THURSDAY, JUNE 5, 1884

THE ORIGIN OF THE CRYSTALLINE SCHISTS

Untersuchungen über die Entstehung der Altkrystallinischen Schiefergesteine mit besonderer Bezugnahme auf das Sächsische Granulitgebirge, &c.," von Dr. Johannes Lehmann. (Bonn: Hochgürtel, 1884.)

NO problem in modern geology stands out with such prominence as the origin of that remarkable group of rocks to which the name of the Crystalline Schists has been given, and to none in recent years has so large a share of the literature of the science been devoted. The question is attacked on all sides. By some observers its solution is sought in laborious investigations of the hilly and mountainous regions where these rocks hold their empire among the grander solitudes of nature. By others the question is studied in the quiet of their own libraries or laboratories with all the resources of modern chemistry and microscopy. Great progress has indeed been made in these various ways. Regarding certain aspects of the problem a general agreement has been arrived at; but there are others as to which the difficulties remain as persistently obstructive as ever.

One of the most important contributions to the study of this fascinating subject has just appeared in the form of a handsome quarto volume, with a large atlas of plates, by Dr. J. Lehmann. This author has enjoyed exceptional opportunities of qualifying himself for the task he has now undertaken. For nine years, as a member of the Geological Survey of Saxony, he was engaged in the investigation of the classical granulite-region of that kingdom, of which he surveyed the southern and rather larger half, while his colleague, Mr. E. Dathe, investigated the northern part. The maps of this Survey are accompanied with explanatory pamphlets, among which Dr. Lehmann's detailed local observations have already been published. But it was desirable to present a generalised description of the whole region and to discuss the bearings of the observations upon theoretical questions. He originally proposed to undertake this task in association with Mr. Dathe; but his transference to Bonn as Privat-docent in Mineralogy and Geology, and the removal of his friend to the Prussian Geological Survey, having prevented the intended cooperation of the two observers, Dr. Lehmann has himself worked up the mass of materials collected during his long course of work in the field. To enlarge the scope of his inquiries and obtain additional data for comparison he has recently extended his investigations into the Erzgebirge, Fichtelgebirge, and the mountains of the Bavarian and Bohemian frontier. And he now offers what we may hope is only a first instalment of his results.

Naumann, whose early account will always be cited as a model of careful observation and accurate description, regarded the granulite of Saxony as an eruptive rock—an opinion in which he has still modern followers, including our author himself. He recognised a fact which seems in more recent times to have been lost sight of, that a gradation can be traced from the more highly crystalline condition of the granulite centre, through successive zones of mica-schist, and other schists, into the older sedimentary rocks of the surrounding districts. These schistose rocks have in more recent times been classed as "Archæan," and as such they appear on the maps of the Saxon Geological Survey, Dr. Lehmann having himself accepted this view in his earlier published descriptions. But more extended study of the subject has induced him to abandon the idea of the existence of any Archæan nucleus and to return to a modification of the original conception of Naumann. How he has been led to this conclusion it is the object of his volume and atlas to show.

Under the deep cover of post-Tertiary deposits, the granulite tract of Saxony forms a central ellipse round which zones of various schistose rocks are grouped, that pass outwards into the normal clay-slates of that part of Germany. These slates on the south-eastern margin are unconformably overlaid by Silurian and Carboniferous rocks. On the north-west side a conformable sequence is traceable from the schists and slates upwards into Cambrian and Lower Silurian rocks, which are precisely like those of the adjacent countries. Instead of being Archæan masses, Dr. Lehmann concludes that the whole of the crystalline schists within the granulite area are metamorphosed Palæozoic sediments. They may be originally of Silurian or Cambrian age, and their metamorphism probably took place during the crumpling and upheaval of the area, that is, later than the Devonian and older than the Carboniferous period.

Towards the establishment of this conclusion the author brings forward a vast mass of detail, which he skifully arranges so that its bearings upon theoretical questions may be clearly seen. At the same time he endeavours to separate rigidly what is demonstrable fact from what is mere inference, and in this lies one of the most valuable features of his memoir. He has collected such a body of evidence as will give a new impetus to the study of metamorphism, while at the same time it provides abundant new and suggestive material for the prosecution of this study. He justly cites the Saxon granulite area as a classic example of the occurrence and origin of metamorphic schists where a complete gradation can be followed from unaltered or little altered sediments into wholly crystalline foliated masses. In this progressive intensity of metamorphism the most notable fact is the corresponding advance in the development of mica. Over and above all local diversities of mineral character, there is a constant augmentation in the quantity and size of the mica-folia. At the same time the muscovite, which is alone present in the outer parts of the area, is replaced further inwards by biotite. Nor is this change confined to the peripheral schists; it extends into the granulite of the centre. Such a rearrangement of the mineral constituents of the rocks cannot be explained by any hypothesis of an eruptive granitic mass. Like so many other concurrent facts, it points to the effects of the molecular movements of the original rocks, sedimentary or other, under the strain to which they were subjected during the process of crumpling and upheaval. Where these movements have been greatest, there the accompanying metamorphism has been most intense, and, as one prominent indication of this change, there is the most abundant development of biotite. Every student of the crystalline schists can furnish parallel examples to those cited by Dr. Lehmann where, on the zigzag puckerings that form so striking a feature among these rocks, a copious growth of biotite or some other mica has taken place.

Among the metamorphosed rocks of the Saxon region some of the most instructive are bands of conglomerate interstratified among the schists. The sedimentary origin of these zones is of course unquestionable, and so obvious that the alteration to which they have been exposed furnishes a kind of sample of the initial stages of change which are so often lost where the clastic materials are of a less prominent and obdurate character. The pebbles of granite, quartz, &c., have been deformed and more or less altered, so that sometimes they seem to shade off into the surrounding matrix. The latter has become a crystalline micaceous mass by which the pebbles are wrapped round. These conglomerate bands have thus been converted into half-crystalline gneiss-like schists.

A specially important part of the memoir deals with Gabbros and Amphibolites. These rocks, as members of the series of crystalline schists, have long been a puzzle to those who have studied them in the field. That they are metamorphic rocks, and not rocks of original chemical precipitation, has been inferred from their association with masses whose original sedimentary origin admits of no doubt. But even those who have held this view have hesitated as to the nature of the original masses out of which they have come. Many years ago Jukes suggested that hornblende-rocks and hornblende-schists might represent ancient lavas and tuffs interstratified with the sediments which are now schists and quartzites. And it seems probable that this opinion is essentially correct. Dr. Lehmann goes into great detail regarding the structure of the diallage and hornblendic rocks of the granulite tract. His study of them leads him to conclude that the gabbro is an eruptive rock, younger than the granulite but older than the granite, which has been involved in the general metamorphism and has consequently assumed schistose modifications. "I know no rock," he adds, "which illustrates so well the effect of mechanical pressure upon a solid rock as the gabbro of the Saxon granulite tract. While other rocks leave us in doubt as to their original condition, the gabbro supplies us with every stage from the beginning to the end of the metamorphism." These conclusions possess at present a special interest in relation to the crystalline schists of this country. The Geological Survey, in the course of an investigation of the schists of the north of Scotland, has recently come independently to similar deductions with regard to the diorites and amphibolites of Aberdeenshire and Banffshire. Among the schists of that region there occur extensive masses of diorite. This rock presents sometimes the typical composition and structure of a diorite, and under the microscope appears as one of the most beautiful examples of a thoroughly crystalline granitoid mass. It behaves in the field as an eruptive rock, which has risen generally parallel with, but also transgressive across, the bedding of the contiguous schists. It is obviously from these characters a mass that has been intruded into the clay-slates, knotted-schists, and other schists of the district. Being traversed by veins and bosses of granite, its protrusion was obviously earlier than that of some at least of the granite. Further examination of it, however, shows that in many places it presents a remarkable parallelism in the arrangement of its crystalline constituents. Sometimes this is shown by the orientation of the feldspars in one definite direction. In other places the feldspar and hornblende are drawn out into more or less distinct bands. Further stages of change reveal the feldspar segregated into an almost pure labradorite rock, while the hornblende appears as a felted mass of hornblende-schist. Some of these schistose aggregates are of exquisite beauty. Over wide tracts biotite has been abundantly developed in the diorite, and sometimes also numerous and large kernels of garnet. It is observable that the direction of the foliation of the diorite coincides with that of the surrounding schists. There seems no reason to doubt that, as these Scottish schists are metamorphosed Lower Silurian sediments, the diorites and amphibolite-schists represent Palæozoic eruptive rocks that have participated in the general metamorphism. Dr. Lehmann recognises, in the Mica-schist and Phyllite groups, hornblende-schists which he thinks may have been embedded masses of diabase that have been more or less altered.

His general conclusions are thus summed up:—"I cannot regard the metamorphic schists (mica-schists, gneisses, &c.) as 'Archæan' formations. It does not appear to me to be established that genuine gneisses anywhere came out of pre-Cambrian sediments. The production of such rocks as mica-schist, &c., belongs to the time of mountain-upheaval, and in actual fact has involved formations of far younger age than the Cambrian. In the Saxon granulite region it is later than the Devonian period." He draws a distinction between what he considers to be "true gneisses" and other rocks to which the general name of gneiss has been applied. He restricts the appellation to the foliated forms of granite. This foliated or true gneissic structure he believes to be more or less due to metamorphism by stretching, seldom wholly original, so that many gneisses may be called metamorphic; only, the original rock was not a sediment but a mass that consolidated from fusion (Erstarrungsgestein). We fear that a theoretical distinction of this kind will involve all kinds of practical difficulties in its general application.

Reference must be made to the atlas that accompanies the memoir. It contains 28 plates, on which are placed no fewer than 159 photographs of thin sections of the rocks described in the text. Unlike the usual illustrations of this kind, these photographs represent the objects of the natural size, or less, or at most only slightly magnified. They are not microscopic studies, but show the actual structure of the rocks as seen by the naked eye or with a weak lens. It is impossible to speak too highly of the success with which they have been produced. With their aid we are rendered in some measure independent of the actual specimens, and can follow with pleasure and satisfaction the detailed descriptions of the author. No such wealth of accurate illustrations has yet been furnished for the study of this important series of rocks. Dr. Lehmann, however, is, we hope, only on the threshold of his inquiries. A vast domain lies before him where the problems are many and the qualified observers are but few. He has done excellent service by presenting in this compendious form such an array of facts as a trained geologist can gather in the field, and by boldly announcing the conclusions to which the study of these facts has led him. But much more may be made of them than he has yet given us. And we trust he may be encouraged to continue the investigation he has so well begun. ARCH. GEIKIE

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 insure the appearance even of communications containing interesting and novel facts.*]

The Marine Biological Association

WILL you allow me space to ask all naturalists and lovers of science who intend to become members of the above Association to send their names and subscriptions *without delay* to Mr. Frank Crisp, 6, Old Jewry, London, E.C. The subscription is one guinea annually, or fifteen guineas for life membership. It is highly desirable that intending members should at once enrol themselves, since the first meeting of the Association for the election of officers and council for the year 1884-85, and for the ratification of by-laws, will be held in London at the end of this month, when Prof. Huxley will be nominated as President. Donations, whether large or small, are earnestly solicited. Those who are interested in the natural history of marine plants and animals, and who foresee the immense help to this study which a well-equipped laboratory will afford, are begged not only to give some pecuniary aid to the present enterprise, but to constitute themselves agents of the Association and to do their best to persuade others to contribute to the fund required for building the first biological laboratory on the English coast. It is only by hearty and earnest support of this kind that our object can be realised.

I may add that several naturalists have contributed each 100*l*. to the Association, others 25*l*., and others less, according to their means and their sympathy with our object. Of the 10,000*l*. required, we have not yet obtained half.

E. RAY LANKESTER,
Secretary (*ad interim*)

11, Wellington Mansions, North Bank, N.W.

The Equatorial Coudé of the Paris Observatory

IN continuation of my first letter I now proceed to answer M. Lœwy's second letter, as published in your issue of May 15 (p. 52).

M. Lœwy has not, as I said in that former letter, raised a single objection which had not already been anticipated and discussed with the exception of one which I shall treat of further on. The several points in this letter I shall dispose of very shortly.

1. As to the dialyte construction, I have to reply that that particular method of achromatising the objective is not an *essential* feature of this instrument. Whether it be adopted or not is in fact much a question of *cost*. If the purchaser desires to get the largest possible aperture at least expense, then I would make it a dialyte, for, notwithstanding all M. Lœwy says, good work can be and has been done with dialytes. If, however, the most perfect instrument is desired, I would dispense with the dialyte construction, and achromatise the object-glass in the ordinary way, which is quite as applicable to my construction of equatorial as is the dialyte. If I mistake not, the celebrated observer M. Dembowski observed for many years with a dialyte, and spoke highly of it; he says: "*L'achromatisme est excellent.*" Again, the present director of the observatory, for whom the first of these instruments is to be made, has worked already with dialytes, and he would not be likely to recommend this construction if his experience agreed with M. Lœwy's. I desire to notice just one further point in this part of M. Lœwy's letter, as it is another example of how his own words (unintentionally, no doubt) confirm my statements. He says (speaking of the limited field of view of dialytes): "But, in order to turn the difficulty, he" (Mr. G.) "suggests that since the field of view becomes smaller as the instrument becomes larger, we may content ourselves with observing at a central point." I never said this; my words were: "The definition at the edge of the field, however, is not so good as in the ordinary form, but this would not be of so much consequence in large instruments, as the field in such cases is never of great extent." And M. Lœwy himself corroborates this for me when he says: "For the observation of comets I have such an eyepiece, which magnifies fifty times, and has a field of view such that I can observe a degree (*i.e.* with the 12" equatorial *coudé*); for a telescope of 27 inches we might have such an eyepiece with a field of 24 minutes." Thus I have a distinct corroboration from M. Lœwy of what I said above.

