THURSDAY, JUNE 7, 1877

THE ANTIQUITY OF MAN

THE Conference on the Antiquity of Man lately held by the Anthropological Institute, and reported in these columns, has led to a result by no means unsatisfactory, when all the conditions of the problem are duly weighed. The result is merely negative, but in arriving at it several misconceptions and errors of fact seem to us to have been swept away. In its discussion there were two parties represented, one eager to show that the antiquity of man has been proved by modern discovery to be far older than the date which had been arrived at by the labours of Falconer, Lyell, Prestwich, Evans, Boyd Dawkins, and others, while the other contended that the subject had not advanced in the least degree during the last few years, and that the so-called discoveries were either errors of observation, or resulting from premises which were altogether unsatisfactory. The field of the discussion lay in matters geological rather than archæological, and the caution which the president urged upon the conference was certainly not urged in vain.

The chief interest of the debate turned upon the question as to whether there was any evidence in this country of man in the caves or river deposits older than post-glacial times. The readers of the works of Messrs. Croll and James Geikie will remember that they ascribe all the traces of palæolithic man in this country either to a pre- or inter-glacial age, basing their conclusions principally upon the fact that in the river deposits and caves some of the associated animals, such as the hyæna, lion, and hippopotamus are now only to be found in hot climates; and seeing that no traces of a warm climate are presented by any post-glacial deposit in Britain, they infer that those in question are of a much higher antiquity. They account for the association of southern and northern animals by the supposition that they occupied the country at different times, during glacial or interglacial æons of from five to twelve thousands years in length. To this it was objected that the intimate association of forms prove that both sets of animals inhabited the country at the same time, and were the result of the overlapping of different faunas during seasonal changes. The reindeer formed a large portion of *the prey* of the hyæna, and must therefore have been a contemporary. It was also pointed out by one of the speakers, that there is no evidence from the animals that there ever was anything like "the perpetual summer," advocated by Mr. Geikie at any time in the pleistocene age. The hippopotamus in Regent's Park takes his tub regularly in spite of the east winds so prevalent in the spring, which remind one of the glacial period; and the tiger crosses the frost-bound rivers of the Amoor to prey upon the reindeer. The lion, now found only in the south, lived in the days of Herodotus in the inclement mountains of Thrace. It seems, therefore, to us, that any argument based upon fossil animals as to a warm inter-glacial period, is worthless. And further, it is obviously unfair in treating of the fauna associated with man to adopt the forensic device of choosing some witnesses to the exclusion of others. It would be as easy to prove the climate in question to have been temperate from the associated remains of bison, stag, and horse, as it would be to prove it to have been arctic from the associated musk sheep, lemmings, and reindeer. It was probably a varying climate, with great extremes, similar to that in Central Siberia, in which the summer heat and winter cold are very severe.

The fossil mammalia of the pleiostocene tell us nothing as to the relation of man to the glacial period. The Arctic species invaded Europe probably from Asia, while the ice was finding its way southwards from the mountains of Scandinavia, and occupied the area north of the Alps and the Pyrenees, *while* the confluent glaciers covered the area north of the valley of the Thames. When the ice ultimately retreated they followed it, and thus were both pre- and post-glacial. Nor do the survivals from the pleiocene age tell us anything, such as the hippopotamus, the *Rhinoceros leptorhinus*, and the *Elephas antiquus*, since they belong both to the earlier and later pleistocene strata, and are also associated with remains of reindeer, and other northern species. The presence of the reindeer in all the palæolithic caverns stamps the age of man as late pleistocene, according to Prof. Boyd Dawkins, but it does not afford any clue as to his pre- or post-glacial age. The glacial period is not a hard and fast line dividing one fauna from another. One palæolithic cave, however, in this country, that of Pont Newydd, in the valley of the Elwy, near St. Asaph, is of well-ascertained post-glacial age.

The argument urged in favour of palæolithic man being pre- or inter-glacial, based upon the distribution of the mammalia in southern and eastern England, and in France, while they are conspicuous by their absence in the glaciated areas of Scotland, Cumberland, and Wales, was met by the view that the barren areas were covered with ice, *while* other districts further to the south were occupied by the animals. The hypothesis that the uplands of Wales and Northern Britain were ever stocked by the same animals as the fertile river-bottoms of the south, seems to us little less than absurd. Yet this is necessary for the view that their remains have been removed from the barren areas by the subsequent grinding of the ice-sheet.

In the course of the discussion the reputed cases of the occurrence of palæolithic remains in the deposits older than the post-glacial were minutely criticised. Prof. Busk stated that the fibula of the Victoria Cave, formerly supposed to be human, was altogether too insignificant a fragment to base any conclusion upon as to man's antiquity. Two small cut-bones, however, of goat were brought forward by Mr. Tiddeman in support of the pre- or inter-glacial age of man in the Victoria Cave. On the other hand, it was urged that these were derived from the superficial stratum containing Roman coins and pottery, &c., in which they were very abundant. From the nature of the cuts it seems to us that if it be established that they were discovered in the undisturbed stratum along with the hyænas, they would prove not only the presence of man, but of a user of a knife or chopper of bronze or iron. The absence of the goat, also (probably a domestic animal) from all undisturbed pleistocene deposits in this country, and in France, Belgium, and Germany, renders it very probable that the animal was introduced into those regions after the close of the pleistocene age. But even

supposing that these difficulties be got over, the age of the deposit in which these fragments are stated to have been found is a matter of dispute in which the authorities are about equally balanced on either side.

The asserted inter-glacial age of the river gravels containing palæolithic implements proved equally unsatisfactory. The cases supposed to be decisive of the question in the neighbourhood of Brandon and Thetford, were considered by Prof. Hughes to throw no light upon it, since the deposits above them, supposed to be boulder clay, are not boulder clay *in situ*. It was forcibly urged by several speakers, and especially by Prof. Prestwich, that the flint implement-bearing strata are proved by their position in the valleys to be later than the glaciation of the district, in every case where it has been glaciated, or in other words, that they are decidedly of post-glacial age.

The general question of the antiquity of man in Europe was not discussed, although we gathered that the evidence of the presence of man in the Italian pleiocenes was not considered satisfactory. The general impression left upon our minds is that in Britain there is no evidence of any palæolithic men, either in caves or the river-deposits of an age older than post-glacial, and that the discoveries of the last fourteen years have merely given us interesting details as to the palæolithic savage, without telling us anything of his relation to the glacial period.

THE VALUE OF NATURAL HISTORY MUSEUMS

WELL-arranged museums are valuable to the state in many ways. The technological department ought to show in what new directions capital may and may not be invested; the geological and mineralogical should point out in what kind of rock and in what parts of the earth's crust ores and minerals are to be sought, and should save the expenditure of money in useless trials. The museum of the Royal School of Mines in Jermyn Street performs these functions. But they are valuable in a still higher sense as encouraging a love of knowledge for its own sake apart from any selfish aims. The visitors to the British Museum, however frivolous they may be, leave it all the better for having been there. It is impossible that they should not carry away some sort of idea, which otherwise would not have occurred to them, even if it be merely the recognition that outside their daily lives there is a world of knowledge vast and indefinite, but real and tangible. In this respect museums are educators of the masses, offering them a means of culture which would otherwise be out of their reach. And lastly, as instruments of training in natural history they are, as I have already observed, as necessary to the student as collections of books to the student in arts.

Natural history pursuits are in themselves one of the forms of higher education, and one that is especially adapted for the culture of the lower, sometimes falsely termed the working classes—as if the higher classes worked neither with head nor hand. In proof of this I may quote the following example, which I am free to mention by the death of the man to whom it relates. Some years ago a mechanic, one of the evening class students at Owens College, took me to see a collection of fossils made by "a hand" in a cotton-mill at Oldham. To my astonishment I found that it consisted not merely of fossils *au naturel*, shells, and the like, but of those of coal plants, polished, and in many cases cut into slices so as to show their minute structure. This had been done by rubbing them down on the kitchen floor, cementing them to a piece of glass, and then grinding them until they became transparent. The care and labour implied in a process of this kind can only be estimated by those who have tried it. But it was necessary to have a microscope to see them, and I actually discovered that the instrument which was given me to use was made by the man himself, who could not afford to buy more than the lenses, which he mounted in tubes that were made to slide in each other after the manner of a telescope. He was also a good local botanist. His collection of fossils, along with another made by a friend of his under similar circumstances, furnished the materials on which Prof. W. C. Williamson has to a great extent founded his admirable memoirs on the coal-plants, now being published by the Royal Society. From time to time I saw a good deal of my friend, and a man more completely lifted out of the usual level of his class into what I may call the unselfish horizon I never met. This could be traced directly to the scientific pursuits to which he was led by seeing somebody one day pick up a piece of coal shale, and hearing him say that there was a fish scale in it. He disbelieved this, examined for himself, took to collecting, and ultimately became what he was, devoting his early mornings and his late evenings not merely to collecting but to knowing. His knowledge embraced other things than natural history. James Whittaker, of Oldham, may be taken as a type of the effect of natural history in elevating a man's character. He is the representative of a small, though very important, body in the Northern Counties, a body which would be largely increased by the foundation of museums of the right sort. From personal contact with men like him I have arrived at the conclusion that in this direction we have a means of spreading culture among the intelligent mechanics, artisans, and mill-hands, who go neither to church nor chapel, who do not read very much, and very often have no aims higher than those of the mere animal life. Had they access to museums on holidays and in the evenings, I am sure that the receipts of public houses would ultimately be lessened. At present they have few recreations and little chance of self-improvement; for the so-called mechanics' institutes, which were originally intended for them, have generally passed into the hands of the class immediately above them.

W. BOYD DAWKINS

PHYSIOLOGICAL ÆSTHETICS

Physiological Æsthetics. By Grant Allan, B.A. 8vo. (London: Henry S. King and Co., 1877.)

WE have here a little work of some 300 pages, which deals with the philosophy of æsthetics almost exclusively on its physiological side. Of course, in thus restricting his subject, the author neglects all the more subtle and intricate parts of that philosophy; but every competent reader will agree with him that it is desirable, for the purpose of analysis, to separate as distinctly as possible the physiological from the psychological elements

of æsthetics. For although the two classes are intimately blended in reality, this only makes it the more desirable to eliminate the one from the other in our analysis; so that we may perceive, as clearly as we can, how much of the total effect which our æsthetic consciousness supplies admits of being resolved into simple constituents, and how much remains over as complex constituents. Now in this respect Mr. Allan has profited well by the experience of previous writers; for while he treats his subject very thoroughly so far as it can be treated on the lower basis of physiology, he never permits himself to be tempted into the alluring superstructure of pure psychology. So rigidly, indeed, does he "stick to his text," that an uninformed reader might peruse the whole essay, and scarcely receive a hint that there is such a thing as "the association theory" in existence; while the names of Burke, Reynolds, Alison, Knight, Stewart, and Jeffrey are not even once mentioned.

The scope of the treatise being thus carefully confined to the more simple factors of our æsthetic emotions, space is afforded for a full exposition of numerous facts and theories relating to this important sub-division of psychological science. And, on the whole, the work has been well done. The arrangement is good, the style admirably lucid, and the spirit throughout scientific. True, there are no ideas of a strikingly original character; but a judicious compilation of facts already known, and a philosophical discussion of the more important theories which have been raised upon them, would be features in a work sufficient of themselves to make the latter a valuable addition to the literature of æsthetics. Mr. Allan, however, has done more than this. In his dedication he characterises his work as a "slight attempt to extend in a single direction the general principles which he (Mr. Herbert Spencer) has laid down;" and in this attempt we must allow that our author has been successful.

Setting out with the object of "exhibiting the purely physical origin of the sense of beauty, and its relativity to our nervous organisation," Mr. Allan begins by "investigating the nature of Pleasures and Pains generally." The most important part of this discussion is that in which he criticises the law thus enunciated by Prof. Bain: "States of pleasure are concomitant with an increase, and states of pain with an abatement, of some, or all, of the vital functions." On this law it is remarked, with justice, "in its endeavour to be antithetical, it misses the real relationship between the two states. If pleasures were the psychical concomitants of an *increase* of the vital functions, then our two greatest, if not our only pleasures ought to be digestion, and repose after exertion. . . . Mr. Bain has sighted this difficulty, but, not perceiving its full force, has endeavoured to avoid it by a supplementary theory of stimulation, which appears to me far more important than his main law. I believe the true principle of connection to be this: Pleasure is the concomitant of the healthy action of any or all of the organs or members supplied with afferent cerebro-spinal nerves, to an extent not exceeding the ordinary powers of reparation possessed by the system. And just as the two laws are not exactly antithetical, so too the feelings themselves are not directly and absolutely opposed to one another as will be seen in the sequel . . . In short, it will be seen that while Prof. Bain refers pleasure to an *increase* in the efficiency of the organism, it may better be regarded as the concomitant of a *normal amount of activity* in any portion or the whole of the organism." Thus "every activity when not excessive nor of a sort to prove destructive of the tissues, is doubtless in itself faintly pleasurable . . . but owing to the commonness and faintness of the feeling, we habitually disregard it." Nevertheless, when the whole organism is "under the influence of abundant food and good rest, the general stimulation of the nerves produces a consciousness of massive pleasure." Moreover, "the special stimulation of a single organ whose periods of activity are long intermittent, and which is at the culminating point of its nutrition, produces consciousness of acute pleasure." From considerations such as these, illustrated by a large number of subsequent examples, there is deduced the general formula, that "the amount of pleasure is probably in the direct ratio of the number of nerve fibres involved, and in the inverse ratio of the natural frequency of stimulation." Hence it is that the possible intensity of pleasures can never approach the possible intensity of pains; for while the organism, or parts of it; may be reduced or injured to a large extent before loss of sensibility supervenes, "efficient working cannot be raised very high above the average." Hence, too, "the æsthetically beautiful is that which affords the maximum of stimulation with the minimum of fatigue or waste, in processes not directly concerned with vital functions."[1]

Such may be said to be the foundation on which the present system of "Physiological Æsthetics" is raised. Thus, to select a few among the copious illustrations which are offered:—"The vulgar are pleased by great masses of colour, especially red, orange, and purple, which give their coarse nervous organisation the requisite stimulus; the refined, with nerves of less calibre but greater discriminativeness," require delicate combinations of *complementaries*. Similarly in music, the complex *harmony* of a Bach's fugue pleases the cultured ear, while a chorus of Offenbach, or the boisterous *melody* of a comic song, is more gratifying to the common people. Again, the æsthetic superiority of musical tones over mere noises is naturally explained by the fact, that "while the nervous apparatus for the perception of the latter receives frequent stimulation, each portion of the nervous apparatus for the perception of the former is comparatively seldom stimulated." Similarly, of course, simple tones are musically "poor," because they "can only arouse a sympathetic vibration in a single one of Cortis's organs;" while tones rich in harmonics are musically "full," because they stimulate a correspondingly greater number of Cortis's organs. *Beats*, again, are disagreeable, because "the ear is conscious of each separate interruption of the tone, and each subsequent reinforcement," thus receiving a *destructive* amount of intermittent stimulation. Similarly, though in a lesser degree, with *dissonance;* and similarly, too, with the optic nerve, when flashes of intermittent light follow one another too rapidly for the receptive material

[1] The latter qualification arises from a lengthy discussion in which Mr. Spencer's view as to the origin of the Play-instinct from a superfluity of nervous energy is explained. This is explained in order that the æsthetic feelings, which by the theory are supposed to have a similar origin, may be analytically differentiated from the playful feelings—the distinction between Art and Play being supposed to consist largely in the fact that while the latter has reference to the over-fed motor fibres, the former has reference to the over-fed sensory fibres, "the organs of sight, hearing, &c.," or "the passive side of our nature"

to undergo repair during the intervals of darkness. The æsthetic superiority of the analytic colours over black, white, and grey is explained by considerations analogous to those which have just been mentioned in the case of musical tones and noises; while harmony of colours is treated in the same way as harmony of sounds. A somewhat curious speculation is ventured to explain the apparent deficiency of the red-perceiving elements. "It is clearly desirable that the eyes of the frugivorous animals should be *pleasurably* stimulated by reds, oranges, and purples; and the simplest contrivance for effecting this end would be to give the greatest possible rest to such elements as answer to stimulations of these orders. Accordingly, they ought only to be excited by comparatively powerful stimulations of their proper kinds."

