THURSDAY, DECEMBER 22, 1892.

MR. C. DIXON ON BIRD-MIGRATION.

The Migration of Birds: an Attempt to reduce Avian Season-Flight to Law. By Charles Dixon. (London: Chapman and Hall, 1892.)

AMONG prevalent fallacies there are few more mischievous than that which holds a man to be an authority on a subject because he has written a book about it. If the subject be one concerning which the scientific hold divers opinions, or even hesitate to deliver an opinion at all, so much is to the good of such an author, for he will be able to pose all the more securely in the character of a *savant*—though after all that only signifies a "knowing one." If the author can boast of some two, three, or even half-a-dozen works already published, the fallacy becomes almost insuperable, notwithstanding that in zoological works of a popular nature, it is scarcely too hard to say that those who write the most know the least. Nevertheless it remains the duty of the conscientious reviewer to be instant in season with his protest against this general confounding of author with authority. We have read several of Mr. Charles Dixon's works, but hitherto we have been so fortunate that we have been able to keep *in petto* the judgment we have formed of them. It is not given, however, even to reviewers to struggle against fate, and it has been ordained that we should have to criticize his recent volume, the title of which may be read above. To the first sentence of his preface—"There is no branch of Ornithology more popular than that which treats of the Migration of Birds"—we offer no strong objection, and rejoice that there is one spot of ground, be it never so small, that we may occupy in common; but (woe it is!) that here we must part company, for the very next sentence contains a statement which we would willingly let pass as a harmless exaggeration, were it not intensified by the words which follow—and "after that, the dark"!

Mr. Dixon's acquaintance with the subject he has selected is shown by the beginning of his second paragraph—"Notwithstanding the immense popularity and importance of Migration, strange as it may seem, no work has hitherto been devoted expressly to its discussion." He is therefore not aware of the essays of Schlegel and of Marcel de Serres, which (whatever we may now think of them) were in their time "crowned" by the scientific society that published them, and though he straightway proceeds to name the works of Professor Palmén and Herr Gätke, it is to complain of them that they "have only dwelt upon a portion of the subject." Far be it from us to say that Mr. Dixon has not read their works, but really there is nothing to show that his knowledge of them is more than may be picked up from the extracts which have been translated into English and published in this country, or that he has read them to any purpose—that of Herr Gätke especially, because, when further on (pp. 181–186) he comes to deal with it more particularly, he regards it as if it were a mere record of captures or reputed captures of birds in Heligoland, speaking of it with contempt, and the original and rather peculiar views on migration of its author are passed over in silence, as though they were utterly unknown to him. Mr. Dixon states that he is "equally cognizant of the researches of Weissemann" (*sic*) and others, which, except that Dr. Weismann, we think, would deny his having made any, we do not take upon ourselves to gainsay, though our older writers are utterly ignored, and we have a shrewd suspicion that the anonymous author of the "Discourse on the Emigration of British Birds," published at Salisbury more than one hundred years ago, was, from actual observation, more familiar with the main facts than Mr. Dixon is—all flourishes about "avian fly-lines" and "season-flight" notwithstanding—and therefore would have been more competent than he "to bring our knowledge of Migration within the limits of order or to reduce it to Law."

Now this is exactly what in our opinion Mr. Dixon has not done. What the "Law of Migration," of which we read here and on the title-page, may be it passes us to discover. The phrase is full of sweetness, but its elucidation, if we may say so, fails in light. So also is that about bringing our knowledge "within the limits of order," though that may be here taken to mean a dissertation within the limits of 300 pages or thereabouts containing something on the origin and descent of birds, a good deal about the precession of the equinoxes and the eccentricity of the earth's orbit, but still more about glacial epochs. Concerning the "Law of Migration" it is pointless. Let our author at once speak for himself in what seems to be a sort of summary of his faith, though it is long and not reserved to the end of his volume:—

"We will now conclude by following in detail the migration of some single species, say from its Post-Pliocene glacial initiation to the present day, in order clearly to demonstrate Why the habit [of migration] has been acquired, and How it is practised.

"We will select the Spotted Flycatcher (*Muscicapa grisola*) for the purpose. It is one of our best known summer migrants, and one whose present geographical distribution admirably illustrates the phenomenon of Migration. When the Sub-Polar regions of the northern hemisphere last enjoyed a warm, almost semi-tropical climate—one of the mild periods of the Glacial Epoch—the Spotted Flycatcher inhabited in one unbroken area the Arctic woodlands from the Atlantic to the Pacific. Probably it was a resident species becoming partially nocturnal during the Polar night; food was abundant; its conditions of life were easy, and it multiplied apace, and became a dominant, firmly established species during the thousands of years that it dwelt in this Sub-Polar habitat. So matters continued until the slow precession of the equinoxes, in conjunction with increasing eccentricity of the earth's orbit, began to have a marked influence on the climate, and gradually the fair forests and the verdant plains were devastated by the ever-increasing cold. Age after age the Spotted Flycatcher was driven slowly south; summer after summer grew colder and shorter, the periods of Polar darkness more severe. At last matters became so serious that the birds began to leave their northern haunts in autumn, probably because their food became scarce as the various insects either retreated south or began to hibernate. Further and further southward these annual journeys had to be taken, until the Flycatcher at last found its way during winter into Africa, Persia, Arabia, India, China, and even the Philippines and the Moluccas. Summer after summer the belt of breeding-ground became wider and wider, and vast numbers of individuals became separated from the rest of the species by the lofty mountain ranges, the deserts, and other physical barriers, which would effectually assist a forest or woodland haunting species. More and more severe became the winters, longer and longer; the glaciers descended lower and lower, exterminating or driving before them all living things. At last the Spotted Flycatcher, or the form which then represented this species, came to be divided into two enormous colonies—an African one and a Chinese one—the individuals of each being completely isolated from each other, summer and winter alike. During the ages that this state of things continued, the Flycatchers became segregated into two species, owing primarily to the absence of any intermarriage; the eastern race became smaller, the tail shorter, and the breast-streaks broader; or the western race became larger, with a longer tail and narrow breast-streaks. It is almost impossible to say which form now most closely resembles the ancestral species; but such are the present differences between the two races known to ornithologists respectively as *Muscicapa grisola* (the Western and British form) and *Muscicapa griseisticta* (the Eastern form). Such was the state of things at the close of this Inter-Glacial Period.

"Then came the gradual immigration north again, as precession and lower eccentricity initiated a milder climate. Age after age the journey in the spring became longer. Certain routes to and fro came to be recognized highways of passage; and so imperceptibly did the northern breeding grounds expand that the birds became regular migrants, looking upon the movement north to higher and cooler latitudes each spring as an undertaking never to be missed. Warmer and warmer became the southern haunts, stimulating and widening migration flight to the cooler temperatures prevailing near the edges of the retreating glaciers, where a suitable breeding climate could only be found.

"Let us confine our attention solely to the birds that bred in the British Islands. In the Præ-Glacial ages this area formed part of Continental Europe; a rich and fertile corner, abounding in insect life, full of haunts the Flycatcher loved. After the banishment of its race and the exile of its ancestors in Africa, the northern journey at first did not extend further than the edges of the glaciers on the Mediterranean coasts of Europe. But as these disappeared, and a warmer climate began to prevail in higher latitudes, the annual summer flight was increased. Every century the northern breeding range had increased, creeping slowly across France; higher and higher with the growing vegetation; nearer and nearer to the haunts of old. During the slow, gradual elevation and submergence that isolated Albion from the rest of Europe during Post-Glacial time, the regular spring journey across the sea became wider and wider; but with the intense and inherited love of home in their tiny breasts, the individuals that were born and bred in this district never failed to return each year. For 60,000 years or more has this species now crossed the sea, returning every season, not only to our islands, but each pair of individuals, as long as they live, come back to the exact locality of their previous nests. This long journey, gradually growing longer and longer during thousands of years, until it is now at least a thousand miles in length, has grown to be a deeply-rooted custom sanctioned by the practice of ages of experience and need, and looked upon now as part of the Flycatcher's very existence!" (pp. 58 62).

This, we think, is Mr. Dixon at his best, and we are anxious that our readers should so see him. He goes on to call it a "thoroughly demonstrable instance," which shows what his idea of a demonstration is. We do not deny that all may have happened as he here prescribes, but who knows that it did? To begin with, we may ask what proof is there of the existence on the earth itself of *Muscicapa grisola* "when the Sub-Polar regions of the northern hemisphere last enjoyed a warm, almost semi-tropical climate"? That its ancestors then lived we do not doubt, but who can tell us what they were like? What is meant by its "becoming partially nocturnal during the Polar night"? If so its eyes must since have undergone a considerable change, and that would hardly be unattended by a corresponding change in other parts of the bird's structure. But still it is a pleasing suggestion that "its conditions were easy" in those millenniums, and we hope Mr. Dixon may be right, though for our own part we cannot help fearing that the struggle for existence must have already begun. Certainly it set in at last, and those terrible glaciers drove the poor bird before them, with the effect—Mr. Dixon, we think, is to blame for not giving us the geographical details (which of course must be known to him) of the process—of dividing the species or the form which represented it, and may be presumed (though this is not mentioned) to have by that time got rid of its owls' eyes, "into two enormous colonies—an African one and a Chinese one." These were so isolated that inter-marriage between the individuals of the two portions was impossible, the remarkable consequence of which was that "the Eastern race became smaller" than the Western—a character distinctive indeed of the human races, the Pygmies excepted, now inhabiting the same lands—but with "the tail shorter"—a contradictory character, since the long tail of a Celestial is the really important part of him. We are also told that "it is almost impossible to say which form now most closely resembles the ancestral species," an unexpected confession of ignorance (the "almost" is good) after so much information, but one to which we see the necessity of bowing. However, what is the upshot of all this? And how is any "law" illustrated by it? Setting aside the vagaries on which we have just commented, it reads to us as being merely an amplification of suggestions that were tentatively and cautiously submitted in these columns more than eighteen years ago (NATURE, vol. x. pp. 416 and 459). The partiality of birds for their old homes was then, and (so far as we know) for the first time, pointed out as one possible factor in establishing migratory habit; and, as another (and equally for the first time), the growing divergence of breeding and feeding areas through climatic causes was briefly and clearly set forth by Mr. Wallace. Notwithstanding Mr. Dixon's assertions, he does not seem to have advanced the question one bit, but he has overwhelmed it in a flow of words with a great deal that is, and apparently always will be, incapable of proof. Here and elsewhere throughout this volume we are brought to face one of that school of biologists, the growth of later years, which may be called the Assertive. In some respects it is a very nice one to join. You have only got to say what first comes into your head, and all goes well. Everybody that differs from you is a fool. To some extent this school resembles that Dogmatic one which a few naturalists here and there still remember, inasmuch as the dissentient from either was regarded with the same contempt. The Dogmatists have had their day, but if we look back upon their doings, we shall see that in most cases they had something to go upon which was not entirely assertion. They were very fond of facts, and undoubtedly preferred founding their dogmatism upon them—indeed, nothing could be more distasteful than to suppose each dogma had not a sound basis. In most cases the worst of which they can even now be accused is that the facts were often above their comprehension, or were understood in the wrong sense. But these men would have scorned the grounding of their dogmas upon imagination. They were perfectly aware (only it had not then been so neatly put) that "Imagination is the fire of Discovery: the best of servants though the worst of masters." Now the Assertive school, of which in this country Mr. Dixon, if he was not the joint-inventor, may be looked upon as a chief leader, rests nearly all on imagination. It matters little whether there is reason behind their assertions or not, and generally, we regret to say, there is none. Conjectures follow upon conjectures and are put forward for the most part as if they were serious deductions from observation. It is not so many weeks since some words, that seem very applicable here were addressed to a scientific audience:—

"We have had enough of the untrained writer of papers, the jerry-builder of unfounded hypotheses whose ruins cumber our field of work."[1]

Mr. Dixon, with his long string of previous books, may demur to being termed a writer of this kind; but he certainly needs to be taught the meaning of the word "probable" and its derivatives. When he has learned it perhaps he will use it in its fit sense. With him, at present, it is in many cases to be rendered "possible," while in not a few *impossible* would be the true equivalent. Now according to all etymologists, and the harmless drudges known as dictionary-makers, "probable" signifies *something that can be proved*. Any reader of average intellect will be able to calculate how seldom this unhappy word is correctly used by Mr. Dixon. It has long been a custom in certain fevers to affix an ice-cap on the patient's head whereby the burning brow is cooled, and some temporary relief afforded; but of late years, as pretty well all know, there has sprung up a small group of writers to whom ice on the brain, instead of being a soothing remedy, is a direct incentive to acts and dicts bordering upon lunacy. On behalf of the Glacial Epoch, the Post-Pliocene Glacial Epoch, to be very particular, we must protest against its being constantly paraded as the greatest event in the history of the globe, to which in its momentous effects all others must give place. That it produced considerable changes and especially in the geographical distribution of plants and animals none can doubt, but that it is accountable for all that Mr. Dixon lays to its charge is hardly likely, and is most decidedly not "probable," since means of proof are wanting. But Mr. Dixon, with others of the Assertive school, is not consistent in his statements, and is apt to forget on one page what he has written on a preceding one. For instance, we are told (p. 33) that "From the commencement of this Glacial Epoch, the Migration of birds, as we see it at the present time, was probably initiated"; and yet, only a few lines further on, our author declares "that we do not require even the occurrence of one Glacial Epoch to account for the Migration of birds," and (p. 34) that "such a cause amply sufficient in every respect is to be found in the varying places of Earth's [*sic*] orbital eccentricity in combination with the precession of the equinoxes"—this statement being immediately followed by a passage, the application, or even the meaning, of which is not easy to understand:—

"That these majestic phenomena are in any conceivable way connected with the migratory movements of birds seems utterly impossible; but in them the habit has its root; and the simple season-flight of a Cuckoo or a Nightingale to and fro between the shores of Africa and England is inseparably and directly connected with the erratic movement of a planet in its orbit; nay, with the constitution of a universe!"

This note of admiration is our author's own: far be it from us to impair its influence.

Though we have confined our remarks to the earlier part of Mr. Dixon's book, we have already devoted a good deal of space to him. There is, however, another point on which we must say a few words. He has thrown out a direct challenge to NATURE, and we should be sorry not to meet it. That he believes in migration the whole volume shows; but there is yet left in his mind a cranny wherein lurks what we may perhaps call a "pious opinion" in favour of torpidity—as a luxury in which a lazy bird may occasionally indulge, even though that bird may be one possessing powers of flight far beyond the average. He is very severe on an anonymous reviewer in these columns in that the "Theory," we use Mr. Dixon's word, of Torpidity "was subjected by him to the bitterest ridicule and denounced as folly." Thereupon he favours us (pp. 12, 13) with another version (substantially, let us say at once, the same as the original, but with fewer details) of the story told by the Duke of Argyll in these pages (NATURE, vol. xv. pp. 527, 528) to say nothing of some other observations, quite irrelevant, as it seems to us, communicated by his Grace to him. But further than this, he cites as an additional witness in defence of the impeached "Theory," Dr. Elliott Coues, who is said to give it "all the support of his authority as an ornithologist of the highest eminence." Now we have a great respect for that gentleman, but his vast reputation fails to hypnotize us, and such support as he gave has already been the subject of comment in these pages (NATURE, vol. xx. p. 2). He will hardly be comforted to learn that the supposition there made has been amply confirmed of late by Mr. Hartert, who informs us (*Cat. B. Brit. Mus.* vol. xvi. p. 481) that the British Museum contains five specimens of *Chætura pelagica* from Central America, beside the one before noticed from Mexico—proving that its range is much about what might have been expected. Thus all the argument based on Dr. Coues's statement, that this species was "not known to winter anywhere out of the United States, nor is it found anywhere in them at that season," falls to the ground, as we are sure that gentleman will readily admit. We allow that it has been very naughty of naturalists if they did prepare this pitfall for Mr. Dixon; but that is not our business, and we cannot imagine they did it intentionally. It is not unlikely that the Chimney-Swift flew out of shot, or too fast for them to bring it down, but they have at last succeeded in "grassing" their bird, with a result so disastrous to the "Theory." One chance

[1] British Association for the Advancement of Science. Edinburgh, 1892. Address of the President of Section H (NATURE, vol. xlvi. p. 379).

yet remains for our author, for though "unfortunately no direct evidence of torpidity has ever come under" his own observation, the *Dundee Advertiser* of April, 1884, supplies him with another straw at which to grasp—not that the newspaper-writer saw it at all in that light, for he called the bird in question a "wanderer," which term is innocently repeated by Mr. Dixon, apparently unconscious that thereby he gives up his case, and drown he must unless some one throws him a life-belt. Meanwhile that of the "hibernating" bird is as desperate. Deprived by the brutal sceptic of its ancient refuge in the depths of Lapland lakes, or in the crumbling banks of Persian rivers, in the mud walls of Orkney or in Irish dung-heaps, or even in nests of its own building in sea-girt Schleswig-Holstein, the *fin de siècle* Swallow desirous of enjoying torpidity has to betake itself to the security of the Bell Rock Lighthouse, and even there to excite no particular astonishment on the part of the honest men who welcomed it. If they had looked upon it as the spirit of Robert Stevenson, or that Abbot of Aberbrothock whose memory was blessed by mediæval mariners, there would have been some excuse for them, but they simply regarded this Swallow as the proverbial one that doesn't make a summer—it was the 12th of March. They will, we think, learn with surprise from Mr. Dixon that "this bird may probably have spent the winter, dormant, near the lighthouse," while he considers "that we here have the most trustworthy evidence of a positive kind." If this does not indicate hibernation capabilities amongst certain birds, pray to what else can it be attributed?" (p. 16). We leave our readers to answer this question as they please, but we fear their answer will not please him. They may, however, like to know how the incident was recorded in the Migration Committee's schedule by the matter-of-fact observer:—"1884, March 12th. One Swallow (Swift) 4 p.m. [Wind] S.E., strong B[reeze] [Weather] cloudy. Arrived much exhausted." No more and no less.

Returning to the position whence we started we must express our deliberate conclusion that Mr. Dixon, author of so many works as he may be, is no authority on the subject of Migration, which he has left exactly as he found it. In the hope he entertains that his volume may form "a basis for more elaborate study and detailed research" we entirely concur. On one, and that the most wonderful part of the whole business, the faculty whereby birds are enabled to perform their extended flights with such punctuality and general unerringness that the more one knows of the subject the more one is amazed at it, he is silent, for it would seem that there are even bounds to his imagination, and for this we are thankful.

DOMESTIC ELECTRIC LIGHTING.

