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"To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH

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THE TRANSITS OF VENUS IN 1874 *AND* 1882

A PARLIAMENTARY paper issued at the close of last session gives some information on what is intended to be done in the matter of the great approaching astronomical events of 1874 and 1882, which will interest many readers of NATURE. The correspondence on this subject between the Astronomer Royal and the Hydrographer of the Admiralty, and between the Hydrographer and the Secretary to the Admiralty, is given at full length, and together with the appended remarks of Captain Toynbee, Admiral Ommaney, Commander Davis, Mr. Stone, and Mr. Warren De la Rue, well merits careful perusal. On the whole it seems to be at least doubtful whether the requisite Antarctic station for the observation of the transit, which in the opinion of the Astronomer Royal should, if possible, be in the neighbourhood of Mounts Erebus and Terror, can be attained, or if, when attained, it is likely to be possible to make observations from it. But this Antarctic station is only required for the transit of 1882, and there is ample time to make a preparatory Antarctic expedition to ascertain the doubtful point. In the meantime, however, let us see what has been settled about the transit of 1874. For the proper observation of this event the Astronomer Royal informs us that it will be necessary, after making allowance for all the aid that may be expected from foreign and colonial observatories, to organise expeditions to the following five stations:—(1) Oahu (Sandwich Islands), (2) Kerguelen's Island, (3) Rodriguez, (4) Auckland (New Zealand), (5) Alexandria. At the first three of these stations—namely Oahu, Kerguelen's Island, and Rodriguez—it will be necessary to make preparatory observations for twelve months, in order to ascertain the absolute longitudes of these places, which are not exactly known. The total cost of these proposed observing expeditions for the transit of 1874 is estimated by the Hydrographer at 10,404*l.*, a sum which, it must be admitted, is moderate for work of such surpassing importance, and, as the Lords of the Treasury state that they have "no objection to offer" to the proposed expenditure, we may take it for granted it will be so far carried out. But what I wish to call attention to at the present moment is the valuable opportunity thus offered for still further augmenting the importance of this event to the progress of science generally, by converting these proposed astronomical expeditions into expeditions for general scientific observation. At three of the spots to be visited it will be necessary to keep up an observing party, more or less extensive, for upwards of twelve months. Now, it so happens that the three spots thus selected for astronomical observatories are also of very great interest for biological studies. The Sandwich Islands are well known to be the seat of a most peculiar indigenous flora and fauna, which has been hitherto very incompletely explored, rivalling perhaps even that of the Galapagos in eccentricity. They are likewise the seat of some of the most stupendous volcanic phenomena known on the globe. Who can doubt that one or more zoologists, botanists, and geologists would find ample work during a twelvemonth's sojourn in these islands, and would reap a rich harvest of results? The little island of Rodriguez was formerly the residence of a bird allied to the Dodo, and probably of other extinct forms o life. Professor Newton and his brother have, it is true, already made us tolerably well acquainted with the osteology of *Pezophaps*, as this gigantic ground-pigeon is termed. But there is no doubt that a careful exploration of the bone-caves of Rodriguez will lead to still further discoveries as regards *Pezophaps*, and most probably result in bringing to light other unknown extinct inhabitants of the submerged continent, which was the ancient focus of Didine life. Kerguelen's Island, the third point selected for a temporary astronomical observatory, is also in many points worthy of renewed investigation. Although we may probably believe Dr. Hooker, who visited it during Sir James Ross's Antarctic Expedition, left but few plants for future botanists to discover, the seals and whales that frequent its shores, together with the sea-fowl and other inhabitants of the coast, would well occupy the attention of zoologists. It is, moreover, of especial importance that the "abundant fossil remains" of its now extinct forests should be thoroughly investigated, in order to obtain more knowledge of the former distribution of land and water in the South Pacific. I have mentioned only some of the principal and most noticeable points for biological inquiry in each of these three localities. But, as every naturalist knows, in the case of such isolated land-areas as these marine islands, it is of especial importance to the progress of our knowledge of general geographical distribution to have a complete account of every branch of their faunas and floras, both recent and extinct. I beg leave, therefore, to urge upon all who are interested in the progress of science, the importance of not losing the opportunity that now presents itself. The additional expense of attaching two or three qualified Natural History observers (or at any rate collectors) to these three expeditions could not be very great. The numerous American and Russian exploring expeditions are invariably accompanied by zoological and botanical collectors, nor is the money required to publish the results obtained by them grudged by the Governments of these countries. Even poverty-stricken Austria did not send the *Novara* round the world without a competent corps of naturalists, and we are now reaping the fruits of the abundant harvest which they gathered in. Far from lagging behind, wealthy England ought to take the lead in such cases, and instead of its being necessary, when an opportunity of the sort occurs, to take all kinds of extraordinary steps in order to induce the Government to take advantage of it, such things should be done as matters of course.

P. L. S.

PHILOLOGY AND DARWINISM

[The following paper was written nearly half a year ago, before the translation into English of Prof. Schleicher's two pamphlets, by Dr. A. V. W. Bikhers. After reading the article on Dr. Bikhers' translation, by Prof. Max Müller, in a previous number of NATURE, it struck me that many readers might be glad to have some further account of Schleicher's views. F. W. F.]

THE relations of the science of language to the Darwinian hypothesis have been touched upon by one of the most acute and learned of modern scholars,

Prof. August Schleicher, of Jena, whose lamented death a year ago,* at the early age of forty-eight, is a severe loss to European science, was an ardent supporter of the doctrine of variability of species. Besides being a most eminent linguist, he had long been interested in practical botany; and as a cultivator of ferns he had enjoyed many opportunities of observing the apparent transformation of natural subdivisions. It was not, however, as a botanist that Mr. Darwin's book was mainly interesting to him, but far more from the light which his theories seemed to throw on the phenomena of language. The first edition of the "Origin of Species" appeared in November 1859, and Prof. Schleicher, three years before he had met with Brown's German translation of it, had in his book, "Die Deutsche Sprache" (pp. 43, 44), called attention to the struggle for existence among words, the disappearance of primitive forms, and the immense development and differentiation which may be produced by ordinary causes in a single family of speech. On receiving Mr. Darwin's book from his friend Prof. Häckel, he wrote him a letter, which has since been published, on "Die Darwinsche Theorie und die Sprachwissenschaft;" and in answer to the objection that he had, in this letter, assumed that languages were material existences, having a real natural life, he wrote a second pamphlet on the "Importance of Language for the Natural History of Mankind."

The general line of illustration which he adopts had probably struck others, and it had certainly struck me before I read or heard of Prof. Schleicher's pamphlet; but as that little work is as yet but slightly known in England, it will probably be interesting to some readers if I sketch the outline of his arguments. There was nothing fanciful or precipitous in Schleicher's writings. He was one of the most strenuous supporters of the strictly scientific character of all true linguistic inquiry; one of the most severe opponents of those vague fancies, imaginative theories, hazardous etymologies, and *à priori* inferences which have thrown suspicion on philological work. He had owed much, even in the study of language, to such books as Schleiden's "Scientific Botany," and the "Physiological Letters" of Carl Vogt; and he wished to found linguistic science on the structure of the organs of articulation and on recognised vital laws. He regarded languages as natural organisms which, in accordance with definite physical influences, and independently of human will, are produced and developed, grow old and die, and therefore manifest the series of phenomena to which we give the name of "life." Hence he regarded Comparative Philology, not as an *historical*, but as a *natural* science; and we think that his views will be shared by all who have added to their linguistic inquiries some sound knowledge of either zoology or botany.

The researches of Sir Charles Lyell have shown that the present condition of the earth's surface is due, not to cataclysms and conflagrations, but to the slow result of natural laws continuing to act during thousands of years. Similarly, Mr. Darwin showed that the existing conditions of species might have been originated by continuous insensible modifications, working for an indefinite period of time. It was the main object of Prof. Schleicher to show that in all essential particulars the working of similar laws accounted for the existing phenomena of languages. The principles of classification apply to language no less than to animal and vegetable organisms. A genus corresponds to a linguistic stem; classes to linguistic families; subspecies to dialects; varieties to minor dialectic peculiarities; and, finally, individuals to those special modes of varying utterances which distinguish man from man.

Mr. Darwin has constructed an ingenious diagram* to illustrate the immense scope which must be allowed for gradual divergence of characters in animal and vegetable species derived by natural selection from an original genus. Schleicher has made an exactly similar table to serve as a genealogical tree for the Aryan Families of language. But here the philologist has a distinct advantage, and the study of his results may be most suggestive to the naturalist: for the Darwinian diagram is to a great extent ideal and hypothetical; while the table of languages is merely an expression of indisputable discoveries. Any one who has clearly understood the certainty of the fact, that languages at first sight so different as Greek and French, Icelandic and Portuguese, Sanskrit and Lithuanian, are yet connected with each other by close bonds of union, and that the phenomena they exhibit are due to gradual differentiation from a single stock, will undoubtedly be more able to conceive the possibility of Newfoundlands, and Greyhounds, and King Charles's Spaniels, and Wolves being lineal representatives of a common type.

And further than this, the philologist has another very positive advantage over the naturalist. The ethnologist can not only *prove*, where the naturalist must be content to *conjecture*, but can also more easily exemplify the birth of new forms out of anterior ones, and can carry out his examination on a greater scale. There are some languages and families of languages which have been under close observation for two thousand years, and which furnish us with written specimens of forms which have undergone immense subsequent modification. In comparing modern French with the Latin of the XII. Tables, or Mahratti with the Sanskrit of the Vedas, we have a sure and solid basis of observation, in which, by the aid of records incapable of falsification, we can observe the corroding and modifying influence of time on human speech. The effects of foreign influences on different languages even furnish us with some analogy to *crossing*, which is so important an element in all zoological inquiries. In point of fact, the possession of written materials extending over many ages led philologists to be among the first to deny the sudden origin of separate species. The Science of Language offers the most demonstrable and instructive examples of the gradual growth of species from common primitive forms, although it is as impossible in language as it is in zoology to draw certain and definite lines of demarcation between genera, species, and varieties.

It may be asked whether the science of language is able at present to demonstrate the growth of all families from one primitive mother-tongue? The answer must be frankly in the negative, and perhaps one reason for this may lie in the fact that there is not any linguistic family except the Aryan, of which the archetypal forms have been reconstructed from their derivatives.† But, on the other hand, as regards the *morphology* of language, we are

* He died on Dec. 6, 1868.

* Origin of Species, p. 130. (4th edition.)

† To effect this was the object of Prof. Schleicher's *Compendium der Vergleichende Grammatik.*

perfectly in a position to show that the rudest forms of the most developed language have sprung, by insensible derivation, from phonetic signs as vague, simple, and monosyllabic as those of Chinese itself. These signs left the mutual *relations* of ideas unexpressed. There were at first no special vocal expressions, no *organs* for the fulfilment of grammatical functions, or the interdistinction of nouns and verbs, much less of conjugation or declension. Such words as *That*, *gethan*, *Thuer*, *Thäter*, *thätig*, all point back to a root *dha*, which contained in itself the undeveloped germ of all sorts of verbal, nominal, and adverbial modifications. And in this respect the ultimate roots of the Aryan languages closely resemble in character the actual words of those languages which have remained to this day as nearly as possible in their primitive condition. Such roots may without fancy be called speech-cells, in which the rudiments of all special organs are implicitly *involved*, but in which they are as little *developed* as in the germinal vesicles which represent the earliest forms of animal and vegetable life. There may have been multitudes of such sound-cells, as it were, from which different families of language have sprung by special lines of development, just as, according to the Darwinian hypothesis, many primordial cells, presenting a close similarity, may have been the earliest rudiments of all living organisms.

In speaking of the extinction of species and the struggle for existence, Mr. Darwin uses language which may be literally applied—applied without even verbal modification—to the phenomena of languages. Here, no less than in the animal and vegetable kingdom, the dominant forms of the prevailing groups tend to leave many modified descendants, while the imperfection of the weaker groups leads to their gradual disappearance. But the complete extinction of a linguistic type is a slow process, and just as extinct animal forms may leave behind them a few decaying representatives in inaccessible or solitary places, so in the mountain-valleys of the Pyrenees and the Caucasus, we find isolated dialects which may be the fragmentary relics of tongues once spoken in immense districts. But a language once extinct, like an extinct species, can never under any circumstances reappear; and its place is occupied by the nearly related but greatly modified groups of predominant families, which are precisely those which undergo the completest differentiation in the course of their gradual victory over less happily constituted forms. And in consequence of the extinction of languages many *intermediate* forms have perished; the primitive relationships of languages have been disturbed by all sorts of external influences, and consequently languages radically different are now found existing side by side. All this, as every naturalist is well aware, represents a condition of things precisely similar to that which prevails in animated nature.

Mr. Darwin, in his great work, devotes a few words to the classification of languages as affording a confirmation of his theories. It does so to an extent of which probably he was not at first aware. In two capital points, viz., (1), the immense changes which can be effected by infinitesimally gradual modification; and (2) the preservation of the best and strongest form in the struggle for life, Mr. Darwin's hypothesis may be confirmed and verified by the entirely independent researches of the comparative philologist. These are the two points to which Prof. Schleicher wished to draw attention in the pamphlet which I have here epitomised. They do not indeed represent the whole of the linguistic facts which might be adduced on this side of the question, and they leave out of sight others which might be alleged with great force in favour of an opposite view. Some of these I have endeavoured to set forth elsewhere,* and possibly there may be some future opportunity of again bringing this subject before the reader. My present object was to make the views of Prof. Schleicher more widely known than they have yet become among English naturalists and scholars.

F. W. FARRAR.

THE PRIVATE LIFE OF GALILEO

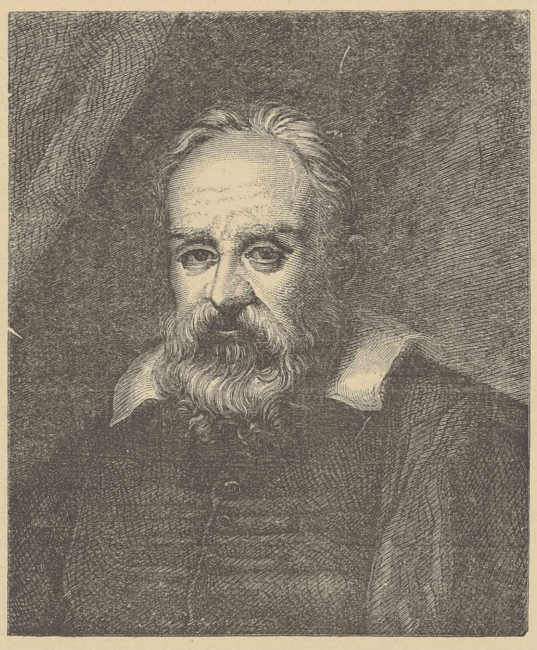
The Private Life of Galileo. Compiled principally from his correspondence and that of his eldest daughter, Sister Maria Celeste, nun in the Franciscan convent of S. Matthew at Arcetri. 307 pp. (London: Macmillan and Co. 1870.)

THE numerous works which have appeared with Galileo for their theme may be divided into three classes. Firstly, those which relate more particularly to his persecution by the Church, the position and influence of the Sacred College in his day, and its attitude towards science. Secondly, those which treat of his scientific labours apart from himself, their nature and character, and their influence on the propagation of truth, the advancement of modern philosophy, and the downfall of Aristotelianism. Thirdly, those which discuss his private life. The first and last of these are often blended, more or less, and of necessity, but we know too little of his scientific labours. M. Parechappe has well remarked, "Le savant s'est effacé dans le martyr." The works of Galileo, if much talked of, are certainly little read—"Il Saggiatore" and the "Dialoghi" are even less read than the "De Augmentis Scientiarum" and the "Novum Organum;" while the "Principia" of Descartes occupies a position of notoriety midway, perhaps, between "Il Saggiatore" and the "Novum Organum," and we have a little difficulty in placing the writings of Hobbes. Yet it is undeniable that the works of these four men have produced a more profound and permanent influence upon human thought than any which preceded them. There is but one epoch in the history of the world to be compared with their epoch; it is that of Aristotle.