Adopting Mr. Spencer's view[1] as to the ideal being a faint central stimulation of such nerve-fibres as would receive strong peripheral stimulation by the reality, Mr. Allan carries his analysis to the limit where "Physiological Æsthetics" must end, and where Psychological Æsthetics ought only to begin. Space, however, will not allow us to follow him into this division of his subject. Enough has been said to show that his work deserves the attention of psychologists; and it may be added that as he throughout clearly explains both the physics and the physiology of his subject, his entertaining little treatise will prove instructive to any general readers who may be desirous of observing the intimacy of those relations between psychology and the lower sciences, which the magnificent generalisations of recent years are now every day bringing into clearer prominence.

GEORGE J. ROMANES

OUR BOOK SHELF

Select Plants readily Eligible for Industrial Culture or Naturalisation in Victoria, with Indications of their Native Countries and some of their Uses. By Baron F. von Mueller, C.M.G., F.R.S., &c. (Melbourne: McCarron, Bird, and Co.)

THIS is another form of Baron Mueller's numerous and widely-spread contributions to the Acclimatisation Society of Victoria—numerous we say, because the Baron's pen is always at work upon botanical matters, the consideration of useful plants being apparently one of his favourite themes, and widely spread, because these papers on "select plants" seem to have been freely distributed not only in Australia and in this country, but also in America, where indeed some portion, if not all, have been republished. The present issue, Baron Mueller tells us, is a rearranged and largely supplemented form, which has been taken up by the Government of Victoria, and published under their authority. The book, which numbers some 293 pages octavo, contains references to an immense number of plants, the information attached to each being brief but withal accurate. The generic and specific names are arranged alphabetically from beginning to end, and this arrangement is perhaps the best for general use. After the scientific name, the vernacular name is given, then the geographical distribution or habitat, followed by a note as to the nature of the plant, whether a tree, shrub, or what not, and finally a brief description of its properties and uses. As a proof that Baron Mueller has corrected this latest issue of his papers, down quite to the present time we may mention that under *Nicotiana tabacum*, Lattakia tobacco is included, and it is only within a comparatively recent date that Mr. Thiselton Dyer has shown this to be right, nearly all previous writers having attributed it to *N. rustica*. At the conclusion of the book a very good plan is adopted of classifying the plants mentioned under distinct heads referring to their uses; thus, under alimentary plants, the generic names of all such are placed; the same under dye plants, fibrous plants, and so on. A good index is given of vernacular names only, which is quite sufficient when it is remembered that the scientific names are arranged alphabetically throughout the book.

[1] Here, as indeed in most other places, Mr. Allan does not express his obligations. Doubtless, having a psychological public in view, he thought it superfluous to state the sources from which such well-known conceptions have sprung; but as his work is in all other respects adapted to badly-informed readers, it would have been desirable, on their account, to have supplied these omissions.

Notes on the Ancient Glaciers of New Zealand. With Map. By J. C. Russell. Reprinted from the "Annals of the Lyceum of Natural History." (New York: November, 1876.)

MR. RUSSELL was attached to the U.S. Transit of Venus Expedition, and finding himself stationed on the shores of Lake Wakatipu among the snow-fields and glaciers of the South Island of New Zealand he read what had been written on the ice-work of that region and supplemented his reading by the personal observations recorded in these notes. Though he does not add any important new fact to our previous knowledge he gives an interesting *résumé* of the physical geography of the glacier region, pointing out the evidence for the former greater extension of the ice-fields of New Zealand, and dwelling especially on the proofs of enormous erosion shown by the valleys and lake-basins.

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. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Nectar-secreting Glands

I HAVE briefly described in vol. xv. of the Linnæan Society's *Journal*, the nectar-glands found at the base of the fronds of the brake fern (*Pteris aquilina*) which are visited by ants for the sake of their sweet secretion. This case seemed to me to show in a striking manner that extra-floral nectar-glands are not necessarily protective in function, because the fern has, in England at least, extremely few enemies. The following extract of a letter lately received from Fritz Müller (of St. Catharina, Brazil) is of considerable interest in relation to this subject. He states that "the honey-glands on our *Pteris aquilina* serve, without doubt, to protect the ferns from the depredations of the leaf-cutting ants (Œcodoma), as is the case with Passiflora, Luffa, and many other plants. The glands of the Pteris are eagerly visited by a small black ant, Crematogaster, of which the Œcodoma seems to stand in great dread. On the other hand, when no protecting ants are present, I have seen Œcodoma gnawing the young fronds; here, as in other cases, it is only the young leaves that stand in need of protection, the older ones not being attacked by the leaf-cutting ants." This fact might, no doubt, be used as an argument by those who believe that all nectar-glands were originally developed as protective organs, and this argument would have great force if it could be shown that *Pteris aquilina* is a form which has arisen in countries where protection is needed; but even in that case there would remain the difficulty of accounting for the continued functional activity of the glands in districts where no such protection is required. Or it may be said that in past ages the glands on our European Pteris served as a protection against enemies which have now become extinct. But here we are again met by the difficulty of accounting for the continued activity of the glands. It is characteristic of evolution that great changes occur in the functions of organs, and I think that it will generally be allowed that even the most beauti-

fully adapted apparatus must have originated in an organ performing some comparatively simple function. The question at issue may perhaps be stated as follows :—In the cases where the nectar-glands are now well developed has there been a special course of structural development in close relation with the need of the plant for protection? Has there been a course of evolution such as we may believe has taken place in the formation of the food-bodies in *Acacia sphærocephala* and *Cecropia peltata*, or should we not rather believe that the sweet secretion has been developed in connection with some unknown process of nutrition; according to this view, a well developed system of glands may continue merely performing some obscure excretory function, and consequently, although the presence of nectar-glands has undoubtedly been of the utmost importance in determining the survival of certain species, yet it is hardly fair to assume that all nectar glands were originally protective in function. For many plants secrete large quantities of sweet fluid, which serves no such purpose. This argument is given by my father in his "Effects of Cross and Self-Fertilisation" (p. 402). In addition to the facts there given in support of this view, a curious case described by Prof. H. Hoffmann may be mentioned ("Ueber Honigthau," 1876). He states that numerous large drops of sweetish fluid appeared on the under-surface of the young leaves of a camellia. He also alludes to a similar abnormal production of honey-dew on an ivy plant.

In the case of introduced plants, we see how an already existing quality may, without any special course of development, become of vital importance to its possessor. Thus, Mr. Belt shows ("Naturalist in Nicaragua," p. 74) that the lime, *Citrus limonum*, is able to exist in a wild state, because its leaves are, from some unknown reason, distasteful to the leaf-cutting ants; whereas the orange, *C. aurantium*, and the citron, *C. medica*, can only survive with the help of man.

Fritz Müller concludes his letter with some curious facts on kindred subjects:—

"The extreme variability of the nectar-glands on the leaves of many plants, is a somewhat remarkable fact. Thus our Citharexylon has normally two large glands at the base of the leaves, but sometimes there is only one, and sometimes none at all; besides these there are small glands scattered over the surface of the leaf, the number of which varies from twenty to none. Similar variations occur in the nectar-glands of *Alchornea erythrospermum*, and of a Xanthoxylon. It seems to me probable that in all the cases at present known, these glands serve to attract protecting ants; and I here agree with Delpino, although I do not hold with him that caterpillars are the chief enemies which are guarded against by Pheidole and Crematogaster; but I think with Belt that these latter ants protect the plant against the leaf-cutting species. Indeed it is precisely those plants which are free from the attacks of ants that seem to be especially well fitted for caterpillars. Thus the larvæ of Gynaeria live on *Cecropia peltata*, those of *Epicalia numilia* on *Alchornea erythrospermum*. On the Cayien (?) whose leaves are furnished with nectar-glands, and are visited by protecting ants, the caterpillars of many species of Callidryas are found. Finally, as far as I know, all the larvæ of the genus Heliconius feed on Passiflora. Moreover, the same relation holds in the case of plants protected in other ways, for instance, by stinging hairs or by poisonous sap. How numerous are the larvæ found on the European stinging-nettle. In this country we find the caterpillars of 'Ageronien' on the stinging Dalechampia; and again those of some species of Danais on Asclepias, which is protected by its milky juice." FRANCIS DARWIN

Down, Beckenham, May 21

Quartzite Implements at Brandon

AT the recent conference held by the Anthropological Society on the present state of the question of the antiquity of man, the president, Mr. John Evans, referred to the finding of implements made of quartzite at Brandon, and remarked that as that rock did not exist in the neighbourhood excepting in the glacial drift, the implements must have been made from pebbles obtained from the glacial beds, and were therefore of post-glacial age. This statement was made at the end of the meeting when there was no opportunity of replying to it, and as its effect must have been great, I shall be obliged if you will allow one who believes that none of the palæolithic implements are of post-glacial manufacture to make some remarks upon it.

For a full description of the implement-bearing deposits near Brandon I must refer to an able paper by Mr. J. W. Flower in the twenty-fifth volume of the *Quarterly Journal of the Geological Society*. Gravel Hill, near Brandon, is an isolated hill rising to a height of 91 feet above the river, from which it is nearly a mile distant. It is covered with gravel which is mostly, and in some places entirely, composed of quartzite pebbles. Mr. Flower estimates that three-fourths of the whole are of quartzite. In this the gravel differs from that of other pits in the neighbourhood of Brandon, as for instance, that at Bromhill, which contains only one thirtieth part of quartzose pebbles.

At Gravel Hill, along with some hundreds of flint implements, four made of quartzite, similar to that of the pebbles, have been found. All the implements are usually found at the bottom of the gravel, and occasionally lie on the chalk. On the supposition that the quartzose pebbles, from which some of the implements have been made, were brought by ice in the glacial period, some such succession of events as the following must have occurred. 1. Ice, from the north, carried thousands of quartzite stones and deposited them in immense abundance over a limited area. 2. Man afterwards made implements from some of them. 3. The whole of the pebbles were rearranged and formed into beds of gravel with the implements at the bottom, whilst the distinctive character of the deposit was retained.

This ingenious but complicated theory is not necessary, for quartzose boulders and pebbles are found in deposits much older than the glacial period not very far away from the locality, and may exist beneath the drift close to it. It has long been known to geologists that there are many fragments of old crystalline rock in the upper greensand. They have been described by Mr. Bonney in his geology of Cambridge, and very fully by Messrs. Sollas and Jukes Browne, who state that fragments of gneiss, mica, and hornblende schists, talcose schists, granites, vein quartz, grits, quartzites, and slates are very numerous in this bed. It ranges northward from Cambridge, and is lost beneath the surface gravels and boulder clays, but it is not at all improbable that it may run along to the west of Brandon, and there contain even more quartzose fragments than in Cambridgeshire.

I may remark in conclusion that Mr. Flower, in his description, states, that he is disposed with the French geologists, to ascribe the outspread of the gravels to some powerful cataclysmal action, and that he does not know of any boulder clays in the course of the river from which such a mass of pebbles could have been derived. THOMAS BELT

Cornwall House, Ealing

The Migration of the Swiss Miocene Flora

WITH reference to the route the plants took which formed the European miocene flora, I should be glad to know *why* Dr. Unger considers it to have been from America to Europe. He says: "There is more than *one* reason for thinking that the centre from which our lignite flora has sprung was far away from Europe—in the southern parts of the United States" (*Journ. of Bot.*, iii. 17). He thinks that the living flora of that part of America is the lineal descendant of that which gave rise, by aid of "Atlantis," to the Swiss miocene flora. But is enough known of the miocene flora of the United States to infer this? Prof. Heer says that the methods of comparison he employed "incontestably prove that Switzerland was inhabited by types now scattered over every part of the world [agreeing in that respect with the existing Arctic flora], but of which the majority correspond with species of South U.S. of America; the Mediterranean region of Europe ranks second; Asia Minor, the Caucasus, and Japan third; the Atlantic Isles fourth, and North Holland fifth" (*Nat. Hist. Rev.*, 1862, p. 154, quoted by Oliver). Prof. Oliver and Sir Charles Lyell think that the route was by Japan, and not by the Atlantis; but still (Sir Charles, at least) *from* America *to* Europe. Heer, in his "Primæval World of Switzerland" (vol. i. p. 325, Eng. ed.), says the *Glyptostrobus heterophyllus* of Japan "has probably been derived from the tertiary species" [of Europe]. Similarly, in comparing the *Taxodium distichum miocenum* with that of America, he observes: "It is very interesting to find that the ancestors of the existing American swamp-cypresses were formerly spread over the whole of Europe, as far as 78° N. lat. Again, of *Sequoia Langsdorfii*, he observes: "It probably formed a zone round the whole earth in high northern latitudes."

Instead, then, of regarding either Switzerland or the South U.S. as a "centre," I would suggest that the miocene flora was uniformly spread over the whole of the regions bordering the

entire Arctic circle, just as the Arctic flora is now; Asia, Japan, and North-West America being then continuous; that as the climate became cooler in the pliocene epoch, it was driven southwards along every meridian, its descendants now existing in the localities above mentioned. It will be seen that these form a belt all round the globe, roughly speaking, between the 30th and 40th parallels of latitude. Migration to some extent might have taken place along that belt, but the great migration I suggest was probably from north to south, and not from east to west, or *vice versa*.

The above theory is simply an adaptation of that given by Dr. Hooker for a similar dispersion of the existing Arctic flora, which replaced the temperate floras of Europe, Asia, and America; but which on the return of a temperate climate retreated northwards as well as up temperate and even tropical mountains, perishing, however, in the low lands.

GEORGE HENSLOW

The Fertilisation of Orchids

WHILE botanising this spring in Portugal, I was struck with the fact that scarcely one of the orchids—species of Ophrys principally—that I had collected for my herbarium, or examined in the field, seemed to be fertilised, for none presented the least indication of having had pollen applied to its stigmatic surface; and I examined flowers in every stage of expansion, from the opening of the bud to the withered and shrunk up floral envelopes. Each one, I remarked besides, contained its own pollinia, their caudicles in their respective glands and in their natural position. I was so struck with this, that one day (March 31) I gathered and examined forty-five different flower-heads, and of all these only one was found to have pollen-grains on its stigma, and all, the fertilised one included, had their pollinia intact. The locality was the Tapada d'Ajuda, or Royal Park, situated just outside the city walls of Lisbon, an inclosure containing many acres of land, clothed in spring with a rich flora, and a favourite entomological hunting ground, teeming with Coleoptera, Hymenoptera, and the commoner Lepidoptera. Several of the orchids contained aphides, and a few harboured a species of small red ant.

On April 21 I again made similar observations, selecting the same place, as it was most accessible to me, and because several orchideæ grew there in the greatest profusion. On this occasion I examined over thirty flowers—none of them the same as I had examined in March, for I had plucked these at the time, but there was not one that did not possess its pollinia, and, as on the previous occasion, none of them showed any appearance of having had pollen-grains in contact with their stigmas.

Yet the Tapada, during the spring, produces these orchids by the thousand, vigorous, healthy, sweet-smelling plants, and in sunny days its air is perfectly alive with insect life, as I have said, of every kind.

HENRY O. FORBES

Old Hall, Highgate, N.

New Meteor Radiant

THE "two remarkable meteors" closely following each other, observed by Mr. Hope on May 13 (NATURE, vol. xvi. p. 43), proceeded most probably from a shower in the small southern constellation Crater. Your correspondent describes the point of first appearance as a *little* south of Arcturus. If this means about 7° or 8° below that star, then the observed courses accord well with the centre indicated, which is apparently quite a new radiant. From the Italian catalogue of 7,512 meteor paths (observed by Schiaparelli, Denza, and others in 1872) which I have lately been reducing, I found this shower at R.A. 170°, Dec. 10° S. (near $\epsilon - \theta$ Crateris) for May 3···15 from nine meteors.

Ashley Down, Bristol, May 27

W. F. DENNING

OUR ASTRONOMICAL COLUMN

ANTHELM'S STAR OF 1670.—The small star which is very close to the position of this object, as determined from the observations contained in Lemonnier's "Histoire Celeste," deserves to be closely watched, as there is more than a suspicion of its variation within narrow limits. Thus in August, 1872, it was exactly equal to a star which follows it 12·5s. in R.A., 4″·9 to the north; while in November, 1874, it was as certainly fainter by half a magnitude. This small star precedes the variable S. Vulpeculæ 43·6s., and is north of it 2′ 11″; two other small stars follow less than 2′ from the parallel, 22·4s. and 30·6s. respectively.