Domestic Electric Lighting, Treated from the Consumer's Point of View. By E. C. De Segundo, Assoc.M.Inst. C.E. (London: H. Alabaster, Gatehouse, and Co., 1892.)

THE author of this small volume is of opinion that there is at present no literature obtainable to enable the untechnical public to form a judgment as to the suitability of applying electrical energy to meet their various requirements, and he states that at present the extended applications of electricity are largely engrossing attention, but owing to the conflicting views expressed by those ignorant or interested, there is a probability of a feeling of disgust being engendered for all things electrical in the lay mind. No doubt there is a good deal of truth in this statement; on the other hand, those who take any interest in this all-important subject will soon learn enough to be able to discern that which is important and worth knowing. The volume under notice has been written in order to fill this want in electrical literature, and to teach the consumer of electrical energy something of the source of the light and power he is using.

It is admittedly a difficult task to make a technical subject clear to untechnical, though interested, readers, and in this case all the more so, on account of the extreme technicalities of electricity as applied to everyday requirements.

The author begins at the very beginning, and in Chapters I. and II. deals with the lighting of rooms with gas, oil, and electricity, naturally pointing out how very soon the atmosphere is vitiated by the two former illuminants, besides the damage done to paintings, book-bindings, &c. When discussing "How shall I light my house best?" the author treats of the efficiency and cost of the different illuminants, and points out that although the electric light may be the more expensive, yet the cost per lamp per hour may be fairly compared with oil or gas, because these illuminants are seldom turned completely out when a room is temporarily empty, whereas the facility of switching on and off an electric lamp must naturally save the current and effect economy.

Chapter III. consists of a short description of some of the systems on which electric light is produced and supplied. Great diversity of opinion exists as to which is the best system on which electric energy should be supplied for public use. In London two systems are in vogue, viz., The high pressure alternating current in conjunction with transformers, and the low pressure continuous current system. From the consumer's point of view, so long as the high pressure current is not allowed to enter the house, it matters little what are the conditions of distribution, provided a continuous low pressure direct supply or a low pressure alternating current from a transformer station is delivered to him, except, of course, where motors are in use, and then the continuous current is a necessity. For installations in the country, separate generating plant is required, and for small installations, where on the average fifteen lamps of 8 c.p. are in circuit at a time, the author says that the electrical energy can be economically generated by chemical means. The primary battery referred to is a modified form of Bunsen, and the cost of producing the light is stated to be one penny per lamp of 8 c.p. per hour. No doubt such a battery takes the place of engine, dynamo, and accumulators, but it is purely a matter of opinion as to the trouble and skill required to look after these batteries, and, without taking the prime cost into account, a gas or oil engine driving a dynamo and charging accumulators to run these fifteen 8 c.p. lamps should not cost fifteen pence per hour. A case is known where an Otto domestic gas engine is easily driving seven and sometimes eight 8 c.p. lamps, and consuming 24 cubic feet of gas per hour, at a cost of considerably under one penny per lamp, the lamps being run direct off the dynamo.

The progress of private electric lighting is simply astonishing, the gas engine being generally the motor used. Many of these engines are run by the Dowson gas, made on the spot, thus rendering a supply of illuminating gas unnecessary.

The author treats somewhat in detail the cost of electric lighting in a house, but as this largely depends on the type of pendants, brackets, &c., used, the outlay naturally varies considerably. The most serious item in the maintenance of an installation is the breakage of the lamps. The author rejoices that this monopoly of manufacture will soon expire, when competition will place better lamps on the market at half the present cost.

Taken as a whole this little book is interesting and useful. It will certainly help the uninitiated consumer to study intelligently the principles of electric lighting, and render his conception of necessary expenses when installing the light more sensible.

OUR BOOK SHELF.

Grasses of the Pacific Slope, including Alaska and the adjacent Islands. Part I. By Dr. Geo. Vasey, Botanist, U.S. Department of Agriculture. 8vo, 50 plates, with descriptions. (Washington: Government Printing Office, 1892.)

THE botanists of the United States Department of Agriculture are working very energetically, and the importance of the herbarium, library, and its publications is increasing year by year. The present work is part of a series of illustrations of the North American grasses, of which we have already noticed two parts, together making one volume, devoted to descriptions and figures of the characteristic species of the South-Western States. The present part, which will constitute half a volume, is devoted to California and the Western States. Dr. Vasey tells us in his introduction that the grasses which are known to grow on the Pacific slope of the United States, including Alaska, number not far from 200 species, which is nearly twice as many as we get in the British Islands. They are all specifically distinct from the grasses growing east of the Mississippi, and also mostly distinct from the grasses of the plains and of the desert, except in that part of California which partakes of the desert flora. A considerable number of the grasses of the mountainous regions of California, Oregon, and Washington reappear in the mountains of Idaho, Montana, and the interior Rockies. The interior of California is a dry region, verging in the extreme south into the desert country, and is deficient in grasses, especially of those species which form a continuous turf. In the present publication fifty of the most interesting species are described and illustrated.

The descriptions are almost wholly the work of Dr. Vasey's assistant, Prof. L. H. Dewey. The illustrations are excellent, and are the work of various artists—F. Muller, W. R. Scholl, T. Holm, and others, and are accompanied by full dissections. The species range under the genera as follows:—Imperata, 1; Panicum, 1; Cenchrus, 1; Phalaris, 2; Hierochloe, 1; Aristida, 1; Spipa, 9; Oryzopsis, 2; Muhlenbergia, 5; Alopecurus, 7; Agrostis, 6; Calamagrostis, 10; Deschampsia, 1; Trisetum, 3; Orcuttia, 2. Only two out of the fifty species are British, *Alopecurus geniculatus* and *Deschampsia cæspitosa*.

J. G. B.

Aids to Experimental Science. By Andrew Gray. (Auckland: Upton and Co., 1892.)

THE chief interest of this little book lies in the fact that it gives a glimpse of the science teaching in one of our colonies. It is a compilation of simple experiments in mechanics, physics, chemistry, physiology and health, and agriculture, to prepare students for what is known as the Class D examination. Naturally, most of the experiments are old ones, but here and there one may gather a new idea. The portion dealing with physiology and health has nothing to do with dissection, but consists of experiments on ventilation, drainage, food stuffs, and the like. An interesting piece of apparatus, devised by Prof. Bickerton for showing the action of the lungs, is described on p. 76. All the experiments are briefly but sufficiently described, and many of them are illustrated.

Science in Arcady. By Grant Allen. (London: Lawrence and Bullen, 1892.)

THIS volume will fully maintain Mr. Grant Allen's reputation as a popular writer on science. The essays of which it consists are written in a bright, lively style, and may be read with pleasure even by original investigators, for the truths with which they deal, if not new, are at least presented from new points of view. Readers who do not profess to know much about any particular branch of science will find in these papers an excellent introduction to some of the more attractive facts and laws of the natural world. The volume includes some archæological essays, which show very effectually that an antiquary has not necessarily much resemblance to Dr. Dryasdust.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Macculloch's Geological Map of Scotland.

IN a recent article in this journal Prof. A. H. Green writes as follows:—

"Macculloch seems to have projected, but never completed, a geological map of the whole of Scotland. The materials collected by him were, however, utilized by the Highland Society in the construction of a general map in 1832."

I am sure that nothing could be farther from the wish of the Oxford Professor than to do an injustice to the memory of one of the greatest of the pioneers in British geology, and he will therefore forgive my calling attention to the following facts, which, judging from a subsequent letter in these pages, would seem not to be generally known.

During the last twenty years of his life Dr. John Macculloch was engaged on a regular survey of Scotland, and in collecting the materials for a geological map of the country. In the earlier part of this period Macculloch seems to have availed himself of the opportunities afforded to him as an official of the Board of Ordnance to carry on his valuable geological explorations. But during the latter part of the period he was regularly employed by Government to complete his geological work, and was paid by the Lords of the Treasury, who in the end published his map.

On July 28, 1834, Macculloch addressed to "His Majesty's Treasury" a series of memoirs respecting the Geological Map of Scotland, which was then completed. In these memoirs he refers to the map as being then in existence, and gives the most minute directions concerning the tints to be employed by the colourists who were to copy the map, in order that it might correspond with his original work. He also expressed his regret that the imperfect topography of the map on which his researches had to be recorded prevented the work from being as accurate as he could have wished at certain points.

Owing to delays in issuing the Government publications—not quite without parallel in more recent times—Macculloch's "Memoirs on the Geological Map of Scotland" did not appear till the year following his death (1836). The date at which the first copies of the map were issued it would probably be difficult to determine, but as to the completion of the map before July 1834, and its subsequent issue by "the Hydrographer to the King by order of the Lords of the Treasury" there can be no doubt whatever.

The first Government Geological Survey undertaken in the British Islands was that of Dr. John Macculloch, and the work that he accomplished single-handed was a very remarkable one. Several geological maps of Scotland, differing very widely from that of Dr. John Macculloch, have been issued and withdrawn during the last fifty years; but any one who will compare the first geological map of Scotland with the latest, also "published with Government authority," will be interested to see how far the work of the early pioneer in Scottish geology has been found to be correct in most of its essential features by those who have come after him.

John Macculloch's title to be the author of the first geological map of Scotland is as indisputable as are the similar claims of William Smith and Richard Griffiths with respect to England and Ireland respectively. JOHN W. JUDD.

16, Cumberland Road, Kew, December 6.

Glaciers of Val d'Herens.

THE two glaciers of Arolla are interesting, inasmuch as one, the Arolla, is retreating, while the other, Glacier de Zigiorenove, is advancing. This has been going on for twelve years, according to the report which appeared in NATURE (vol. xlvi. p. 386), by Dr. Forel, dealing with Alpine glaciers and their changes.

Having visited these glaciers last summer with the object of observing the effect of their respective movements upon the morainic accumulations in front, I think a brief account may be worth recording.

The Arolla glacier occupies the head of the valley, and is fed by the snow from Mont Colon and the fields of nevé extending towards Mont Brulè. The Zigiorenove glacier is one mile further down the valley, but does not descend so low as the Arolla glacier by about 300 feet. It receives its main supply from the Pigne d'Arolla, a mountain which rises immediately above the glacier, and is conspicuous by its massive snow-cap. This glacier is not only nearer to its supply, but descends at a steeper gradient than the Arolla glacier.

This may in some measure account for the former advancing while the latter is retreating, or, more correctly speaking, melting backwards. I was informed by local guides that the Arolla glacier has been swelling behind for some years; if this be correct, and the seasons remain normal, then, in a short time, this glacier must advance also; in other words, being longer or further away from its feeding ground, the extra supply has not yet had time to reach its extremity. Appearances at the end of the glaciers are in themselves quite sufficient to indicate their respective movements; the snout of the Arolla glacier is buried in its own debris, composed of rocks and loam borne upon or concentrated to the surface as the ice melts. This debris is being constantly shot down grooves or water-courses furrowed in the sloping end or side of the glacier. At the bottom of these spout-like grooves conical-shaped mounts are formed, one behind another, as the ice melts backwards, resulting in a moundy, zigzag arrangement constantly seen lower down the valley, many miles away from existing glaciers, and can be seen also in many of the higher valleys of North Wales.

The advancing Zigiorenove glacier, instead of being buried in debris, turns up in shell-like flanges, exposing its under surface in its endeavour to climb over, rather than push forward, the loose rocks in front; a part of this loose material only is pushed forward, forming a small bank, which in no place exceeded 5 feet in height, up which the ice mounts with the shell-like flanges projecting beyond; the under part of these projections being fluted into a perfectly symmetrical pattern.

In some way the projecting rocks forming the glacier bed produced these convolutions, but whether by cutting, melting, or by the ice crystals flowing round each side, I was unable to determine, but I may state there was no appearance of cutting or grinding which would necessarily leave behind the shavings or ice particles cut out or ground off, and little or no water was present as if from melting. We had much snow for three days, with a total absence of sunshine; the atmosphere at mid-day, when making these observations, registered 38° F. Appearances rather suggested the idea that the ice crystals were displaced in a similar manner to water in a rapid river when it meets with obstructing boulders in its bed.

Climbing up one of the old lateral moraines, for there are several, the glacier is seen to be encroaching laterally as well as longitudinally; it soon extends to the base of the inner moraine, and then climbs upwards in a similar manner as it advances in front by turning up its edges, carrying upwards a few loose rocks in front, but without materially disturbing the moraine itself. Higher up it reaches the top of this moraine, and then rises above it like a wall, having caused the loose rocks it had borne up to roll down into the valley between the inner and outer moraine, with masses of ice broken off the edge of the glacier.

As far as I am able to ascertain, by creeping under the flanges the glacier slowly spreads out the bank which it had pushed up in front as it advances into a more even bed to travel over, but my observations were restricted to a few yards owing to the contraction of the sub-glacial space between the ice roof and the floor. I was much impressed by the fact that the glacier did little in exerting that force which might be expected from such a power behind, in the way of ploughing through or removing such comparatively trifling obstructions as loose rocks or moraine banks.

It is evident from the various ages of moraines that the lower parts of those glaciers have fluctuated considerably during recent times, but the line of demarcation between these minor fluctuations and the period after these lateral valley glaciers had been confluent with the main Valais glacier, and were retreating backwards, is very marked. Beyond two miles down the valley from the Zigiorenove glacier no striæ from the trunk glacier could be found, but at this distance it suddenly appears with fresh glaciated forms; precisely the same phenomenon was noticed in the neighbouring Val d'Anniviers, as if this were the limit of the more recent fluctuations.

At the Col de Bertol, 4500 feet above Arolla, or about 11,000 feet above sea level, there are no indications of glaciation, ancient or modern, above the surface of the snow; so it would appear, however much the ice level has varied below, at this dividing ridge it has always remained the same, or, in other words, it has been dispersed by wind or ice movements to lower levels at a rate equal to deposition, when the snow line was low enough to allow the ice to travel down the Val d'Herens, the higher ice streams would simply run down upon its surface, instead of descending as low level glaciers or mountain rivers, as at present.

WILLIAM SHERWOOD.

Eastbourne House, Sutton Coldfield.

Ancient Ice Ages.

MESSRS. BLANFORD, in their letter (NATURE, p. 101) called forth by Dr. Wallace's notice of a palæozoic glacial conglomerate in Victoria, Australia, say:—"It has become an accepted article of faith amongst most European geologists that no ice-age occurred before the last glacial epoch." There is no doubt that the tendency of opinion has been in that direction, notwithstanding the evidences to the contrary brought forward by Dr. Blanford and others. The late Sir Andrew Ramsay was the first, so far back as 1855, to suggest that the Permian conglomerate of Abberley and the Clent hills was an ancient glacial deposit. Although this reading was accepted by so cautious a philosopher and critic as Sir Chas. Lyell ("Principles," 10th ed., vol. i. p. 223), the idea has languished from disfavour. Having devoted a considerable portion of the last twenty years to the study of glacial phenomena, I early this year paid my first visit to the district. The best section I saw is an excavation at Abberley in what may be called boulder-gravels, and, except for its prevailing red colour, the deposit, if dropped down on the coast of Wales, would, in general appearance and arrangement of the materials, be undistinguishable from many of the glacial deposits to be found there. Many of the stones have flattened faces, and in general shape resemble glaciated boulders, but the striæ, so far as I could observe in the limited time at my disposal, were not very pronounced. Since then I have found a deposit of glaciated gravels on the top of the Screes, Cumberland, 1600 feet above the sea, which on comparison with the specimens brought home from Abberley enables me to understand the latter better. The volcanic rocks of the lake district of which these gravels are composed break up into very similar shapes, and are planed and striated in a very similar way to those of Abberley. I also brought home from Abberley two specimens of the "paste" which fills up the interstices of the finer gravel.

On washing and riddling this clayey matter, well waterworn and smooth gravel was separated from it up to about the size of a bean, and on many of these minute pebbles, with the aid of a microscope of low power, beautifully-developed striæ are to be seen, sometimes on more than one face. This latter fact appears to have been unnoticed before. The conclusion I have come to is that Ramsay had reasonable grounds for his belief in this being a Permian glacial deposit, and think that if he had given more details in his otherwise able paper, geologists would probably have followed him more freely.

T. MELLARD READE.

Park Corner, Blundellsands, December 5.

The Earth's Age.

As Dr. A. R. Wallace's "Island Life" may be regarded as one of the best authorities on its subject, it appears desirable that any errors in it should be pointed out, lest any of its numerous readers should be misled.

In Chapter X. (2nd edition, 1892) is an estimate of the earth's age based on the following data:—Land area of globe, 57,000,000 square miles, coast line, 100,000 miles, width of shore deposits, 30 miles, hence area of shore-deposits, 3,000,000 square miles, hence rate of deposition 19 times as fast as that of mean rate of denudation, which latter is taken to be 1 foot in 3000 years.

Thickness of stratified rocks 177,200 feet, hence time required for deposit 28,000,000 years. This last result is taken to be approximately the earth's age.

It appears to me that Dr. Wallace's data warrant no such conclusion, for, in the 28,000,000 years in question, all that would have been deposited would be a thickness of 177,200 feet of rock, over an area of only 3,000,000 square miles, whereas, what has to be accounted for is an area of 57,000,000 square miles (neglecting igneous rocks and sedimentary deposits beneath existing seas) of the same thickness. Therefore, so far from Dr. Wallace's data leading to 28,000,000 years as the earth's age, they actually lead to a result 19 times as great, viz. 532,000,000 years.

Sir A. Geikie's estimate is (NATURE, vol. xlvi., p. 322), 100 to 680 million years. Personally, I think, the method of taking maximum thicknesses of deposits unsatisfactory, for it assumes that every formation was deposited, with its maximum thickness, over the whole land area of the globe. The absurdity of this supposition is obvious. The only defence of it is that it is held to make an ample allowance (of unknown amount) for repeated denudation. It would, perhaps, be better to ascertain the actual thickness of a great series of successive formations, say in the Colorado Cañon and other regions, and from such data to estimate the total *average* thickness. This estimate, of course, would allow nothing for repeated denudation, but would enable one to form an idea of the earth's *minimum* age.

BERNARD HOBSON.

Owens College, Manchester, December 5.

I AM glad that Mr. Hobson has formulated his difficulty as to the measurement of geological time by the comparative rates of denudation and deposition, because it shows that I cannot have explained my views as clearly as I thought I had done; yet on again reading over pp. 217–223 of "Island Life," I can hardly understand how he has missed the essential point of the argument. Fortunately, there is no dispute as to the data, only as to the conclusions to be logically drawn from them.

