. was transpired in an hour, in a dry atmosphere in the sun 20·52 per cent. In the shade, the numbers were reversed—viz., in a saturated atmosphere nothing, in a dry atmosphere 1·69 per cent. When immersed in water, the leaves absorbed 4·57 per cent. of their weight in seventeen hours, in a saturated atmosphere nothing whatever in eighteen hours. The under surface of the leaf transpired nearly ten times as much as the upper surface.

* See NATURE, vol. ii. p. 515.

WE are very glad to see that the second series of penny science-lectures delivered in November in the Hulme Town Hall have been reprinted. They embrace:—Coral and Coral-reefs, by Prof. Huxley; Spectrum Analysis, by Prof. Roscoe; Spectrum Analysis in its relation to the Heavenly Bodies, by Dr. Huggins; and On Coal, by Mr. Boyd Dawkins. The reports have all been revised by the respective lecturers; and being published at one penny each, ought to have a very large sale. We cannot conceive a greater aid to scientific teaching than the circulation of these lectures; both for the information they contain, and as models to lecturers of what scientific lectures to working men should be.

THE last number of the *Bulletin de la Société Royale de Botanique Belgique* contains an interesting paper by M. Devos on the plants naturalised in, or introduced to, Belgium. Of the 1,566 phanerogams recorded for that country, no less than 512 are supposed to have been introduced. Of these 91 are from southern Europe, 137 from the east, 14 from central, and 5 from north Europe; "alpine regions" have furnished 16, 34 are from America, and 5 from Africa; while the native countries of the remaining 210 are unknown. The distribution of each species is traced out with reference to its occurrence in other countries under similar circumstances; and the paper is a valuable contribution to phyto-geographical science.

AMONG the curiosities of scientific literature a little work, published a few years since, must find a place. It is entitled "Principles and Rudiments of Botany, delivered according to an Iulian system of arrangement and Iulian method of classification; by C. R. W. Watkins, Gent., late Captain in the Bombay Army." These "principles and rudiments" are here, according to the preface, delivered in language "better adapted for the intellectual amusement and instruction of young persons of both sexes" than that employed in previous works; and "Botanical science" is "rendered more agreeable to students in modern times." The following extract will give a faint idea of the mode in which these promises are fulfilled, and also of the contents of the volume:—"The pink (*Dianthus*) has four or five idola; ten to twenty ikona, and twenty to forty petala. The flowers are few, and di, tri, quinque ligate, and they terminate separately and irregularly. The Sweet William (*Dirythme*) has two idola, ten ikona, and five petala. The flowers are numerous and chorovinkulate, and the mode of gemmation comprises several synterminal and equimarginal chorrythma, or conturrythma. They cannot, therefore, be of the same genus; because the numerical indices, and typical characters of each gemmos, or hermaphral gemm bud of the two kinds of plants, are not symbolical; but differ, as well as the mode of gemmation, more widely than the specific, and physical circumstances of their constitutional, or peculiar veget-organical structure."

THE laws which formerly existed in Scotland, and are still enforced in Denmark, to compel the extirpation of the Corn Marigold (*Chrysanthemum segetum*), have their parallel in New Zealand, in some parts of which it is a punishable offence to allow the growth of thistles. In the colony of Lyttelton proceedings were taken, during the present year, against a gentleman for having neglected to eradicate certain thistles, after having been requested to do so. The defendant alleged that men had been employed for ten days in striving to exterminate them, and that ten donkeys were kept for the sole purpose of eating off the tops of the thistles. The Bench, however, were of opinion that no adequate steps had been taken since a previous conviction, and imposed a fine of 5*s*. per day from that date.

WE learn from Dr. Müller's last report of the Melbourne Botanic Gardens that the noxious "Cape Weed" (*Cryptostemma calendulaceum*), is becoming suppressed in his vicinity by the gradually denser growth of lucerne, clover, and grass. The plant was noticed as an inexterminable weed of Australia, by Baron von Huegel, in 1833. It would appear that more than one plant is known as "Cape Weed," as another Composite (*Hypochœris radicata*), is so called in New Zealand, where, in the neighbourhood of Dunedin, it is spreading to a serious extent.

A WRITER in the *Field* for Dec. 17, advocates the cultivation of *Symphytum asperrimum* as food for cattle. He recommends that the plants should be set about two feet six inches from each other; and that the most forward of the leaves should be plucked as they develop. Horses are very fond of it, and it is beneficial in its effects upon them. It is not recommended that it should be made store of, but the leaves should be gathered and eaten fresh. It will be remembered that Prof. Buckman, when at Cirencester, instituted some experiments upon this plant, which led him to the conclusion that it was not specifically distinct from the common Comfrey (*S. officinale*); and it is therefore probable that the latter would be equally suitable for cattle. *S. asperrimum* is a plant of very rapid growth; boiled as a vegetable it is palatable, and in Germany it is a favourite ingredient in salad.

AMONG the various notes upon tree-worship which have lately appeared, no mention has been made of the Cotton-tree (*Eriodendron anfractuosum*). Of this Dr. Macfadyen, in his "Flora of Jamaica," writes as follows: "Perhaps no tree in the world has a more lofty and imposing appearance, whether overtopping its humbler companions in some woody district, or rising in solitary grandeur in some open plain. Even the untutored children of Africa are so struck with the majesty of its appearance, that they designate it the God-tree, and account it sacrilege to injure it with the axe; so that, not unfrequently, not even the fear of punishment will induce them to cut it down. Even in a state of decay it is an object of their superstitious fears; they regard it as consecrated to evil spirits, whose favour they seek to conciliate by offerings placed at its base."

AN early mention of tobacco is that in Hakluyt's "Voyages," by M. Jaques Carthier, in 1534. Speaking of the people of "Hochelaga, up the river of Canada," he says, "There groweth also a certain kind of herbe, whereof in Sommer they make great prouision for all the yeere, making great account of it, and onely men vse of it, and first they cause it to be dried in the Sunne, then weare it about their neckes wrapped in a little beasts skinne made like a little bagge, with a hollow peece of stone or wood like a pipe, then when they please they make pouder of it, and then put it in one of the ends of the said Cornet or pipe, and laying a cole of fire upon it, at the other ende sucke so long, that they fill their bodies full of smoke, till that it commeth out of their mouth and nostrils, even as out of the Tonnell of a chimney. They say that this doth keepe them warme and in health, they neuer goe without some of it about them. We ourselves haue tryed the same smoke, and hauing put it in our mouthes, it seemed almost as hot as Pepper."