Prof. Schönfeld found the place of Anthelm's star from the observations of Hevelius and Picard,

R.A. 19h. 41m. 37s. N.P.D. 63° 2′·3 for 1855·0.

The place of the suspicious star alluded to above is, for the same epoch,

R.A. 19h. 41m. 43·5. N.P.D. 63° 2′ 7″.

It was meridionally observed at Greenwich in 1872.

D'ARREST'S COMET.—As yet we hear nothing of observations of the short-period comet of D'Arrest, for which M. Leveau has worked so laboriously, with the view to facilitate its discovery at the present return. The intensity of light was at a maximum about May 22, but diminishes gradually during the summer. Nevertheless, early in August it is still of the same amount as when Prof. Schmidt discontinued his observations at Athens in December, 1870, at which time he stated he could have followed it longer but for the want of an ephemeris. Subjoined are the comet's calculated positions during the absence of moonlight in the present month, for Paris noon:—

	Right Ascension.	North Polar Distance.	Distance from the Earth.
	h. m. s.	° ′	
June 7 ...	1 18 58 ...	85 26·2 ...	1·613
,, 9 ...	1 25 14 ...	85 11·8 ...	—
,, 11 ...	1 31 27 ...	84 58·2 ...	1·611
,, 13 ...	1 37 35 ...	84 45·2 ...	—
,, 15 ...	1 43 40 ...	84 33·0 ...	1·609
,, 17 ...	1 49 41 ...	84 21·5 ...	—
,, 19 ...	1 55 38 ...	84 10·7 ...	1·607

Though the comet will not arrive at its least distance from the earth during the present visit (1·396) until October 20, it passed its perihelion on May 10. The period of revolution is now 2,434 days, or 35½ days longer than at its last appearance in 1870.

THE D'ANGOS COMET OF 1784.—Perhaps no person who has been occupied in astronomical observation and calculation has obtained for himself, rightly or wrongly, a more unenviable notoriety than the Chevalier D'Angos, who, in the latter part of the eighteenth century, was possessed of a small observatory in the island of Malta. From the unusual character of some of his statements his name came to be associated with anything in the way of observation that appeared to be apocryphal, and we find not only Zach was in the habit of terming doubtful assertions "*Angosiades*," but even Pastorff, who himself put upon record more than one suspicious statement, appeared to consider that he was establishing the good faith of an observation of a comet in transit across the sun's disc by declaring that it was not an observation made "à la D'Angos." We pass over on this occasion the reported observations by D'Angos in 1784 and 1798 of a comet or planet upon the sun, with the view to presenting the reader with a brief outline of the actual state of a case that has been open to still greater suspicion, viz., his observation and calculation of what appears in some of our catalogues as the second comet of 1784; and we may be pardoned for bringing together here particulars which though probably known to those who have access to a good astronomical library, are not so likely to be within the cognisance of those who cannot command such a collection. And further, it is almost essential to bring the main points in the case into one view, to enable the reader to judge for himself whether D'Angos is deserving of the opprobrium which has been cast upon him or not.

Writing from Malta on April 15, 1784, D'Angos apprised Messier at Paris that he had discovered a comet in Vulpecula on April 11, and he inclosed two approximate positions observed on the mornings of these days. He stated that the comet was very small, without tail, and with only "a slight appearance of nebulosity." Messier

did not receive this letter until May 14, when he sought in vain for the comet. Pingré who wrote before any suspicion had been raised with respect to D'Angos, attributed this to its having in the interval receded to too great a distance from the earth, or having attained too great south declination. It appears that Messier did not receive any further observations from Malta, but D'Angos some time afterwards communicated to him elements of the orbit, calculated by himself, and it was to be presumed with the aid of further positions. The observatory at Malta was burnt at a subsequent period, and the whole of the papers, &c., of D'Angos were stated to have fallen a prey to the flames, so that it was supposed in France that the observations were irrecoverably lost. Burckhardt had endeavoured by successive hypotheses to extract some idea of the nature of the orbit from the two rough observations which he had received, and as his results differed widely from those of D'Angos, and even the elements of the latter did not represent these observations, Delambre, at the instance of Burckhardt, wrote for further particulars. In reply, D'Angos stated that he had only saved from the fire his meteorological journal, in which, under date April 22, was mentioned an observation of the zodiacal light, without any reference to the comet, whence he concluded that on this date the latter was no longer visible.

This assertion will appear a most extraordinary one when it is stated that so far from the observations being lost, they had appeared in a memoir drawn up by D'Angos himself, in a periodical conducted by Bernoulli and Hindenberg, entitled—*Leipziger Magazin für reine und angewandte Mathematik*, Leipzig, 1786, where they were discovered by Olbers, as he mentions in a letter to Encke, inviting his discussion of them. Positions of the comet in longitude and latitude are there given for fourteen nights between April 10 and May 1, and they are followed by the elements of the orbit, which D'Angos says he had calculated from them.

Zach in 1812 had suspected that the observations of the second comet of 1784 were imaginary, and had suggested that the orbit should be omitted from the catalogues, but he adds as he had only great probabilities and moral, not mathematical, proofs to support his view, he did not insist upon it. To provoke an explanation, however, he states he had enveloped "ce mystère d'iniquité" in a problem in vol. iii. of his *Correspondance Astronomique*, where he printed a series of positions of a body, which he invited his readers to explain, and which puzzled Olbers and Bessel who failed, like others, to discover Zach's meaning. Burckhardt also on receiving intimation from Olbers of his having brought to light what purported to be the observations of D'Angos, remarked upon the importance attaching to the circumstance, since it might lead to proof that they had been fabricated.

It remains to describe in a future note or notes, the results of Encke's investigation and of later inquiries relative to the comet of D'Angos.

PROF. SYLVESTER ON TEACHING AND "RESEARCHING"

IN the address of Prof. Sylvester at the Johns Hopkins University, to which we have already referred, he spoke as follows on the above subject :—

Let me take this opportunity of making my profession of faith on a subject much mooted at the present day, as to whether the highest grade of university appointments should be conferred with or without the condition of teaching annexed.

I hesitate not to say that, in my opinion, the two functions of teaching and working in science should never be divorced. I believe that none are so well fitted to impart knowledge (if they will but recognise as existing, and take the necessary pains to acquire, the art of presentation) as those who are engaged in reviewing its methods and extending its boundaries—and I am sure that there is no stimulus so advantageous to the original investigator as that which springs from contact with other minds and the necessity for going afresh to the foundations of his knowledge, which the work of teaching imposes upon him. I look forward to the courses of lectures that I hope to deliver in succession within the walls of this university as marking the inauguration of a new era of productivity in my own scientific existence; nor need I consider any subject too low (as it is sometimes foolishly termed) for me to teach, when I remember to have seen the minutes of the conversation held between the delegates of the Convention, at the time of the French Revolution, and the illustrious Lagrange, the son of the pastry-cook of Turin, possibly the progenitor of the Marquis Lagrange, of turf celebrity (Citoyen Lagrange, as he is styled in the record), who, when asked what subject he would be willing to profess for the benefit of the community, answered meekly, "I will lecture on Arithmetic."

At this moment I happen to be engaged in a research of fascinating interest to myself, and which, if the day only responds to the promise of its dawn, will meet, I believe, a sympathetic response from the Professors of our divine Algebraical art wherever scattered through the world.

These are things called Algebraical Forms. Prof. Cayley calls them Quantics. These are not, properly speaking, Geometrical Forms, although capable, to some extent, of being embodied in them, but rather schemes of processes, or of operations for forming, for calling into existence, as it were, algebraic quantities.

To every such Quantic is associated an infinite variety of other forms that may be regarded as engendered from and floating, like an atmosphere, around it—but infinite in number as are these derived existences, these emanations from the parent form, it is found that they admit of being obtained by composition, by mixture, so to say, of a certain limited number of fundamental forms, standard rays, as they might be termed in the Algebraic Spectrum of the Quantic to which they belong. And, as it is a leading pursuit of the Physicists of the present day to ascertain the fixed lines in the spectrum of every chemical substance, so it is the aim and object of a great school of mathematicians to make out the fundamental derived forms, the Covariants and Invariants, as they are called, of these Quantics.

This is the kind of investigation in which I have, for the last month or two been immersed, and which I entertain great hopes of bringing to a successful issue. Why do I mention it here? It is to illustrate my opinion as to the invaluable aid of teaching to the teacher, in throwing him back upon his own thoughts and leading him to evolve new results from ideas that would have otherwise remained passive or dormant in his mind.

But for the persistence of a student of this University in urging upon me his desire to study with me the modern Algebra I should never have been led into this investigation; and the new facts and principles which I have discovered in regard to it (important facts, I believe,) would, so far as I am concerned, have remained still hidden in the womb of time. In vain I represented to this inquisitive student that he would do better to take up some other subject lying less off the beaten track of study, such as the higher parts of the Calculus or Elliptic Functions, or the theory of Substitutions, or I wot not what besides. He stuck with perfect respectfulness, but with invincible pertinacity, to his point. He would have the New Algebra (Heaven knows where he had heard about it, for it is almost unknown in this continent), that or nothing. I was obliged to yield, and what was the consequence?

In trying to throw light upon an obscure explanation in our text-book, my brain took fire, I plunged with requickened zeal into a subject which I had for years abandoned, and found food for thoughts which have engaged my attention for a considerable time past, and will probably occupy all my powers of contemplation advantageously for several months to come.

OUR INSECT FOES

AN important conference was held at the Society of Arts on Tuesday afternoon on the subject of insects injurious to agriculture and methods of stamping them out. Its origin was a proposition by Mr. Andrew Murray (who has had the arrangement of the collection of economic entomology made by the Science and Art Department), which he laid before the Lord President of the Council. The proposition was printed by order of the president, and copies were sent to the agricultural societies and chambers of agriculture of the country. After the proposition had been before them for two months and there was no indication of any notice being taken of it, it was arranged that a conference of delegates of agricultural societies should be held at the Society of Arts. The Duke of Buccleuch, K.G., took the chair, and there were present representatives of the Scottish, Cheshire, Warwick, Hampshire, and Banbury Chambers of Agriculture; the Farmers' Club, Dr. Maxwell Masters, representing the president of the Royal Society, Prof. Voelcker (chemist to the Royal Agricultural Society), Mr. Sewell Reed, M.P., &c. The conference was opened by a paper read by Mr. Murray.

The paper commenced by assuming as an axiom that, besides the occasional great injury done by insects, by which whole districts are ravaged, a continual drain is constantly kept up by them, which constitutes a very perceptible percentage of deduction from the cultivators' profits; and, further, that where this loss can be prevented at less cost than the loss it occasions, it should be prevented.

It next maintained that, if we wish to rid a district or a country of an injurious insect, to be effective, any attempt to do so must be simultaneous and combined, for to what purpose would it be if one man cleared his farm if his neighbour did not clear his; or if the one cleared his one year, and the other cleared his another? A central authority, therefore, is needed to secure united action.

It next considered the various ways in which the insects injurious to agriculture might be extirpated. The first, the simplest, the most powerful, and the most efficient of these is county or district rotation of cropping. Farmers know well enough the advantage of a rotation of cropping (or its equivalent) on their own farms. By long-continued growth of the same crop on the same land the soil becomes exhausted of some of the elements necessary for the proper development of that kind of crop, and a change of crop brings other elements into use, and relaxes the demand upon those that have been too much drawn upon.

Exactly the converse of this takes place with regard to certain insects. The great majority of vegetable-feeding insects do not feed on all kinds of plants indiscriminately; most of them are restricted to one kind of plant, and if by cultivation of that plant its numbers are enormously increased, so will naturally be the number of the insects that feed upon it; while, if we should cease to grow that plant, the number of the insects would correspondingly diminish. Thus, for instance, if a district is almost entirely in pasture, there will be very few wheat-feeding insects in it, but if it is turned into a wheat country they will be myriads. If these numbers reach such a pitch as to deteriorate the crops the remedy is plain. Change the rotation, and grow some other crop instead of wheat. Most of the wheat insects are only annuals. If they could be banished for one year they would be banished entirely, or until re-introduced. Now, if there were a controlling authority, what would be easier than to say to the farmers, "Gentlemen, in the common interest you will substitute barley for wheat in your next year's rotation." The insect, deprived of its proper nidus, must then either lay its eggs in an unsuitable place where they will perish, or have recourse to the pasture fields for *Triticum repens*, or other suitable grasses. By this, of course, the fly would not be exterminated, but its numbers would be so reduced as to render it comparatively harmless, at all events for a time, when, if it again reappeared in force, the same means of defence would be resorted to. Nay, it might be so arranged that two or more counties might brigade themselves together, so as to establish a permanent see-saw by which they should play into each other's hands. But no single man can carry out such a rotation. He may try it upon his own fields, but they will be replenished continually from the fields of his neighbours, unless they at the same time are compelled to follow the same rotation.

Mr. Murray then went over the various other means of extirpation—picking and burning infected plants, the collecting caterpillars, poisons, and local remedies, in relation to which he drew attention to the destruction of what are called ticks and lice upon sheep. Everyone knows how readily such vermin can be communicated by contact or even proximity, and it does seem a very hard case that a man, who has kept his flock clean by taking proper precautions, should be liable to have them infected by a neighbouring neglected flock, by stray sheep, or even by sheep passing along the road. It is said that, *cæteris paribus*, the difference in value between a sheep that has been kept clean for the season and one that has been worried by vermin will be 20*s*. If that is so it is a wonder that sheep farmers have not long since clamoured for some supervision.

At the conclusion of the paper the following resolution was put from the chair and carried:—"That thanks are due to the President and Lords of the Council for having brought the subject of insect damage under the consideration of the agricultural bodies of the kingdom."

Dr. Maxwell Masters moved the next resolution, and in doing so said he was charged to express the regret of the President of the Royal Society that he was unable to be present. He spoke of the great ignorance throughout the country on the subject of insect damage, and as an indication of the amount of damage done, said that half the time of the Scientific Committee of the Royal Horticultural Society was occupied with answering inquiries from all parts as to how to deal with insect foes. The resolution he moved was,—"That much of the loss occasioned by insects is preventible, and ought to be prevented." This was seconded by Mr. Maclagan, and carried.

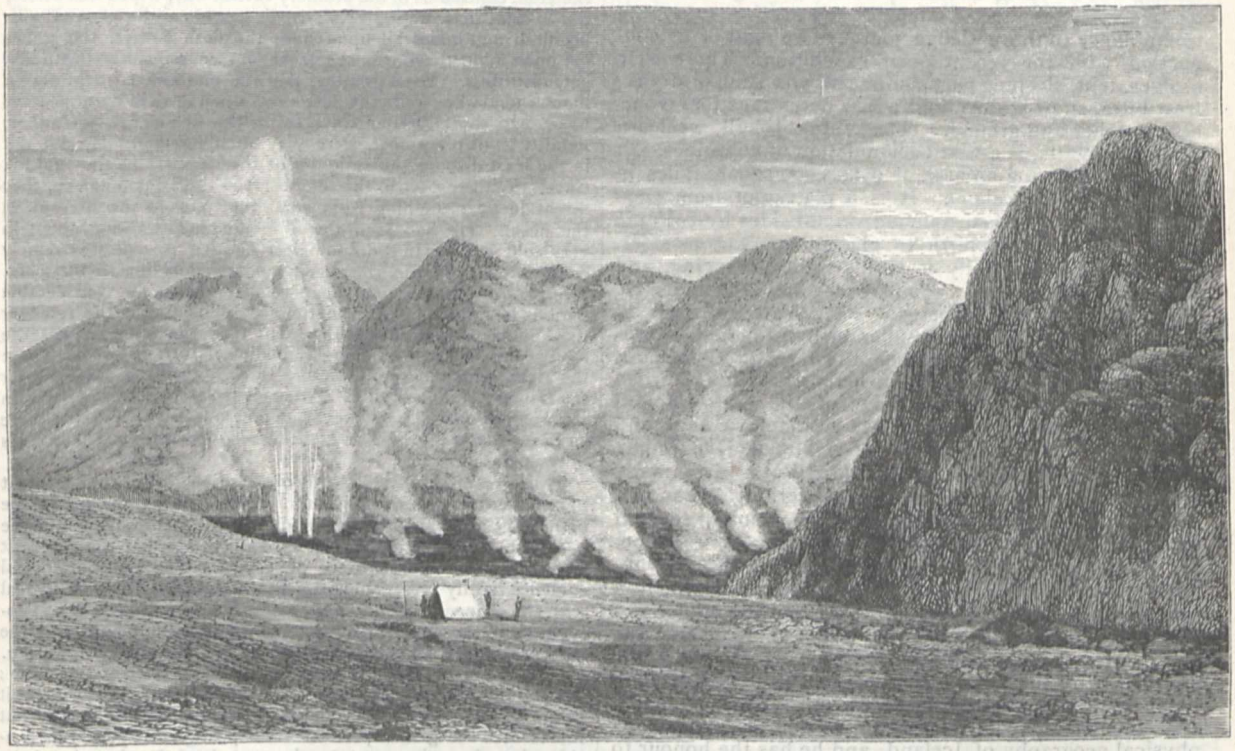
Mr. Mechi then moved—"That it properly belongs to Government to provide the necessary means for protecting cultivators from this loss, as it is only by combined and simultaneous action over considerable districts that it can be effectually done, and Government alone possesses or can obtain the requisite means of enforcing such action."