Mr. Hobson says that I account for a deposit of 177,200 feet (the supposed thickness of all the stratified rocks) over an area of 3,000,000 square miles (the estimated area over which at any one epoch stratified rocks are being deposited) in 28,000,000 years (the deduced estimate of known geological time); and then adds: "Whereas, what has to be accounted for is an area of 57,000,000 square miles *of the same thickness*" (my italics). This seems to me a most amazing misconception; for it means that every single formation and every stratum or member of each formation, was deposited to the same average thickness over the whole land surface of the globe (area 57,000,000 square miles)! And this implies that at every successive period, from the Laurentian to the Pliocene, the conditions of denudation and deposition were totally different from what they are now, since at the present time it is demonstrable that the area of deposition of continental debris is only a fraction of the whole continental area. It implies further, that during each geological period the whole of the existing land area must have been, either at once or in rapid succession, sunk beneath the sea in order to allow of its being all covered with each successive formation—an amount of repeated upheaval and depression which hardly the most extreme convulsionist of the old school would have postulated. I cannot make the matter clearer, and trust that on further consideration Mr. Hobson will admit that his objection is invalid. ALFRED R. WALLACE.

The Colours of the Alkali Metals.

IN NATURE (vol. xlvii. p. 55) is a communication by Mr. G. S. Newth, entitled "Note on the Colours of the Alkali Metals."

I write to call attention to my article on "The Colour and Absorption-Spectra of Thin Metallic Films, and of Incandescent Vapours of the Metals; with some observations on Electrical Vitality," published in the *American Chemical Journal* (vol. xiv. p. 185) and reprinted in the *Chemical News* (vol. lxvi. p. 163), which gives the method employed by Mr. Newth, as well as other methods for obtaining metals in thin films.

In it attention is also called to the fact that the colour of the film of the metal and the colour of the vapour are widely different.

Mr. Newth, however, succeeded in getting a film from sodium on glass, while I did not, and his success was probably due to the use of a higher vacuum than I employed. He also obtained a rubidium film.

In my paper I called attention to the similarity in colour of the film by transmitted light and that of the *incandescent* vapour which is very striking in many cases. In this respect the film of rubidium as obtained by Mr. Newth follows the rule fairly well; but the film which he got from sodium is exceptional, as according to the analogy furnished by other metals it should be yellow. The presence of potassium, however, might cause the green colour which he observed, by the combination of yellow and purple. WM. L. DUDLEY.

Vanderbilt University School of Chemistry,
Nashville, Tenn., December 2.

Osmotic Pressure.

IN an article on osmotic pressure, in NATURE (*ante*, p. 103), Mr. Rodger very truly remarks that "at present the attitude of the prominent upholders of the new theory [of solutions] is one of indifference as to the exact mechanism of osmotic pressure. The numerical agreement between the measurements on solutions and those on gases is regarded as ample justification for considering dissolved substances to be in a pseudo-gaseous condition." Such an indifference is surely to be regretted from any truly scientific point of view, especially as those explanations which have been given of the mechanism of osmotic pressure have been based on the supposition that the dissolved substance is in a *veritable* and not merely *pseudo*-gaseous condition. There are, however, many reasons for supposing that while the dissolved substance may for many purposes be regarded as analogous to a gas, it must in reality be in a very different condition, and that osmotic pressure is not due to the bombardment of the free molecules of the dissolved substance against a diaphragm through which they cannot pass. The impenetrability of the diaphragm to certain molecules can scarcely be attributed to any other cause than that the molecules are too large to pass through the interstices of the former, and it is scarcely conceivable that the molecules of water which do pass through can be much smaller than the molecules of simple salts, which do not pass through; still less that they can be smaller than the single atoms into which these salts are said to be dissociated.

A very simple experiment, which I mentioned some time ago in the *Ber. d. deutsch Chem. Gesell.* (24, 3639), appears to settle definitely against the view that osmotic pressure is due to impenetrability to the dissolved substance. A solution of propyl alcohol and water was put into a porous pot and immersed in a vessel of water; the water passed through the pot to the solution, and this, according to the usual explanation, would show that the pot was impermeable to the propyl alcohol. The same solution was then immersed in a vessel containing propyl alcohol, when the alcohol was found to pass through to the solution, from which we should have to draw the diametrically opposite conclusion that the pot is impermeable to the water. The true conclusion obviously is that the pot is impermeable, neither to the alcohol alone, nor to the water alone, but to the solution of these in each other, and that the molecules composing this solution must be larger than those of either of the two substances when separate, the solution consisting of compounds or hydrates of the two. I showed, moreover, in the paper above referred to that the hydrate theory of solutions was quite capable of accounting for and explaining the fact that the dissolved substance may for many purposes be regarded as being in a quasi-gaseous condition in weak solutions, and that calculations based on the idea of its being truly gaseous would yield very nearly correct results.

The hydrate theory will also, as I showed, give an explanation of the fact that electrolytes will give abnormally high osmotic pressures, and that the magnitude of these pressures can be calculated from their electric conductivity: and the explanation based on this theory also obviates many of the objections to which the idea of dissociation into ions is open. Moreover, the only critical experiment which, as far as I know, has ever been made to test the validity of the dissociation hypothesis, gives an unequivocal answer against it, and in favour of the hydrate theory. When, for instance, sulphuric acid is dissolved in excess of water, it is represented by the dissociationists as splitting up into its ions, so that the solution will contain more acting units (ions and molecules) than the acid and water together contained before they were mixed: whereas, on the hydrate theory, combination will have occurred, and there will be fewer acting units present. The number of acting units may be ascertained by observing the depression produced by the solution on some other solvent, such as acetic acid (that is, by using the very method which the dissociationists use to prove the supposed dissociation of substances), and when this is done it is found that the sulphuric acid solution contains *fewer*, instead of *more*, units than the acid and water separately.

Even if the above were the only arguments to be urged, it is evident that although the idea of the dissolved substance being gaseous and often dissociated may be a good working hypothesis for the directing of investigation, it can scarcely be accepted as a true theory of the nature of solutions.

SPENCER PICKERING.

On a Supposed Law of Metazoan Development.

IT is difficult not to feel disappointed that Dr. Beard has given only "a preliminary sketch by way of clearing the ground" (NATURE, vol. xlvii. p. 79), in place of "producing the full argument" for a law in the existence of which he has by "observation and reflection" been led to believe. For it is not easy to gather from his sketch how he is able to apply a universal law to so varied a series of events and phenomena such as he mentions, and at the same time to point out "the analogy which obtains between the suggested mode of Metazoan development, and the accepted fact of an alternation of generations in the life histories of all plants above the lowest Thallophytes." For in the higher plants the alternation of generations referred to occurs with constancy as regards period of life history, and varies only slightly within the limits of the same group.

Dr. Beard alludes, I presume, to one form of alternation of generations—that of sexual with asexual generation only, or Metagenesis. This he asserts constitutes a general law in the development of Metazoa.

In a sense this may be true enough. If, for instance, we regard the division of each cell as a new asexual generation, then Metagenesis is a very constant phenomenon amongst Metazoa. In this case the life history of a Metazoon consists of a sequence of thousands of asexually produced generations alternating with one sexually produced generation, which gives apparently a stimulus for another run of asexual generations in which polymorphism and division of labour are exhibited in extraordinary complexity and beautiful harmony.

But this is not at all what Dr. Beard means. The series of instances which Dr. Beard gives, or system of "nursing" as Steenstrup termed it, is at most a series of disconnected phenomena of frequent occurrence, and not a law.

Because most Metazoa possess eyes, it is not therefore a law of Metazoan development that eyes should be developed. Diversity in form, number, and time of appearance of eyes, is sufficient to show that the law cannot exist; so also is it in the cases of nursing to which Dr. Beard alludes, and on which he bases his argument.

It seems to me that no "law" of alternation of generations in Metazoa can be "enunciated" unless there is evidence forthcoming of its constant action at corresponding periods in the life histories of all animals of different groups, and in a closely similar manner in individuals of one and the same group. Also a law of such a nature, if it is to be found to act universally amongst Metazoa, must surely have come into action at a very early period in the evolution of Metazoa.

Metagenesis is of constant if not universal occurrence in the cycle of life of Protozoa. A long series of generations produced asexually is followed by a generation produced sexually, that is, a generation produced by the conjugation of two individuals; this is followed again by another long series of asexually produced generations, and so on. If this is so constant among unicellular organisms of the present day, it is not very unreasonable to suppose it was common among the protozoan ancestors of the Metazoa and of the Plants. If we are to find any form of Metagenesis as a *universal phenomenon* in the Metazoa, it must be to the most protozoon-like stages of development of the Metazoon that we should look.

There is but one strict meaning to the phrase sexual generation, and that is a fusion of two cells. If Metagenesis means anything it means the alternation of a generation resulting from the fusion of two cells, with one or more generations resulting from the division of cells.

This we can perhaps find in the protozoon-like stages of Metazoan development, and in a way analogous to the alternation of generations among plants.

Spermatozoon and ovum fuse and form the fertilized ovum which is the true sexually produced generation. This produces by division a vast number of cells, and if we regard these as a number of generations then Metagenesis is obvious enough. But it is no more metazoic—if I may use such a word—to call the whole animal resulting from the segmentation of the fertilized ovum, the sexually produced generation.

This generation buds off the immature ovum. This is really the "Primitive ovum" of the embryo. I see no reason why this may not be regarded as a distinct asexually produced generation—like the formation of the spore of the plant.

The immature ovum divides into two cells—first Polar body, and more mature ovum. The more mature ovum divides into two cells, namely, second Polar body and mature ovum. It does not materially affect the argument whether we should regard these two processes as two separate consecutive asexually produced generations, or as one asexually produced multicellular generation. If we take the latter view, then the maturation of the ovum is more analogous to the prothallus stage of the life history of plants.

In either case the result is the formation of the mature ovum, comparable to the oosphere of plants.

The mature ovum fuses with the mature spermatozoon, and the sexually produced generation recurs, and the cycle of development is completed.

I cannot help thinking that if Dr. Beard wishes to discover a law of Alternation of Generations applicable to the whole of the Metazoa, he will find a more favourable hunting ground amongst those stages of development at which the several groups of Metazoa approximate, than amongst those stages where they are farthest apart; and also Dr. Beard will find the analogy between the supposed Metazoan law and the accepted law of the vegetable kingdom closer than he could ever hope to find it if he continues his present line of search.

If the above theory of the cycle of Metazoan life can be considered tenable, we see that both in the Higher Plants and in the Metazoa there are constantly alternating "sporophyte" and "gamophyte" generations, and further, we can find evidence, as we should expect to do, of the origin of such a universal phenomenon in the single celled or protozoan life, where the continuance of the species may be secured in both these ways, namely, by the formation of asexually produced spores, and as a consequence of cell fusion, *i.e.* conjugation.

RIC. ASSHETON.

Oxygen for Limelight.

THE employment of oxygen for limelight and other purposes has increased enormously since the commercial introduction of the Brin method, by which the gas is separated from atmospheric air by a now well-known chemical process. The gas so obtained is practically pure, analysis showing that as now supplied by the Brin companies it contains on an average 95 per cent. of oxygen, the remaining five per cent. consisting of inert nitrogen.

The success of this comparatively new industry has been so marked, that, as a natural result, competitors with rival processes have come forward. Some of these met with failure at an early stage of their career, but others are supplying oxygen to the public. This is by no means a state of things to be deplored from the consumer's point of view, if the product from the one source is as good as the other, for benefit generally arises from healthy competition. But when the rival product turns out to be not oxygen, but a half and half mixture of oxygen and air, with a slight excess of the latter, the competition is of a decidedly unhealthy character, and is correspondingly bad for the consumer. I recently obtained a sample of gas from a dealer, which on testing (with a Hempel absorption pipette, charged with metallic copper and ammonia) I found to be a mixture containing only 60·6 of oxygen. I next tested the illuminating value of this highly-diluted oxygen with a limelight jet, and for sake of comparison, placed by its side a precisely similar jet supplied with Brin's oxygen, and, as might have been expected, the light given by the former was little more than one-half as intense as that afforded by the latter. With the good oxygen the lime cylinder was quickly pitted, whilst the other showed no symptom of destruction. It is also to be remarked that the consumption of the diluted gas was, for a given period, about one-third more—striving with both jets to get the best possible light—than that of good oxygen. On the same principle a mountaineer at a high altitude will pass more (rarefied) air through his lungs than he will when he is in the valley breathing that which contains the normal quantity of oxygen.

As this matter is of great importance to many workers, I trust that you may be able to find room in your valued publication for these words of necessary caution.

T. C. HEPWORTH.

45, St. Augustine's Road, Camden Square, N.W., December 6.

THE STAR OF BETHLEHEM.

IN the *Astronomical Journal* of November 26 we find the second of two very interesting articles written by Mr. J. H. Stockwell, bearing on the chronology of certain ancient events. In the introduction the author discusses and sums up some of the more important and historical dates which he has determined by calculations of ancient eclipses. He next refers to the help which may be obtained in the same direction by means of calculations of conjunctions of the planets, and quite appropriately to the present season points out that the appearance of the star of Bethlehem may have been due to the conjunction of the planets Venus and Jupiter, instead of Saturn and Jupiter, as was suggested on incomplete data by Kepler nearly three hundred years ago. We cannot do better than lay this part of Mr. Stockwell's communication before our readers.

"Although the heliocentric conjunctions of the planets occur with a considerable degree of regularity, and are also very easily calculated, the geocentric conjunctions are subject to many inequalities in the periods of their successive occurrences; so that it requires somewhat elaborate computations to determine accurately the character of any geocentric conjunction of two planets which occurred in ancient times. On account of the frequency of planetary conjunctions, and the indefinite manner in which they are usually described, it becomes a matter of very great difficulty to identify any particular conjunction unless it is associated with some other event whose data can be independently determined. A remarkable case of this character is given in the Bible, for Matthew informs us in the days of Herod the King 'there came wise men from the East to Jerusalem saying, "Where is he that is born King of the Jews? for we have seen his star in the East, and are come to worship him."' From the subsequent inquiries and mandates of Herod the King concerning the time when the star appeared, we are led to infer that its appearance took place within two years preceding the death of Herod, and it has been sought to explain the appearance of the star by means of a conjunction of the planets—the Creator employing celestial phenomena to proclaim 'the good tidings of great joy, which shall be to all people.'

"The illustrious Kepler was the first to suggest that the star of the wise men might be explained by means of a conjunction of the planets Jupiter and Saturn, and he even undertook to calculate the times when such conjunctions took place. Much has been said and written on the subject of the 'star of the wise men' during the past few years; but no important contribution to the natural history of the star has been made since the days of Kepler, nearly three hundred years ago. But the supernatural history and functions of such a star have been discussed in a very able and interesting manner by many writers in theological, literary, and semi-scientific periodicals during the past twenty years, and perhaps nothing of interest and importance can now be added to what has already been published on that subject.

"I find, however, that Kepler overlooked one important element of the problem in his calculations, and consequently left the natural history of the problem in an incomplete and unsatisfactory condition. I shall therefore here attempt to complete more fully what Kepler began, and show that the Biblical narrative concerning the 'star in the east' is better satisfied by a conjunction of Venus and Jupiter than by any of the conjunctions computed by Kepler.

"We have already seen that the death of Herod took place early in the year B.C. 4, and if we can now show that there was a very conspicuous conjunction of two bright planets, visible only in the east, within two years preceding that date, the hypothesis that such conjunction was the event referred to in the Biblical narrative will at least be rendered plausible, if not entirely legitimate; and for this purpose I have here undertaken the calculation of all the conjunctions of the planets which took place near that epoch. I shall first enquire whether there was a conjunction of the planets Jupiter and Saturn about that period of time which would satisfy the required conditions. The mean interval between two heliocentric conjunctions of Jupiter and Saturn is 7253·4638 days; and they were in mean conjunction B.C. 6, January 30. Now the time of true heliocentric conjunction may differ from the time of mean heliocentric conjunction by 241 days, on account of the inequalities in their elliptic motions, and by 23 days more by reason of the great inequalities of long period in their mean motions. But the time of geocentric conjunction of Jupiter and Saturn may differ from the time of heliocentric conjunction by 102 days; so that a geocentric conjunction may occur one whole year before or after the time of mean heliocentric conjunction. In the present instance I find that the true heliocentric conjunction took place B.C. 7, September 23, which is 129 days before the mean heliocentric conjunction; and that there were three geocentric conjunctions during the year B.C. 7, which took place as follows:—

"The first conjunction took place June 7, in which Saturn passed $1^{\circ}\ 4'$ to the south of Jupiter; the second conjunction took place September 18, in which Saturn passed $1^{\circ}\ 2'$ to the south of Jupiter; and the third conjunction occurred on December 15, in which Saturn passed $1^{\circ}\ 8'$ to the south of Jupiter.

"In the first conjunction the planets would have an elongation of about 73° to the westward of the sun, and would be seen during four or five hours in the east in the morning. The second conjunction took place near the time of opposition with the sun, and would be visible during the whole night, so that it could not properly be designated as a star in the east any more than a star in the west. In the third conjunction the planets would have an elongation of about 84° to the eastward of the sun, and could therefore appear only as evening stars.

in the west. Moreover, Saturn is not an especially bright planet, and consequently no one of these three conjunctions could have been very conspicuous in the heavens. The first conjunction was the only one that was visible in the east, but it occurred nearly three years before the death of Herod; it could hardly be said to satisfy the conditions required by the narrative. No other conjunctions of Jupiter and Saturn could possibly occur till about twenty years later, so that we may conclude with a light degree of probability that the phenomenon alluded to in the Bible was not occasioned by a conjunction of Jupiter and Saturn. Since the planet Mars is a conspicuous object when near its opposition with the sun, it may be well to inquire whether a conjunction of Mars and Jupiter might not occasion the phenomenon referred to. But since Mars is conspicuous only near its opposition with the sun, it is evident that any conjunction when in that direction would appear as a star in the west as much as in the east, and consequently it would not fulfil the required conditions. There was, however, a conjunction of Mars and Jupiter on March 5, B.C. 6; but at that the planet's elongation was only 18° to the eastward of the sun, and consequently could have been visible only in the west. But Mars was then so far from the earth, and so nearly in conjunction with the sun, that the conjunction would be wholly invisible. At the same time Saturn was not very far from Jupiter, and hence it was said there was a triple conjunction of the planets Mars, Jupiter, and Saturn in the spring of B.C. 6.

"It is evident without calculation there could be no conspicuous conjunction of Venus and Mars at any time; because Mars is not a conspicuous planet unless its elongation from the sun be greater than the greatest elongation ever attained by Venus, so that it would be a waste of time and labour to enter into the computations of any such conjunctions.