As a general rule, plants which are casually introduced to, and become firmly established in, any country, are by no means to be regarded as useful acquisitions; some of the worst weeds of cultivation may be found among them. An exception to this, however, may be noticed in *Lespedeza striata*, "Japan clover," or "wild clover," as it is called in the localities to which it has introduced itself. It has recently sprung up in great abundance in all parts of the Southern States, and has proved a great acquisition to the farmers. The roots, which are long and fibrous, penetrate and flourish even in sandy roads and in yards; and a single root will send out as many as six hundred branches. The *Lespedeza* is a close-growing plant, covering the ground as with a carpet of green, and is taking the place of the sedges and other weeds upon the waste lands and clearings. Cattle, horses, and sheep eat it greedily; and it is in every way an important addition to the fodder-plants of the country.

A CONTRADICTION is given to the reported discovery of coal in the Bellary district in Madras.

ON the 3rd of October the Faculty of Science and Polytechnic School were opened at Quito in Ecuador. There is reason to believe that these establishments of the repudiatory republic are more pretentious than real.

CINCHONA has so fully succeeded in the Neilgherry Hills in India that the first shipment of bark from a private plantation to the extent of 4,000 lbs. is taking place. The Government promoted Cinchona plantations chiefly for the supply of India, but they are already engaging in the home trade.

A RARE discovery has been made near the port of Mejillones in that district of the rainless desert of Atacama belonging to Bolivia, of a spring of fresh water. This has been granted to the discoverer for ten years, and then to become the property of the State.

ON the 18th of October, at 5 P.M., a slight earthquake was felt at Salvador in Central America. There had been heavy rains for some days.

THE Queensland Acclimatisation Society, under the patronage and with the assistance of his Excellency the Governor and other influential persons in the colony, appears to be in a satisfactory condition, both financially and with regard to the work done, as well as the earnestness and ability of its workers. In the park belonging to the society a considerable number of foreign trees have been introduced, and amongst them the splendid tree of Madagascar (*Poinciani regia*), which, we are told, is being raised by hundreds. This success of foreign trees is very gratifying at a time when the question of want of shade trees is a matter of much interest, not only in Queensland, but also in other colonies. The Shola (*Æschynomene aspera*) is likewise amongst the recently-introduced plants which promise success. It is a native of India, and is well known on account of its light wood being used for making the "pitte" hats, so much used in tropical countries. Among British birds introduced by the Society, and which survived the voyage from London, the blackbird, thrush, starling, rook, sparrow, and lark, have been liberated in the Botanic Garden; but at the time the report was drawn up little could be said as to their condition or whereabouts, except that one pair of sparrows were then rearing their second brood in the heart of the city. Large numbers, also, of Chinchilla rabbits, and of the wild English breed, have been turned loose on the islands of Moreton Bay, but care has been taken to keep the different kinds separate.

IT is not a little remarkable that *Corchorus capsularis*, one of the plants yielding the jute of commerce now so largely imported and used in the manufacture of carpets, and also largely used in India for gunny bags, should, though growing wild in Sumatra, not be cultivated in that island. The bags in which Java coffee is exported are made of this fibre. Large quantities of these ready-made bags are annually sent from Bengal, even to Java.

THE ECLIPSE EXPEDITION

As we intimated last week, the weather was more or less unfavourable at nearly all the stations for the observation of the Total Eclipse of Dec. 22nd. We give, in another column, an account of the preparations made by the Sicilian department of the Expedition, received by us. The following account of the results obtained at some of the other stations is compiled chiefly from reports furnished to the *Times* and *Daily News*.

From Cadiz we have an interesting account by the Rev. S. J. Perry, as follows:—

"The situation of San Antonio is found to be lat. 36° 37′ 13″ N., long. 24′ 15″ W. of Greenwich. Time signals were daily received from the San Fernando Observatory, and all our chronometers carefully rated by Capt. Toynbee. Our thirteen observers were distributed as follows: Mr. Moulton, of Christ's College, Cambridge, with Mr. Baines, of Oxford, were to observe with the polariscope at Sanlucar, the extreme W. point on the Spanish central line of totality, and 12 miles N.W. of San Antonio. Near Xeres, 5 miles N.E. of San Antonio, were stationed Mr. P. Naftel, for an eye sketch of the corona, Mr. F. C. Penrose to sketch the same as seen through a telescope, and Mr. Abbay, of Wadham College, Oxford, to observe with the spectroscope. Mr. W. Smyth sketched near Arcos, 17 miles E.N.E. of San Antonio, using a telescope of the same aperture as that of Mr. Penrose. The rest of the observers remained at San Antonio. At this station the spectroscopic observers were Capt. Maclear, R.N., and myself, assisted by Mr. Hostage. Polarisation was to be observed by Mr. Hudson, of St. John's, Cambridge, and Mr. Ladd, optician; and an eye sketch of the corona to be made by Mr. Browne, of Oxford. The weather has been unexceptionally bad ever since our arrival, the only fine day being the 21st. Our observers were therefore spread out as much as possible, in hopes of not failing altogether on account of bad weather. The results justified our anticipations. The fine weather of the 21st lasted but a day, and at two A.M. of the 22nd the clouds and rain returned. At San Antonio a break only came some 48^s after first contact, when a distinct notch was observed on the solar disc. This break was only a change from thick cloud to thin cirrhus, but we were enabled to observe the time of contact of the limb of the moon with several of the more remarkable solar spots. In the north the sky was partially clear, but in the south no part of the heavens was free from cloud. A very striking change of light on the landscape was noticed when little more than three-fourths of the solar disc was covered, and a chill was felt by all. The thermometer observed by Capt. Toynbee fell 3°F. from the commencement to totality, and rose again 1·7° before the end of the Eclipse. The barometer was falling rapidly all the time of the Eclipse, and also afterwards, at the rate of 0·04 in. an hour. The wind was W. by N. true. During totality it lulled, but freshened afterwards with very heavy rain. The moment of totality approached, and no chance remained of even a momentary break in the thin cirrhus that enveloped the sun, and obscured most of the southern heavens. As the crescent became thinner, the cusps were observed first to be drawn out a length of several minutes, and then blunted; the well-known Baily beads were formed, and the corona burst forth more than 20^s before totality. Viewed through a telescope of very moderate dimensions the spectacle was grand, but the cirrhus clouds destroyed almost all the grandeur of the effect for the naked eye. The red prominences were numerous, but none apparently very remarkable; Mr. W. H. Browne, of Wadham College Oxford, considers their colour to have been of a bright yellowish red tint. The same observer notices that the corona was perfectly free from striation, outline distinct, and approximately quadrilateral, but extending farthest in the direction of first contact. The brightest part of the corona appeared to the unassisted eye to be scarcely more than one-tenth of the sun's diameter, fading rapidly when one-fifth, but being still clearly visible at seven-eighths. Some observed two curved rays, but the general appearance was that of a diffuse light interrupted in four places distinctly, and in a fifth faintly, by dark intervals. The corona was white, and rendered faint by the clouds. The darkness was never sufficient to prevent sketching with comfort without the aid of a lamp. Venus alone was visible. Totality ended by the formation of Baily's beads, and the corona was visible to the naked eye 15^s or 16^s after totality. The corona was seen for $2^m\ 50^s$, totality lasting less than $2^m\ 10^s$. The clouds obscuring the sun appear to have destroyed almost all chance of detecting any except atmospheric polarisation. Mr. Ladd remarked that the polarisation was stronger on the corona than on either the moon's surface or the cloudy sky.