2. Writing on the matter of stability, M. Lœwy curiously mixes up stability and accuracy of movement. Now while I claim that I can and will obtain greater stability in my form than exists in M. Lœwy's, I do not claim accuracy of movement, but on this point I propose to say very little at present for several reasons. In the first place, it would hardly be possible to discuss this and put it in an intelligible form to your readers without a careful drawing; secondly, the well-known stability of the instruments which have emanated from my workshops are quite sufficient guarantee that this point is not one likely to be neglected in any of my work; and thirdly, I find it utterly impossible to understand the sentences of M. Lœwy's paper bearing on this point, and if I, though familiar with the proposed construction, fail to understand them, I am hopeless of serving any useful purpose by discussing them in your columns, particularly as few of your readers have ever seen the design of the instrument referred to. M. Lœwy talks of "all movements of transmission being broken at right angles." I do not know what he means, but he omits to tell your readers that, according to my design, in the larger sizes I propose that all movements be effected by two hydraulic cylinders the valves of which are within reach of the observer while sitting in his chair; so that, without more physical exertion than is necessary to open a water-tap, he has full command of all the movements of the great instrument, a pair of vertical scales on the walls of his study giving the approximate position of instrument in Æ and declination, an arrangement eminently calculated to reduce the work of the observer.

3. Lastly, as to its want of universality. This is distinctly stated in my paper as a disadvantage of my form; but when M. Lœwy asserts that "it is based on a principle which no astronomer can admit, viz. that it is superfluous to observe the greater part of the northern heavens," it is evident that M. Lœwy has here gone too far, since that portion of the heavens within 20° of the Pole is only about 6 per cent. of the northern hemisphere. Ask any practical astronomer possessing a moderate-sized equatorial how many hours out of the total number of hours which he has worked in the year has his instrument been pointed to objects within 20° of the Pole, and, with the exception of a few who apply themselves to special work, the great majority will give a reply which will show how very little will be lost by the fact that this instrument cannot command that portion of the heavens. I have myself put this question to many, and with the result above mentioned. On this point I cannot do better perhaps than give an extract from a letter I have just received from the director of one of our public observatories:—"Instruments of large aperture are rarely if ever used for observations where extreme accuracy of measurements is required, such as annual parallax, nor for searching for nor observing comets, except to search along a known track for an expected periodical comet. This your instrument could do well. There is hardly an instrument in existence which is equally well adapted to all kinds of observations. The circumpolar zone of about 20° may be explored by other instruments, but for almost every kind of *systematic* work the remainder of the visible heavens will give plenty to do." The foregoing would be a sufficient answer to a question which M. Lœwy has put directly to me.

He says:—"Permit me to ask Mr. Grubb how he is going to study that part of the heavens which lies between 20° from the zenith and the Pole."

To any one who has seen my paper it will be evident that this point, which in M. Lœwy's letter is put forward as a discovery of his own, was already fully dealt with by me. I said: "The instrument commands the heavens from east to west and from south horizon to about 20° beyond zenith." And again: "As regards this instrument (equatorial *coudé*) I would observe that it possibly possesses an advantage over my form in being absolutely universal."

No doubt universality unaccompanied by such disadvantages as the equatorial *coudé* possesses is very much to be desired, but I do not yet see how it is to be obtained. With respect to the solitary original objection that M. Lœwy has raised, viz. that the light reflected by the mirror varies with the different angles of inclination, and therefore renders the instrument unfit for photometric researches, I confess this objection did not occur to me before, and I am inclined to think M. Lœwy is right, and that my instrument will not be well adapted for photometric researches, but I ask: Is the equatorial *coudé* any better? On consideration it will be evident to your scientific readers that the light after the first reflection is elliptically polarised, and if so the quantity reflected by the second mirror is variable at the various angles of declination; consequently photometric observations made with the equatorial *coudé* cannot be relied upon.

In attempting to prove his case M. Lœwy gives in his first letter a considerable number of numerical details, and no doubt most of your readers have taken these figures as correct. I will ask them, however, to verify for themselves a few of them, and the result will, I think, show how very loosely M. Lœwy has put these data together. For instance, he mentions the weight of a 40-inch mirror, whose thickness is just one-sixth of the diameter, to be 380 kilos., and he calculates (see further down) that a mirror of 38 inches diameter and proportional thickness would weigh 280 kilos.—100 kilos. less. Now, if the thickness be proportional, the weight should be as the cube of the diameter. If your readers will try this themselves, they will find that M. Lœwy has in this case exaggerated the difference to the amount of about 100 per cent.

I find I omitted to notice just one point in Mr. Lœwy's first letter. He says:—"If Mr. Grubb had looked at the drawing which I published in the *Journal de Physique* of last year, he would have seen that it is almost identical with that which he has communicated to the Royal Dublin Society, so far as the general arrangements for sheltering the observer and instrument are concerned."

Permit me to inform M. Lœwy that this would hardly have been a novelty to me last year, inasmuch as I had such arrangements not only on paper but in actual work for some years back, and a description of the same was published in the Royal Dublin Society's *Proceedings*, April 1879.

Your readers will see from the foregoing that M. Lœwy's whole letter is based on a series of misconceptions of statements in my paper in the *Transactions* of the Royal Dublin Society.

Some of the mistakes that M. Lœwy has fallen into were perhaps due to the fact that the plate issued with the Royal Dublin Society's *Transactions* was merely a diagram without details, introduced to illustrate the principle of the mounting. He assumes that details not figured in the diagram are not to be provided, in spite of the fact that in the text of my paper I discussed several of them.

M. Lœwy occupies nearly half a column of NATURE in speaking of the labour involved in working this instrument, because no tube is shown in the diagram connecting the equatorial part with the ocular; all this trouble would have been saved if he had read my paper a little more carefully, for then he would have found that not only did I say, "In most cases it would be desirable to have a connecting tube," but I even discuss the best form of tube for the purpose. There are some special cases in which a tube would not be actually necessary.

It appears to me that M. Lœwy is very unnecessarily disturbed in his mind by the advent of my instrument. No doubt the equatorial *coudé* and my siderostatic telescope have each their own sphere of work, and there may be room for both. An observatory having at its back a generous individual who (as was stated at a late meeting of the Royal Astronomical Society), has already expended a quarter of a million on astronomical observatories and is willing to spend more, can afford a large instrument perhaps on M. Lœwy's plan; but as all observatories are not equally fortunate, there may occasionally be one found which will be glad to get equally great optical power at one-third the cost.

I barely alluded in my last letter to this question of cost. On this point it may be desirable to supplement what I have said in my former letter, bearing in mind that the cost of the instrument will, as I have above stated, depend somewhat on whether or not the objective is achromatised on the ordinary principle or that of the dialyte.

In order to put the matter of cost in the clearest light, let us consider the four forms which we have at present to select from, viz. the ordinary equatorial, M. Lœwy's equatorial *coudé*, my siderostatic telescope with objective achromatised in the ordinary way, and the same instrument with objective achromatised on the dialyte principle.

Let us consider first what apertures we can obtain in the several forms for a given sum; assuming M. Lœwy's figures for the equatorial *coudé*. For 1760*l*. can be obtained—

(*a*) Equatorial *coudé* of 12-inch aperture.

(*b*) Ordinary equatorial of 12-inch aperture, including its dome and observatory.

(*c*) Siderostatic telescope with objective achromatised in the ordinary way of 18 inches aperture.

(*d*) Siderostatic telescope with objective achromatised on the dialyte principle of 24 inches aperture.

It would be for the astronomer to say whether the double aperture of the objective would not more than counterbalance the disadvantages of want of absolute universality.

Let us, secondly, consider for what prices the *same* aperture could be obtained in the various forms:—

(*a*) Equatorial *coudé* 12″ aperture	£1760
(*b*) Ordinary equatorial of 12″ aperture, including dome and observatory	1760
(*c*) Siderostatic telescope of 12″ aperture with objective achromatised in the ordinary way ...	1000
(*d*) Siderostatic telescope of 12″ aperture, with objective achromatised on the dialyte principle	500

The difference between cost of equatorial *coudé* and siderostatic dialyte (about 1200*l*. for this size) will probably be considered by the purchaser rather too large a sum to pay for the possibility of examining the 6 per cent. of the northern hemisphere which is beyond the reach of my siderostatic telescope, particularly when it is borne in mind that that portion is the least important part of the heavens.

M. Lœwy does not say whether the 1760*l*. includes cost of observing hut. If not, the comparison is still more striking, for, although the equatorial *coudé* requires a special building, my siderostatic telescope does not. HOWARD GRUBB

Dublin, May 27

The Earthquake

CHANCE brought me to Colchester about a week after the earthquake, and since then I have been amusing myself mapping the effects of it, and hope to read a paper on the subject at the meeting of the Royal Geological Society, Ireland, next month. In the meantime I would like to draw attention to a few of the general facts that seem not to be recorded.

The area of structural damage lies at and southward of Colchester, principally to the west of the Colne estuary, and in it there are five smaller areas in which are found the greatest damage. These areas occur in the following order:—Wivenhoe, Peldon, Abberton and Langenhoe, Colchester, and West Mersea; each of these have two or more well-marked margins; where these margins can be easily studied, there are found to be lines of breaks, and alongside one, or in places two of them, the greatest destruction occurred, while at the other side of such lines the damage is a bagatelle in comparison.

Thus at Wivenhoe, where there was the greatest damage done, the shock came from the north-east; but when it reached the break of the Colne River valley, it seems to have recoiled as if from a percussion blow. Westward of the estuary of the Colne the damage at Rowhedge was slight when compared with that at Wivenhoe, while at Hornwood it was still slighter, although the last is only divided from Wivenhoe and Rowhedge by the valleys of the Colne and Roman Rivers.

At Peldon, where the shock appears to have been nearly as bad as at Wivenhoe, the damaged area is very well defined, being bounded northward and southward by stream valleys. The shock seems to have travelled southward and to have recoiled from the southern boundary, causing excessive damage alongside it. Here also the shock appears to have had a rotary motion, which possibly may be due to the recoil against the southern boundary.

In the Abberton and Langenhoe area the shocks seem also to have had a rotary motion, the main direction seems to have been from the south-east; here the greatest damage occurs at the western boundary.

At Colchester the shock was going north, while at West Mersea it went south. In both of these places the boundaries of the areas are in part obscure. In the first, however, we can trace the tract of maximum damage from Head Gate along the south Roman wall and eastward to Colne valley, east of which very little damage was done. Thus everywhere except at West Mersea there are one or more lines, at one side of which there was excessive damage not to be found at the other side.

In the area of excessive damage, according to Mr. Dalton's map, the geological formations are *Alluvium*, *Glacial Drift*, and *London Clay*. On the first we find damage done to houses near Eastbridge, Colchester, and at Wivenhoe, although elsewhere they escaped. In the north portion of Wivenhoe and the north portions of Colchester, structures on the Glacial Drift were injured, but elsewhere the damage nearly invariably is confined to tracts and small exposures of the London Clay. This is very conspicuous in places—at Colchester there is a narrow outcrop of London Clay which widens eastward near the Colne, and on this narrow tract the greatest damage was done; similarly at Wivenhoe the excessive damage is along the outcrop of the London Clay. At Fingrinhoe and Frenchman's Lane the damage margins an outlying tract of Glacial Drift, while very good examples can be seen between Colchester and Ardleigh, the structures on narrow tongues of the clay being injured, while those on the intervening tracts of gravel have escaped, except in one instance.

Victoria Road, Colchester J. HENRY KINAHAN

ONE of the most curious effects of the earthquake in the Peldon district is the evidence of a decided *twist* or apparent rotation of the shock evident in many cases upon standing buildings. Is is very apparent in the cracks throughout Dr. Green's house, which take a complete screw round some of the rooms and the staircase. It is also evident in the twist of the tapering mill chimney shaft where the upper 20 feet (still standing) is screwed round at the fracture upon the lower part about one inch. The same is apparent in a chimney at the "Peldon Rose" Inn, the screwing in this instance being about two inches. As such twists as are evident could not exist within the areas of separate single buildings, it appears to me that they must have been the resultants of the effects of two separate shocks, the first about north to south, and the second immediately following about east to west. That there were two shocks appears to be the general impression of the inhabitants of whom I made inquiry. Another matter of interest is the very peculiar fracture of the eastern side of Dr. Green's house. This fracture leaves the lower northern corner of the wall, and passes diagonally across the house to the upper southern corner. The crack is open about one inch through solid modern brickwork. In this case the line of fracture does not follow a line of weakness in the wall, but cuts directly through the thick chimney breasts, and equally across a window opening, as though there was present no difference in resistance. The angle of fracture is about 47° to the horizon, and it appears to me that this must have been the direction of the first or greater shock in this district, which was therefore more one of upheaval than of horizontal motion. This is also confirmed upon inquiry, as I find many persons in the district felt distinctly the motion of upheaval, but no one who was standing at the time is known to have been thrown down.