The work before us belongs both to the first and third of the above divisions, it relates mainly to the private life of Galileo, and resembles Arduini's "Primogenita di Galileo Galilei," more than any other work on the subject. The account of the private life of Galileo, unlike many such accounts, does not give us much insight into the manners and customs and conditions of society at the time of which it treats, both because Galileo had so little real domestic life, and because the main correspondence which furnishes these private details took place between a nun (who of all persons can know least of the external world) and Galileo himself, and her letters to him have been preserved, while his answers to them have perished. Your great philosopher as a rule is exceedingly

* See a paper on "The Growth and Development of Language" in the *Cambridge Journal of Philology*.

undomestic, and the proofs of this are so common that we need not quote a single example; the petty details of home weary them, and prevent the abstraction requisite for their labours: so the ancient Brahmans, who reasoned as profoundly as any light of Western civilisation, lived in the solitudes of the forests of Ancient India; so Descartes withdrew himself from the world, and remained

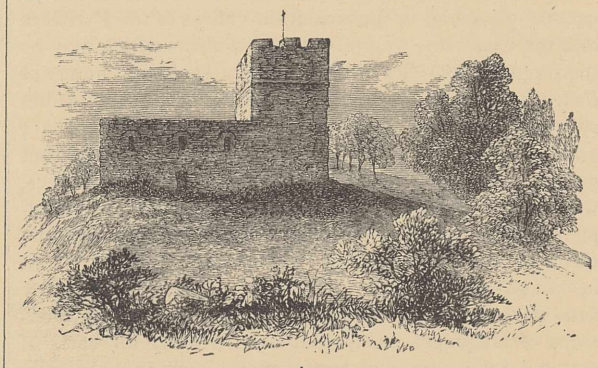


GALILEO
(*From Ramsey's picture in Trinity College, Cambridge*)

buried in the quiet of his country house while he produced his "Meditations."

Galileo also was by no means domestic. Of his three natural children, his son Vincenzio was a constant thorn in his side. He was a lazy fellow, who was always writing to his father for money, and who, Italian-like, preferred to idle away his life in singing and lute-playing, to adopting any profession or attempting to get his own livelihood. We cannot find one good quality in Vincenzio Galileo; he was mean, selfish, inconsiderate and unnatural in his behaviour towards his father. One example of this is sufficient. He had quartered himself on his father, together with his wife and children, when the plague broke out in the neighbourhood; whereupon Vincenzio deserted the old man, and went to a more healthy locality, leaving his father to take his chance with the other inhabitants of the district. Galileo's daughters Polissena and Virginia were placed in the Convent of S. Matthew, at Arcetri, in 1614, when the eldest was only thirteen years old; henceforth they became Sister Maria Celeste and Sister Arcangela. Of the latter we hear but little, but Sister Maria Celeste constantly corresponded with her father, and the greater number of her letters have been preserved, and are now in the Palatine Library at Florence. These letters contain some interesting details of convent life of the period, but of necessity they do not bear upon many of the doings of the outside world; their general tenor is the same throughout; they are full of her love for Heaven and for her "dear lord and father," as she was wont to call Galileo, and they almost invariably pass to an opposite extreme of matters exceedingly of the earth, earthy—the baking of cakes, the mending of linen, the getting up of his collars and so on. She tells her father all the minute details of her work, as: "I have been extremely busy at the dinner-napkins. They are nearly finished; but now I come to putting on the fringe, I find that of the sort I send as a pattern a piece is wanting for two dinner-napkins: that will be four *braccia*." The last paragraph of this desultory letter begins, "These few cakes I send are some I made a few days ago, intending to give them to you when you come to bid us adieu;" and ends, "I thank Him for everything, and pray that He will give you the highest and best felicity;" and a postscript immediately follows this—"You can send us any collars that want getting up."

Galileo's villa was very near the convent, and a constant interchange of courtesy seems to have taken place; Galileo sent money and presents of meat and wine, while Sister Maria Celeste sent him plums, and baked pears, and candied fruits, and cakes, and mended his linen and kept his wardrobe in order. Her love for him amounted almost to worship, at least to veneration. When at length, worn out by watching in the convent infirmary, by ill health, and by the many privations inseparable from a convent life, she felt her end approaching, Galileo was in confinement at Siena, and she feared she should see him no more; but he was allowed to retire to his own house, and arrived at Arcetri in time to see his daughter before her death. Writing of this time (1634), Galileo says: "Here I lived on very quietly, frequently paying visits to the neighbouring convent, where I had two daughters who were nuns, and whom I loved dearly; but the eldest in particular, who was a woman of exquisite mind, singular goodness, and most heartily attached to me."



GALILEO'S TOWER

There is much in this "Private Life of Galileo" of great interest in connection with his scientific work, his books, his persecutions and trial by the Sacred College, and his condemnation; but we have preferred to keep strictly to his more private life, as the theme is so large, that if we once touched upon his scientific work and its results, we should require far more space than could be placed at our disposal here.

Galileo continued actively employed to within a few years of his death, in January 1642. During his latter years he was a great sufferer. "I have been in my bed

for five weeks," he writes to Diodati, in 1637, "oppressed with weakness and other infirmities, from which my age, seventy-four years, permits me not to hope release. Added to this, *proh dolor!* the sight of my right eye, that eye whose labours (I dare say it) have had such glorious results, is for ever lost. That of the left, which was and is imperfect, is rendered null by a continual weeping." Thus the poor old man complained, until finding that his blindness was incurable, and that his many ills were increasing, he ceased repining, and begged his friends to remember him in their prayers, till his unhappy chequered life was closed by death.

G. FARRER RODWELL.

OUR BOOK SHELF

Reptiles and Birds. A Popular Account of their various Orders, with a Description of the Habits and Economy of the most interesting. By Louis Figuier. Illustrated with 307 woodcuts. Edited and adapted by Parker Gillmore. 1870. (London: Chapman and Hall.)

A VERY pretty book for a drawing-room table. The description of the several families of both reptiles and birds is filled with anecdotes culled from all sorts of writers, some of them sufficiently amusing, others, to say the least, of doubtful accuracy; witness the following in reference to the stork:—"The inhabitants of Smyrna, who know how far the males carry their feelings of conjugal honour, make these birds the subject of rather a cruel amusement. They divert themselves by placing hen's eggs in the nest of the stork. At the sight of this unusual production the male allows a terrible suspicion to gnaw his heart. By the help of his imagination he soon persuades himself that his mate has betrayed him; in spite of the protestations of the poor thing he delivers her over to the other storks who are drawn together by his cries, and the innocent and unfortunate victim is pecked to pieces." We should like to see this cruel amusement played out once to the bitter end, and should then, but not till then, believe it.

The drawings and woodcuts are as excellent as they are numerous.

Beiträge zur Lehre von den Functionen der Nervencentren des Frosches. "Essays on the Functions of the Nerve-centres in the Frog." By. Prof. D. Fried. Goltz, of Konigsberg. pp. 130. (Berlin, Hirschwald, 1869.)

THIS little brochure, which, though small, contains the result of much work, is divided into four sections. 1. On the reflectorial excitation of the voice in frogs. 2. On the physiology of generation in the frog. 3. On the inhibitory influence which can be exerted on the reflex actions; and 4. On the seat of the mind (Seele) in frogs; beside investigations on the centre for the maintenance of equipoise, and the centre for locomotion. It may be observed that notwithstanding the experiments were all undertaken in frogs, those little martyrs to science, yet that some of the results at least have a direct bearing on the functions of the centra in the higher animals, and even on man himself. The results of his experiments in reference to the seat of the mind are at variance with those of Pflüger and others, who hold that the spinal cord participates with the brain in its possession. M. Goltz maintains, on the contrary, that the brain is the *exclusive* seat of all intellectual processes, and consequently, that a frog from which the whole encephalon has been removed, is an organism presenting only a complex series of reflex processes. The removal of the *cerebrum* alone deprives the animal of all voluntary movement, and of all those faculties which are included under the general head of consciousness; it still retains, however, certain powers of co-ordination. If the corpora quadrigemina are then removed, it no longer possesses the power of preserving the equipoise of its body or the accommodation of its movements. The corpora quadrigemina therefore, he concludes, constitute the centre for the maintenance of the equilibrium of the body. The cerebellum, on the other hand, is the centre for locomotion of the whole body.

Schriften der Naturforschenden Gesellschaft in Danzig Neue Folge, Zweiten Bandes, Zweite Heft, 1869.

THE Danzig Natural History Society publishes annually a part of its Transactions, which, although but little known in this country, often contain valuable papers. In the part for 1869, which we have just received, we find an elaborate memoir by Dr. Bail, on the epizootic fungi which affect the caterpillars injurious to forests, and it is some comfort to think, that while these vegetable parasites do nothing but mischief among the silk establishments of the south of Europe, they are regarded as serviceable in other quarters. This part also contains the continuation of M. A. Menge's valuable monograph of the Prussian spiders, of which the author has now described and figured 157 species. This memoir is indispensable to the archæologist, and is in itself a wonderful result of the most minute research—research so minute, in fact, that the author is unfortunately led to magnify the importance of slight differences, and thus to establish a great number of new genera upon very slight grounds. M. Menge also describes and figures a species of scorpion and two species of spiders from amber; each of the latter forms the type of a new genus. Dr. Bail contributes a short but interesting paper on the occurrence of androgynous flowers in monœcious and dicœcious plants. Besides some minor communications on subjects connected with natural history, the part contains two memoirs which one would hardly expect to find in the Transactions of a society of naturalists, namely, a description of the construction and theory of a marine distance-measurer, and an investigation of the moon with reference to its ellipsoidal form, by M. E. Kayser, who describes himself as "Astronomer to the Natural History Society of Danzig." The former of these papers is illustrated with three folding plates.

Notes on Microscopic Crystals included in some Minerals. By Isaac Lea. From the Proceedings of the Academy of Natural Sciences of Philadelphia. Read February 16 and May 11, 1869.

IN these two papers the author gives an account of the minute crystals included in sapphire, garnets, and several other minerals, which in some cases are arranged in a number of definite planes, so as to give rise to the appearance seen in the so-called "star sapphires." The essays are illustrated by a plate, which shows the character of the crystals in a very satisfactory manner. The author is, however, not quite correct in thinking that such included crystals had not been previously described by several authors. Söchting, in his excellent work,* gives an account of some facts similar to those observed by Mr. Lea; and Messrs. Sorby and Butler, in their paper on the microscopical structure of rubies, sapphires, &c.† describe "the small plate-like crystals, often triangular in form, with an angle very acute. They are very thin, and arranged parallel to three principal planes of the sapphire," and are thus precisely like those now figured by Mr. Lea. There can be no doubt that the study of the minute crystals included in minerals often throws much light on their origin, and they play a far more important part than is often supposed, and serve to explain some of the discrepancies met with in their chemical composition.

* Einschlüsse von Mineralen u. s. w. Freiberg, 1860.
† Proceedings of the Royal Society, vol. xvii., p. 291.

LETTERS TO THE EDITOR

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

Rotation of a Rigid Body

My previous communication about the rotating ellipsoid to this journal, has attracted the attention of M. Radau. "One touch of *Nature* makes the whole world kin." In a note addressed to me full of true dignity, this gentleman has made much more than sufficient reparation for his previous trifling act of inadvertence, and states that to his great regret he had misunderstood my meaning, in the passage of my memoir in question, and that "sa critique n'est pas fondée." I, on my part, deeply lament the unnecessary tone of acerbity in which my reference to this criticism was couched, and wish I could recall every ungracious expression which it contains. "When I spoke that, I was ill-tempered too."

I will pass over this, to me, painful topic, to say two or three words on the mode in which the rotating ellipsoid may be supposed to roll or *wobble* on a rough plane, with its centre fixed. My solution may remind the reader of Columbus's mode of supporting an egg on its point—or, rather, of a fairer mode which Columbus might have employed, and which would not have necessitated the breaking of the shell, viz., by resting the blade of a knife or rough plate on the upper end of his egg.

So, to make an ellipsoidal or spheroidal top roll, with its centre fixed—say, upon a rough horizontal plane—imagine a second horizontal plane in contact with the upper portion of its surface; then the line joining the two points of contact will pass through the centre of the top. We may conceive a slight perforation in either or each plane at its initial point of contact with the top, and a screw wire introduced through this, and inserted into a female screw in the body to be set rolling (a mode of spinning which Sir C. Wheatstone recommends as the most elegant in any case, and in this case evidently the most eligible). On withdrawing the wire with a jerk, the top may be set in motion about its centre, in such a direction as to remain in contact with the two planes, and if these be sufficiently rough the motion will eventually be reduced to one of pure rolling between them, the axis (*i.e.*, the line joining the two points of contact) continually shifting, but the centre remaining absolutely stationary: for, vertical motion this point cannot have, so long as the top continues to touch both planes, and any slight horizontal motion (if it should chance to take on such at the outset) would be checked and ultimately destroyed by the friction, which would also keep the two points of contact stationary (like the single point of contact of a wheel rolling on a rail), in each successive atom of time. Thus the motion upon the lower plane would in the end be precisely the same as if the upper plane were withdrawn, and the centre of the top kept fixed by some mechanical adjustment. If the spin were not sufficiently vigorous, after a time the rolling top might quit the upper plane, and of course sooner or later by the diminution of the *vis-viva* due to adhesion, resistance of the air, imperfection or deformation of the surfaces, and other disturbing causes, this would take place, but abstracting from these circumstances the principal axes of the spheroidal or ellipsoidal top would move precisely in place and time like the "axes of spontaneous rotation" of any free body of which the top was the "Kinematic Exponent."

I do not pretend to offer an opinion what materials for the planes and rolling body (ground glass and ebony or roughened ebonite have been suggested to me) it would be best to employ, or whether the "wobbling top" could easily be made to exhibit its evolutions. It is enough for a non-effective, unpractical man (as unfortunately I must confess to being) to have shown that there is no intrinsic impossibility in the execution of the conception.

With regard to the friction and pressure: if W be the weight of the body, F and P the friction and pressure in the case of a single plane (the values of which are set out in my memoir, pp. 764—766, "Philosophical Transactions," 1866), it may easily be proved that eventually the friction at each point of contact will be $\frac{F}{2}$, the pressure upwards at the lower point $\frac{P+W}{2}$, and downwards at the upper one $\frac{P-W}{2}$, so that if P should become equal to W the top would quit the upper plane and the experiment come to an end. At p. 766 of my memoir the factor $\sqrt{M\Lambda}$ has accidentally dropped out of the expression for P which I mention here, in case any one should feel inclined to consult the memoir in consequence of this note. Mr. Ferrers has taken up my investigations, and given more compendious expressions than mine for F and P; with the aid of these it would probably be not difficult to determine the maximum value of $\frac{F}{P}$ so as to assign the necessary degree of roughness of the confining planes, and also to ascertain under what circumstances P — W would become zero, but I do not feel sufficient interest in the question, nor have I the courage to undertake these calculations with the complicated forms of P and F contained in my memoir. Mr. Ferrers' results are contained in a memoir ordered to be printed in the "Philosophical Transactions," and will shortly appear.

In my memoir will be found an exact kinematical method of reckoning the time of rotation by Poinsot's ellipsoid when the lower surface is made to roll on one fixed plane at the same time that its upper surface is shapened off in a particular way (therein described) so as to roll upon a parallel plane which turns round a fixed axis; this upper plane is compelled to turn by the friction, and acts the part of a moveable dial in marking the time of the free body imaginarily associated with the ellipsoid. I have also shown there that the motion of any free body about a fixed centre may be regarded as compounded of a uniform motion of rotation and the motion of a disc, or, if one pleases, a pair of mutually bisecting cross-wires left to turn freely about their centre. But I fear that NATURE, used to a more succulent diet, has had as much as it can bear upon so dry a topic, and, although having more to say, deem it wiser to bring these remarks to an end.

J. J. SYLVESTER

"Dutch" or "Deutsch"

THERE is a short note in Mr. Huxley's lecture in the last number of NATURE, which I have read several times in the vain hope of finding out its meaning. Mr. Huxley speaks of "the much debated question, did the Germans of Cæsar and Tacitus speak 'Deutsch' (not 'Dutch,' *pace* Mr. Freeman) or Celtic." What has my "peace," or anybody else's peace—save, perhaps, the *Pax Romana*—to do with it?