"No report has as yet been received of the polarisation observations at Sanlucar. The observations with the spectroscope were also greatly interfered with by the cirrhus, and the best instrument was rendered entirely useless. The intensity of the light from the corona, as seen through the clouds, could not, I think, have been more than one-eighth of that of the bright moon, if so much, and, consequently, I was unable to detect the faintest trace of light through the three compound prisms I was using. The chances of observing satisfactorily, considering the state of the sky, were greatly diminished by the largeness of the direct image given by the Cassegrain I was compelled to use. Knowing that an unfavourable sky would render observations with a powerful spectroscope quite impracticable, I desired Captain Maclear to observe with a small direct vision Browning spectroscope, attached to a four-inch achromatic by Jones, mounted equatorially. The slit was placed radial at the centre of the east limb, and close to it, and immediately totality commenced the ordinary solar spectrum was replaced by a faint diffused light, and bright lines near C, D, b (or E), and F. No absorption bands. The slit was then removed to a distance about 8′ from the limb, and the same lines remained visible. The centre of the moon was next tried, and the bright lines were still seen, but only half as strong as before. The slit was then placed 8′ outside the W. limb, and the lines became as strong as before, and were C, D, one three-quarters of the distance from D to E, and another half way between E and F. Lastly, placing the slit near the sun (on a prominence) two new green lines, and a very brilliant line beyond F, were added to those already visible, but the line near E may have disappeared. The lines seen on the moon were, I suppose, due to the diffusive power of the cirrhus clouds; and the same may perhaps be true of the apparent coronal lines.

"Mr. Abbay, observing at Xeres with a spectroscope of two prisms of 45°, belonging to Professor Young, saw the bright lines C, D, F; and afterward F, and a line rather more bright than F, at some distance on the less refrangible side of B, C not noticed then. These two observations were, I think, taken at points external to the prominences, but I cannot at present speak with certainty, as no note to that effect is entered in the memorandum I received. A comparison of these observations with those of other observers more favoured than ourselves will doubtless lead to valuable results. Shortly after totality the clouds thickened still more, and nothing further could be observed."

The view of the Eclipse obtained near Arcos is described as very magnificent; a sketch was made there by Mr. Warrington Smyth. At the American station near Xeres there was a break in the clouds, which lasted somewhat more than half the totality. But Lord Lindsay's party was the most favoured in this country, having seen the sun through a rent in the clouds for five minutes, and this time embracing the whole of totality. Mr. O. Airy and

Mr. Hammond, of Trinity College, Dublin, observed at San Antonio, and were kindly assisted by Lieutenant P. H. Worgan and Mr. T. H. Atkinson, of H.M.S. *Lee*.

Another correspondent, from Cadiz, writes to say that Lord Lindsay succeeded in taking several excellent photographs from a vineyard belonging to Mr. Campbell, half way between Port St. Mary's and San Lucar.

The American party at Xeres saw the totality for about a minute.

From Gibraltar, Mr. R. M. Parsons sends the following report :—

"The party that left England for the purpose of observing the Total Eclipse of the sun at Gibraltar—namely, Messrs. Carpmael, Gordon, Lewis, Buckingham, Beasley, Harrison, Anson, Abbatt, Talmage, and myself—disembarked from Her Majesty's ship *Urgent* on the 14th Dec, and all but the three last-mentioned proceeded to Estepona, a village in Spain, about thirty miles north-east from Gibraltar, and situated in the central line of totality. By this division of the party an additional chance was afforded of observing the phenomenon in case of bad weather, and Estepona offered the advantage of some 13^s longer time of total obscuration than Gibraltar, a condition very desirable for the particular class of observations required by some of the party. The weather at Gibraltar was wet and cloudy almost from the time we landed until the day before the Eclipse, when a strong breeze W. by N. gave a beautifully clear sky, which lasted till about midnight. Mr. Talmage, the director of the Leyton Observatory, and I, determined to observe from the Moorish Castle if the atmospheric conditions of the 21st should hold good on the 22nd ; but I arranged to receive constant telegrams of the weather from Europa Point on the morning of the 22nd, and conveyance was provided to move the instruments at the latest practicable period, in case any other position afforded better chances of success. Mr. Talmage was to take angular measurements of Saturn, if seen through the corona, Mr. Abbatt to sketch the corona, and I was to examine it with a polariscope. The westerly wind increased in force on the 22nd, but brought with it scud and dense clouds across the Bay from the Spanish mountains; everywhere these clouds were massed in the sky, separated by small intervals of hazy blue. The last telegram was received from Europa at a quarter-past eleven, forty minutes before commencement of totality, stating 'sky quite overcast, heavy clouds moving south-east, sun hardly visible.' This, together with the circumstance that the Rock did not appear to affect the clouds which were moving under the influence of a westerly wind, led me to conclude that the chances of good vision were equal at any position on the Rock. After waiting on the Line Wall for these telegrams, at which place I failed to observe the first contact, while Mr. Talmage failed in the same endeavour at the Moorish castle, I joined him there, leaving Mr. Abbatt with his telescope erected on the flat roof of a house about a quarter of a mile west of the castle. The cloud which caps the Rock of Gibraltar, the summit of which is 1,396ft. above the sea, during east winds, or Levanters, leaves this comparatively low level clear. About 30^s before the commencement of totality, a hazy blue break in the clouds enabled us to see the thin bright crescent of the sun, but unfortunately this patch of hazy blue sky, which favoured others for a few seconds, came a little too early in front of our position, and it was followed by a dense cloud, behind which the entire phenomenon of totality was hidden from us. The darkness was considerable, but not so great as when I observed the Total Eclipse of 1860, at Nisqually, in an unclouded sky. Then a lamp was necessary to enable a white-faced pocket chronometer to be read ; yesterday I could see the divisions distinctly at the distance of eight inches without such aid. Mr. Abbatt had the good fortune to see the corona and some red prominences, but only for about two seconds before they were lost in the same dense cloud ; he estimated the breadth of the corona at about a sixth part of the moon's radius. Professor Newcomb, of the United States' Expedition, was able to see all four contacts, and to take several measurements that were necessary for the work he has in hand ; he also caught a glimpse of the corona, but says he could make no use of it.