"It now remains to inquire whether the two brightest planets of the solar system, Venus and Jupiter, might not have been in conjunction within a short time before the death of Herod, and constitute the phenomenon alluded to in the biblical narrative; for it was the beautiful phenomenon presented by these two planets when in conjunction last February that suggested this investigation. Now the conjunctions of Venus with the sun occur with great regularity at intervals of about 584 days, while those of Jupiter at intervals of 399 days. Moreover, it may easily be shown that all geocentric conjunctions of Venus and Jupiter must take place within about 60 days before or after Jupiter's conjunction with the sun. Therefore, by tabulating the times of Jupiter's conjunction with the sun, we have only to investigate the longitude of Venus for a period of 60 days before or after that event in order to determine whether a conjunction of those planets will then take place. Now I find Jupiter was in near conjunction with the sun B.C. 6, March 29, while Venus was in conjunction on the preceding November 5, or 144 days earlier than Jupiter. Venus was therefore past her greatest western elongation, and was moving towards her superior conjunction, and she would overtake Jupiter on May 8, when their mutual elongations from the sun would be 27° 44′ to the west. At that time the heliocentric latitude of Venus and Jupiter were 3° 21′ and 1° 20′ south, while their geocentric latitudes were 1° 40′ and 1° 8′ south respectively. It therefore follows that at the time of their geocentric conjunction Jupiter was only 32′, or about the angular breadth of the moon to the northward of Venus; and as they were then to the westward of the sun, they would be visible only as a star in the east a couple of hours before sunrise. These two brightest planets in the sky would therefore at the time of conjunction, B.C. 6, May 8, be apparently very close together and produce a striking and beautiful appearance. The date also at which it took place being about 50 days less than two years before the death of Herod, harmonizes well with the spirit and other conditions of the narrative; for it is probable that the mandate for the slaughter of the children of two years old and under was issued some months before his decease, and the limit of two years would leave an ample margin for any uncertainty as to the time of the appearance of the star as related by the Magi.

"There were no other conjunctions of Venus and Jupiter until the year B.C. 2, or nearly two years after the death of Herod, when there were two conjunctions, one of which occurred on August 31 and the other on October 4. The first of these was invisible on account of being too near the sun; but the second took place when Venus was nearly at her largest elongation to the westward of the sun.

"If the preceding calculations, and the references based on them, are correct, it follows that Christ was born as early as May in the year B.C. 6; and if He was crucified at the time of the paschal full moon, which occurred on a Friday, it must have taken place on April 3, in the year A.D. 33. And since any given phase of the moon is repeated on the same day of the week, and also within two days of the same time of the year, at intervals of 334 lunations, or 27 years, it follows there was no paschal full moon on a Friday between the years A.D. 6 and A.D. 60, except the one on April 3, A.D. 33; whence it would seem to follow that Christ was thirty-eight years old at the time of His crucifixion and death, and this would vindicate the sagacity of the Jewish doctors, who had recently affirmed that He (Jesus) was not then fifty (forty) years old."

FUJISAN.[1]

ALL who remember the beautiful plates illustrating the volume on "The Great Earthquake of Japan, 1891," which was issued by the same authors a few months ago, will welcome the first instalment of a work which promises to illustrate, in a manner worthy of the subject, the magnificent volcanic phenomena of Japan. The present part contains ten plates, and is devoted to the illustration of the most famous and beautiful of all the Japanese volcanoes—Fujisan. The number of parts that the authors will publish will depend partly, we are told, on the encouragement they receive, and partly on the number of photographs that they have been able to secure during the past summer.

The photographs in the present part, which are all reproduced as permanent collotypes, 11 inches by 8 inches in size, are exquisite examples of what can be accomplished by this method of illustration, and show that Japan is certainly not behind any country in the world so far as the resources of the publisher go. Where all are so excellent, it is difficult to select any particular plate for especial praise, but one of the most remarkable is certainly Plate II., which gives a view over the great cloudbanks as seen from the summit of Fuji. Nothing can be more striking than the manner in which the effect of the great fleecy masses of vapour are reproduced, and here nothing whatever is lost from want of colour. The plate of greatest scientific interest is perhaps the last, which shows the interior of the crater of Fuji—a great pit 600 to 700 feet deep, with perpendicular walls. The sides are built up of rings of variously-coloured rocks, while snow rests in the sheltered hollows. The remaining pictures illustrate the sacred mountain as seen from different points of view, the graceful curves of its outline, the variation in the distribution of snow on its flanks, and the

[1] "The Volcanoes of Japan. Part I. Fujisan." By John Milne, F.R.S., Professor of Mining and Geology, Imperial University of Japan; and W. K. Burton, C.E., Professor of Sanitary Engineering, Imperial University of Japan. Plates by K. Ogawa. (Yokohama, Shanghai, Hongkong, and Singapore: Kelly and Walsh, Limited, 1892.)

character of the foreground, giving rise to great diversity in these eight pictures.

As an example of these beautiful views, Plate IV.—

“Fujisan from above Hakone”—has been reproduced, although necessarily much of the delicacy of the original has been lost in the process by which it has been copied.

“In the foreground, looking like a river, is the Lake of Hakone, at the back of which are hills some 4000 feet high. At the lowest gap in these hills is the Otome pass.

In the background, overlooking both lake and mountains, is the upper part of Fuji. This portion of the mountain is particularly conical, with sides sloping at an angle of

30°, its logarithmic sweep being lost behind the intervening mountains. The almost triangular notch in the snow-cap may possibly represent the scarp that is supposed to have been formed by the great earthquake of 1891, causing a strip of ground in unstable equilibrium to slip downwards." The reader should compare this view with that given in Plate IX., which shows the lake, with the reflection of the mountains behind, and the snow-covered Fuji rising in the background. This plate, and the view, Lake Kawaguchi, given on Plate V., are so delicate and faithful in their portrayal of water and atmospheric effects as to defy reproduction.

No attempt has been made by the authors to produce a scientific treatise, the information contained in the text being of a popular character, and the reader is referred to the Transactions of the Seismological Society of Japan for more detailed information on the subjects treated of. It is nevertheless true that the text published with these plates contains, as the authors claim for it, information not readily obtainable from other sources. The introduction gives a sketch of the volcanic phenomena of the Japan and Kurile Islands, in which we are informed that the number of volcanoes still preserving their form, and with distinct craters, is one hundred, distributed as follows:—In the Kuriles 23, in Yezo 28, in Honshiu 36, and in Kiushiu and the Southern Islands 13. Of these no less than 50 emit steam, while 39 are distinguished by their beautiful and graceful outlines. The number of great eruptions of which there is any published record is 233, the greater frequency, as with earthquakes, having been during the colder months of the year. One line of vents, which is more than 2000 miles long, begins in Kamsatka, passes through the Kuriles, Yezo, and down by Honshiu to the ever-smoking Asama. Here it is joined by a line branching away to the south-west, which runs through the great Fujisan and Oshima, till it reaches the Ladrones, a distance of 1200 miles. The last line begins at, or near, the gigantic crater of Mount Aso, and extends 1300 miles through Formosa to the Philippines. Extremely basic rocks are rare, but so far as observations have gone, it may be said that the lava poured out from the northern vents is more acid in composition than the southern. All are magnetic, and lavas that will turn a compass-needle through 180° are not rare. By their decomposition, the soil of the country is in many places so filled with grains of magnetite, that a magnetized knife passed over the gravel of a garden path will be covered with a brush of this unoxidizable material.

The twelve pages devoted to the description of Fujisan are replete with interesting information. The word Fuji is said (on the authority of the Rev. John Batchelor, of Sapporo) to be a corruption of the Ainu word *Huchi*, which is the name of the "Goddess of Fire." Professor Milne ascended the mountain in 1880, and found that it was not quite extinct, as is usually supposed, for small quantities of steam were detected by him issuing through the ashes on the eastern side of the mountain just outside the lip of the crater. Von Fritsch and Ludecke have shown the lavas composing Fuji to be dolerites, and analyses by several chemists are given in this work. The beautiful and symmetrical outlines of the mountain are well known, but on the south side of the mountain there is an excrescence, at a height of 9000 feet, which was produced by the last great eruption in 1707. The recorded eruptions of the mountain are as follows:—B.C. 301, 294, or 286, and A.D. 799, 802, 864, 937, 1021, 1082, 1329, 1560, 1627, 1649, 1700, and 1707. Professor Milne records the interesting observations made by him with a tromometer or tremor-measure during a stay of five days on the top of Fuji. These observations tend to prove that the great mass of the mountain actually yields to force of wind playing around its summit. The height of Fujisan is proved by various observations to lie between 12,400 and 12,450 feet.

The authors are to be congratulated on the excellence of this first instalment of a work which promises to be one of great scientific value. J. W. J.

THE GALILEO CELEBRATION AT PADUA.

THE celebration of the three hundredth anniversary of the day on which Galileo began his labours as a Professor at the University of Padua was even more successful than had been anticipated. Its success was in every way worthy of the large number of scientific men who assembled to do honour to Galileo's memory, and of the great institution with which, as it remembers with veneration and pride, he was so intimately associated.

On December 6 the Rector, Prof. C. J. Ferraris, received in one of the courts of the old University, adorned everywhere with portraits of the most illustrious professors, delegates from the Universities, the polytechnic schools, and Italian and foreign Academies, amounting to nearly a hundred, and amongst them many of those who shed most lustre on contemporary science. The University of Cambridge was represented by Prof. George Howard Darwin, F.R.S., who also represented the Royal Society as Mr. Norman Lockyer, its delegate, had been prevented from attending. The University of Oxford by Prof. E. J. Stone; the Royal College of Physicians, London, by Sir Joseph Fayrer, F.R.S.; the Chemical Society and British Association by Prof. Ludwig Mond, F.R.S.; the Harvard University, Cambridge, U.S.A., by Prof. William James, and the Princeton University by Prof. Allan Marquand; the University of Lund by Prof. R. A. V. Holmgren; the Astronomical Observatory of Paris by its Director, Prof. F. Tisserand; that of Berlin by Prof. W. Foerster; the Polytechnic Schools of Berlin, Karlsruhe, Monaco, Brunswick, Stuttgart, by Profs, Lampe, Keller, Sohncke, Blasing, Lemcke; the University of Göttingen by Prof. Voigt; that of Budapest by Prof. Lanczy; that of Dorpat by Prof. Schmourlo; that of Lausanne by its Rector, Prof. Favey; the Academy of Paris by Prof. Gariel; the Faculty of Letters at Grenoble by Prof. de Croyals; the General Council of the Faculty at Nancy by Prof. Molk, &c., &c. There were also delegates from the towns of Florence, Pisa, Venice, and representatives from the foremost Italian Universities, Academies, and Technical Schools.

The great academical celebration took place on December 7 in the large hall of the University, in the presence of the Hon. Ferdinando Martini, Minister of Public Instruction, who represented the King of Italy. The ceremony was begun with a discourse prepared for the occasion by the Rector Magnifico, and devoted principally to a cordial expression of thanks to the King and to the Minister who represented him; to the foreign and Italian delegates; and to the ladies of Padua, who had given the University a most beautiful banner, on which were various emblems indicating the history of the University, the genealogical tree of the Galileo family, and the ancient inscription above the door of the University—*Gymnasium omnium disciplinarum*.

Next came the commemoration of Galileo by Prof. Antonio Favaro, who has for nearly fifteen years devoted himself almost exclusively to the study of the life and works of Galileo, and to whom was confided by the Government the care of the national edition of the philosopher's works, under the auspices of the King of Italy. The orator kept his discourse within the limits marked out for him, speaking chiefly of Galileo at Padua. Constrained to leave the University of Pisa, Galileo had been welcomed in that of Padua, where he found the "natural home of his mind," a "theatre worthy of his talents." The conditions at Padua at that time were eminently favourable to Galileo's work, for the Venetian

Senate granted the lecturers the utmost liberty, and experimental methods, which could not be learned from books, had been practised at the University for more than a century. Galileo had many opportunities for the development of his genius, both in the lecture-room and in the home, in the preparation of scientific publications, and in the workshops of scientific instrument-makers both in Padua and Venice. To Venice he frequently went, attracted thither by the means it afforded him for study; by that grand arsenal which had already been sung by Dante, and which in his reputed Dialogues is spoken of by Galileo with admiration; but above all by the advantages he derived from scientific intercourse with eminent men who resided in the dominion. The culminating point of the discourse was naturally reached when the orator had to deal with the invention of the telescope, and with the astronomical discoveries made by means of it, the immediate result of which was the recall of Galileo to Tuscany. This did not aid Galileo in his glorious career, or help to protect him from the attacks which were for a long time made on him by invidious adversaries. Even some of his own servants changed at once to implacable and dangerous enemies, and at last he was involved in all the miseries which sprang from the memorable lawsuit. This led the orator to recall the fact that when the clouds assumed their most threatening aspect, the Venetian Republic, forgetting with real magnanimity whatever resentment it might have felt at Galileo's abandonment of his chair at Padua, offered to re-appoint him, and to print at Venice the work which had brought upon him so much trouble. He said also that a pleasant memory of Padua must have passed through the mind of the prisoner of the Holy Office, when there came to him his only comfort, the message from the favourite of his childhood, the nun who in Padua had tenderly cared for him during the first ten years of his youth.

After Prof. Favaro's oration discourses were delivered by the foreign delegates, Holmgren, Fayrer, Darwin, Tisserand, Lampe, Keller, Foerster, Sohncke, Blasing, Lemcke, Farey, Lanczy, Schmourlo, and by Italian delegates, Nardi-Dei, Mantovani-Orsetti, and Del Lungo. Then followed the conferring of University honours, of which seven had been set apart by the Council for seven men of science, one for each nation, all distinguished for their devotion to the studies in which Galileo excelled, viz. Schiaparelli, Helmholtz, Thomson, Newcomb, Tisserand, Bredichir, and Gylden. The degree of philosophy and letters was given to the Minister Martini; of natural philosophy, and philosophy and letters, to the leading delegates. The ceremony was closed by the inauguration of a commemorative tablet in the large hall.

Of the other festivals connected with the celebration it would be out of place to speak here, and it will be better to add a list of the publications which have been issued on the occasion. The oration read in the Great Hall by Prof. Favaro has been published, with the addition of twenty-five facsimiles of documents containing the various decrees of the Senate concerning Galileo, the date of the early prelections given by him at regular intervals, several autographic records of Galileo, chosen in order to give a more exact idea of what are the most precious materials for his biography, the frontispieces of the various publications issued by Galileo, or relating to the time of his sojourn in Padua, the geometric and military compass, the writing presenting the telescope to the Doge, and the first observations of the satellites of Jupiter. A portrait of the great philosopher, from a painting which represents him at the age of forty, taken in 1604, is prefixed.

By favour of the University there have also been published two other works, one containing all the notices of the studies at Padua in 1592, the other proving which was the house inhabited by Galileo and the place in which he made his astronomical observations. The ancient Academy of Padua, among whose founders Galileo is numbered, has issued a publication in which are collected several works dedicated to his memory; and the students of the University have sought to perpetuate the remembrance of this festival by the publication of a "unique number," bringing together all the documents relating to the sojourn of Galileo in Padua, collected from all quarters. These publications will serve as suitable memorials of a great and most interesting celebration.

ANTONIO FAVARO.

SIR RICHARD OWEN.

IT is with great regret that we record the death of Sir Richard Owen He died on Sunday, after a lingering illness, at Sheen Lodge, Richmond Park, in his eighty-ninth year. In publishing his portrait in the series of "Scientific Worthies" (NATURE, vol. xxii. p. 577) we have already presented an estimate of his work and of his place in the history of science. It is only necessary now, therefore, to recall some of the leading facts of his career.

He was born at Lancaster on July 20, 1804, and received his early education at the grammar school of his native place. Afterwards he matriculated at the University of Edinburgh as a medical student. In 1825 he joined the medical school of St. Bartholomew's Hospital, London, and in 1826 he took his diploma at the Royal College of Surgeons. His professional studies having been completed, he began to practise in Serle Street, Lincoln's Inn Fields; but the bent of his mind was towards purely scientific investigation, and he soon had a good opportunity of exercising his powers. Dr. Abernethy, with whom he had acted at St. Bartholomew's as a dissector, had recognized his ability; and, in accordance with the advice of this famous surgeon, he was invited in 1828 to undertake the task of cataloguing the Hunterian collection at the Royal College of Surgeons. The invitation was accepted, and in 1830 the first catalogue of the invertebrate animals in spirits was published. In the same year Owen read at the first meeting of the Zoological Society's committee of Science a valuable paper on the anatomy of the orang-utan, and afterwards he made many important contributions to the Society's Transactions and Proceedings. He was also well known as a reader of papers before the Medical Society of St. Bartholomew's and the Medical and Chirurgical Society of London. In 1832 appeared his well-known essay on the Pearly Nautilus (*Nautilus Pompilius*), in which he gave most striking proof of his power of interpreting the facts of natural history in a thoroughly philosophical spirit.

Before he was thirty years of age Owen had achieved so good a reputation that in 1834 he was appointed to the newly-established chair of comparative anatomy at St. Bartholomew's Hospital. Two years afterwards he succeeded Sir Charles Bell as professor of anatomy and physiology at the Royal College of Surgeons, and he was elected to the newly-established Hunterian professorship at the Hunterian Museum. He also became conservator of the Hunterian Museum on the death of Mr. Clift, whose daughter he had married. He had gradually been withdrawing from the practice of his profession, and ended by devoting the whole of his time and energy to scientific work.

His connection with the Royal College of Surgeons lasted for twenty years, and during this period he achieved results which placed him in the front rank of original investigators. In the article to which we have referred we have already indicated the nature and importance of these results, and need not go over the same ground again. It must suffice to mention the completion, in five volumes, of his catalogue of the Hunterian collection; his "Odontography"; his Lectures on Comparative Anatomy and Physiology; his "Archetype and Homologies of the Vertebrate Skeleton"; his memoirs on "The Nature of Limbs" and on "Parthenogenesis"; his monograph of British fossil reptiles; and his papers on the fossil birds of New Zealand, and on some fossil mammals of Australia. In 1856 he was appointed Superintendent of the Department of Natural History in the British Museum. How splendidly he fulfilled the duties of this position all the world knows. He fought steadily and earnestly to obtain proper accommodation for the magnificent collection placed under his charge, and to him, more than to any one, Great Britain owes the fact that this particular set of her scientific treasures is now so securely preserved and so finely displayed. The practical duties of his office were not allowed to interrupt his scientific researches, and year after year he continued to give fresh evidence of the astonishing range of his knowledge and of his remarkable capacity for far-reaching and brilliant generalization. Among the writings of this period are his Manual of Palæontology, and his memoirs on the classification and geographical distribution of mammals, on the British fossil reptiles of the Liassic formations, ichthyosaurs and plesiosaurs, on the British fossil cetacea of the Red Rag, on the British fossil reptiles of the Mesozoic formations, pterodactyls, and on the fossil reptiles of South Africa.