W. F. STANLEY

DARWIN relates that the earthquake of February 20, 1835, which overthrew Concepcion, although it was severely felt in Chiloe, yet on the neighbouring Cordillera (near Mellipulli) it was not felt at all. "Some men who had been employed in the mountains splitting fir planks, when they returned in the evening to Calbuco and were told of the shock, said that 'about the time mentioned they recollected that they had not been able to strike fair with the axe, and that they had spoilt a board or two by cutting too deep.' This probably is not so fanciful as it appears; at least it shows that if there was any motion it was of an exceedingly gentle kind" (*Trans. Geol. Soc.*, vol. v. p. 605).

A parallel case occurred during the late earthquake in Essex. Some men hoeing wheat at Frating, about seven miles northeast of the focus of the shock, did not perceive the shock, but felt as if they could not get their hoes to the ground.

May 30 O. FISHER

Jupiter

THIS planet is now so unfavourably placed that very few further opportunities will occur of observing the chief features during the present apparition. It is, however, important that the red spot and equatorial white spot should be followed as long as practicable, and I give a list of the times when they will be situated on or near the central meridian:—

	Red spot h.		White spot h.
June 5	8·8	June 5	9·1
7	10·4	7	10·3
10	7·9	12	8·3
12	9·5	14	9·5
17	8·7	21	8·8
22	7·8	28	8·1

The two spots will come to the same longitude on June 7, but at the time of their transit Jupiter will be too low to admit of satisfactory observation.

Erratum.—The dark satellite transit which I observed on May 18 (NATURE, May 22, p. 77) referred to the *fourth* satellite and not to the first as described. The three dark spots seen were really the shadows of the first and second satellite and the fourth satellite itself. The first satellite was also projected on the disk of Jupiter at the time of the observation, but it was not seen under the form of a dark spot. The error in the original description arose from a mistake in the identification of the satellites and their shadows, four of which were on the planet at the same time. W. F. DENNING

Bristol, June 1

Animal Intelligence

THE instances of intelligence which I am about to relate, to the credit of a cockatoo, were described to me by the owner, a lady, in whose presence they were displayed, as well as in that of several other witnesses, one of whom (her husband) was also present on two occasions when I heard the accounts.

The bird is fond of white lump-sugar, and ordinarily drops it into his saucer of tea or other drink to soften it. On one occasion when he was thought to be thirsty, a glass of water was offered him, which appears to have been of the goblet kind, about 6 inches high, with a foot and stem, and holding, it would seem, something more than a large wine-glass and less than a small tumbler. Shortly after, the bird received a piece of sugar, and, as usual, dropped it into the water. But now, alas! the depth of liquid was too great for him to recover the saturated lump; and unfortunately, not having myself witnessed the occurrence, I am unable to describe the indications of mental effort which doubtless preceded the attempt to solve the problem of extracting the lump of sugar before it should disappear. I was told that the like difficulty recurred next day, and, whether on account of the practical failure of the first attempt, or in consequence of a fresh inspiration at the moment, a different *and entirely successful* plan was then adopted. It is no doubt to be regretted that the experiment was not followed up, but the reason will shortly be apparent. Now, as to the first attempt. There was no endeavour to upset the glass; it was too high for the claw to be used, and too deep for the beak to be plunged in. To *drink* all the water would indeed have been, as remarked by the narrator, "an heroic remedy." What "Koko" *did* do was to *bale* the water out with his scoop-like lower mandible. Here again I find myself unable to describe the action more exactly, but it must have been in the highest degree interesting to watch the operation, with its increasing difficulty, and constantly diminishing prize at the bottom. Finally we may suppose that the sugar having disappeared the last portions were at least partly enjoyed. Still the result was to some extent evidently a disappointment; for on the next occasion "Koko," without the least hesitation, put in practice a device which we may fairly suppose he had thought out meanwhile. He began forthwith to drop in lumps of sugar one after another until the last was level with the surface, when he recovered that one and left the rest to their natural fate, while he peacefully enjoyed the fruits of his invention.

I have unfortunately too slight an acquaintance with the ways of these birds to know certainly whether this is above the average of their intelligent acts and as such worthy of space in your columns. For the same reason I hesitate to give, at second hand, other indications which, however interesting to me, might prove less so to others. I will only add that it is so distressing to see so nice a creature almost naked, through its inveterate propensity to pluck out every feather within reach, that I should be glad to hear of any possible remedy. J. HERSCHEL

23, Suffolk Street, Pall Mall East, S.W.

P.S.—At the suggestion of a gentleman whose name is well known to readers of NATURE in connection with animal intelligence, a proof of the above letter was sent to the bird's headquarters for confirmation. Although I cannot now regret that all these interesting details have been thus elicited, most readers will agree with me in wishing that the first account had been also written in M. d'Abbadie's characteristic style. However that may be, I am sure that I shall only be expressing the wishes of every reader as well as my own in taking this opportunity of thanking him cordially for his amusing and interesting response to my appeal for corroboration.—J. H.

[The following is M. d'Abbadie's letter :—]

MY DEAR SIR,—I have met few people who can pen a narrative quite accurately as you have done. Were I writing history, I should add that "Koko" (a *Cacatua moluccensis*) seems to hail with satisfaction the appearance of a coffee-tray. As soon as it is laid on the table he lifts by its central knob the sugar bowl's cover, picks up a lump of sugar, and drops it in an empty cup, on which he taps with his beak to intimate that he is thirsty, for dry sweets are not to his taste.

This bird is the wonder and plague of my life. One day curiosity (?) impelled him to pull into shreds my only kaleidoscope. He got for his industry a few touches of a whip, which he tore to pieces twenty-four hours later, showing apparently that memory is one of his gifts. He has a knack of tearing my pens and papers, sometimes with good reason. As, however, I arrogate those privileges to myself, "Koko" is excluded from my study. Its door, partly glazed, has a pad to prevent draughts, and, before I added a bolt, was closed only by a knob two centimetres thick. Climbing along the pad eighty centimetres above the floor, he looked slily with one eye through the glass, and if the coast was clear he then proceeded to business by turning the knob with his powerful beak and then pulling it, while one claw pushed against the doorpost, the other holding on to the pad. Another door has a lock, and servants thought to exclude the intruder by turning the key, but "Koko" soon learnt to turn it back before applying his energies to the knob. Having thus put the door ajar, he descends cautiously with beak and claws along the pad, opens the door by a push or pull of his beak, as the case may be, and, stopping on the threshold, exclaims modestly, "Koko!" like a Wolseley or a Graham announcing his recent victory.

I say *modestly*, for this bird assumes often a haughty tone when uttering what we call his public speech. It is a rambling gabble, and, like the sayings of a French orator who shall be nameless, it is wholly unintelligible. However, its varying tones are splendid. Those of indignation and command prevail, but in the course of seeming argument "Koko" expresses also and most forcibly concession, interrogation, pity, disdain, ridicule, contradiction, and even logical inference. Without having read the Roman author who advises orators to take hold of their beards when pausing to reflect, "Koko" halts now and then his wordy torrent to seize his chin with his claw, as if pondering on the best line of action. Other appropriate gestures add to the seeming reality of his discourse. I have seen him stand suddenly on one leg, double up the other claw like a fist, and deal a blow on the air as if to knock down an enemy. In spite of their wide sleeves, our barristers might well envy the fulness of gesture imparted by his wings. He raises them expanded over his head, then throws them down impetuously before himself with a seemingly clenching argument. I have heard that Burke used his arms in the same way when beginning his outbursts of eloquence.

The wisdom of nations has sometimes found it necessary to put even statesmen in durance vile, and "Koko" has not escaped the lot of his betters. As, however, he contrived to unfasten several kinds of common spring padlocks, and even one which requires, like my door, three simultaneous manœuvres, my astronomical artist boasted of making one which would puzzle even a Christian. Our bird was chained with this ingenious invention, and immediately busied himself for about two hours pressing on every side the brazen problem. It seemed to cause a heavy expense of thought for his slender brains. On the following day he opened this new-fangled padlock, but with evident difficulty. Finally, having mastered it the third morning, he then freed himself with the greatest ease. Withal he cannot ged rid of a padlock that requires a key, nor has he yet pushed back a loose bolt on the very door where he overcomes a fastening apparently more complicated. May we infer that bird reason differs from human reason?

Although he has not been in a theatre, "Koko" whistles when displeased. He can laugh, bark, and cackle. Two first-rate players at fives laid a wager that they would be the gainers in my private court, even with their feet tied together. This forced them to jump in order to meet the ball, and their eagerness brought on several falls, some dozens of witnesses laughing at each mishap. "Koko" had always inspected the fives games from his favourite perch on a neighbouring balcony. He now wished perhaps to improve an incident new to his experience. Having alighted in the court, he proceeded to stand on one foot, then jumped, falling each time sometimes forwards, sometimes sideways, like the players, and took good care to laugh loudly at each pretended stumble.

The foregoing facts, where a bird's reasoning powers seem to rival those of men, suggest two questions: (1) Where is the boundary between them? and (2) whether intelligence depends, as is often supposed, merely on size of brain? Unable to answer these queries, I remain very truly yours,

ANTOINE D'ABBADIE,
de l'Institut de France

Paris, May 20, 1884

METEOROLOGY IN VICTORIA

THE monthly and other publications on meteorology and terrestrial magnetism issued by the Melbourne Observatory continue to be regularly received by us, the last *Monthly Record* being for December 1883. Since we reviewed these *Records* (NATURE, vol. xiv. p. 153) we have observed with much interest the steady, and latterly the rapid, extension of climatological stations over the colony. During the ten years ending December 1883, while the number of fully equipped stations has remained nearly the same, stations at which temperature is observed have increased from 10 to 27, and stations for rain observations from 34 to 170. These 170 stations are conveniently classed into coast, watershed, and river-basin groups, and the individual gauges of each group are further arranged in the tabular returns in the order of their heights, which rise to about 4000 feet. The *Records* conclude with a detailed report for all the stations of thunderstorms, hail, snow, frost, gales, hot winds, auroras, earthquakes, &c., observed during the month.

While isobars can be drawn with tolerable correctness from the observations of a small number of stations, and isothermals from the returns of a few more, but still a comparatively small number of stations, it cannot be too strongly insisted on that a very large number of rain-gauges are required to give even a tolerable approximation to the actual rainfall of a country for a definite period, say, a week or a month, which may not seriously mislead those interested in the rise and fall of prices of agricultural and other products that depend on the weather. The meteorological authorities of South Australia and New South Wales are, equally with Mr. Ellery, so fully alive to the paramount importance of an adequate observation of the rainfall, that after a few years' continued vigorous effort this large portion of Australia will take rank, in respect of its rainfall, as one of the best observed regions of the globe.

At Melbourne the wind is observed and the results are discussed with admirable fulness. In summer the prevailing winds are southerly, and in winter northerly. The strongest winds are north and north-west, and the lightest east and south-east, the south-easterly winds in some seasons blowing with only about a third of the velocity of the north-westerly winds. The diurnal velocity of the wind falls to the minimum from about midnight to 4 a.m., and rises to the maximum from about 10 a.m. to 4 p.m. As regards season, the absolute maximum occurs at noon in winter, but in summer two hours later. Another important feature in the diurnal velocity of the wind is that from April to August the daily maximum is only a half more than the minimum velocity, whereas from October to February it is more than double. In other words, the maximum velocity rises to a greater extent above the daily mean during the period of the year when the temperature is rising from winter to summer than when it is falling from summer to winter, agreeing in this respect with what has been observed in similar regions.

The *Monthly Records* give, in addition to the month's results, the averages of that month for each station based on previous years' observations. In the review referred to above we drew attention to the temperature observations at Portland as being evidently too high. In the following February (1877) the mistake was rectified, and since then the observations of temperature at this station agree with those made at the other stations. A comparison shows that down to January 1877 the published temperatures at Portland were about 5°·0 too high. As regards the averages published since then, however, no allowance has been made down to December 1883 for this large error. The result is that while at the other stations of the colony the mean temperatures of the months since February 1877 rise above and fall below their averages as at other places, Portland all but uninterruptedly appears as very much below its average. Indeed, except the unusually warm months of September 1879 and February 1880, not one of the other forty-six months shows a temperature as high as the average. It is the more necessary to draw attention to this point seeing that the faulty mean temperatures of Portland still continue to appear in works on climatology, either in the text, or they have been used along with the means of other places, similarly faulty, in drawing the isothermals of the globe.

HABITS OF BURROWING CRAYFISHES IN THE UNITED STATES

On May 13, 1883, I chanced to enter a meadow a few miles above Washington, on the Virginia side of the Potomac, at the head of a small stream emptying into the river. It was between two hills, at an elevation of 100 feet above the Potomac, and about a mile from the river. Here I saw many clayey mounds covering burrows scattered over the ground irregularly both upon the banks of the stream and in the adjacent meadow, even as far as ten yards from the bed of the brook. My curiosity was aroused, and I explored several of the holes, finding in each a good-sized crayfish, which Prof. Walter Faxon identified as *Cambarus diogenes*, Girard (*C. obesus*, Hagen), otherwise known as the burrowing crayfish. I afterwards visited the locality several times, collecting specimens of the mounds and crayfishes, which are now in the United States National Museum, and making observations.