Both Mr. Mechi in moving it and Prof. Voelcker in seconding it, spoke of the want of knowledge throughout the country on the subject. Mr. Sewell Reed urged it was not a question for government but for agricultural societies. The resolution was declared carried, though many hands were held up against it.

The last resolution was—"That the President and Lords of the Council and the Agricultural Societies of the United Kingdom be informed of the opinion of this Conference, and urged to take the subject at once into their consideration, with a view of providing a remedy," which, after a long discussion, was carried.

THE VOLCANOES OF ICELAND

DURING the past year the Danish Government despatched the well-known geologist, Prof. Johnstrup, to Iceland, for the purpose of making a thorough scientific investigation of the scene of the recent volcanic disturbances. A short time since he laid before the Danish Parliament a report of his journey, with a brief account of the results so far obtained. The first part of the expedition was devoted to the volcanoes in the Dyngju Mountains, encircling the valley of Askja, and was accompanied with many difficulties resulting from the conformation of the region and the prevalence of violent snow-storms. The mountains themselves are not of volcanic origin, but consist of basalt and palagonite-breccia. In former times the Askja Valley was evidently much deeper than at present. Repeated flows of lava have gradually filled it up, and these Prof. Johnstrup believes to have occurred within the historic period, although no mention of volcanic disturbances in this district is to be found in the annals of the island. Along the outer edge of the Dyngju Mountains are numerous craters, some of considerable size, which have contributed most of the lava covering the plain of Odadahrann to the extent of sixty square geographical miles. Part of this enormous quantity of lava had its origin in the neighbouring volcano of Trölladyngja. It is, however, sharply distinguished from the twisted, contorted, masses of the former, by its more regular character and smooth crusts. In the neighbourhood of the newly-formed craters the earth is covered to the distance of over a mile with the bright yellow pumice-stone ejected during the eruption of March 29, 1875. Most of the pieces are seven to eight inches in diameter; many contained two to three cubic feet. In places where the pumice-stone is several feet in depth, it covers a layer of snow twenty-five



The Oskjagja. (From Watts's "Across the Vatna Jökull.")

feet deep, which fell in the winter of 1874-1875, and has been protected from the effects of solar warmth by the feeble conductive power of the pumice-stone. It is fortunate for the land that the outbreak was of this nature, for from its lightness the pumice-stone can be easily removed from the surface of the country. The party examined the most northerly of the craters, which was 300 feet wide and 150 feet deep. It was filled with steam, which was driven out with such force as to give rise to a most deafening roar. No solid matter, however, was borne along with the vapour. Not far from the crater an extensive depression in the valley of Askja has taken place, and the fresh surfaces of rock exposed thereby give a clear picture of the peculiar formation of the valley by successive deposits. It presents a remarkable similarity to the basalt and dolerite formations so prevalent in the mountain ranges of Iceland.

The most surprising feature of these late eruptions was the ejection of such enormous masses of pumice-stone, while not a trace of a lava stream is to be found. A similar outbreak is not mentioned in the records of the island. On account also of the vast development of steam, which gave rise to the pumice-stone formation, they are without a parallel amongst volcanic phenomena. At present the craters are to be regarded as gigantic steam escape tubes, the activity of which will continue for an uncertain period, but with gradually decreasing intensity. As long as these safety valves remain open it is not probable that a repetition of the eruptions will occur in the immediate future.

The volcanoes in Myvatns Öræfi were found to present entirely different characteristics. This barren plain is about thirty-five miles long and thirteen miles wide. Suddenly, on February 18, 1875, a volcano appeared in the centre. Four others appeared at subsequent dates, all of the craters falling into a straight line north and south. No eruptions have occurred here within historic times. The mass of lava which issued from these various craters is estimated at 10,000,000,000 cubic feet, eighteen times the amount supposed to have been emitted by Vesuvius in 1794 and 1855. The lava was basaltic and

viscous when emitted, and crystals of chloride of ammonium were found in the vicinity of the craters. Only slight traces of the volcanic action remain now, where warm air arises from the thicker layers of lava.

Prof. Johnstrup is engaged at present in the preparation of maps showing the successive deposits of lava from the older eruptions, as well as from the more recent. The Hlidar range, hitherto regarded as a palagonite formation, was found by him to consist of trachytic masses, a more ancient, and in Iceland rarer, formation than palagonite.

In connection with Prof. Johnstrup's Report we may refer to Mr. Watts's interesting narrative of his journey across the Vatna Jökull.[1] Mr. Watts's name is already well known in connection with recent exploration in Iceland. He has for long had a strong desire to cross the Vatna Jökull, and at last succeeded. We infer—for his narrative is almost innocent of dates—that the feat took place in the summer of 1875. The preparations made remind one of those necessary before setting out on an Arctic expedition, and the whole journey bore a strong resemblance to those sledge journeys we read of in connection with the recent polar expedition. There were sledges, tent, sleeping bag, pemmican, and similar stores; frost-bites, snow-storms, and weary detentions for favourable weather and ground. The Vatna Jökull, we learn from Mr. Watts, is a vast accumulation of volcanoes, ice, and snow, covering an area of over 3,000 square miles in the south-east of Iceland. It is a plateau of from 4,000 to 6,000 feet high, is surrounded on all sides by volcanic mountains, and gives birth to glaciers on various sides. On the south especially it seems to be advancing, and there the glacier may soon reach the sea and give birth to miniature icebergs. Mr. Watts crossed at the east side, and after suffering considerable hardships he and his party reached the farm of Grimstadr, in the north of Iceland. From here Mr. Watts returned southwards to the northern edge of the Vatna Jökull for the purpose of examining the Oskjagja, a huge and active crater on the south of the Askja, or Dyngjufjall, referred to by Prof. Johnstrup. Mr. Watts gives many interesting and important details concerning this mountain and the desolate country in its vicinity, covered with pumice dust and other products of eruption. Mr. Watts also visited the region around the Myvatn Lake, near which are the sulphur deposits which a company was started to work. After visiting one or two places on the north coast he returned to Reykjavik right across the centre of the country. Notwithstanding the defects of style, the want of dates, and occasional vagueness, Mr. Watts's narrative is a really valuable and interesting contribution to a knowledge of the physical geography of Iceland, and he has the honour to be the first, so far as known, to have crossed the great Icelandic waste.

THE ANTIQUITY OF MAN

IN the number for May 24 we gave abstracts of the papers read by Professors Dawkins and Hughes, and Mr. Tiddeman at the Conference on the subject of the Antiquity of Man at the Anthropological Institute, and this week we give a report of the discussion which followed the reading of these papers, the remarks of the various speakers, we may state, having been revised by themselves.

Prof. Busk wished to explain, before the discussion commenced, the circumstances connected with the interesting fragment of bone, for the determination of which he was personally responsible. This "bone of contention" was represented by the cast which he held in his hand. He was surprised that such a large superstructure had been raised upon that particular piece. It was merely a fragment, evidently of a fibula, one of the most variable bones in the body. It was received by him, together with a large collection of other remains from Mr. Tiddeman, and for a long time remained an insoluble problem. At last, after many conjectural determinations by himself and others, Mr. James Flower, the well-known articulator to the Royal College of Surgeons, discovered in the College a human fibula of unusual size, and with which, as he pointed out, the Victoria Cave bone corresponded in many particulars. This determination, with the reasons for it, and illustrated by figures, was published in the *Journal* of the Institute. At the same time Mr. Busk was perfectly open to be convinced that it might be ursine. But although Prof. Boyd Dawkins had been good enough to show him bones of fossil bears of surprising size, none of them quite came up to the one in question. Nor at Toulouse, where there is such an enormous collection of ursine remains, did Mr. Busk observe any of corresponding dimensions. He was himself still disposed to regard the specimen as a fragment of an abnormally large human fibula, but thought that at present it would be unsafe to build any strong conclusions upon it.

Prof. Rolleston stated that in digging out a British skeleton he came upon a fibula standing vertically. They went on and he took out every bone with his own hands and they came to a skeleton, contracted in the ordinary British way, which was whole, minus that one fibula. A man is put into the ground with his flesh and bones all upon him, the flesh decays, the stones get upon him, the bones are loose and consequently the fibula gets disturbed. Even granting that the one before them was a human fibula he would lay less stress upon it than on any other bone. In the Gibraltar Cave series the fibulæ, owing to their liability to displacement, were very often missing. He did not in the least dispute the antiquity of the deposits in the Victoria Cave. With respect to the reindeer and the hippopotamus, they might judge something from what they saw in the life and in the flesh. He had seen the hippopotamus walking about in very cold weather in the Zoological Gardens seeming extremely comfortable, and the rhinoceros and reindeer the same. Mr. Evelyn, of Wotton, had kept reindeer alive for considerable periods in England. At the time of Julius Cæsar the reindeer lived in Germany. At the present time the reindeer was the food of the tiger in the Isle of Saghalien, North of Japan. There the tiger, which has a black and thick fur, crosses the ice after the reindeer. The skull of a young hippopotamus was found in England, showing that the hippopotamus really did live here and breed here too. Hence, mammals were not good indicators of temperature.

Prof. Prestwich referred to the observation of the president, that to consider the present subject thoroughly required the knowledge of the palæontologist, the anthropologist, the archæologist, and the geologist. He thought that it specially concerned the geologist with regard to the sequence of events. The palæontological evidence hardly presented sufficient differences. We had to deal with the sequence of man from his first appearance in time geologically to the present period. He would confine himself to the evidence in the south of England and in the north of France. In the south of England it was particularly clear and decisive; the datum line was distinct. It was afforded by the deposit of the boulder clay, which ranged as far south as London. That represented the glacial period. The post-glacial period he considered to be subsequent to the period of the deposit of the boulder clay. Most of the discoveries made in this country have been made in the districts of the south which have been covered by the boulder clay, and it is in the drift and gravel of the valleys excavated in the boulder clay of those districts that the flint implements have been so largely found; therefore he believed that in all that area man is of post-glacial age. If we got two levels on either side of a valley, so many feet above sea-level, with the boulder clay cut off on either side, then of course the *débris* at the bottom of the valley would consist of gravel, and so on, derived from materials which had been formed by the destruction of the several strata which originally traversed that valley. The materials so spread out were necessarily newer than the boulder clay; consequently man in the valleys was post-glacial. There were sometimes two or three successive levels of gravels in those valleys. If a valley was excavated to a certain depth, and a deposit was formed in which they could find no traces of the existence of man, whilst at another and deeper level flint implements were found, then man was introduced in that place only when the valley was excavated to its greatest depth and the gravel was spread out on the site now nearly occupied by our present rivers. Unfortunately the mammalian remains of those nearly connected periods were so alike that it was impossible to determine from the distinction of age. Bone caves were also found on the sides of valleys and in districts where there was scarcely any

[1] "Across the Vatna Jökull; or, Scenes in Iceland," by William Lord Watts. (London: Longmans and Co.)

boulder clay, and we were then left to the palæontological evidence. With regard to the possible correlation of other deposits found in the south of England with the deposits which preceded the glacial period in the north, there was evidence in both areas of the land having been inhabited previous to the boulder clay period by animals which were likely to serve as the food of man. There was no *à priori* reason why man should not have existed before that period. Much would have to depend upon that complete palæontological evidence which possibly Mr. Tiddeman might have at some future period to bring before them rather than upon geological position. He was disposed to consider with Mr. Tiddeman that the cave he was now investigating might be of pre-glacial age. He thought that the evidence rather tended to show it was pre-glacial, but it was not conclusive. What might be decided upon that particular point must, however, depend upon further research. Taking again the valley of the Thames, we found flint implements in terraces raised some twenty, thirty, or forty feet above the present level of the river. At Reculver we found such evidence of the existence of man in a gravel eighty feet high, but as we ascended the valley we found the flint implements confined to the lower levels. At Reading no flint implements or mammalians are found in the high-level gravel. So also in the neighbourhood of Oxford mammalian remains and implements are found in the low-level gravel but none in the higher. Thus at the entrance of the Thames valley near to France we find evidence of man in the later high-level gravels, but man had not then penetrated into the Upper Thames valley. It was evident that at the period that those higher terraces were deposited in the upper valley of the Thames as far down as Maidenhead, very cold conditions pervaded, though post-glacial to the boulder clay. In the neighbourhood of Oxford there have been found in this upper gravel boulders of several tons in weight which had been carried from a very long distance, and he had recently observed in the neighbourhood of Reading some high-level gravel resting upon an ice-pitted surface of stiff clay in which there was no calcareous matter, presenting that sort of section (drawing it on the black board). A surface the size of that room was exposed. It seemed to him, however, that with respect to pre-glacial man there was an important "suspense account" now accumulating. In France an large series of observations had been made by competent observers, and it would not do to ignore the points they had brought forward. He had some reason now to believe from his own observations that there was evidence of man being pre-glacial even in the north of France. He also produced one specimen from the Red Crag which had been in his possession for many years. He could not answer for the labelling but only for the locality and the condition of the bone, but from the peculiar way in which it had been cut and then broken it had all the appearance of having been artificially worked, but he should certainly only put it to a suspense account. With respect to one observation of Prof. Dawkins, that the oldest implements were ruder than the newer ones, he would remark that one cause why the implements of Creswell Cave were so rude was because they were made of quartzite, which could not be finished in the same way as flint. At Amiens the older high-level implements were often more finished and finer than those of the low-level gravel.