"Mr. Lewis states that the party at Estepona only saw the total phase through a break in the clouds for about 10^s or 15^s, when it was covered by light cloud. Mr. Buckingham, at Estepona, states that they had there heavy rain ; he could take no photographic pictures, but Mr. Carpmael had observed three bright lines in doubtful positions, and Mr. Lewis found the corona polarised ; the rest of the party had negative results. Mr. Harrison, who was distant a mile from the others, did not see the total phase. Mr. Anson had not time at the moment to sketch what he saw, but probably may be able to do so from memory, and Mr. Fison, who had then joined the party, had no opportunity of obtaining satisfactory observations."

Mr. Abbatt reports :—"In no part did the corona or the prominence extend beyond 1-8th or at most 1-6th of the moon's radius beyond the limb. I thought the moon darker than the sky. I noticed four high red prominences—there were more, but when, for an instant, I took my eye off the telescope a dense black cloud had obscured everything till the narrowest streak of the sun appeared on the western side, and nothing but the ordinary phenomenon of a partial eclipse was to be seen. The darkness during totality was not so great as I expected it would be. Two stars were seen, one near the sun and the other overhead, but I can get no further information as to their exact position."

Another observer writes as follows :—"The eclipsed orb presented itself through a rent in the clouds not greater in area than ten times that of the disc of the moon's shadow. That part of the opening which was above the eclipsed orb was clear like the sun at twilight, and in it were visible to the naked eye the planets Venus, Mercury, and half a dozen stars. The remaining part was covered with a thin haze. The moon's shadow appeared to the eye, assisted by a somewhat weak binocular glass, to be a dark circular disc with an even boundary and of uniform shade. Within the corona, and touching the circumference of this shadow, appeared five or six spots of brilliant carmine, varying in form and size and at irregular distances apart. Two of these spots, or 'red flames,' as they are called, on the eastern side of the disc, and at about 55° and 80° respectively from the vertex, seemed decidedly the largest and most prominent ; they were tongue-shaped, and protruded about 1-6th the width of the corona. In their neighbourhood the corona was brightest and widest. There, too, the rays of the corona appeared to be gathered more distinctly into groups than elsewhere, faint shadows being visible between the groups. The corona consisted of brilliant rays of extremely faint prismatic hues ; these rays at first sight appeared pretty evenly distributed all round, but closer examination seemed to detect the fact of there being bundles of rays in nearly regular groups. The width of the corona was about 1-8th the apparent diameter of the moon's shadow. It was very nearly concentric with the disc of the shadow ; its boundary was well defined, but 'jagged ;' the perimeter, except opposite the two most prominent red flames above-mentioned, where the boundary slightly protruded, was circular."

From Seville, we have a report from M. E. A. De Cosson :—

"The Eclipse began at 10.30 A.M. (Seville time). At 10.45 one-sixth of the sun's diameter was obscured ; at 11.15 one-half ; and at 12 the Eclipse was total. At 12.10 it began to rain, and the sun was lost to sight until the conclusion of the eclipse, which occurred at 1.30 P.M. The eclipse was total for 70^s and the effect was very striking, the whole sky becoming of a lurid purple and very dark. The birds were hushed and frightened, and the charcoal burners' fires in the mountains, some five miles distant were plainly visible."

At Oran the weather appears to have been still more unpropitious than at the European stations. For the observations with the spectroscope, this section of the Expedition was provided with a 6-inch equatorial, of eight feet focal length, by Cooke, of York, lent by the Royal Observatory at Greenwich, to which was adapted an extremely ingenious recording spectroscope, the invention of Dr. Huggins, who himself observed with it; the management of the telescope devolving upon Captain Noble. A second smaller instrument, of 4 inches aperture, with a somewhat similar recording apparatus, was entrusted to Mr. W. Crookes, F.R.S., the Rev. F. Howlett attending to the telescope itself. The polariscopic observations were undertaken by Captain Salmond, R.E., and Lieutenant Collins, R.E.; the instrument of the former being attached to a telescope of about thirty inches focal length and $2\frac{1}{2}$ inches aperture; that of the latter to another of 6 inches aperture. General observations, sketching of the corona, &c., were to have been made by Mr. Carpenter, of Greenwich Observatory, with an exquisite 3-in. Dolland equatorial; by Mr. Hunter, with a 4-in. of rougher construction; by Lieut. Wharton, R.N., of the *Urgent*, with a $2\frac{3}{4}$ achromatic, mounted temporarily equatorially; by Admiral Ommaney, with a small table telescope; by Lieut. Ommaney, R.E., with a binocular field-glass, on cards arranged for the purpose; and by the ship's officers generally, with such telescopes as they could command. The 6-in. and 4-in. equatorials were erected under a tent; the rest were employed more or less in the open air. Prof. Tyndall had established himself on a neighbouring fort, some 500 or 600 yards from the rest of the party, with a 5-in. telescope by Dallmeyer, which had been used by Mr. Dela Rue at the Total Eclipse in Spain. His observations were to be of a general character.

The atmospheric conditions in Oran in December are exceedingly like those of a very wet and windy English autumn, the day of entering into port having been the only moderately fine one experienced at all. The wind particularly was tremendous. On the 21st the observatory tent was blown down, and it was only by great exertions that the instruments themselves were saved from destruction.

The morning of the 22nd broke very doubtfully, heavy driving clouds obscuring the sun. The party were, however, all at their posts betimes, and arranged their various instruments long before the predicted instant of first contact. The clouds obscured the sun at this particular time; but soon, through a rift in them, the dark limb of the moon was seen encroaching on the sun's disc. Observation after this became very trying, as it was only possible at distant intervals to glimpse the progress of the moon, as she gradually advanced in her path. Spectroscopic observation, or even polariscopic observation, was practically impossible. Finally a glimpse was caught of the sun some eight or nine minutes before totality as a very thin crescent, and then every chink and cranny in the clouds closed up, and one dense and impenetrable pall covered the earth. Nothing remained now but to go out of the tent and observe the effect in the open air. It was certainly very striking. As the moment of totality approached, the western sky turned of an awful livid purple, the clouds over head assuming a black tint. As the shadow swept over the earth, the eastern sky became obscured, while the western horizon lighted up like the grey dawn of a dull autumn morning. At no time, however, during the totality, was it difficult to see the seconds on the face of a watch. The light was that of a cloudy evening, about an hour after sunset. One curious effect was observed, the apparent contraction or closing in of the celestial vault. Speedily, however, the dawn in the western horizon spread over the heavens; but the sun did not reappear again for at least half an hour after totality. M. Janssen, the French astronomer, who escaped, at the risk of his life, with his instruments in a balloon from Paris, was stationed on a mountain some seven miles from Oran. He can have seen nothing whatever of the phenomenon which he had braved so much to observe.