At that time of the year the stream was receding, and the meadow was beginning to dry. At a period not over a month previous, the meadows, at least as far from the stream as the burrows were found, had been covered with water. Those burrows near the stream were less than six inches deep, and there was a gradual increase in depth as the distance from the stream became greater. Moreover, the holes farthest from the stream were in nearly every case covered by a mound, while those nearer had either a very small chimney or none at all; and subsequent visits proved that at that time of year the mounds were just being constructed, for each time I revisited the place the mounds were more numerous.

The length, width, general direction of the burrows, and number of the openings were extremely variable, and the same is true of the mounds. Fig. 1 illustrates a typical burrow shown in section. Here the main burrow is very nearly perpendicular, there being but one oblique opening having a very small mound, and the main mound is somewhat wider than long. Occasionally the burrows are very tortuous, and there are often two or three extra openings, each sometimes covered by a mound. There is every conceivable shape and size in the chimneys, ranging from a mere ridge of mud, evidently the first foundation, to those with a breadth one-half the height. The typical mound is one which covers the perpendicular burrow in Fig. 1, its dimensions being six inches broad and four high. Two other forms are shown in Fig. 2. The burrows near the stream were seldem more than six inches deep, being nearly perpendicular, with an enlargement at the base, and always with at least one oblique opening. The mounds were usually of yellow clay, although in one place the ground was of fine gravel, and there, the chimneys were of the same character. They were always circularly pyramidal in shape, the hole inside being very smooth, but the outside was formed of irregular nodules of clay hardened in the sun and lying just as they fell when dropped from the top of the mound. A small quantity of grass and leaves was mixed through the mound, but this was apparently accidental. The size of the burrows varied from half an inch to two inches in diameter, being smooth for the entire distance, and nearly uniform in width. Where the burrow was far distant from the stream, the upper part was hard and dry. In the deeper holes I invariably found several enlargements

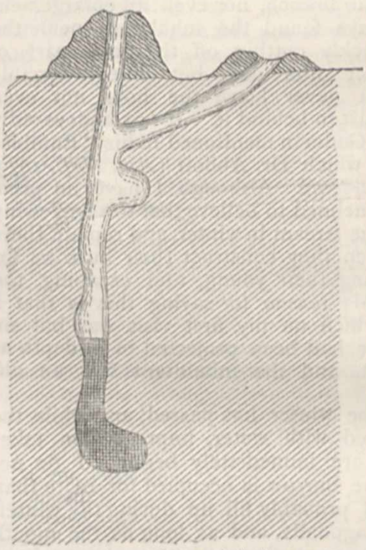


Fig. 1 Section of Crayfish burrow

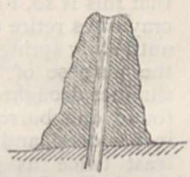


Fig. 2 Crayfish mound

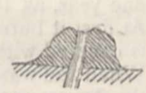


Fig. 2 Crayfish mound

at various points in the burrow. Some burrows were three feet deep, indeed they all go down to water, and, as the water in the ground lowers, the burrow is undoubtedly projected deeper. The diagonal openings never at that season of the year have perfect chimneys, and seldom more than a mere rim. In no case did I find any connection between two different burrows. In digging after the inhabitants I was seldom able to secure a specimen from the deeper burrows, for I found that the animal always retreated to the extreme end, and when it could go no farther would use its claws in defence. Both males and females have burrows, but they were never found together, each burrow having but a single individual. There is seldom more than a pint of water in each hole, and this is muddy and hardly suitable to sustain life.

The neighbouring brooks and springs were inhabited by another species of crayfish, *Cambarus bartonii*, but although especial search was made for the burrowing species, in no case was a single specimen found outside of the burrows. *C. bartonii* was taken both in the swiftly-running portions of the stream, and in the shallow side pools, as well as in the springs at the head of small rivers. It would swim about in all directions, and was often found under stones and in little holes and crevices, none of which appeared to have been made for the purpose of retreat, but were accidental. The crayfishes would leave these little retreats whenever disturbed, and swim away down stream out of sight. They were often found some distance from the main stream under rocks that had been covered by the brook at a higher water-mark; but although there was very little water under the rocks, and the stream had not covered them for at least two weeks, they showed no tendency to burrow. Nor have I ever found any burrows formed by the river species *Cambarus affinis*, although I have searched over miles of marsh-land on the Potomac for this purpose.

The brook near where my observations were made was fast decreasing in volume, and would probably continue to do so until in July its bed would be nearly dry. During the wet seasons the meadow is itself covered. Even in the banks of the stream, then under water, there were holes, but they all extended obliquely without exception, there being no perpendicular burrows, and no mounds. The holes extended in about six inches, and there was never a perpendicular branch, nor even an enlargement at the end. I always found the inhabitant near the mouth, and by quickly cutting off the rear part of the hole could force him out, but unless forcibly driven out it would never leave the hole, not even when a stick was thrust in behind it. It was undoubtedly this species that Dr. Godman mentioned in his "Rambles of a Naturalist," and which Dr. Abbott (*Am. Nat.*, 1873, p. 81) refers to *C. bartonii*. Although I have no proof that this is so, I am inclined to believe that the burrowing crayfishes retire to the stream in winter, and remain there until early spring, when they construct their burrows for the purpose of rearing their young, and escaping the summer droughts. My reason for saying this is that I found one burrow which on my first visit was but six inches deep, and later had been projected to a depth at least twice as great, and the inhabitant was an old female.

I think that after the winter has passed, and while the marsh is still covered with water, impregnation takes place and burrows are immediately begun I do not believe that the same burrow is occupied for more than one year, as it would probably fill up during the winter. At first it burrows diagonally, and as long as the mouth is covered with water is satisfied with this oblique hole. When the water recedes, leaving the opening uncovered, the burrow must be dug deeper, and the economy of a perpendicular burrow must immediately suggest itself. From that time the perpendicular direction is preserved with more or less regularity. Immediately after the perpendicular hole is begun, a shorter opening to the surface is needed for conveying the mud from the nest, and then the perpendicular opening is made. Mud from this and also from the first part of the perpendicular burrow is carried out of the diagonal opening and deposited on the edge. If a freshet occurs before this rim of mud has a chance to harden, it is washed away and no mound is formed over the oblique burrow. After the vertical opening is made, as the hole is bored deeper, mud is deposited on the edge, and the deeper it is dug the higher the mound. I do not think that the chimney is a necessary part of the nest, but simply the result of digging. I carried away several mounds, and in a week revisited the place, and no attempt had been made to replace them; but in one case, where I had, in addition, partly destroyed the burrow by dropping mud into it, there was a simple half rim of mud around the edge, showing that the crayfish had been at work; and as the mud was dry the clearing must have been done soon after my departure. That the crayfish retreats as the water in the ground falls lower and lower, is proved by the fact that at various intervals there are bottle-shaped cavities marking the end of the burrow at an earlier period. A few of those mounds farthest from the stream had their mouths closed by a pellet of mud. It is said that all are closed during the summer months. How these animals can live for months in the muddy, impure water is to me a puzzle. They are very sluggish, possessing none of the quick motions of their allied *C. bartonii*, for when taken out and placed either in water or on the ground they move very slowly. The power of throwing off their claws when these are grasped is often exercised. About the middle of May the eggs hatch, and for a time the young cling to the mother, but I am unable to state how long they remain thus. After hatching they must grow rapidly, and soon the burrow will be too small for them to live in, and they must migrate. It would be interesting to know more about the habits of this peculiar species, about which so little has been written. An interesting point to settle would be how and where it gets its food. The burrow contains none, either animal or vegetable. Food must be procured at night, or when the sun is not shining brightly. In the spring and fall the green stalks of meadow grasses would furnish food, but when these become parched and dry they must either dig after and eat the roots, or search in the stream. I feel satisfied that they do not tunnel among the roots, for if they did so these burrows would be frequently met with. Little has as yet been published upon this subject, and that little covers only two spring months, April and May, and it would be interesting if those who have an opportunity to watch the species during other seasons, or who have observed them at any season of the year, would make known their results. RALPH S. TARR

THE YOUNG GORILLA OF THE JARDIN DES PLANTES

THROUGH the courtesy of the editor of *La Nature* we are able to give an illustration from an instantaneous photograph of a young male gorilla obtained at the commencement of last winter by the Natural History Museum at Paris. It had been imported from the Gaboon, and it was the first living specimen of this great anthropomorphic ape which had been brought to France. Its study would have presented many points of interest, not alone from the Natural History point of view, but also from the opportunity it would have afforded of studying the development of its intellectual faculties. This young specimen was about three years of age, he had already his full complement of milk-teeth, and the long and sharp canines were decidedly longer than the molars. In disposition he appeared to be very different from either the orang-outan or the chimpanzee. While these in a state of captivity are mostly gentle and sociable, this young gorilla on the other hand was savage, morose, and brutal; he never gave his keeper the least mark of affection; he never allowed himself to be touched without evidencing the greatest aversion, and for the most part he returned caresses by snappings. He never took the least part in the games of the other apes, and he most reluctantly tolerated having them near him. He was but little active, and most generally kept himself crouched up in a corner of the cage, or sitting on a branch with his back up against the wall, and scarcely ever moved but to look about for something to eat. He used his hands with much readiness, and they were extremely well developed. His lips were less mobile than in the chimpanzee, especially the lower lip, which was never pouted out when drinking into a spoon-shaped form. His eyes were extremely mobile, and were crowned with immense superciliary ridges; his nose was flat, with excessively large nostrils, giving him a quite peculiar physiognomy.

His intelligence was feebly developed, and was in any case quite below that of the other anthropoid apes, or

even of the gibbons. Since the above note was presented by Alph. Milne-Edwards to the Paris Academy of Sciences the young gorilla died at the Jardin des Plantes.

NOTES

THE portrait of the late Sir William Siemens, which we give this week as one in our series of Scientific Worthies, belongs to the previous volume of NATURE, and is intended to accompany the memoir at p. 97.

THE Secretary of State for India has determined that India shall be represented at the forthcoming International Prime Meridian Conference at Washington, and has nominated as the India Office delegate Lieut.-General R. Strachey, C.S.I. Capt. Sir Fred. J. O. Evans, R.N., K.C.B., F.R.S., and Prof. J. C. Adams, F.R.S., on the recommendation of the Science and Art Department, have been appointed delegates to represent the United Kingdom at the Conference.

ON May 28, at an extraordinary meeting of the Vienna Academy of Sciences, Count Hans Wilczek was elected Honorary Member. In the Mathematical and Natural Science Section, Dr. Albrecht Schrauf, Professor of Mineralogy at the Vienna University, and Dr. Leopold Gegenbauer, Professor of Mathematics at the Innsbruck University, were elected Corresponding Members. As Foreign Honorary Members were elected Sir William Thomson, and Charles Hermite (Paris); as Foreign Corresponding Members, Prof. L. Leuckart (Leipzig), Prof. Edward Frankland, and Prof. Carl Nägeli (Munich).

THE "Rede" Lecture was delivered on Wednesday, last week, in the Cambridge University Senate House, by Mr. Francis Galton, M.A., F.R.S. Mr. Galton, who had selected as the subject for the lecture "The Measurement of Human Faculty," stated that, although it had been a matter of controversy whether a more complete measurement of man's capacity could be attained than was already discovered by means of examinations which measured intellectual capacity, yet he would endeavour to demonstrate that as the capacity of man, taken in its widest sense, including character and physique, was finite, therefore it was measurable. He pointed out that an important adjunct to the attempt to measure human faculty would be records containing particulars regarding eyesight, colour-sense, hearing, breathing capability, height, span of arms, &c. At the Johns Hopkins University in America physical education and hygiene were compulsory on all students, and although the physical measurements taken were not compulsory yet few objected, and the result was that the most valuable statistics were collected, and in many instances good advice given to the students in what way to counteract the effects of any abnormal condition observed, such as irregularity of muscular development. Mr. Galton concluded by suggesting that a laboratory should be opened at Cambridge to investigate this new science. The cost would be small; the result, he ventured to predict, would be large and beneficial, for, by the compiling of continuous records of health, growth, and disease, much useful knowledge would be acquired, and by a long series of observations on the lines he had indicated it would be possible to measure the human faculty as accurately as, if not more so than, our system of examinations measures the intellectual faculty.

WE understand that the Fishery Board for Scotland is anxious to have powers enabling it to remove obstructions which interfere with the ascent of salmon up several Scottish rivers. It is specially desirous to introduce as soon as possible a fishway at the falls, and this, when done, would open up some 500 miles of excellent fishing and spawning ground. Last week there were several interesting arrivals from fishery officers:—(1) A fine specimen of the "Opah fish" (*Lampris guttata*) was taken off Unst Island, Shetland, on May 22, and forwarded by the fishery officer to Prof. Ewart, University of Edinburgh. The fish, which measured 4 feet in length and over 2 feet in depth, has been handed over to Prof. Turner. This Opah will enable Prof. Turner to complete an account of the fish begun some years ago, when he received a somewhat smaller specimen from the Moray Firth. (2) A turbot with peculiar frontal process, an eye on each side of the head, both sides of the body equally dark, and provided with spines. (3) Mature sprats. It seems proved that sprats leave the estuaries in spring to spawn at sea. (4) Developing herring eggs. Taken along with similar specimens which have been arriving since October last, these show that herring spawn on the east coast from August until May, and not, as is usually supposed, only during August and September and during February and March.

