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THE EDUCATION OF CIVIL ENGINEERS

ATTENTION has been called to this important subject by a pamphlet recently issued by the Institution of Civil Engineers, containing a clear and well-digested account of the education and status of engineers at home and abroad. The pamphlet, however, is more remarkable for its omissions than for its contents, among which we find premisses warranting a conclusion or many conclusions concerning the education and position of the engineer in different countries. The documents which have been employed in its compilation have been collected, arranged, and issued, under the supervision of the Council of the Institution of Civil Engineers, a body most able to draw conclusions, and to give practical effect to any resolution they may adopt, and yet no conclusion whatever is drawn, and no resolution whatever is adopted. Perhaps, indeed, the Council consider that the education of engineers in England cannot be improved; this interpretation may easily be given to the short summary given of the English system, contained in the following passages:—

There is, further, in England no public provision for engineering education. Every candidate for the profession must get his technical, like his general education, as best he can; and this necessity has led to conditions of education peculiarly and essentially practical, such being the most direct and expeditious mode of getting into the way of practical employment.

The education of an engineer is, in fact, effected by a process analogous to that followed generally in trades, namely, by a simple course of apprenticeship, usually with a premium, to a practising engineer; during which the pupil is supposed, by taking part in the ordinary business routine, to become gradually familiar with the practical duties of the profession, so as at least to acquire competency to perform them alone, or, at least, after some further practical experience in a subordinate capacity.

It is not the custom in England to consider *theoretical* knowledge as absolutely essential. It is true that most considerate masters recommend that such knowledge should be acquired, and prefer such pupils as have in some degree attained it, and it is also true that intelligent and earnest-minded pupils often spontaneously devote themselves, both before and during their pupilage, to theoretical studies; but these cases, though happily much more frequent now than formerly, really amount only to voluntary departures from the general rule.

This thorough proficiency in practical matters tends largely to compensate for—in many cases to outweigh—the deficiency in theoretical attainments; and it is undoubtedly this, influenced in some degree by the natural self-reliance and practical common sense inherent in the English character, which has given such a high standing to the profession in this country. The *practical* education in England is perhaps the most perfect possible, if the opportunities obtained during the pupilage are ample, and the pupil properly avails himself of them.

In marked contrast with this language comes the summary of the description of foreign engineering education.

The education of foreign engineers is strongly contrasted with that in England in every particular. Practical training by apprenticeship is unknown; the education begins at the other end, namely, by the compulsory acquirement of a high degree of theoretical knowledge, under the direction, and generally at the expense, of the Government of the country. Partly with this, and partly afterwards, there is communicated a certain amount of information on practical matters; but this is imparted in a way differing much from the English plan, and probably with less efficient results.

Thus, while the English engineer is launched in his profession with the qualification of a considerable practical experience, but with perhaps little or no theoretical knowledge, the foreign one begins with a thorough foundation of principles, but with a limited course of practice; a deficiency, however, which tends to correct itself with time.

After a few paragraphs showing how these principles are worked out in various countries, we have a summary of suggestions made by engineers and others, without approval or condemnation. The body of the pamphlet consists of dry statistics, which would have been of greater value had not much of the matter been already published in various blue books; and at the end we find suggestions by individuals and extracts from published documents quoted, without any partiality for one rather than another.

It is very difficult to understand why the Council of the Institution has issued a document of this kind. Parents and guardians may find the list of schools and classes valuable, but neither the general public nor the engineers required information of the kind here given. Since the Paris Exhibition, we have been deluged with letters, pamphlets, and evidence as to foreign Polytechnic Schools. What we do require is some authoritative recommendation of one scheme rather than another for raising the standard of engineering education in England. The Council may plead in extenuation of this grievous omission that they are not a legislative body; that their decisions would bind no one, and that they have always disclaimed all responsibility for the opinions expressed in the engineering papers which they publish. If they take no higher view of their functions than this, they might well have abstained from publishing this dry collection of statistics. If, on the other hand, they really mean that the education of young engineers in England needs no improvement, it is a pity that the opinion is not stated in so many words.

The language used conveys a mild expression of paternal approval of the good boys who "spontaneously elevate themselves to theoretical studies," but we almost see the bland smile of compassion with which the successful Nestor regards the proud enthusiasts.

We have the list of colleges where these good boys may be (spontaneously) diligent, but not a word indicating that the number of institutions is insufficient; that the number of classes in the institutions is in defect or excess of the requirements; far less any recommendation that engineers should make attendance on science classes compulsory instead of voluntary. We have an excellent short account of the Whitworth Scholarships, but no word of approval, no hope expressed that the example set may be followed. There is no suggestion that any new technical chairs are required, such as those lately founded in Manchester and Edinburgh, and we can well imagine that when other colleges or patrons approach Government asking for assistance to supplement local efforts, the Treasury may point to this pamphlet and say, Surely if the leading engineers in the kingdom are satisfied, we cannot be justified in giving you the assistance you ask.

No preference is indicated for Mr. Scott Russell's tremendous scheme, of Great Eastern proportions, no condemnation is hinted of collegiate education for our Indian engineers. Engineering degrees meet neither favour nor contempt; the gods of the profession seem to live far removed from all this turmoil, and do not deign even to nod approval.

Seriously, it seems impossible that the Council of the Institution can rest satisfied with such a contribution to the cause of education as this barren pamphlet. It is their duty to take action; their recommendations would have no legal force, but great moral weight. Let them say whether they desire great Polytechnic Schools on the continental model. Not improbably public money will ere long be granted for some such. Let them approve or condemn the Indian College. Let them recommend engineers to compel the attendance of their pupils at suitable classes, and to refuse all students as apprentices who cannot show that they have received proper preliminary training. If proper classes do not now exist for the students, let them tell us where they are wanted and what they ought to be. Let them declare what the preliminary training of a pupil must be. Let them fix a practical value on the engineering degrees of those colleges which deserve such encouragement; or finally, if they will do none of these things, let them say, if they dare, that they are perfectly well satisfied with things as they are.

RECENT PETROGRAPHICAL LITERATURE
II.

Untersuchungen über die Mikroskopische Zusammensetzung und Structur der Basalt-Gesteine. Von Dr. F. Zirkel. (Bonn, 1870.)

Mikromineralogische Mittheilungen. Von Dr. F. Zirkel. Pp. 801. (Neues Jahrbuch für Mineralogie, 1870.)

Beiträge zur Petrographie der plutonischen Gesteine. Von Justus Roth. (Reprinted from the Transactions of the Royal Academy of Sciences of Berlin.)

Sur les Crystallites, études crystallogeniques. Par H. Vogelsang. (Archives Néerlandaises, 1870.)

Kritische Mikroskopisch-mineralogische Studien. Von H. Fischer. (Freiburg.)

Mikroskopische Unterscheidung der Mineralien aus der Augit, Amphibole und Biotit-gruppe. Von G. Tschermak. (Proceedings of the Vienna Academy of Sciences, 1869.)

AMONG the Continental petrographers who have led the way in the recent reform and extension of this branch of science, none can claim a more prominent place than Dr. Zirkel. Although still a young man, he has held professorships successively at Lemberg and at Kiel, and we rejoice to hear from him that he has been selected to succeed the venerable Dr. Naumann at Leipzig. He is the author of many excellent mineralogical and petrographical papers, and of the best text-book of petrography which has yet been published. Especially has he distinguished himself by the zeal with which he has followed out the ideas first broadly sketched by Mr. Sorby, and has shown how absolutely indispensable is the application of the microscope to the study of the composition and history of rocks. His researches, while extending over the length and breadth of Germany, have not been confined to the Continent, but have been carried with characteristic enthusiasm even as far as the peaks of Arran, and the cliffs and glens of our north-western isles.

A few years ago he resolved to devote himself to a comprehensive study of the rocks to which the general name of basalt has been given. Though abundant chemical analyses had made the ultimate chemical constitution of these rocks well known, the mineralogical composition of them still remained rather vaguely defined. Their compactness and dark opaque hue made it difficult to investigate the separate mineral ingredients of which they consisted, and men were still speculating about the mineralogical nature of that part of basalt which is soluble in acid, when Dr. Zirkel set to work to collect specimens of basalt from every available locality, and to prepare thin transparent sections of them for examination with transmitted light under the microscope. The result of these investigations appears in the little volume now before us, which is appropriately dedicated to Mr. Sorby. In a brief introduction the author recounts the state of the question when he took it up. Having collected and prepared upwards of 300 sections of basalt from the most varied localities, he believes that he has obtained samples of at least the chief types of composition and structure among the basalts, and he now gives us this first instalment of his labour.

The first section of the volume treats of the microscopic structure and peculiarities of the minerals which enter as chief ingredients into the composition of basalt—augite, felspar, nepheline, leucite, olivine, magnetic iron, &c. This is an especially valuable part of the book, seeing that it furnishes materials for speculating both upon the conditions under which basalt was erupted, and on the various metamorphic changes which the rock as a whole and its component minerals in particular, undergo under the influence of percolating water and atmospheric weathering.

The second part deals with the general microscopic structure of basalt-rocks. The common notion regarding that structure has hitherto been that down to its minutest particles basalt is a crystalline rock, that its individual microscopic ingredients mutually impinge on each other, and that the difference between the structure, for example, of granite and basalt consists in little more than in the varying relative size of their component minerals. Prof. Zirkel, however, shows that this notion, which has been founded on mere deduction and not on direct observation, must be changed. He finds that in the majority of the specimens examined by him, there exists between the most minute ingredients a more or less abundant substance, not individualised into crystals, but amorphous, acting like a cement, sometimes glassy in character, sometimes half-glassy, owing to the appearance of hair-like particles, and sometimes completely dentrified so as to present a confused aggregate of darker or lighter minute granules, needles, hair, and crystals. He regards it as hardly possible to doubt that this glassy base in basalt is the residuum of the original *magma* out of which the recognisable minerals in the rock crystallised, and that it furnishes us with a new proof of the igneous origin of basalt.

In the next section the author proceeds to offer a new subdivision, and detailed descriptions of the basalts. He bases his classification upon the mineralogical composition of the rocks, as made known by microscopic analysis; and taking the non-ferruginous, colourless silicate as his guide, finds that the rocks hitherto classed under the general term basalt, group themselves naturally in three divisions: (1) the Felspar-basalts; (2) the Leucite-basalts; and (3) the Nepheline-basalts. All the three groups always contain augite and magnetic or titaniferous iron, and almost always olivine. So far as Dr. Zirkel's researches go, it appears that all our British basalt-rocks belong to the first or felspar group.

In the last few pages of his memoir the author adds some pertinent remarks on the hitherto vaguely defined series of rocks, which, as he remarks, under the various names of greenstone, trap, melaphyre, &c., play among the secondary and palæozic formations a part like that which is performed by the basalts in the tertiary formations. And here let us remark that in the chronological separation of igneous rocks made use of by our German fellow-workers, there is something eminently unsatisfactory. The term basalt is restricted by them to tertiary and post-tertiary rocks. But by what methods has the age of each rock been determined? No geologist who has ever had any experience in mapping a district of igneous rocks, can fail to realise how exceedingly difficult it sometimes is to decide upon the true age of such rocks. It is of course easy to say that all basalt is of tertiary or post-tertiary date, and, regarding this as an axiom, to look on every mass of basalt as of later origin than the secondary formations. But the axiom seems to us exceedingly doubtful. In Dr. Zirkel's memoir itself, we have basaltic rocks described which are not only certainly not tertiary, but are probably palæozoic. That igneous rocks have varied in the geological past is highly probable, but geologists are hardly yet in a position dogmatically to assert that no basalt was ever erupted before tertiary times. We cordially wish that our excellent friend Dr. Zirkel will take up the so-called melaphyres; and from what we have ourselves seen of the microscopic structure of the British examples, we shall be greatly surprised if he does not find that from these rocks to true basalt there is such an insensible gradation that no sharp line can be drawn between them. In the meanwhile he deserves the thanks and congratulations of all lovers of mineralogical geology for this admirable memoir on the basalts.

That Prof. Zirkel is still busy with his researches, is shown by the paper (second in the list at the head of this article) which appeared in a recent part of the *Neues Jahrbuch*, and in which he investigates the peculiarities in the minute structure of rock-forming minerals, and also of artificially-fused basalt and syenite.

If the limits of this journal and the patience of its readers permitted, a good deal ought to be said about Roth's most laborious work on the Petrography of the Plutonic Rocks. It is based on the analyses published from 1861 to 1868, which, given in full as an appendix, form half of the book. The word plutonic is used by the author in the sense of originating from igneous fusion, and he includes under it, not only igneous rocks commonly so called, but also gneiss, schist, and clay-slate. These, according to his view, are "the first crusts formed by the cooling of the earth's mass, not metamorphic, that is, not altered in various ways by dark, strange processes which appeared but once and never afterwards; although, indeed, these gneisses and schists, like other rocks, and even more than other rocks, by virtue of their antiquity and position, have undergone chemical changes." From this extract one may judge of Herr Roth's geology. He is a chemist rather than a geologist, and has gained deserved distinction for the great labour he has expended in the collection and discussion of analyses. In his present work, read as a memoir before the Berlin Academy of Sciences, he has amassed all the analyses he could find, which have appeared since the publication in 1861 of his *Gesteinsanalysen*, and has prefixed to them a discussion of the chemical composition of the various rock-species. As a work of reference in the chemical part of petrography, the book is of great value. Two important features are the analyses of decomposed rocks, and the account given of weathering.

Herr Voglesang is another ardent student of the microscopic structure of rocks. A few years ago he published a little work containing the most beautiful coloured illustrations of that structure which have yet appeared. In the present paper he describes under the name of *crystallites* the non-crystallised but yet more or less regularly grouped inorganic bodies which are found in crystals and rocks. As the paper, however, is to be followed by others, we reserve our notice of it for the present.

Professor Fischer's little pamphlet is a modest production, but one which could not have been prepared without a great deal of hard work. Finding that minerals, which to all outward appearance are simple and homogeneous, can yet be resolved by microscopic examination into as many as sometimes four distinct minerals, he has analysed by this method some sixty minerals, and publishes his results in the present paper, which should be in the hands of every petrographer.

Professor Tschermak's essay shows how by microscopical examination with polarised light, it is possible to distinguish augite and hornblende, even when minutely diffused through a rock. The paper is too important to be noticed at the end of this article, and we propose to return to it on a future occasion.

ARCH. GEIKIE

GODMAN'S NATURAL HISTORY OF THE AZORES

Natural History of the Azores or Western Islands. By Frederick Ducane Godman, F.L.S., F.Z.S. (Van Voorst, 1870.)

SINCE the time when Mr. Darwin called attention to the peculiarities of the fauna and flora of the Galapagos in his "Journal of Researches," and showed in his "Origin of Species" how important were the lessons to be learnt from oceanic islands in general, the subject has had great attractions for naturalists, and much material has been collected for its elucidation. Mr. Wollaston's bulky volumes on the Coleoptera of Madeira, the Canaries, and the Cape de Verdes, are models of careful research; but Mr. Godman appears to be the first who has, after a personal exploration of one of these oceanic groups, endeavoured to collect all that is known of its natural productions, and published the result in a condensed and convenient form; and for so doing he deserves the thanks of all naturalists.

Oceanic islands, as defined by Mr. Darwin, are those smaller islands or groups which are more than 300 miles from the nearest continent, from which they are separated by deep sea. Their distinguishing character is, that they possess neither terrestrial mammals nor batrachians, and rarely any reptiles, and that a large proportion of their animal and vegetable inhabitants are quite peculiar to them, although allied to those of the nearest continent. Mr. Darwin was the first to maintain that all such islands had derived their organic productions by migrations from the adjacent land, rather than by union with it; and the facts and arguments he has adduced have convinced most naturalists of the justness of this view. It is powerfully supported by the fact that a connection can in almost every case be traced between the adaptability for migration of a group of animals or plants, and the amount of distinctness of the island species of that group from their allies elsewhere. Thus the land-shells of Madeira are the most peculiar of all its productions, the beetles and the plants less so, while the birds (some of which still come over annually from the continent) are almost all of European species.

The Azores, being about a thousand miles from the nearest coast of Europe, and being separated from it by a profoundly deep ocean, are pre-eminently oceanic islands, and an especial interest therefore attaches to their natural history. The facts, however, are different from what would have been expected, since some of the most striking peculiarities of such islands are far less manifested here than in others much nearer to the mainland; yet on that very account they offer a most convincing illustration of the truth of Mr. Darwin's view, since the cause of their deviation can be detected. Although so much farther from the mainland than the Galapagos or the Madeiras, they possess a far smaller proportion of endemic forms, either of animals or plants; about eighty or ninety per cent. being European species, except in the case of the land shells, where only forty per cent. are European. The explanation of this anomaly is to be found in the fact that the islands are situated in an exceptionally stormy region, gales of wind from every point of the compass being very frequent. As a result, strange land birds of many species appear after these storms; and we cannot doubt that winged insects and the seeds of plants also arrive, although these pass unnoticed. Thus, although these islands may have been isolated quite as long as the Madeiras, their productions being continually crossed by fresh arrivals from the Continent, have not been able to become as much modified by local influences. It is a most interesting fact that the Galapagos, whose productions are so highly peculiar, are situated in an exceptionally calm region. No emigrant land birds are known to visit them, and the result is, that the few wanderers who, by some strange accident reached their shores in the distant past (when circumstances may have been more conducive to such emigration), have been isolated ever since, and have thus had time to become modified into very distinct species.