Another account from the same station states that the earlier stages of the Eclipse were seen and a photograph taken, but ten minutes before totality a dense bank of clouds came over the sun, which extinguished all hope, and rendered useless the laborious preparations of many weeks. M. Janssen was of course equally unsuccessful. Though the darkness was almost appalling as it rapidly increased up to the moment of totality, still the scene was deprived of a large part of its impressiveness by the dense mantle of cloud which concealed nearly the whole heavens.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, December 15.—Prof. Frankland, F.R.S., Vice-president, in the chair.—The following gentlemen were elected Fellows:—P. T. Atkinson, R. Koma, J. F. Stark. Mr. Perkin, F.R.S., read a paper "On some new Derivations of Coumarin." The author succeeded in obtaining the following new bodies:—

Dibromide of Coumarin	$C_9 H_6 O_2 . Br_2$
Dichloride of Coumarin	$C_9 H_6 O_2 . Cl_2$
α Bromocoumarin	$C_9 H_5 Br O_2$
β " "	"
α Chlorocoumarin	$C_9 H_5 Cl O_2$
β " "	"
α Dibromocoumarin	$C_9 H_4 Br_2 O_2$
β " "	"
Tetrachlorocoumarin	$C_9 H_2 Cl_4 O_2$
Coumarilic acid	$C_9 H_6 O_3$
Bromocoumaric acid	$C_9 H_5 Br O_3$
Sulphocoumaric acid	$C_9 H_6 O_2 . SO_3$
Disulphocoumaric acid	$C_9 H_6 O_2 . 2SO_3$

Dibromide of Coumarin.—A solution of coumarin in carbon disulphide is mixed with a similar solution of bromine, and the mixture, after leaving it to stand for twelve hours, allowed to evaporate spontaneously. When dibromide of coumarin is treated with an alcoholic solution of potassic iodide it becomes brown, and on evaporation deposits needles apparently consisting of a mixture of iodine and coumarin crystals, *α Bromocoumarin.* A simple process for the preparation of Bromocoumarin is to decompose the dibromide of coumarin with alkalis, the following reaction taking place:—

$$\underbrace{C_9 H_6 O_2 Br_2}_{\text{Dibromide of coumarin.}} + KHO = \underbrace{C_9 H_5 Br O_2}_{\text{Bromocoumarin.}} + K Br + H_2 O$$

α Bromocoumarin, when left in contact with cold alcoholic ammonia, decomposes with formation of ammonic bromide, and a non-crystalline sticky mass easy soluble in water. Heated with potassic hydrate it yields potassic bromide and a new acid. *α Dibromocoumarin.*—At a previous occasion Mr Perkin prepared this body by heating in a sealed tube to 140° C. a mixture of one part of coumarin, two parts of bromine, and four or five parts of disulphide of carbon. He since found, however, that this process is greatly improved by the addition of iodine to the mixture, as it is then only necessary to heat the sealed tube for four or five hours in a bath of salt and water to complete the reaction. The fusing point of this substance is 183° C. and not 174° C. as had been previously given. *β Bromocoumarin.*—The hydride of sodium bromosalicyl when submitted to the action of acetic anhydride, yields a quantity of hydride of bromosalicyl and a body which, when crystallised from alcohol, yields colourless flat prisms, the analysis of which showed it to be monobromocoumarin, $C_9 H_5 Br O_2$. It greatly differs in properties from the bromocoumarin previously described, its fusing point being 160°C., or 50° higher, and when boiled with alcoholic or aqueous potassic hydrate it does not decompose with formation of potassic bromide, but simply dissolves like ordinary coumarin. *β Dibromocoumarin.*—On treating the hydride of sodium *di*bromosalicyl with acetic anhydrate in exactly the same manner as for the preparation of β bromocoumarin, a beautifully crystalline product is obtained of the composition $C_9 H_4 Br_2 O_2$. It is not the same body as that obtained by acting on coumarin with bromine and iodine. It fuses at 176° C., and is not decomposed by boiling with a solution of potassie hydrate. Mr. Perkin has, therefore, designated it as β dibromocoumarin. *Dichloride of coumarin*—A solution of coumarin in chloroform absorbs chlorine gas, only minute quantities of hydrochloric acid being formed. On allowing the solution to evaporate spontaneously, after the chlorine has been passed through it for an hour or two, a syrupy product is obtained very like new honey. This is the dichloride of coumarin. From its products of decomposition, there can be no doubt that it possesses the formula—

$$C_9 H_6 O_2 Cl_2$$

Chlorocoumarin.—A mixture of one part of coumarin and three parts of pentachloride of phosphorus, when mixed and heated in a retort placed in an oil bath, slowly react upon each other as the temperature rises, and when the oil has reached about 200 C. the product becomes a dark brown liquid. During this reaction, a volatile liquid consisting chiefly of terchloride of phosphorus distils over. The contents of the retort after treatment with water, becomes a pasty mass of crystals, which is first purified by distillation, and then by several crystallisations from alcohol. Its analysis gave the formula $C_9 H_5 Cl O_2$. It fuses at 122°—123° C., and, when heated, possesses an agreeable aromatic odour. *Tetrachlorocoumarin.*—Chlorine gas when passed through a solution of coumarin and iodine in tetrachloride of carbon is rapidly absorbed, hydrochloric acid being evolved. If the gas be passed for two or three hours a quantity of a reddish body separates; on evaporating the product so as to separate the tetrachloride of carbon an oily residue is obtained, the red substance having fused with the impurities. On mixing this with alcohol, it soon becomes a white paste. On pressing this in a small linen bag, a white product is obtained which is further purified by being several times crystallised from spirit. The numbers of the analysis lead to the formula—

$$C_9 {}_2H Cl_4 O_2$$

It fuses at 144°—145° C. Bromocoumarin, when boiled with a solution of potassic hydrate, decomposes, yielding potassic bromide and the salt of a new acid, which has the formula $C_9 H_6 O_3$. Mr. Perkin proposes to call it *Coumarilic* acid. It fuses at 192°—193° C., distils with partial decomposition, but sublimes undecomposed when gently heated. It is monobasic, and forms well defined salts with the alkalis, the alkaline earths, lead, silver, mercury, and iron. *Bromocoumarilic acid.*—It is prepared like the above, but substituting α dibromocoumarin for bromocoumarin. It possesses the formula $C_9 H_5 Br O_3$, and fuses at 250° C. *Sulphocoumaric acid.*—It is obtained on digesting a mixture of about one part of coumarin and five parts of fuming sulphuric acid in the water bath for an hour or two. From the analysis of its salts, the formula of this acid when anhydrous is $C_9 H_6 O_2 SO_3$. Mr. Perkin has prepared ammonic, potassic, sodic, baric, and strontic sulphocoumarinates. *Disulphocoumaric acid.*—On heating a mixture of about 8 parts of fuming sulphuric acid and 1 part of coumarin to a temperature of 150° or 160° C. for an hour or two, the produced will contain two sulphoacids, viz., sulphocoumaric acid and disulphocoumaric acid. Its barium salt has the formula—