IN response to the appeal of the Prince of Wales, as President of the City and Guilds of London Institute, the following contributions have been already promised to the General Fund and to the Equipment Fund of the Central Institution, Exhibition Road, by the undermentioned Companies:—Fishmongers, 4000*l.*; Mercers, 2000*l.*; Clothworkers, 2000*l.*; Corporation of London, 1000*l.*; Skinners, 1000*l.*, and an increased subscription of 500*l.* annually; Leather-sellers, 500*l.*; Carpenters, 500*l.*; Armourers and Braziers, 300*l.*; Tallow Chandlers, 105*l.*; Scriveners, 105*l.*; Stationers, 52*l.* 10*s.* annually; Clockmakers, 26*l.* 5*s.* The letter of the President is still under the consideration of the Courts of several Companies, and it is confidently expected that the sum required will be obtained.

A CORRESPONDENT writes:—"Last week you had occasion to refer to the little attention which is given in our schools to the study of geography, and the consequent withdrawal of certain medals which were awarded by the Royal Geographical Society to the most proficient candidates from the public schools. The superiority of our Continental neighbours in this matter will be apparent to any one who visits the Educational Section of the International Exhibition, which has been so well organised in the new Technical Schools by Mr. R. Cowper. Here a society of teachers, known as the Brothers of the Christian Schools, exhibit a number of hypsometrical wall maps and physical atlases admirably calculated to give correct notions of the real configuration of the earth's surface. There is also a collection of small models in relief, some of local topographical interest, and others of general utility as illustrating the definitions and leading facts of physical geography. These maps and reliefs were all made by a member of the Society—Brother Alexis—who has successfully striven, in France and Belgium, to introduce rational methods into the teaching of political and physical geography. The interest taken in this subject by students in the schools and colleges of the Brothers in France is shown by a large number of albums of maps sent in by them. It is interesting to notice that many of the local maps were drawn from surveys made by the students under the direction of their teachers. Such field-work is decidedly the best for advanced students, and is sure to be done *con amore*. A second collection sent from the Collegiate Schools of the same Society in Belgium will be found in the Belgian Court. The teachers of those of our public schools from which geography has not been absolutely ostracised will do well to carefully examine the physical maps and reliefs of the Brothers, as well as the cartographical work of their students. The result must be an improvement in our methods. If the Educational Exhibition does no more than raise the standard of geographical teaching throughout the country, it will have served a useful purpose."

IT is worthy of note that the General Assembly of the Free Church of Scotland have approved of the establishment of a Chair of Natural Science in their Glasgow Theological College.

THE Council of the Hartley Institute, Southampton, have issued a circular with reference to the Geological Survey of Hampshire and the Isle of Wight. The Council think that it would be of great service to the landowners and inhabitants of the county that the Geological Survey should now be revised on the map of the 6-inch scale. This revision of the work previously carried out on the old 1-inch map would enable many errors in detail in the former work to be corrected, and would give the county a much more valuable and detailed geological map, the sheet of which relating to any parish could be had separately by paying the cost of the map and the cost of colouring. At present this is not to be obtained, nor is it likely to be obtainable unless the Government can be induced to survey the county, geologically, on the map of the 6-inch scale as some other counties have been surveyed. This work, taking the old Survey as the basis, would not be expensive. The Hartley Council intend to request the members of the House of Lords connected with the county, and the representatives of the county and boroughs of Hampshire in the House of Commons, to urge the Government to revise the County Geological Survey upon the 6-inch map. We trust the Government will readily accede to the request; a minute revision of this geologically interesting district would be of great service to science.

ONE afternoon, *Science* states, during the recent cruise of the *Albatross* in the Caribbean Sea, several boobies were flying around the ship, and finally one of them alighted on the forecastle, when he was caught by one of the men, who, after amusing himself and his shipmates a while, tossed it overboard, expecting it would take itself off as quickly as possible; but, to their surprise, it returned immediately, alighting on the rail, where nearly every man of the crew had congregated to watch its performance. It did not seem to be distressed in any way, and went deliberately to work rearranging its plumage, which had been somewhat ruffled by handling, calmly surveying the noisy crowd of men gathered around it. They tried to feed it, offering everything that could be found, but nothing seemed to suit its taste. It would not submit quietly to being handled, but made no attempt to fly away; and, although tossed overboard six times during the afternoon, it returned as often, invariably alighting in the same place among the men, where it finally took up its quarters for the night, remaining till six o'clock the next morning, when it left without ceremony, and was not afterwards seen.

THE Esposizione Generale Italiana was opened at the end of April in Turin, and at the end of May the inauguration of the International Electrical Exhibition took place. Prof. Ferraris is the director of the International Department. A good display of meteorological instruments has been made in a tower belonging to the front monumental entrance. An inscription states that the Italians having inaugurated meteorology in the world are anxious to show the progress which this department of knowledge has made in its native country. The extent of the exhibition is about 1 kilometre by 450 metres on the banks of the Po, in the old gardens of Valentino Castle, where Beccaria executed his celebrated experiments on thunderstorm phenomena about 100 years ago.

A VIOLENT shock of earthquake occurred on the night of May 19 at the Island of Kishm, near the mouth of the Persian Gulf. No less than twelve villages were destroyed. Two hundred people were killed, and many others injured. Kishm is the largest island in the Persian Gulf, and is surrounded by many smaller islands. It is seventy miles long, and averages twelve miles broad. The population, chiefly Arabs, number about five thousand.

THE latest news from the Sagastyr Meteorological Station on the Lena, published by the St. Petersburg *Izvestia*, is dated November 25. The expedition remained to winter a second year, and will continue its observations until the present month. Last summer M. Eigner made the survey of two branches of the Lena, from the place where Capt. De Long landed on his sad journey. Dr. Bunge revisited the place where the Adams mammoth was found, and mapped also the place where De Long perished with his companions. The summer was altogether very cold, the average temperature of the three summer months being only 3°·25 C. The sky was cloudy throughout, and fogs were nearly continuous. The lowest temperature observed last winter was on February 7, when the thermometer fell to − 52°·3 C.

THE East of Scotland Union of Naturalists' Societies holds its first annual meeting at Dundee to-morrow and Saturday.

MR. ARCHIBALD BARR, B.Sc., C.E., "Young" Assistant to Dr. James Thomson, F.R.S., Professor of Civil Engineering and Mechanics in the University of Glasgow, has been appointed Professor of Civil and Mechanical Engineering in the Yorkshire College.

MR. W. PHILLIPS, F.L.S., of Shrewsbury, has in preparation "A Manual of the British Discomycetes, with Descriptions of all the Species of Fungi hitherto found in Britain, included in the Family, and Illustrations of the Genera."

THE last volume of the *Memoirs* of the Ethnographical Section of the Russian Geographical Society (vol. xii.) contains a rich collection of Russian folk-lore collected in the Samara province on the Volga, by D. N. Sadovnikoff.

THE additions to the Zoological Society's Gardens during the past week include a Himalayan Bear (*Ursus tibetanus*) from

North India, presented by Lieut. E. A. P. Hobday; three Black-eared Marmosets (*Hapale penicillata* ♂ ♂ ♂) from South-East Brazil, presented by Mr. H. F. Makins, F.Z.S.; a Purple-faced Monkey (*Semnopithecus leucoprymnus* ♀) from Ceylon, presented by Mr. J. W. Dring; a Common Heron (*Ardea cinerea*), British, presented by Mr. T. E. Gunn; a Leach's Laughing Kingfisher (*Dacelo leachi*) from Queensland, presented by Dr. Carl Lumholtz; a Laughing Kingfisher (*Dacelo gigantea*) from Australia, presented by Mr. E. R. Oliver; a Great Grey Shrike (*Lonius excubitor*), British, presented by Mr. J. Pratt, F.Z.S.,; a Spotted Bower Bird (*Chlamydodera maculata*) from Australia, presented by Lieut.-Col. W. Hill James; four River Frogs (*Rana fortis*) from Germany, presented by Mr. G. A. Boulenger, F.Z.S., a Green Turtle (*Chelone viridis*) from West Indies, presented by Mr. J. Wyan Thomas; a Tarantula Spider from Brazil, presented by Mr. C. A. Craven, C.M.Z.S.; a Common Boa (*Boa constrictor*) from South America, deposited; a Chimpanzee (*Anthropopithecus troglodytes* ♀), a Bosman's Potto (*Perodicticus potto* ♂) from West Africa, a Duyker-bok (*Cephalophus mergens* ♀) from South Africa, a Ring-tailed Coati (*Nasua rufa*) from South America, two Blood-stained Finches (*Carpodacus hæmorrhous*) from Mexico, a Snow Bunting (*Plectrophanes nivalis*), North European; an Angola Vulture (*Gypohierax angolensis*) from West Africa, a Guatemalan Amazon (*Chrysotis guatemalæ*) from Central America, four Elegant Grass Parrakeets (*Euphema elegans*) from South Australia, two Wild Ducks (*Anas boschas*), two Call Ducks (*Anas boschas*, var.), two Common Wigeon (*Mareca penelope*), two Common Pintails (*Dafila acuta*), six Common Teal (*Querquedula crecca*), two Muscovy Ducks *Coirina moschata*), European, two Mandarin Ducks (*Æx galericulata*) from China, purchased; a Common Wombat (*Phascolomys wombat*) from Tasmania, received in exchange.

OUR ASTRONOMICAL COLUMN

THE TOTAL SOLAR ECLIPSE OF 1889, JANUARY 1.—The general circumstances of most of the total eclipses of the sun more or less available for physical observations before the close of the present century have been already described in this column: it remains, however, to make reference to that which will take place on January 1, 1889, and which will be total in the western part of the United States.

The central eclipse commences on the North Pacific Ocean in about longitude 178° E. and latitude 53° N.; it occurs with the sun on the meridian in 137° 57′ W. and 36° 42′ N., and ends in about 95° W. and 52° 15′ N. It strikes the American coast in the State of California in latitude 38° 50′, and the town of Hamilton, according to our approximate computation, would appear to be upon the central line: here the middle of totality occurs at 1h. 40m. 34s. local mean time, with the sun at an altitude of nearly 25°, and the duration of the total phase is 2m. 4s. The important observatory lately established on Mount Hamilton is outside of the zone of totality, the magnitude of the eclipse at that station being 0·98, and the middle at 1h. 45m. The following are points upon the line of central eclipse:—

Longitude	112° 34′ W.	Latitude	43° 15′ N.	Sun's altitude	15°·9
,,	106 14 ,,	,,	46 15 ,,	,,	10·2
,,	100 21 ,,	,,	49 9 ,,	,,	5·0

It will be seen that the sun will set totally eclipsed on British territory after the total phase has crossed the Assiniboine River and the southern extremity of Lake Winnipeg.

The second total solar eclipse in 1889 was described in NATURE in June 1877. It will be visible in Martinique, St. Lucia and Barbados, but with the sun at a low elevation, totality continuing about one minute and three-quarters, and will meet the coast of Africa in Angola in about 10° south latitude, where the total phase will have a duration of 3m. 30s., the sun at an altitude of 56°.

VARIABLE STARS.—In 1859 Hencke of Driesen drew attention to a star in Carrington's Redhill Catalogue which he had found to be variable. It is No. 1902, and was observed on three nights in March and April 1856, the magnitude being twice noted 9·5 and once 10·5. Hencke had observed it 8m. at the time he wrote, but believed it had probably been invisible with his means for some years previously. His notice appears in Peters' *Zeitschrift für populare Mittheilungen aus dem Gebiete der Astronomie*, &c., vol. i. p. 131. The star is not found in the catalogues of Fedorenko or Schwerd. Its approximate position for the beginning of 1885 is in R.A. 12h. 44m. 49s., N.P.D. 7° 39′·8.

At p. 150 of the Redhill Catalogue Carrington mentions that Oeltzen's No. 515, a seventh magnitude once observed by Schwerd had been looked for ineffectually. Oeltzen had re-examined his reduction of the observation which was made at Speyer on October 19, 1826, and found it correct. The star's place for 1885 is in R.A. 8h. 27m. 54s., N.P.D. 6° 52′·7. Close to this position there is a star in Fedorenko's catalogue from Lalande's observations (Nos. 1305-6) which is once called 8m., and once 5·6, the observations having been apparently made on March 19 and 20, 1790. It is 6m. in Groombridge, and 7m. in the *Durchmusterung* and in the Radcliffe Catalogue. Perhaps the discordance in Lalande's published estimates is occasioned by a misprint, and unfortunately there are several obvious errors of this kind in the catalogue deduced from his observations. The star in question is Groombridge 1431.

While writing upon polar variables we may once more refer to Bradley 396, R.A. (1885) 2h. 53m. 58s., N.P.D. 8° 58′·6, which, unless the existence of very improbable errors of estimation in the various catalogues is admitted, would appear to vary between the fifth and seventh magnitudes at the least, and there is a suspicion that the period may not be long.

A minimum of χ Cygni was due on May 22, and a maximum may be expected about November 16; from three determinations Schmidt found that the minimum preceded the maximum 178 days. The average period since 1877 has been 408½ days. The variable is the true χ (Bayer) Cygni, not the 17 Cygni of the catalogues.