Col. Lane Fox wished to say a few words upon a point not yet touched upon in any of the papers which had been read, viz., the means by which valleys had been eroded, and the time necessary to accomplish it. The uniformitarian theory, by which it was assumed that all the work of excavating valleys had been performed by means of their rivers flowing under the same conditions as at present, had been a good deal modified of late years, and he thought he could add a few facts from personal observation tending to show that some modification of the theory was necessary. With respect to the valley of the Somme, there was evidence afforded by relics of the Roman and bronze age found in the peat in the bottom of the valley, that the river had not materially lowered its bed since those relics were deposited, and therefore it must have taken an enormous time to work out the whole valley by means of a river which flowed with the same eroding power as at present. The valley of the Somme, however, was so comparatively narrow that it was possible the whole of it might have been eroded by such means, if sufficient time were allowed. But if it could be shown that the same conditions prevailed in other very much larger valleys where the work to be done was much greater, that would afford fair presumptive evidence that the eroding force must have been greater. He could mention one or two facts which showed that the Thames like the Somme had never shifted its bed since the bronze period. The first of these was that the river some way below Oxford, at the village of Dorchester, made a great bend; the ground on one side was high, and on the other, in the space inclosed by the bend perfectly, flat and low; there was an ancient intrenchment running across this low ground from bank to bank, and converting the promontory formed by the bend of the river into a fortress. It had been ascertained by means of the relics, consisting of pottery, flints, bronze implements, &c., associated with this intrenchment, that it was certainly as early as the bronze period, and perhaps earlier, no relic of Roman work having been found there, although Dorchester, close by, was a Roman station. The intrenchment in order to serve its purpose must have rested its flanks on the river at the time it was made, and the fact of their resting on the banks at the present time, although they are only a foot or two in height, showed that the river had not shifted or lowered its bed since the bronze age. Other evidence giving the same results was found in the same river lower down. Between Richmond and Battersea the Thames makes three or four bends in the comparatively flat bottom of the valley which is here more than four miles wide. He had found flint implements of the drift type deposited in sedimentary sand and gravel at Acton eighty feet above the present river, the discovery of which was communicated by him to the Geological Society and published in their journal. The river then since these implements were deposited must not only have lowered its bed eighty feet, but, according to the uniformitarian theory, must at each successive level have shifted its bed repeatedly so as to work out the valley here more than four miles wide. Yet bronze and stone implements have been found in considerable numbers in all the various bends of the present river dredged up from the gravel at the bottom by the dredging machines that have been employed of late years, and proving that the river had neither lowered nor shifted its bed since the bronze period, but if anything it had risen since that time. Was it possible, he would submit, that at this rate of progress, if progress it could be called, the erosion of the valley could be attributed to the present river flowing under the same conditions as at present? But if, as believed by Prof. Boyd Dawkins and Mr. Tiddeman, man existed in these parts during the subsidence of the glacial epoch, that would account, he thought, for a much greater flow of water having passed down these valleys in palæolithic times than was the case at present. In the valley of the Solent the same class of evidence was obtained. Mr. Evans had shown what a large amount of depression and erosion must have taken place in this valley since drift implements were deposited on the hill at Southampton. The valley of the Solent, from Portsdown to the Isle of Wight, is nine miles wide, and we have evidence in the Roman fortress at Porchester how little it has changed in modern times; yet in the centre of this valley near Southsea common, Col. Fox had some years ago discovered a flint station of the neolithic age, including celts, scrapers, and flakes in great abundance, the site of which was less than ten feet above the present high-water mark, showing that flint implements continued to be fabricated in the valley after land and water had assumed its present distribution. All these facts, he thought, favoured the opinion that powerful eroding forces must have been at work before that time. The very valuable papers which had been read treated only the geological aspects of the question, but as the President had observed there were ethnological and sociological problems to be solved, how long would it have required for the various races of man to diverge, and the earliest traces of culture to be evolved? He trusted that even if no other result came of the conference it would show that we had not yet exhausted the subject.

Prof. A. H. Sayce had to confess that the evidence of language as regarded the antiquity of man was not so decisive as that of geology. Under certain conditions the vocabulary of a language changed rapidly, under other conditions it changed slowly. The grammar of a language may be said to change never, and its structure to change very rarely. If these conclusions were applied to two or three of the principal families of speech, the results would be something like this: Take the Semitic class of languages; by means of the Assyrian monuments we are able to get back to 2000 B.C. for a starting point, when those languages were pretty much as they are to-day. Scarcely any of the structure, or grammar, or vocabulary has changed, but it is plain enough that they pre-supposed several earlier stages of existence, and when compared with the grammar of the old Egyptian there was a time when the parent

language seems to have been the parent also of the old Egyptian. But in order to allow for the changes that had taken place in the structure of the Semitic languages, and the structure of the Egyptian language, we must assume a very great period of time. With regard to the Aryan family, the different dialects could be traced back to the parent speech spoken in some part of Western Asia. That parent language could be restored by comparison with the later languages and dialects. In all points that parent speech was as fully developed as Sanscrit, or Greek, or Latin, the people who spoke it were in an advanced stage of civilisation, and the language itself was in a highly advanced condition. When the grammatical details of the language were analysed, it became quite plain that it was the product of a long series of successive stages of growth. Take another language, the old language of Chaldea. The earliest monuments that contained that language were between 3000 and 2000 B.C. On these monuments the language appeared in a stage of the most utter decline and decay. Therefore there was evidence of a language which had behind it a long and undetermined past. If, as several scholars believed, that language belonged to the Ural-Altaic family, in order to get back to a period when those languages were one and the same, they must suppose an enormous period of time. There was another consideration connected with the evidence of language. It would seem that most languages, whatever their present structure might be, were at one time in a condition similar to that of the Esquimaux language at the present time, that is to say, a time when as yet the single word is not distinguished from the sentence as an independent unit, but forms part of the sentence in which it is embodied. In the case of languages so highly developed as, say, the Aryan languages, in order to get back to a time when those languages were in a condition similar to the present condition of the Esquimaux language, they must allow not hundreds but thousands of years. Those were the conclusions to which the present investigations of language would appear to point.

Mr. T. K. Callard, referring to the outline of the head of a horse, drawn upon a bone represented as belonging to the palæolithic age, found in association with extinct animals, said they had always been led to think that palæolithic man was a rude savage who could only chip his flint implement, but who could not smooth it (that would indicate the neolithic period), but they were now getting evidence of a different character. They had heard of a bone needle being found in the cave-earth, which at once suggested a step in civilisation, as men did not make bone needles unless they intended to use them, and that would lead their thoughts to a palæolithic tailor. In that very cave were found traces of a no mean artist, for not one man in three at the present time could make a sketch like that of the horse. It struck him that that Royal Academician of the palæolithic age had for his model a horse with his mane *clipped*, which indicated another stage of civilisation. Were they justified in saying that because the remains of mammoth and woolly rhinoceros were found in close proximity to the remains of man, therefore man lived at such a remote period? He was inclined to think that it proved not so much the antiquity of man, as that the extinct mammalia were more modern than they are supposed to be. The works of man being found with the remains of the extinct mammalia, tells nothing of the period of man's existence, unless it is also proved when the mammalia referred to became extinct; of this there was no proof adduced, and therefore, to his mind, the argument for man's antiquity based on the contemporaneity of man and the extinct mammalia has not been sustained.

Mr. Harrison said the palæolithic character of the flint implements found at Cissbury in connection with the remains of existing fauna, including goat and pig, showed that the form and finish of prehistoric tools and weapons were not of themselves a safe criterion of age. Though the earliest implements would necessarily have been the rudest, the converse was by no means true. There were doubtless art-centres in early times, as there are now, and Cissbury would not appear to have been one of them, but rather belonged to the far larger class of village manufactories. Some of the pits, he wished to say as the result of personal observation, may have been opened but a short period before our era. Their age does not directly affect the question of the antiquity of man in this country, which depends for its solution on geological facts.

The President, Mr. Evans, in summing up, said the questions principally discussed were—In the first place were they to assign any implement found in this country to a pre-glacial or inter-glacial period? or must they restrict them to a post-glacial period? Some of the implements found in the river gravels were made from stones derived from glacial drift, and were therefore clearly post-glacial. The characteristic forms of the implements gave a guide by which they might fairly argue that others of a similar character belonged approximately to the same date. Some implements were very persistent in their type; but if in a certain part of England post-glacial implements were found associated with a certain fauna, and in another part the same forms of implements were found alone, these also would appear to be post-glacial. There were certain distinctions to be pointed out in cave-deposits. In the cave described by Prof. Dawkins there were a succession of beds, and he thought it was in the upper beds of more recent date that the relics of the tailor and the artist were found. Looking at the enormous lapse of time comprised in the palæolithic period, which was evidenced by the amount of time requisite for the erosion of river valleys, he thought they would eventually be able to establish some chronology. If they could form any idea of the amount of time requisite for the excavation of a valley such as the valley of the Thames, they could approximately estimate the antiquity of man in this country, but for the last 2,500 years the variation of the river bed and its level were practically nothing, and therefore they were entirely at a loss without falling back on some hypothesis as to variations in the climate. It was difficult to say with certainty whether the implements discovered abroad in reputed miocene and pliocene beds were of necessity worked by the hand of man, and whether they had in all cases been found under the circumstances which were attributed to them. With regard to the other deposits by which the early existence of man had been traced, such as the skull alluded to by Prof. Rolleston, if it was found with a highly-finished spear-head, he (the speaker) could not regard it as of pleistocene date. The evidence of cut bones was by no means satisfactory. Some of those incisions were probably induced by natural causes. Some present might remember a pair of horns of an Irish elk which by mere pressure were embedded in each other. Still, all such evidence should be carefully collected, and it would become to a certain extent accumulative. The question as to the distinction between the glacial period in the South of England and that of the North was of very great importance. If geologists carried back the early appearance of man in this country to a time but little removed from the glacial period, they might safely infer that he must have existed in other parts of Europe at a much earlier period.

As this interesting discussion could not well be postponed, and as the time at the disposal of the Conference was necessarily brief, it now only remained for the three principals to reply to any objections that may have been raised to their statements and arguments.

Prof. Boyd Dawkins said that the first point to be considered was the antiquity of man in the Victoria Cave, based upon a small fragment of fibula, and two fragments of goat's bones which presented the appearance of having been cut. The fibula seemed to him to be ursine rather than human, and in size came within a very little (two-tenths of an inch) of the circumference of one of *Ursus spelæus* from Lozère. With regard to the goat's bones, he shared the opinion of Mr. Davies, of the British Museum, that they are not fossil, but recent, in other words, he did not believe that they were originally imbedded in the stratum containing the remains of the hyænas, but were derived from an upper stratum of post-Roman age in the cave, in which they are exceedingly abundant. The goat hitherto has not been found in any pleistocene strata in this country or in France, all the repeated cases of its occurrence turning out on examination to be the result of the mixing of two suites of animal remains, the one pleistocene, and the other historic or pre-historic. This is very generally done by the workmen, and this was probably the case in the Victoria Cave. But if these equivocal data be assumed to prove that man was living in this district while hyænas occupied the cave, the evidence is still unsatisfactory as to their pre- or post-glacial age. The hyæna stratum itself appeared to him, while the explorations were under his direction, not to be of clearly defined pre- or inter-glacial age; and his doubts as to this point were, he believed, shared by Prof. Hughes. He further remarked that the reindeer found in the hyæna stratum had been omitted from Mr. Tiddeman's list of species. The rudeness of the palæolithic implements in the Cresswell caves from the lower strata as compared with the more highly finished ones found above them, seemed to him to imply a progress in the arts in that district.

A priori, the more highly finished should succeed the ruder implements, although of course many cases of their being mixed together were on record. Into the other avenues of discussion he would forbear to enter.

Prof. Hughes, in reply, said that he wished the subject had been divided, so that they might have considered separately the different parts of the evidence and the different sources of error which had still to be eliminated. For instance, he thought it would be very well if they could have an exhibition of, and discussion on, the various ways in which nature breaks, cuts, and otherwise marks bone and stone as well as of various ruder forms known to be the result of human agency, so as to get clearer ideas as to what might really be taken as evidence of design. He pointed out that the measure of the antiquity of the deposits containing the remains of man depended chiefly upon the time it would take to bring about certain geographical changes, either assuming that surrounding conditions had practically remained the same, or allowing for such differences as must have occurred, and of which we can estimate the effect. Applying this, while he agreed with Col. Lane Fox's remarks on the slow rate of waste of the Thames valley, he felt that we must make a very considerable allowance for the probability that during the period from the bronze age to our own man had interfered far more with the free course of the river than during all previous time. Nature might also entirely change the rate of waste in such a case by a gentle upheaval or depression causing the more rapid or slower cutting back of the stream. With regard to the existence of depressions in non-calcareous strata he thought we could detect two ways in which they were formed. One by the forcing out of the plastic material all round the mass of gravel or clay thrown on it, and another when the gravel worked down into the puddled surface of a clay, the softer portions of which oozed up between the sinking stones. In all the cases which had come under his observation in which such phenomena occurred above palæolithic beds, the last appeared to be the explanation, as also in most cases where it was the only evidence for the more southerly extension of glacial phenomena.

With regard to the Victoria Cave, he thought that the evidence was as yet decidedly against the pre-glacial age of any of the deposits containing even a suspicion of man. He believed that the deposits along the sides and in the side chambers of a limestone cave were frequently newer than those in the main cave, as the carbonated water, being thrown off the clay, must work the sides down. Whatever might be the age of the boulder clay on the floor at the mouth of the cave, he believed that the thin layer which occurred in the talus had fallen out of a pipe of which there were plenty in the limestone above, and that this clayey bank had ponded back the flood-waters and caused the accumulation of mud in the talus inside and the formation of the laminated clay.

Mr. Tiddeman[1] had thought it unnecessary on this occasion to call attention to geological minutiæ at the cave mouth, but as Prof. Hughes had raised the question of the age of the boulder clay there he was bound to follow him. Prof. Hughes said the boulder clay fell from the cliff at a time long subsequent to the date when the bones were deposited, but in drawing his section he had omitted a very important feature. They had to dig through twenty feet of talus before they came to the boulder clay, at the back of which was the hyæna bed; that represented a very considerable lapse of time since the boulders were deposited there. If the boulder clay fell at a subsequent period how was it that it was at the base of all the talus and not mixed up with it. If it fell before the talus began to form it might practically be considered of glacial age. As regards the reindeer in the lower bed, only one very doubtful specimen had been found since he had had charge of the excavations. The chief matter to be considered was whether this fauna which had been found in Europe and in England with human handiwork, occurred at a time which could be correlated with certain great physical events. All the facts which he had noticed seemed to harmonise with the idea that there had been in England two well-marked glacial periods, and these both prior to the much lesser event of the upper boulder clay of Lancashire. For instance, the boulders made into implements which Prof. Hughes had referred to in Pontnewydd Cave, need not have belonged to the latest glaciation of that country. Mr. Tiddeman would not dispute the fact of a fibula making its way down into the earth, especially after Prof. Rolleston's experience, but in the Victoria Cave it would have great difficulty. If it were soft mud it might have a chance of getting down, but if it were modern other modern things would go down with it unless it had a start. [Prof. Rolleston said it was pointed at both ends.] Mr. Tiddeman did not think there was a possibility of its working its way down. There were large blocks of stone and beds of stalagmite which had to be blasted in getting down to it. He hoped geologists would bear in mind as new facts cropped up, the suggestion that we had had two glacial periods.

[1] In the abstract of Mr. Tiddeman's paper, p. 70, line 40, *non-gravels* should be *river-gravels*; line 44, *then* post-glacial should be *there* post-glacial.

THE GREENWICH OBSERVATORY REPORT

THE Report of the Astronomer-Royal at the annual visitation on Saturday contained nothing extraordinary with respect to the ordinary work of the Observatory. With reference to extraneous work, there are one or two points worthy of notice.

First, as regards the operations for the transit of Venus, the Astronomer-Royal reports as follows:—

The computing staff under Capt. Tupman has by degrees been reduced to two junior computers within the Observatory; and one or two computers external to the Observatory, who are employed on large groups of systematic calculations, for which they are remunerated by tariff. The principal part of the calculations remaining at the last report was that applying to the determination of the geographical longitudes of fundamental stations. At the moment of my writing, the last of these (the longitude of Observatory Bay, Kerguelen) is not absolutely finished; but I trust that it will be so before my presentation of this report; and then I shall be in position to offer the first determination of correction to parallax from eye-observations of the transit.

The method of determining the geographical longitude of the principal station in each group by vertical transits of the moon has been found very successful at Honolulu and Rodriguez. For stations in high south latitude, horizontal transits are preferable; for Kerguelen, as I have mentioned, the work is not quite completed. (It will be remembered that the longitude of Mokattam, the principal Egyptian station, was determined by telegraph.) The corrections to the moon's tabular places have been determined with much care from meridional observations at the principal European observatories.

The differences of longitude, or the relations of clock-times, within the groups of stations, are ascertained.

These calculations must be followed by the preparation of the factors of errors of various elements. Little progress is made in these; the work will not be heavy.

No further advance is made in the photographic reductions. The work is large, but it is simple, and will not be oppressive.

Second, as regards the numerical lunar theory:—

In the algebraical theory an alteration has been made, by the substitution of the equation of radial forces for the equation of *vis viva*. Nearly all the numbers had been computed, and the additional numerical operation was small.

The numerical calculations of the factors of symbolical variations are advancing; and the computations of the perturbing side of the equations, with due attention to the terms requiring extension of decimals, are in progress.

The numerical errors to which I alluded in the last report are corrected; and I do not think that any systematic error now remains.

With the view of preserving, against the ordinary chances of destruction or abandonment, a work which is already one of considerable magnitude, I have prepared and have printed as Appendix to the Greenwich Observations (with additional copies as for a separate work) the ordinary equations of lunar disturbance, the novel theory of symbolical variations, and the numerical developments of the quantities on the first side of the equations. The last of these will ultimately require some additions for the terms whose magnitude is increased (in algebraical development).