Mr. Godman's volume consists of a brief account of his journey, of carefully compiled lists, with critical remarks on all the chief groups of terrestrial animals and plants, and of a well-written summary of results. He has obtained the assistance of Mr. Crotch for the beetles, of Mr. Tristram for the land shells, of Mr. H. C. Watson, who has given a most elaborate critical review of all that is known of the flowering plants and ferns, and of Mr. W. Mitten for the mosses and hepaticæ. There are two useful maps of the islands, and the relations of the several species to those of the Atlantic Islands, Europe, and America, are fully pointed out. In conclusion we may notice that there is a full index, that the type and arrangement are very clear, and that the book is issued with cut edges; and we may congratulate the author on having given us more useful matter in a small compass and in a convenient form, than is often to be found in works of much higher pretensions and at ten times the cost.

W.

OUR BOOK SHELF

Hardwicke's Science-Gossip. An Illustrated Medium of Interchange and Gossip for Students and Lovers of Nature. Edited by M. C. Cooke, M.A. (London: Hardwicke, 1871.)

THIS is the sixth volume of a magazine which may be said to fill in scientific literature very much the position which *Notes and Queries* takes in the literary world. The two resemble each other, indeed, in many particulars, and in none more than in the very unequal value which attaches to the articles contained in their pages. There can be no doubt that *Science-Gossip* has fulfilled its object in becoming "a medium of interchange and gossip;" the large number of writers who discuss a yet larger number of subjects in its columns give evidence of this.

As an example of the best of the papers in the present volume, we would refer to one entitled "The Towing-net," by Major Holland, which appeared in the September number, which is pleasantly written, well illustrated, and thoroughly correct in its details—although somewhat marred by an unfortunate mislettering of the engravings. It is evidently written from personal and practical experience, and is just the paper to awaken a taste for marine studies in any one who has time and opportunity to devote to them. Mr. Taylor's geological papers demand a word of praise; and a long account of a "formicary" in the November number will be read with interest. Mr. Harting and Mr. Robert Holland contribute respectively useful ornithological and botanical articles; and microscopy is well represented. In some of the papers, however, "gossip" appears to take precedence of "science," and thus we find a lady correspondent expressing her wonder "whether flowers suffer pain," and writing a paper of nearly two columns on "errors of the press;" which may be amusing, but certainly cannot by any effort of the imagination be called scientific. On the whole, however, the volume is a satisfactory one, the illustrations being especially well executed; and we have no doubt that it exercises a beneficial influence upon amateur naturalists. We should be glad to see the rule which obtains in *Notes and Queries*—that correspondents replying to queries should refer to volume and page where such questions are to be found—enforced by the editor of *Science-Gossip;* the convenience of reference to a correspondence on any particular subject would thus be much augmented.

Etudes faites dans la collection de l'Ecole des Mines sur des Fossiles noveaux ou mal connus. Premier fascicule. Mollusques Tertiaires. Par F. Bayan, &c. 4to., pp. 81, 10 plates. (Paris: F. Savy, 1870. London: Williams and Norgate.)

M. BAYAN, who occupies the office of Engineer of Bridges and Roads in connection with the Ecole des Mines, presents us in the work before us with descriptions of 47 genera and 106 species, illustrated by 139 well-executed figures of Fossil Tertiary Mollusca. The localities represented comprise the *Calcaire Grossier*, &c. (Eocene), of Longpont, St. Parres, Rilly, Roncã, Parnes, Aizy, Grignon, Anvers, Chaumont, and numerous other localities in France, Belgium, Italy, and Algeria. This is the author's second work, but out of the 106 species, upwards of 80 are christened by M. Bayan; which implies that either he has in the course of his researches come upon an unusually large number of new forms, or, not having been able to refer his new examples to the species already described by M. Deshayes and others, on account of their imperfect condition, he has preferred the easier method of giving them new names. Thus we find fifteen new Tertiary species of the genus *Cerithium* added to our already overburdened nomenclature, one half of which, at least, might have been referred to well-known species. This is all the more surprising as M. Deshayes' grand collection is now deposited in the Museum of the Ecole des Mines, besides numerous other well-known typical collections. The book is written and transferred to stone by a new auto-lithographic process, the result of which is that there are forty-seven more or less important errata given, which the reader must correct before he can use the book with safety. We are sorry to be unable to avoid what may appear a harsh criticism of M. Bayan's work, but those only know the labour which such monographs cause who require to use them as works of reference in the scientific determination of fossils. We are arrived at a period in palæozoology when we cannot be satisfied with merely getting a name to a fossil, but *we must have the right name*—that which expresses its identity with, or distinctness from, other nearly-allied forms obtained from other similar or different geological horizons around; so that as our work progresses we find we are helping to reconstruct a history of these old marine faunas, as a whole; not a series of disjointed essays. We want to see the work of Professors Beyrich and von Koenen in Germany, fitted in with that of M. Nyst in Belgium, and these with our countrymen Messrs. S. V. Wood and Frederick E. Edwards, joined to M. Deshayes' grand work on the Paris Basin; these again with Michelotti, Bellardi, and other Italian works, joined to Hörnes's great work on the Vienna Basin. By no other plan can we hope to arrive at a clear notion of the value of terms applied to geological horizons, and the names of the species by which they are characterised.

H. W.

LETTERS TO THE EDITOR

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

Scientific Instruction in Elementary Schools

WHAT is to be brought under the new Act in our elementary schools? The never-ending permutations and combinations of the three R.'s, attendant on Mr. Lowe's Revised Code of Education, will, doubtless, soon be at an end, or at least limited in number. What improvement will come? What encouragement will Government give to Science teaching? Under the Revised Code it well-nigh disappeared, or, if it lingered on in some few spots, became almost worthless, *per se*, owing to its necessarily disconnected and unsystematic nature. We say it *disappeared*, which implies that it once had a footing; it certainly had, and was to some considerable extent followed out in very many of our elementary schools. The Committee of Council encouraged it, not only by simply recognising it, which they have not done of late years, but by making special reduced rates to assist the teacher in experimental lessons. Ten or twelve years ago educational periodicals teemed with hints on the subject, and specimen lessons were frequently inserted at full length; books for the use of teachers were written by scientific men; the teaching of "common things," though not altogether scientific in its way, yet showed the general opinion of competent persons in the matter. Why has all this been allowed to die out? We know schools at the present time where the apparatus liberally granted by Government, in days long gone by, has been carefully locked up in the cabinet for years, waiting for more enlightened times to return. We do not say that the Committee of Council on Education has positively prohibited all scientific instruction in our elementary schools; of course, they have done no such thing directly, but, indirectly, they have prevented it:—(1) by not recognising it as formerly; (2) by discontinuing grants of apparatus; (3) by making the examination in reading, writing, and arithmetic so rigid as virtually to confine the attention of the master to these three subjects. The examination of the boys in these schools has, in fact, been proportionally much more severe than that of candidates for the Civil Service, and at the same time more so than that of the pupil teacher placed over them. Hence the teacher has hardly dared to venture on giving time to other subjects with the value of which he was at the same time well acquainted.

Our middle-class schools, as well as those of a still higher grade, are gradually becoming more and more convinced of the value of a scientific education, and we must not lose sight of it in our elementary schools. It is even more important there than in the others; there the "working man" receives his education, and if our artisans are to become really intelligent men, fitted to compete in their various branches of labour with their fellow workmen on the Continent, we must prepare them for it in early life, and they are to be found as children, not in such places as Eton, Harrow, or Marlborough, but in our National and British schools. Education, we are told, is to be extended and improved; how can it be better improved than by the increase of scientific instruction? Any elementary schoolmaster will tell us that the general education given in our schools now is far below what it was before Mr. Lowe introduced his changes. The three R's are, no doubt, better taught in many instances, but the intellectual powers of the children have not been drawn out and properly cultivated; we question whether in many cases they have not been stunted in their growth. The same three lessons morning and afternoon, day after day, for twelve months, until H. M. I. comes round again—what has it been to both teacher and children, but unwelcome drudgery? We do not think the Educational Department need feel the slightest apprehension that ordinary subjects of instruction would suffer by restoring the former state of affairs (though it is to be hoped they will go further than that); there can be no doubt that an extra hour or two during the week set apart for science lessons would be not only no loss of time but a positive gain in many respects, by enlarging the intellect of the children, and enabling them better both to apprehend and comprehend what is placed before them in other subjects. It is a great relief to teacher and children to turn for even half an hour a week from the usual monotonous course to some subject totally different, both in its nature and in the mode of treatment. We have seen the faces brighten up, and the eyes sparkle again at the preparations made for an experimental lesson in physical science, or for one in natural history.

We hope, then, to see a change for the better in this respect; we trust that the almost forgotten apparatus and the rolled up diagrams will once more see the light of day, and be restored to their legitimate use. As to the plans to be adopted, each teacher is the best judge, perhaps, in his own school; he knows how may hours per week can be spared from other subjects; but two branches of natural science at least should be taken up, one of which should be natural history, as at once the most interesting, the best adapted for cultivating the powers of close observation and discrimination, and as a study which can be most easily followed up by the pupil himself. But every boy in our elementary schools ought to have a fair knowledge of the laws of heat, of sound, of fluids, of health and ventilation, of mechanics; of the outlines of electricity, the chemistry of the commoner elements and their combinations, *e.g.* oxygen, hydrogen, nitrogen, carbonic acid, atmospheric air, coal gas, &c. &c.

The branches to be taught might vary in different localities, according to the nature and productions of the district, and the industrial occupations of the inhabitants, *e.g.*, in mining districts a certain amount of practical geology should be taught, together with a course of lessons on the minerals obtained there; in coal districts, the nature of the coal, the formations in which it is found, the principles of mining, the nature and properties of carbonic acid, light and heavy carburetted hydrogen, coal-gas, the principles of ventilation, especially in reference to mines; also to some extent lessons on flame and heat, the "Davy" lamp, and others used in the mines, &c. In manufacturing districts, similarly, a practica course on mechanics and steam might be followed out, and a certain amount of chemistry with regard to the operations performed such as bleaching. While in agricultural districts much might be done towards making the field labourer a more intelligent being than he is usually supposed to be, by giving lessons in agricultural chemistry, the art of draining, the laws of evaporation and condensation, the value of woodland, the construction of various implements used, &c. But in every locality lessons should be given in physiology, health, and ventilation, while natural history would everywhere form a not less pleasing than instructive alternative subject. The walls of every school, upper, middle, or elementary, should be covered with diagrams, and at the foot of these should be descriptive and explanatory notes.

As regards the teachers themselves, it may be left an open question as to whether they should be required to go through the ordeal of an examination in the subjects they take up, or whether it may be taken for granted that the mental discipline undergone for two years in the training college is sufficient to enable them to perfect themselves for the task. Those among them who have been at work for ten or fifteen years might probably feel indisposed to prepare again for examination; and we are ready to admit that if a man can bring up his pupils to the required standard, he may well be excused from any other test. At any rate it is to be hoped that the subject of scientific instruction in our elementary schools will receive that attention from Government which is due to it, and that having been once again taken up, it will never be allowed to fall into desuetude.

HENRY ULLYETT,
Hon. Sec. Folkestone Nat. Hist. Soc.
National School, Folkestone

The Prevalence of West Winds

IN NATURE of 29th September, 1870, there is an abstract of a paper read at the British Association by Mr. Laughton, maintaining that the preponderance of west over east winds over the entire globe is such as to point to a cosmical force of some kind, moving the atmosphere round the earth from west to east; and concluding "that the motive force for which we are seeking, is really the disturbing force of the attraction of the heavenly bodies."

The course here suggested, if it were operative at all, would act in the opposite direction to that required. The only possible effect of the attraction of the heavenly bodies on the atmosphere is an atmospheric tide, and the motion of a tide is necessarily from east to west, with the apparent diurnal motion of the heavens. But it is only when resisted that a tidal wave can give origin to currents, and it is scarcely probable that any tidal current can be formed in the atmosphere of sufficient force to blow a feather. I maintain the truth of the ordinary theory, that all winds originate in a disturbance of atmospheric equilibrium by unequal solar heating in different parts of the earth, but are changed in direction by the earth's rotation. This theory is quite consistent with the fact on which Mr. Laughton insists, that west winds preponderate over east winds. It is true that the mechanical effect of any wind whatever must be balanced by an equal mechanical effect in the opposite direction. Mr. Laughton appears to see that this would necessarily be the case if the ordinary theory of the winds is true, but he does not see how the compensation is effected. Were the winds all from the west, they would in an infinitesimal degree accelerate the earth's rotation; were they all from the east, they would in an infinitesimal degree retard it: and if either of these effects were proved to take place, the inference would be certain that the winds are set in motion by some other agency than unequal heating in different places; because, in virtue of the universally true principle of the "conservation of rotation," no such motive power could have any effect on the earth's rotation whatever.

The unbalanced effect of any wind on the earth's rotation will be due to the product of three factors, namely, the area covered by the wind, the east or west component of its force, and the radius of the parallel of latitude at the place. Mr. Laughton has probably overlooked the last-mentioned factor. It gives *leverage*. An east wind near the equator has more effect in retarding the rotation of the earth than a west wind of equal extent and force at a higher latitude has in accelerating it, just as a weight at the end of the long arm of a lever outweighs an equal weight at the end of the short arm. It is for this reason that the west winds, which are mostly in the higher latitudes, are of greater force, and probably cover a greater area than the east winds, which, under the name of the trade-winds, predominate near the equator. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, Jan. 25

Can Weather be Influenced by Artificial Means?

SOME remarks on this subject made by the Rev. R. B. Belcher before the Geological Section of the British Association (reported in the *Athenæum* of October 29), reopen a question of popular meteorology, which has not, perhaps, been sufficiently attended to, from an exact and scientific point of view. Such evidence on the subject, as is at present available to the public, is too general to have much claim to correctness of detail on points which require particular and *local* information; and I offer the following abstract of it principally in the hope that it may lead to further inquiry and observation.

The idea that large fires do, in some way, bring on rain, is very old; but it was, I believe, for the first time stated as a fact and explained on scientific grounds by the late Professor Espy. His theory is, that the heating of the air causes a rapidly ascending current, and that the moisture which air near the surface always contains, is thus carried into the upper regions of the atmosphere to be condensed and to fall as rain. In support of this view, he has given several instances in which rain did immediately follow the kindling of a fire, when no clouds had previously been visible, but in a problem of this nature, negative examples have more weight than positive; and it is necessary to admit that, though in some very remarkable instances rain has followed a large fire, in other instances, quite as remarkable, there is no notice of rain. It is, of course, difficult to speak with absolute certainty; it may almost always be said that a few drops have fallen, sufficient to bear out the truth of Espy's theory, but it seems to me that to establish it in its entirety, something more than this is necessary, and that an extraordinary fire ought to produce a very decided shower, if not a heavy downpour. Last summer, in July, a large fire raged for several days on the moorland and through the pine copses a few miles to the east of Wimborne-Minster. The weather was hot and dry. I was myself on a walking tour in that part of the country, and passed close by this fire, near the village of Longham, where it had been burning with great fury for a week. The farmers were all complaining of the long and excessive drought, and I can give my personal testimony to the fact that there was absolutely no rain. Similarly in the very dry summer of '68, the papers were for some weeks filled with accounts of fires raging fiercely in various parts of Wales and Scotland; but there was no word of any break in the drought. It is only by referring to exceptional and recent examples of this kind that we can feel any degree of certainty; wet weather and heavy rain are so common in this country, that under circumstances not very remarkable, they would scarcely be noticed; and these instances I have just mentioned show the necessity for caution before attributing to fire and consequent rain the relationship of cause and effect; and, as bearing directly on Espy's theory, I may add that there are no observations which would show that the rainfall in our large towns is greater than in the adjacent country; yet the thousands of fires in London, for example, must heat the air sufficiently to cause some ascensional tendency.