$$C_9 H_4 O_2 Ba'' 2 SO_3.$$

—The next communication was by Dr. Debus. "On the formula of glyoxylic acid," Dr. Debus showed that this acid ought to be written $C_2 H_2 O_3$ and not $C_2 H_4 O_4$. He considers it in reality to be the aldehyde of oxalic acid. Among other reasons for this view he quoted its behaviour towards the bisulphites. Dr. Odling was of the same opinion. He saw the aldehydic character of glyoxylic acid in its property of combining with one atom of water or of ammonia, or ethylic chloride, &c. Mr. Perkin defended the formula $C_2 H_4 O_4$, quoting among other evidences for the correctness of his views, the behaviour of glyoxylic acid when treated with phosphoric pentabromide; instead of losing water and being converted into CO, it takes up three atoms of bromine.

TAUNTON

Somersetshire Archæological and Natural History Society, December 12.—W. E. Surtees, president, in the chair. In a paper by Mr. Cecil Smith on some rare birds found in the immediate neighbourhood of Taunton, a few observations were made on the following birds, specimens of all of which were produced. The first mentioned was the Pied Flycatcher, which had been killed in an orchard near French Weir, close by the town. Though rare in this and other southern and western counties, it was much more common in more northern counties, especially in the Lake district, where it was a summer visitant, remaining from April to November—it had, however, been taken as late as the middle of October at the Scilly Islands, when on its southern migration. The next bird mentioned was White's Thrush, of which four, or at most five, British specimens had been recorded. The specimen produced, which brought this bird within the limits to which Mr. Smith's paper was confined, had been killed at Hestercombe Wood, in January 1870; it was shot at by mistake for a woodcock. Of this bird it was observed that although it might be difficult to account for its presence in England, or even in Europe, there could be no doubt as to its identity, as this and other British killed specimens did not differ in the slightest degree from those brought over from Japan and the East. On the difficulty of accounting for its presence in England, Mr. Smith observed that the journey performed being almost entirely over land, the reason given for the appearance of many, especially of American birds on our shores, that being driven out of their usual migratory course, and out to sea by strong gales, they found no place of rest before reaching our shores, was not applicable. Neither would the supposition that it had got mixed up with flocks of other perhaps nearly allied species on what had been a common breeding ground, and accompanied them, be applicable. Still less would the theory hinted at in Yarrell that the occasional appearance of these Eastern birds in Europe is not so very strange, since as many as 114 species are enumerated by Temminck as common to Europe and Japan; but of these 114 species the great majority are inhabitants of both countries, and not wanderers from one to the other. One instance, the almost universally distributed Turnstone, was taken as an example, which, instead of wandering to such an enormous extent, appeared to be equally content, whether on the muddy shores of Somerset or at the Cape of Good Hope or in Japan. Perhaps, after all, the appearances of White's Thrush were to be attributed to a truant and vagrant disposition in the bird itself, which might lead it far from his own home. Probably the habits of the bird contributed slightly to its rarity in Europe, as it appeared to keep much out of sight in woods and plantations, and it had consequently, as in the present instance, been shot by mistake for a woodcock. The next bird mentioned was Tithy's Redstart, which was considered worthy of notice as having occurred so close to the town as Gulmington-lane. The difference between this bird and our summer visitant, the common redstart, was pointed out, and specimens of each shown. The peculiarity of this bird being a winter rather than a summer visitor to these islands, was remarked on especially, as it is a regular summer visitor to the middle and northern parts of Europe. Also its choice of localities, namely, rough, rocky situations, such as the Parson and Clerk rock at Teignmouth (at which place Mr. Smith had several times seen it in the month of November), in which it differed much from the common redstart, which preferred gardens, orchards, and hedgerows. The difference of the eggs was also remarked upon, those of the present species being white instead of the well-known blue of the common redstart. The occurrence of one specimen of the Sevin Finch, within the town of Taunton itself, brought this bird within the range of the paper, Mr. Smith observing that in the Birds of Somerset he had thrown some doubt upon this specimen, supposing it might be an escape, the time of year at which it was killed, the end of January or beginning of February, more than the rareness of the bird itself, leading to this doubt; he felt, however, bound to admit that there did not appear about the bird itself any signs of its having been in confinement. The other British specimens appear to have occurred in the summer between April and October, as might be supposed from its being an inhabitant of the south of Europe, and growing scarcer as we get farther north. The similarity to the Siskin was also mentioned, and specimens of this bird produced for the purpose of comparison, the difference in the shape of the beak being pointed out as the most reliable distinction, especially by candle light, which considerably increased the difficulty of distinguishing the colours. The Little Bittern and Baillon's Crake were also mentioned, and a specimen of each produced, both having been killed nearly in the same place, some rushy pools by the side of the river in Priory Fields. Attention was called to the similarity between the Little Bittern and its big relation, in the absence of feathers at the back of the neck. Mr. Smith added that he had seen by chance, in the shop of Mr. Petherick, the bird stuffer, a specimen of the Woodsandpiper, which had been killed by Mr. Petherick himself, on the 9th of May last, as near as Cheddar, which would bring it quite within range for mention in the paper