The work is perhaps somewhat larger than I anticipated, and the regularity of its progress has been disturbed by very frequent interruptions of my own attention, occasioned chiefly by annoying occupations on the transit of Venus. I trust that it will in future go on in a more orderly and more rapid way.

Sir George Airy concludes his report with the following general remarks:—

The subject which, I think, must first present itself to the mind of anyone who has traced the history of the observatory is the increase in the number and the fulness of our occupations.

Of these one in particular (altazimuth-observations of the moon) has originated with myself; others, from the suggestions of the Board of Visitors, or from the obvious demands of the scientific world.

This increase is felt even in our buildings and grounds; every corner of every room is or will shortly be occupied; and the form of the ground almost forbids extension.

The printing of the steps of the reductions of observations (which originated with myself more than forty years ago) naturally increased the labour within the observatory, as well as the expenses without it. This printing, however, must never be abandoned. But there is another part, of which the policy still appears to me somewhat doubtful, namely, the printing *in extenso* of every figure of original observations, it being remarked that the originals or extracts are always open to astronomers. I brought the question of suppressing these before the Board of Visitors many years ago; but the opinions of astronomers (I cite in particular the honoured name of M. Biot and that of Mr. Johnson) were so strongly adverse to it, that I laid aside all further thoughts of it; and I do not even now profess to entertain a decided opinion.

The three points, however, to which I have alluded (the extent of scientific occupations, the enlargement of buildings, and the amount of printing) must before long engage the attention of the Visitors.

RECENT RESEARCHES AMONG THE LOWER SARCODE ORGANISMS

THE customary annual address on the occasion of the anniversary of the Linnean Society was, on Wednesday, the 24th May, delivered by the President, Prof. Allman, F.R.S. In continuation of his last year's summary of the progress in this department of biology, he dwelt upon the important additions to our knowledge of these organisms, due to the investigations of Archer in our own country and of Hertwig and Lesser, Franz Eilhard Schulze, and Greeff in Germany.

The discovery of many new monothalamic Rhizopods of fresh water and the important additions made by the British and German investigators to our knowledge of their protoplasmic bodies were brought in review before the meeting. These monothalamic forms may be divided in accordance with the nature of their pseudopodia; in some these processes being short, thick, and finger-shaped (*Lobosa*); in others long, slim, and filiform (*Filifera*). The former were illustrated by *Hyalosphenia*, with its smooth, transparent shell, and by *Quadrula*, with beautifully sculptured shell; and the latter by *Gromia*, with its very long filiform reticulated pseudopodia; and by *Microgromia socialis*, which has the curious habit of forming colonies by the association of numerous individuals, which become united to one another by the mutual fusion of their pseudopodia. The remarkable form of reproduction discovered by Hertwig in *Microgromia* was also described. Hertwig had shown that in this Rhizopod the protoplasm divides by spontaneous fission into two segments, one of which remains in the shell, while the other forces its way out, assumes an oval shape, develops, instead of pseudopodia, two vibratile flagella, and becomes a free-swimming flagellate Zoospore, capable of ultimate development into the form of the adult. The very interesting discovery by Haeckel, that the contents of the so-called "yellow cells" of the Radiolaria become of a deep violet colour under the action of iodine, and are therefore mainly composed of starch, was also referred to among recent additions to our knowledge of the lower organisms. An account was then given of the remarkable and very significant researches of Messrs. Dallinger and Drysdale among the so-called "Monads,"—microscopic organisms which become developed in putrifying solutions of organic matter, and which, in their ordinary and apparently adult state swim about by the aid of vibratile flagella. These laborious and trustworthy investigators have shown that the flagellate monads may acquire an amœboid condition and move about by the aid of pseudopodia; that two such amœboid forms when they come in contact with one another become instantly blended together at the point of contact, that this blending becomes more and more intimate until the two individuals become completely fused together, when their mingled protoplasm assumes the form of a spherical sac filled with particles of immeasurable minuteness. These particles are germs destined for the reproduction of the individual. Their form can be demonstrated only by the highest powers of the microscope; and by following them by means of a one-fiftieth of an inch object glass, Messrs. Dallinger and Drysdale were enabled to trace their gradual development into the form of the adult. They further proved the remarkable and unexpected fact that these minute germs may be subjected to a temperature of from 258° F. to 300° F. without losing their vitality and power of development, a fact of vast significance in its bearing on experiments connected with the question of spontaneous generation. Finally attention was drawn to the quite recent discovery of Hertwig and F. E. Schulze of a nucleus in the Foraminifera. By this discovery the true systematic position can now be assigned to the Foraminifera, which must accordingly be removed from the region of Cytodes or non-nucleated protoplasm masses (to which they had been hitherto relegated), and placed on a much higher stage in the great division of the Rhizopoda. Resting on these facts F. E. Schulze has attempted to represent by the aid of a genealogical tree the mutual affinities and derivation from one another of the various members of the Rhizopoda. The base of the tree where its stem is as yet undivided, consists of the primitive forms—mere non-nucleated Cytodes represented by Haeckel's Monera (*Protogenes*, *Protamœba*, &c.). From these by the differentiation of a nucleus in their protoplasm are evolved the nucleated forms (*Amœba*, fresh-water Monothalamia, Foraminifera, Heliozoa, &c.) which constitute the sub-divisions into which the stem branches off. These repeat the various modifications of pseudopodia (Lobose, Filiform, &c.) which had already existed in the primitive forms, and which they thus derive by inheritance from their non-nucleated progenitors. Finally through the branch of the Heliozoa we are conducted to the ultimate twigs formed by the families of the Radiolaria, in which we find not only nuclei but a "central capsule" indicating the highest grade of differentiation attained by any member of the group.

THE NORWEGIAN DEEP-SEA EXPEDITION

THE Norwegian Deep-Sea Expedition will have started from Bergen on its second summer cruise in the steamer *Bæringen*. It has been decided by the proper authorities that the expedition, like last year's, shall be commanded by Capt. Wille with Lieut. Petersen as first officer. The scientific staff of the expedition is also the same as the previous year with the exception of the chemist, whose post is this year filled by Herr H. Tornœ.

The following is the approved plan of the expedition of the present year.

The equipment of the vessel and the determination of its magnetic constants were to be completed by June 1. In studying the temperature in the deep sea over the banks off the West Coast, it has become evident that accurate observations are wanting in the Norwegian Rende. In order to obtain these the vessel will go from Bergen direct to sea, and following the bottom of the Rende, take accurate observations there. Farther to the north several of last year's observations may also be verified.

The first proper field of work is the Norwegian coast banks to the north of Ramdalen. From existing observations it is probable that the "Havbro," where the bank sinks toward the depths of the Polar Sea, and where the ice-cold water begins at the bottom, lies at least twenty-five geographical miles from the coast. Between Rœst and the point off Ramdalen, where the expedition last year found a depth of about sixty fathoms with a rocky bottom ten miles from land, it is considered probable that there runs a more or less continuous ridge of rock.

The position and characteristics of the "Havbro" and the supposed ridge form main points in the examination of the banks. This goes on by forming cross-sections perpendicular to the coast. The sections, like last year's, are to be at a distance of twelve or thirteen geographical miles asunder. Their inner boundary is to be the outermost line of the special hydrographical survey. Their outer boundary is where the temperature at the bottom of the sea is ÷ 1° C., or thereby. In each section besides the observation of the temperature at each sounding, at least three other series of observations are required, one at the inner boundary, one at the "Havbro," at its inner edge, and one at the outer limit of the section. The number of soundings will depend on the bottom being found more or less even as the work goes on.

In order to leave as much time as possible for work in the depths of the Polar Sea, and at Jan Mayen and the Greenland ice, there will be carried on, along with the survey of the banks, the examination of the Umbellularia region to a depth of 1,000 fathoms in every third cross-section. If circumstances permit

several other bank-sections may also be extended to this depth. While working in this region it will be proper to call at Bodoe.

A series of observations of temperature made in West Fiord with the newest deep-water thermometers will be carried out both during the voyage northwards in June and during the return voyage in August, in order to examine the abnormal state of things found there in the summer of 1875, the minimum temperature being at a depth of seventy fathoms. As the zoological surveys in West Fiord have hitherto only embraced the fauna of the coast, the opportunity will be sought to be utilised for zoological work in this fiord at a greater distance from land.

Magnetic absolute observations will be carried out at Rœst, where circumstances, from Lieut. Petersen's observations in 1875, appear favourable, and where the most projecting point is found for comparison with the proposed observations on the Greenland ice.

In the course of June it is supposed, with average weather, and with the experience gained last year, that the survey of the banks and of the Umbellularia Region according to this plan will have reached the latitude of Tromsœ.

The expedition will in the end of June or beginning of July, be equipped for a cruise to the westward, going first along the line Andœ, Jan Mayen, surveying it and its neighbourhood on both sides. Round the north-east end of Jan Mayen as a centre, several series of soundings are to be taken towards the north-east, the north, and the north-west. For in that neighbourhood where the outermost mountain of the Iceland volcanic region is supposed to be, the bottom appears to sink very rapidly towards the deep sea in the directions named.

Provided Jan Mayen can be reached, it is proposed to land on it for the purpose of undertaking an examination of its geography, geology, hydrography, zoology, botany, &c. It is also intended to make a survey of the sea on the west and south sides of Jan Mayen, for there is ground for supposing that Jan Mayen is connected with Iceland by a sub-oceanic ridge.

In order to examine the phenomena at the boundary between the warm surface current from the Atlantic and the cold polar current in the Greenland sea, the Greenland ice north-west of Jan Mayen will be visited. When the examination of a part of the Greenland ice is completed the course will be taken to a point about midway between Iceland and Jan Mayen, and the cross-section is to be examined from this point in the direction of Ranen with the view of discovering and exploring the supposed sub-oceanic ridge. When this cross-section is worked out to the Norwegian Umbellularia region, previously examined, the expedition will sail to Tromsœ to carry on work to the northward, if time permit.

As July is supposed to be devoted to the Jan Mayen cruise, the first half of August will be employed in a survey of the banks, the "Havbro," and part of the Polar Sea between Norway and South Spitzbergen. The eastern limit will be the line North Cape, Bear Island, South Cape. The latter half of August will be devoted to the return voyage to Bergen and dismantling. The scientific work will be carried out mainly in the same way as in 1876. In the zoological work, along with the use of the dredge, the trawl, and the swab, special weight is laid on the use of nets in the intermediate depths and fishing on the banks. On Jan Mayen the capture of birds ought to be an object of importance, and on the Greenland ice possibly seal and bear-hunting, &c.

In taking soundings the form and extent of the banks is to be determined, and the way in which the bottom falls off from these to the greatest depths of the polar sea. It is of fundamental importance for understanding the orography and geology of this sea and the neighbouring land and for the distribution of animal life, whether the bottom falls off towards the deep sea with an even slope or in terraces and escarpments with plateaus lying between. The position and number of the soundings is therefore to be determined with this view.

The measurements of the temperature in the deep sea are to be carried out to the extent necessary for a certain determination of the isotherms in the deep water of the cross-sections. It is also to be kept in view that the points in the cross-sections may be used for longitudinal sections. The newest deep-sea thermometers are to be used as frequently as possible, along with those employed last year.

At every sounding a specimen of water is to be taken from the bottom, and at chosen places at intermediate depths. The specific gravity of all specimens is to be ascertained. At every sounding, also, specimens of the bottom are to be taken and preserved for future examination, which is also to be done with the material of the bottom brought up by the dredge and trawl.

The chemical work is to be carried on mainly as last year. The specific gravity of the surface-water is to be determined once or twice a day, and oftener at places where the state of the currents or other circumstances render it desirable. Observations of the currents in the sea are to be carried out where circumstances make it desirable, and the weather permits.

Magnetic observations are to be carried out on board at sea, special weight being laid on obtaining declination observations, Absolute determinations are to be carried out at Rœst, and on the Greenland ice, besides the determination of the ship's magnetic constants at Husœ, and elsewhere where possible.

A geological survey is to be made of Jan Mayen, if a landing there is possible, to the extent circumstances may permit. It would be of great interest to carry out astronomical determinations of geographical position, topographical and hydrographical surveys on Jan Mayen. A determination of geographical position at Rœst, in connection with the magnetic observations, is also desirable, as the place is not connected with the trigonometrical net.

Botanical observations and collections are to be carrried out on Jan Mayen to the greatest possible extent.

The meteorological observations on board are to be carried out in all essential points as in 1876. H. MOHN.

NOTES

AMONG those on whom honours were conferred on the occasion of Her Majesty's birthday, was Dr. J. D. Hooker, President of the Royal Society and Director of the Royal Gardens, Kew. Dr. Hooker has been made a K.C.S.I.

THE twenty-sixth annual meeting of the American Association for the Advancement of Science commences at Nashville on August 29. Our own association meets at Plymouth this year on August 15, under the presidency of Prof. Allen Thomson, of Glasgow.

WE have received the programme of an Anthropological Exhibition to be held in connection with the Paris International Exhibition of 1878, under the superintendence of the Paris Anthropological Society. That Society has nominated a large Commission to organise the exhibition, with Prof. de Quatrefages as president. M. Krantz has placed at the disposal of the Society a spacious and fine position in the central pavilion of the Trocadero Palace. The Commission makes a warm appeal to all interested in the progress of the anthropological sciences, both in France and abroad. It wishes to prepare a complete inventory of the present state of these sciences. The following are the classes under which the exhibition will be arranged:—1. Crania and bones, mummies, and specimens relating to the comparative anatomy of the human races; 2. Instruments, methods of education; 3. Prehistoric and ethnological collections; 4. Photographs, paintings and drawings, sculpture, and modelling; 5. Geographical maps and tables relating to ethnology, prehistoric archæology, linguistic, demography, medical geography, &c.; 6. Books, journals, brochures. In order to facilitate the work of collection and arrangement, the Commission has appointed the following members to superintend the special departments named:—Dr. Broca, 1, rue des Saints-Pères, for what concerns Anthropological Societies; Dr. de Ranse, 4, place St. Michel, Anthropological Instruction; Dr. Topinard, 97, rue Rennes, General Anthropology and Craniology; M. Gabriel de Mortillet, Château de St. Germain-en-Laye (Seine-et-Oise), Prehistoric Archæology and Anthropology; M. Girard de Rialle, 64, rue de Clichy, Ethnography of Europe; M. Abel Hovelacque, 35, rue de l'Université, Linguistic Anthropology; Dr. Dureau, 16, rue de la Tour-d'Auvergne, Bibliography; Dr. Bertillon, 20, rue Monsieur-le-Prince, Demography, or the Statistical Study of Population, and Medical Geography; M. Louis Leguay, 3, rue de la Sainte-Chapelle, for all concerning the general management and arrangements. Each of the gentle-

men named should be communicated with in relation to all concerning his special department. The programme is a very inviting one, and if successfully carried out the result must be highly interesting and valuable. We hope British anthropologists will do all in their power to help the Commission to carry out their aims.

THE Maritime and Piscatorial Exhibition, which was opened at the Aquarium on Monday, contains a large collection of stuffed river fish—the largest collection, probably, ever brought together. Fourteen angling clubs, and many private individuals, have sent contributions. The Prince of Wales has also lent his collection of Indian fish obtained during his tour, and there is also a collection from the Indian Museum. As the exhibition includes, among other things, all subjects connected with fishing, there is a good show of fishing-tackle, and with them a fine set of flies. Those by Mr. Ogden Smith are quite works of art. Messrs. Sotheran and Co. have, at considerable trouble, made a collection of books on fish and pisciculture, which deserves attention.

A HUNGARIAN prelate, the Archbishop Louis of Haynald, has constructed an astronomical observatory at his own expense at Kalocsa, lat. 46° 31′, long. 16° 32′. Among the instruments are a Browning telescope, a small (4-inch) Merz refractor, and a Cooke transit instrument. The arrangement of the new observatory is superintended by M. de Konkoly, already known as having built on his own property, O-Gyalla, a well-furnished observatory. We may add that the Archbishop of Haynald has already devoted considerable sums to botanical researches.

THE *conversazione* of the Society of Arts is fixed to take place at the South Kensington Museum, on Wednesday, June 27.

THE Commission appointed by the U.S. Government to examine Capt. Howgate's proposal for the establishment of an exploring colony within 400 miles of the North Pole recommend that 50,000 dollars be granted for the purpose.