The evidence regarding the effect of violent explosions is in very much the same state of imperfection. Theoretically, we can admit that a violent shock may throw large masses of the lower air to a considerable height, so as to cause condensation of its vapour; and there are many instances which seem to support the conclusion that it does do so. The clearest on record are perhaps those mentioned by Dr. Darwin, in his Journal of Researches, where he describes how on several occasions a shower of rain fell, in the middle of the *dry* season, in Chili and other parts of South America, after an earthquake. After the earthquake in Peru, in the latter part of the year 1868, I went carefully through the published accounts, but found no mention of any similar showers. In our own climate, in which rain is not uncommon at any time of the year, the circumstances require to be examined into more critically, and the evidence afforded by them can never be quite so satisfactory. Whitaker's Almanack for last year, in the list of Remarkable Occurrences, &c., for 1869, mentions several serious explosions of powder mills, collieries, &c., but not one of them is followed by any notice of remarkable weather, storm, or rain; still, as one of these is the conflagration at Bordeaux on September 28th, which, as Mr. Belcher has pointed out, was followed by very heavy gales, both in the Bay of Biscay and in the English Channel, it is likely enough that heavy weather following some of the other accidents may have been omitted, or have been insufficient to call for special mention in such a general list.

The effect of great battles, again, has always been a favourite theme amongst popular meteorologists; but in regard to this also there is the same uncertainty. Several battles and bombardments have been followed by storms; but, on the other hand, several have not. The general opinion amongst naval men is that heavy firing beats down the wind and produces a calm; and it is tolerably certain that this is frequently the case, more especially if the wind is light. Whether it does or not, the sudden irruption of millions of cubic feet of gas into the lowest strata of the atmosphere and within a very limited area, must have a tendency to cause disturbance, a tendency increased by the undulatory movement due to the noise of the guns. The battle of Trafalgar (October 21st) was fought in a very light westerly breeze, which, towards the afternoon, almost entirely died away. During the night the wind gradually freshened, and by noon the next day blew very hard. Lord Collingwood in his despatch, dated the 22nd, says it had "blown a gale of wind ever since the action;" but that this is merely a general way of speaking is seen by a reference to the logs of the various ships of the fleet. (Despatches of Lord Nelson, vol. vii., pp. 159, *et seq.*) A westerly gale, on the coast of Portugal or Spain, is not such an unusual occurrence in the end of October, as to compel us to attribute one to a battle which took place twenty hours before it came on. Again, the battle of St. Vincent, of the Nile, Rodney's action off St. Domingo, and many others, do not seem to have been followed by any interruption to the usual fine weather. Quatre-bras was followed by heavy rain; but the night after Waterloo was fine, and the bright moonlight is specially mentioned as advantageous to the pursuers; and though Mr. Belcher speaks of rain as having set in in the east of France during the present campaign, I have not seen any notice of its following the battles of Gravelotte or Sedan, so as to be clearly referable to them.

What appears to me the only explanation of the apparent contradictions in the evidence is this; that large fires, explosions, battles, and earthquakes, do tend to cause atmospheric disturbance, and especially, to induce a fall of rain; but that for the tendency to produce effect, it is necessary that other conditions should be suitable; that rain does not follow, unless the lower air contain a great deal of moisture; and that therefore the ascensional movement does not reach to a height such as we might at first be led to conceive; that, in fact, the height is for the most part very trifling. With regard to storms said to have been caused by some of these agencies, the evidence is still more unsatisfactory; and, in our present ignorance of the cause of storms generally, is quite insufficient to compel us to attribute any one particular gale, extending probably over a wide area, to some very limited and comparatively insignificant disturbance.

J. K. LAUGHTON

Natural Science at Cambridge

THE rejoinder in your last number (p. 287) to my inquiries is rather ingenious than ingenuous. "The Writer of the Article in Question" vouchsafes no answer to my very plain questions (p. 264), but is kind enough, instead of justifying his own statement, to propound sundry suggestions, surmises, and what not. I simply asked for confirmation of a certain assertion. This he avoids giving, and, though I regret the cause, I do not wonder at it. Some persons may think it would have been better taste to have acknowledged that the assertion was unfounded. Perhaps your correspondent is of another opinion; if so, I do not wish to interfere with him. I have only to remark that substantially he admits the force of the doubt I expressed by now modifying his previous statement as to "most of the colleges" into "some colleges"—perhaps "one college" would have been nearer the mark, but I will let that pass, hoping that when he again writes on the subject, events may have proved him to be a true prophet.

Cambridge, Feb. 10 M. A.

Glass Globes

IN consequence of the letter of Col. Greenwood, which appeared in NATURE of the 1st December last (No. 57, p. 87), I wrote to Dr. G. O. Sars, of Christiania, who is the Inspector of Sea-fisheries in Norway, for information on the subject. In his reply, just received, he says he was not aware that such globes were used as net floats in any other country than Norway, and that until quite recently they had been used there in the great cod fisheries at Lofoten only. At Söndmör, in Christiansand, they had begun to come into use in 1869, but by no means generally

He has no doubt that the floats washed ashore in Shetland and the Hebrides came from Lofoten; but he remarks that their course in a south-westerly direction is quite contrary to what might be expected from the action of the equatorial current; and he suggests that before reaching our northern coasts the floats made a very long course, having been first carried northwards until they got within the range of the Polar stream, by which they may have been carried southwards along the coast of Greenland, and so at last coming within the influence of the Gulf-stream, they were carried eastward across the Atlantic, and thence drifted by south-westerly winds to their destination. He adds that the fact of their having been found on the *west* coast of the Isle of Lewis seems to confirm this hypothesis.

Feb. 13 J. GWYN JEFFREYS

P.S. on another subject. The correspondents in recent numbers of NATURE do not appear to have been aware of Dr. Carpenter's and my remarks in the *Athenæum* of the 22nd of October last on the hypothesis of the Cretaceous formation being continued or not continued up the present time.

The Primary Colours

HERE is another proof that violet is the third primary. Prof. Tyndall, with that sagacity which results from the right use of his imagination, has given us a reason for the blue colour of the sky. It is probably the true reason, and it proves that blue is not a primary colour. The smallest light-waves are those of the extreme violet. These, therefore, are the waves which will be reflected in greatest proportion by the minute particles of foreign matter in the air. Why is not the sky violet then, instead of blue? Plainly, because although violet predominates in the reflected light, there is also a small proportion of green and a still smaller proportion of red. Suppose the ratios to be 1 red, 3 green, 6 violet, and suppose the ratios for white light to be 1 red, 2 green, 4 violet. Then, the light of the sky will be white *plus* 1 green, 2 violet, which is blue. In Professor Tyndall's experiment on the decomposition of sulphurous acid gas by a beam from the electric lamp, he tells us that, as the particles of the sulphur cloud grow larger, the colour changes from pale blue to deeper blue, and then to whitish blue and white.

No colour but blue makes its appearance, because there is always some green and red, as well as violet; and always a surplus of green *over* red, more than enough for white light.

Leicester, Jan. 28 FREDERICK T. MOTT

Yellow

IT would seem to me that the great difficulty of conceiving yellow as a compound colour is the brightness or lightness of yellow, as compared with its components. In the spectrum, we have the maximum of light in the yellow, and it is against our experience to put two dark colours together and form one light one, as, for example, to put the red and green together and form yellow. But there is just the same difficulty in the production of white light from all the colours of the spectrum, for we regard white as lighter than any of the colours, even than yellow. This, however, is a mental fallacy, and if once exposed, seems to me to do away with the difficulty of conceiving white as made up of a series of colours. Our ideas of colours and tints are derived from our own experience, and we produce tints by precisely the opposite method to that of nature. Nature's method of painting a blade of grass is, to throw on it all the colours of the spectrum, and afterwards pick out those which shall have a *residue* of green. The artist's method is to see what colours will produce green and then lay those on. I do not think it too much to say that all colours are merely tints of white—that, for example, yellow is really yellowish white, green, greenish white, and so on. If we take two cards, one green, the other red, and hold them so that the light is properly reflected from the one on to the other, the cards do not appear black but white. If the red and green were pure colours, the cards should of course appear black, for the light from the green card would be totally absorbed by the red card, and similarly with the red card. The white cannot be produced by a combination of the red and green, but by the extinction on one card of the green that gives the greenish white, and on the other of the red which gives the reddish white. That yellow is but little different from white is well illustrated in the beautiful experiment of Newton's of synthesising the colours of the spectrum by reflection from seven moveable mirrors. With the mirrors placed at equal distances from each other, the spot of compound light is not white but yellow, that is to say, it is yellowish white, the colours lost between the mirrors being just those necessary to bring out the full white.

C. J. WOODWARD

Meteor

A FINE meteor was seen here to-night at about 9.10 P.M.; it was described to me as starting from near θ Orionis, and proceeding towards a point a little north of γ Eridani, when it was lost behind a belt of cloud. J. M. WILSON

Rugby, Feb. 13

Snake Bites

IN NATURE of Dec. 22 I notice a note extracted from the *Pall Mall Gazette*, giving a return of the excessive number of deaths which take place annually in the Bengal Presidency, from the effects of snake-bite. That 11,416 persons die from this cause alone, "and that no efficacious means are adopted to check its ravages," are very startling announcements, and strike me as being well worth the attention of the readers of NATURE.

Upwards of a year ago Dr. Fayrer recorded an elaborate series of experiments on snake poison, in the *Indian Medical Gazette*, from which he concludes "that if an animal, and probably a man, be fairly bitten by a fresh and vigorous cobra or daboia, it or he will inevitably succumb unless some immediate or direct method of arresting the entering of the poison into the circulation be practised." This direct method is to apply ligatures and cauterisation; for, says the same authority, "to conceive of an antidote, in the true sense of the term, to snake poison, one must imagine a substance so subtle as to follow, overtake, and neutralise the venom in the blood, or that shall have the power of counteracting and neutralising the deadly influence it has exerted on the vital forces." I remember reading some time ago of another doctor in India or Australia, who had tried ammonia as an antidote, but I cannot recollect with what result. It seems to me, however, that this real antidote has still to be found: and cauterisation, to prove effectual, must follow the course of the poison, which it cannot do; nor, indeed, is it possible for it to do much more than burn the walls of the wound, so it is not to be wondered at if some of those subjected to this powerful treatment do not recover.

I have long thought that the best cure for snake-bite would be powerful suction, applied to the wound by means of an instrument made for the purpose, and similar in principle to a boy's sucker. This would draw off a considerable quantity of the blood in the neighbourhood of the wound, and by so doing wash the poison out before it. Above the wound there might be a ligature applied, but sufficiently distant from it to ensure that the blood in the small vessels between the wound and ligature be competent to wash out the poison.

Where such an instrument is not at hand, and the assistance of a second party can be obtained, he might tie a ligature above the wound, and suck the latter with his mouth for a considerable time, spitting out all the saliva and blood which accumulates. It would be advisable, too, to make the wound a little larger before commencing to suck, with a sharp knife or otherwise, in order that the greater flow of blood may the better discharge the poison. The operation of sucking the poison into the mouth need not be feared, for even although a small portion of it were swallowed, it could do no harm. I believe I am correct in stating that a quantity of poison which will prove fatal on entering the circulation, would have no injurious effect when taken into the stomach. Where the bite is in such a part of the body that the party bitten can easily suck it himself, then he ought to do so: but unfortunately this is seldom the case, it usually being the lower extremities which are attacked.

In support of this method, I may say that I read, two or three years ago, an account of how the bites of snakes were counteracted in a woody portion of South America. The writer said when any one was bitten—and there were one or two almost daily—he was sure to die in thirty minutes to one hour afterwards if his wound was not immediately sucked. There, however, in order to make quite certain that the poison, when sucked out of the wound, would have no injurious effect on the sucker, the latter filled his mouth with olive oil before applying it to the wound: and I imagine this would be a sure precaution, for it provides a plentiful supply of matter for the poison to diffuse in, without interfering much with the absorbents of the mouth. The result of this writer's experience was that the bitten person seldom, if ever, died; and he who sucked the wound never felt an injurious effect.

This subject seems so important that I have ventured to address you at this length upon it, in the hope of drawing from some of your correspondents further details concerning antidotes and methods of curing snake-bite. T. L. PATTERSON

The Cretaceous Period

IN NATURE of Jan. 19, a letter appeared from Prof. Wyville Thomson defending the expressions, "we are still living in *the* Cretaceous epoch," "*the* chalk is being formed at present in the bed of the Atlantic." When first this announcement was made, it was followed up by various strong comments implying that the similarity of the Atlantic mud to the chalk in lithological character, and in many of the imbedded organisms * "would seem to unsettle much that has generally been accredited to geological science," would, in fact, revolutionise geological classification.

As these unfortunate expressions are again put forward, notwithstanding the protest of our most distinguished geologists,† Sir Roderick Murchison and Sir Charles Lyell,‡ it may be useful to consider what the question at issue really is. Simply stated, it is this: Have we sufficient evidence for drawing one of our strongest lines at the base of the tertiary deposits, and saying that it marks the commencement of a new epoch or period? §

In grouping the rocks, we have been obliged frequently to adopt an arbitrary classification. The thickness will not furnish the necessary tests, as accumulation is more rapid at one time and place than another. Lithological character will not do alone, as a bed often passes both vertically and horizontally into one quite different. The organic remains will not do alone, as the fauna migrated and re-migrated to suitable areas when favourable conditions recurred.

But sometimes we have the commencement of a new period well defined by seeing that the group of deposits which form the record of it rest unconformably on an older formation, which has been in part at least heaved up, denuded, and used to form the new series. At the bottom of this newer series we must infer a considerable break—a portion of unrepresented time.‖ This time may be represented elsewhere, but we have a point in time well marked by the first grain of the new deposit laid upon the older denuded rocks.

So we are quite safe in saying that there had been a considerable lapse of time, and that new conditions prevailed over large areas when the Cambrian was laid unconformably upon the Laurentian, when the Upper Old Red was laid unconformably on the Lower Old Red and Silurian, when the Permian was laid unconformably on the Carboniferous and more ancient rocks.

Probably deposition went on longer over one area than another—very likely deposition has never in the earth's true geological history been entirely arrested, so that the connecting deposits between any two formations and intermediate forms of life may possibly still be preserved under the depths of ocean or on the vast still unexplored continents. It would be of course difficult under such circumstances to identify in a series of more or less continuous deposits the base line we have so well marked elsewhere by visible unconformity; and this difficulty occurs in the older rocks, as, for instance, in the case of the base of the Upper Old Red or Carboniferous in South Wales, of the Permian when the Rotheliegende is present, and many others, but this arises from our want of data. We might fairly hope that if we could find the continuous deposits after enormous intervals represented by known unconformities, we should read a wonderful story, and know, for instance, more clearly by what variation of forms and invasion of stronger life from adjoining areas we are left at the end of a long period with an ammonite instead of a goniatite, or a nautilus instead of either.

Now, to return to the particular case under notice. Have we at the bottom of the Tertiary formations evidence of a break so large, of a lapse of unrepresented time so long, of a change in conditions over large areas so great that, we are justified in saying that this is a convenient place to draw one of our strong lines?

* Carpenter, Lecture Royal Institution, Ap. 1869.
† Pres. Add. Geog. Sect. Brit. Assoc., Liverpool, 1870.
‡ Students' Elements of Geology, 1871.
§ See also an able article by Mr. Green, *Geol. Mag.*, Jan. 1870.
‖ See Ramsay, Pres. Add. Geol. Soc., 1863-4.

The overlap and irregular occurrence of the Tertiary on various parts * of the Cretaceous deposits, the immense banks of flints containing Cretaceous fossils in the Tertiary beds, point to an enormous amount of change and denudation between the consolidation of the Cretaceous and accumulation of the Tertiary deposits. This is accompanied by an almost entire break in the higher forms of life.† It is true that the researches of Dr. Carpenter and his colleagues have brought to light many forms which have survived from the Cretaceous to our own time; but these discoveries are only of the same kind as the discovery in recent times of the genus Lingula, or of forms allied to Encrinites. When we trace back to a remote antiquity ferns and other plants not very unlike those of our own day, Crustacea differing but little from our King Crab, Paludinas hardly distinguishable from recent forms—that does not throw doubt upon the useful grouping of the rocks from carboniferous to recent times.

Species are continually being found common to two beds known to be separated by enormous intervals of time. Upon this fact Barrande founded his theory of the Colonies. But the classification into Mesozoic and Tertiary depends upon evidence that cannot be shaken by the discovery of a few more forms common to the two. The wonder always was that the break in life was so complete as it appeared to be at the close of the Cretaceous period, and the deep-sea dredging expeditions confirm what was *a priori* almost a necessary inference, that deep-sea conditions prevailed somewhere during the whole of the period from the Cretaceous age to our own, and that some forms of life have not been destroyed or developed into anything else during that period; but that is a very different thing from saying that there is not sufficient reason for holding that the base of the Tertiaries marks the commencement of a new epoch.

T. M'K. HUGHES

Insulation of St. Michael's Mount, Cornwall

HAVING read Mr. Peacock's letter in your publication of the 2nd inst., I beg, through the same medium, to show that his reasons for supposing "the mount could not have been an island in 1086" are groundless.