I do not see why Mr. Huxley brings in my name. He can hardly suppose that I do not know that *Deutsch* is the German form—I can hardly suppose that he does not know that *Dutch* is the English form—of the name otherwise written *Tütsch*, *Teutonicus*, *Theotonicus*, *Theotiscus*, and endless other ways. He can hardly think that I have never opened a modern German book: I can hardly think that he has never opened an English book of the sixteenth or seventeenth century. If he has opened any such book in which matters of this kind are likely to be touched upon, he must surely know that the words *Dutch* and *Dutchman* were then used in a very wide sense. A "Dutchman" might be a native of Holland; he might be a native of Bavaria. And the division into *High Dutch* and *Low Dutch*, or *Nether Dutch*, was then perfectly well understood.

I do not know what Mr. Huxley's objection is. I use the word as one ready made, as more convenient than the Latin for *Teutonic*, and as more easily admitting the addition of the qualifying syllables *High* and *Low*. I should not use *Deutsch* in this sense for two reasons. First, it is not an English form, and I should no more, in writing English, say that certain people talked "Deutsch," than I should say that they talked "Français." Secondly, the word *Deutsch* (like the word *German*) would to most people convey the idea of one particular Teutonic tongue, while I am probably speaking of Teutonic tongues in general. Mr. Huxley's question, "Did the Germans of Cæsar and Tacitus speak *Deutsch?*" may mean either "Did they speak a Teutonic tongue of any kind?" or "Did they speak the particular Teutonic tongue which to most peoples' minds would be suggested by the words *German* or *Deutsch*, namely, the *High-Dutch?*" Which Mr. Huxley means I do not know. For my own part I believe that they spoke Dutch or Teutonic, but *Low* Dutch and not *High*.

EDWARD A. FREEMAN

Somerleaze, Wells, March 21

The American Eclipse

WILL you grant me space for a few words on my spectroscopic observations of the American eclipse, and what seem to me the inferences to be drawn from them? I make the request the more freely because I have met from time to time allusions to them in your journal, and remarks, some of which seem to require my notice, if only to express my appreciation of

the considerate courtesy with which they have been treated by gentlemen who differ very widely from my conclusions.

1. Regarding the use of the spectroscope in the observation of "contacts."

I think the language of Mr. Stone and some others in a discussion of the matter at a meeting of the Astronomical Society, reported in your columns in December last, implies a misapprehension. What I have proposed (and executed in the case of the moon) is to use the extinction of the bright C line in the spectrum of the chromosphere as a criterion of contact with the limb of the *photosphere*, not with the upper surface of the chromosphere, which would, of course, as indicated by Mr. Stone, be a perfectly worthless observation.

The advantage of the method lies in this, that it furnishes an easily apprehended phenomenon to be watched for, and gives every advantage of preparation to the observer.

With an instrument of moderate dispersive power, the slit must be normal to the sun's limb, and an accurate knowledge of the expected point of contact is required: with a more powerful instrument the slit can be placed tangential, opened somewhat widely, and thus all difficulty on this score avoided, as I have pointed out in my report. I see by a paper of Mr. Proctor's in the December number of the "Monthly Notices," that Mr. Huggins suggests the same plan.

Perhaps I may remark in passing that the idea of using the spectroscope in this manner to observe the contact of the moon with the sun's disc, was conceived before the event, so that the observation was made deliberately and by pre-arrangement—not at all accidentally, as would rather seem to be implied by one of the opening sentences in the article of Mr. Proctor's above referred to. To M. Faye, however, belongs whatever merit there may be in the method, for he proposed essentially the same thing in January 1869. But I knew nothing of this at the time of the eclipse, nor indeed till long after.

2. The self-luminosity of the Corona.

It is not impossible that the so-called corona may be complex. Some portion of its radiance may *perhaps* originate in our own atmosphere, although I do not yet find myself able to accord with the conclusions of Dr. Gould and Mr. Lockyer in this respect, and am strongly disposed to believe that the *whole* phenomenon is purely solar.

This much appears certain, however, that there exists outside of the chromosphere properly so called (*i.e.*, the envelope of *red hydrogen*), and as distinct from *it*, as it is from the *photosphere*, an immense atmosphere of self-luminous substance, extending to a distance of from 5′ to 8′ from the sun's surface, and probably much further in places—phosphorescent dust or fog in a glowing gas.

In support of this idea I adduce the photograph of Mr. Whipple, taken at Shelbyville, Ky., with an exposure of 40^{s}. On this, the *photolytic corona* (if I may use the expression to distinguish it from the *visible corona*, whose points of maximum brilliance were, according to Dr. Gould, entirely different), reaches a height of 6′. Prof. Harkness observed the 1474 line in the spectrum of the corona at a distance of nearly 5′ from the sun's limb, and not near to any prominence. I do not know the precise elevation at which I saw it, but it was not less than 3′ or 4′.

Indirectly, also, the idea is confirmed by the spectroscopic observation of Prof. Pickering, who used a single prism instrument, with the slit simply directed towards the sun, not attached to a telescope. He saw only three or four lines, the brightest in the green near E. Now, since this line, when observed by throwing a large image of the sun on the slit, is very faint as compared with C, D_3, and F, its intensity, as seen by him, can only be accounted for by supposing that the luminous area from which it was derived far exceeded that of the chromosphere and prominences.

I have noticed also that some of the observers of the Indian eclipse (Rayet and Pogson) speak of the intensity of the green line. Did they observe in the same manner as Prof. Pickering?

I need hardly add that Prof. Pickering's observation of the non-polarisation of the corona concurs with what has been said.

As to the faint continuous spectrum, I am sure that the reported absence of dark lines was not the result of insufficient observation.

I could not have failed to see D, E, *b*, 1961, F and G had they existed, for in a spectrum of similar brightness formed by a light from a cloud, not only these but many other lines are visible in my instrument. Now, the absence of some of these might, perhaps, be accounted for on the ingenious hypothesis proposed by Mr. Lockyer, and reported in your No. of February 3; but this would not apply to D, E, or G.*

But if we admit the existence of faintly luminous solid or foggy matter near the sun, either meteoric or arising from the cloudy condensation of a non-permanent gas, the whole is at once easy of comprehension.

3. The Auroral Theory of the Corona.

The objection pressed by Mr. Lockyer that the bright line 1474 is only occasionally visible, is, I think, unfounded. At any rate I have never failed to see it myself when looked for, and very seldom to make it visible to others when I have wished to exhibit to them. It is faint, and, like a difficult microscopic object, requires management to bring it out with five prisms; but by placing the slit tangential to the sun's disc, and giving the instrument a slight jar, it is seen to flash out as the limb passes off the slit. It is worth noting too, that it is often especially plain at portions of the limb where the chromosphere is unusually shallow and faint.

But while I think it probable that this line coincides with the aurora line reported by Prof. Winlock at 1550 of Mr. Huggins' scale, I am by no means sure of it. I understand its assigned position rested upon a single observation with a chemical spectroscope, and the probable error of such a determination cannot well be less than ten divisions of Kirchhoff's scale. I have naturally made many attempts to determine its position for myself, but have never seen it except thrice, and then not long enough at a time to complete a measurement. I am only sure that its position lies between 1460 and 1490 of Kirchhoff.

For this reason, although I do not at all abandon the hypothesis, which appears to have other elements of probability in the general appearance of the corona, the necessity of intense electrical disturbances in the solar atmosphere as the result of the powerful vertical currents known to exist there, as well as the curious responsiveness of our terrestrial magnets to solar storms; yet I do not feel in a position to urge it strongly, but rather await developments.

As to the substance which causes this line, I observe that Father Secchi, in a recent communication to the French Academy, is disposed to think it hydrogen; while Mr. Lockyer still believes it to be iron. I am in hopes that experiments now in progress may throw some light on the subject.

May I suggest, in closing this long communication, that it seems to me that valuable observations might be made at the Eclipse of next December, by fitting up telescopes with a ground glass sliding screen, upon which an image of the corona two or three inches in diameter should be thrown; the ground glass having the roughened side next the observer, so that he could sketch upon it with a lead pencil the outlines of the image, the glass being made long enough to allow of several such sketches.

The comparison of a series of such outlines would decide the question of changes in the coronal streamers, as the sketches, being simple tracings, could not but be accurate in their indications of position.

Dartmouth College, N.H., March 1 C. A. YOUNG

P.S.—I think that the position of the line reported by me as 2602 should have been 2581·5, an error of one revolution of the micrometer screw having been made. At any rate on two occasions since the eclipse I have seen a bright line in the latter position, and I have never been able to find one in the former.

Professor Huxley's Address

MAY I be permitted to advert to one view in connection with that part of Professor Huxley's admirable address to the Geological Society, which treats of distributional provinces inhabited by the terrestrial vertebrata, and the subsequent incorporation of these provinces into one another at different periods.

The view to which I refer is that wherein Mr. Huxley attributes the origin of the eocene types to their evolution *during the Mesozoic epoch* in some province which then was isolated from the European area, and their introduction by geographical changes into the European area in the interval between the Mesozoic and Tertiary epochs.

Having brought forward ten years ago the view that the Australian province was an actual and isolated remnant of the Triassic continent and of its mammalian fauna; and that the geographical distribution of organised beings pointed to the inference that other portions of the land tracts of the Mesozoic period, with their more ancient faunæ, had at different times

* Why not? [ED.]

become incorporated with the post-cretaceous continent,* it is with much satisfaction that I find views for the most part so similar developed by Professor Huxley with the ability which marks all his work. Nevertheless, I venture to submit that the view I then advanced as to the period of the origin of the Eocene types is more in accordance with the facts, as far as we know them, than the hypothesis of their origin in a detached province *during* the Mesozoic period.

The view I advanced was that great changes in the distribution of the continents and seas took place at the close of the Palæozoic, and again at the close of the Mesozoic epoch; and so far I am at one with Professor Huxley: but I inferred that the geographical changes taking place at the close of the Mesozoic epoch were accompanied by the formation of a continent extending over all the *geologically known* parts of the globe, whose endurance was so prolonged as to have afforded the necessary time for the evolution upon it of the Eocene types.

In support of this inference I dwelt upon the entire disappearance of the orders *Pterosauria*, *Enaliosauria*, and *Dinosauria* among vertebrates, and of the *Ammonitidæ* among invertebrates; as well as upon the great extinction that took place in various other forms of life. Such a process as the one suggested by Mr. Huxley would lead us to look in Eocene strata for an intermingling of these distantly evolved types with forms belonging to the several orders just named; however much these forms might in their specific, or even in their generic characters, have been changed during the interval in which these distantly evolved types were introduced. But instead of this we find an absolute disappearance of several important orders of life, of which, from their *habitat*, some—especially the *Pterosauria*—would seem to have been independent of geographical changes simply.

Mr. Huxley intimates that he is led to his view by arguments which he had previously used to demonstrate the necessity of the existence of all the Eocene types in some period antecedent to the Eocene; but may we not suppose that the interval thus marked by the disappearance of so many great orders was vast enough even for this evolution? Indeed so much did this great extinction weigh upon me that even the intervention of a vast lapse of time seemed scarcely sufficient; and I felt driven to suppose that these geographical changes in some degree altered the general conditions under which life had previously existed; and that this alteration, while stimulating evolution on the newly-formed continent, contributed to the great extinction which marks the intra-cretaceous and Tertiary epoch.

Subtle as are the causes which have brought into existence the various types of being, those which have produced their extinction are not less so; though they have not yet received that attention which has been directed to the origin of species. I feel how crude were the suggestions I offered in 1860 to explain this great extinction, and how wide a field of conjecture upon the subject is left open; for these orders of life were not only various in their *habitat*, but equally various in their food. We may imagine the extinction of a species to take place from failure of its food, from destruction by enemies, or—and I think this may be a cause more potent than any other, especially with forms possessing great fecundity—by a failure of the reproductive function; just as among men families diminish and eventually leave no descendants. Be the causes, however, what they may, this great extinction requires us, I submit, to suppose the occurrence of an interval of time as great, and accompanied by changes of conditions as complete, as any that we can urge as necessary for the evolution of the Eocene types. Moreover, the cretaceous period itself, whose terrestrial fauna is as yet unknown, may, so far as we yet know to the contrary, have witnessed in the European area the commencement of, or even some progress in, the evolution of the Eocene types.

Brentwood, March 10 SEARLES V. WOOD, Jun.

Transactions of the Royal Society of Edinburgh

WITH reference to G.'s letter in the last number of NATURE, I have merely to observe that (as you will see by the accompanying list) the Transactions of the Royal Society of Edinburgh are *regularly* sent to no less than twenty-three different societies, institutions, or museums in London alone—besides being sent to many Honorary and Ordinary Fellows residing there. As regards the special case of the British Museum, I have in my possession at this moment their acknowledgments of receipt of the successive parts of our Transactions up to March 1869, and expect immediately to hear that they have received our last published Part. J. H. BALFOUR, *Sec. R. S. Edin.*

Euclid as a Text-Book

"THE first four books of Euclid: or the principal properties of triangles, and of squares and other parallelograms treated geometrically: the principal properties of the circle and its inscribed and circumscribed figures treated geometrically." Such is the wording of the programme put forth by the University of London, of the Mathematical portion of the examination for matriculation candidates. Whether the papers have ever been drawn up in accordance with it I cannot say, but certainly my experience for the last four or five years has led me to believe that the alternative side has, of late, at least, been altogether ignored.

The slightest inspection of recent papers will show that they are constructed on the Euclidean type, and so long as Euclid was generally taught in schools, I think rightly so. But that such a course should now be persisted in (with such latitude as the programme provides) is hard upon those establishments which have taken up the modern views of the subject, such as those so ably advocated by Professor Hirst,* and Mr. J. M. Wilson of Rugby.† It can hardly be thought that so advanced an examining body as the London University will continue to act as an obstructive—for non-encouragement is almost tantamount to tabooing the subject; and the practical result of persistence, I fear, will be this, that the course pursued will press unfairly upon those schools in which (as in University College School, where Wright's Geometry is now the text-book) Euclid has been almost ‡ discarded. Boys are required to study in their school work this modern geometry, founded on French mathematical works; and yet, seeing what value is set upon the same in the examination papers I am discussing, feel themselves constrained to read Euclid that their prospects of good places may be enhanced.

I am disposed to believe that "something will shortly be done," but the reform, though it ought rightly to commence here, ought not to stop here. Every examining body, if a fair field is to be given to the students of modern geometry, should put forth a scheme similar to that which heads my letter, and not merely put it forth "as a sop to Cerberus," but act upon it and let it be a reality.

University College School R. TUCKER

MECHANICAL PROPERTIES OF ICE, AND THEIR RELATION TO GLACIER MOTION

A FEW weeks ago I prepared for the February number of the *Alpine Journal* a review of the contributions made by the Rev. Canon Moseley to the theory of glacier motion, which have appeared at various times during the last fifteen years in the *Proceedings of the Royal Society* and the *Philosophical Magazine*. Some new facts having come to my knowledge since the publication of my paper, I venture to recur to the subject, and to invite discussion upon those memoirs of Canon Moseley in which he endeavours to prove that the descent of glaciers by their weight alone is a mechanical impossibility.§ The arguments he advances in support of this conclusion may be epitomised as follows:—

If a transverse section of a glacier were to be made, the ice would be found to be moving differently at every point of it. The velocity is greater at the surface than deeper down, at the centre of the surface than the edges. There is a constant displacement of the particles of ice over one another, and alongside one another, to which is opposed the resistance known as *shearing force*. By the property of ice called regelation, where a surface so sheared is

* "On the probable events which succeeded the close of the Cretaceous period;" read before the Geological Society on February 1st, 1860. The publication of the paper, beyond a brief abstract, having been denied by the Council, the desideratum was kindly supplied by Dr. Francis; and the paper in extenso is given in the *Philosophical Magazine* of March, April, and May, 1862; the title having been changed to "The form and distribution of the Land Tracts during the Secondary and Tertiary periods, &c."

* In his college lectures, and lectures to ladies at St. George's Hall, &c.

† "Euclid as a text-book of Elementary Geometry" (read before the London Mathematical Society, and printed in the *Educational Times*, Sept. 1868), and in his "Elementary Geometry."

‡ *Almost.* In consequence of pressure from without, arising from the circumstances with which my letter deals, Euclid is again read in one class.