GEOGRAPHICAL NOTES

THE fifth fascicule of A. E. Nordenskjöld's "Popular Scientific Appendix to the Voyage of the *Vega*" ("Studier och Forskningar föranledda af mine resor i höga norden") will be most welcome to the general reader, and we hope it may be translated into English. It contains a profusely illustrated, lively sketch, by M. Hans Hildebrand, on art among lower primitive populations. The drawings of the Chukches are especially remarkable. Caravans of sledges drawn by reindeer or by dogs, hunting scenes, splitting drift-wood, and sea-hunting, are most interesting, and not the slightest mistake is possible as to what the Chukche artist intended to represent. The Chukches are as successful, too, in drawing subjects less known to them, such as the *Vega* at its winter-quarters, or two men of the crew exercising in fencing. The most remarkable piece is that given to Baron Nordenskjöld by Lord Walsingham, which is reproduced by means of photography. The original is drawn on walrus-skin, and represents on the borders of the skin the shores with their hills, Chukche settlements, and a variety of scenes from Chukche life on shore; while the interior contains a variety of scenes from sea-hunting, harpooned whales pretty well represented with their waterspouts, ships, boats, and so on. The Europeans, sometimes with umbrellas, sometimes fighting with Chukches, are perfectly recognisable. The engravings showing the carvings in bone that are made by Chukches and Esquimaux are also very interesting, whilst other drawings allow us to compare the Northern primitive art with the art of Boshmans and North American Indians. M. Hildebrand's remarks on the art of prehistoric man and his parallels with the Normannic drawings—also well illustrated—will be equally attractive to the general reader. The same fascicule contains the first pages of a paper on the life of insects in Arctic regions, by M. Christopher Aurivillius.

THE *Bolletino* of the Italian Geographical Society for May contains a brief account of Signor Maurizio Buonfanti's late expedition across North Africa. The traveller, leaving Tripoli early in the month of April 1881, proceeded first in the direction of Lake Chad, mainly along the route already followed by Denham and Clapperton, Barth, Rohlfs, and other modern explorers. His chief object was to penetrate into the hitherto unexplored region stretching south from Adamawa, which territory was reached by the direct road from Kuka on Lake Chad through

Dikoa to Doloo. But a further advance in this direction was prevented by the disturbed state of the frontiers between Bornu and Adamawa. Buonfanti was consequently compelled to retrace his steps to Kuka, whence he turned westwards along the route recently opened by Lieut. Massari to Kano. After some trips to Yakoba and other little-known parts of Sokoto, he made his way through Gando to the Niger at Say, about midway between Timbuktu and the Binue confluence. Here he turned north, and for the first time *ascended* the Niger as far as Timbuktu. This feat, hitherto supposed to be impossible, was performed *in the dry season*, and the problem thus successfully solved possesses considerable geographical and commercial importance in connection with the attempts now being made to establish regular lines of water communication between Western and Central Sudan and the Gulf of Guinea. From Timbuktu the route lay through the States of Massina and Bambarra to the almost unknown territory of Tombo, the attempt to explore which region ended in disaster. Attacked in the Sanghi district by the natives, the expedition was plundered and almost completely dispersed, being reduced from an escort of 250 to six persons. Thus reduced to the greatest straits, the traveller was driven eastwards, and after enduring fearful sufferings reached the Bussanga country north of Dahomey. Here he fortunately came upon a Roman Catholic mission, which provided him with the means of continuing his journey southwards to the coast of Guinea. He arrived at Lagos on March 5, 1883, having lost all his scientific collections during the disastrous journey through Tembo.

WE received last year complete reports of the state of the ice around Greenland, from Nordenskjöld, and in the Siberian Seas, from Hovgaard, but no report as to the conditions around Spitzbergen. As complete reports of the state and conditions of the ice in the various Arctic seas from year to year will greatly tend to assist glacialists in their researches and future Polar travellers, we publish some particulars furnished by the well-known Arctic hunter, Capt. M. E. Arnesen, of Tromsö, of his voyages in the Spitzbergen seas last summer :—Leaving Tromsö on April 21, he encountered the ice on April 28 in lat. 68° 28′ N. and long. 41° 18′ E. On May 4 the first seal was shot in lat. 68° 50′ N. and long. 42° 10′ E. A storm clearing the ice away, he was able to sail as far as 69°. Here a large ice-field stretched west-north-west as far as lat. 69° 55′ N. and 44° 30′ E., where it curved in a north-easterly and easterly direction. During the fifteen years Capt. Arnesen has sailed in the Arctic seas he never experienced such an early and warm spring. The heat was at times quite oppressive. On the night of July 14 he rounded South Cape at Spitzbergen. The ice lay towards Whales Point, close to the western shore. The Thousand Islands were on July 16 entirely surrounded with ice, stretching about a mile out to sea on the west side. From High Rocks an ice-field runs to the south-south-west. The wind was generally northerly and light, with alternating fogs and clear weather. Deicrow's Sound was entirely free from ice, but, at Black Point, passage between Halfmoon and the other islands was impossible. Encountering the ice on July 20, west of Whales Point, he found no change in its state. On July 22 the edge of the ice was lying from High Rocks to the southern point of Hope Island. For two days a thick fog prevailed. On July 24 the southern point of Hope Island was passed, where close ice stretched south-south-west. The wind was during this week slight, but came alternately from all quarters, sometimes with rain and fog. On July 28 the current set the ice southwards, so that the Thousand Islands were in open water, and towards Hope Island only a few floes were seen. The Halfmoon Islands were in clear water. On the 29th the wind fell, "ice-blink," *i.e.* the reflection of new ice in the sky, being seen to the eastward. On the 30th compact ice was encountered south of Ryk Vs's Islands. On July 31 Whales Point was found free from ice. On August 4 the country at the mouth of Walter Thyrnen Strait was perfectly free from ice, only old glaciers being visible on the mountains. The grass was quite out. The north-eastern part of Hans Foreland forms a great low plateau with good grazings for the reindeer, where large herds are found. The reindeer were in a very good condition, a circumstance which further proves the early and mild spring of last year. On the afternoon of August 6 the temperature in the shade was 12° C., and that of the surface of the water 9° C. On the night of the 17th a little snow fell in the mountains. An old ox, castrated and marked in the ear, was shot. It was believed to be one of those which escaped from Nordenskjöld at Mossel Bay in 1872. East of Hans Foreland and Barents Land there was then no trace of ice; in fact the sea ran mountains high on that side.

THE last volume of the *Memoirs of the Russian Geographical Society* (vol. xii. No. 4) contains the "Memoirs of the Interpreter Otano Kigoro on Corea," translated from the Japanese by M. Dmitrevsky. The author was interpreter of the Corean language on the Tsousima Island, and compiled his book in 1794 on information gathered from Corean officials, as also from Chinese and Japanese works on Corea. The Russian translator of this book has added to it most valuable information gathered especially from the great Corean Code, published in 1785 (Da-dyang-tun-byang), which contains a detailed description of Corea, as well as from several other Chinese and European works, such as the "History of the Corean Church," by Dallet. The extracts from the Corean Code are especially numerous and of great value. The work of Kigoro contains interesting descriptions of the "Customs at the Court," the provincial administration, the geography of Corea, its inhabitants, their customs, habitations, food, and agriculture, as also notes on the Corean administration, army, and literature.

THE last number of the Irkutsk *Izvestia* contains an interesting paper by Dr. Martianoff on his journeys in the north-eastern part of the Minusinsk district. In a note on antiquities in the basin of the Yenisei M. Bogolubsky mentions, among others, that on the Ouzynjoul gold-washings on a river of the same name belonging to the basin of the Abakan, implements consisting of a red copper nail, a marmor ring, and a knife and an arrow of bone, were found, together with bones of mammoth, rhinoceros, *Bos urus*, horse, antelope, wolf, and domestic animals, at a depth of from ten to thirteen feet. If implements from different levels were not confounded together, this find would surely be of great value. We notice also a note on a little-known subject, the "Scythic disease" among Aleutes and Kamchadales, by M. Grebnitzky, and another on the rapids of the Angara, with a map.

THE prospects of a trade between Europe and Siberia, through the Kara Sea, do not seem to be cheering. According to a private correspondent in Moscow, the steamer *Dallmann*, built at the Vulcan Engineering Works, Stettin, for towing on the Yenisei, lies at the trading station, Strelka, 75 versts south of Yeniseisk, where also two iron lighters of 5000 poods carrying capacity, and a wooden one capable of carrying 2000 poods, as well as two steam launches, now are. They are all to be sold, along with the buildings, depots, and factories at Strelka and the stations not far from the mouth of the Yenisei, about 800 versts north of Turukhansk. At the latter station large quantities of wheat, rye, and oats have been collected with a view to being exported to Europe. There seems at present little probability of their ever reaching their destination. During the last five or six years the steamer *Louise* has only twice succeeded in reaching the Yenisei and returning with cargo to Europe; three times the vessel failed in the attempt.

THE last issue of the *Journal of the Ceylon Branch of the Royal Asiatic Society* (Colombo, 1883) is wholly occupied by a translation of that part of Ibn Batuta's travels relating to Ceylon and the Maldive Islands, accompanied by notes. The account of the customs of the primitive inhabitants of the Maldives is especially interesting.

ON THE NOMENCLATURE, ORIGIN, AND DISTRIBUTION OF DEEP-SEA DEPOSITS[1]

III.

IT remains now to point out the area occupied by the red clay. We have seen how it passes at its margins into organic calcareous oozes, found in the lesser depths of the abysmal regions, or into the siliceous organic oozes or terrigenous deposits. In its typical form the red clay occupies a larger area than any of the other true deep-sea deposits, covering the bottom in vast regions of the North and South Pacific, Atlantic, and Indian Oceans. As above remarked, this clay may be said to be universally distributed over the floor of the oceanic basins; but it only appears as a true deposit at points where the siliceous and calcareous organisms do not conceal its proper characters.

Having now indicated its distribution, we must consider the mode of its formation, and give, in addition, a concise descrip-

[1] A Paper read before the Royal Society of Edinburgh, by John Murray and A. Renard. Communicated by John Murray. Continued from p. 117.

tion of the minerals and of the organic remains which are commonly associated with it. The origin of these vast deposits of clay is a problem of the highest interest. It was at first supposed that these sediments were composed of microscopic particles arising from the disintegration of the rocks by rivers and by the waves on the coasts. It was believed that the matters held in suspension were carried far and wide by currents, and gradually fell to the bottom of the sea. But the uniformity of composition presented by these deposits was a great objection to this view. It could be shown, as we have mentioned above, that mineral particles, even of the smallest dimensions, continually set adrift upon disturbed waters must, owing to a property of sea water, eventually be precipitated at no great distance from land. It has also been supposed that these argillaceous deposits owe their origin to the inorganic residue of the calcareous shells which are dissolved away in deep water, but this view has no foundation in fact. Everything seems to show that the formation of the clay is due to the decomposition of fragmentary volcanic products, whose presence can be detected over the whole floor of the ocean.

These volcanic materials are derived from floating pumice and volcanic ashes ejected to great distances by terrestrial volcanoes, and carried far by the winds. It is also known that beds of lava and of tufa are laid down upon the bottom of the sea. This assemblage of pyrogenic rocks, rich in aluminous silicates, decomposes under the chemical action of the water, and gives rise, in the same way as do terrestrial volcanic rocks, to argillaceous matters, according to reactions which we can always observe on the surface of the globe, and which are too well known to need special mention here.

The detailed microscopic examination of hundreds of soundings has shown that we can always demonstrate in the argillaceous matter the presence of pumice, of lapilli, of silicates, and other volcanic minerals in various stages of decomposition.

As we have shown in another paper,[1] the deposit most widely distributed over the bed of modern seas is due to the decomposition of the products of the internal activity of the globe, and the final result of the chemical action of sea water is seen in the formation of this argillaceous matter, which is found everywhere in deep-sea deposits, sometimes concealed by the abundance of siliceous or calcareous organisms, sometimes appearing with its own proper characteristics associated with mineral substances, some of which allow us to appreciate the extreme slowness of its formation, or whose presence corroborates the theory advanced to explain its origin.

In the places where this red clay attains its most typical development, we may follow, step by step, the transformation of the volcanic fragments into argillaceous matter. It may be said to be the direct product of the decomposition of the basic rocks, represented by volcanic glasses, such as hyalomelan and tachylite. This decomposition, in spite of the temperature approximating to zero (32° F.), gives rise, as an ultimate product, to clearly crystallised minerals, which may be considered the most remarkable products of the chemical action of the sea upon the volcanic matters undergoing decomposition. These microscopic crystals are zeolites lying free in the deposit, and are met with in greatest abundance in the typical red clay areas of the Central Pacific. They are simple, twinned, or spheroidal groups, which scarcely exceed half a millimetre in diameter. The crystallographic and chemical study of them shows that they must be referred to Christianite. It is known how easily the zeolites crystallise in the pores of eruptive rocks in process of decomposition; and the crystals of Christianite, which we observe in considerable quantities in the clay of the centre of the Pacific, have been formed at the expense of the decomposing volcanic matters spread out upon the bed of that ocean.