He begins by giving the measurement of "Domesday Book," date 1086, of "the Land of St. Michael," and afterwards writes as follows:—

"There is an entire absence in 'Domesday Book' of any mention of island or islands on any of the coasts of Cornwall. . . . In short, the mount could not have been an island in 1086, because it contained at least eight times as much land as it does at present, probably connecting it with the main land, from which it is even now only one-third of a mile distant. . . . The present area of the mount is only thirty acres, so that there are 210 acres missing . . . since 1086; and in 1099, thirteen years after, we have a record of a catastrophe which would fully account for the loss"—that being the great irruption of the sea in 1099, as recorded in the Anglo-Saxon Chronicle.

When yonr correspondent quoted the measurement of "The Land of St. Michael" above referred to, he evidently imagined St. Michael's Mount, with the Church or Monastery on its summit, to have been like a nobleman's seat in the midst of a large park, with the sea at a great distance from the centre—and all this to have been comprehended in "The Land of St. Michael." The fact, however, is that in 1086 the Mount was, as it still is, a rock about five furlongs in circumference at its base, and insulated by every tide, whilst the two parishes on the mainland nearest to it—viz., those of St Hilary and Perran-Uthnoe (which may be identical with "The Land of St. Michael"), were then holden by the Church or Monastery of the Mount."‡ As the mount, however, is now almost universally allowed to be the *Iktin* of Diodorus Siculus, we may be sure that it was long before the commencement of the Christian era insulated daily as it is at present. I have written very fully on this subject in my work already referred to, published in 1862, and also in a paper printed in the Transactions of the Plymouth Institution for 1867-68 (pp. 17-37), in both of which I have exposed the error of all the translators of Diodorus in calling the mount *Iktis* instead of *Iktin*, and have also shown that the Mount, which was called in the Cornish language *Bre*-tin ("Tin-*Mount*") as well as *Ik*-tin ("Tin-*Port*"), has given its name, not only to *Mount's Bay*, but probably also to the whole of *Britain*.

R. EDMONDS

Plymouth, February

P.S.—I had written the above before I saw Mr. H. Michell Whitley's letter in your last number, which states that instead of "*Keiwal holds the Church of St. Michael*," as Mr. Peacock has translated the passage in Domesday Book (p. 2), it should have been "*The Church of St. Michael holds Keiwal*" (or Treuthal, as it is also called on p. 11), which is the name of a manor in the parish of St. Hilary. This confirms what I have above written, although I have adopted a different way of disproving Mr. Peacock's theory.

* See Lyell, Student's Elements of Geol., pp. 258, 261, where attention is called to higher cretaceous beds than those on which the Tertiaries rest in England.

† Lyell, Op. cit. p. 256.

‡ See my "Land's End District—its Antiquities and Natural History," p. 166.

Aurora Borealis

A FINE Aurora was seen last night, or this morning, from 1 to 3 A.M. It first appeared as a *transverse* band from N.E. to S.W., and passed in that course far South of the Zenith, or between Arcturus and Mars. Subsequently it spread laterally and upwards; presently radiated from near the Zenith to all azimuths; and at 2.30 A.M. some of the rays N.E. were strongly pink.

In the spectroscope, the usual green line was gloriously bright. I saw it first, with a hand spectroscope, in the darkened light of the rough glass panels of a stair door. There were also faint lines more refrangible over the regions of E, b, and F. Rather to my astonishment, I was totally unable to see a red line, even when looking at rays abundantly pink to the naked eye. This was a disappointment, to say the least of it, because I had prepared, and had in the lower part of the field of view, red chemical lines to compare with anything red that should appear in the Aurora; and I had seen the red line perfectly well in the fine auroras of last autumn, but then I had no such checks on its place.

However, my spectroscope is still a very rough, home-made affair; and I am living in hopes of something better when Government supplies this Observatory at last with its long-desired, long-delayed equatorial.

Royal Observatory, Edinburgh, Feb. 13 C. P. S.

THE THEORY OF GLACIAL MOTION*

MR. CROLL'S papers on Ocean Currents are a powerful application of the modern theory of heat and force, to show the fallacy of Captain Maury's explanation of the causes of oceanic circulation. They also discuss other matters of great interest, but as the concluding part is not yet published, we shall say no more about them at present, but that they well deserve careful study.

The other paper is a criticism of the Rev. Canon Moseley's supposed proof that glaciers do not descend by the force of gravity, and of the arguments of Messrs. Ball and Matthews on the other side. It will be remembered, that Canon Moseley determined by experiment the "shearing" force of ice, that is, the force required to fracture it by parallel pressure. A plug of ice of known cross-section is fitted into a hole through two smooth boards, and the force required to break the ice by sliding the boards over each other is the "shearing" force. Increasing this in proportion to the dimensions of a glacier, or of any large portion of one, it was calculated that the force required to cause the different parts of a glacier to slide over each other (as they must do in descending a valley of constantly varying form and size) was at least thirty times greater than the force of gravity on a slope such as glaciers easily descend. Canon Moseley came to the conclusion that expansion and contraction of the ice by heat and cold was the moving power; and the fact that the glaciers move slower by night than by day, and in winter than in summer, was supposed to prove conclusively that heat is the cause of motion.

Mr. Croll believes that Canon Moseley has demonstrated that gravity alone does not cause glaciers to descend, but he completely demolishes the theory of contraction and expansion. He admits that heat aids the motion, but maintains that it does so by acting on the molecules of

* "On Ocean Currents." By James Croll, of the Geological Survey of Scotland (3 parts). "On the Cause of the Motion of Glaciers." By the same author (Extracted from the *Philosophical Magazine* of 1870.)

the ice, which it loosens momentarily from their mutual cohesion, and allows to be re-arranged under the influence of gravity. Heat, he says, is the *condition*, gravity the *cause* of the motion which takes place, molecule by molecule rather than in masses. It seems very doubtful, however, if this theory is more tenable than the one it is intended to supersede. If heat entering the glacier loosens the molecules in its passage and enables them to move insensibly into new positions, it is difficult to understand what causes the numerous longitudinal and transverse fissures of a glacier, the production of which is often attended by loud reports, and which indicate movements of masses, not of molecules. And how could molecular motion lead to that heavy grinding of the ice over its bed, which scores and wears down the hardest rocks, and whitens great rivers with the finely triturated mud?

None of the opponents of Canon Moseley have noticed what seems to the present writer to be a radical fallacy in his argument about "shearing force." He assumes that, whatever the bulk or weight of the glacier, or of any portion of it to which the formula of the shearing force may be applied, the whole mass shears at once by the action of gravity on the same mass, and does not recognise the possibility of one portion of a glacier acting by its weight to shear another and much smaller portion. But this must inevitably occur; for, owing to the excessive irregularity of the bed in which every glacier moves, the mass must be every where in varying states of tension and compression, and must contain at each instant certain lines and planes of least resistance, the extent of which lines and surfaces may be very small compared with the dimensions of the glacier itself. At any moment, therefore, the whole descending weight of a portion of the glacier containing perhaps thousands of cubic yards of ice, may act so as to cause the shearing of a few superficial feet where the tension is greatest. This being effected, a partial equilibrium is produced there; but the points or surfaces of greatest tension are shifted, and another small shear or fracture occurs; and by this process and the continued regelation of fractured surfaces brought into contact, it may easily be seen that the glacier as a whole would be gradually moulded to its bed, which it would descend as surely as if it were a viscous mass. Another source of motion not taken into account either by Canon Moseley or Mr. Croll is the irregular melting away of the under surface of the glacier by terrestrial heat, which would often form unsupported hollows till a fracture occurred, and every such fracture must result in a downward motion of a portion of the glacier. The observed difference of the rate of motion between winter and summer, day and night, is more probably due to the different quantities of water which descend the crevasses into the bed of the glacier at those periods, than to any direct action of the heat. It is well known that in the higher portions of a glacier the supply of water from melting snow diminishes during the night, as it does in a still greater degree during the winter; and the large quantity of water that flows beneath every glacier in the summer must greatly assist its motion, both by melting away its lower surface, and by, to some extent, buoying it up.

Mr. Matthews's important experiment of the bar of ice which gradually curved by its own weight, should be tried again in an atmosphere kept at the freezing point. This would settle the question whether heat is an essential condition for the curvature or motion of ice by gravitation; but so far as the facts lead us at present, the arguments of Canon Moseley and Mr. Croll by no means *prove* that glaciers do not descend by the force of gravity alone.

ALFRED R. WALLACE

[The publication of this article has been delayed. It was in our hands before the appearance of Mr. Ball's paper in the *Philosophical Magazine* for February, where a view almost identical with Mr. Wallace's is ably advocated.—ED.]

AN ACCOUNT OF THE ECLIPSE AS SEEN FROM VILLASMUNDA BY AN UNSCIENTIFIC OBSERVER

THOSE set in authority over the branch of the Eclipse Expedition stationed at Agosta having decided against depending only upon observations to be made from the Observatory there, deputed Mr. Ranyard to proceed to another point upon the line of totality, and selected me as his coadjutor. Accordingly we set off, accompanied by Jarvis and Burgoyne, two of Colonel Porter's Sappers, at half-past nine in the morning of the eventful day; and, after driving some eight miles inland, we attained about eleven o'clock a point which appeared to my companion to present advantages for our object. Leaving the road, we went into the middle of a field of springing oats, on the highest point of a rocky ridge at an elevation of 600ft. above sea level, and of 520ft. above the *glacis* of Fort Agosta, where were posted the rest of our friends. The spot which Mr. Ranyard selected as the most suitable lay about a hundred

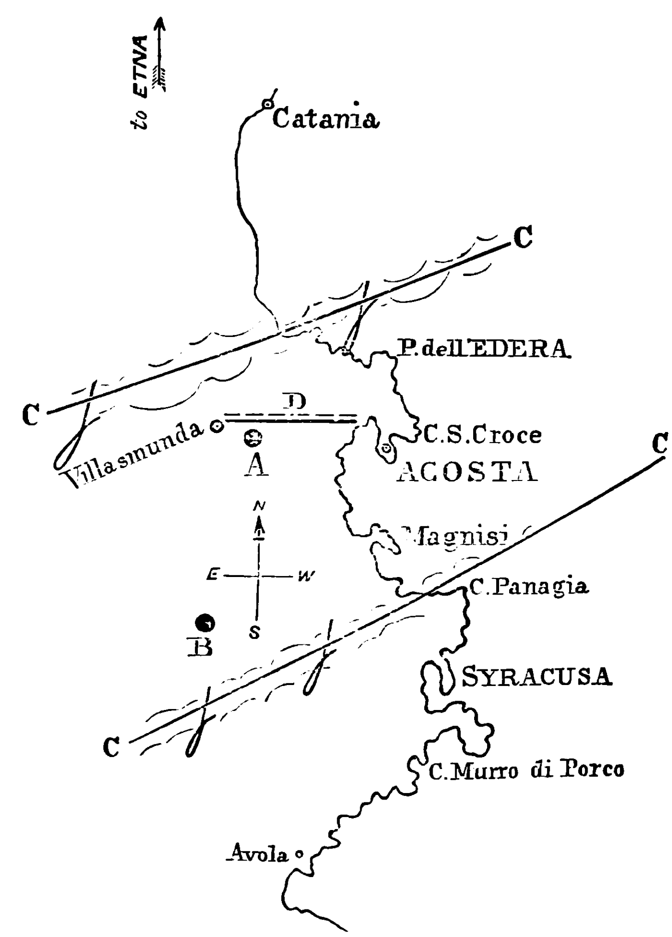

FIG. 1.
A our position; B the sun; CC, CC the lines of cloud; D the road to Agosta.

yards from a roadside farmhouse, called Casa Vecchia, upon the property of that friend of Science, the Marchese di Sangiuliano, and about two miles distant from the village of Villasmunda. A keen wind was blowing with considerable violence from the north-west, and the situation we had chosen being exposed to its full fury, we at first felt very uneasy with regard to our probable success, for we feared every moment that the telescope would be overturned and injured. A happy thought, however, soon extricated us from our dilemma. Causing our luckless coachman (who wept true Sicilian tears over the imaginary danger to his springless vehicle) to drive it, in the cause of Science, over the rock-sprinkled field, we utilised our carriage as a temporary shelter for the precious instrument, and were ready some time before the first contact took place. During the time occupied in perfecting the necessary preliminaries, I noted the position and the structure of the cloud-banks which were instilling into our minds feelings of the keenest anxiety. We were standing in the centre of what I may describe as a comparatively cloudless longitudinal "slit" in the sky, which was otherwise completely covered; so that, while over our heads the sun was shining brightly, its refulgence obscured only occasionally by light, fleecy, flying clouds; to our front and rear were lying parallel lines of heavily-banked "cumuli-strata" running from south-west outh-east. Perhaps the accompanying rough diagram (Fig. 1) may serve to illustrate their position in relation to our own.

I also set down the following readings of the barometer and thermometer (wind N.N.W.):—

	At first contact.			Five minutes before totality.	At 2.20 P.M.
Barometer	28·65	28·79	28·88	28·80	28·75

At first contact.

Thermometer in shade	56°
" in sun exposed to wind	54°
" in sun	58°

Immediately after totality.

Thermometer in sun exposed to wind	55°

Two minutes before the commencement of totality, the clouds behind us, and those in front of us, were black and threateningly lowering, as if a thunderstorm were imminent. Etna, which lay well in our view to the N.N.W., was clothed to its very base with a shroud of the most sombre clouds, while as the seconds flew by the temperature fell sensibly lower and lower.

To the S., and S.W. also, the sky was filled by a strange, steamy, V-shaped (the point of the V being near the earth), filmy mist, through which the sun showed such a feeble and uncertain light, as to give me cause for fear lest our observations should be brought to an abrupt and resultless termination by the total disappearance of the sun behind this curious veil. *Immediately* before totality commenced, a dark vaporous shadow glided very swiftly up over the heavens from the westward, or a little south of west, and, as it came on towards us, seemed to swallow up the earth, leaving it dark in its rear, until at the moment of totality it reached the sun. As it drew near him, a herd of oxen feeding behind us, with one or two exceptions, lay down. With the beginning of totality the air was colder than ever, and for about one minute's space, not more, there fell a small thin rain, which I fancy must have been the result of the condensation of the steamy mist which I have a little while ago described. Of totality itself, as a spectacle, I am almost afraid to speak. To endeavour to describe the inconceivable grandeur of the sight would be a hopeless task. I can only say that nothing will ever efface it from my memory. But if I cannot hope to give you any idea of the sublimity of the scene, at any rate I will do my best to state simply the appearances which I saw. Round the dark moon gleamed the luminous circle of the corona, shining with about half the radiance of an English sun upon a winter's afternoon; while there streamed forth from it in eight directions as many sets of brilliant rays. These "sets" of rays were composed of four sets in the position of a Greek cross, as in Fig. 2; with a St. Andrew's cross, as in Fig. 3, placed upon it, forming something similar to Fig. 4, Fig. 3 extending only about half as far as Fig. 2, which reached as far outwards as the apparent diameter of the sun.

I described the phenomenon to Mr. Ranyard as having struck me by its resemblance to the "glory" round the heads of statues of saints in Roman Catholic shrines.

Jarvis and Burgoyne also made sketches of the "rays" separately, and without consultation with me or with each other, at my request. The similarity of the three is striking. Jarvis described the rays as resembling "the pipes of an organ."

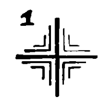

FIG. 2.

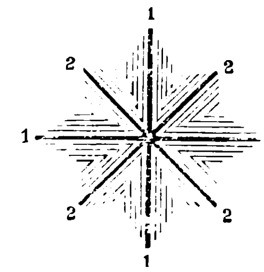

FIG 4.

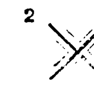

FIG. 3.

As the moon passed off from the sun's face, and for some seconds after the contact had wholly ceased, the clouds in the S.E. were suffused with deep red copper colours, which gradually faded away as the sun regained his power. During totality I made the following observations (according to Mr. Ranyard's previous request) with a Nicol's prism with Savart's bands, in the use of which instrument Mr. Ranyard had instructed me, and with which I had constantly practised during our week's preliminary residence in camp at Agosta; viz., I determined the polarisation of the sky at two points: (1) high up on the sky to the S.E. of the sun; (2) under the sun; and at both these points I found the plane of polarisation to be vertical. Totality ceased while I was taking a third observation.