§ Proceedings of the Royal Society, Jan. 7, 1869. Philos. Mag., May 1869. Philos. Mag., Jan. 1870.

brought into contact with a similar surface, it unites with it so as to form one continuous mass. Between the resistance to shearing and the force which tends to bring the glacier down there must be a mechanical relation, so that if the shearing resistance were greater, the force would be insufficient to cause the descent. By a series of experiments upon cylinders of ice inserted in a cylindrical hole bored through two pieces of wood perpendicularly to the surface along which the one was made to slide upon the other, it was found that the force necessary to part the ice along the sliding surface varied from 75 to 119 lbs. per square inch. Canon Moseley has calculated that for the Mer de Glace to descend by its own weight, its shear per square inch cannot exceed 1·3193 lbs., and that to produce the actual motion with a shear of 75 lbs. per square inch, a force in aid of the weight and thirty-four times as great must be called into existence, and applied in the direction of motion. For such a force to be produced by the weight of the glacier alone the density of ice would require to be increased more than 400 times.

In this reasoning Canon Moseley has neglected, as it appears to me, the capability of ice when in a state of deliquescence to slide along a surface of small inclination, as demonstrated by the well-known experiment of William Hopkins. It is, however, not the motion of a block of ice as a whole, but the differential motions of its particles that we have now to consider. It occurred to me that the Canon's arguments upon this branch of the question might be put to an easy practical test by subjecting a block of ice to a strain produced by its own gravitation, and observing its behaviour under this condition, and I was fortunate in obtaining the assistance of my friend Mr. A. F. Osler, F.R.S., in carrying out the experiment.

A plank of ice 6 inches in width and $2\frac{3}{8}$ inches in thickness was sawn from the frozen surface of a pond, and supported at each end by bearers exactly six feet apart. From the moment it was placed in position it began to sink and continued to do so until it touched the surface over which it was supported, drawing the bearers with it, so as to make their upper ends converge. At its lowest point it appeared bent at a sharp angle, and it was rigid in its altered form. The total deflection was 7 inches, which had been effected in about as many hours under the influence of a thaw, during which the plank diminished slightly in width and thickness. On observing the under surface of the plank near the point of flexure, I noticed a number of very minute fissures extending a short distance into the ice, but they certainly were not sufficient to account for the flexure of the plank.

The question at once suggested itself, was the change of form in the ice plank due to fracture and regelation? I did not think it was, but the experiment was not decisive. Some weeks afterwards an opportunity occurred of trying it under other conditions. During the last frost we cut out another ice-plank. Its length was 6 feet $9\frac{1}{2}$ inches, its width varied from $6\frac{1}{4}$ to $6\frac{1}{2}$ inches, and its thickness was $1\frac{7}{8}$ inches. Two large bricks, of a width exceeding that of the plank, were set up on end, on a horizontal surface, exactly 6 feet apart, and the plank was laid upon them at five p.m. on the 12th of February. At 3·15 p.m. on the 13th it was continuously curved from end to end, so that it only rested on the edges of the bearers, and the middle point of its upper surface was deflected $1\frac{3}{4}$ inches below the line joining its two extremities. The temperature was 26° F. The curved plank was perfectly rigid, as was proved by taking it off the bearers and inverting it. I examined it again on the two subsequent days with the following results :—

Feb. 14th, 9.30 A.M. Temp. 29° 5 F.

Deflection of upper surface below chord .	$2\frac{3}{8}$ inches
,, of lower surface below its original horizontal position . .	$2\frac{1}{4}$,,

Feb. 15th, 9.30 A.M. Temp. 30° 0 F.

Deflection of upper surface below chord .	$3\frac{5}{8}$ inches
,, of lower surface below its original horizontal position . .	$3\frac{1}{8}$,,

During the whole of this interval, in which the temperature never rose above the freezing point, there was no indication of fracture in the plank, nor did the optical continuity of the ice suffer the slightest interruption. On the 15th it began to thaw, and the bearers having become frozen to the ground, and the plank to the bearers, the suspended portion was unable to yield to the strain produced by its gravitation ; and when I re-visited the plank on the afternoon of the 15th, it was broken into half-a-dozen pieces.

These experiments were very rough and imperfect ; we intend to renew them on some future occasion, and to conduct them with much greater care and proper mechanical appliances, when we hope to be able to bend an ice-plank double, without destroying its continuity.

The following conclusions may fairly be drawn from them :—

1.—A mass of ice may change its form under strains produced by the gravitation of its particles, without becoming fractured, and without returning to its original form when the strain ceases.

2.—The change of form takes place at temperatures both below and above the freezing point, but is greatly accelerated in the latter case.

I shall not now attempt to discuss the nature of the molecular displacements to which the change of form is due. Their occurrence is indisputable ; whether or not they are to be dignified by the name of shearing is a mere verbal question of little moment. In a very able paper in the *Philosophical Magazine* for March 1869, Mr. James Croll adduces good reasons for believing that when a mass of ice has a deliquescent surface, its molecules may experience repeated momentary losses of their shearing force. While, therefore, he admits the conclusiveness of Canon Moseley's reasonings for temperatures below freezing, he conceives that ice at all higher temperatures may shear by its own gravitation. It is evident that the former concession in Canon Moseley's favour cannot now be maintained, and that the point to which our experimental researches should be directed is not what amount of force will suddenly rend asunder the molecules of ice beyond the sphere of their mutual attractions, but what amount of force will produce molecular displacement within that sphere, with time allowed for its operation.

If we conceive an ice-plank, instead of being placed horizontally between bearers, to be laid with its narrowest face upon a plane of small inclination, with its upper edge horizontal, and its ends confined between vertical walls converging in the direction of motion, with its under surface deliquescent, so that friction would almost be annihilated ; and if we further imagine the diminution of gravity due to resolution along the plane to be compensated by increasing the length or diminishing the thickness of the plank, the plank would alter its form in a way presenting a striking resemblance to the actual movement of a glacier. Its central portions would move more rapidly than its lateral ones ; its surface more rapidly than its base ; and when the strain upon its particles exceeded their cohesive power, it would fracture obliquely to the axis of the channel.

If the conclusions drawn from the experiments above described are legitimate, plasticity must be admitted by the side of sliding, and fracture and regelation as one of the constituent elements of the theory of glacier motion, and a more important place in that theory must be assigned to the views of the late Principal Forbes than has for some years been conceded to them.

WM. MATHEWS, Jun.

THE INDIAN TOTAL ECLIPSE

THE 37th volume of the memoirs of the Royal Astronomical Society, containing Major Tennant's report on the total eclipse of the sun of August 17th and 18th, 1868, has just been issued, and we are enabled, by the courtesy of the Council of the Society, to lay an illustrated notice of it before our readers.

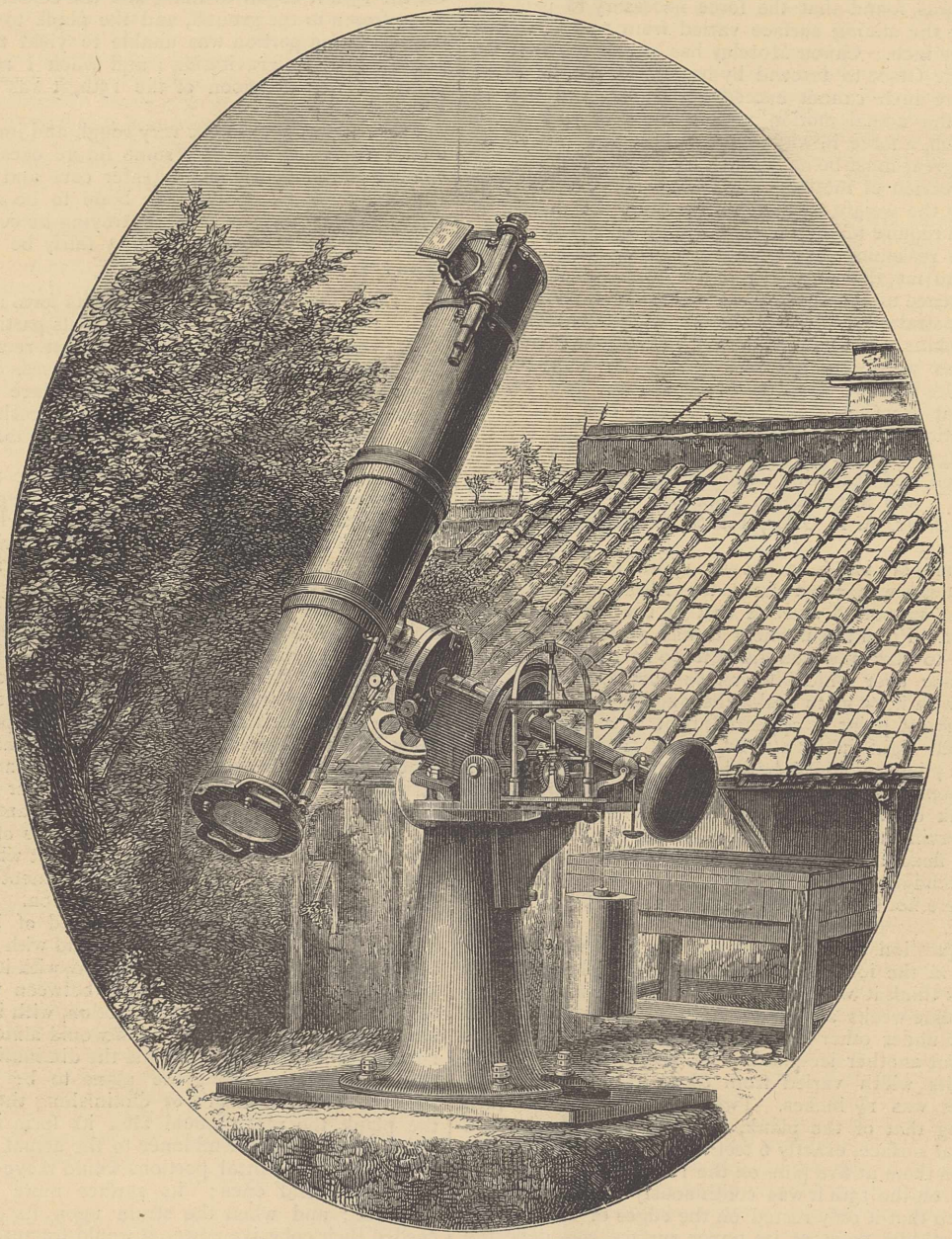


FIG. 1.—THE SILVER GLASS EQUATOREAL

The Indian eclipse was a notable one in the history of astronomy, for as the eclipse of 1860 endorsed the notion that the prominences were solar, so that of 1868 set at rest the gaseous nature of the red flames or red protuberances—so that we have two successive eclipses "settling" two important problems.

Thanks to the care of Mr. De la Rue, Major Tennant was fully provided with the means of photographing the eclipse as well as of determining by means of the spectroscope the nature of the spectrum of the prominences and of the corona. In our notice we may pass over the preface and the narrative of operations which includes the astronomical determination of the position of the observatory, and come to the spectroscopic observations.

We first have the spectrum of the corona. Major Tennant writes:—

Directly I saw the whole moon in the finder I set the cross-wires immediately outside its upper limb. By the time I got to the spectroscope, the cloudy range, seen in the photographs, had vanished from the slit, and I saw a very faint continuous spectrum. Thinking that want of light prevented my seeing the

bright lines, which I had fully expected to see in the lower strata of the corona, I opened the jaws of the slit, and repeatedly adjusted by the finder, but without effect. *What I saw was undoubtedly a continuous spectrum, and I saw no lines.** There may have been dark lines, of course, but with so faint a spectrum and the jaws of the slit wide apart, they might escape notice.

We next have the spectrum of the Great Horn :—

One line in the red was so beautiful that it needed an effort to turn my attention to anything else; there was a line in the orange not so well defined, and one in the green which seemed

FIG. 2.—OBSERVATORY TENTS.

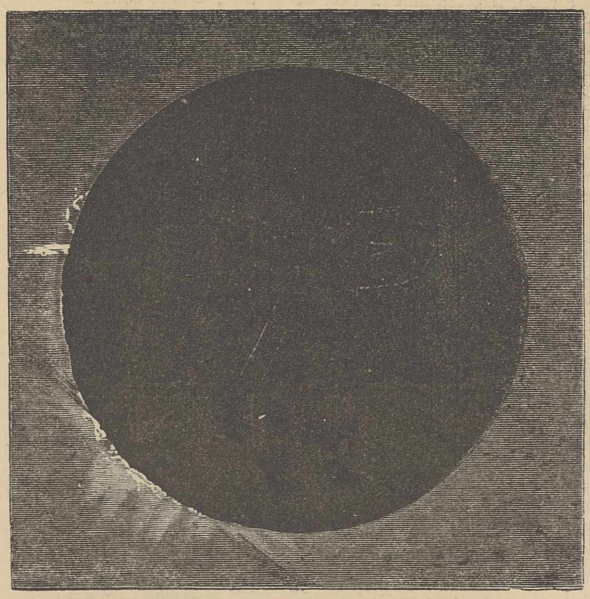


FIG. 3.—ONE OF THE FIRST PHOTOGRAPHS, SHOWING THE GREAT HORN

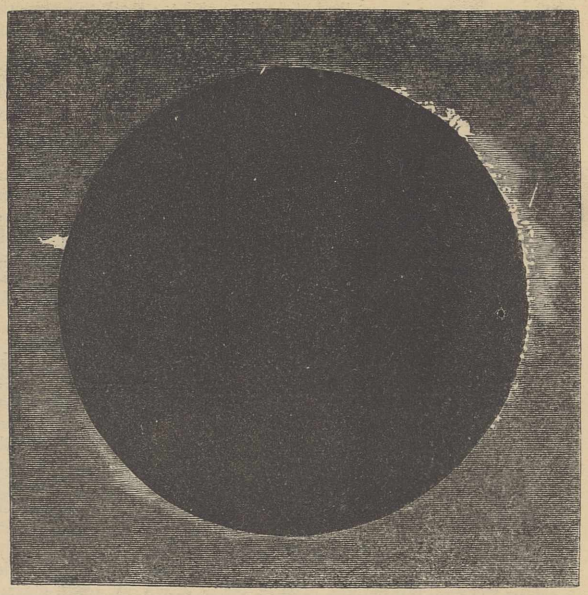


FIG. 4.—THE LAST PHOTOGRAPH TAKEN, SHOWING THE ANIMAL-LIKE PROMINENCE AND THE ECLIPSE OF THE GREAT HORN

multiple (it must be remembered that I had not time to adjust the jaws of the slit accurately, and that the brilliancy of these lines made them broader by irradiation); beyond, I saw a line just defined, which, as will be seen from the measures before given, must have been near to F, and still further off in the blue I saw a hazy light probably beyond G.

. . . . . .

The red and yellow lines were evidently C and D, the reading of the green line coincides with that of the brightest line in *b*, instead of the mean of the three, which I read as a verification; the line near to F was in all probability F itself; E was certainly not seen by me. The line in the blue it is useless from my data to speculate upon, I must hope that some one else has identified it.

It is pleasing to point out how very nearly Major Tennant's observations, as now given, approximate to the true state of the case, which we can now determine any day that the sun shines. He must be entirely congratulated on the degree of success of his spectroscopic observations—observations made, according to his report, under difficulties which he ought not to have encountered. Major Tennant's evidence in favour of the continuous spectrum of the corona has been entirely confirmed by the observations since made in America.

The photographic results we may introduce by a woodcut of the observatory and of the instrument, with speculum of nine inches aperture, mounted by Mr. Browning, with which the photographs were taken.

* In the instructions for Lieut. Herschel his attention was first drawn to the protuberances. I therefore had resolved to attend first to the corona, lest each of us should have only the same partial tale.

The most notable phenomenon visible during the Indian eclipse was the Great Horn visible in Fig. 3, the structure of which was extremely curious. Below it in the drawing we have a part of the chromosphere, including a "flaring," which has been since called a "radiance" by the American astronomers. Fig. 4 was the last photograph taken, and here it is seen that the Great Horn is nearly eclipsed, and that the chromosphere on the opposite side of the sun is now exposed, including a strange animal-like form, which much struck the observers. An examination into the structure of the Great Horn is not the least interesting part of the report.

Major Tennant thus sums up his results :—

First.—The corona is the atmosphere of the sun not self-luminous, but shining by reflected light. It is evidenced both by the spectroscope and polariscope that this is the case, but there is one reservation to be made. The polariscope has shown clearly that the light of the brightest part of the corona is mainly reflected ; but, looking to the flare which is seen in photographs No. 2 and 3, it seems impossible to doubt that in those places there must have been some inherent luminosity in the corona ; unless indeed we consider the flare as a modified form of protuberance. It is, I think, now certain that luminous gas issues from what is more strictly the sun, and I apprehend this flare to be some of this.