In connection with this formation of zeolites, reference may be made to a chemical process whose principal seat is the red clay areas, and which gives rise to nodules of manganiferous iron. This substance is almost universally distributed in oceanic sediments, yet it is not so much of the areas of its abundance that we intend to speak as to the fact of its occurrence in the red clay, because this association tends to show a common relation of origin. It is exactly in those regions where there is an accumulation of pyroxenic lavas in decomposition, containing silicates with a base of manganese and iron, such for example as augite, hornblende, olivine, magnetite, and basic glasses, that manganese nodules occur in greatest numbers. In the regions where the sedimentary action, mechanical and organic, is, as it were, suspended, and where, as will appear in the sequel, everything shows an extreme slowness of deposition,—in these calm waters favourable to chemical reactions, ferro-manganiferous substances form concretions around organic and inorganic centres.

These concentrations of ferric and manganic oxides, mixed with argillaceous materials whose form and dimensions are extremely variable, belong generally to the earthy variety or wad, but pass sometimes, though rarely, into varieties of hydrated oxide of manganese with distinct indications of radially fibrous crystallisation. The interpretation to which we are led, in order to explain this formation of manganese nodules, is the same as that which is admitted in explanation of the formation of coatings of this material on the surface of terrestrial rocks. These salts of manganese and iron, dissolved in water by carbonic acid, then precipitated in the form of carbonate of protoxide of iron and manganese, become oxidised, and give rise in the calm and deep oceanic regions to more or less pure ferro-manganiferous concretions. At the same time it must be admitted that rivers may bring to the ocean a contribution of these same substances.

Among the bodies which, in certain regions where red clay predominates, serve as centres for these manganiferous nodules, are the remains of Vertebrates. These remains are the hardest parts of the skeleton—tympanic bones of whales, beaks of Ziphius, teeth of sharks; and just as the calcareous shells are eliminated in the depths, so all the remains of the larger Vertebrates are absent, except the most resistant portions. These bones often serve as a centre for the manganese iron concretions, being frequently surrounded by layers several centimetres in thickness. In the same dredgings in the red clay areas some sharks' teeth and Cetacean ear-bones, some of which belong to extinct species, are surrounded with thick layers of the manganese, and others with merely a slight coating. We will make use of these facts to establish the conclusions which terminate this paper.

In these red clays there occur in addition the greatest number of cosmic metallic spherules, or chondres, the nature and characters of which we have pointed out elsewhere.[1] We merely indicate their presence here, as we will support our conclusions by a reference to their distribution.

Reviewing, then, the distribution of oceanic deposits, we may summarise thus:—

(1) The terrigenous deposits, the blue muds, green muds and sands, red muds, volcanic muds and sands, coral muds and sands, are met with in those regions of the ocean nearest to land. With the exception of the volcanic muds and sands, and coral muds and sands around oceanic islands, these deposits are found only lying along the borders of continents and continental islands, and in inclosed and partially inclosed seas.

(2) The organic oozes and red clay are confined to the abysmal regions of the ocean basins; a Pteropod ooze is met with in tropical and subtropical regions in depths less than 1500 fathoms, a Globigerina ooze in the same regions between the depths of 500 and 2800 fathoms, a Radiolarian ooze in the central portions of the Pacific at depths greater than 2500 fathoms, a Diatom ooze in the Southern Ocean south of the latitude of 45° south, a red clay anywhere within the latitudes of 45° north and south at depths greater than 2200 fathoms.

Conclusions.—All the facts and details enumerated in the foregoing pages point to certain conclusions which are of considerable geological interest, and which appear to be warranted by the present state of our investigations.

We have said that the debris carried away from the land accumulates at the bottom of the sea before reaching the abysmal regions of the ocean. It is only in exceptional cases that the finest terrigenous materials are transported several hundred miles from the shores. In place of layers formed of pebbles and clastic elements with grains of considerable dimensions, which play so large a part in the composition of emerged lands, the great areas of the ocean basins are covered by the microscopic remains of pelagic organisms, or by the deposits coming from the alteration of volcanic products. The distinctive elements that appear in the river and coast sediments are, properly speaking, wanting in the great depths far distant from the coasts. To such a degree is this the case that in a great number of soundings, from the centre of the Pacific for example, we have not been able to distinguish mineral particles on which the mechanical action of water had left its imprint, and quartz is so rare that it may be said to be absent. It is sufficient to indicate these facts in order to make apparent the profound differences which separate the de-

[1] "On Cosmic and Volcanic Dust," *Proc. Roy. Soc. Edin.*, 1883-84.

[1] "On Cosmic and Volcanic Dust," *Proc. Roy. Soc. Edin.*, 1883-84.

posits of the abysmal areas of the ocean basins from the series of rocks in the geological formations. As regards the vast deposits of red clay, with its manganese concretions, its zeolites, cosmic dust, and remains of Vertebrates, and the organic oozes which are spread out over the bed of the Central Pacific, Atlantic, and Indian Oceans, have they their analogues in the geological series of rocks? If it be proved that in the sedimentary strata the pelagic sediments are not represented, it follows that deep and extended oceans like those of the present day cannot formerly have occupied the areas of the present continents, and as a corollary the great lines of the ocean basins and continents must have been marked out from the earliest geological ages. We thus get a new confirmation of the opinion of the permanence of the continental areas.

But without asserting in a positive manner that the terrestrial areas and the areas covered by the waters of the great ocean basins have had their main lines marked out since the commencement of geological history, it is, nevertheless, a fact, proved by the evidence derived from a study of the pelagic sediments, that these areas have a great antiquity. The accumulation of sharks' teeth, of the ear-bones of Cetaceans, of manganese concretions, of zeolites, of volcanic material in an advanced state of decomposition, and of cosmic dust, at points far removed from the continents, tend to prove this. There is no reason for supposing that the parts of the ocean where these Vertebrate remains are found are more frequented by sharks or Cetaceans than other regions where they are never or only rarely dredged from the deposits at the bottom. When we remember also that these ear-bones, teeth of sharks, and volcanic fragments, are sometimes incrusted with two centimetres of manganese oxide, while others have a mere coating, and that some of the bones and teeth belong to extinct species, we may conclude with great certainty that the clays of these oceanic basins have accumulated with extreme slowness. It is indeed almost beyond question that the red clay regions of the Central Pacific contain accumulations belonging to geological ages different from our own. The great antiquity of these formations is likewise confirmed in a striking manner by the presence of cosmic fragments, the nature of which we have described ("On Cosmic and Volcanic Dust," *Proc. Roy. Soc. Edin.*). In order to account for the accumulation of all the substances in such relatively great abundance in the areas where they were dredged, it is necessary to suppose the oceanic basins to have remained the same for a vast period of time.

The sharks' teeth, ear-bones, manganese nodules, altered volcanic fragments, zeolites, and cosmic dust are met with in greatest abundance in the red clays of the Central Pacific, at that point on the earth's surface farthest removed from continental land. They are less abundant in the Radiolarian ooze, are rare in the Globigerina, Diatom, and Pteropod oozes, and they have been dredged only in a few instances in the terrigenous deposits close to the shore. These substances are present in all the deposits, but owing to the abundance of other matters in the more rapidly forming deposits their presence is masked, and the chance of dredging them is reduced. We may then regard the greater or less abundance of these materials, which are so characteristic of a true red clay, as being a measure of the relative rate of accumulation of the marine sediments in which they lie. The terrigenous deposits accumulate most rapidly, then follow in order Pteropod ooze, Globigerina ooze, Diatom ooze, Radiolarian ooze, and, slowest of all, red clay.

From the data now advanced, it appears possible to deduce other conclusions important from a geological point of view. In the deposits due essentially to the action of the ocean, we are at once struck by the great variety of sediments which may accumulate in regions where the external conditions are almost identical. Again, marine faunas and floras, at least those of the surface, differ greatly, both with respect to species and to relative abundance of individuals, in different regions of the ocean; and as their remains determine the character of the deposit in many instances, it is legitimate to conclude that the occurrence of organisms of a different nature in several beds is not an argument against the synchronism of the layers which contain them.

The small extent occupied by littoral formations, especially those of an arenaceous nature, shown by our investigations, and the relatively slow rate at which such deposits are formed along a stable coast, are matters of importance.

In the present state of things there does not appear to be anything to account for the enormous thickness of the clastic sediments making up certain geological formations, unless we consider the exceptional cases of erosion which are brought into play when a coast is undergoing constant elevation or subsidence.

Great movements of the land are doubtless necessary for the formation of thick beds of transported matter like sandstones and conglomerates.

In this connection may be noted the fact that in certain regions of the deep sea no appreciable formation is now taking place. Hence the absence, in the sedimentary series, of a layer representing a definite horizon must not always be interpreted as proof either of the emergence of the bottom of the sea during the corresponding period, or of an ulterior erosion. Arenaceous formations of great thickness require seas of no great extent and coasts subject to frequent oscillations, which permit the shores to advance and retire. Along these, through all periods of the earth's history, the great marine sedimentary phenomena have taken place.

The continental geological formations, when compared with marine deposits of modern seas and oceans, present no analogues to the red clays, Radiolarian, Globigerina, Pteropod, and Diatom oozes. On the other hand, the terrigenous deposits of our lakes, shallow seas, inclosed seas, and the shores of the continents, reveal the equivalents of our chalks, greensands, sandstones, conglomerates, shales, marls, and other sedimentary formations. Such formations as certain Tertiary deposits of Italy, Radiolarian earth from Barbados, and portions of the Chalk where pelagic conditions are indicated, must be regarded as having been laid down rather along the border of a continent than in a true oceanic area. On the other hand, the argillaceous and calcareous rocks recently discovered by Dr. Guppy in the upraised coral islands in the Solomon Group are nearly identical with the Pteropod and Globigerina oozes of the Pacific.

Regions situated similarly to inclosed and shallow seas and the borders of the present continents appear to have been, throughout all geological ages, the theatre of the greatest and most remarkable changes; in short, all, or nearly all, the sedimentary rocks of the continents would seem to have been built up in areas like those now occupied by the terrigenous deposits, which we may designate "*the transitional or critical area of the earth's surface.*" This area occupies we estimate, about two-eighths of the earth's surface, while the continental and abysmal areas occupy each about three-eighths.

During each era of the earth's history the borders of some lands have sunk beneath the sea and been covered by marine sediments, while in other parts the terrigenous deposits have been elevated into dry land, and have carried with them a record of the organisms which flourished in the sea of the time. In this transitional area there has been throughout a continuity of geological and biological phenomena.

From these considerations it will be evident that the character of a deposit is determined much more by distance from the shore of a continent than by actual depth; and the same would appear to be the case with respect to the fauna spread over the floor of the present oceans. Dredgings near the shores of continents, in depths of 1000, 2000, or 3000 fathoms, are more productive both in species and individuals than dredgings at similar depths several hundred miles seawards. Again, among the few species dredged in the abysmal areas furthest removed from land, the majority show archaic characters, or belong to groups which have a wide distribution *in time* as well as over the floor of the present oceans. Such are the Hexactinellida, Brachiopoda, Stalked Crinoids and other Echinoderms, &c.

As already mentioned, the transitional area is that which now shows the greatest variety in respect to biological and physical conditions, and in past time it has been subject to the most frequent and the greatest amount of change. The animals now living in this area may be regarded as the greatly modified descendants of those which have lived in similar regions in past geological ages, and some of whose ancestors have been preserved in the sedimentary rocks as fossils. On the other hand, many of the animals dredged in the abysmal regions are most probably also the descendants of animals which lived in the shallower waters of former geological periods, but descended into deep water to escape the severe struggle for existence which must always have obtained in those depths affected by light, heat, motion, and other conditions. Having found existence possible in the less favourable and deeper water, they may be regarded as having slowly spread themselves over the floor of the ocean, but without undergoing great modifications, owing to the extreme uniformity of the conditions and the absence of competition. Or we may suppose that, in the depressions which have taken place near coasts, some species have been gradually carried down to deep water, have accommodated themselves to the new conditions, and have gradually migrated to the regions far from land. A few species may thus have migrated to the deep sea during each geological period. In this way the origin and distribution of the deep-sea fauna in the present oceans may in some measure be explained. In like manner, the pelagic fauna and flora of the ocean is most probably derived originally from the shore and shallow water. During each period of the earth's history a few animals and plants have been carried to sea, and have ultimately adopted a pelagic mode of life.

Without insisting strongly on the correctness of some of these deductions and conclusions, we present them for the consideration of naturalists and geologists, as the result of a long, careful, but as yet incomplete, investigation.

THE FIXED STARS[1]

THERE is no science which has so long and so continuously occupied the thoughtful minds of successive generations of men as has astronomy; and of its various branches there is one which has for all ages possessed a special fascination, viz. that of sidereal astronomy.

There has ever been a desire to burst aside the constraints imposed upon our research by the distances of space, to pass from the study of the planets of our solar system to that of the suns and galaxies that surround us, to determine the position and relative importance of our own system in the scheme of the universe and the whence we have come and the whither we are drifting through the realms of space.

Questions without number crowd upon the mind. The galaxy or Milky Way—what is it? Is our sun one of its members? What is the shape of that galaxy? What are its dimensions? What is the position of our sun in it?