Before concluding these imperfect remarks, which I have only ventured to make public because I thought that they might be of some interest, from the fact of my having had the good fortune to be one of the very few who obtained a perfectly clear and distinct view of the Eclipse during totality, I should like to bear my testimony to the great kindness of Colonel Porter, R.E., in placing all the resources at his command at the service of our party, and to his care of our creature comforts during our ten days' "dwelling in tents" under his charge. It is but just also to mention the zeal and alacrity displayed by his party of Sappers, and their intelligence in mastering the details of duties which were certainly new to them. Nor will it be considered out of place if, in conclusion, I thank the authorities of Agosta, and the Italian astronomers of distinction who did so much, not only by giving us every assistance in their power to render our stay at Agosta so successful, but also by their hospitable attentions to make it as enjoyable as possible. For my own part I can only say that I shall always look back with feelings of pleasure to the Sicilian Eclipse Expedition of 1870.

HENRY SAMUELSON

NOTES

THE Royal Commission on Scientific Instruction and the Advancement of Science is now in full work. This week they meet three times, and last week they met twice.

IT has been announced at the Royal Geographical Society that Livingstone has arrived at Ujiji on his return journey.

THE Hunterian Oration has been delivered this year by Sir W. Fergusson, who *inter alia*, according to the *Times* report, "playfully referred to the suggestion of a distinguished philosopher, whose views appeared in the columns of the *Times* last autumn, that when the microscope did not seem to give satisfaction in minute research, imagination might be substituted; and pointed out that this style of philosophy was not new, for it had been put into Hamlet's mouth by Shakespeare:—

Imperial Cæsar, dead, and turned to clay,
Might stop a hole to keep the wind away."

MR. J. C. BUCKMASTER has been appointed by Her Majesty's Commissioners to deliver an address on the value of the International Exhibition, with its bearing on industrial instruction, designed particularly for the working classes in all the large towns of the country which express a desire to hear it.

SCIENCE has derived many wrinkles from the siege of Paris, and we now learn from the correspondent of the *Daily Telegraph* that all the galleries of the Louvre are filled with sacks of earth to protect the interior from shells; and the damp and comparative warmth of the last few days have provoked active vegetation, so that the bags are covered with grass and weeds; each window is converted into a lively and promising garden. If the arrangement is left undisturbed much longer, we shall have flower beds there. This is certainly a novel kind of window-gardening, which we have no desire to see introduced into this country.

M. STEENAEKERS is about to issue a report on aëronautical ascents which took place during the siege of Paris. Individual reports are asked from aëronauts, but, unfortunately, very few of them are in a position to give a correct idea of their impressions, being mostly sailors of the Royal Navy.

AMONGST the learned men who escaped from Paris in a balloon we must cite M. d'Almeida, author of a treatise on Physics. He escaped on the 17th of December, at one o'clock in the morning, in the "Guttemberg," with four or five other persons. The "Guttemberg" was despatched a few minutes after the "Parmentier," which carried three persons and a bag of letters, and fell within Prussian lines. M. d'Almeida landed at Sanson, eight miles from thence, but he has not been heard of since that time, and it is to be feared that he was taken prisoner with his companions and sent to some German fortress.

THE French Government had amalgamated the post-office and telegraphs during the siege of Paris, under the management of M. Steenaekers, for the whole of France except the besieged capital. This reform was inaugurated in imitation of the English system under the pressure of circumstances. But a retrograde step was taken a few days ago, and two different services have been created as formerly. It is to be hoped that so unscientific a measure will soon be cancelled.

FRENCH *savants* do not appear to be successful politicians under universal suffrage. Amongst more than 2,000 citizens who offered themselves as candidates to the Parisian electors, we notice only a few members of the French Académie des Sciences. The only names who have come under our notice are M. Sainte-Claire Deville (we do not know if it is Henri or Charles), and M. Nelaton, the physician who cured Garibaldi's wound. M. Berthelot was also a candidate, but he does not belong to the Académie des Sciences. The Académie Française was more successful, as Michelet, Victor Hugo, Henri Martin, Thiers, Jules Favre, and many others, were chosen.

WE learn from the *British Medical Journal* that the distinguished ophthalmologist, Dr. Liebreich, has just passed an examination and been admitted a member of the Royal College of Surgeons. It is stated that he will be invited to accept the ophthalmic chair of St. Thomas's Hospital.

THE death is announced of Dr. Sheridan Muspratt, F.R.S., the well-known analytical chemist of Liverpool, and author of a Dictionary of Chemistry.

THE following is the list of officers and council of the Royal Microscopical Society for the current year:—President: W. K. Parker, F.R.S. Vice-Presidents: Chas. Brooke, F.R.S., J. E. Gray, F.R.S., J. Millar, F. H. Wendham. Treasurer: Richard Mestayer. Secretaries: Henry J. Slack, Jabez Hogg. Council: R. Braithwaite, M.D., John Berney, James Glaisher, F.R.S., W. J. Gray, M.D., Henry Lawson, M.D., Henry Lee, Jas. Murie, M.D., G.W. Royston Pigott, M.D., J. W. Stephenson, Chas. Stewart, Chas. Tyler, T. C. White. Assistant Secretary: Walter W. Reeves.

MR. A. R. WALLACE has printed his Anniversary Address, delivered before the Entomological Society on Jan. 23rd. After referring to the loss Entomology has sustained during the past year by the death of J. T. Lacordaire and A. H. Haliday, and to the publication of Mr. Crotch's papers on the Genera of Coleoptera studied chronologically, McLachlan's Catalogue of British Neuroptera, Dr. A. S. Packard's Guide to the Study of Insects, and some other works of the year, the greater part of the address is occupied by a critique on Mr. Andrew Murray's important paper "On the Geographical Relations of the chief Coleopterous Faunæ." Of Mr. Murray's division of the Coleoptera of the world into three grand stirpes or races—the Indo-African, the Brazilian, and the Microtypal—the first comprehending all the characteristic forms of the eastern tropics, the second all those of tropical America, and the third those of the temperate regions of the whole world, not excluding even America—Mr. Wallace remarks that the two first will probably be generally accepted, while the third group, as of equal value to the others, will be as generally rejected. Mr. Murray's theory, derived from the geographical distribution of the Coleoptera, that the whole of the Atlantic Islands, from the Azores to the Cape de Verdes, and even to St. Helena, are portions of a vast submerged continent connected with Southern Europe, is combated by Mr. Wallace as far as the Madeira group is concerned, while he considers there is more to be said in favour of their connection with one another at some remote period.

THE third session of the Ladies' Educational Association of London is considered a successful one. The number of tickets issued has been:—for English Literature, 56; English Language, 40; Psychology and Logic, 46; French Literature, 39; French Language, 20; Experimental Physics, 25; Chemistry, 10; Total, 236, against 292 last year. The falling off is almost entirely in the class of English Literature, in which 104 tickets were issued last year, a decrease of 48 in the present session. The attendance is reported as regular. Lecture associations of a similar kind for ladies have also been established in the northern and eastern districts of the metropolis.

THE Annual Report of the Canadian Natural History Society states that the recent dredgings of Mr. Whiteaves have added many facts to our knowledge of the creatures which inhabit Canadian seas. The marine mollusca have been carefully monographed, and instead of 60 or 70 species, we now know of nearly 130, the number having been thus nearly doubled. The careful identification of the inhabitants of the deep sea, in addition to its zoological importance, will do much to illustrate the conditions under which the Canadian post-tertiary deposits have been accumulated.

WE have on our table a number of important papers referring to the Colony of Victoria, prepared by Mr. W. H. Archer, Registrar-general to the Colony, and published by authority of the Colonial Government: viz. Patents and Patentees from 1854 to 1866, Abstracts of Specifications of Patents applied for from 1854 to 1866 Ac. to Bu., and Indexes to Patents and Patentees for 1867 and 1868. Also Reports of the Mining Surveyors and Registrars for the quarter ending Sept. 30th, 1870, Report on the present condition of the Geological Survey of the Colony, and Abstracts of English and Colonial Patent Specifications relating to the Preservation of Food, &c., compiled from original documents, by Mr. W. H. Archer. We would call especial attention to the last as a well-arranged epitome of very useful information.

DR. M. T. MASTERS and Dr. J. H. Gilbert reprint from the *Proceedings of the Royal Horticultural Society* "Reports of Experiments made in the Gardens of the Society at Chiswick in 1869 on the Influence of various Manures on different Species of

Plants." The experiments afford striking illustrations of the varying powers of accumulation and assimilation possessed by plants belonging to different families, to different genera of the same family, and even to different species of the same genus, under the same external conditions of supply in soil and atmosphere, indicating, as the authors think, not only very varying ranges of root-collection, but also quantitatively varying functional characters, both of the feeders and the elaborating organs. It is expected that results of considerable practical value to agriculturists will be obtained from the second series of experiments which were conducted during 1870. The Royal Horticultura Society appears to be applying its resources to a very useful end in the carrying out of these investigations.

WE have received from Mr. F. Bradley, of Chicago, a pamphlet, entitled "Northern Lights, Shooting Stars, and other Meteoric Phenomena proved to be not of modern origin," in which he explains several well-known Biblical figures by the supposition of an acquaintance on the part of the writers with the phenomena of the Aurora Borealis, and combats the views of a recent writer that the alleged occurrence of the Aurora within the last three centuries for the first time, is a sign of the approaching end of all things.

THE great difficulty which has been experienced by farmers during the present winter in procuring sufficient food for their cattle may have the effect of directing attention to fresh substitutes for the ordinary winter keep. Turnips and Swedes have been a complete failure throughout a large breadth of the country, and the hay crop has been generally so deficient that many agriculturists have been already brought to the verge of ruin. In the Argentine Republic, as is well known, vast herds of cattle are reared upon the natural grasses of the pampas, or upon the lucerne which grows there in great abundance, but in the province of Catamarca both man and beast depend for support mainly upon the leaf and fruit of the algarrobo. It provides their principal stock of food during the winter months, and is said to be exceedingly nutritious. The algarrobo (*Hymenæa Courbaril-Jetaiba*) is indigenous to the country, and its fruit is gathered annually and stored with much care. The long pods are pounded in a wooden mortar, and the residuum is then passed through a sieve, and the meal converted into circular cakes, which, after having been dried in the sun, are fit for use. In this state it is called "patay," and is exported as a bread-stuff into other districts of the Republic, in some of which it forms the exclusive food of the people. Its merits as a means for fattening cattle are also thoroughly appreciated, and it might be a great boon to our farmers, if the Acclimatisation Society would ascertain whether the algarrobo might not be introduced into this country with advantage. The tree grows to a height of forty feet, with wide-spread branches, and a rather slender stem, and flourishes best upon a dry soil.

CONSIDERABLE discusion took place in the *Times* last autumn as to whether acorns were suitable for employment as food for cattle; and the evidence adduced certainly favoured a negative view. Dr. Robert Brown, however, tells us that those produced in California by several species of oak form an important article of food. "The acorns of California are mostly large, and the trees in general produce abundantly, though in some years there is a great scarcity, and much misery ensues among the poorer natives. The acorns are gathered by the squaws, and are preserved in various methods. The most common plan is to build a basket with twigs and rushes in an oak tree and keep the acorns there. The acorns are prepared for eating by grinding them and boiling them with water into a thick paste, or by baking them into bread. The oven is a hole in the ground about eighteen nches cubic. Red-hot stones are placed in the bottom, a little dry sand or loam is placed over them, and next comes a layer of dry leaves. The dough or paste is poured into the hole until it is two or three inches deep; then comes another layer of leaves, more sand, red-hot stones, and finally dirt. At the end of five or six hours the oven has cooled down, and the bread is taken out, an irregular mass, nearly black in colour, not at all agreeable to the eye or to the palate, and mixed with leaves and dirt."

THE Proceedings of the Somersetshire Archæological and Natural History Society for 1868-69, just published, consists almost entirely of papers which would come under the first head. The only ones bearing directly on Natural Science are "On the Geology of the Mendips," by Mr. C. Moore, and "On the Rodentia of the Somerset Caves," by Mr. W. A. Sanford. Is it necessary that Reports of the Proceedings of Local Societies should be published at so high a price as practically to take them out of the reach of the general public?

MR. F. ABBOTT has reprinted a paper read at the meeting of the Royal Society of Tasmania, Oct. 11, 1870, "On the Sun and its Office in the Universe."

WE recently referred to Mr. Townend Glover's paper "On the Food and Habits of Beetles," issued in the Report of the Commissioner of Agriculture for Washington. A work, similar in style, but more comprehensive, has been published by the Board of Agriculture for the State of Missouri, which has secured "an appropriation for this purpose from the Legislature of the State." This "First Annual Report on the noxious, beneficial, and other Insects of the State of Missouri" is furnished by Mr. Charles V. Riley, State Entomologist, and from its plain descriptions, and rough, yet accurate, woodcuts, seems admirably adapted to fulfil the purpose for which it is published—that of giving the farmer a knowledge of his insect friends and foes which he may turn to practical account.

THE *Geraniaceæ*, taken as a whole, are by no means noted for their economic properties. In Tasmania, however, a form of the common *Geranium dissectum* having a thick rootstock is employed by the aborigines, who are in the habit of digging up the large, fleshy roots and roasting them for food. About Launceston it is called "native carrot," and is common throughout the colony. The typical form of *G. dissectum* is generally diffused over the temperate regions of the northern hemisphere in th Old World, where it is annual. In the Eastern United States a biennial or annual form, *G. carolinianum*, takes its place, the typical *G. dissectum* being only known as an introduced weed; although connecting links between the two may readily be detected. West of the Rocky Mountains the stock often appears to be perennial, and it cannot then be distinguished from some of the Australian forms.

WE may call attention to an interesting paper, reported in our present number, by Mr. Cecil Smith, read before the Somersetshire Natural History Society, on the relations of the Great Bustard, which has once more visited England during this severe winter. Mr. Smith removes the bird from the Rasores, among which it was placed by Mr. Yarrell, and restores it to the position assigned it by Cuvier, among the Grallatores, near to the Plovers.

THE question of killing deadly snakes at Government expense in India is again under discussion. The Government is losing its subjects at the rate of above a hundred a day, or 40,000 a year by snake bites, but it fears losing rupees in the crippled state of its treasury. The last enforcement of the law was under Mr. Commissioner Plowden, many years ago in the Bancorah district, one of the smallest portions of the Burdwan division. Deadly snakes were brought in at the rate of some 1,200 a day, and although the scale was only from threepence to sixpence apiece, in about a couple of months 10,000*l*. was drawn out of the treasury, and the Government ordered the snake crusade to be stopped.

MOUNT WASHINGTON IN WINTER

BY the kindness of an American friend, we have been favoured with a Boston newspaper containing an account of an important meteorological experiment which has recently been tried in America with the greatest success; we refer to the establishment of a winter observatory on the summit of Mount Washington at an elevation of something like 10,000 feet above sea-level.

Everybody knows that Mount Washington, in New Hampshire, is visited by thousands of persons in the summer months, and that its climate corresponds better with that of Labrador or Greenland than with that of New England. In the winter universal desolation reigns there; not even the proprietors of the hotels upon the summit venturing from their snug quarters below to learn what events are transpiring upon the icy cone. The aborigines declared it to be sure death if anyone climbed the mountain—since it was the sanctuary of divinities who would not suffer their abode to be scanned with impunity. Their successors first adventurously found the summit—then erected rude places for shelter, and finally constructed a carriage road and railway, so that even the most feeble persons could view the broad panorama. But these visits were confined to the warmer months, with a few rare exceptions.

Twelve years ago Profs. C. H. Hitchcock and J. H. Huntington, independently of each other, conceived the project of spending the winter upon the summit of Mount Washington, but the project did not take shape till the organisation of the Geological Survey of New Hampshire in 1868. They found it impossible to make the necessary preparations for occupying the summit during the winter of 1868, chiefly for want of a dwelling. Hence they sought for a less elevated summit, where a single winter's experience might prepare the way for the greater adventure. That peak was Mooselauke, nearly 5,000 feet above the ocean. The lessee, D. G. Marsh, of Warren, N.H., obligingly placed his house at the service of Prof. Huntington and his comrade, A. F. Clough, of Warren, photographer. Their three months' occupation of Mooselauke was full of adventure, and experiences were acquired of the highest importance. The scientific results were important, disclosing the knowledge of violent winds there accurately measured, and remarkable forms of frost-work never before described or photographed. Attention was called to this mountain, and a carriage road became a necessity, which was constructed in the following summer.

In the month of September, 1869, the Mount Washington Railway Company generously tendered the use of their depôt upon the summit to this meteorological party during the winter; and the necessary supplies were immediately purchased and forwarded to the mountain. The enterprise, though of a meteorological character, has been adopted by the Geological Survey of the State, while the expense has been assumed by the State geologist, relying upon a sympathising public to provide the funds by subscription.

Congress recently appropriated funds for the establishment of a "Bureau of Telegrams and Reports for the Benefit of Commerce." After some correspondence with the efficient officer in charge of this bureau, General Myer ordered an insulated telegraph wire with suitable instruments to be sent to the mountain, in order to facilitate the transmission of the meteorological reports, both to the public and to the office of the bureau in Washington. The wire has been laid, and the summit is now in telegraphic communication with the world. And the chief signal officer also detailed for special service upon the mountain an experienced telegrapher and meteorologist, Sergeant Theodore Smith, of the U.S. Army.