In 1883 he resigned his official position, but he did not cease to interest himself in the studies in the prosecution of which he had displayed so commanding a genius. In 1884 he issued in three volumes his great "History of British Fossil Reptiles," and until a comparatively recent date he submitted to the Royal Society from time to time papers embodying the more important results of his labours.

In the course of his long career Owen did much good service as a member of various Commissions, and it is scarcely necessary to say that honours of many different kinds were conferred upon him. About these matters we have given all necessary information in our previous article. Owen was very far from being content merely with the collection and classification of facts; he sought also to bring out the ideas in which his facts seemed to him to find their ultimate significance. He was unable to adopt the theory of evolution as presented by Darwin, but his researches did much to prepare the way for the general and rapid acceptance of Darwin's hypothesis, since it was felt that there must be some strictly scientific explanation of the affinities by which he had shown vast groups of animal forms to be allied to one another. Apart altogether from its speculative aspects, his work is universally acknowledged to be of high and enduring value, and there can be no doubt that he will rank among the strongest and most impressive figures in the intellectual history of the nineteenth century.

He desired that his body should be buried beside that of his wife in Ham Churchyard, and his wish is, of course, to be complied with. At the funeral, which will take place to-morrow (Friday), there will be representatives of all the learned societies with which he was connected.

NOTES.

The following memorial, numerously signed, has been presented by Sir Henry Roscoe to the Right Hon. the Earl Cowper, Chairman of the Royal Commission on the Gresham University:—The undersigned desire hereby respectfully to record their strong opinion that the foundation of a Teaching University for London, without due provision being made for higher Education and original Research, would be unworthy of the Metropolis, and would entail the neglect of an admirable opportunity for promoting the advancement of Science and Learning. The signatures cannot fail to command attention. The following learned Societies are represented by their Presidents:—The Royal Society, the British Association for the Advancement of Science, the Royal Dublin Society, the Royal Society of Edinburgh, the Iron and Steel Institute, the Physical Society, the Institution of Electrical Engineers, the Institute of Mechanical Engineers, the Chemical Society, the Royal Horticultural Society, the Pharmaceutical Society of Great Britain, and the Institute of Chemistry of Great Britain and Ireland. Eton College, Harrow School, Rugby School, and St. Paul's School are represented by their head-masters. There are also representatives of the University of Oxford, Cambridge, Edinburgh, Glasgow, Aberdeen, and St. Andrews, the Victoria University, the British (Natural History) Museum, the Royal College of Science, London, University College, London, Mason College, Birmingham, Durham College of Science, Firth College, Sheffield, University College, Dundee, University College, Bristol, City and Guilds of London Central Institution the Royal College of Science, Dublin, and the Pharmaceutical Society of Great Britain. A special group of signatures consists of the names of a number of Fellows of the Royal Society.

Sir Joseph Lister, Sir Henry Roscoe, and Prof. Ray Lankester, will represent the Royal Society at the Pasteur celebration in Paris on the 27th inst. Captain Abney has been invited to represent the Society at the 150th anniversary of the American Philosophical Society in May 1893.

We are glad to see that a movement has been started for the purpose of securing that due honour shall be done to the memory of Jean Servais Stas, one of the most illustrious of modern chemists. It is proposed that a new edition of his writings shall be issued, his memoirs, notes, and reports being grouped in their proper order, and that a commemorative monument shall also be erected. An influential committee, representing science in all parts of the world, has been appointed to take the necessary steps. Subscriptions will be received by M. L. Errera, 1, Place Stéphanie, Brussels.

The Committee of the International Electrical Exhibition to be held at Milan in 1894 proposes, according to *La Lumière Electrique*, to offer a prize for the most important invention or discovery in the province of electricity, especially in connection with the transmission of energy to a great distance, and its distribution and transformation for industrial uses.

Successful experiments have been made in France relative to the introduction of telephones for use in warfare. The telephonists are organized in sets of two men, each set being provided with equipment for a mile line. The very simple receiving and transmitting apparatus are attached to the military cap, and the wire is on reels in a sort of breast-plate, the whole being so light that a man's ordinary equipment weighs less than six pounds.

The tunnel at Niagara Falls is finished, and the power plant will be in operation by next March. It is expected that a current of 45,000 electric horse-power will be transmitted from there to Buffalo, and 30,000 to other points.

M. Maurice Mallet, in *L'Aéronaute*, describes what he claims to be the longest balloon ascent on record. His balloon, "Les Inventions Nouvelles," started from the gasworks of La Villette, Paris, on October 23, and the voyage terminated at Walhen, in Central Germany, at 6 a.m. on the 25th, after a total journey of 36 hours 10 minutes above ground. The flight was interrupted several times by the snow which fell in the higher regions of the atmosphere. When lower strata were reached, the snow melted, and the balloon regained its ascending power. During one of these descents it was stopped and examined by a Prussian *gendarme*, who had followed it at a gallop for some distance. The route passed over part of Belgium, the Taunus, and the Odenwald, and the towns of Metz and Frankfurt were recognized in passing.

THE "Annals of the Harvard College Observatory" contain a discussion by H. H. Clayton of the cloud observations made at Mr. A. L. Rotch's observatory at Blue Hill, Massachusetts. One of the most noticeable facts brought out by the measurements of cloud heights and velocities, which have been conducted with great care, is the difference in height between the same clouds in summer and winter, the clouds, with few exceptions, being lowest in winter. The bases of the cumulo-nimbus clouds, however, are generally lower in summer, while, at the same time, their tops are higher than in winter. The heights of the different clouds were found to maintain an almost constant ratio to each other. The mean velocities recorded showed that the entire atmosphere moves twice as fast in winter as in summer. The mean velocity of the highest clouds in winter was about 100 miles an hour; the extreme velocity amounted to 230 miles an hour, from which it appears that the upper currents are much more rapid over America than over Europe, which possibly explains the greater velocity of the storms in America. As regards the directions of cloud movement, the tables show that from the highest clouds to the earth's surface, the prevailing wind is west; above 4000 metres more than 90 per cent. of the observations show the clouds from some point between south west and north-west inclusive. In the cirrus and the cumulus regions, and near the earth's surface, the prevailing direction is from a little north of west, but in the intermediate levels, from a little south of west, the excess of the southerly component in these regions being possibly due to the influence of cyclones.

THE weather during the past week has been generally very dull, and scarcely any rain has fallen over the southern parts of the kingdom. Butween Friday and Monday there were several depressions to the northward of our islands, passing in an easterly direction, which caused very severe gales and high seas on the coasts of Scotland, the difference on pressure on Sunday between the north and south of our islands being more than an inch. During the first part of the period the temperature was unusually high for the season, the maxima exceeding 55° in some parts, and the night minima were occasionally higher than the average daily maxima for the month; subsequently, however, a decided fall occurred, with fog and mist in most parts of England, while in Scotland hail and sleet showers were experienced. The *Weekly Weather Report* of the 17th instant shows that for that period the temperature was from 2° to 4° above the mean. Rainfall exceeded the mean in the north of Scotland only, and just equalled it in the north of Ireland; in all other parts there was a deficiency. Bright sunshine was much less prevalent than during the preceding week, although in most parts of England the amount exceeded the average.

PROF. COLE writes from Dublin that the afterglow in the west and zenith on Saturday, December 17, was of a superbly brilliant character. Mr. R. Langton Cole observed that in London on December 15 the whole sky was covered by the glow, which was deeper all round towards the horizon.

AN interesting lecture on "Water and Water Supply" was delivered last week at the London Institution, by Major L. Flower, of the Sanitary Institute. As an instance of the important part which water played in the economy of nature, he mentioned that if a man weighing 140 lbs. were placed under a hydraulic press and squeezed flat, the result would be 105 lbs. of water and only 35 lbs. of dry residue, which was a fact for conceited people to reflect upon. Major Flower gave some interesting facts about the rainfall of England. It is, of course, highest in mountainous districts, the maximum fall being found in Cumberland, where the record for six years shows an annual rainfall of 165 in. The lowest in England is between Biggleswade and Bedford, where it reaches only 20 in. London and the east coast average about 25 in. Speaking of drinking water, Major Flower said the best way to get it was to bottle it at the fountain-head and have it delivered in bottles, which had been done already and might be done to a greater extent in the future.

MR. W. F. HOWLETT writes to us from Pahiatua, New Zealand:—"Can you inform me what is now sold in England as gum arabic? I used to be able to buy a soluble gum; what I get now is the same in appearance, but it will not dissolve. It swells up, truly, but will not form a homogeneous filterable solution. It would be a great boon to small buyers if such things were sold under their proper names. Am I right in supposing that since the Soudan trouble gum arabic has disappeared from commerce?"

A VERY interesting report on artesian boring, by Mr. J. W. Boultbee, is included in the volume containing the annual report of the Department of Mines and Agriculture, New South Wales, for the year 1891. Mr. Boultbee shows that, as a rule, artesian waters are suitable for irrigation purposes, only those heavily charged with salt or alkaline matters being unsuitable; and he can see no reason why such irrigation should not be an element of immense value, deserving the utmost consideration in connection with the development of that north-western portion of the colony, where the fertility and recuperative powers of the soil are so wonderfully illustrated by the growth of feed after rainfall at the proper season. The average quantity of water required for the irrigation of grain crops, based upon the experience of other countries, may be roughly estimated at 72,600 cubic feet, or 543,485 gallons per acre. One inch of rain would equal 3630 cubic feet, or 22,622 gallons per acre. A rainfall of 20 inches would therefore yield 72,600 cubic feet, or 543,485 gallons per acre. 640 acres would consequently require 46,464,000 cubic feet, or 347,830,400 gallons upon them as an equivalent to 20 inches of rain. When it is considered that the flow per diem from the Native Dog Artesian Bore, 45 miles from Bourke, is approximately 2,000,000 gallons per diem, or 730,000,000 gallons per year, it will be seen that upon the foregoing basis a supply of water equal to a rainfall of 40 inches per annum, per 640 acres is available, or that an area of considerably over 1280 acres can be supplied with water equalling a rainfall of 20 inches per annum.

THE Cambridge Local Lectures Syndicate have just issued an announcement of their next Summer Meeting of University Extension students, to be held at Cambridge in August, 1893. The programme is a large and varied one, and a number of well-known lecturers have already promised their services. Among the scientific lecturers we notice the names of Sir Robert Ball, Sir H. E. Roscoe, Mr. Pattison Muir, and several of the best known of the Cambridge Extension lecturers. Cambridge has always laid great stress on the importance of providing, as far as possible, practical work in science as well as theoretical teaching. It has seldom been found possible to arrange much practical work in connection with the lectures given in the provinces, chiefly on account of the difficulty of finding laboratory accommodation. But students who can spare a fortnight—or, better still, a month—have now the opportunity of coming to Cambridge and seeing, at any rate, something of the resources of the University laboratories. Even two or three weeks' work in a well-equipped laboratory may easily be a revelation to a student who has hitherto learnt his (or her) science from books or lectures. The laboratory work has always formed an important and highly appreciated part of the

Cambridge Summer Meetings. Next year no less than five practical courses are promised, viz. in physics, chemistry, botany, physiology, and palæontology, thus providing for a considerable variety of taste, and for the accommodation in the laboratories of a fairly large number of scientific students. Another feature in the programme is an entire novelty. It is proposed to give a series of short courses of lectures on the growth of various sciences—astronomy, physics, chemistry, and geology—to illustrate from different points of view the methods by which discoveries are actually made, and science makes progress. These will be accompanied by a short theoretical course on scientific method. The sciences selected only cover a small portion of the whole field, and some aspects of scientific method—such as classification—will obviously scarcely be represented. The organic sciences generally are left out, and may possibly form the groundwork of a similar scheme on some future occasion. The idea of illustrating scientific method by the history of science is a familiar one, and is the basis, for example, of Whewell's great books on "The Philosophy and the History of the Inductive Sciences." Few men, however, possess the encyclopædic knowledge of science which Whewell had, and the progress of science since his day would make such a task as he undertook well-nigh impossible for a more modern writer. The Cambridge Syndicate do not attempt to find a Whewell, but hand over the history of each science to competent specialists, and hope to give real unity to the whole by the lectures on method, in which the lessons taught by the history of the various sciences will be brought into a focus, and made to lead up to general principles. The experiment is certainly an interesting one, and we shall watch with some interest to see how it succeeds. The programme includes also lectures on history, literature, art, and other subjects. But we have dwelt only on the science as being of special interest to our readers.

IN the Herz oscillator, as used hitherto, the spark discharge of a Rhumkorff has been produced in air between two balls. MM. Sarasin and de la Rive lately thought (*Arch. de Sciences*) to place the balls in an insulating liquid, and they find that this gives a more intense effect in the resonator. Olive oil does best; oil of turpentine, liquid paraffin, and petroleum were also tried. Placed near the oscillator the resonator gives quite a bright spark, and at about 30 ft. distance, with a resonator of large diameter, the spark is strong enough to be visible a good way off.

ATTEMPTS are being made to create a silk-producing industry in the district of Nicolaieff, in South Russia: and, according to the British Vice-Consul at Nicolaieff, the result is not unlikely to be satisfactory. He says that the mulberry tree, for the growth of which the soil and climate are well adapted, flourishes wherever it is planted, and that with very little trouble or expense every little plot of ground, now yielding nothing more than a crop of weeds, might in a short time be transformed into a remunerative feeding-ground for the silkworm. The matter has been taken in hand by a society, and every encouragement is given to the peasants and poorer classes to take advantage of the opportunities provided for them. If seriously followed up, the scheme may, the Vice-Consul thinks, prove a source of revenue to many a poor family, and eventually be the means of establishing a large and flourishing industry.

AT a recent meeting of the Trinidad Field Naturalists' Club there was some discussion as to the question whether the bite of the tarantula (Mygale) spider is poisonous. Mr. C. W. Meaden, writing to the Club's journal on the subject, describes an incident which came under his own observation. Early in the present year he had a gang clearing some land after burning, and on visiting them one afternoon he saw a black tarantula dart from a heap of bush and deliberately bite one of the prisoners on the heel and then scamper away, which it did with safety to itself, although chase was made after it. The spider seemed to be in an angry mood at being disturbed in a favourite haunt for food and shelter. The bite drew blood, about two or three drops. A Trinidad labourer's foot is thick enough almost to resist an auger, yet the spider managed to penetrate, so it may safely be asserted it was in earnest. Immediately the bite was given a shout went up, "The man is bitten by a big black spider—a tarantula!" This made the bitten one almost frantic with fright, and he cried out piteously, "Me God, me go die in gaol, me God," &c. Mr. Meaden took him to the infirmary, some 300 yards distant, and the sufferer carried his heel in his hand, *i.e.* hopped all the way. His foot was fomented with hot water, and spirits of ammonia were applied, with the addition of a little liquid ammonia, and he received a dose of ether mixture. About two hours afterwards he ate his dinner heartily and slept well at night. He complained of no pain in the morning, and went to work as usual. There was no local swelling or inflammation, and but little pain at any time. Fright was the only ill effect.

SOME interesting results in application of cold have been recently recorded. Thus M. d'Arsonval has found that while with rising temperature, microbes die before soluble ferments, with lowered temperature the opposite occurs. The invertine of beer yeast cooled to −40°C., does not lose its power, but it is destroyed as a ferment at −100°. On the other hand, the yeast itself cooled to −100° is still active. M. Raoul Pictet has lately observed that at −150° all chemical reaction is suppressed. Thus, if sulphuric acid and potash are brought together at this temperature, they do not combine. Litmus paper, introduced, keeps its colour. Curiously, it is possible to restore their energy to these inert substances, by passing an electric current, and the current passes readily whatever the substances; at −150° all bodies are good conductors. The disappearance of affinity at a low temperature can be utilized to get absolutely pure substances, and M. Pictet has thus obtained alcohol, chloroform, ether, and glycerine.

SOME good notes on the Shuswap people of British Columbia, read before the Royal Society of Canada by Dr. George Dawson, F.R.S., are now printed in the Society's Transactions, and have also been issued separately. In an interesting section on the superstitions of the Shuswaps he notes that they have a singular idea about certain small lizards. A man who sees one of these creatures is supposed to be followed by it wherever he may go during the day, till at length, when he is asleep during the following night, it finds him, and entering his body, proceeds to eat out his heart, so that he quickly dies. The late Mr. Bennett, of Spallumsheen, told Dr. Dawson in 1877 that the Indians employed by him in making a ditch for purposes of irrigation, on coming into camp in the evening, would jump several times over the fire in order to lead the possibly pursuing lizard to enter the fire and be destroyed in attempting to cross. He also noticed that they carefully tied up the legs of their trousers when retiring. If while at work during the day they saw one of these little lizards, which appeared to be abundant in that locality, it would be caught in a forked twig, the ends of which were then tied together with a wisp of grass and the butt end of the twig afterwards planted in the soil. Thus treated, the lizard soon died and become a natural mummy. If during the progress of the work any one found and carelessly tossed aside one of these lizards, the Indians would throw down their tools and search diligently until they found it, and secured it in the manner just described. Dr. Dawson thinks that this superstition must be widespread among the Indians, for it was afterwards related to him in identical form by a man of the Nicola River, who further pointed out a small lake, singularly situated on the summit of a high ridge about a mile and a half south of the mountain named Za-kwās'-ki, as a noted resort—possibly the only place known to the man—of this peculiar animal. He described it as being a few inches in length and nearly black. Za-kwās'-ki, to which other stories attach, is south of Nicola River, at the source of the Nicoamen River.

A COMMON impurity in many seeds which are used as food for live-stock is the seed of corn-cockle (*Agrostemma githargo*). Notably is this the case on the Continent, and especially in Hungary, where the refuse from the machines used in cleaning grain consists chiefly of cockle-seed, and is largely used in feeding swine. It appears, as a rule, to have no ill-effect upon these animals. Upon other animals, however, it sometimes has serious and even fatal effects, especially upon calves and dogs. According to Kobert (*Landw. Centralblt. Provinz Posen*, 19) it would appear that the seeds contain a glucoside—saponin $C_{32}H_{54}O_{18}$,—which acts as a poison either when eaten in the form of cockle-seed or when introduced into the blood. Various animals are affected in different degrees, but dogs, cats, and birds soon die when fed upon the seed. The poison decomposes the blood, dissolving the red corpuscles, and also destroys the sensitive albuminoid portion of the nerve elements. Heating to 50° C. decomposes the saponin, and renders the seed harmless. Since this glucoside is found to lie only just below the surface of the seed, Kobert suggests that the seed should be coarsely ground and the outer husk separated ; to cook the meal would be a still safer precaution. A good deal of cockle-seed comes into the port of Hull, chiefly, it is presumed, amongst grain which has not been screened. From such seeds as linseed it is removed by screening before pressing, but it is too often found in the cake which results after the oil is expressed from the linseed. A considerable quantity of corn-cockle is handled in Hull, whatever its ultimate destination may be, and it sometimes occurs in feeding-stuffs in far too large a percentage to be considered as an accidental impurity. Its use in admixture (as impurity or otherwise) with other feeding-stuffs is strongly to be deprecated so long as there is the slightest risk attending its consumption by any domestic animal. Its detection is very easy, the peculiar rough husk of the seed being characteristic ; the husk, after clearing with dilute sulphuric acid, and then with caustic soda, and examined under a low power of the microscope, will exhibit dark-red convoluted markings which distinguish it clearly from the husk of any other well-known seed.