AT the meeting of the Zoological Society of France on June 1, M. Perier, Professor of Conchology at the Paris Museum, explained that the number of specimens was far too large to be properly exhibited in the galleries, and that consequently it had been resolved to exhibit only specimens of each genus, and to have the types of species arranged systematically in drawers. Each species is to be entered in an alphabetical and systematic catalogue, so carefully compiled that the information it contains may be got at instantly. Any visitor wanting to inspect a particular species will have only to make an application to the galleries for conchology. The work is immense, but it is supposed that in ten years it will be completed.

THE French Society of "Amis des Sciences," instituted twelve years ago for distributing pensions to the families of deceased men of science or to *savants* themselves when incapacitated by old age, has held its anniversary meeting under the presidency of Prof. Berthelot. The report was read by M. Pasteur. It shows that the assets of the Society amount to 19,000*l*. The sum spent in pensions was 1,200*l*. last year. An *éloge* of M. Charles Sainte Claire Deville was read by M. Fouquier, his successor as professor at the Collége de France.

WE have received the yearly report for 1876-7 of the new Scientific Club of Vienna, and its perusal is well calculated to fill a cultured Londoner of moderate means with envy. For an entry money of five florins and a yearly contribution (payable quarterly !) of sixteen florins all the advantages of a good London club can be obtained combined with those of the Royal Institution. The club possesses a spacious building with lecture-halls, reading and conversation-rooms for smokers and non-smokers, writing-rooms, refreshment-rooms, splendid library, all the best journals of every kind from all parts of the world. During the greater part of the year there are scientific and other lectures, entertainments and receptions, excursions during summer, and it is contemplated to publish a weekly journal connected with the affairs of the club. The members, numbering already upwards of 500, belong to all classes of society and to all professions, their only bond of union being a desire for cultured intercourse. The only institution in London at all approaching to this Vienna club is the modest German Athenæum.

UNDER the care of Dr. P. P. C. Hoek there has been published in German a *catalogue raisonné* of zoological works and papers that have appeared in the Netherlands during 1875-6. There are in all eighty-seven titles. The title is "Die Zoologie in den Niederlanden," and the publishers are E. J. Brill, of Leiden, and C. F. Winter, Leipzig.

INTELLIGENCE received at New York, June 1, from the Sandwich Islands announces that simultaneously with the earthquake at Iquique, Peru, a tidal wave struck the group of islands on May 10, between 4 and 5 A.M. The sea suddenly receded and returned with great violence in a wave sixteen feet high, which entered the harbour at Hilo, and swept away the wharves and storehouses in the front part of the town. All the houses at Waiakin within 100 yards of the shore were destroyed. Five persons were drowned; many were picked up in the harbour. The earthquake undulations continued during the day, the difference between the highest and lowest water-mark varying from three feet to thirty-six feet in various parts of the islands. Cocoanut (*sic*, but probably Molokai) island was entirely submerged, and the hospital at that place was swept away. A fresh eruption of the Kilauea volcano commenced simultaneously with this oceanic disturbance. The same earthquake wave was also felt all along the Mexican Pacific Coast. Late particulars announce that great devastation and loss of life were caused by the tidal wave which swept the Peruvian coast. Six hundred persons are reported to have perished.

IN connection with the above the following is of interest:—A Press despatch from Washington, of May 12, states that Assistant-George Davidson telegraphs to C. P. Patterson, Superintendent of the Coast Survey, in relation to the earthquake waves registered in the tide-gauge at Fort Point, at the entrance of San Francisco Harbour, to the following effect:—"Sharp earthquake waves commenced on Thursday, May 10, at 6.18 a.m., five rises and falls of 9 in. in 80 minutes; then nine *maxima* or crests 48 minutes apart, with secondary *maxima*, the largest of which were 15 in.; then six sharp rises of 14 in. each to irregular broken crests one hour apart; then to 5 A.M. Friday, double *maxima* as at the commencement, the largest rise being 18 in. From 5.20 A.M. Friday, to 1.15 P.M., irregular *maxima*, then a sudden fall of 16 in., and action, continuing until 5 P.M. No well-defined *maximum* of action, but exhibitions of markedly different character. It may be stated that the earthquake wave of the great earthquake in Japan some years ago was twenty-three minutes in traversing the Pacific to San Francisco."

ON Monday afternoon, about 3.30, a tornado partially destroyed Mount Carmel, a flourishing town of 3,000 inhabitants on the Wabash River, South-Eastern Illinois. It struck the town on the southern quarter and passed north, destroying almost everything in its path. Three churches, two newspaper offices, two schools, the Court-house, and 120 buildings were demolished. The ruins caught fire, burning almost till midnight before the flames were extinguished. Many persons were killed or injured.

ON the vote in Parliament last Thursday to complete the sum of 24,569*l*. for the Patent Office, Dr. Playfair remarked that while the Government derived a revenue of 180,000*l*. from the in-

ventors of this country, it made them a very insignificant return. Only about 2,000*l.* was spent upon a Patent Museum. The Patent Museum at South Kensington was very inferior to the corresponding institutions in France and America, and if we had such a museum at all it ought to be a good one, and such as would promote invention. He wished to ask whether the Government had bought or rented a large building for the purposes of an efficient Patent Museum? Mr. W. H. Smith explained that the space intended for the Patent Museum at South Kensington had been taken up by the collection of scientific apparatus, but that the Government still intended to arrange for a suitable exhibition of inventions.

A NOTE has been circulated in German papers, warning parents against the use of india-rubber toys manufactured in France and sold largely on the north of the Rhine. They were said to be poisonous owing to a certain quantity of oxide of zinc which was mixed with vulcanised india-rubber. The French Minister of Agriculture and Commerce ordered an inquiry to be made by the Council for Public Health, and the result is a declaration that these objects are quite harmless.

ACCORDING to recent news received by M. Sidoroff as to the mammoth discovered in Siberia, the carcass was found in the gold-bearing sands of a gold-washing on the river Kundola, at a depth of five mètres. The flesh was very soft and of a light red colour when freshly dug out, but soon hardened, becoming like a white clay. It seems to be much impregnated with lime. The digging out of the whole body will be very difficult because of the access of water. M. Polyakoff has already left St. Petersburg for the excavation of the body.

A TASHKEND telegram, dated May 10, announces that the Russian embassy, sent last spring to Kurl, has made a thorough survey of the route it followed, together with numerous barometrical measurements of heights. Capt. Kuropatkin and the naturalist M. Wilken, have explored the lake Bastan-nor and the route to Karashar, twenty-seven miles east of Kurl, whilst Lieut. Senorguroff has explored and surveyed the route from Aksu to Karakol, following the Babel pass.

AT the last meeting, May 16, of the Russian Geographical Society, it was agreed to postpone the exploration of the Angara river until next year, whilst the exploration of the water-parting between the Obi and the Yenissei will be made during this summer. Besides, M. Sonirnof undertakes a tour in Russia for magnetical measurements.

WE learn from the *Gardeners' Chronicle* that Prof. Boulger, the Professor of Natural History at the Royal Agricultural College, Cirencester, is preparing a "Flora" of Gloucestershire, and will be glad of any information as to rare species, localities, &c.

A COMMITTEE has been formed having for its object the presentation of a testimonial to Mr. F. W. Wilson, whose connection with the Crystal Palace, after twenty-five years' service, has just terminated. During the past quarter of a century Mr. Wilson, by his genial disposition, obliging manners, and thoroughly zealous efforts to promote and successfully carry out the various interesting natural history and other exhibitions held at the Crystal Palace, has earned the respect both of his colleagues and the general public.

OUR Paris correspondent writes: We were present at the Alliance Electric Works to witness an experiment on the new caolin-light, which we had seen in operation at the Physical Society, and which was recently described in NATURE. The Ruhmkorf machine was fed by a more powerful primary current given out by an Alliance electro-motor of three-horse power. The bar of caolin, ignited by induction-spark, was eight centimetres long, and fully equal to eight gas-burners. At the same time three so-called electric candles were in operation, each of them giving about forty gas-burners, and fully equal to five caolin lights. Consequently not less than twenty-five lights could be fed at once in separate parts of the same building or at any distance. The light is admirable for its constancy and duration; the quantity of caolin destroyed is quite insignificant—no more than 1 mm. per hour; the thickness of the caolin plate or bar used is not more than 4 millimetres. The electric candles are formed, as is known, by two graphite bars separated by a caolin bar. It was proved that the agitation of the candlestick produces no interruption of the current. Consequently a single Alliance-work machine can feed the three lights required by the regulations for steamers. The graphite pencils are consumed at the rate of eight centimetres per hour, which is a difficulty; but it is expected that any length required may be supplied by means of a proper clock-work. Experiments were tried on the same day at the Palais de l'Industrie for the purpose of illuminating the exhibition of pictures with electric light. The ordinary Gramme machines were set into operation. The light was found steady, but the effect was not quite favourable artistically. It is expected that with the system of light division, an improvement may be effected. The experiments were made in order to test whether electric light may be utilised on the occasion of the forthcoming International Exhibition.

IN connection with the above we may state that the experiment with Jablochkoff's electric light at the West India Docks on Tuesday night, was not quite successful, owing to some part of the apparatus going wrong. The experiment is, however, we believe, to be repeated.

THE destruction of agricultural and garden produce by the systematic attacks of insects is yearly becoming more and more serious. The gravity of this subject has been felt of late years not only by the growers of food crops themselves, but by scientific men, especially entomologists, and it is therefore with satisfaction we notice that some steps have been taken to obtain accurate records of the habits and conditions most favourable to the development and increase of certain well-known insects that devastate our crops. A little pamphlet called "Notes for Observations of Injurious Insects" has recently been issued by Mr. T. P. Newman, of 32, Botolph Lane, E.C. In these notes the necessity of such observations is pointed out, not only on scientific grounds, but also with a view to diminish the yearly losses of food crops to the country. The "Notes" are illustrated with woodcuts of many well-known insect depredators as a guide to their identity, and a ruled sheet is furnished for entering the records and any remarks that may appear desirable. These, as well as the "Notes" themselves, are to be obtained *free* on application to the printer, as above, and any information required will be furnished by the Rev. T. A. Preston, the Green, Marlborough, or by Mr. E. A. Fitch, Maldon, Essex.

MR. OSCAR BROWING, in lecturing on Friday evening last at the Royal Institution on the history of education, drew attention to the science of teaching being in advance of the art. He urged that, like medicine, pædogogics should be made a science of observation on results obtained. Teachers should interchange experiences on results as well as on theory.

THE additions to the Zoological Society's Gardens during the past week include a Mesopotamian Fallow Deer (*Cervus mesopotamicus*), an African Leopard (*Felis pardus*), a Cheetah (*Felis jubata*) from South Africa, a Humboldt's Lagothrix (*Lagothrix humboldti*) from South America, deposited; a Prevost's Squirrel (*Sciurus prevosti*) from Malacca, purchased; a Black-backed Jackal (*Canis mesomeles*) from South Africa, presented by the Earl of Guildford.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE, May 30.—The Sheepshanks Astronomical Exhibition has been adjudged to John Edward Aloysius Steggall, scholar of Trinity College.

The twenty-second annual report of the Botanic Garden Syndicate has been issued. It is stated that during the past year much attention has been paid to the labelling of the arranged trees, shrubs, and herbaceous plants in the open ground, and it is believed that they all, or nearly all, are correctly and legibly named. There has not been time, without neglecting other important work, to name with similar completeness the plants scattered in the belt. It is believed that very few of the plants in the houses are without names, although a few duplicate specimens may be in that condition. About 1,700 new labels have been written. The culture of the plants is such as to give satisfaction to the syndicate. About 170 species of herbaceous plants have been raised from seed to supply the places of those which have died. Among the presents acknowledged are packets of seeds from the Indian Botanic Gardens, from Baron F. v. Mueller, and five large ferns from Australia from the last-mentioned gentleman.

OXFORD.—At the ensuing commemoration the honorary degree of D.C.L. will be conferred upon Mr. J. Evans, the distinguished antiquary; Dr. Harold Browne, the Bishop of Winchester; and Lord Coleridge. It is probable that degrees will be conferred on certain other distinguished persons, whose names, however, it would be premature to announce at present.

On November 23 next there will be an election to a Brackenbury Natural Science Scholarship at Balliol College, worth 80*l.* a year, tenable during residence for four years, open to all such candidates as shall not have exceeded eight terms from matriculation. Papers will be set in (1) Mechanical Philosophy and Physics, (2) Chemistry, (3) Physiology; but candidates will not be expected to offer in more than two of these.

DURHAM.—The University Mathematical Scholarship has been awarded to Mr. F. W. Sanderson, Hatfield Hall.

THE UNIVERSITIES BILL.—The attempt was made twice on Monday in the House of Commons to get a clause inserted in the Universities Bill abolishing Clerical Fellowships. As might have been expected, the attempt failed, though in the case of Mr. Goschen's motion by a very narrow majority—only 9.

UNIVERSITIES AND NATIONAL LIFE.—The following forcible remarks on universities and national life occur in the address of Prof. Sylvester at the Johns Hopkins University, to which we have already referred:—"The mention of Germany brings to my mind the importance of universities to the maintenance or development of a national spirit in the countries in which they are fostered and carried on with an animus free from local or sectarian prejudices. I think that there can be little doubt that the greatest fact in modern history, the consolidation of the German empire, the resurrection of the German people, is mainly to be attributed to the feeling of brotherhood and the spirit of nationality kept alive in those ganglions of thought, those centres of intellectual activity, the German universities. It is the university professors who have made German unity a possibility, and I cannot but deplore the unpatriotic short-sightedness of those in my own country who, until so late a period, have struggled, and still covertly struggle, to make our universities in England not the representatives of the universal English mind, but the monopoly of a party and the appanage of a sect."

SCIENTIFIC SERIALS

American Journal of Science and Arts, May.—On vortex rings in liquids, by J. Trowbridge.—An account of the discoveries in Vermont Geology of the Rev. Augustus Wing, by J. D. Dana.—Notes on the history of *Helianthus tuberosus*, the so-called Jerusalem artichoke, by J. H. Trumbull and Asa Gray.—A new investigation of one of the laws of friction, by A. S. Kimball.—Examination of American columbic acid minerals, by J. Lawrence Smith.—On the sensitiveness to light of various salts of silver, by M. Carey Lea.

Poggendorff's Annalen der Physik und Chemie, No. 3.—On the cohesion of salt solutions, by G. Quincke.—On the theory of stationary electric flow in curved surfaces, by A. Topler.—On normal magnetisation, by M. Petruscheffsky.—On the temperature in the conducting wire of a galvanic current, by M. Streintz.—Remarks on a statement of F. Kohlrausch on thermo-electricity, by M. Clausius.—On the galvanic resistance of haloid compounds, by M. Lenz.—On the dynamical significance of the quantities occurring in the mechanical theory of heat, by M. Szily.—On a paradox of the mechanical theory of heat, by M. Ritter.—Researches on the movements of radiating and irradiated bodies (concluded), by M. Zöllner.—On the connection between absorption and dispersion, by M. Ketteler.—On the neutral combs of the Holtz machine, by M. Riess.—Galvanic dipping battery for elements with two liquids, by M. Hertz.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti, vol. x. fasc. iv. v. vi.—Observations on Borrelly's comet, by M. Schiaparelli.—Ditto by P. Secchi.—On a singular congenital and lipomatous pigmentary alteration, by M. Scarenzio.—Contribution to the study of Addison's disease, by M. Valsuani.—New barometric formula for the measurement of altitudes, and the reduction of barometric heights to the sea-level, by M. Grassi.—General method of obtaining diagrams of the motion of a point, by M. Padelletti.—On algebraic differential equations of the first order and first degree, by M. Pincherle.—On some questions of electrostatics, by M. Beltrami.—On some unpublished letters from Lagrange to Euler, by M. Schiaparelli.—Origin and anatomy of intestinal diverticula, and their application in practical surgery, by M. Sangalli.—On a new species of Dochmius (*Dochmius balsami*), by MM. Parona and Grassi.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 31.—"On the Amplitude of Sound-Waves," by Lord Rayleigh, M.A., F.R.S.