Secondly.—The Great Horn certainly was composed of incandescent vapours, and probably all the brilliant protuberances are the same. In the Great Horn these vapours were hydrogen, sodium, and magnesium. It seems to me perfectly certain that the ignited hydrogen issued from the sun itself, and that it carried up with it the light vapours of sodium and magnesium far above the level at which they would naturally lie ; hydrogen naturally would be the very highest of the gaseous vapours, and consequently the coolest ; if, however, it were set free at the surface of the sun it would be intensely hot, and seek, with great violence, to ascend, in which process, if there be a stratum of heated vapours, such as is usually believed to exist round the sun, the hydrogen would partly displace and carry up these vapours, and the lighter would be taken in preference. In fact, in this case it has carried the two lightest, and that of iron, which is so much heavier (I think we may presume this from the absence of the line corresponding to E), was either displaced or dropped sooner than the height at which I observed. Photograph No. 1 shows that there were two jets of vapour concerned in forming this Horn. One, the largest and most northerly, is seen nearly perpendicularly to the limb, and seems also to have been the most luminous ; the other issues about 20,000* miles towards the south, and at an angle. They met at a height of some 16,000* miles, and the result was the rapid vorticose motion, which is evidenced in all the photographs as having existed in the upper portion. I believe I have the good fortune to be the first person to recognise such a phenomenon.

. . . . . .

I think that gases or vapours issuing quietly from the solar surface would tend locally to raise the superincumbent ignited vapours. In places where they were most abundantly given out the elevation would be greatest, everywhere the gas would leak through in streams, producing occasionally such phenomena as the flare I have spoken of in Nos. 2 and 3. For a time the ignited vapour might, I think, form, as it were, a case for the light included gas, which would be to all appearance inflated like the animal figure in No. 6. Soon, however, the slightly coherent casing would be burst and the gaseous contents would issue freely ; the heavier vapours would, of course, to some extent be carried off by the gas, but would mainly settle down in small masses. Such, I think, is the state depicted in the southern protuberance of No. 1.

I would now draw attention to Plate No. 1, and the glare and luminous stratum. If that glare be from sunlight, it must, I think, be acknowledged that the remaining ray was but small. The luminous nearly even stratum then is not the sun ; but it is intensely bright, so much so, that nowhere is it lost in the solar glare. Its height is but small (I estimate it at 7,200 miles), and I believe it to be the mass of heavy luminous vapours, to whose elective absorption we owe the Fraunhofer lines in the solar spectrum. At the north end of this stratum near the Great Horn, it is broken into beads of light ; † and I am disposed to think these are the veritable Baily's beads, of which I have always felt that the description would be difficult to apply to sunlight ; I mean the statement which has been made of the light being silvery, &c. If these beads are really phenomena of the absorbing stratum, one can well understand the use of such terms.

In an addendum, in which the author's theory is attempted to be maintained, Major Tennant refers to the work which has been done in this country between the eclipse and the issue of the report. He considers that the sun is surrounded by an atmosphere sufficiently dense to reflect the solar light, but it is not explained why a continuous spectrum and not the solar spectrum is actually reflected; and that the hydrogen is enveloped in a denser atmosphere, resisting its diffusion and expansion, but why the spectrum of this atmosphere is so simple is not explained.

The author concludes by acknowledging the services rendered by Mr. De la Rue in the preparation of the report.

* These dimensions refer to the projections on a plane perpendicular to the visual ray.

† This is the same place where Captain Branfill saw beads.

FALL OF A METEORITE

THE Director of the Meteorological Office has forwarded the following extract from a letter from M. Coumbary, Director of the Imperial Meteorological Observatory at Constantinople for publication :—

Constantinople, 9 mars, 1870

Mon cher Monsieur,—Je saisis l'occasion qui m'est offerte pour vous transmettre la communication que vient de nous faire M. Carabella, Directeur des Affaires Etrangères du Vilayet de Tripoli de Barbaru.

" Tripoli, 2 février, 1870

" Le Mutasserif de Mourzouk (Fezzan), latitude 26° N., longitude 12° E. de Paris, nous fait savoir que vers le 25 décembre, 1869, il est tombé à l'est de la ville, vers le soir, un immense globe de feu, mésurant un mètre à peu près de diamètre, et qu'au moment où il a touché terre il s'en est détaché de fortes étincelles qui, en se produisant, claquaient comme des coups de pistolet, et exhalaient une odeur que l'on n'a pas specifiée. Cet aërolithe est tombé à peu de distance d'un groupe de plusieurs arabes, parmi lesquels se trouvait le Chiok-el-Veled de Mourzouk. Ceux-ci en ont été tellement effrayés qu'ils ont immédiatement déchargé leurs fusils sur ce monstre incompréhensible. Son Excellence Ali Riza Pacha a écrit à Mourzouk pour faire transporter ici l'aërolithe ; au cas probable où il soit trop pesant on le mettra en pièces ; nous vous enverrons tout cela. Il y a un mois de voyage d'ici à Mourzouk. Ce n'est donc que dans deux mois à peu près que nous pourrons vous faire cette expédition. S'il peut vous être de quelque intérêt de le savoir, je vous dirai que quelques voyageurs du Waddad que j'interrogeais m'ont dit que le Sultan du Waddad et tous les grands personnages de sa cour ont des poignards, des sabres et des lances faits avec du fer tombé du ciel, et qu'il en tombe de grandes quantités dans ce pays-là. (Sd.) " L. CARABELLA "

Je crois devoir vous informer qu'au reçu de cette lettre et à la suite des démarches nécessaires, S.A. le Grand Vizir a bien voulu faire donner ordre immédiatement par télégraphe à Tripoli, pour que l'on prenne les mesures nécessaires afin que ce météorite nous parvienne intact.—Recevez, cher monsieur, &c.,

(Sd.) ARISTIDE COUMBARY

NOTES

ON the 5th of March died at Vienna, Joseph Redtenbacher, Professor of Chemistry at the University. He was born in 1810, and studied under Eichig, conjointly with whom he published a determination of the atomic weight of carbon, and several other memoirs. His principal merit consists in the discovery of acrolein and acrylic acid. Most of his papers were published between 1839 and 1848. With his death chemistry in Austria passes entirely into younger hands ; his colleague, Professor Schröber, the discoverer of amorphous phosphorus, having lately been nominated Master of the Mint, and replaced in his chair by Illasiwetz. The succession of Redtenbacher will be divided into two parts, and the building of a new laboratory

on the largest scale will open a vast field to the activity of his successors.

THE African traveller, Dr. Nachtigal, writing from Mursuk to the *Cologne Gazette*, on the 29th January last, gives some further particulars relative to the murder of Miss Tinné. He says that Sultan Omur, of Bornu, has sent a *meheri* (courier) to Mursuk, in order to make inquiries respecting the murder. The deceased lady's effects and servants have at length been despatched to Tripoli, though the former have not yet arrived. Yehenouchen, the Tuareg chief, has written to Europe, disclaiming all responsibility for the murder, as he had not promised his protection to Miss Tinné, or urged her to undertake the journey to Rhat. He is ninety years old, says Dr. Nachtigal, and has a great reputation for uprightness in his own country, but on the other hand, it is a fact, that his own nephew, Hadji el Schich, who attacked the unfortunate lady and her caravan, and murdered her after receiving her presents and other marks of friendship, is still living with Yehenouchen, and that the latter had directed the Marabout Hadji Ahmed Bu-Slah, who witnessed the murder without attempting to prevent it, either by word or deed, to provide Miss Tinné with an escort to Rhat.

THE chair of chemistry at Königsberg has been accepted by Dr. Gräbe of Leipzig, known through his papers on chinone and alizarin.

WE regret to announce that Professor Magnus of Berlin has been suffering for several months from a painful disease, which will, most likely, oblige him to interrupt his lectures for some time to come.

WE learn from the *Journal of the Society of Arts* that by the death of Mr. William Gibbs, which took place on the 28th ult., the South Kensington Museum acquires the collection of Roman and Anglo-Saxon antiquities collected by him.

MISS GARRETT has been admitted as a member of the medical staff of the East London Hospital for children, and was appointed one of the physicians on Wednesday last. This is the first hospital in Great Britain which has recognised in this manner the female medical movement.

IN the *Monthly Microscopical Journal* for March is an obituary notice by Mr. Joseph Lister, F.R.S., Professor of Clinical Surgery in the University of Edinburgh, of his father, the late Mr. J. J. Lister, F.R.S., to whom science is so much indebted for improvements in the microscope.

THERE is a project for the establishment of a Scientific College at Perth, intended to combine, on a small scale, the advantages of the *Ecole des hautes Etudes* and *Ecole Normale Supérieure* of Paris.

THE thirty-seventh session of the *Congrès Scientifique de France* will be held at Moulins in August next.

THE *Society of Arts Journal* reports that an International Exhibition of Agricultural Machines will be held at Arnheim, in June, July, and August.

THE *Scientific American* calls attention to an excellent improvement applied to the passenger car of the Beach Pneumatic Transit Company. The car, in size, is about the same as an ordinary street car, and a single zircon light illuminates its interior with brilliancy. Two small cylinders of compressed oxygen and hydrogen are carried on the car, from which pipes extend to a small burner that supports a piece of zircon, not more than a quarter of an inch long and one-eighth of an inch in diameter. Against this little piece of zircon the two gases impinge, and heat it so intensely as to make it glow with a clear and steady light. Those who fancy that underground railway riding in New York is likely to be a dark and dismal affair will receive new impressions on the subject when they enter the premises connected with the Broadway tunnel. One of the great advantages of the zircon light is that it burns like any other light without requiring adjustment. The light carried on the car before mentioned, burns steadily for seven hours without being touched. The zircon pencil lasts for three months, and is, in effect, the wick of the light.

THE annual meeting of the Cotteswold Naturalists' Field Club was held at Gloucester on Feb. 23rd. It was stated that the forthcoming volume of the "Transactions" would contain a most valuable and elaborate paper by Mr. Lucy, F.G.S., which attempts to unravel the intricate history of the distribution of the erratic boulders, boulder drift, quaternary gravels, &c., of the Severn valley, over a given area; a contribution of the highest value to science, and one full of facts of the greatest importance, in elucidating the old physical geography and geology of the Severn valley since the close of the Pliocene epoch. The Transactions would also contain an admirable paper from the pen of Dr. Wright, F.R.S.E., on the "Correlation of the Jurassic beds of France and Switzerland with those of Gloucestershire and Wilts." These papers, both splendidly illustrated by maps and woodcuts, many of which were exhibited to the meeting, were highly eulogised by the president, and referred to as fully maintaining the already high scientific reputation of the "Transactions of the Cotteswold Club." The field meetings for the year have been fixed to take place at Cirencester, Ross, Painswick Hill, Moreton-in-Marsh, and a "foreign meet" at Watchett in Somerset, later in the season, if the weather permit.

A SOMEWHAT violent shock of earthquake was felt at Trieste on the 28th of February, at 12·20 p.m. The oscillatory movement from east to west lasted from two to three seconds. The following day at 8·56 p.m. the motion was repeated with greater violence. There was a loud rolling noise, and articles of furniture were thrown down in the houses.

DR. WILHELM HAMM, an official of the Austrian Ministry of Agriculture, and well known as author of the "Weinbuch" (Leipzig, J. Weber), has constructed a Weinkarte, published by Costenoble, Jena, exhibiting at a glance the topography, climatology, and statistics of wine-growing in Europe and the islands of the Atlantic. The habitats, climates, and quantities produced; the various growths and qualities of each; and the wine measures, &c., are all indicated in this map.

SIR WILLIAM THOMSON, having had his attention directed to the very great differences that exist in the conducting quality of copper wire professing to be of the "highest conductivity," had a large number of specimens carefully tested and the following are some of the results obtained—the quality is indicated by the resistance of a metre length weighing one gramme. The best specimen was one supplied by M. Bréguet, Paris, of which the resistance was ·153 of an ohm per metre weighing one gramme. Specimens from English manufacturers varied as follows:—·165, ·169, ·171, ·178, ·206, ·213, ·221. Seven specimens labelled "highest conductivity," stood as follows:—·156, ·182, ·201, ·205, ·223, and ·258. As it is to the interest of all scientific men that the copper wire used in electrical instruments shall be of the best quality, there should be general co-operation to discourage as much as possible the use of inferior copper. Variations in conductivity like those in the samples of copper mentioned above would produce instruments varying to the extent of 40 per cent.

THE publishers of the *Canadian Naturalist* announce that the periodical is in future to appear quarterly instead of six times a year as heretofore, but the quantity of matter, 480 pages, is not to be diminished. Another and an important particular is, that "the basis of the magazine has been so extended as to include a larger field of popular scientific scope than formerly. Especially with regard to the technology of geological, mining,

chemical and agricultural science, new materials will be made available while a general summary of scientific facts and discoveries will form an important feature." An editorial committee, comprising some of the foremost scientific men in Canada, has been appointed and the publication remains, as before, under the "auspices" of the Natural History Society of Montreal. The number just received in this country contains, in addition to a batch of articles on different subjects in natural history, one on the partial eclipse of the sun in August last, as observed at Montreal, by Dr. Smallwood.

AN interesting report on the ravages of the Borer in Coffee estates has just been published by George Bidie, M.B., F.R.G.S. The coffee plant, as is well known, is not indigenous to Southern India, but was first introduced into India upwards of two centuries ago, by a Mussulman pilgrim, Babababooden, who on his return from Mecca brought a few berries in his wallet, and taking up his abode in the hills of Mysore, planted them near his tent, and from these the greater portion of the coffee now growing in Southern India has been derived. It is a native of Caffa in Southern Abyssinia. It is now largely cultivated in Mysore, Cuddoor, Coorg, and other parts along the crests and slopes of the Ghauts. It is a remarkably hardy plant, thriving at various elevations, and under the most different conditions of moisture, soil, and temperature. It is, however, liable to the attacks of certain insects, amongst which the Borer is the most formidable. This is shown by Dr. Bidie to be the larva of a beetle belonging to the Cerambycidæ, and termed the *Xylotrechus quadrupes*. The female lays its eggs in the bark of the plants, hot sunshine favouring their hatching. The larva immediately pierces the bark, and derives its nourishment from the more juicy layers, producing, by the damage it causes, exhaustion of the tree and loss of the crop. The whole duration of the life of the animal from the deposition of the ovum to the death of the beetle does not exceed twelve months. The animal appears to be indigenous, and the causes that have led to the great increase in its ravages during the last few years are drought, want of shade, bad culture, destruction of forest trees in which the insect used to live, and departure of some of its enemies.

THE following changes are announced in the arrangements of the staff of St. Bartholomew's Hospital. One assistant-physician is to have charge of the casualty department; three casualty physicians are to act with him; and one casualty surgeon is to superintend the surgical side. The house-surgeons will have charge of casualty patients.

THE Geographical Society of St. Petersburg has decided to send a scientific expedition into Manchouria and Eastern Mongolia, for the purpose of making archæological and ethnographical researches in those countries. The expedition is to start next April, under the direction of the Archimandrite Palladiy, chief of the Russian mission at Pekin. It is said that the Emperor has contributed 5,000 roubles towards the expenses of the expedition.

THE rainfall of the year ending December 31, 1869, as taken in the neighbourhood of Charing Cross, is registered as follows:—

Height of gauge above ground.	Number of inches fallen.
6 feet	23·760
64 ,, ...	22·110

MR. E. W. HILGARD, in his Geological Reconnaissance of Louisiana, finds reason for the assumption that "the Gulf coast has in late quaternary times suffered a depression to the extent of at least 900 feet—perhaps more—and during the terrace epoch a contrary motion to the extent of about half that amount." Elsewhere he remarks, "the age of the great gypsum formation has been the subject of much discussion. It has always seemed to me that the great extent of the area over which the cretaceous beds and underlying gypsum are known to be co-extensive, went far to prove that they belonged substantially to the same epoch. Whatever weight may attach to this argument it is greatly enhanced when we find the crystalline limestone and underlying gypsum not only reappearing in northern Louisiana, but actually accompanying each other beneath the waters of the Gulf of Mexico. Whether the volcanic agencies which even now so frequently disturb that great basin, have been instrumental in reducing the sulphur, distilling the petroleum and crystallising the rock salt of southern Louisiana, may be more profitably discussed when more extensive excavations shall have given us an opportunity of closer inspection of the facts."