IT is a well-known fact that sea-anemones have a sense by which they recognize food. This has been studied recently by Herr Nagel at the Zoological Station in Naples, and he has endeavoured to localize it. Among other experiments, a small piece of a sardine was brought carefully to the tentacles of one of these animals ; the tentacle first touched, then others, seized the food and surrounded it, and the morsel was swallowed. A similar ball of blotting-paper saturated with sea-water, brought near in the same way, was not seized. If, however, the ball was soaked in the juice of fish, it was seized with the same energy as the piece of fish, but often liberated again after a time without being swallowed. Blotting-paper saturated with sugar acted like the other, but more weakly. If saturated with quinine, it was refused, the tentacles drawing back. On the outer surface of the body, as also in the part between the tentacles and the mouth, quinine had no effect, nor had coumarin, vanillin, or picric acid. When a piece of meat was placed in or near the mouth of a widely-open animal, no notice was taken of it ; it was only seized when the tentacles were touched. Thus the sense of taste seems to be in these alone. Cutting the tentacles did not evidently give pain, but these organs appeared sensitive to heat and to touch, so that they appear to be the seat of three senses.

MR. JOHN MURRAY has published a fourth edition of Dr. W. Fream's "Elements of Agriculture." The work was originally issued at the beginning of the present year, and two editions were sold out before the end of January. The third edition has for some time been out of print. The book has now been thoroughly revised, and enriched with a completely new set of illustrations.

A NEW edition of Dr. John Casey's "Sequel to the First Six Books of the Elements of Euclid" has been issued as a volume of the Dublin University Press Series. The work has been edited by Prof. P. A. E. Dowling, by whom it has been carefully revised and considerably enlarged. The editor has obtained much valuable aid from Prof. Neuberg, of the University of Liège.

MESSRS. BLACKIE AND SON have issued a second edition, revised and enlarged, of Mr. J. McGregor-Robertson's "Elementary Text-book of Physiology."

A FURTHER communication concerning the nature and properties of hydroxylamine, NH_2OH, is contributed to the *Recueil des travaux chimiques des Pays-Bas* by M. Lobry de Bruyn, whose isolation of the free base was described in our note of vol. xlv. p. 20. It may be remembered that pure hydroxylamine was found to be a solid substance, crystallizing in colourless thin plates or needles, which are extremely deliquescent. So powerful indeed is the affinity of hydroxylamine for water, that the crystals rapidly dissolve when exposed to the air, in the moisture attracted. The crystals melt at a temperature of 33°, and the liquid boils at 58° under the reduced pressure of 22 millimetres. If the liquid is heated under ordinary atmospheric pressure in contact with the air, it explodes with great violence when a temperature between 60° and 70° is attained ; if the experiment is carried out in a vessel from which air is excluded, the liquid may be heated as far as 90° without accident, regular decomposition into gaseous products occurring at this temperature. Explosion, however, usually follows at once if this temperature is much exceeded, and generally after a short time if the source of heat is removed as soon as the thermometer has reached 90°, inasmuch as the decomposition which is induced at this temperature is accompanied by evolution of heat. The crystals are without odour. They react with considerable violence with the halogen elements, the reaction in the case of chlorine being accompanied by production of flame ; the products do not appear to have been investigated as yet beyond ascertaining the presence among them of the halogen acids. Metallic sodium also vigorously attacks hydroxylamine, brilliant incandescence occurring. Warm zinc dust reduces it to ammonia so rapidly, that if any considerable quantities are employed a violent explosion follows. Highly oxidized compounds, such as potassium permanganate, chromates, bichromates, or chromic acid react with crystals of hydroxylamine, as may be expected, in a most energetic manner, brilliant flame being produced often accompanied by detonation. Chlorates, perchlorates and bromates behave similarly in the presence of a drop of sulphuric acid. Hydroxylamine liberates iodine from iodic anhydride, and rapidly reduces iodates to iodides. Dehydrated sulphate of copper inflames in contact with the crystals of the base, and powdered nitrate of silver is reduced to metallic silver. Addition of trichloride or pentachloride of phosphorus to the crystals likewise brings about ignition. Hydrogen peroxide oxidizes hydroxylamine to nitrous acid. These reactions, selected from a large number which M. de Bruyn describes, amply demonstrate the remarkable chemical energy with which anhydrous hydroxylamine is endowed. It is interesting to learn that the melted substance is capable of dissolving a considerable volume of ammonia gas. Moreover, carbon dioxide and sulphuretted hydrogen are so soluble in melted hydroxylamine that viscous liquids are produced which remain liquid even at −10°. As regards the preparation of the base, M. de Bruyn has now succeeded in obtaining a hundred grams of the pure crystals from a little more than a kilogram of the hydrochloride, by the method described in our previous note above referred to.

THE additions to the Zoological Society's Gardens during the past week include a red and yellow macaw (*Ara macao*) from Central America, presented by the Rev. T. N. Talfourd Major; two gold pheasants (*Thaumalea picta* ♀ ♀) from China; an Alpine Chough (*Pyrrhocorax alpinus*), European, purchased.

OUR ASTRONOMICAL COLUMN.

COMET HOLMES (NOVEMBER 6, 1892).—The following ephemeris, taken from *Astronomische Nachrichten*, No. 3131, gives the position for Comet Holmes for the ensuing week:—

Berlin, Midnight.

1892.	R.A. (app.). h. m. s.	Decl. (app.). °	Log *r*.	Log Δ.
Dec. 23 ...	0 55 44 ...	+34 19·2		
24 ...	56 37 ...	16·0 ...	0·4050 ...	0·3049
25 ...	57 31 ...	12·9		
26 ...	58 27 ...	10·0		
27 ...	0 59 24 ...	7·2		
28 ...	1 0 22 ...	4·5 ...	0·4073 ...	0·3167
29 ...	1 1 21 ...	34 1·9		

M. Deslandres, in *Comptes rendus* for December 12 (No. 24), informs us that on November 21 he obtained a photograph of this comet between 10h. 40m. and 11h. 20m. Paris mean time, showing distinctly "un commencement de dédoublement." Owing to the bad weather no other negatives were taken until December 10, but although the time of exposure was an hour, the comet's impression was not obtained, thus confirming the present eye observations that its intensity is slowly decreasing.

COMET BROOKS (NOVEMBER 20, 1892).—The following ephemeris of Comet Brooks is that obtained by Berberich, and varies a little from that given last week, as will be seen by comparing the values for December 22, with those given last week:—

Berlin, Midnight.

1892.	R.A. h. m. s.	Decl. ° ′	Log Δ.	Log *r*.	Br.
Dec. 22...	14 26 9 ...	+42 50·2			
23...	14 33 28 ...	44 40·4 ...	9·9211 ...	0·0880 ..	5·59
24...	14 41 35 ...	46 34·4			
25...	14 50 41 ...	48 31·6 ...	9·9013 ...	0·0861 ...	6·18
26...	15 0 55 ...	50 31·4			
27...	15 12 26 ...	52 32·9 ...	9·8838 ...	0·0845 ...	6·75
28...	15 25 30 ...	54 34·5			
29...	15 40 21 ...	56 34·4 ...	9·8694 ...	0·0831 ...	7·26

SWIFT'S COMET.—*Knowledge* for December 1 contains three most interesting photographs of Comet Swift, taken by Prof. Barnard at the Lick Observatory on April 4, 6, and 7 respectively. These photographs, which are obtained from the original negatives after an enlargement of 2¼ times, show what good photographic work can be done even with small instruments when exposures are somewhat lengthened. In this case a 6-inch Willard lens of 31 inch focal length was strapped on to the tube of a 6½-inch equatorial, and the exposures given amounted to 60, 65, and 50 minutes. The ordinary driving-clock, combined with a slight hand movement at the eye end, were all that was required to compensate for the diurnal and proper motion of the comet. The star trails on the plates pointed out then the comet's proper motion. Although these photographs were taken at such short intervals the changes recorded are most striking, the pictures bearing very little likeness to one another. On this point Prof. Barnard says: "Had they been drawn by the most competent observer, most astronomers would leave their remarkable differences to the unskilful hand of the artist, for there is absolutely no resemblance among them." The photographs here referred to are from a series taken at Mount Hamilton, and in examining them he mentions that in the case of this comet he has been led to forcibly believe that in a comparatively short period there occurred a rotation of the tail "upon an axis through the nucleus."

ULTRA-VIOLET SPECTRUM IN PROMINENCES.—In the current number of the *Memorie della Società Degli Spettroscopisti Italiani*, Prof. G. E. Hale communicates a note on some photographs of the ultra-violet region in the spectra of solar prominences. On October 15 at 3h. 15m. a photograph of the spectrum of a metallic prominence was obtained, which contained as many as 74 bright lines in the ultra-violet between wave-lengths 3970 and 3630. The photograph, besides displaying all the lines previously recorded by Prof. Hale and M. Deslandres, contained 32 additional lines which had not been previously known. The following table shows their respective wave-lengths, which are to be regarded yet as only approximate:—

λ		λ		λ
3964	...	3863	...	3724·3
3956·9	...	3850·5	...	3716·9
3945·2	...	3813·5	...	3710·3
3938·1	...	3774	...	3699·5
3913·5	...	3767·1	...	3683
3965	...	3758	...	3681
3895·5	...	3757	...	3679·5
3893·8	...	3749·7	...	3674·2
3891	...	3741·7	...	3662·2
3878·8	...	3733·3	...	3647·8
				3632
				3630·8

Besides these lines the photograph shows traces of the lines λ 3807·2, 3802, 3764, 3763, 3758·2, 3709·5, 3707·8, 3676, 3643.

EPHEMERIS FOR BODIES MOVING IN THE BIELA ORBIT.—In *Astronomical Journal*, No. 281, Dr. Chandler communicates an ephemeris for the use of those wishing to search for bodies which may be moving in the orbit of Biela's comet. The ephemeris is given for every eight days. It is based on the orbit obtained by Michez, who calculated the principal perturbations up to 1866. In the present computations Dr. Chandler has not taken into account any disturbance that may have been produced by the proximity of the planet Jupiter, or any perturbation that might have ensued from an approach to our earth. The values are given up to the end of February, 1893.

MADRAS MERIDIAN CIRCLE OBSERVATIONS.—The Government of Madras has lately issued the results of observations of the fixed stars, made with the meridian circle during the years 1874–76. During this interval no change whatever was made either in the instrument or in the methods of reduction. The volume gives the instrumental corrections for these years, the separate results of observations for each year, with the mean positions of the stars brought up to January 1 of each year, and corrections to the Nautical Almanac stars for the period in question.

THE JUBA RIVER.

AT the meeting of the Royal Geographical Society on Monday evening, Commander F. G. Dundas, R.N., read a paper describing his ascent of the Juba river. This was the first serious attempt to explore the river since Von der Decken's ill-fated expedition in the *Guelph* in August, 1865. The stern-wheel steamer *Kenia*, belonging to the Imperial British East Africa Company, under the command of Captain Dundas, crossed the bar of the Juba on April 25, 1892, an operation of much danger, as the vessel was exposed broadside on to heavy rollers; the depth at high water is only one fathom, and the water swarms with sharks and crocodiles. The coast Somalis lined the bank with hostile movements as soon as they saw that the vessel was to go up the river, and detained the expedition for a fortnight, until a message was sent to the head chief, the Sultan of the Ogaden Somalis. It was July 3 before amicable arrangements could be made, and the expedition fairly started. The Somalis met with everywhere were very strict Mohammedans, and secluded their women, but a number of Galla slave-girls were seen amongst them. There were few villages, Hajowan and Hajaualla opposite each other near the mouth being the only large ones until Munsur, 360 miles, and Bardera, 387 miles from the sea, were reached. The lower reaches of the river were very winding. On one occasion Captain Dundas observed a stream flowing parallel to the river he was on, and going across to see it recognized the landmarks as those he had passed three hours before. The Waboni tribe, who live by hunting, and use the bow and arrow, occupied the thick woods of the lower river. Above them the curves became more gentle, and the Gusha district was reached, where the people cultivated the land, which was cleared by burning; and for a hundred miles the *Kenia's* furnaces were fired with the dead trees which had been killed, but left unburnt by the fire. Cotton is cultivated as well as food plants, and there is a primitive system of weaving. Above Bilo, and about 100 miles from the sea, a branch was found to run off from the main river to the south-west through very dense forests. This is probably the Sheri, which reaches the sea midway between Lamu and Kismayu; the land between this and the Juba mouth being probably of deltaic origin. This branch was explored in a boat for twenty miles. The dense forests formed a broad belt on both sides on the river, and after steaming for five days through uninhabited woods the *Kenia* suddenly emerged into open country on August 2. The people were of very mixed race, friendly and well supplied with all sorts of food. Hills began to appear, and the river grew shallower, until on August 10 the steamer moored to the bank opposite Bardera. Here the Sultan forbad a landing, and the people, who numbered about 1200, were hostile, but ultimately peace was arranged, and one of the subordinate sheiks accompanied the *Kenia* to the rapids, where the river sweeps between steep rocky hills 300 to 400 feet high. There are three channels in the rapids, but at the time of the visit none was navigable, and the natives reported a waterfall over a ledge of rock about four hours' march further up, in latitude 2°34′ N. The wreck of the *Guelph* was visited and examined, but the rapid falling of the water made it necessary to hasten back to the sea. The climate throughout was found agreeable, and there were few mosquitoes. The river does not overflow, so there are no malarial swamps along the banks.

BREATH FIGURES.[1]

FIFTY years back Prof. Karsten, of Berlin, placed a coin upon glass, and by electrifying it made a latent impression, which revealed itself when breathed upon. About the same time Mr. W. R. (now Sir W. R.) Grove made similar impressions with simple paper devices, and fixed them so as to be always visible. A discussion of Karsten's results occurs in several places, but I have not been able to find details of his method of performing the experiment. During my attempts to repeat it some effects have appeared which seem to be new and worthy of record.

After many trials I found the following method the most successful:—A glass plate, six inches square, is put on the table for insulation: in the middle lies a coin with a strip of tinfoil going from it to the edge of the glass: on this coin lies the glass to be impressed, four or five inches square, and above it a second coin. It is essential to polish the glass scrupulously clean and dry with a leather: the coins may be used just as they usually are, or chemically cleansed, it makes no difference. The tinfoil and the upper coin are connected to the poles of a Wimshurst machine which gives three or four inch sparks. The handle is turned for two minutes, during which one-inch sparks must be kept passing at the poles of the machine. On taking up the glass one can detect no change with the eye or the microscope; but when either side is breathed upon, a clear frosted picture appears of that side of the coin which had faced it: even a sculptor's mark beneath the head may be read. For convenience those parts where the breath seems to adhere will be called white, the other parts black. In this experiment the more projecting parts of the coin have a black counterpart, but there is a fine gradation of shade to correspond with the depth of cutting in the device: the soft undulations of the head and neck are delicately reproduced.

The microscope shows that moisture is really deposited over the whole surface, the size of the minute water granulation increasing as the point of the picture is darker in shade.

There seems to be no change produced by the use of coins of different metals.

If sparking is allowed across the glass instead of at the poles of the machine, traces of metal are sometimes deposited beyond the disk of the coin, but not within it.

[1] Paper read by Mr. W. B. Croft before the Physical Society of London on June 24, 1892.

Around the disk is a black ring quarter inch broad: sometimes the milling of the coin causes radial lines across this halo.

If carefully protected there appears to be no limit to the permanence of the figures, but commonly they are gradually obscured by the dust gathered up after being often breathed upon: some of the early ones, done more than two years back, are still clear and well defined in the detail.

It is possible to efface them with some difficulty by rubbing with a leather whilst the glass is moist. They are best preserved by laying several together when dry and wrapping them in paper: they are not blurred by this contact.

It is a curious fact that certain developments take place after a lapse of some weeks or months. The dark ring around the disk gradually changes into a series of three or four, black and white alternately; other instances of such a change will be noted below.

Let it be noticed that in coin pictures the object is near to, but not in contact with, the glass: for in the best specimens the rim of the coin keeps the inner part clear of the surface.

Obviously a small condenser is made by the coins: it is not essential; at the same time images made by a single coin, put to a single pole, are inferior.

The plan which gives the surest and most beautiful results is to place five or six coins, lying in contact side by side in a cross or star, on either side of the glass: it is not necessary that each coin should exactly face one on the other side.

There has not appeared any distinction between the figures made by positive and negative electricity.

When several coins are placed side by side, touching one another, there appear in the spaces between them, which are mostly black, well-defined white lines, common tangents to the circular edges of the coins. If these are of equal size the lines are straight; otherwise they are curved, concave towards a smaller coin. They seem to be traces in that plane of the loci of intersection of equipotential surfaces.

Similar effects are obtained when coins and glasses are piled up alternately, and the outer coins are put to the poles of the machine. With six glasses and seven coins perfect images have been formed on both sides of each glass. With eight glasses the figures were imperfect; but there is little doubt this could be improved by continued trials as to the amount of electricity applied.

If several glasses are superposed and coins are applied to the outer surfaces, there are only the two images at the outside. After the electrification there is a strong cohesion between the plates.

It requires some practice to manage the electrification so as to produce the best results. There are two forms of failure which present interesting features. Sometimes a picture comes out with the outlines dotted instead of being continuous. At other times, if the electrification is carried too far, the impression comes out wholly black; but on rubbing the glass when dry with a leather the excess is somehow removed. Naturally it is difficult to rub down exactly to the right point, but I have succeeded on several occasions in developing from a blank all the fine detail of elaborate coins.

Here, again, we have another instance of the development by lapse of time, for an over-excited piece of glass usually gives a clear picture after an interval of a day or two.

Impressions from stereotype plates have been taken of which the greater part is legible: the distinctness usually improves after a few days. In default of a second plate, a piece of tin-foil about the same size should be put on the opposite side of the glass.