The photographers of the expedition are Mr. A. F. Clough, of Warren, and Mr. Howard A. Kimball, of Concord, N.H. The latter gentleman spent much time in providing photographic material for the mountain, and in soliciting subscriptions. Their views of the peculiar phenomena of the mountain will soon be exhibited.

Thus the party consists of six persons: Prof. Hitchcock, whose office in Hanover, N.H., is connected by telegraph with the summit; Prof Huntington and Mr. Nelson, observers; Messrs. Clough and Kimball, photographers; and Sergeant Smith, telegrapher and observer. All of them are not upon the mountain at the same time. They relieve each other to a considerable extent in the work, and the public will be kept informed of their whereabouts.

From the dépôt of the Mount Washington Railway in summer, the ascent on foot, if a person is accustomed to walking, is comparatively easy. Although the ties are three feet apart, and there is a rise of a foot in three part of the way, yet a person with muscles strong from exercise, can walk to the very summit of the mountain without sitting down to rest. But suppose it is winter. The snow has accumulated to a considerable depths even in the ties, but then it is no great hindrance; should it, however, be attempted a second time, you will find that the snow that was compressed beneath the feet has changed to ice, and the oval forms give a still less secure footing; if it is thawing, and the ice is almost ready to slip off as you tread upon it, every one will see that upon a trestle thirty feet high walking is *somewhat* dangerous, and to walk down is a feat from which even a most expert acrobat would shrink. If at the dépôt we take snow shoes, we can walk with comparative ease half way up, and then the snow is so compact that they are no longer needed, and as there are few irregularities in the surface the walking is better than in summer.

Above the limit of the trees the railway is covered with ice of every fantastic shape, and the framework of the Gulf tank is now so ornamented that one can hardly believe that it is the rude structure seen in summer. The Lizzie Bourne Monument, which one is accustomed to see only a rough pile of stones, is now an object of architectural beauty such as no sculptor can carve from marble. Immediately above the monument the timber trestles are completely covered with deposits of frozen mist extending two or three feet horizontally from the timber on which the track is laid, and every piece of timber which forms the trestle is ornamented with beautiful forms of frost-work, deposited in graceful curves as the wind sweeps through the trestle.

On the summit, the buildings, the piles of rock and stones, so rough in summer, are now completely covered with frost, while the snow fills the spaces between the jagged rocks. On the sides of the buildings towards the north-west the frost has accumulated, so that now it is more than a foot in thickness. The frost-work on the dépôt, while it has everywhere the same general appearance, the points show exactly the direction of the wind as it came into every nook and corner of the building. The frost on the braces and timbers that extend outward seems like one triangular mass, and on the chains it is often two feet in diameter. The correspondent then adds:—

"Although I was on the mountain for ten days in October, yet I did not go to stay permanently until November 12. It was expected that some of the party would be ready to go with me, but as they were not I went alone, and it was not till the last day of November that anyone came, so that I had the house and the mountain all to myself for nearly three weeks. During the greater part of November the weather was remarkably pleasant. On the 15th, all day long, and far into the night, the clouds were below the summit of the mountain, and most of the time they covered the entire country. At times only the very highest point of Mount Washington was above the clouds. It reminded me forcibly of the time, during the Champlain period, when the whole of New England, except Mount Washington, was beneath the waves of the ocean. As the masses of clouds came over Adams and Jefferson, I looked until I almost persuaded myself that there were immense icebergs coming from the uplifted frozen North, but the illusion vanished as the summits appeared above the clouds. But what was remarkable is that the next day was pleasant.

"During the last two weeks in November, the average of the thermometer was 16°. The wind was north-west nearly the whole time. The weather was generally very mild, and the number of clear days was much greater than the average."

We may add to this interesting account that the daily telegrams received from the mountain have been full of interest. A temperature of 40° below zero has not been uncommon, and we may well hope that the efforts of the solitary watchers on the inclement mountain-top will tend to advance the science of which they are such devoted students.

SCIENCE IN VICTORIA

THOSE who have read the Marquis de Beauvoir's "Voyage round the World" will recollect the high praise with which he speaks of Sir Redmond Barry's prolonged and continuous efforts for the advancement of Science in Victoria. His latest good deed in this direction is the aid which he has afforded in the establishment of a School of Mines at Ballarat, which, to use his own words, may now be regarded as "an accomplished fact," since "everything connected with its establishment and maintenance has been thoroughly debated and determined on." The prospectus of the Institution states that "the object sought to be obtained is the combination of the highest scientific with the most practical training for all men engaged in the enterprise of mining in its various branches — whether so engaged as mining managers, engineers, surveyors, mechanists, working miners, directors or promoters of companies;" and, as there is every reason to believe that the school will be supported by the Government, by the several mining boards, and by the general public, we may hope to see it in actual operation in a few months.

We have not space to give a sketch of Sir Redmond Barry's excellent address, further than to remark that he points out the general bearings of chemistry, electricity, geology, steam, &c., on the progress of mining; and we must content ourselves with the following extract relating to the progress of Science in Australia :—

"We are not," he observes, "even in our time, satisfied to import the discoveries of others, and to invite the man of science and the skilled artisan to direct their operations. Australia can point with a modest satisfaction to the invention of Osborne in photo-lithography, and the test for blood by Dr. Day;* to the cure for snake-bites by Professor Halford; to the method of removing pyrites, that inveterate enemy of the metallurgist, as well as the native alloys, from gold, and toughening the metal by the use of chlorine gas, by Mr. Miller of the Sydney Mint; to the scientific preservation of the meat of our redundant flocks and herds, so largely exported; and also to many others, mention of which would delay us too long."

THE CONTRACTION OF THE EARTH †

THE phenomena, which were ably presented by the distinguished geologist, Mr. Lesley, to the National Academy of Sciences, and which seem to demonstrate that the outer shell of the earth has sensibly shrunk, in some directions at least, since its original formation, naturally invite the attention of physicists to the possible causes of such a result. The most obvious cause of the shrinking of the earth is its cooling. But to shrink two per cent. linearly, which is the amount deduced by Mr. Lesley from the observed geological phenomena, involves a probable cooling of the whole earth of not less than two thousand degrees centigrade, which would require that its original temperature should be higher than would be consistent with the solidity of these shrunk strata.

Another source of change of form, which would produce shrinkages in different directions in different parts of the earth, is to be found in the diminution of oblateness arising from the diminished velocity of rotation upon the axis. Such diminution of the velocity of rotation has several years ago been shown by Mr. Ferrel to be caused by the action of the moon in producing the tides; this is, therefore, a true cause, and it is only necessary to examine how great its amount can be under any circumstances. This is all which is proposed in the present investigation, and the application to facts is reserved for geologists.

It is sufficient, for the present object, to regard the earth as homogeneous. Under this condition Laplace has shown that the time of the earth's rotation could not be less than about one-tenth of a day, which corresponds to a ratio of the axis of the equator to that of the pole, equal to 2·7197, and an equatorial circumference 94 per cent. greater than the present one. Such is then the amount of shrinking which might have taken place, if any cause could be assigned capable of producing so great a reduction of the earth's velocity. The whole surface of the earth would have been about 130 per cent. larger than at present.

But the only cause at present known which would produce a sensible reduction of the earth's velocity is the lunar action upon the tides. But in this mutual action between the moon and the earth, the common rotation area of the earth and moon must remain unchanged. The question then arises, How great a reduction of the rotation area of the earth would have passed into that of the moon? In this inquiry it may be assumed that the moon revolves in a circular orbit in the plane of the earth's equator.

Now the moon's rotation area is 3·716 times the earth's. But if, in the origin, it had revolved just in contact with this earth, its rotation area would not have been less than 0·480 times the earth's, so that it could not have absorbed a rotation area from the earth greater than 3·236 times the earth's present rotation area, and therefore the earth's rotation area could never have exceeded 4·236 times that which it has at present. But, with the maximum velocity of rotation given by Laplace, the earth's rotation area would have been $37\frac{1}{2}$ times greater than at present. It can never, therefore, have been reduced to so great an extent by the moon's action on the tides. But since, when the oblateness is small, the rotation area is nearly proportional to the velocity; and the excess of the square of the equatorial above that of the polar axis is nearly proportional to the square of the velocity, this excess may have been originally nearly eighteen times as great as at present, or about $15\frac{1}{2}$ per cent. of the square of the polar axis. This would correspond to a figure of the earth in which the equatorial radius would have been about $2\frac{1}{2}$ per cent. greater than at present; so that it is sufficient to account for the observed phenomenon.

This peculiar form of shrinkage would produce the highest mountains at the equator, and the tendency of the mountain ranges would then be to assume the direction of the meridian. But nearer the poles the mountains would be less elevated, and would rather tend towards the direction of the parallels of latitude.

It is, next, expedient to consider the mechanical question of the loss of living force in the case of the moon's action upon the waters of the earth, and its effect upon their different motions. In this connection there are problems worthy of the attention of geometers; such as the relative motions of bodies rotating above the same vertical axis, towards which they are drawn by weights, and acting upon each other through the friction on the axis. For one of the bodies a rotating wheel may be substituted. There is also the case of two planets revolving about a primary, and acting upon each other through some form of friction.

In this way it will be seen that the planet or satellite once formed is constantly removed from the primary, and that planets tend to approach each other. It is interesting to consider whether this may not be one of the actual problems of nature.

B. PEIRCE

* A full account of Dr. Day's remarkable colour-tests for blood and pus may be found in Dr. Richardson's Report on Toxicology in the last number of the *Medico-Chirurgical Review.*—ED.

† From the Proceedings of the American Academy of Arts and Sciences, vol. viii.

SOCIETIES AND ACADEMIES

London

Royal Society, February 9.—"Abstract of Paper on the Effect of Exercise upon the Bodily Temperature." By Dr. T. Clifford Allbutt. The object of the author in carrying out the experiments recorded in the present paper was to inquire whether the regulating power of the organism held good under great variations of muscular exertion. For this purpose he made frequent daily examinations of his own temperatures during a short walking tour in Switzerland, and found that the effect of continuous muscular exertion upon himself was to sharpen the curve of daily variation, the culmination being one or two-tenths higher than usual, and the evening fall coming on more rapidly and somewhat earlier. Charts of the daily temperatures were handed in with the paper. The author made reference also to some observations of M. Lortet, which differed from his own. These observations, which did not come into Dr. Clifford Allbutt's hands until his own experiments were partially completed, were adduced by M. Lortet to prove that the human body was very defective in regulating power under the demands of the combustion needed to supply the force expended in muscular exertion. Dr. Clifford Allbutt's results were very decidedly opposed to those of M. Lortet; for only on two occasions did he note the depressions of temperature which M. Lortet regards as constant. It would seem, however, that the body is more or less liable to such depressions when engaged in muscular exertion; but the cause of them is very obscure. Of the two low temperatures noted by the author, one occurred during a very easy ascent of lower slopes, and the second was observed during a descent. The author thinks that they may be due to some accidental deficiency in combustion, and inquires whether the capacity of the chest in different individuals may account for the varying influence of muscular effort upon them, and perhaps for the earlier or later sense of fatigue. The sphygmographic tracings added by M. Lortet to his temperature charts seemed to show a great inadequacy of circulation.—"Observations of the Eclipse at Oxford, December 22, 1870." By John Phillips, F.R.S.

Geological Society, January 25.—Mr. Joseph Prestwich, F.R.S., President, in the chair. Richard Atkinson Peacock, of St. Helier's, Jersey; Arthur W. Waters, Davos Plaz, Canton of Grisons; R. Koma, of University College, London; and Ransom Franklin Humiston, M.A., Professor of Chemistry in Cleveland University, U.S., were elected Fellows of the Society. The following communications were read: 1. "On the Physical Relations of the New Red Marl, Rhætic beds, and Lower Lias," by Prof. A. C. Ramsay, LL.D., F.R.S., F.G.S. The author commenced by stating that there is a perfect physical gradation between the New Red Marl and the Rhætic beds. He considered that the New Red Sandstone and Marl were formed in inland waters, the latter in a salt lake, and regarded the abundance of oxide of iron in them as favourable to this view. The fossil footprints occurring in them were evidence that there was no tide in the water. The author maintained that the New Red Marl is more closely related to the Rhætic, and even to the Lias, than to the Bunter; and in support of this opinion he cited both stratigraphical and palæontological evidence. He described what he regarded as the sequence of events during the accumulation of the later Triassic deposits and the passage through the Rhætic to the Lias, and intimating that the same reasoning would apply to other British strata, especially some of those coloured red by oxide of iron, including the Permian, the Old Red Sandstone, and a part of the Cambrian. Mr. Etheridge thought the question of the nature of the Rhætic beds was to a great extent palæontological. The main point in connection with them was as to how the British beds were to be connected with the Lombardic and Middle European areas. It certainly seemed probable that in this part of the world the conditions of life were different, the deposits being much less in thickness, and the fauna much diminished; and where represented at all, the shells occurred in a dwarfed and stunted form. The exact horizon and nature of the Sutton beds had still to be determined. Mr. Godwin-Austen believed that every mass of red sandstone would ultimately be referred to either a brackish or freshwater origin. A comparison of the ancient and present area of the Caspian Sea would tend to remove any doubt that might remain on the mind of geologists as to the possibility of the existence of such vast internal seas as those which had to be called in to account for these formations. He regretted that former observers had not attached more importance to the duration and extent of those freshwater conditions which were found so commonly to have prevailed between the periods of deposit of the great marine formations. There was another fact to be borne in mind, that even in existing lakes the water at the one end was sometimes completely fresh, and at the other end salt, each, of course, with a different fauna. Prof. Rupert Jones said that, although there were good grounds for the lake-theory, something might be said for shallow seas. He remarked that sulphate of lime was deposited from sea-water before salt, that oxide of iron might originate from chloride of iron diffused in water whether of lakes or seas, and that the hæmatites of Permian age were probably deposited in the sea. He considered that Foraminifera required great caution when used as criteria, as the varietal forms giving the facies were of more importance than the genera and species. The *Estheriæ* were never marine, although often occurring in plenty in temporary freshwater pools on the sea-shore. In his monograph of *Estheria* he had said much to substantiate the notion that freshwater conditions often prevailed during the formation of the Keuper. Both in the Old Red Sandstone of the Baltic provinces and in the Lettenkohle and Keuper of Germany, when *Estheria* comes in, *Lingula* dies out. The *repeated* set of formations in the Permian and the Trias precludes their contemporaneity, as supposed by Messrs. Godwin-Austen and Marcou. Mr. Bauerman marked that the Hallstatt beds which had been cited as marine contained large deposits of rock-salt. M. Marcou thought that the difficulties in regarding these beds as of freshwater origin were greater than the author supposed. The absence of fossils in gypsum, though almost universal, was not total. He had himself seen three specimens of *Trigonia* in gypsum from Stuttgart. Mr. Tate mentioned the discovery by Mr. Burton of marine fossils in the Red Marl, in one instance in combination with vegetable remains. He commented on the sharp demarcation observable in Ireland between the Rhætic beds and the marl below, whereas it was almost impossible to separate them from the Lias above. He doubted, however, whether the true relations of the Rhætic beds were to be worked out in this country. As to the fossils of the Sutton Stone, they were all purely Liassic. Mr. Burton stated that the fossils from the Red Marl came from a spot about five miles from Retford, in the direction of Gainsborough, but he had not seen them *in situ*. There are, however, no Rhætic beds within some miles. Rev. Mr. Winwood, in the absence of Mr. C. Moore, from ill health, inquired whether the author regarded the White Lias as Rhætic or Liassic. Prof. Ramsay, in reply, was quite willing to accept marine fossils as coming from the Red Marl. The fact of *Estheria*, a brackish or freshwater form, occurring in certain bands, was in favour of his views, as he considered that at intervals the freshness of the water in such a lake as he had suggested must have varied. He could not accept the probability of oxide of iron having been deposited in a large sea area to such an extent as to colour the sands. All rocks that could be proved to be of marine origin, even when they contained iron, were not stained red unless by infiltration from above. He pointed out that the old area of the Caspian was far larger than the lake in which he had suggested that the New Red Marl had been deposited. If, as was more than probable, there had been during all geological time continental areas somewhat in the same positions as those of the present day, there must have been large areas of inland drainage in which some such deposits as those in question must of necessity have been formed.—2. "Note on a large Reptilian Skull from Brooke, Isle of Wight, probably Dinosaurian, and referable to the genus *Iguanodon*." By J. W. Hulke, F.R.S., F.G.S. The author stated that the skull described by him was obtained from a Wealden deposit at Brooke, in the Isle of Wight, from which many remains of Dinosauria have been obtained. He described its characters in detail, and remarked that its most striking peculiarities were:—the completeness of the bony brain-case; the obliteration of the sutures, especially those of the basicranial axis; the massiveness of the skull; and the great downward extension of the basisphenoid, with the attendant upward slant of the lower border of the basipresphenoidal rod. The first of these characters occurs elsewhere among reptiles only in *Dicynodon*; and the first and second characters combined were regarded by the author as approximating the skull to the ornithic type. The reference of this skull to *Iguanodon* was founded chiefly on the place from which it was obtained, which has furnished abundant remains of that genus, and on the obliteration of the sutures, which the author stated to be a character of the mandibles of *Iguanodon*. Prof. Huxley congratulated the society on the progress being made in our knowledge of this interesting group of reptiles and of their ornithic affinities. Mr. Seeley remarked on the similarity of the internal cavity of the skull to that of *Ichthyosaurus*. Some of the external characteristics differed much from what he was acquainted with in other Dinosaurian skulls, which more closely resembled those of ordinary lizards. He considered that the affinities of *Dinosaurs* were in the direction of *Teleosaurus*, from which the position of what were supposed to be the optic nerves in this skull materially differed. On the whole, he was not at once prepared to accept this skull as that of an Iguanodon. Mr. Hulke briefly replied, and observed that he had limited his speculations to those which legitimately arose from the facts before him. The following specimens were exhibited to the meeting:—Specimens from the Keuper Marls and Rhætic beds near Gainsborough; exhibited by Mr. F. M. Burton, F.G.S. Examples of the borings of two species of *Pholas*; exhibited by Mr. E. Charlesworth, F.G.S.