IN reference to the electro-deposition of nickel, M. Bouilhet states that Jacobi published a method of obtaining thick coatings of nickel by using perfectly neutral solutions. *Cosmos* points out that nickel is especially abundant in Spain.

*ON THE TEMPERATURE AND ANIMAL LIFE OF THE DEEP SEA**

II.

DURING the first and second cruises of the *Porcupine*, the temperature of the eastern border of the great North Atlantic basin was examined at various depths between from 54 to 2,435 fathoms, and in widely different localities, ranging from lat. 47° to lat. 55°. The bottom-temperature was ascertained at thirty stations, and serial soundings were taken at seven stations; making the total number of observations eighty-four. (Table II., p. 20.) Amongst all these the coincidence of temperatures at corresponding depths is extraordinarily close; the chief differences showing themselves in the temperature of the surface and of the stratum immediately beneath it. A decided superheating is observable in this superficial stratum, not extending to a depth of much more than 70 or 80 fathoms, and more considerable at the southern than at the northern stations. Whether this "superheating" is entirely due to the direct influence of solar heat, or depends in any degree on an extension of the Gulf Stream as far as the southern part of the area examined, is a question which can only be resolved by the determination of its relative amount at different seasons. Between 100 and 500 fathoms, the rate of decrement is very slow, averaging only about 3° in the whole, or three-fourths of a degree for every 100 fathoms; and this body of water has a temperature so much above the isotherm of the northern stations at which the observations were made, as decidedly to indicate that it must have found its way thither from a southern source. Between 500 and 750 fathoms, however, the rate of decrease becomes much more rapid, the reduction being 5·4°, or above 2° per 100 fathoms; while between 750 and 1000 fathoms it amounts to 3·1°, bringing down the temperature at the latter depth to an average of 38·6°. Beneath this there is still a slow progressive reduction with increase of depth, the temperature falling a little more than 2° between 1000 and 2,435 fathoms; so that at the last-named depth, the greatest at which it was ascertained, it was 36·5°.—Thus it is obvious either that the vast body of water occupying the deeper half of the Atlantic basin has been itself derived from a colder region, or that its temperature has been reduced by the diffusion through it of frigid water from a Polar source. The latter supposition best accords with the gradual depression of temperature exhibited between 500 and 1000 fathoms, which corresponds with the "stratum of intermixture" of the cold area.

The temperature soundings recently taken by Commander Chimmo, R.N., and Lieutenant Johnson, R.N., at various points in the North Atlantic basin, when the requisite corrections are applied for the influence of pressure on the bulbs of the unprotected thermometers employed by them, give results which are remarkably accordant with our own; so that it may be stated with confidence that the temperature of the deeper parts of the North Atlantic sea-board is but a very few degrees above the freezing-point.

Now a glance at the North Polar region, as laid down either on a globe, or any projection of which the Pole is the centre, shows that the Polar basin is so much shut in by the northern shores of the European, Asiatic, and American continents,

* A Lecture delivered at the Royal Institution (continued from p. 490).

that its only communication with the North Atlantic basin—besides the circuitous passages leading into Hudson's and Baffin's Bays—is the space which intervenes between the eastern coast of Greenland and the north-western portion of the Scandinavian peninsula. If, therefore, there be any such general interchange of Polar and Equatorial water as that for which we have argued, the Arctic current must flow through the deeper portions of this interspace, at the north of which lies Spitzbergen, whilst Iceland and the Faroes lie in the middle of its southerly expanse. Now in the channel that lies between Greenland and Iceland, the depth is such as to give a free passage to such a frigid stream; but between Iceland and the Faroe Islands there is no depth so great as 300 fathoms at any part, except in a narrow channel at the south-east corner of Iceland; so that an effectual barrier is thus interposed to any movement of frigid water at a depth exceeding this. A similar barrier is presented, not merely by the plateau on which the British Islands rest, but also by the bed of the North Sea; the shallowness of which must give to such a movement a not less effectual check than would be afforded by an actual coast-line uniting the Shetland Islands and Norway. Consequently, it is obvious that a flow of ice-cold water, at a depth exceeding 300 fathoms from the surface, down the north-eastern portion of this interspace, can only find its way southwards through the deeper portion of the channel between the Faroe and Shetland Islands; which will turn it into a W.S.W. direction between the Faroe Islands and the north of Scotland, and finally discharge such part of it as has not been neutralised by the opposing stream coming up from the south-west, into the great North Atlantic basin, where it will meet the Icelandic and Greenland currents, and unite with them in diffusing frigid waters through its deeper portion. In thus spreading itself, however, the frigid water will necessarily mingle with the mass of warmer water with which it meets, and will thus have its own temperature raised, whilst lowering the general temperature of that mass; and hence it is that we do not find the temperature of even the greatest depths of the Atlantic basin nearly so low as that of the comparatively shallow channel which feeds it with Arctic water.

It may be questioned, however, whether the whole body of Arctic water that finds its way through the channels just indicated, could alone maintain so considerable a reduction in the temperature of the enormous mass which lies below 1,000 fathoms in the Atlantic basin; subject as this must be to continual elevation by the surface-action of the sun on its southern portion. And as the few reliable observations on deep-sea temperatures under the equator indicate that even there a temperature not much above 32° prevails, it seems probable that part of the cooling effect is due to the extension of a flow of frigid water from the Antarctic Pole, even north of the Tropic of Cancer. Of such an extension there is evidence in the temperature-soundings recently taken in H.M.S. *Hydra* between Aden and Bombay, where the cooling influence could scarcely have been derived from any other source than the Antarctic area.*

The unrestricted communication which exists between the Antarctic area and the great Southern ocean-basins would involve, if the doctrine of a general Oceanic circulation be admitted, a much more considerable interchange of waters between the Antarctic and Equatorial areas, than is possible in the Northern hemisphere. And of such a free interchange there seems adequate evidence; for it is well known to navigators that there is a perceptible "set" of warm surface-water in all the Southern oceans towards the Antarctic Pole; this "set" being so decided in one part of the Southern Indian Ocean, as to be compared by Captain Maury to the Gulf Stream of the North Atlantic.† Conversely, it would appear from the application of the necessary pressure-correction to the temperatures taken in Sir James Ross's Antarctic expedition, the voyage of the *Venus*, &c., at depths greater than 1,000 fathoms, that the bottom-temperature of the deepest parts of the Southern Oceanic basin really approaches the freezing-point, or is even below it. And if the temperature of the deeper portion of the North Pacific Ocean should be found to exhibit a depression at all corresponding to that of the North Atlantic, it must be attributed entirely to the extension of this Antarctic flow; since the depth of Behring's Strait, as well as its breadth, is so small as to permit no body of Arctic water to issue through that channel.

If further observations should substantiate the general diffusion of a temperature not much above the freezing-point over the deepest portions of the ocean-bed, even in Intertropical regions, as a result of a general deep movement of Polar waters towards the Equator, forming the complement of the surface-movement of Equatorial water towards the Poles, it is obvious that such diffusion must exert a very important influence on the distribution of animal life; and, in particular, that we may expect to meet with forms which have hitherto been reputed essentially Arctic, in the deep seas of even the Intertropical region, and again in the shallower water of the Antarctic area. Such, there is strong reason to believe, will prove the case. In his recent annual address as President of the Royal Society, Sir Edward Sabine cites observations on this point made by Sir James Ross in his Antarctic expedition, as confirmatory of the view entertained by that distinguished navigator, "that water of similar temperature to that of the Arctic and Antarctic seas exists in the depths of the intermediate ocean, and may have formed a channel for the dissemination of species." The "similar temperature" believed by Sir James Ross to have had this general prevalence, seems to have been 39°; whereas the observations made in the *Porcupine* expedition distinctly prove that a temperature even below 30° may be conveyed by Polar streams far into the temperate zone, and that the general temperature of the deepest part of the North Atlantic sea-bed has more of a Polar character than he supposed.

Again, the deep-sea dredgings of the *Porcupine* expedition have shown that many species of mollusks and crustacea previously supposed to be purely Arctic, range southwards in deep water as far as those dredgings extended—namely, to the northern extremity of the Bay of Biscay; and it becomes a question of high interest whether an extension of the same mode of exploration would not bring them up from the abysses of even intertropical seas.

Now, as there must have been deep seas at all geological epochs, and as the physical forces which maintain the oceanic circulation must have been in operation throughout, though modified in their local action by the particular distribution of land and water at each period, it is obvious that the presence of Arctic types of animal life in any marine formation cannot be accepted as furnishing evidence *per se* of the general extension of glacial action into temperate or tropical regions. How far the doctrines now current on this point may need to be modified by the new facts now brought to bear on them, it will be for geologists to determine; the question may be left in their hands with full assurance of a candid reception of the fresh evidence now adduced.

The general results of the dredging operations carried on during the *Porcupine* expedition will now be concisely stated.

In the first place they show conclusively that there is no limit to the depth at which animal life may exist on the ocean-bed; and that the types found at even the greatest depths may be not less elevated in character than those inhabiting shallower waters. It would even be premature yet to affirm that the higher types occur in less abundance and variety than at more moderate depths; for it is by no means impossible that the use of the improved method of collection devised by Captain Calver,* which was employed with extraordinary success in the third cruise, may make as large an addition to our knowledge of the life of the sea-bottom explored by the dredge in the first and second cruises of the *Porcupine*, as it has done in the case of the cold area, where it revealed the astonishing richness of the bottom, which the *Lightning* dredgings of the previous year had led us to regard as comparatively barren.

Secondly, they confirm our previous conclusion that temperature exerts a much greater influence than pressure on the distribution of animal life. Not only have we found the same forms presenting themselves through an enormous vertical range—no amount of fluid pressure being incompatible with their existence—but we have also, by a more complete survey of the relations of the warm and cold areas, established the very marked difference between the faunæ of two contiguous portions of the sea-bed lying at the same depth, which was indicated by the *Lightning* dredgings. It is remarkable, however, that this difference showed itself more in the crustaceans, echinoderms, sponges,

* The lowest temperature actually observed in these soundings was 36½°. The temperature of 33½° given in the previous discourse, as existing below 1,800 fathoms, proves to have been only an estimate formed by Capt. Shortland under the idea that the rate of reduction observed at smaller depths would continue uniform to the bottom, which the serial soundings of the *Porcupine* prove to be by no means the case.

† "Physical Geography of the Sea," §§ 748-750.

* This consists in the attachment of "hempen tangles" to the dredging apparatus, by which the floor of the ocean is *swept* as well as *scraped*. These tangles often came up loaded, when the dredge was empty.

and foraminifera, than it did in the mollusca, of which a considerable proportion were common to both areas. The abundance and variety of animal life on a bottom of which the temperature is at least 2° (Fahr.) below the freezing-point of fresh water, is a fact which has all the interest of surprise; and it is scarcely less remarkable that the forms of mollusks, echinoderms, and sponges, which seem to be the characteristic inhabitants of this cold area, should attain a very considerable size. The precise limitation of the Globigerina-mud and of the vitreous sponges to the warm area, was a very striking manifestation of the influence of temperature, and has very important geological bearings.

Thirdly, they have largely added to the number of cases in which types that had been regarded as characteristic of earlier geological periods, and to have long since become extinct, prove to be still existing in the depths of the ocean; and greatly increase the probability that an extension of the like method of research to more distant localities would produce even more remarkable revelations of this character.

The doctrine propounded by Professor Wyville Thompson, in the report of the *Lightning* expedition, as to the absolute continuity of the cretaceous formation with the deposit of globigerina-mud at present in progress on the North Atlantic sea-bed, has received such striking confirmation from the discovery of the persistence of numerous cretaceous types, not merely in our own explorations, but also in those carried on by the United States Coast Survey in the Gulf of Mexico, that it may be fairly affirmed that the *onus probandi* rests upon those who assert that the formation of true chalk has ever been interrupted since the cretaceous period. That period is usually considered to have terminated with the elevation of the cretaceous deposits of the European area into dry land. But according to the accepted doctrines of geology, it is highly probable that, coincidently with the elevation of the European area, there was a gradual subsidence of what is now the Atlantic sea-bed; so that the *Globigerinæ* of the former area, with many accompanying types of animal life, would progressively spread themselves over the latter, as its conditions became favourable to their existence. And there seems no reason why they should not have maintained themselves in its deepest parts, through the comparatively small changes of level which took place in this portion of the earth's crust during the Tertiary epoch.

Fourthly, the *Porcupine* explorations have enormously extended our knowledge of the British marine fauna; alike by the discovery of new types, and by the addition of types previously known only as inhabitants of other localities.—The mollusca alone have as yet been fully examined; and Mr. J. Gwyn Jeffreys, whose authority upon this part of the subject is not second to that of any other naturalist, reports as follows:—The total number of species of marine mollusca enumerated in his recently completed "British Conchology" (excluding the Nudibranchs) is 451; and to these the *Porcupine* expedition has added no fewer than 117, or more than one-fourth. Of these as many as fifty-six are undescribed, whilst seven were supposed to be extinct as Tertiary fossils. Sixteen genera, including five which are undescribed, are new to the British seas. "All that I can do," he says, "by continual dredgings in comparatively shallow water during the last sixteen years was to add about eighty species to the number described by Forbes and Hanley. I regard the present (although a large) addition as merely an earnest of future discoveries. In fact the treasury of the deep is inexhaustible." The complete examination of the crustacea, which are in the hands of the Rev. A. M. Norman, and of the annelids, which have been undertaken by M. Claparède and Dr. Macintosh, will probably yield results scarcely less striking. It is, however, in the echinoderms and sponges, which are being examined by Professor Wyville Thomson; in the stony corals, which have been referred to Dr. P. M. Duncan; and in the foraminifera, which constitute the speaker's own speciality, that the most interesting novelties present themselves.

W. B. CARPENTER

SCIENTIFIC SERIALS

THE February number of the *American Naturalist* (Vol. iii. No. 12) contains only three original articles, and of these the first and most important is really a reprint of Professor Wyman's observations on the development of the thornback, with a few introductory remarks on the natural history of the skates, by Mr. F. W. Putnam. The other two are the continuation of Mr. J. A. Allen's notes on the rarer birds of Massachusetts, and a paper on common fresh-water shells, by Mr. E. S. Morse. Professor Williamson's article on *Bathybius* is reprinted from the *Popular Science Review*.

A SHORT paper appears in the last number of Tröschel's *Archiv für Naturgeschichte* from the pen of Dr. A. A. Krohn, on the earliest development of the Botryllus stock, which, as most of our readers are probably aware, constitutes one of the Tunicate Molluscs analogous to the misshapen bodies found so commonly on our sea coasts, and known as "dead men's fingers." Hitherto certain processes found at the anterior end of the larva have been regarded as the germs or buds of new individuals which subsequently become completely differentiated, but M. Krohn shows that these are clavate processes, constituting the first rudiments of the blood-vessels which make their appearance soon after the metamorphosis of the larva. After a short time the vessels begin to branch, each branch terminating in a dilated cœcal enlargement resembling the calyx of the common *Erica tetralix* in form, and at this time a round projection appears on the right side of the body, near the heart, into which a stream of blood from the mother sets, and having circulated around it returns to its starting point. It now, curiously enough, begins, together with the mother animal, to shrink, and finally disappears, and in its place a daughter Botryllus is developed. The daughter Botryllus forms two buds, a right and a left, while itself passes through the same stages as the original mother, becoming also fluid and disappearing. The two buds of this third generation, when fully developed, have their cloacal apertures opposed, and each gives off two buds which are arranged with the parents in a circular manner, and these four buds may again give off others, and so regularly arranged systems of the animals are produced, the vascular system undergoing corresponding development and extension. The blood contains colourless corpuscles, and under certain circumstances a number of dark pigment granules present in the bodies of the successive generations, on undergoing atrophy appear to gain entrance into the circulating fluid.

THE *Revue des Cours Scientifiques* for the 19th inst. contains a report of a lecture by M. Claude Bernard on the history of medical science and its actual condition; of one by M. Harny, on human remains in the tertiary deposits in America, and on the theories of multiple centres of creation; and of one by Dr. Bertillon on the mortality of different departments of France.