Sheet and plate glass of various thicknesses have been used without any noticeable change either in the treatment or the results.

I have put an impressed glass on a photographic plate in the dark, but did not get any result on developing: my imperfect skill in photographic matters leaves this experiment inconclusive.

Probably all polished surfaces may be similarly affected: a plate of quartz gives the most perfect images, which retain their freshness longer than those on glass.

Mica and gelatine give poorer results: it is not possible to polish the surface to the necessary point without scratching it.

On metal surfaces fairly good impressions can be produced if, as Karsten advises, oiled paper is put between the coin and the surface.

In the order of original discovery the figures noticed by Peter Riess should come first. He discusses a breath-track made on glass by a feeble electrical discharge; as well as two permanent marks, noticed by Ettrick, which betray a disintegration of the surface.

I have found that when a stronger discharge is employed more complex phenomena of a similar kind are produced. A six-inch Wimshurst machine is arranged with extra condensers, as if to pierce a piece of glass. If this is about four inches square the spark will generally go round it. For a day, more or less, there is only a bleared watery track, $\frac{3}{16}$ inch wide, when the glass is breathed upon; but after this time others develop themselves within the first, a fine central black line with two white and two black on either side, the total breadth being the original $\frac{3}{16}$ inch. These breath-lines do not precisely coincide in position with the permanent scars, but the central one is almost the same as a permanent mark, which the microscope shows to be the surface of glass fractured into small squares of considerable regularity: on either side is a grey-blue line always visible, which Riess ascribes to the separation of the potash. After several months I found two blue lines on either side, which I believe were not visible at first. Of course these blue lines may be seen on most Leyden jars, where they have discharged themselves across the glass.

In 1842 Möser, of Königsberg, produced figures on polished surfaces by placing bodies with unequal surfaces near to them; the action was ascribed to the power of light, and his results were compared with those of Daguerre. Möser says, "We cannot therefore doubt that light acts uniformly on all bodies, and that, moreover, all bodies will depict themselves on others, and it only depends on extraneous circumstances whether or not the images become visible." In general, the multitude of images would make confusion; it can only be freshly polished surfaces that are free to reveal single definite impressions. However great Möser's assumption may be, there are many achievements of modern photography that would be as surprising if they were not so familiar. I have not the means of knowing the precise form of Möser's methods: in the experiments which follow there is usually contact and light pressure, and if they are not wholly analogous, they may for that cause help to generalize the idea: in none of these is electricity applied.

A piece of mica is freshly split, and a coin lightly pressed for thirty seconds on the new surface: a breath-image of the coin is left behind. At the same time it may be noticed that the breath causes abundant iridescence over the surface, whilst it is in a fresh state. It is not clear how the electricity of cleavage can have an active agency in the result.

It is familiar to most people that a coin resting for a while on glass will give an outline of the disk, and sometimes faint traces of the inner detail when breathed upon.

An examination-paper, printed on one side, is put between two plates of glass and left for ten hours, either in the dark or the daylight: a small weight will keep the paper in continuous contact, but this is not necessary if thick glass is used. A perfect breath-impression of the print is made, not only on the glass which lay against the print, but also on that which faced the blank side of the paper. Of course the latter reads directly, and the former inversely; the print was about one year old, and presumably dry.

More often both impressions are white, sometimes one or other or both are black. At other times the same one may be part white and part black, and they even change while being examined.

During a sharp frost with east winds early in March, 1890, these impressions of all kinds were easy to produce, so as to be quite perfect to the last comma; but in general they are difficult, more especially those from the blank side.

At the best period those from the blank side of the paper were white and very strong; also there were white spots and blotches revealed by the breath. They seemed to correspond with slight variations in the structure of the paper, and suggest an idea that the thickness of the ink or paper makes a minute mechanical indentation on the molecules: the state of these is probably tender and sensitive under certain atmospheric conditions, as happens with steel in times of frost.

The following experiments easily succeed at any time:— Stars and crosses of paper are placed for a few hours beneath a plate of glass: clear white breath-figures of the device will appear. A piece of paper is folded several times each way to form small squares, then spread out and placed under glass: the raised lines of the folds produce white breath-traces, and a letter weight that was above leaves a latent mark of its circular rim.

Some writing is made on paper with ordinary ink and well dried: it will leave a very lasting white breath-image after a few hours' contact. If, with an ivory point, the writing is traced with slight pressure on glass, a black breath-image is made at once. Of course this reads directly, and the white one inversely. It is convenient to look through the glass from the other side for inverse impressions, so as to make them read direct.

Plates of glass lie for a few hours on a table-cover worked with sunflowers in silk: they acquire strong white figures from the silk.

In most cases I have warmed the glass, primarily for the sake of cleansing it from moisture; but I have often gone to a heat beyond what this needs, and think that the sensitiveness has been increased thereby.

It is not easy to imagine what leads to the distinction between black and white, different substances act variously in this respect. I have placed various threads for a few hours under a piece of glass, which lay on them with light pressure: wool gives black, silk white, cotton black, copper white. A twist of tinsel and wool gives a line dotted white and black; after a time these traces show signs of developing into multiple lines as in the spark figures.

Two cases have been reported to me where blinds with embossed letters have left a latent image on the window near which they lay; it was revealed in misty weather, and had not been removed by washing. I have not had a chance to see these for myself, but both my informants were accustomed to scientific observation.

A glass which has lain above a picture for some years, but is kept from contact by the mount, will often show on its inner side an outline of the picture, always visible without breath. It seems to be a dust figure easily removed: possibly heat and light have loosened fine paint particles, and these have been drawn up to the glass by the electricity made in rubbing the outer side to clean it. The picture must have been well framed and sealed from external influences; most commonly dust and damp get in and obscure such a delicate effect.

I am not able to suggest simple causes for these varied effects. I am not inclined to think, except in the case of water-colours, which is hardly part of the enquiry, that there is a definite material deposit or chemical change; one cannot suppose that imperceptible traces of grease, ineradicable as they may be, would produce complete and delicate outlines. The cleaning off of impressions may at first seem to indicate a deposit; but this renewal of the surface might rather be like smoothing out an indented tin-foil surface: such a view might explain the case where a blank over-electrified disk is developed into fine detail. The electrified figures seem to point to a bombardment, which produces a molecular change, the intensity of electricity bringing about quickly what may also be done by slow persistent action of mechanical pressure. At present it seems as if most of the phenomena cannot be drawn out from the unknown region of molecular agency.

While experimenting I was not within reach of references to former researches, but I have since done my best to find them out, and to indicate all I have learnt in the body of my paper.

Poggendorff, vol. lvii. p. 492; translated in *Archives de l'Electricité*, 1842, p. 647.
Riess' "Electrische Hauchfiguren" in "Repertorium der Physik"; translated in *Archives de l'Electricité*, 1842, p. 591.
Reiss' "Die Lehre von der Reibungs Electricität," vol. ii. pp. 221–224.
Mascart, "*Electricité Statique*," vol. ii. p. 177.
Taylor's "Scientific Memoirs," vol. iii.

SCIENTIFIC SERIALS.

American Journal of Science, December.—An experimental comparison of formulæ for total radiation between 15° C. and 110° C., by W. de Conte Stevens. The formulæ given by Dulong and Petit, by Rosetti, Stefan, and Weber, were tested for a comparatively small range of differences by a determination of the heat radiated from an iron disc at a distance of about 30 cm. from a thermopile. The results tended to show that H. F. Weber's formula (*Sitzungsber.*, Berlin, 1888) agrees most closely with experiment. Stefan's formula, according to which the heat emitted in unit of time is proportional to the fourth power of the absolute temperature, is also fairly accurate, but Dulong and Petit's values are too high, and Rosetti's too low.—Notes on silver, by M. Carey Lea.—Notes on silver chlorides, by the same. Fused silver chloride poured into petroleum and placed in the sunlight without removing it from the liquid, is instantly darkened. From this it appears that the presence of oxygen or moisture is not essential to the darkening of silver chloride in light. The chlorine may be taken up by some other substance.—A remarkable fauna at the base of the Burlington Limestone in north-eastern Missouri, by Charles Rollin Keyes.—Glacial pot-holes in California, by H. W. Turner.—The lavas of Mount Ingalls, California, by H. W. Turner.—A method for the quantitative separation of barium from strontium by the action of amyl alcohol on the bromides, by Philip E. Browning. The solubility of barium bromide is about 0·0013 grm. on the oxide in 10 cc. of amyl alcohol, while that of strontium bromide is 0·2 grm. To obtain the bromides, the precipitated and thoroughly washed carbonates of Ba and Sr are treated with hydrobromic acid obtained by the action of dilute sulphuric acid on potassium bromide.—Note on the method for the quantitative separation of strontium from calcium by the action of amyl alcohol on the nitrates, by P. E. Browning. Recent work on this method has shown that the total correction amounts to 0·0006 grm. on the strontium oxide, and 0·0010 on the calcium as sulphate.—Study of the formation of the alloys of tin and iron, with descriptions of some new alloys, by W. P. Headden.—Notes on the Cambrian rocks of Pennsylvania and Maryland from the Susquehanna to the Potomac, by C. D. Walcott.—Volcanic rocks of South Mountain in Pennsylvania and Maryland, by G. H. Williams.

Wiedemann's Annalen der Physik und Chemie, No. 11.—On the behaviour of allotropic silver towards the electric current, by A. Oberbeck.—On the indices of refraction of dilute solutions, by W. Hallwachs.—On capillary constants, by M. Cantor.—On the chemistry of the accumulator, by M. Cantor.—On the fall of potential during discharges, by O. Lehmann. A series of important investigations on discharges between electrodes and in tubes without electrodes.—Expansion of water with the temperature, by K. Scheel.—A method for determining the density of saturated vapours and the expansion of liquids at higher temperatures, by B. Galitzine. This method has the advantage of extreme simplicity combined with accuracy. A small glass tube, about 5 cm. long and a few mm. thick, is closed at one end and drawn out into a capillary at the other. After determining the weight and internal volume of the tube, a small quantity of the substance to be investigated is introduced into it in the liquid state. This is made to boil, and then the tube is sealed by fusing. On raising the temperature, the surface of separation between the liquid and its vapour is displaced, until at a certain temperature all the liquid is converted into saturated vapour. The tube is then cooled until the vapour reappears, when the temperature is again taken. This can be repeated several times, thus giving an accurate value for the density of saturated vapour at a certain temperature. The same process can be used to determine the expansion of the liquid. As the temperature rises, the volume of the liquid will in general increase up to a certain point, when the vaporization becomes more pronounced. This maximum, which can be observed more accurately by drawing out the tube near that point, gives a value for the expansion. For the density at that point is a function of the density at 0° C. and the temperature, and the pressure is that of the saturated vapour at the same temperature. Thus it is only necessary to find the volumes of the liquid and the vapour, and the density of the latter from the previous experiment.—On radiant energy, by B. Galitzine.—Note on the electricity of waterfalls, by J. Elster and H. Geitel.—Apparatus for demonstrating the Wheatstone bridge arrangement, by A. Oberbeck.—Determination of the coefficient of self-induction by means of the electro-dynamometer, by O. Troje.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 15.—On some new reptiles from the Elgin Sandstone, by E. T. Newton, communicated by Sir Archibald Geikie, F.R.S.

During the last few years a number of reptilian remains have been obtained from the Elgin Sandstone at Cuttie's Hillock, near Elgin, which are now in the possession of the Elgin Museum and of the Geological Survey. These specimens represent at least eight distinct skeletons, seven of which undoubtedly belong to the Dicynodontia, and one is a singular horned reptile, new to science. All the remains yet found in this quarry are in the condition of hollow moulds, the bones themselves having entirely disappeared. In order, therefore, to render the specimens available for study, it was necessary, in the first place, so to display and preserve these cavities that casts might be taken which would reproduce the form of the original bones. Gutta-percha was found to be the most suitable material for taking these impressions; and in some instances, especially in the case of the skulls, the casts had to be made in several parts and afterwards joined together.

The first specimen described is named *Gordonia Traquairi*; it is the one noticed by Dr. Traquair in 1885, and referred to the Dicynodontia; besides the skull, it includes fragmentary portions of other parts of the skeleton, and is contained in a block of sandstone which has been split open so as to divide the skull almost vertically and longitudinally. The two halves have been so developed that casts made from them exhibit the left side and upper surface, as well as the main parts of the palate and lower jaw. In general appearance this skull resembles those of *Dicynodon* and *Oudenodon*. The nasal openings are double and directed laterally; the orbits are large and look somewhat forwards and upwards. The supra-temporal fossa is large, and bounded above by the prominent parieto-squamosal crest, and below by the wide supra-temporal bar, which extends downwards posteriorly to form the long pedicle for the articulation of the lower jaw. There is no lower temporal bar. The maxilla is directed downwards and forwards to end in a small tusk. Seen from above, the skull is narrow in the inter-orbital and nasal regions, but wide posteriorly across the temporal bars, although the brain-case itself is very narrow. There is a large pineal fossa in the middle of a spindle-shaped area, which area is formed by a pair of parietals posteriorly and a single intercalary bone anteriorly.

The palate is continuous with the base of the skull; the pterygoids on each side send off a distinct process to the quadrate region. Towards the front the mediam part of the united pterygoids arches upwards, and the outer sides descend, forming a deep groove; from the evidence of other specimens it is clear that the palatines, extending inwards, converted this groove into a tube, and thus formed the posterior nares. The ramus of the lower jar is deep, with a large lateral vacuity, and the two rami are completely united at the symphysis. The back of this skull is not seen, but two other specimens, referable to this same genus, show that the occiput had two post-temporal fossæ on each side.

This specimen is distinguished from *Dicynodon* by the presence of two post-temporal fossæ on each side of the occiput, by the small size of the maxillary tusk; and probably by the elongated spindle-shaped area enclosing the pineal fossa, and also by the slight ossification of the vertebral centra.

A second and much smaller specimen, provisionally referred to *G. Traquairi*, has, besides the skull, a fore-limb well preserved. The humerus of this shows the usual Anomodont expansion of its extremities; its large deltoid crest is angular, and set obliquely to the distal end.

Three other species are referred to the same genus, namely:—

Gordonia Huxleyana, which is distinguished from *G. Traquairi* by its proportionately wider and more depressed skull, and by the absence of the concavity between the orbits which is present in the latter species. The humerus has the distal extremity oblique to the deltoid crest, which was probably rounded and not angular.

G. Duffiana has the skull even wider than in *G. Huxleyana*, and the portion of a humerus found with this skeleton has the two extremities set nearly at right angles to each other.

G. Juddiana has an elongated skull resembling that of *G. Traquairi*, but the parietal crests are less developed, the bones of the nasal region are much thickened and overlap the nasal apertures, the small tusk is placed a little further back and points more directly downwards, and the pineal fossa is smaller than in either of the other species.

A second generic form is named *Geikia Elginesis*. This is a skull nearly allied to *Ptychognathus*, Owen, but is distinguished by its shorter muzzle and the entire absence of teeth; the upper part of the skull, between the orbits, is also peculiar, forming a deep valley open anteriorly, with a ridge on each side, the anterio end of which forms a large prominence above and in front of the orbit. The occiput has only one (the lower) post-temporal fossa open on each side. The maxilla is produced into a tooth-like prominence, which occupies a similar position to the tusks of *Gordonia;* but the bone is too thin to have supported a tooth, and in all probability it was covered by a horny beak. The lower jaw has a strong symphysis, a distinct lateral vacuity, and the oral margin, at the foot of each ramus, bears a rugose prominence.

Elginia mirabilis is the name proposed for the skull of a reptile, which, on account of the extreme development of horns and spines, reminds one of the living lizards *Moloch* and *Phrynosoma.* The exterior of this skull is covered in by bony plates, the only apertures being the pair of nostrils, the orbits, and the pineal fossa. The surfaces of the bones are deeply pitted, as in crocodiles and labyrinthodonts. The horns and spines, which vary from ½ in. to nearly 3 in. in length, are found upon nearly every bone of the exterior. The development of the epiotics, and the arrangement of the external bones, resemble more the Labyrinthodont than the reptilian type of structure; while the palate, on the other hand, conforms more nearly to the Lacertilian type, and, with the exception that the pterygoids are united in front of the pterygoid vacuity, agrees with the palate of *Iguan* and *Sphenodon.* There are four longitudinal ridges along the palate, some of which seem to have carried teeth. The oral margin was armed with a pleurodont dentition, there being on each side about twelve teeth with spatulate crowns, laterally compressed and serrated. With the exception of the smaller number of the teeth, we have here, on a large scale, a repetition of the dentition of *Iguana.* This peculiar skull seems to show affinities with both Labyrinthodonts and Lacertilians, and is unlike any living or fossil form; its nearest, though distant, ally apparently being the *Pareiasaurus* from the Karoo beds of South Africa.

Linnean Society, December 1.—Prof. Stewart, President, in the chair.—A letter was read from the Rev. Leonard Blomefield, expressing his high appreciation of the compliment paid him by the presentation of the illuminated address which had been signed by the Fellows present at the last meeting of the Society and forwarded to him.—Messrs. H. and J. Groves exhibited specimens of several Irish *Characeæ* collected during the past summer. *Nitella tenussima* from Westmeath and Galway had not been previously recorded from Ireland, and a large form of *N. gracilis* from two lakes in Wicklow had been only once previously met with. Referring to the former, Mr. H. Groves remarked that although it might be expected to occur in all the peat districts it had only been found in two widely separated localities in England, namely, in the Cambridgeshire Fens and in Anglesea.—Mr. A. Lister made some remarks on the nuclei of Mycetozoa, exhibiting some preparations under the microscope.—Mr. E. Cambridge Phillips forwarded for exhibition a hybrid between red and black grouse, which had been shot in August near Brecon.—Mr. J. E. Harting exhibited and made remarks on some coleopterous larvæ which had been vomited by a child at Tintern, and had been forwarded by the medical attendant, Dr. J. Taylor Brown, for identification. The precise species had not been determined, but was considered to be allied to *Blaps mortisaga.* Mr. Harting drew attention to the fact that cases of voiding coleopterous larvæ were mentioned by Kirby and Spence (7th ed. p. 71), and by the late Dr. Spencer Cobbold in his work on parasites (1879, p. 269).—Mr. D. Morris exhibited some tubers of *Calathia allonia,* eaten as potatoes in Trinidad, where it is known as Tapee Nambour. Apparently a corruption from the French *topinambour* (artichoke).—A communication was read from Mr. J. H. Hart, of the Botanic Gardens, Trinidad, on *Œcodoma cephalotes* and the fungi it cultivates.—Prof. F. Jeffry Bell contributed a short paper on a small collection of Crinoids from the Sahul Bank, North Australia, some of which were new, and Mr. Edgar Smith communicated descriptions of some new land shells from Borneo.—The meeting adjourned to December 15.