Zoological Society, February 7.—Mr. G. R. Waterhouse, Vice-President, in the chair.—The Secretary read a report on the additions to the Society's Menagerie during the month of January, amongst which were particularly mentioned a specimen of the kakapo, or night-parrot of New Zealand (*Strigops habroptilus*).—Mr. J. E. Harting exhibited and made remarks on a fine specimen of the red-breasted goose (*Anser ruficollis*) lately killed in England.—Mr. H. E. Dresser exhibited some specimens of rare European birds'-eggs, amongst which were those of *Micronisus brevipes* and *Reguloides superciliosus*.—Mr. E. Ward exhibited a skin of a white variety of the tiger (*Felis tigris*), obtained from an animal killed in the Mirzapore district.—Mr. W. B. Tegetmeier exhibited a specimen of an eel, believed to be new to the fauna of Great Britain. It had been obtained from fresh-water in the Scilly Islands, and was referred by Dr. Günther to a variety of *Anguilla vulgaris*, called *A. cuvieri* by Kaup.—A communication was read from Dr. Robert O. Cunningham, containing notes on some distinctive points in the osteology of *Rhea americana* and *Rhea darwinii*.—Mr. J. E. Harting read a paper on the Arctic collection of birds presented by Mr. John Barrow to the University Museum, Oxford, and drew attention to some interesting facts in connection with the geographical distribution of birds in northern latitudes.—A communication was read from Professor Carl J. Sundevall, containing an account of the birds obtained in the Galapagos Islands, during the voyage of the Royal Swedish frigate *Eugenia*.—Mr. R. B. Sharpe read a paper on the birds of Angola, founded on collections made in that country by Mr. J. J. Monteiro and Mr. Charles Hamilton, being his third communication on this subject.—Mr. J. Verreaux pointed out the characters of a new species of *Promerops* from Natal, which he proposed to call *Promerops gurneyi*.—Dr. J. E. Gray communicated a description of *Platasterias*, a new genus of *Astropectinidæ* from Mexico.—A communication was read from Mr. D. G. Elliot, containing the description of a new species of pheasant of the genus *Euplocamus* from Burmah, proposed to be called *Euplocamus andersonii*.—Dr. John Anderson pointed out the characters of three new species of squirrels (*Sciurus*) recently obtained by him during the Yunan Expedition. Dr. Anderson also gave an account of a new Cetacean which he had lately discovered in the Upper Irrawaddi, and which he proposed to call *Orcela fluminalis*. A third communication from Dr. Anderson contained a note on the occurence of the remarkable parasitic Crustacean *Saccalina* in the Bay of Bengal. The species, which had been found on the common swimming crab of that district (*Thalamita crenata*) did not appear to differ from that which is found on *Carcinus mænas* on the shores of Great Britain.

Royal Institution, February 6.—Monthly General Meeting. Sir Henry Holland, Bart., F.R.S., President, in the chair. Henry Edward Colvile, Esq.; John H. Dallmeyer, Esq.; Duncan Davidson, Esq.; Warren William De la Rue, Esq.; James N. Douglass, Esq.; Mrs. Gibbs; Abraham Goodall, Esq., F.R.C.S.; Alexander Macfarlan, Esq.; Robert Turtle Pigott, Esq., F.R.G.S.; Robert Sabine, Esq.; Charles Southwell, Esq.; Henry Stilwell, Esq.; Richard Valpy, Esq.; Mrs. Jacob Waley, were elected members of the Royal Institution. The presents received since the last meeting were laid on the table, and the thanks of the members returned for the same.

Entomological Society, February 6.—Mr. A. R. Wallace, President, in the chair.—Pastor Kawall, of Pussen, Kurland, was elected a corresponding member.—Mr. Bond exhibited *Pachinobia alpina*, *Gelacha borcella*, varieties of *Thera juniperata*, and *Larentia cæsiata*, and portions of web of *Hyponomenta padi* over a yard long, all from Perthshire; also photographs of eggs of bird parasites, taken from slides prepared by Mr. Norman. He likewise exhibited a curious instance of monstrosity in *Vanessa Atalanta*, the butterfly still retaining the larval head.—The Rev. H. S. Gorham exhibited *Oxytelus fulvipes* of Erichson, a new British beetle.—Mr. Müller exhibited specimens of oak-galls from Tangiers, collected by Mr. Blackmore.—Prof. Westwood exhibited a minute species of *Corixa* from India, destructive there to the eggs of the most valuable freshwater fish; also drawings of a species of *Coccidæ* injurious to the leaves of an exotic *Cypripedium*. The male shield consisted of a small disc, with six raised radiating lines produced into spines.—Mr. Butler read a descriptions of a new genus, and of seven new species of *Pierinæ*.

MANCHESTER

Literary and Philosophical Society, January 24.—Mr. E. W. Binney, president, in the chair.—Mr. Brothers exhibited a drawing from the fine photograph of the solar Corona, taken by him at Syracuse, during the late total eclipse of the sun.—Dr. Joule, F.R.S., read the following letter, dated January 21, 1871, which he had received from Mr. William H. Johnson, of Bowdon:—"Since the last meeting of the Philosophical Society I have made some further experiments on the 'Effect of cold on the strength of iron.' In these I have maintained a nearly fixed temperature, and thus avoided to a great extent the error occasioned by the rise in temperature, consequent on sudden torsion. January 11. A piece of a charcoal wire rod, ·237in. diameter, gave the following results:—

	1st.	2nd.	3rd.
At about 40° F.	20 twists	19 twists	17 twists.
Adjacent 6″ at temperature of melting zinc	10 twists	9 twists	$7\frac{1}{2}$ twists.
		4th.	5th.
Twisted very slowly, surrounded by salt and snow		$19\frac{1}{4}$ twists.	16 twists.
Adjacent 6″ at about 40° F		15 twists	

The twisting under salt and snow was performed so slowly, each experiment lasting a quarter of an hour or more, that the temperature cannot have been affected by the torsion. The same care was taken at the temperature of 40° F. The great diminution of strength at the melting point of zinc is remarkable. I take the liberty of communicating these results to you, as unfortunately I shall be away at the next meeting, and thus shall not have an opportunity of seeing you." Mr. Brockbank remarked that these experiments did not affect the conclusions stated in his paper, read at the last meeting. He believed that the strength of iron under torsion was most affected by the heat developed by the twisting, and that the cooling mixture employed by Mr. Johnson would have the effect of making the wire stand a greater number of twists by counteracting the excessive heat produced by the torsion. Mr. Brockbank exhibited a drawing of the machine used by him in his experiments on the strength of cast iron at different temperatures.—"Experiments on the Oxidation of Iron," by Prof. E. Crace Calvert, Ph.D., F.R.S., &c. Some two years since Sir Charles Fox inquired of me if I could give him the exact composition of iron rust, viz., the oxidation found on the surface of metallic iron. I replied that it was admitted by all chemists to be the hydrate of the sesquioxide of iron, containing a trace of ammonia; to this he answered that he had read several books on the subject, in which the statements referring to it differed, and from recent observations he had made, he doubted the correctness of the acknowledged composition of iron rust. He further stated that if he took a bar of rusted wrought iron, and put it in violent vibrations, by applying at one end the fall of a hammer, scales would be separated which did not appear to him to be the substance I had described. This conversation induced me to commence a series of experiments which I shall now detail. I first carefully analysed some specimens of iron rust, which were procured, as far as possible, from any source of contamination. Thus one of these samples was supplied to me by Sir Charles Fox, as taken from the outside of Conway Bridge, the other secured by myself at Llangollen, North Wales. These specimens gave the following results when submitted to analysis:

	Conway Bridge.	Llangollen.
Sesquioxide of iron	93·094	92·900
Protoxide of Iron	5·810	6·177
Carbonate of iron	0·900	0·617
Silica	0·196	0·121
Ammonia	Trace	Trace
Carbonate of lime	——	0·295

These results clearly show the correctness of Sir Charles Fox's foresight, that the composition of the rust of iron is far more complicated than is stated in our text-books. Therefore the question may be asked, Is the oxidation of iron due to the direct action of the oxygen of the atmosphere, or to the decomposition of its aqueous vapour; or does the very small quantity of carbonic acid which it contains determine or intensify the oxidation of metallic iron? To reply to it I have made a long series of experiments, extending over two years, and which I hope will throw some light on this very important question. Perfectly cleaned blades of steel and iron, having a gutta percha mass at one end, were introduced in tubes which were placed over a mercury trough, and by a current of pure oxygen conducted to the top of the experimental tube; the atmosphere was displaced, and it was then easy to introduce into these tubes traces of moisture, carbonic acid, and ammonia. After a period of four months, the blades of iron so exposed gave the following results:—

Dry Oxygen	No oxidation.
Damp "	In three experiments, only one blade slightly oxidised.
Dry Carbonic Acid	No oxidation.
Damp "	Slight appearance of a white precipitate of the iron, found to be carbonate of iron. Two only out of six experiments did not give these results.
Dry carbonic acid and oxygen	No oxidation.
Damp oxygen and carbonic acid	Oxidation most rapid, a few hours being sufficient. The blade assumed a dark green colour, which then turned brown ochre.
Dry oxygen and ammonia	No oxidation.
Damp " "	No oxidation.

The above results prove that under the conditions described, pure and dry oxygen does not determine the oxidation of iron, that moist oxygen has only feeble action; dry or moist pure carbonic acid has no action, but that moist oxygen containing traces of carbonic acid acts most rapidly on iron, giving rise to protoxide of iron, then to carbonate of the same oxide, and lastly to a mixture of saline oxide and hydrate of the sesquioxide of iron. These facts tend to show that carbonic acid is the agent which determines the oxidation of iron, and justify me in assuming that it is the presence of carbonic acid in the atmosphere, and not its oxygen or its aqueous vapour, which determines the oxidation of iron in common air. Although this statement may be objected to at first sight, on the ground of the small amount of carbonic acid gas existing in the atmosphere, still we must bear in mind that a piece of iron, when exposed to atmospheric influences, comes in contact with large quantities of carbonic acid during twenty-four hours. These results appeared to me so interesting that I decided to institute several series of experiments. When perfectly clean blades of the best quality of commercial iron are placed in ordinary Manchester water, they rust with great facility, but if the water is previously well boiled and deprived of oxygen and carbonic acid, they will not rust for several weeks. Again, if a blade of the same metal is half immersed in a bottle containing equal volumes of pure distilled water and oxygen, that portion dipping in the water becomes rapidly covered with the hydrate of the peroxide of iron, whilst the upper part of the blade remains for weeks unoxidised; but if a blade be placed in a mixture of carbonic acid and oxygen, a very different chemical action ensues, as not only that portion of the blade dipping in the water is rapidly attacked, but the upper part of it immediately shows the result of chemical action, and also the subsequent chemical reactions are greatly modified by the presence of the carbonic acid. For in this case that portion of the blade is only covered with a film of carbon, together with a dark deposit composed of carbonate of the protoxide and hydrate of the sesquioxide. The fluid, instead of remaining clear, becomes turbid. These series of experiments substantiate the interesting fact observed—that carbonic acid *promotes* oxidation. A long series of experiments were also made to try and throw some light on the curious fact, first published by Berzelius, subsequently studied by other chemists, and well known to soap and alkali manufacturers, namely, that caustic alkalies prevent the oxidation of iron; my researches can be resumed as follows:—(1) that the carbonates and bicarbonates of the alkalies possess the same property as their hydrates; and (2) that if an iron blade is half immersed in a solution of the above-mentioned carbonates, they exert such a preservative influence on that portion of the bar which is exposed to an atmosphere of common air (oxygen and carbonic acid), that it does not oxidise even after a period of two years. Similar results were obtained with sea water, to which had been added carbonates of potash and soda.

Microscopical and Natural History Section, January 9.—Mr. J. Baxendell, President of the Section, in the chair. "On *Carex flava* L., and its allies, of the Manchester Flora," by Charles Bailey. The prevailing form in the district, and one very common to the south of Manchester, is the *Carex lepidocarpa* Tausch.; this is the *C. Œderi* Sm., and of Grindon's Manchester Flora, and the *C. flava* var. β of Buxton's Guide. The true *C. flava* (α *genuina* E. B.), as stated long ago by Mr. Buxton, is nowhere met with in the district. Specimens of *C. Œderi* Ehrh. from Mere Mere, the locality mentioned in Buxton's Botanical Guide, were recently exhibited at a meeting of the Society, and the sandhills at Southport are, so far as I know, the only locality in the neighbourhood for this species.

TAUNTON

Somersetshire Natural History Society, February 6.—The following notice was read by Mr. Cecil Smith.—The Great Bustard has now so long been considered extinct in England, that we may look upon it as interesting in an archæological as well as in a natural history point of view. This bird, one of a flock of eight that made their appearance on the last day of the old year at Braunton, near Barnstaple, in North Devon, is probably a young hen bird of the first, certainly not more than the second, year. On looking at this bird, perhaps, one of the first things that strikes one is the shape of the foot, the three toes in front and no hind toe; and this brings me at once to the subject of classification or order. Yarrell, whose system is best known and most generally adopted, has made the bustards a group of the Rasoreal order, where I cannot help thinking they are not a little out of place, there being no very nearly allied group in that order. I am much more inclined to agree with Baron Cuvier and some of the other older systematic authors who placed the bustards amongst the Grallatores or Stilted birds, where they seem naturally to fall into place next to their near relations, the Plovers; the absence of the hind toe, a conspicuous mark of the true Plover, would at once suggest this position. The form of the sternum or breast bone also points to a relationship with the plovers rather than with any of the Rasores. Another point which can scarcely be passed over in silence in a paper on the Great Bustard is the gulor pouch. It seems surprising that the use, and even the existence, of this pouch has so long been a matter of doubt and perplexity to naturalists. Much light has, however, lately been thrown on the subject by Prof. Newton, Mr. Bartlett, and Dr. Murie, and it seems finally to have been set at rest by Dr. Cullen, the result of whose examinations has been reported in the *Ibis* for 1865. He readily found the opening into the pouch under the tongue, and describes it as large enough to admit the little finger; the pouch itself, he says, extended as far down as the furcular bone, and was a separate and distinct, though delicate, bladder. He then describes the performances of the male bustard in the breeding season, at which time, he says, it makes a peculiar sound, resembling the word "ook," and he strongly favours the idea that the pouch is merely an organ of sound, and that it is acted upon by the muscular tissue covering it; in fact, that it is a sort of bird bag-pipe, and seems not to produce much more melodious sounds. Although the Great Bustard was formerly resident in England throughout the year, it is generally a migratory bird, its migratory propensities being much developed by stress of weather and scarcity of food; whether either of these causes or the war in France, has been the reason for the present unusual migration to England is perhaps difficult to say; myself, I should be inclined to think bad weather and the consequent loss of food the more probable cause. The gradual extinction of the Great Bustard in England has been the general theme of most of onr writers on ornithological subjects from the time of Bewick and Montagu to the present time of Mr. Stevenson, who in his still unfinished work on the "Birds of Norfolk," gives a most interesting account of the decline and fall of the Great Bustard in that county. In Devonshire, where this bird was killed, the Great Bustard, although never resident, seems from time to time to have paid occasional migratory visits. Montagu mentions the occurrence of one near Plymouth in the year 1798, two more in the next year, and one in 1864. Another Devonshire specimen occurred, after a long interval, on the 31st December, 1851, near Clovelly, and was recorded by Mr. Gatscombe, in the *Naturalist*. The 31st December seems to have been a favourite day with the Great Bustard in North Devon, for the flock, of which this present specimen was one, appeared on Braunton Burrows, near Barnstaple, on that day last year; the flock consisted of eight, and was first observed in a field near Croyde, where two were killed and one wounded. The remainder of the flock then alighted. Some boys, who were sliding close to Braunton, pelted them with stones, upon which the birds flew off, and were not heard of for some days; subsequently, I heard they were seen at Halsworthy, in the west of Devon, not very far from the border of Cornwall, but none were obtained there. In Devon they were considered to be wild turkeys, and the following paragraph appeared in the *North Devon Journal*:—"Wild fowl.—During Christmas week a flock of eight wild turkeys visited this parish, and alighted in a field at Croyde. They were seen by Mr. William Nuich, who followed and shot one which weighed 9lb., and was much admired. The others took their flight to the west, and have not since made their appearance." This migration of the Great Bustards was not confined to Devon alone, for in the February number of the *Zoologist*, specimens are reported from Middlesex, Northumberland, Wiltshire, and Somersetshire. The Somersetshire specimen is of course the most interesting to us: it was seen by Mr. Harting on the 27th of September, when journeying by rail from Bishops Lydiard to Wells in the low marsh country near Shapwick. His attention was first attracted by seeing a bird crouch at the approach of the train. He kept his eyes on the bird until the distance was considerably decreased, when the bird jumped up and ran swiftly away, exhibiting to his astonishment the long legs and white flanks of a bustard. Mr. Harting subsequently published an account of his interview with this bustard in the *Field*. He seems to have no doubt himself that the bird he saw was a bustard, and as he seems to have had sufficient opportunity to identify it, we must therefore take it that this Shapwick bustard was the pioneer of the December migrants, and that we have to thank Mr. Harting for this important addition to our Somersetshire avi-fauna. Although this migration has refreshed our memory of the Great Bustard for a time, I am afraid we must in England look upon it as a bird of the past, certainly as a resident, one or two migratory appearances like the present may no doubt from time to time stir up the ornithological world, and cause a temporary excitement; but that a bird of such considerable size and conspicuous plumage should for any time continue to exist in such a highly cultivated and thickly inhabited country as England seems impossible. Should it do so in any considerable numbers we should very soon have an outcry from some of our farmers, as it is addicted to feeding upon corn, both when green and ripe, and is moreover especially fond of turnip greens. No doubt in more ways than one the present system of high farming and the amount of machinery used is most fatal to the Great Bustard as a resident, and we must soon look upon him as having his memory perpetuated like the Druid,* only as the sign of a public house, and being classed by some future Mr. Weller with a "griffin, a unicorn, or a King's Arms, as is well known to be a collection of fabulous animals."