IN the just published *Proceedings of the Royal Society of Edinburgh* for the session 1868-69, Professor Allman gives a description of Rhabdopleura, a new genius of Polyzoa. The cœnæcium or common stem consists of a branched tube partly adherent and partly free, the free portion forming tubes of egress through which the polypides move in the acts of exsertion and retraction. In the walls of the adherent portion a rigid chitinous rod is developed along their attached side, from distance to distance, each by a flexible cord or funiculus. The polypides are hippocrepian, and each carries a shield-like process on the hæmal side of its lophophore, external to the tentacular series. In development the polypide at an early stage is included between two fleshy plates on the right and left sides respectively, and which are partially united. For some time the two plates keep pace with the general development of the bud, but ultimately they cease to increase in size, and then remain as the shield-like processes carried by the lophophore of the polyzoon. Professor Allman regards these plates as representing the right and left lobes of the mouth in a Lamellibranchiate Mollusk, from which it follows that the relations of the Polyzoa are more intimate with the Lamellibranchiata than with the Brachiopoda, with which of late years they have been associated, but whose mantle lobes lie dorsally and vertically, instead of lying right and left as in the Lamellibranchiata. The lophophore of the Polyzoa he considers to have its representative in the labial palps of the Lamellibranchiata. The animal was obtained by the Rev. A. Norman and Mr. J. Gwyn Jeffreys in the course of deep-sea dredging in Shetland.

THE *Journal of the Chemical Society* for February is mainly occupied by a long paper by Mr. F. A. Abel, entitled "Contributions to the history of explosive agents," abstracted from the Philosophical Transactions for 1869. There are also shorter articles on nontronite, and on a new chromium oxychloride, by Dr. T. E. Thorpe, and observations on the solution of gases in water by Dr. Williamson.

THE *Monthly Microscopical Journal* for March contains the President's Address, an obituary notice of the late J. J. Lister, F.R.S., and articles on the structure of the stems of the arborescent *Lycopodiaceæ* of the coal measures, by Mr. Carruthers, and on the mode of examining the microscopic structure of plants.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 10.—The following paper was read: "On some elementary principles in animal mechanics. No. 3—On the muscular forces employed in parturition," by the Rev. S. Haughton.

March 17.—Papers were read as follows:—"On the law which regulates the relative magnitude of the areas of the four orifices of the heart," by D. H. Davies; and "On the estimation of ammonia in atmospheric air," by Mr. H. T. Brown. The process recommended is by the absorption of the ammonia by extremely dilute sulphuric acid, and the results obtained are said to be far more concordant than those procured by other means.

The Royal Astronomical Society, March 11.—Mr. W. Lassell, president, in the chair. Sixty-three presents were announced as having been received since the third meeting, and the thanks of the society were voted to their respective donors. A communication from Mr. Marsh was then read, in which that observer records the position-angles and apparent distances of the satellites of Uranus. It was ordered that this paper be printed forthwith.—The next paper was by Lieut. Herschel. In a letter to his brother, Prof. Alexander Herschel, the writer remarked on certain singular objects seen to traverse the sun's disc on October 17th and 18th, 1869. He was about to apply his spectroscope to the observation of a solar prominence when his attention was attracted to certain shadows traversing the disc of the sun, which became bright streaks when they had passed beyond it. At first he thought these appearances were due to sparks in the tube of the telescope, but the phenomenon lasted too long for this explanation to be available. He next thought that perhaps a system of meteors might be in transit, and prepared to subject the phenomenon to careful scrutiny. The equatorial was set in motion, the sun's disc being projected on a screen. The shadows were seen persistently traversing the solar disc, but at different velocities, the larger ones travelling most swiftly. There appeared to be two streams. He noticed that when the sun was in focus the objects were indistinct, and that they appeared very distinctly when he focussed on a distant cloud. At length, while he was attentively scrutinising the phenomenon he saw one of the objects come suddenly to a standstill, and then whisk off in a different direction; and then he perceived that the phenomenon he had been examining with such anxious care was not in reality an astronomical phenomenon at all, but consisted merely of a flight of locusts. He considered, however, that not only was the existence of so enormous a swarm of locusts as the duration of the stream indicated, an interesting fact in itself, but that we might find in the occurrence the explanation of many statements which had been made respecting meteors supposed to have transited the sun, and also of some peculiarities noticed by astronomers in America during the total eclipse of last year. Mr. Stone said that it was important when appearances of this sort were noticed that the observer should examine, as Lieut. Herschel had done, whether the objects seen in transit required the same focus as the sun. This was the best way of determining whether the objects were terrestrial or not. —Capt. Noble communicated three short papers to the society. The first had reference to occultations of stars by the moon. The second referred to the visibility of Venus near her inferior conjunction. He could detect only an arc of light less than a semicircle, and saw the body of the planet projected on the sky beyond, the planet appearing sensibly darker than the sky. On March the 3rd he saw the Zodiacal Light for the first time. He was struck by the fact that instead of appearing nearly coincident with the ecliptic, the light seemed inclined at an angle of about twenty degrees to that circle. It seemed, in fact, almost perpendicular to the horizon.—Mr. Browning exhibited some more drawings of Jupiter, remarking on the changes of form and colour which the belts on the planet had exhibited during the past few months. The president inquired whether a picture by Mr. Browning, in which the equatorial belt appeared twice as wide as usual, was not slightly exaggerated; but Mr. Browning remarked that it presented the planet exactly as he had seen it.—Mr. Procter then read a paper entitled "Notes on the Corona and the Zodiacal Light, with suggestions respecting the modes of observation to be applied to the Eclipse of next December." He remarked that if we have in reality sufficient evidence to determine whether the corona is or not a solar appendage, it would be a misfortune, and in a sense discreditable, to science, were the short time at the disposal of observers wasted in futile observations directed to settle a point determinable beforehand. He then expressed his conviction that the corona cannot be a terrestrial phenomenon. He pointed out that the very blackness of the moon as compared with the corona showed that the coronal light is behind the moon. The moon is, in fact, projected on the corona as a background, he urged, whereas the theory that the light is due to atmospheric glare requires that the corona should be a foreground. But passing over this argument, which is liable to the fatal objection of being too simple, he proceeded to inquire whether air which lies between the observer and the corona is in reality illuminated. He showed that all round the sun, for a distance of many degrees, there should be perfect darkness if the illumination of the atmosphere by direct solar light were in question. As to the atmospheric glare due to the chromosphere and prominences, he argued that it must be relatively small because it could bear no higher proportion to the actual light of the chromosphere, than ordinary atmospheric glare bears to actual sunlight, and we know this proportion is very small indeed. Again, as to light reflected from the atmosphere outside the shadow-cone, or from the surface of the earth, he urged that that also must be small, since not any part of the atmosphere above the observer's horizon was illuminated by more than a half-sun, while all the parts near the shadow-cone were in nearly total shadow. But a fatal objection to the view that the corona could be due to either the glare from the prominences or to light reflected from the surrounding air consisted in the fact that such glare ought to cover the moon's disc. He then referred to a number of observations confirming the view that the coronal light is not terrestrial; as the appearance of glare during partial eclipses, this glare always trending on the moon's disc; the relatively greater darkness of the central part of the moon's disc in annular eclipses; the visibility of that part of the moon's disc which lies beyond the sun in partial eclipses, the limb being seen dark on the background of the sky; the visibility of the corona in partial eclipses, even its most distinctive peculiarities having been recognised when the sun's disc is not wholly covered; and several other phenomena. He then adduced evidences to show that a solar appendage which one would expect to appear during total eclipses, actually does exist. First the Zodiacal Light shows that the sun is surrounded by such an appendage. Dr. Balfour Stewart's theory of this object, however physically sound, was opposed, he urged, by too many astronomical objections to be accepted for a moment: an object which exhibits no appreciable parallax, which rises and sets as the celestial objects do, and maintains a position in the heavens having a nearly constant relation to the ecliptic, cannot by any possibility be due to any peculiarity of the earth's atmosphere. Then Leverrier has shown that the motion of Mercury's perihelion indicates the presence of a ring of bodies in the sun's neighbourhood; and Mr. Baxendell has drawn a similar conclusion from the meteorological records of well-known observatories. Lastly, judging of the meteor systems according to the laws of probability, we have every reason to believe that for each one our earth encounters there must be millions whose perihelia lie within the earth's orbit. Since the earth encounters fifty-six such systems, it will be seen how enormous must be the total number. These should be visible during total eclipses, and since they would shine in part by reflected light, and in part through their intrinsic light (for those which come as near the sun as some comets have been observed to do, must be melted or even vaporised by the sun's heat) we have an explanation of the contradictory accounts given by those who have applied the polariscope and the spectroscope to the solar corona. Mr. Stone remarked that there ought to be three sets of observations made with the polariscope next December, since if there were but two the result would probably be contradictory, as was the case with regard to the observations made in India in 1868, and in America last year. Different parts of the corona ought also to be examined.

Zoological Society, March 10.—John Gould, F.R.S., V.P., in the chair. The secretary spoke of the additions to the society's menagerie during the month of February, amongst which were particularly noticed a collared fruit-bat (*Cynonycteris collaris*), born in the society's monkey-house on the 25th inst., being the first instance on record of a birth amongst these animals in captivity, and a kangaroo, believed to belong to a new species, for which the name *Macropus erubescens* was proposed.—Prof. Flower exhibited a drawing of a Cetacean animal lately captured in a

mackerel-net off the coast of Cornwall, which he identified with *Globiocephalus rissoanus*—a species hitherto only known to occur in the Mediterranean. The specimen was stated to be an adult female, about eleven feet long.—Mr. Sclater gave some additional details as to the correct locality of Amherst's pheasant (*Thaumalea Amherstiæ*), in reference to Mr. Swinhoe's communication on the same subject at the previous meeting.—Prof. Owen read a memoir containing descriptions of various bones of *Aptornis defossor*, *A. otidifornis*, *Notornis Mantelli* and *Dinornis curtus* obtained from deposits in different parts of New Zealand, and forming the fifteenth part of his series of memoirs on the extinct birds of the genus *Dinornis* and their allies.—Mr. R. Swinhoe read three papers on new or little known birds obtained by him in different parts of the Chinese Empire. The first of these related to new species obtained during a recent voyage up the Yangtze, amongst which were species of *Parus*, *Lanius*, *Ægithalus*, and other genera of *Passeres*. The second contained supplementary remarks on the pied wag-tails (*Motacillæ*) of China, in continuation of a former paper on the same subject. The third paper contained a notice of the different species of shore-plovers (*Ægialitis*) found in China, amongst which was a conspicuous new species obtained on the Yangtze, and proposed to be called *Ægialitis Hartingi*.—Mr. H. B. Sharpe read the second of a series of papers on the birds of Angola, containing an account of collections forwarded from that couutry by Mr. Monteiro. Amongst these was a bush-shrike belonging to the genus *Laniarius*, supposed to be new, and proposed to be called *Laniarius Monteiri*.—Dr. Günther read a note on the locality of the Batrachian recently described by him as *Megalixalus robustus*, which was stated to be from the Seychelles.—Dr. Günther read a paper on the genus *Prototroctes*, which contains two species of fishes from the fresh waters of Southern Australia and New Zealand. In general appearance these fishesr esemble *Coregonus*, but their internal structure had led Dr. Günther to constitute them along with the South American genus *Haplochiton*, a distinct family, *Haplochitonidæ*, which appeared to be the representative of the Salmonoid group in the southern hemisphere.—A communication was read from the Rev. O. P. Cambridge, containing descriptions of three new species of the Arachnida of the genus *Idiops*, in continuation of a former paper on the same subject.

Mathematical Society, March 10. — Professor Cayley, president, in the chair. Mr. E. Bradshaw Smith was elected a member, and Messrs. A. and W. M. Ramsay were admitted into the society. Mr. Tucker (hon. sec.) then read two communications by Mr. Clerk Maxwell, F.R.S.; the one on topographical geometry, which led to a discussion in which Mr. Archibald Smith, F.R.S., and the president took part; the other a note on a case of fluid motion. "In most investigations of fluid motion we consider the velocity at any point of the fluid as defined in magnitude and direction as a function of the coordinates of the point and of the time. We are supposed to be able to take a momentary glance at the system at any time, and to observe the velocities, but are not supposed able to keep our eye on a particular molecule during its motion. This method therefore properly belongs to the theory of a continuous fluid alike in all its parts, in which we measure the velocity by the volume which passes through unit of area rather than by the distance travelled by a molecule in unit of time. The molecular theory, as it supposes each molecule to preserve its identity, requires for its perfection a determination of the position of each molecule at any assigned time. As it is only in certain cases that our present mathematical resources can effect this, I propose to point out a very simple case with the results. Let a cylinder of infinite length and of radius a move with its axis parallel to z, and always passing through the axis of x with a velocity v, uniform or variable in the direction of x through an infinite homogeneous incomprehensible perfect fluid." The solution of the problem involved work hardly suited for the columns of NATURE.—Mr. S. Roberts then discussed the following problem which occurred in his paper on the pedals of conic sections. If two circles are given, one of which passes through the centre of the other, and if a line equal in length to the radius of the latter circle moves with an extremity on each, the locus of any point rigidly connected with the moving line will be composed of a circle and a bicircular quartic having a finite double point. His account of the communication closed with a discussion of the following problem, intimately connected with the subject-matter of the paper:—Given the paths of two points of an indefinite plane, moving in plane space, to find the path of an arbitrary point of the plane. If $F(x,y)=0$, $\phi(x,y)=0$ are the equations of the given path, we have to eliminate θ from

$$F(l\cos\theta + p\sin\theta + X,\ l\sin\theta - p\cos\theta + Y) = 0$$
$$\phi(l\cos\theta - q\sin\theta + X,\ l\sin\theta + q\cos\theta + Y) = 0$$

The president, Professor Hirst, Mr. Cotterill, and the author, took part in a discussion on the paper.—Mr. Archibald Smith then made some remarks on the scale for compensation in the Irish Land Bill.

MANCHESTER

Philosophical Society, March 8.—Dr. J. P. Joule, president, in the chair. Sir James Cockle, President of the Queensland Philosophical Society, was elected a corresponding member of the society. A letter was read from Mr. Dancer, on Dr. A. Ransome's paper "On the Organic Matter of Human Breath." Mr. E. W. Binney called the attention of the meeting to the frightfully high death-rate of Manchester and Salford, which continued to increase, notwithstanding the appointment of officers of health, and the doings of the councils of the two towns. A paper was read, "On the Suspension of a Ball by a Jet of Water," by Osborne Reynolds, Professor of Engineering, Owens College.

Microscopical and Natural History Section, February 28.—A paper was read "On some Shell Deposits at Llandudno," by Mr. Joseph Sidebotham.

Physical and Mathematical Section, January 4.—Mr. E. W. Binney, president of the Section, in the chair. A paper was read "On the Rainfall of 1869, at Old Trafford, Manchester," by Mr. G. V. Vernon.

March 1.—Mr. E. W. Binney, president of the Section, in the chair.—A paper was read "On the Results of Rain-gauge and Anemometer Observations made at Eccles, near Manchester, during the year 1869," by Mr. Thomas Mackereth.

HEREFORD

Woolhope Naturalists' Field Club, February 22. — Annual meeting; Mr. James Rankin in the chair. After the usual club business had been transacted, and a committee appointed to report upon the practicability of establishing a local museum, the Rev. F. T. Havergal detailed the progress that had been made with reference to the publication of the *Mappa Mundi*, one of the chief curiosities of the Cathedral. It has been satisfactorily established that the date at which the map was executed was the very commencement of the fourteenth century. It is drawn in accordance with the prevailing notions of geography at that period; the habitable earth is represented as a circular island with the "ocean-stream" flowing around it. Jerusalem is placed in the centre. Asia occupies nearly the whole upper (or eastern) half of the circle, while Europe holds the lower quarter on the left hand, and Africa that on the right. The Hereford map is distinguished from most mediæval maps not only by its great size, but also by its illustrations of objects in natural history, and its numerous inscriptions. It is proposed to publish a fac-simile (obtained by photography) in colours, accompanied by an exhaustive account of the history of the map, and of the legends upon it. The price has been limited to two guineas, and Mr. Stanford, of 6, Charing Cross, London, has been empowered to receive orders.—A paper upon the Reproducton of the Mistletoe, by the Rev. R. Blight, was read, and drawings exhibited which showed the gradual penetration of the parasite through the bark of the Magnolia in search of the sap.—A new *Clavis agaricnorum*, by Mr. Worthington Smith, F.L.S., was also exhibited. Its principle of classification is based on the colour of the spores, and the book is divided into sections respectively coloured white, pink, brown, purple, and black. In each of these sections the typical forms of the different agarics are given, and the identification of any species is thus the work of a moment.—Dr. Bull communicated the discovery of an Agaric entirely new to Britain, the *Cortinarius russus*, which he had met with in several woods near Hereford. He had also collected specimens of the rare *Asarum Europæum*, or *Asarabacca*, near the ruins of Limebrook Priory, a habitat which confirms the belief in its having been a cultivated plant.—At the evening meeting the President reviewed in his address the progress of science during the past year, and referred to the contributions made by different members of the club to various branches of natural history and archæology.—Dr. Bull read an interesting paper upon Deerfold Forest which, when published, will form a very valuable addition to the topography of the county.—The Rev. H. Cooper Key was elected president for the ensuing year.