MANCHESTER

Literary and Philosophical Society, November 29.—R. Angus Smith, Ph.D., F.R.S., vice-president, in the chair. Mr. R. D. Darbishire, F.G.S., exhibited a series of palæolithic instruments from the valley of the Little Ouse, and explained (after Mr. J. W. Flower, Q. J. Geolog. Soc. xxv. 449) the general features of the district and the deposit of the beds and the implements.—Mr. W. Boyd Dawkins, F.R.S., indicated the age of these deposits as related to the period of the existence of *Elephas primigenius* in the district of the south-east of England and the adjoining portions of the bed of the German Ocean and the north-west portions of France.—"The Tails of Comets, the Solar Corona, and the Aurora considered as Electric Phenomena," by Prof. Osborne Reynolds, M.A. Although the tails of comets are usually assumed to be material appendages which accompany these bodies in their flight through the heavens—and the appearance they present certainly warrants such an assumption—yet this is not the only way in which these tails may be accounted for. They may be simply an effect produced by the comet on the material through which it is passing, an effect analogous to that which we sometimes see produced by a very small insect on the surface of still water. We see a dark spot, and on looking closer we find a small fly or moth flapping its wings and creating a disturbance which was visible before the insect which produces it. There is nothing else that we can conceive their tails to be, so that they must be one or other of these two things: either (1) material appendages of the nucleus, whether the material be limited to the illuminated tail or surround the comet on all sides; (2) matter which exists independently of the comet, and on which the comet exerts such a physical influence as to render it visible. There can be no doubt that if these tails are matter moving with the comet, this matter must be endowed with properties such as we not only have no experience of, but of which we can form no conception. This alone would seem a sufficient reason for rejecting the first hypothesis. Moreover, on the second hypothesis there is no difficulty in the immense velocity with which these tails are projected from the head or whirled round when the comet is in perihelio. For to take the "negative shadow" as an illustration, here we should have a velocity of projection equal to that of light, and the only effect of the whirling would be a slight lagging in the extremity of the tail, causing curvature similar to that which actually exists. And whatever the action may be, if its velocity of emission or transmission be sufficiently great, this effect will be the same; but whether this hypothesis is to be rejected because involving assumptions beyond conception or contrary to experience, must depend on the answers to the following question: Do we know, or can we conceive, any physical state into which any substance which can be conceived to occupy the space traversed by comets could possibly be brought so as to make it present the appearance exhibited by comets? Now I think the answer must be in the affirmative, and that we may leave out the terms conceive and conceivable. For electricity is a well-known state, and gases are well-known substances; and when electricity, under certain conditions, as in Dr. Geissler's tubes, is made to traverse exceedingly rare gas, the appearance produced is similar to that of the comets' tails; the rarer this gas is, the more susceptible is it of such a state, and so far as we know there is no limit to the extent of gas that may be so illuminated. Hence we may suppose the exciting cause to be electricity, and the material on which it acts and which fills space to have the same properties as those possessed by gas. What is more, we can conceive the sun to be in such a condition as to produce that influence on this electricity which should cause the tail to occupy the direction it does. For such an electric discharge will be powerfully repelled by any body charged with similar electricity in its neighbourhood. The electricity would be discharged by the comets on account of some influence which the sun may have on them, such an influence being well within the limits of our conception. The appearances of the comet in detail, such as the emission of jets of light towards the sun and the form of the illuminated envelope, are all such as would necessarily accompany such an electrical discharge. In fact, if the possibility of such a discharge is admitted, I believe it will explain all the phenomena of comets. As to the possibility, or even the probability of such a discharge, I think it may be established on very good grounds. The tails of comets may or may not be one with their heads; but whichever is the case, it is certain that the difference in the appearance of comets and of planets indicates some essential difference either in the materials of which these bodies are respectively composed, or else in the conditions under which their materials exist. Now from the motion of comets we know that their heads follow the same laws of motion and gravitation as all other matter, and therefore we have good evidence, so far as it goes, that comets and planets are similarly constituted as regards materials. And since the appearance of a comet changes very much as it passes round the sun, any assumptions with regard to the material of comets, in order to account for their difference from planets, could not account for the variety of appearance the same comet presents at different times. On the other hand, the conditions of comets and planets must necessarily be very different, from the extreme difference in the shapes of the orbits they describe. Each planet remains nearly at a constant distance from the sun (whatever that distance may be), so that the heat or any physical effect the sun may have upon it will also be constant; on the comets its action must change rapidly from time to time, particularly when the comet is in certain parts of its orbit. Hence we may say that the temperature and general physical condition of planets is nearly constant, and that of comets for the most part continually varying. From these reasons it seems to me not only possible, but probable, that these strange visitors to our system are clothed in electrical garments with which the regular inhabitants are unacquainted. The electricity must after all depend on the composition of the comet, for known substances do not all show the same electrical properties. Hence by assuming comets to be composed of various materials, we have a source to attribute the different appearances presented by the different individuals. To the same source we may attribute the irregularity in the direction of their tails and the lateral streamers they occasionally send out. Secondly, I think this electrical hypothesis is supported by the to me seeming analogy between comets, the corona, and the aurora; an analogy which suggests that they must all be due to the same cause. They may be all described as streams of light or streamers, having their starting point more or less undefined, and traversing spaces of such extent and with such velocities as entirely to preclude the possibility of their being material in any sense of that word with which we are acquainted. The aurora has long been considered as an electric phenomenon, and recently the same effect has been produced by the discharge of electricity of very great intensity through a very rare gas, there being no limit to the space which it will thus traverse. This being so, why should not the tails of comets and the corona also be electric phenomena? Their appearance and behaviour correspond exactly with those of the aurora, and there is surely nothing very difficult in imagining the sun which is the source of so much heat being also the source of some electricity. Neither will there appear anything wonderful in the electricity of comets when we consider that of the earth. We must not look on our inability to explain the cause of such an electric discharge as fatal to its existence, for we cannot any more explain the existence of the electricity which causes the aurora. If we cannot explain from whence these electricities come, we can at least show that the conditions which are most favourable to the development of the aurora exist in much greater force on the comets than they do on the earth. The greatest development of the aurora borealis takes place at the equinoxes. There is a cessation in summer, and another in winter. Now, the equinoxes are the times when the action of the sun on our northern hemisphere is changing most rapidly. Hence the condition favourable for the aurora is change in the action of the sun. The same thing is pointed out by the diurnal variation in the electricity of the atmosphere. Now, as has been already shown, the change in temperature on the comets is incomparably greater than it is on the earth, and its variation corresponds with the variation in the splendour of the comet. Angström has also shown that the light from the aurora, the corona, and the zodiacal light, are all of the same character, or all give the same bright lines when viewed through the spectroscope, and that these lines correspond to the light from no known substance. This indicates that whatever this light may be, the incandescent material is the same in all cases; or may we not assume that it is the medium which fills space that is illuminated by the electric discharges? This would be supported by the fact that the light from the heads of two small comets indicated carbon, whereas that from the tails only gave a faint continuous spectrum. For an electric discharge would first illuminate the atmosphere of the comet, or even carry some of the solid material off in a state of vapour, and then pass off to the surrounding medium. Thus while the spectrum from the head would be that of cometary matter, the tail would be due to the incandescent ether. I would here suggest that gas, when rendered incandescent by electricity, may reflect light—it will certainly cast a shadow from the electric light—and if this be the case, part of the light from comets' tails may after all be reflected sunlight. At any rate, it is certain that the appearance of streamers, the rapidity of change and emission, the perfect transparency and the wave-like fluctuations which belong to these phenomena, are all exhibited by the electric brush; in fact, the electric brush will explain all these appearances which have defied all attempts at explanation on a material hypothesis. I have only to add that the main assumption involved in the electric theory is that space is occupied by matter having similar electrical properties to those of gas; and I would ask, is it not more rational to make such an assumption than it is to attribute unknown and inconceivable properties to cometary matter? Theories, even if founded only on rational speculation, often, I believe, prove very useful, inasmuch as they afford observers a definite purpose in their observations—something to look for, something to establish or to refute; and I publish these speculations of mine at this particular moment in the hope that they may perchance serve such a purpose.