PHILADELPHIA

Academy of Natural Sciences, Nov. 8.—Mr. Thomas Meehan referred to a potato presented to the Academy some months ago by Mr. Henszey, a member, which had the appearance of one potato growing out of the centre of another. The opinion of all who saw it was that it was really a case of this kind. It had been handed to him by the curators, and on dissection, though no exact place of origin could be traced, there seemed nothing to indicate any other theory of origin than that one potato had really grown out of the centre of the other. But there were serious physiological reasons in the way of such a theory. A potato tuber is really but a thickened axis, in which the greater part of the interior structure would be incapable of developing a bud which would produce a tuber such as this one had done. The origin of a new tuber from an old one would be nearer the old one's surface. He had been looking for some further explanatory facts, and believed he had them here this evening, in the potato tubers he now handed to the members. They were about the size of hen's eggs, and were pierced in every direction by stolons of the common couch grass, *Triticum repens*. They had gone completely through, as if they were so much wire, and in one instance two tubers had become strung together by the same stolon, as if they were two beads on a string. One would suppose that the apex of the stolon, when it came in contact with the hard surface of the tuber, would turn aside and rather follow the softer line of the earth; but there was no appearance of any inclination to depart from their direct course. They had gone apparently straight through. He had no doubt the potato before referred to was a similar case, a potato stolon had penetrated another potato, and instead of going through as these grass spears had done, terminated in the centre, and formed the new potato there. It was worthy of thought whether so much attention had been given to this direct force in plants as the subject deserved. It was well known that a mushroom would lift a paving-stone many times its own weight, rather than turn over and grow sideways, which it would appear so much easier for it to do; and tree roots growing against walls would throw immensely strong ones over, though one would think the pressure against the softer soil would give room for their development, without the necessity of their expending so much force against the wall.

November 15.—Dr. Ruschenberger, President, in the chair. Prof. Leidy directed attention to some fossil bones which had been submitted to his examination by Prof. J. D. Whitney. According to the accompanying label, they were found under Table Mountain, near Shaw Flat, Tuolumne Co., California. The bones are friable, and have attached portions of a light ash coloured gravel. Several masses of the latter substance accompanying the bones, contain casts of some fruit. Prof. Leidy further directed attention to a fossil fragment of the lower part of a small pachyderm, which Prof. Hayden had obtained from Henry's Fork of Green River, Wyoming. The specimen contained the fourth, the sixth, and the seventh molars. The teeth resemble in form and constitution those of the *Lophiotherium cervulum*, a small pachyderm, described by Prof. Gervais, from an Eocene formation of France. The crowns of the fourth to the sixth molars have four lobes; that of the seventh molar has an additional lobe. The crescentic summit of the postero-external lobe joins, by its anterior horn, the antero-internal lobe. A proportionately well developed basal ridge embraces the crowns, except internally, where it is entirely absent. The series of the back four molars occupies a space of 16 lines. The last molar is $5\frac{1}{2}$ lines fore and aft. The base of the jaw is nearly straight the length of the fragment, which is an inch and a half. The depth of the jaw below the fifth molar is about an inch. The species may be named *Lophiotherium sylvaticum*.

November 29.—Dr. Ruschenberger, President, in the chair. On "Bud Varieties," by Thomas Meehan.—A few years ago Mr. Isaac Burk called my attention to a form of *Rubus villosus*, L., in which the terminal leaflet was very large, cordate, and on very long petioles. It is a very striking variety, the leaflets appearing at first glance like large linden leaves. On the idea that varieties originated from one common centre, it is not easy to account for the existence of the same forms so many miles apart, as we find in the above, except by the accidental carrying of seeds. But I have reason to believe that seeds of *Rubus* rarely germinate in a wild state. In experiments which I have made in raising the seeds artificially, none of the seedlings come exactly like the parent. There is a certain general resemblance, but some distinction, more or less, can be traced in each individual. But, in native places, one exact form will be found to occupy extensive tracts. Sometimes several forms will be together, but only a very few. If the seeds made plants readily, there would be innumerable forms, instead of the very few we see. I found, in my experiments, that it took a long time for a blackberry seed to germinate; sometimes a whole year. Such seedlings have a poor chance to vegetate in a state of nature. Other more rapidly-growing vegetation would crowd it out. The only distributing agency I can think of is that of birds. But I find no birds eat blackberry seeds; and, if they did, when we consider that of the millions of seeds which fall about the place of their origin, few, if any, grow; the chance of those growing which birds may carry, even if there be some to eat them, which I have failed to find, is extremely small. Hence, we find great difficulty in believing that identical forms of *Rubus*, widely separated, can have originated from a common centre. It is well-known that fruits, after being grown for some time in one locality, will change their characters to such an extent that a person acquainted with one will fail to recognise it elsewhere, and all this without the intervention of any seminal power. Thus, the nectarine is believed to be a bud evolution from the peach; the Penn apple is a similar creation from Baldwin, and the Reading from the common Isabella grape. Though apparently originating in this way from external or local causes, the characters peculiar in this

* The Druid and the Bustard are both the signs of public-houses on Salisbury Plain.

change are retained when, by grafts or cuttings, the plants are removed to other localities. I have here, however, and exhibit with this paper, evidence of bud variation, in which there is no possibility of hybridism,—a root of the common sweet potato, *Convolvulus batatas*, in which some of the tubers are of the red Bermuda, and the others of the white Brazilian variety. The sweet potato never flowers in this part of the country, so that seminal power could have had no influence whatever on the phenomenon. Even in the south, and I believe elsewhere, where this plant is cultivated for its roots, it rarely flowers, and I think there is little doubt but that the whole ten or twelve varieties under culture have originated without seed, and in the way we see them here. The points I wish to make in this paper are :—1st. That identical varieties sometimes appear in localities unfavourable to the idea of a common centre of origin. 2nd. Varieties have originated in which probably no hybridism or any seminal agency operated. 3rd. Varieties have certainly originated in the sweet potato by evolution, without seminal agency, and that the same variety in this way has appeared in widely-separated districts. 4th. As the discoveries of Darwin have shown, in many cases, varieties to be the parents of species, species may originate in widely-separated localities by bud variation.—" A Sketch of the Classification of the American Anserinæ," by B. H. Bannister. The following remarks are based upon an examination of the specimens of American geese in the collection of the Smithsonian Institution. The subfamily Anserinæ by many recent authors is made to include the genera *Dendrocygna* and *Chenalopex*, and doubtlessly correctly. In the present paper, however, we shall not consider these genera, leaving them provisionally out of the sub-family ; if included, they would form at least one well-marked section, following those we are about to describe. The distinguishing characters of the Anserinæ, as thus limited to the true geese, are, the lengthened tarsus, covered with hexagonal or subquadrate scales ; the neck more elongated than in the ducks and less so than in the swans ; the short, high bill gradually narrowing toward the tip, which is altogether composed of the large recurved nail ; together with the more or less terrestrial habit of life, and the usually similar plumage of the two sexes. The geese of the North American continent have been long known, and being for the most part closely allied to, and in many cases identical with, well known European forms, they fall readily into the systematic subdivisions based upon the latter. Another basis of division of the American Anserinæ is found in the presence, in two species—one North American and the other a Southern form—of deep rough superorbital depressions and reversed relative proportions of the tarsus and middle toe, together with an exclusively sea-coast habitat, and a carnivorous diet, corresponding in some of these respects to the Oidemiæ and Somateriæ amongst the ducks. These latter characters we have taken as the basis of the two sections into which we divide the subfamily, as at present considered, since they correspond with equivalent characters in one of the subdivisions of the Fuligulinæ. The presence of the deep superorbital depressions is a very general character amongst the carnivorous Natatores, though not universal.

BOOKS RECEIVED

ENGLISH.—The Year-Book of Facts for 1871 : J. Timbs (Lockwood and Co.).—A Treatise on the Action of Vis Inertiæ in the Ocean : W. L. Jordon (Longmans and Co.).

FOREIGN.—(Through Williams and Norgate)—1er Nachtrag zum Lehrbuche der Aufbereitskund : P. R. von Rittinger.—Biblioteca Malacologica, II., Ipsa Cheireghinii Conchylia di Spiridion Brusina – Populäre wissenchaftliche Vorträge : H. Helmholtz.

PAMPHLETS RECEIVED

ENGLISH —Quarterly Weather Report of the Meteorological Office, July-October 1869.—On the Relations between Chemical Change, Heat, and Force : the Rev. H. Highton.—On Ocean Currents : James Croll.—On the Cause of the Motion of Glaciers : James Croll.—Letter to the Right Hon Col. Wilson-Patten on the Future Establishment and Organisation of our Land Forces : Lieut.-General Sir Percy Douglas, Bart.—Report of the Cheltenham College Natural History Society for the year 1870.—Statistical Review of Ten Years of Disease in Manchester and Salford : Dr. A. Ransome.—Double Spectra : W. Marshall Watts.—On the Spectra of Carbon : W. Marshall Watts.—On the Reason why the Difference of Reading between a Thermometer exposed to Direct Sunshine and one Shaded Diminishes as we Ascend in the Atmosphere : James Croll.—An Address read at the Anniversary Meeting of the Entomological Society of London, January 23, 1871 : Alfred R. Wallace.—On the Chemical Composition and Microscopic Constitution of certain Cornish Rocks : J. A. Phillips.—Proceedings of the Somersetshire Archæological and Natural History Society for 1868-69.

AMERICAN AND COLONIAL.—Report of the Present Condition of the Geological Survey of Victoria.—Reports of the Mining Surveyors and Registrars (Victoria) for the Quarter ending September 39, 1870.—Abstracts of English and Colonial Patents and Specifications relating to the Preservation of Food, &c. : W. H. Archer (Melbourne).—Patents and Patentees (Victoria), 1814-1865 ; Index to ditto for 1868 and 1869 ; Abstracts of Specifications of Patents applied for from 1854 to 1866, Ac-Bu (Victoria) : W. H. Archer.—Descriptions of new Fossil Shells of the Upper Amazon : T A. Conrad (from the *American Journal of Conchology*).—Report of Committee on New Remedies to the Muskingum County, Ohio, Medical Society for October, 1870 (Buffalo).

FOREIGN.—Die Geschichte der Forschungen über die Phosphorite des mittlern Russlands von W. v. Gutzeit (Riga).—Rend'conti del reale istituto lombardo, Ser. ii. vol. 3, fasc 19, 20, and vol. 4, fasc. 1.—Correspondenzblatt der Naturforscher-vereins zu Riga.

DIARY

THURSDAY, FEBRUARY 16.

ROYAL SOCIETY, at 8.30.—On some of the more important Physiological Changes induced in the Human Economy by Change of Climate, as from Temperate to Tropical, and the Reverse (concluded) : Dr. Rattray, R.N. —On a Registering Spectroscope : Dr Huggins, F.R.S.
SOCIETY OF ANTIQUARIES, at 8.30.—On the Topography of Jerusalem, with special reference to the results obtained by the Palestine Excavation Fund and the Site of the Temple of Antonia and of the Acra : Thomas Lewin, M.A., F.S.A.
LINNEAN SOCIETY, at 8.—On Tremellineous Fungi and their Analogues : L. R. and C. Tulasne.—Bryological Remarks : S. O. Lindberg, M.D.
CHEMICAL SOCIETY, at 8.
ROYAL INSTITUTION, at 3.—Davy's Discoveries : Dr. Odling.

FRIDAY, FEBRUARY 17.

ROYAL INSTITUTION, at 9.—On the Wolf-Rock Lighthouse : James N. Douglass.
GEOLOGICAL SOCIETY, at 1.—Anniversary Meeting.
ROYAL COLLEGE OF SURGEONS, at 4.—On the Teeth of Mammalia : Prof. Flower.

SATURDAY, FEBRUARY 18.

ROYAL INSTITUTION, at 3.—Socrates : Prof. Jowett.

SUNDAY, FEBRUARY 19.

SUNDAY LECTURE SOCIETY, at 3.30.—On the Religion of Health : Dr. Elizabeth Blackwell.

MONDAY, FEBRUARY 20.

ENTOMOLOGICAL SOCIETY, at 7.—On the Dispersal of Non-migratory Ins by Atmospheric Agencies : Mr. Müller.
VICTORIA INSTITUTE, at 8.—Phyllotaxis ; or the Arrangement of Leaves according to Mathematical Law : Prof. Henslow.
ROYAL COLLEGE OF SURGEONS—On the Teeth of Mammalia : Prof. Flower.
LONDON INSTITUTION, at 4.—On the First Principles of Biology : Prof. Huxley. (Educational Course.)
ROYAL UNITED SERVICE INSTITUTION, at 8.30.—On the Turret Ships now building for Her Majesty's Navy : E. J. Reed, C.B.

TUESDAY, FEBRUARY 21.

ZOOLOGICAL SOCIETY, at 9.—Note on the *Tænia* from the Rhinoceros : Dr. W. Peters, F.M.Z.S.—Remarks on certain species of Abyssinian Birds : J. H. Gurney.—On certain Indian Reptiles : Dr. J. Anderson.
STATISTICAL SOCIETY, at 7.45.—On Currency and Pauperism : Mr. Ernest Seyd.
ROYAL INSTITUTION, at 3.—Nutrition of Animals : Dr. Foster.

WEDNESDAY, FEBRUARY 22.

SOCIETY OF ARTS, at 8.—On Water Meters : F. E. Bodkin.
GEOLOGICAL SOCIETY, at 8.
ROYAL COLLEGE OF SURGEONS—On the Teeth of Mammalia : Prof. Flower.
ROYAL UNITED SERVICE INSTITUTION, at 8.30.—The Organisation of our Military Forces : Lieut.-Colonel Arthur Leahy, R.E. (Adjourned Discussion.)

THURSDAY, FEBRUARY 23.

ROYAL SOCIETY, at 8.30.
SOCIETY OF ANTIQUARIES, at 8.30.
ROYAL INSTITUTION, at 3.—Davy's Discoveries : Dr. Odling.
LONDON INSTITUTION, at 7.30.—On the Action, Nature, and Detection of Poisons : F. S. Barff, M.A., F.C.S.

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