Physical Society, December 9.—Mr. Walter Baily, Vice-President, in the chair.—The Chairman announced that an extra meeting would be held on January 13, 1893.—Prof. S. P. Thompson's communication on Japanese magic mirrors was postponed.—Mr. W. B. Croft read a paper on the spectra of various orders of colours in Newton's scale. After referring to the definition of the *order* of colours by reference to the retardation in wave-lengths, produced by different thicknesses of selenite between crossed polarizer and analyzer, the author went on to say that several books on optics implied that the number of bands in the spectra of these colours was the same as the order of the colour. On obtaining selenites of the first four orders of red from Messrs. Steeg and Reuter, he found that the first three orders gave one dark band each, and that of the fourth order three dark bands. Further experiments showed that the thicknesses of the selenites were in the proper proportions required to give the first four orders of red. The numbers of bands, the author explained, depended on the numerical possibilities of wave-length within the visible spectrum—that is, whether a multiple of the wave-length of one visible wave can be another multiple of a different wave. For example, taking the visible spectrum as extending from A (0·000760) to H (0·000394) and the wave-length of the line E in the green as 0·000527, it was shown that the first order of red was due to extinction of green by a thickness of crystal proportional to 1 × 0·000527, and would give one band in the green. For the second order, the thickness of crystal was proportional to 2 × 0·000527, viz. 0·001054, and this number was no other integral multiple of any other wave-length between A and H; consequently there could only be one band. Similarly it was shown that the third order of red could only have one band or possibly produce a shortening of the spectrum. With the fourth order of red three bands were obtainable, for 4 × 0·000527 = 3 × 0·000703 and = 5 × 0·000422. Three bands were therefore possible near E, A, G, respectively. At the conclusion of his paper, Mr. Croft directed attention to a very simple form of diffraction apparatus, by which most of the ordinary diffraction phenomena could be well seen, and which also served for spectrum observations. Mr. H. Miers pointed out that in Lewis Wright's "Practical Optics" a chart showing the bands corresponding to the first four orders of red was given. So far as he was aware, the subject was not fully discussed in the book. In reply, Mr. Croft said he had noticed Mr. Wright's chart, but believed the text implied that the number of bands should be the same as the order of the colour. Tyndall made definite statements to that effect.—Dr. W. E. Sumpner read a paper on the diffusion of light. The influence of diffusion in increasing the illumination of rooms and open spaces, had not, in the author's opinion, been sufficiently appreciated. Being impressed with the great importance of the subject, he was led to make determinations of the co-efficients of reflection, absorption, and transmission of diffusing surfaces. To give precision to terms sometimes vaguely used, several definitions were proposed. *Reflecting power* was defined as the ratio of the amount of light reflected from a surface to the total amount of light incident upon it; *illumination* of a surface, as the amount of incident light per unit of surface; *unit quantity* of light as the flux of radiation across unit area of a sphere of unit radius at whose centre a unit light is placed; and *brightness* as the candle-power per unit area in the direction normal to the surface. Denoting these quantities by η, I, Q and B respectively, and assuming the cosine law of diffusion (*i.e.* the candle-power in any direction is proportional to the cosine of the angle between the direction and the normal to the surface) it was shown that $\pi B = \eta I$, and that the average illumination (I′) of the walls of a room is related to the illumination (I) due to the direct action of the lights as expressed by the formula $I' = \frac{I}{1 - \eta}$. If the reflecting power of the walls, &c., be 50 per cent., $\eta = \frac{1}{2}$, and $I' = 2I$, whilst if $\eta = 0·8$, a number approximately true for white surfaces, then $I' = 5I$. The illumination due to the walls may, therefore, be far more important than that due to the direct rays from the lights. When the surfaces consist of portions of different reflecting power, the average reflecting power may be found from the equation $\eta = \frac{\eta_1 A_1 + \eta_2 A_2 + \&c.}{A}$, A being the total surface, and A_1, A_2, &c., the areas of surfaces whose reflecting powers are η_1, η_2, &c., respectively. This law is shown to be quite accurate for spherical enclosures. In measuring reflecting power, the surface was attached to a large screen of black velvet placed perpendicular to a 3-metre photometer bench. Two lights were used, one a Methven 2-candle standard placed at the end of the bench remote from the reflecting surface, and the other, a glow lamp of about 20-candle power, was attached to a slider which also carried a Lummer-Brodhun photometer. The glow-lamp served to illuminate the reflecting surface, but the photometer was screened from its direct rays. The formulæ used in reducing the observations are worked out in the paper and tables of results given. Absorbing power was determined by measuring the candle-power of a glow-lamp, first when uncovered, and then when surrounded by a cylinder of the substance under test. It was found to be of great importance to distinguish between apparent and real absorption, for reflection from the surfaces of the cylinders increases the internal illumination. The true absorption coefficient (α) is given by $\alpha = (1 - \eta)\frac{k_0 - k_1}{k_0}$, where η is the reflecting power and k_1 and k_0 the candle-powers with and without the envelope of material under test. In determining transmitting power, the Methven standard and photometer were placed on one side of the surface and the glow-lamp on the other. Difficulties were experienced from the fact that some materials such as tracing paper, transmit part of the light directly (like transparent substances), and another part by diffusions according to the cosine law. Methods for discriminating between the different parts were therefore devised both in the reflection and transmission experiments, and consistent results subsequently obtained. Tables and curves showing the close agreement of calculated and observed values, are included in the paper. An abstract of some of the tables of numbers is given below:—

Material.	Percentage reflecting power η.	Percentage absorption α.	Percentage Transmission τ.	$\eta + \alpha + \tau$
Blotting paper	82	13·8	9·2	105·0
Cartridge paper ...	80	12·2	11·2	103·4
Tracing cloth	35	15·0	54·4	104·4
Tracing paper	22	7·0	76·0	105·0
Ordi ary mirror... ...	82			
Ordinary foolscap ...	50 to 70			
Tissue paper (one thickness	40			
Tissue paper (two thicknesses	55			
Yellow wall-paper ...	40			
Blue paper	25			
Dark brown paper ...	13			
Yellow painted wall ...	20			
Black cloth	12			
Black velvet	0·4	(apparent)		
Arc lamp globes—				
Light opal	—	15		
Dense opal	—	39		
Ground glass ...	—	42		

Theoretically the sum of the reflecting, absorbing and transmitting powers should be unity, but from the above table it will be noticed that they exceed 100 per cent., by amounts greater than can be accounted for by experimental error. This discrepancy, the author thought, might be attributed to the law of cosines not being exactly fulfilled. Mr. A. P. Trotter said he had been interested in the subject of diffusion for many years with a view to obviating the glare of arc lamps. Some experiments he made on reflecting power gave unsatisfactory results, owing, as he now saw, to his not taking the solid angles subtended by the reflecting surfaces into account. The reflecting power of substances was of great importance in the illumination of rooms; in one case measured by Dr. Sumpner and himself, two-thirds of the total illumination was due to the walls. It would greatly simplify measurement of reflecting power if some substance could be adopted as a standard. Referring to the cosine law, he said he had found it true, except when the angles of incidence approached 90°. In cases where considerable total reflection took place the apparent brightness near the normal direction was greatly in excess of that in other directions. These points he illustrated by polar curves. He had also considered what should be the nature of a roughened or grooved surface to give the cosine law of diffusion. No simple geometrical form of corrugations, &c., seemed to fulfil the required conditions. Dr. Hoffert said the high numbers given for the reflecting powers of substances were very interesting. Most people had noticed the effect of laying a white table cloth in an ordinary room. He had also observed that wall papers of the same pattern, but slightly different in colour, had very different effects in producing increased illumination, and wished to know if the influence of small differences in colour and texture on diffusing power, had been investigated. Mr. Blakesley defended the cosine law, and suggested that the summation of the powers exceeding unity might be due to the fact that the enclosure reflected heat as well as light, thus raising the temperature and increasing the efficiency of the radiant. Mr. Addenbrooke said the importance of the subject was impressed on him when he passed through America three years ago and noticed the crude manner in which electric lighting was there carried out. If using good reflecting surfaces increased the illumination of a room 50 per cent., it was like reducing the cost of electricity from 8*d*. to 4*d*. per unit. He could hardly conceive any subject of more practical importance than the one before the meeting. Dr. C. V. Burton did not understand why the cosine law should be objected to, for it was possible that no surface was perfectly diffusive. The effect of reflection from walls, &c., say in illuminating a book would not, he thought, be so great as would appear from the numbers given, for one usually read near a light, and the reflected light falling on the book was only a small part of the whole, on account of the greater distances of the walls. Another member pointed out that in experiments such as those described, it was very important to screen the photometer and surfaces from all radiation other than that under test. He rather doubted whether any surface reflected as well as mirrors. White surfaces might appear to do so, but this was probably because the eye would overestimate it, owing to the superiority of white in aiding distinct vision. Dr. Sumpner in reply said he had, as stated in the paper, used white blotting paper as a standard of reflecting power and found it very convenient. His most careful measurements had been made on whitish surfaces and not on coloured ones. Where one colour, say red, preponderates in a room, the average light would be much redder than that emitted by the source owing to the other colours being absorbed. In considering illumination as related to distinct vision, it was necessary to take account of the eye itself, for the pupil contracted in strong lights and opened in feeble ones. This subject he hoped to treat fully in a subsequent paper.

Entomological Society, December 7.—Frederick DuCane Godman, F.R.S., President, in the chair.—The President announced the death, on December 2, of Mr. Henry T. Stainton, F.R.S., an ex-President and ex-Secretary of the Society.—Mr. Jenner Weir exhibited a species of Acræa from Sierra Leone, which Mr. Roland Trimen, F.R.S., who had examined the specimen, considered to be a remarkable variety of *Telchinia encedon*, Linn. It was a very close mimic of *Limnas alcippus*, the usual West African form of *Limnas chrysippus*. The upper wings of the specimen were rufous and the lower white, as in the model, and the resemblance in other respects was heightened by the almost total suppression of the black spots in the disc of the upper wings, characteristic of the usual markings of *T. encedon*.—Mr. F. J. Hanbury exhibited a remarkable variety of *Lycæna adonis*, caught in Kent this year, with only one large spot on the under side of each upper wing, and the spots on the lower wings entirely replaced by suffused white patches. He also exhibited two specimens of *Noctua xanthographa* of a remarkably pale brownish grey colour, approaching a dirty white, obtained in Essex, in 1891; and a variety of *Acronycta rumicis*, also taken in Essex, with a dark hind margin to the fore wings.—Mr. H. J. Elwes exhibited a living specimen of a species of *Conocephalus*, a genus of *Locustidæ*, several species of which, Mr. McLachlan stated, had been found alive in hothouses in this country.—Dr. T. A. Chapman exhibited immature specimens of *Tæniocampa gracilis*, *T. gothica*, *T. populeti*, *T. munda*, *T. instabilis* and *T. leucographa*, which had been taken out of their cocoons in the autumn, with the object of showing the then state of development of the imagos.—Mr. F. W. Frohawk exhibited a living specimen of the larva of *Carterocephalus palæmon* (*Hesperia paniscus*) hybernating on a species of grass which he believed to be *Bromus asper*. The Rev. Canon Fowler and Mr. H. Goss expressed their interest at seeing the larva of this local species, the imagos of which they had respectively collected in certain woods in Lincolnshire and Northamptonshire. Mr. Goss stated that the food-plants of the species were supposed to be *Plantago major* and *Cynosurus cristatus*, but that the larva might possibly feed on *Bromus asper*.—Mr. C. G. Barrett exhibited a long series of remarkable melanic varieties of *Boarmia repandata*, bred by Mr. A. E. Hall from larvæ collected near Sheffield.—Mr. W. Farren exhibited four varieties of *Papilio machaon* from Wicken Fen; also a series of two or three species of *Nepticulæ* pinned on pith with the "minutien Nadeln," for the purpose of showing these pins.—Canon Fowler exhibited specimens of *Xyleborus perforans*, Woll., which had been devastating the sugar-canes in the West Indies.—Mr. E. B. Poulton, F.R.S., showed, by means of the oxy-hydrogen lantern, slides of various larvæ and pupæ, in illustration of his paper, read at the October meeting, entitled, "Further experiments upon the colour-relation between certain lepidopterous larvæ and their surroundings." He stated that he believed that nineteen out of twenty larvæ of *Geometridæ* possessed the power of colour adjustment. Mr. F. Merrifield, the Rev. J. Seymour St. John, and Mr. Jacoby took part in the discussion which ensued.—Mr. F. Merrifield read a paper entitled, "The effects of temperature on the colouring of *Pieris napi*, *Vanessa atalanta*, *Chrysophanus phlæas* and *Ephyra punctata*," and exhibited many specimens thus affected. Mr. Poulton, Dr. F. A. Dixey, Mr. Elwes, and Mr. Jenner-Weir took part in the discussion which ensued.—Mr. Kenneth J. Morton communicated a paper entitled, "Notes on *Hydroptilidæ* belonging to the European Fauna, with descriptions of new species."—Dr. T. A. Chapman read a paper entitled, "On some neglected points in the structure of the pupa of Heterocerous Lepidoptera, and their probable value in classification; with some associated observations on larval prolegs." Mr. Poulton, Mr. Tutt, Mr. Hampson, and Mr. Gahan took part in the discussion which ensued. —Mr. J. Cosmo-Melvill communicated a paper entitled, "Description of a new species of butterfly of the genus *Calinaga*, from Siam."—Mr. W. L. Distant communicated a paper entitled, "Descriptions of new genera and species of Neotropical *Rhynchota*."

PARIS.

Academy of Sciences, December 12.—M. d'Abbadie in the chair.—On certain asymptotic solutions of differential equations, by M. Emile Picard.—Description of a new electric furnace, by M. Henri Moissan. The furnace consists of two bricks of quicklime one upon the other, the lower one of which is provided with a longitudinal groove which carries the two electrodes, and between them is a small cavity serving as crucible, which contains a layer of several centimetres of the substance to be experimented upon. The latter may also be contained in a small carbon crucible. The highest temperature worked with was 3000° C., produced by a current of 450 amperes and 70 volts consuming 50 horse-power. In the neighbourhood of 2500°, lime, strontia and magnesia crystallized in a few minutes. At 3000° the quicklime composing the furnace began to run like water. At the same temperature the carbon rapidly reduced the oxide of calcium to the metallic state. The oxides of nickel, cobalt, manganese, and chromium were reduced in a few seconds at 2500°, and a button of uranium weighing 120 gr. was obtained from the oxide in ten minutes at 3000°.—Action of a high temperature on metallic oxides, by M. Henri Moissan. In all the experiments, the simple elevation of temperature produced the crystallization of all the metallic oxides experimented upon.—On the existence of the diamond in meteoric iron of the Cañon Diablo, by M. C. Friedel. A careful analysis has placed beyond doubt the existence of diamond in a portion of the Arizona meteorite presented to the Ecole des Mines. It occurs in small grains or a fine powder disseminated through the iron.—On the laws of expansion of fluids at constant volume; coefficients of pressure, by E. H. Amagat.—On the means of diminishing the pathogenic power of fermented beet-root pulp, by M. Arloing. —On the employment of free balloons for meteorological observations at very great heights, by M. Ch. Renard.—Photographic observations of Holmes's comet, by M. H. Deslandres.—On the locus of the mean distances of a point of an ordinary epicycloid, and of the successive centres of curvature which correspond to it, by M. G. Fouret.—On ordinary linear differential equations, by M. Jules Cels.—On the common cause of the evaporation and surface tension of liquids, by M. G. van der Mensbrugghe. —On the relation between the velocity of light and the size of the molecules of refracting liquids, by M. P. Joubin. From a comparison of a large number of substances the following law is deduced: The refraction is proportional to the square root of the quotient of the weight of the molecule by the number of constituent atoms (mean weight of the atom).—On the anomalous propagation of the light waves of Newton's rings, by M. Ch. Fabry.—On transparent diffusing globes, by M. Frédureau.—On a relation between molecular heat and the dielectric constant, by M. Runolfsson.—On the employment of guard-ring condensers and absolute electrometers, by M. P. Curie.—On the density of oxide of carbon and the atomic weight of carbon, by M. A. Leduc.—Critical reduction of Stas's fundamental determinations on potassium chlorate, by M. G. Hinrichs.—On a chloro-iodide of carbon, by M. A. Besson.—Action of anhydrous hydrofluoric acid on the alcohols, by M. Maurice Meslans. —Action of sulphuric acid on citrene, by MM. G. Bouchardat and J. Lafont.—Analysis of sulphate of quinine and quantitative determination of quinine in presence of the other cinchona alkaloids, by M. L. Barthe.—On the assimilation of the omasum to the abomasum of the Ruminants from the point of view of the formation of their mucous membrane, by M. J. A. Cordier. —On the differential osteological characters of rabbits and hares; comparison with leporides, by M. F. X. Lesbre.—Remarks on the preceding communication, by M. Milne-Edwards.—Myxosporidia of the bile-duct of fishes; new species, by M. P. Thélohan.—Method for ensuring the conservation of vitality in plants brought from distant tropical regions, by M. Maxime Cornu.—On the difference of transmissibility of pressures across ligneous, herbaceous, and succulent plants, by M. Gaston Bonnier.—On the structure of the *Gleicheniaceæ*, by M. Georges Poirault.—Salivary secretion and electric excitation, by M. N. Wedensky.—Action of the extract of cows' blood on animals affected with glanders, by M. A. Babes.—The blizzard of December 6 and 7, 1892, by M. Ch. V. Zenger.

BOOKS and SERIALS RECEIVED.

BOOKS.—The Elements of Graphic Statics: L. M. Hoskins (Macmillan). —Qualitative Analysis Tables and the Reactions of certain Organic Substances: Dr. E. A. Letts (Belfast, Mayne and Boyd).—Lord Rosse on the Gospel: Modernized by E. L. Garbett (W. Reeves).—An Atlas of Astronomy: Sir R. S. Ball (Philip).—Pioneers of Science: Prof. O. Lodge (Macmillan).—Collected Mathematical Papers of Prof. A. Cayley: Vol. V. (Camb. Univ. Press).—British Journal Photographic Almanac, 1893 (Greenwood).—A Manual of Bacteriology: Dr. G. M. Sternberg (New York, Wood).—La Terre Les Mers et Les Continents: F. Priem (Paris, J. B. Baillière).

SERIALS.—L'Anthropologie, Tom. 3, No. 5 (Paris, Masson).—Economic Journal, December (Macmillan and Co.).—Journal of the Chemical Society, December (Gurney and Jackson).

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