EDINBURGH

Edinburgh Botanical Society, January 13.—Mr. Robert Brown, V.P., in the chair. The following communications were read :—

1. "Note on the embryo of *Ruscus aculeatus*." By Professor Dickson.

2. "Notice of plants collected in Spitzbergen and Nova Zembla in the summer of 1869." By William Livesay.

3. "Notice of some botanical excursions with pupils during the summer of 1869." By Professor Balfour.

At the last meeting of the Edinburgh Botanical Society, Dr. Balfour stated that during the course of last summer he made several botanical trips with his pupils, the results of which seemed not unworthy of notice. On 12th June, a party visited Manuel, Woodcockdale, Carriber Glen and Castle, Bowdenhill, Cockleroy and Linlithgow. On 19th June, a party visited Denny, and proceeded up the banks of the Carron as far as the Hermitage. On 26th June, a party of 94 proceeded to Cleghorn, and walked along the banks of the Mouse as far as Cartland Crags, dividing then into two parties, one of which visited Stonebyres and the other Corra Linn. On 3rd July, a party of 50 proceeded by Stirling to Dollar, and thence to the Ochils. On 17th July, an excursion was undertaken to Perth and Dunkeld, and some of the party visited Methven bog. On 22nd July, a party proceeded by Perth and Forfar to Kirriemuir, and thence to Clova, and next day visited Loch Brandy and the mountains above it. In the course of two days a large number of the rarer alpine plants of Scotland was gathered, and in all the other excursions many interesting and rare plants were gathered.

4. "On the botany of the Dominion of Canada and adjacent parts of British America (Part I., *Ranunculaceæ*)." By Professor Lawson, Dalhousie College, Halifax, Nova Scotia.

5. "On the introduction of Ipecacuanha plant (*Cephælis Ipecacuanha*)." By Mr. M'Nab.

6. "Notice of *Sicana odorifera*, Naudin (*Cucurbita odorifera*, Velloso, Flor. Flum.") By Senhor Joaquim Correa de Mello, Camprinas, Province of St. Paulo, Brazil. Communicated by Mr. Daniel Hanbury.

7. Hints for Collecting Cryptogamia. By Prof. Dickie.

8. Miscellaneous communications.—*Ruscus aculeatus.*—*Cones of Abies.*

GLASGOW

Philosophical Society of Glasgow—*Chemical Section*, January 31.—Dr. William Wallace, F.R.S.E., F.C.S., vice-president, in the chair. A paper was read by Mr. John Christie, on "the history of Madder, the various investigations relating to its character and composition, and the proposed sources of Artificial Alizarine." After giving an elaborate account of the progress of the art of dyeing, by the use of the two madder plants, *Rubia cordifolia*, or munjeet, of Bengal, and *Rubia tinctorum* of various European countries, the author proceeded to mention the various persons who had undertaken scientific investigations with a view to determine the number and nature of the colouring ingredients of madder-root. He stated that these investigations commenced about the end of last or beginning of the present century. Watt, Bartholdi, and Haussmann were amongst the earliest investigators. Kuhlman, in 1823, published a complete approximate analysis of madder. He obtained two colouring matters, his *matière colorante rouge*, and a fawn colour, which he did not consider worthy of investigation. Robiquet and Colin published the results of their researches on Alsace madder in 1826. The particular colouring matter of madder they named *alizarine*, and another body, which they considered to be a modification of alizarine, they termed *purpurine*. Gautier de Clanbry and Persoz obtained two colouring matters in the following year—*matière colorante rouge* and *matière colorante rose*. They were the first chemists to prepare madder extract, or *garancine*, a substance which was first manufactured largely at Avignon in 1829. Dr. Schunck obtained no fewer than seven substances from madder, two of them being colouring matters, having the composition indicated by the formulæ $C_{14}H_{10}O_4$, and $C_{32}H_{22}O_{10}$. His results were published in 1848. Debus, by treating Zealand madder, obtained two colouring bodies, which he called Lizaric acid and Oxylizaric acid. Wolff and Strecker obtained alizarine and purpurine; the latter they regarded as oxide of alizarine. The author next enlarged on various series of researches made by Strecker, Schützenberger, and Lanth, P. and E. Depouilly, Dr. Anderson (Glasgow), Rochleder (Prague), and Graebe and Laubermann, which seemed to indicate an approximation towards the accomplishment of a long-wished-for desire—namely, the production of artificial alizarine. Anthracene, one of the coal-tar products, came to be regarded as the starting-point. Graebe and Laubermann obtained a product closely allied to alizarine, and in December 1868 they obtained provisional protection for their process in this country. They employed anthracene ($C_{14}H_{10}$), converting it into anthrachinon ($C_{14}H_8O_2$) by using bichromate of potash. They transformed that body into bibromanthrachinon, ($C_{14}H_6Br_2O_2$), a substitution product; and, by subsequent treatment with potash and an acid, they obtained from it a body which they termed artificial alizarine. In the course of last year other patents were secured by Brönner and Gulzkow, of Frankfort-on-the-Maine, and by W. A. Perkins, F.R.S.; and recently Messrs. Lucius & Co., of Hoechst, near Frankfort, have prepared artificial alizarine by a secret. Mr. Christie concluded his paper by giving an account of numerous investigations which he had made with commercial artificial alizarine in order to test its colorific and other properties, and stated his reasons for regarding the natural and artificial compounds as not being identical. In the discussion which followed, Mr. Hogg and other speakers mentioned facts in support of the identity of the two products, one of the facts being that mordanted cloth dyed with pure artificial alizarine stands soaping better than that dyed with garancine.

PARIS

Academy of Sciences, March 14.—M. Faye presented a memoir on the photographic observation of the transits of Venus, and on an apparatus of M. Laussedat's.—The author noticed the imperfection of Halley's method of observation, which has already been recognised by the German astronomers, suggested the employment of photography as a means of observing the transit of Venus which will take place in 1874, and communicated a letter from M. Laussedat, describing an arrangement by which photographic observations may be taken.—A letter from M. Wolf, of Zurich, accompanying a printed memoir, was read. The author stated that the invention of the bubble-level was to be ascribed to a Frenchman named Capotos, and suggested that a search should be made in France for documents relating to this subject, and to some others to which he referred. — A memoir was read by M. Phillips on the changes of condition of a mixture of a saturated vapour and of its liquid, according to an adiabatic line.—A note by M. Zaliwski was communicated, on the selection of the bodies which should be placed in contact with carbon as the positive pole of a battery. The author stated that these should be oxydising bodies, and among these such as are impressionable by light, such as nitric acid and the manganates of potash, seem to be most efficacious. He described a battery in which the carbon is impregnated with an ammoniacal solution of chloride of silver, dried and treated with nitric acid to remove the excess or ammonia; with pure water this forms a battery of great intensity.—M. de Saint-Venant communicated a note by M. F. André, containing an account of experiments on the velocity of propagation of sound in water in a cast-iron conduit of 0·80 M. in diameter. The author found the velocity of propagation to be only 897·80 M. per second—M. H. Sainte-Clair-Deville communicated a second memoir on the "nascent state," in which he discussed the phenomena observed when zinc is brought in contact with a mixture of sulphuric or hydrochloric and nitric acids.—M. E. J. Maumené read a second memoir on a general theory of chemical action.—M. Combes presented a note by M. L. Gruner on the mechanical properties of phosphuretted steels. The author referred to the statements of Sir. W. Fairbairn as to Heaton's steel, and showed by analysis that it contains from 0·002 to 0·003 of phosphorus. He remarked that the favourable character of this steel under the ordinary tests was negatived when the test of a shock was applied to it. M. Boussingault supported the opinions of M. Gruner.—M. A. Milne-Edwards communicated a note on the Ornithological fauna of the Bourbonnais during the middle Tertiary period, in which he stated that the birds of the Miocene deposits of that district possessed a tropical, and especially an African character. He noticed remains of a parrot, a trogon, a sand-grouse, a swift of the group of the Salanganes, a Marabou stork, and a Secretary bird.—M. Leveillé presented a note on the discovery of remains of quaternary man in the manufactories of stone implements at Grand-Pressigny; and M. Richard noticed the discovery of instruments of the Stone age in Arabia and Egypt. The author stated that he has found worked stones at the foot of Mount Sinai, near Cairo, and at Thebes.—M. C. Wœstyn presented a memoir on

the means of destroying the contagious miasmata of hospitals, upon which MM. Dumas and Bouillaud made some remarks.—M. Wurtz presented a note by M. Verneuil on the cure of tracumatic tetanus by chloral.—Several other communications were made of which the titles only are given.

BERLIN

German Chemical Society, March 14.—M. Schultz-Sellak has obtained a liquid modification of sulphuric anhydride, which, under ordinary circumstances, speedily passes into the solid form.—M. Tiemann has converted trinitrotoluol into toluylendiamide. The same, conjointly with Mr. Judson, has studied several isomeric dinitrobenzoic acids.—Mr. Genz reported on some derivatives of xylidine.—Dr. Rüdorff showed large crystals of carbonate of ammonium deposited from coal gas.—M. Bornemann exhibited glass tubes which had for some months been exposed to the action of steam under a pressure of eleven atmospheres. The glass showed deep notches and furrows where acted upon by the steam, but was not attacked in those parts which had remained immersed in the water of the boiler.—A curious transformation of forged iron into large crystals has been observed by M. Egelts. A cylinder used in a cotton-mill proved to consist of crystals apparently affecting the form of pentagonic dodecahedra of two or three millimetres in diameter.—Prof. Hoppe Seyler reported on the colouring matter of blood. He has found that hæmato-globulin does not pass directly into hæmatine, as has been supposed until now, but that the latter product derives by oxidation from hæmatochromogen discovered by the author.—Prof. Kekulé and Hideck have converted diazoamidobenzol into azobenzol.—Dr. Köhler attacked the views lately published by Wanklyn, on the atomicity of sodium.—Dr. Schaer reported on the presence of ozone in the fluor spar of Wölberndorf, Saxony.

PHILADELPHIA

Academy Natural Sciences, January 11.—Professor O. C. Marsh, of Yale College, exhibited a series of specimens of the remains of birds from the Cretaceous and Tertiary of the United States, which showed that this class was well represented during these periods, although no species have yet been described from these formations in this country, and none indeed from older rocks, since it now appears to be well established that the bird-like footprints in the Connecticut Valley were made by Dinosaurian reptiles. Among the specimens shown were the remains of at least five species of Cretaceous birds, although but one or two species have hitherto been described from strata of this age in Europe. The present Cretaceous specimens were all found in the greensand of New Jersey, and with one exception in the middle marl-bed. They are all mineralised, and in the same state of preservation as the bones of extinct reptiles found with them in these deposits, and hence are readily distinguished from the remains of recent birds which have occasionally been found near the surface in the marl excavations of New Jersey. The most interesting of the specimens exhibited was the distal portion of a large and robust tibia, apparently of a swimming bird, about the size of a goose. It was found in the greensand at Birmingham, New Jersey, in the pits of the Pemberton Marl Company. For this new genus and species Professor Marsh proposed the name *Laornis Edvardsianus*. Two species of small wading birds, which appear to have been allied to the Curlews, were also represented each by the distal end of a tibia, and probably by some other less characteristic portions. The larger of these species, which was found in the greensand of the middle marl-bed at Hornerstown, New Jersey, was named *Palæstringa littoralis*. The smaller species, which was called *Palæstringa vetus*, was founded on the specimen mentioned by Dr. Morton in his "Synopsis of Cretaceous Fossils," p. 32, which has since, however, been generally regarded as a recent species. The specimen was found in the lowest marl-bed at Arneytown, New Jersey, and is now in the collection of the Academy. Portions of the humeri of two small and closely-related species, apparently of the Heron family, were part of the series shown. They were found deep in the greensand of the middle marl-bed near Hornerstown, New Jersey, in the pits of the Cream Ridge Marl Company. For the species thus represented the names *Vetardea elegans* and *Vetardea affinis* were proposed. The remains of several species of Tertiary birds were also exhibited by Professor Marsh. Among these was the lower extremity of tibia, closely resembling that of some of the Cranes. It was found in the Miocene o the Niobrara River, by Dr. F. V. Hayden, and is interesting as the only representative of a fossil bird yet detected in the Tertiary deposits west of the Mississippi. This specimen, which belongs to the Academy, indicated a new species, which was named *Grus Haydeni*. Another species of extinct birds was represented by portions of a humerus and radius, also in the collection of the Academy; they were found many years since in the Miocene of Maryland by Mr. T. A. Conrad. This species, which appears to be closely related to the Gulls, was named *Larus Conradii*. Several other interesting specimens of bird remains were shown, but most of them were not sufficiently characteristic to admit of determination. With the exceptions already mentioned, the fossils exhibited belonged to the museum of Yale College.

DIARY

THURSDAY, MARCH 24.

ROYAL SOCIETY, at 8.30.—On the Madreporia dredged up by the expedition in H.M.S. *Porcupine*: Prof. Duncan.
ROYAL INSTITUTION, at 3.—Chemistry of Vegetable Products: Prof. Odling.
ZOOLOGICAL SOCIETY, at 8.30.—On the Birds of Veragua: Osbert Salvin.—Exhibition of a metamorphosed Axolotl: W. B. Tegetmeier.—On two rare species of Pheasants recently added to the Society's Collection: Mr. Sclater.
LONDON INSTITUTION, at 7.30.—Geology: Dr. Cobbold.
SOCIETY OF ANTIQUARIES, at 8.30.—On the Greek Inscriptions found at Autun: Rev. W. B. Marriott.

FRIDAY, MARCH 25.

ROYAL INSTITUTION, at 8.—Anglo-Saxon Conquest: Prof. Rolleston.
QUEKETT MICROSCOPICAL SOCIETY, at 8.30.

SATURDAY, MARCH 26.

ROYAL INSTITUTION, at 3.—The Sun: J. Norman Lockyer, F.R.S.

MONDAY, MARCH 28.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.
ROYAL INSTITUTE OF BRITISH ARCHITECTS, at 8.
INSTITUTE OF ACTUARIES, at 7.
LONDON INSTITUTION, at 4.—Chemistry: Prof. Bloxam.

TUESDAY, MARCH 29.

ROYAL INSTITUTION, at 3.—Nervous System: Prof. Rolleston, M.D., F.R.S.
INSTITUTION OF CIVIL ENGINEERS, at 8.—Description of the St. Pancras Station, Midland Railway: W. H. Barlow, M.I.C.E., F.R.S.

WEDNESDAY, MARCH 30.

SOCIETY OF ARTS, at 8.
CHEMICAL SOCIETY, at 8.—Anniversary Meeting.

THURSDAY, MARCH 31.

ROYAL SOCIETY, at 8.30.
SOCIETY OF ANTIQUARIES, at 8.30.

BOOKS RECEIVED

ENGLISH.—The State, the Poor, and the Country: R. H. Patterson (Blackwood and Sons).—Quarterly Journal of the Geological Society (Longmans) —Photographic Art Journal, No. 1; illustrated (S. Low, Son, and Marston).—Choice and Chance: Rev. W. A. Whitworth (Bell and Daldy).—The Arts in the Middle Ages; illustrated: Paul Lacroix (Chapman and Hall).—United States Geological Survey of Colorado and New Mexico: F. D. Hayden (Washington).—Introductory Text-book to Physical Geography: D. Page (Blackwood and Sons).—Mrs. Loudon's First-book of Botany. Edited by D. Woosten (Bell and Daldy).—Principles of the Science of Colour: W. Benson (Chapman and Hall).

FOREIGN.—Handbuch der Lehre von den Geweben: S. Stricker.—Für Baum und Wald: M. F. Schleider.—Die Eiszeit der Erde: A. Braun.

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