Scarcely any attempts have been made, so far as I am aware, to measure the actual amplitude of sound-bearing waves, and indeed the problem is one of considerable difficulty. Even if the measurement could be effected, the result would have reference only to the waves actually experimented upon, and would be of no great value in the absence of some means of defining the intensity of the corresponding sound. It is bad policy, however, to despise quantitative estimates because they are rough, and in the present case it is for many reasons desirable to have a general idea of the magnitudes of the quantities with which we have to deal. Now it is evident that a superior limit to the amplitude of waves giving an audible sound may be arrived at from a knowledge of the energy which must be expended in a given time in order to generate them, and of the extent of surface over which the waves so generated are spread at the time of hearing. An estimate founded on these data will necessarily be too high, both because sound-waves must suffer some dissipation in their progress, and also because a part, and in some cases a large part, of the energy expended never takes the form of sound-waves at all.

The source of sound in my experiment was a whistle, mounted on a Wolf's bottle, in connection with which was a syphon manometer, for the purpose of measuring the pressure of wind. This apparatus was blown from the lungs through an india-rubber tube, and with a little practice there was no difficulty in maintaining a sufficiently constant blast of the requisite duration. The most suitable pressure was determined by preliminary trials, and was measured by a column of water 9½ centimetres high.

The first point to be determined was the distance from the source to which the sound remained clearly audible. The experiment was tried in the middle of a fine still winter's day, and it was ascertained that the whistle was heard without effort at a distance of 820 metres. In order to guard against any effect of wind, the precaution was taken of repeating the observation with the direction of propagation reversed, but without any difference being observable.

The only remaining datum necessary for the calculation is the quantity of air which passes through the whistle in a given time. This was determined by a laboratory experiment. The india-rubber tube was put into connection with the interior of a rather large bell-glass open at the bottom, and this was pressed gradually down into a large vessel of water in such a manner that the manometer indicated a steady pressure of 9½ centimetres. The capacity of the bell-glass was 5,200 cubic centimetres, and it was found that the supply of air was sufficient to last 26½ seconds of time. The consumption of air was therefore 196 cubic centimetres per second.

In working out the result it will be most convenient to use consistently the C. G. S. system. On this system of measurement the pressure employed was $9\frac{1}{2} \times 981$ degrees per square centimetre, and therefore the work expended per second in generating the waves was $196 \times 9\frac{1}{2} \times 981$ ergs. Now the mechanical value of a series of progressive waves is the same as the kinetic energy of the whole mass of air concerned, supposed to be moving with the maximum velocity of vibration (v); so that, if S denotes the area of the wave-front considered, a be the velocity of sound, and ρ be the density of air, the mechanical value of the waves passing in a unit of time is expressed by $\frac{1}{2} S . a . \rho . v^2$, in which the numerical value of a is about 34,100, and that of ρ about ·0013. In the present application S is the area of the surface of a hemisphere, whose radius is 82,000 centimetres; and thus, if the whole energy of the escaping air were converted into sound, and there were no dissipation on the way, the value of v at the distance of 82,000 centimetres would be given by the equation—

$$v^2 = \frac{2 \times 196 \times 9\frac{1}{2} \times 981}{2\pi(82000)^2 \times 34100 \times \cdot0013},$$

whence

$$v = \cdot0014 \text{ centimetres per second.}$$

This result does not require a knowledge of the pitch of the sound. If the period be τ, the relation between the maximum excursion x and the maximum velocity v is

$$x = \frac{v\tau}{2\pi}.$$

In the present case the note of the whistle was f^{iv}, with a frequency of about 2730. Hence

$$x = \frac{\cdot0014}{2\pi \times 2730} = 10^{-8} \times 8\cdot1,$$

or the amplitude of the aerial particles was less than a ten millionth of a centimetre.

I am inclined to think that on a still night a sound of this pitch, whose amplitude is only a hundred millionth of a centimetre, would still be audible.

Linnean Society, May 24.—Annual General Meeting.—Prof. Allman, F.R.S., president, in the chair.—The Senior Secretary (Mr. Currey) read his report, among other items, mentioning that twelve fellows and five foreign members had died during the past year. On the other hand forty-three fellows, three foreign members, and one associate had been elected. Of active scientific workers that had passed away, J. Scott Bowerbank, Edward Newman, and Alfred Smee called for special mention; the labours of the first named, in a previously little-worked department, the sponges, marking an epoch in British natural history. A passing tribute was due to the memory of the foreign members that had died during the year, for von Baer, Braun, De Notaris, Ehrenberg, and Hofmeister in their several departments worthily represented biological science in its broad aspects.—Mr. Gwyn Jeffreys, treasurer, in his financial statement, showed an increased balance in favour of the Society, and this, notwithstanding extra outlay in valuable additions to the library, improvements in the Society's scientific publications, &c. The demise of the late Charles Lambert, F.L.S., had brought the handsome bequest of 500*l.* to the funds of the Society.—The president and officers were re-elected and the following gentlemen, viz., Lieut.-Col. Grant, C.B., W. Carruthers, R. Hudson, Dr. J. Millar, and Dr. R. C. A. Prior were elected into council in lieu of the subjoined, who retired by rotation:—G. Bentham, Gen. Scott, C.B., R. B. Sharpe, H. T. Stainton, and C. Stewart. We elsewhere give an abstract of the presidential address, devoted to a *résumé* on "Recent Researches among the Lower Sarcode Organisms," a subject of daily increasing interest.

Meteorological Society, May 16.—Mr. H. S. Eaton, M.A., president, in the chair.—Messrs. Stephen Bretton, J. Gulson Burgess, David Milne Home, and F. Gartside Tippinge were elected fellows of the Society.—The following papers were read:—An improved form of mercurial barometer, by Mr. R. E. Power, F.M.S. The improvement consists in the use of a double column of mercury, so that in the event of a vacuum being formed by the escape of some mercury into the cistern, the shock is no longer felt by the tube but in the first place is received by the mercury alone and then reflected much diminished into the cistern, where it is modified by the presence of the atmosphere. At the same time, owing to the peculiar construction of the cistern, the probability of any mercury leaving the tubes is much less than in the case of the standard barometers at present in use. It is also believed that the employment of the double tube will do away with the necessity of boiling the mercury.—The relation between the upper and under currents of the atmosphere around areas of barometric depression, by the Rev. W. Clement Ley, F.M.S. This paper gives a description of the mean directions of the movements of cirrus clouds over the different segments of areas of depression. The subject is treated on its observational side, as it is not yet considered ripe for much theoretical discussion, but one or two points seem likely to throw some light on the theory of the movements of the atmosphere. The direction of the upper currents round a depression is found to be most intimately related to the direction in which the depression itself is progressing. In the rear of a depression where the mean direction of the surface winds is nearly parallel to the isobars, or at right angles to the radius, the cirrus current almost coincides with the surface wind, except near the central calm. In the front of the depression on the other hand, where there is the greatest indraught near the earth's surface, the upper currents flow greatly away from the centre. The current in the rear of a depression has therefore the greatest, and that in the front the least vertical depth. The majority of our depressions travel towards some point between N. and E., and so far as the author has been able to calculate, the mean height of our south-easterly winds is not half that of our north-westerly winds, even supposing the latter to extend no higher than the stratum of cirrus. The angle of deviation increases with the increase of friction; and it is possible, therefore, that the great incurvation of the surface winds in the front of a depression is closely related to the greater amount of friction which they encounter, for they are comparatively shallow currents, and experience resistance above as well as at their base. The contrast between the upper currents on the right and those on the left of the trajectory is quite as remarkable as that between the upper currents in the front and in the rear, and it is very constant and well-marked in its general character. As regards the centre, the upper-current, when traceable over this district, commonly coincides, or very nearly, with the wind previously felt at the earth's surface.—Contributions to the meteorology of the Pacific—the Island of Rapa, by Robert H. Scott, F.R.S. Rapa is a small island, eighteen miles in circumference, in the South Pacific, in latitude 27° S. and longitude 144° W. The observations were made by Capt. D. E. Mackellar, on board the depôt ship *Medas*, during the period extending from 1867, December 15, to 1869, May 27. The climate appears to be an equable one.

Physical Society, May 26.—Prof. G. C. Foster, president, in the chair.—The following were elected members of the Society:—Lieut-Col. A. C. Campbell, Dr. H. Debris, F.R.S., Mr. W. T. Thiselton Dyer, M.A., B.Sc., W. Jack, M.A., and Capt. Sale, R.E.—Lieut.-Colonel Campbell explained and exhibited a double slit which he has employed for measuring the distances between the lines in the spectrum and finds of great service in cases where the illumination is so slight as to preclude the possibility of using the ordinary micrometer. One slit remaining stationary the other can be moved at right angles to its direction by means of a very delicate micrometer screw of 200 threads to the inch, the graduated head of which is capable of distinctly indicating one-five-millionth of an inch in the motion of the slit. If now a reading of the micrometer be taken when the slits are superposed and form one continuous slit, and a second reading when any given line has been superposed upon any other line at a moderate distance from it, the difference between these readings will enable us at once to ascertain the distance between the lines if the micrometer be calibrated in terms of the spectrum as seen in the observing telescope. The author has made several measurements with this apparatus, and finds it to be capable of extreme accuracy, but it is of course essential that the movable slit remains within a moderate distance of the axis of the collimator. He then described a simple arrangement for automatically fixing a prism, when placed on the table of a goniometer at the angle of minimum deviation when different coloured rays are under examination. To the arms which support the telescopes of the goniometer are attached two short links of equal lengths connected at their extremities with a nut sliding freely on an arm which is fixed radially to the centre table of the instrument. The prism is held on this table with its base at right angles to this arm, and it thus remains adjusted for all the rays of the spectrum.—Mr. O. J. Lodge then read two papers by Profs. Ayrton and Perry, jointly,

of the Imperial College of Engineering, Japan. The first contains an account of an elaborate series of experiments on ice as an electrolyte. They state as a result of their experiments that the capacity per cub. cent. of ice at $-13°{\cdot}5$ C. is $0{\cdot}002$ micro-farad, and the specific inductive capacity is 22,160 (that of air being called unity), while that of water at $8°{\cdot}7$ C. is about 2,240 times this amount. Commencing with ice at $-13°{\cdot}6$ C the temperature was allowed to rise and the conductivity determined by galvanometer readings. From these a very regular curve was deduced which shows that the conductivity increases regularly, and that there is no sudden rise in passing from the solid to the liquid state. The apparatus was also employed for determining the electromotive force of polarisation currents at different temperatures by replacing the copper by a zinc disc.—The second communication contained suggestions for experiments on the viscosity of water and other liquids. It is accompanied by working drawings of an apparatus which the authors have designed for determining the relation between the viscosity of a liquid and the velocity of a surface moving in contact with it. They have, however, no facilities for making such an apparatus, and therefore place it at the service of any one who may be willing to study the subject.

Victoria Institute, June 4.—At the Annual Meeting of this Institute, the address was delivered by Mr. J. E. Howard, F.R.S.—Capt. F. Petrie (the honorary secretary) read the eleventh annual report; 107 members and associates had joined during the year, and the total number had risen to over 700, two-thirds of whom were country and foreign members.

Institution of Civil Engineers, May 29.—Mr. George Robert Stephenson, president, in the chair.—A paper was read on an economical method of manufacturing charcoal for gunpowder, by Mr. George Haycraft, F.C.S.

PARIS

Academy of Sciences, May 28, M. Peligot in the chair.—Some remarks were made on M. Roudaire's Algerian scheme. M. de Lesseps thought it practicable and useful; M. d'Abbadie desired that study might be given for a whole year to the quantity of evaporation and the *régime* of the winds in that region, &c.—The following papers were read:—Reply to M. Tacchini's note inserted in last *Comptes Rendus*, by M. Janssen. While accepting M. Tacchini's figures in comparison of maximum and minimum years, he yet holds that the numerous and rapid appearance and disappearance of spots witnessed during the past year indicates very violent movements of matter.—On Gay Lussac's law of volumes, reply to M. Saint Claire Deville, by M. Wurtz. The system of chemical equivalents which prevailed about 1840 over the atomic notation of Berzelius, has not taken account of Gay Lussac's discoveries on the combinations of gases, and the maintenance of the principle of equivalence in chemical notation would bring science back to the times of Dalton, Wollaston, and Richter, which would be an anachronism.—Reply to M. Wurtz's note on the law of Avogadro and the atomic theory, by M. Berthelot.—Experimental critique on the glycogenetic function of the liver, by M. Cl. Bernard. He proves the function directly during life, laying bare the liver in a dog, cutting off a piece of it, which is then put into boiling water. The tissue contains a proportion of sugar varying from 1 to 3 per 1,000. The influence of vivisection does not cause variation in the quantity, unless the circulatory and respiratory functions are greatly disturbed. M. Bernard also demonstrates that the saccharine matter continues to be formed in the liver after death.—Observations on the work presented to the Academy by M. Villarceau, entitled the "Nouvelle Navigation," by M. Mouchez. He objects to the title; and the analytic method proposed (application of Taylor's series in place of the old formula) though good in theory, is found impracticable. The graphic method is nearly the same, according to M. Mouchez, as he has himself long practised and recommended. Lastly there is but rare need to use M. Villarceau's new process for determining *the most probable point*.—On an algebraic method for obtaining the *ensemble* of the fundamental invariants and covariants of a binary form, and of any combination of binary forms (continued), by Prof. Sylvester.—Description of new manœuvres executed with the economising apparatus at the dam of Aubois, by M. de Caligny.—Experiments made in order to appreciate the diffusion of the vapours of sulphide of carbon introduced into the ground as an insecticide, by M. Gastine. In permeable soil the diffusion reached a maximum radius of about 1 metre about the hole of injection (which received 20 grammes of the sulphide). The vapours persisted at 30 cm. distance from March 1 to 5, or about 100 hours; nearer the hole, 150 hours. In clayey soil the diffusion was as extensive, and the persistence was about twenty-four hours more.—Historical remarks on the theory of movement of one or several bodies of constant or variable forms, in an incompressible fluid; on the apparent resultant forces and on the experiments relating thereto, by M. Bjerknes.—On Gauss's formula of quadrature, by M. Callandreau.—Thermal researches on the substituted anilines, by M. Longuinine.—Electrolysis of ordinary pyrotartaric acid, by MM. Reboul and Bourgoin. This acid is very stable; it is electrolysed like mineral acids, and is in this respect quite unlike succinic acid, which is decomposed easily.—Researches on the synthesis of acids of the series $C_nH_{2n-2}O_2$ and $C_nH_{2n-4}O_2$; allylic and diallylacetic acids, by M. Reboul.—On the decomposition of carbonic acid in the solar spectrum by the green parts of plants, by M. Timiriazeff. A spectrum was formed with a bisulphide of carbon prism and a trough containing chlorophyll solution was put in the path of the rays. Next were interposed in a row five vessels inverted over mercury, each containing air with about 5 per cent. carbonic acid, and these vessels received green organs of plants (pieces cut from a bamboo leaf). The vessels, being in different parts of the spectrum, were left there six to ten hours on fine days in July, and the gas was afterwards analysed. The maximum of decomposition of CO_2 was always found in the vessel corresponding to the position of the characteristic absorption band of chlorophyll; in orange, yellow, and green, the amount of decomposition showed successive decrease, and in red there was even production of CO_2 through respiration.—On the nature and signification of the small red corpuscles of the blood, by M. Hayem. He concludes that they are young corpuscles incompletely developed.—On the changes of volume, and the delivery of the heart, by M. François-Franck. His method was to connect the cavity of the pericardium (in live dogs) with one of Marey's registering apparatuses. The heart increasing in volume in diastole drove a certain quantity of air into the tambour, while the contraction in systole permitted return of this air.—On the histological alterations of the uterus in their relations to the principal diseases of this organ, by M. Courty.—Statistical Researches on the Sologne, especially with regard to recruiting and movement of the population, by M. Coste.—On a process for recognising the presence of fuchsine in wines, by M. Baudrimont. A drop of fuchsinated wine left a few seconds on the skin of the hand, produces a mark which cannot be washed out with water.

GÖTTINGEN

Royal Academy of Sciences, January 31.—Report on the Botanical Institute of Göttingen University for 1876.

February 21.—A contribution to the theory of reflection-phenomena, by M. Rethy.

March 21. Separation of arsenic from nickel and cobalt.

April 4.—On the electric conductivity of aqueous solutions, especially of salts of the alkalies and alkaline earths, caustic alkalies, and some acids, by M. Kohlrausch.

CONTENTS