PHILADELPHIA

American Philosophical Society, October 7.—Dr. Binton made some observations on a Mazahc theological work and grammar, indicating probable resemblances between the language of that nation and that of the Aztecs. He also described a grammar of the Moska nation of New Granada, prepared by Father Lugo.—J. A. Macneil described the ruins and other remains of the ancient nations, which he had discovered during several expeditions in Nicaragua, Costa Rica, Chiriqui, and Chiapas. He made especial reference to an extensive series of ruins he had discovered near the boundaries of Chiriqui and Costa Rica. One of the buildings was 600 feet in length, and 25 to 30 feet elevation. Among other sculptures he observed a well-executed stone alligator of large size.—Prof. Cope exhibited the remains of a new cretaceous tortoise, of the genus *Adocus* Cope, to be called *A. syntheticus*. He explained that he had been able to establish more fully the characters of the genus *Adocus*; that it was found to possess an intergular shield as in the Pleurodira, but had not the sutural union of the inferior pelvic elements with the plastron of that type. He said that these characters had been heretofore known as correlatives from the cretaceous period to the present day, and that this genus presented us with the first exception to the rule. The genus was therefore regarded as a generalised type, and to be elevated to the rank of a family. Prof. Cope exhibited a metatarsus of *Taelaps aquilunguis*, the first known, and said it proved the distinctness of those elements from each other in that type, and their slenderness, taken collectively. The specimen was an external one, without trace of a rudimental one outside of it; that its measurement, 16 inches, was indicative of a length of 18 inches to the median metatarsus, a length he had already assigned to it on theoretical grounds. Prof. Cope read a paper entitled, "Contribution to the Ichthyology of the lesser Antilles." Two new genera were described in it, viz., *Eleutheractis* and *Cryptotomus*.

Academy of Natural Sciences, November 8.—Prof. Leidy characterised three species of extinct turtles, obtained by Prof. Hayden's party from the tertiary deposits of Wyoming. Two were named *Emys Haydeni* and *E. Jeanesi*. The third, partaking of characters of the genera *Chelydra* and *Dermatemys*, was named *Baena arenosa*. A lacertian, as large as the largest of our living Iguanas, was characterised from the greater part of the bones of a skeleton imbedded in a rock of tertiary age, from Wyoming. The vertebræ have the characteristic ball and socket-joint to the bodies, but they are devoid of the zygosphene and zygantrum. The teeth are compressed conical, slightly curved, sharp-pointed, and trenchant. The remains were referred to a species with the name of *Saniwa ensidens*. The names *Baena* and *Saniwa*, according to Prof. Hayden, are those given to the turtle and lizard by one of the aboriginal tribes of the Upper Missouri.

VIENNA

I. R. Geological Institute, December 6.—Dr. Laube presented a memoir on "the Echinidæ of the Upper Tertiary Deposits in the Austro-Hungarian Empire," which will be printed in the fifth volume of the Memoirs of the Institute. The total number of distinct species is 37, six of which belong to the lower, and 31 to the upper part of the Mediterranean formation.—M. F. Foetterle "on the Sarmatic formation in the Bukowina and Northern Moldavia." It occupies an enormous space in both countries, and is easily divided in two members, an upper formed of yellow sand and sandstone, and a lower which consists of blue clay. Both contain fossils in abundance. The clay is everywhere permeated with very thin veins and layers of fine sand, which give free access to water, and thus cause the greatest difficulties for the railroad between Czernowitz and Jassy, which for long tracts passes over the clay, and is damaged by very long continued falls of rain.—M. Th. Fuchs reported on the Fauna of the Congeria-beds of Tihany and Kup in Hungary.—M. Ch. v. Hauer communicated the analysis of fire-proof clay of Fohnsdorf (Styria) which forms there a layer 9 feet thick in the browncoal basin. It consists of a hydrosilicate of alumina and magnesia, and belongs to the so-called soap-stones.—M. E. Tietze gave notice of the discovery of fossiliferous beds belonging to the brown Jura at Boletin, in Servia, and of neocomian and turonian beds in north-eastern Servia.—M. M. Neumayr presented a memoir on the jurassic flint-limestones of the Carpathians.—M. D. Stur exhibited a magnificent collection of eocene fossils from the environs of Vicenza, which had been purchased for the Museum of the Institute.

DIARY

FRIDAY, JANUARY 6.

GEOLOGISTS' ASSOCIATION, at 8.

SATURDAY, JANUARY 7.

ROYAL INSTITUTION, at 3.—Burning and Unburning: Prof. Odling (juvenile lectures).

SUNDAY, JANUARY 8.

SUNDAY LECTURE SOCIETY, at 3.30.—Malta and the Maltese, with a visit to St. Paul's Bay: Dr. Carpenter.

MONDAY, JANUARY 9.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

TUESDAY, JANUARY 10.

PHOTOGRAPHIC SOCIETY, at 8.
ETHNOLOGICAL SOCIETY, at 8.—On the Prehistoric Remains in Brittany: Lieut. S. P. Oliver, R.A.—Exhibition of Stone Implements from Queen Charlotte's Island: Dr. Hooker, C.B.—On a Cairn near Cefn, St. Asaph: Rev. D. R. Thomas, M.A., and Mr. T. McK. Hughes, M.A.

WEDNESDAY, JANUARY 11.

GEOLOGICAL SOCIETY, at 8.—On the older Metamorphic Rocks and Granite of Banffshire: Mr. T. F. Jamieson, F.G.S.—On the Connection of Volcanic action with Changes of Level: Mr. J. J. Murphy, F.G.S.—On the Geology of the neighbourhood of Malaga: Don M. de Orueba.
ROYAL MICROSCOPICAL SOCIETY, at 8.—On the Anatomy of *Ascaris lumbricoides*: Mr. B. T. Lowne, M.R.C.S.—On the use of Colloid Silica in preparing Crystals for the Polariscope: Mr. H. J. Slack.

THURSDAY, JANUARY 12.

ROYAL, at 8.30.
SOCIETY OF ANTIQUARIES, at 8.30.
LONDON MATHEMATICAL SOCIETY, at 8.—On Systems of Tangents to Plane Cubic and Quartic Curves: Mr. J. J. Walker.—On the Order and Singularities of the Parallel of an Algebraical Curve: Mr. S. Roberts.

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