The star-clusters—what are they, these wondrous aggregations where hundreds and even thousands of suns may be seen in the limited field of view of a powerful telescope? Are these clusters galaxies? Have these suns real dimensions comparable with those of our sun, and is it distance alone that renders their light and dimension so insignificant to the naked eye? Or are the real dimensions of the clusters small as compared with our galaxy? Are their component suns but the fragments of some great sun that has been shattered by forces unknown to us, or have they originated from chaotic matter, which, instead of forming one great whirlpool and condensing by vortex action into one great sun, has been disturbed into numerous minor vortices, and so become rolled up into numerous small suns?

The nebulæ—what are they? Are they too condensing into clusters or stars, or will their ghost-like forms remain for ever unchanged amongst the stars? or do they play some part in the scheme of nature of which we have as yet no conception?

These and many others are the questions which press on the ardent mind that contemplates the subject; and there arises the intense desire to answer such questions, and where facts are wanting to supply facts by fancy. The history of deep and profound thought in some of these subjects goes back through 2000 years, but the history of real progress is but as of yesterday. The foundation of sidereal astronomy may be said to have begun with the art of accurate observation. Bradley's meridian observations at Greenwich about 1750, his previous discovery of the aberration of light in 1727, and Herschel's discovery of the binary nature of double stars, his surveys of the heavens, and his catalogues of double stars—these are solid facts, facts that have contributed more to the advancement of sidereal astronomy than all the speculations of preceding centuries. They point to us the lesson that "art is long and life is short," that human knowledge, in the slow developing phenomena of sidereal astronomy, must be content to progress by the accumulating labours of successive generations of men, that progress will be measured for generations yet to come more by the amount of honest, well-directed and systematically-discussed observation than by the most brilliant speculation, and that in observation concentrated systematic effort on a special thoughtfully-selected problem will be of more avail than the most brilliant but disconnected work.

I hope that no one present thinks from what I have said that I undervalue the imaginative fervid mind that longs for the truth, and whose fancy delights to speculate on these great subjects. On the contrary, I think and I believe that without that fervid mind, without that longing for the truth, no man is fitted for the work required of him in such a field—for it is such a mind and such desires that alone can sweeten the long watches of the night, and transform such work from drudgery into a noble labour of love.

It is for like reasons that I ask you to leave with me the captivating realms of fancy this evening, and to enter the more substantial realms of fact. And if at any time I should become too technical or dry I beg that you too will remember the noble problems for the solution of which such dry work is undertaken.

We suppose ourselves then face to face with all the problems of sidereal astronomy to which I have hastily referred—the human mind is lost in speculation, and we are anxious to establish a solid groundwork of fact.

Now what in such circumstances would be the instinct of the scientific mind?

The answer is unquestionable—viz. to measure—and no sooner were astronomical instruments made of reasonable exactness than astronomers did begin to measure, and to ask, are the distances of the fixed stars measurable?

I should like to have given a short history of the early attempts of astronomers to measure the distance of a fixed star. I had indeed prepared such an account, but I remembered that there is in this theatre a relentless clock that has curbed the exuberant verbosity of many a lecturer before me, and I found that if the real subject-matter of this evening's lecture were to be reached and dealt with before 10 o'clock, I must pass over this earlier history, instructive and interesting though it is, and come at once to the time when the long baffled labours of astronomers began to be crowned with success.

Perhaps I cannot summarise it better than in the words of Sir John Herschel. In one of his presidential addresses he says:—"The distance of every individual body in the universe from us is necessarily admitted to be finite. But though the distance of each particular star be not in strictness infinite, it is yet a real and immense accession to our knowledge to have *measured* it in any one case. To accomplish this has been the object of every astronomer's highest aspirations ever since sidereal astronomy acquired any degree of precision. But hitherto it has been an object which, like the fleeting fires that dazzle and mislead the benighted wanderer, has seemed to suffer the semblance of an approach only to elude his seizure when apparently just within his grasp, continually hovering just beyond the limits of his distinct apprehension, and so leading him on in hopeless, endless, and exhausting pursuit."

Those who have read the history of exact astronomy from the days of Flamsteed—*i.e.* from 1689—down to 1832, will understand how exactly these words of Sir John Herschel describe the position of the problem.

But these laborious pursuits, like all honest researches in quest of truth, were not without reward, even though the immediate object in view was not attained. Bradley was rewarded by his great discovery of aberration, and Sir William Herschel by the greatest of his great discoveries, the binary nature of double stars, when engaged in vain attempts to measure the distance of a fixed star. Time forbids that I should tell more of this instructive story—for the story of failure is often fully as instructive as that of success—and I must begin the history of our problem between 1832 and 1842, when success was first attained.

But before I begin it will save both time and circumlocution if I define a word that we must frequently use—viz. the word parallax.

Here on the table is a large ball representing the sun, and here, travelling on a circular railway round the larger ball, is a smaller ball which we shall suppose to represent the earth. The larger ball is suspended from the ceiling by a white string, the small ball is suspended from the same point by a red string. At the far end of the white string you can suppose a star whose true direction is represented by this white string, and whose apparent direction as looked at from the earth is represented by the red string. Now if the star is within a measurable distance, the red string which indicates the star's apparent direction as seen from the earth will always be displaced inwards towards the sun. This displacement is called "parallax." It may be defined as the change in the apparent place of a star produced by viewing it from a point other than that of reference. Our point of reference for stars is the sun, and as we view the stars now from one side of the sun, and six months afterwards from a point on the

[1] Lecture on Friday evening, May 23, at the Royal Institution, "On Recent Researches on the Distances of the Fixed Stars, and on some Future Problems in Sidereal Astronomy," by David Gill, LL.D., F.R.S., Her Majesty's Astronomer at the Cape of Good Hope.

opposite side of the sun—that is, from two points 186 millions of miles apart—we might expect to find a considerable change in their apparent places.

But previous to 1832 astronomers could not discover with any certainty that such changes were sensible—that, in other words, the red and the white strings met at a point so distant that, as far as they were able to measure, the two strings were practically parallel—or, putting it another way, the stars were so distant that the diameter of the earth's orbit viewed from the nearest star subtended a smaller angle than their instruments could measure. Bradley felt sure that if the star γ Draconis were so near that its parallax amounted to 1″ of arc he would have detected it—that is, if the earth's orbit viewed from γ Draconis measured 2″ in diameter, that is, if it looked as big as a globe one foot in diameter would look if viewed at forty miles distant, he would have detected it. But the real distances of the stars were greater than that.

The time at last arrived when the two great masters of modern practical astronomy, Bessel and Struve, were preparing by elaborate experiment and study for the researches which led to ultimate success. After vain attempts to obtain conclusive results by endeavours to determine the apparent changes in the absolute direction of a star at different seasons of the year, both astronomers had recourse to a method which, originally proposed by Galileo in 1632, was carried out first on a large scale by Sir William Herschel. I shall refer in the first place to the researches of the great Russian astronomer Struve.

Astronomers had sufficiently demonstrated that the distances of the stars were very great, and it was reasonable to argue that as a rule the brighter stars would be those nearest to us. If, therefore, two stars are apparently near each other—the one bright, the other faint—the chances are that in reality they are far apart, though accidentally nearly in a line.

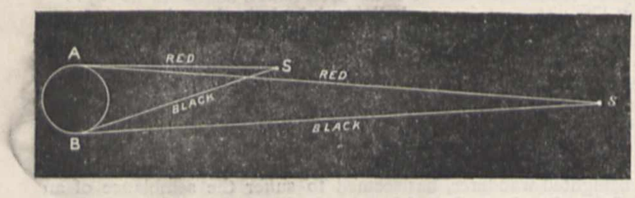


Fig. 1.

If two such stars are represented by S s in Diagram I., they would appear near each other viewed from one side of the earth's orbit at A, but not so near each other viewed from B, the opposite side of the earth's orbit, the red lines obviously indicating the apparent angle between the stars when they are viewed from A, and the black lines the apparent angle when they are viewed from B. Struve selected for the star S the bright star Vega (α Lyræ). From its brilliancy he considered it probably one of our nearest neighbours amongst the stars, and a faint star apparently near it seemed to afford a suitable representative of the really distant star *s*. Struve was careful to ascertain that this comparison star was not physically connected with α Lyræ, and he was able to prove this from the fact that whilst α Lyræ has a small annual motion relative to all neighbouring stars, this motion is not shared by the faint comparison star. Struve was provided with a telescope driven by clockwork to follow the diurnal motion of a star, and thus the hands of the observer were free to make the necessary measures. These were accomplished by an instrument such as I hold in my hands applied to the telescope. This micrometer contains two parallel spider webs each attached to a slide, one slide being moved by one screw, the other by the other screw. The screws are provided with drum-heads divided into 100 parts. One web was placed on the image of α Lyræ, the other upon that of the faint comparison star, and the angle between the stars was thus read off in terms of the number of revolutions and decimals of a revolution of the screws. A number of such observations was made on each night, and the result for each night depended on the mean of the numerous observations made each night.

By observations on ninety-six nights between November 1835 and August 1838, he showed that the distance between α Lyræ and the faint comparison star changed systematically with a regular annual period, and that the maxima and minima of those distances corresponded with the times of the year at which these maxima and minima should occur if the brighter star were really much nearer than the fainter one.

Assuming that the fainter star is at a practically immeasurable distance, Struve showed that α Lyræ had a parallax that amounted to about a quarter of a second of arc, which is equivalent to the statement that a globe whose diameter is equal to that of the earth's orbit—that is, to 186,000,000 of miles—would at the distance of α Lyræ present an apparent diameter of half a second of arc. If you wish to realise this angle, place a globe one foot in diameter at a distance of eighty miles, or look at a coin half the diameter of a silver threepenny piece at a distance of one mile from the eye, and try to measure it.

The great German astronomer, Bessel, was simultaneously engaged in like work at Königsberg. He selected as the object of his researches a very remarkable double star—61 Cygni.

This star had already been the subject of similar researches on his part with much inferior means. He now attacked the problem with the splendid heliometer which had been made for him by Frauenhofer for the purpose. The principle of this instrument I shall presently explain. His reasons for choosing 61 Cygni were that the two components of this star, though not remarkable for brightness—they are just visible to the naked eye—yet have this peculiarity, that they have a remarkably large proper motion, the largest then known, though now surpassed by that of two other stars which I shall afterwards mention. They have an apparent angular motion relative to other stars of more than five seconds of arc per annum.

Struve had argued that if the stars were on the average of similar brightness, those stars which were brightest would probably be those nearest to us, and Bessel, in like manner, argued that if the absolute motions of the stars were similar on the average, those motions which appeared the largest belonged to stars which on the average were nearest to us—just as the motion of a snail could be easily watched at the distance of two or three feet from the eye, but could not be detected except after a long interval, if the animal were a good many yards distant.

Bessel employed two faint comparison stars at right angles to each other with respect to 61 Cygni, and he made two separate series of observations, the first extending from August 1837 to October 1838, the second from October 1838 to March 1840.

Both series confirm each other, and the results deduced separately from the measures of the two comparison stars also agree within very narrow limits. From all the observations combined Bessel found the parallax of 61 Cygni to be 35/100 of a second—a quantity which has been shown by the modern researches of Prof. Auwers and Dr. Ball to be more nearly half a second of arc. Thus at 61 Cygni the diameter of the earth's orbit round the sun would appear of the same size as a globe a foot in diameter viewed at forty miles distance, or of a silver threepenny piece a mile off. But whilst these great masters of astronomy—Struve and Bessel—had been exhausting the resources of their skill in observation, and that of the astronomical workshops of Europe in supplying them with the most refined instruments, a quiet and earnest man had been at work at the Cape of Good Hope, and, without knowing it at the time, had *really made the* FIRST *observations* which afforded strong presumptive evidence of the existence of the parallax of any fixed star.

Henderson occupied the post of Her Majesty's Astronomer at the Cape of Good Hope in 1832 and 1833, and during his brief and brilliant tenure of office there he made, amongst many others, a fine series of meridian observations of α Centauri—a bright and otherwise remarkable double star. When, after his return to England, Henderson reduced these observations, and compared them with the earlier observations of other astronomers, he found that α Centauri had a large proper motion; he was therefore led to examine and see whether his observations gave any indication of an annual parallax. He found that they did so, and not of a small parallax but of one amounting to nearly a second of arc. But it was not till this was confirmed, not only by the observations with the mural circle but by those of the transit instrument also, not only by his own observations but by those of Lieut. Meadows, his assistant, that Henderson ventured to publish his remarkable result.

In the year 1842 it was felt by the astronomical world at large that the problem which hitherto had baffled astronomers had begun to yield, that some approximation to the truth had at last been arrived at with regard to the distance of a fixed star, and it was fit and proper that the Royal Astronomical Society of London should acknowledge the labours of him who had most effectually contributed to this end.

Henderson's results seemed sufficiently convincing, but they depended upon determinations of the absolute place of α Centauri. The experiences of the skilful astronomer Brinkley at Dublin were still fresh in the minds of astronomers. He had arrived by similar though less perfect means at results like those of Henderson; but his results had been proved to be fallacious, though the causes of their being so still remain somewhat inexplicable. In the case of Struve's observations the weight of evidence which he produced and the excellence of his method were admitted, but men were not prepared by experience for accepting as accurate the minute changes of angle which Struve had to measure—nor, I am bound to admit, was the proof afforded by Struve's series of observations so entirely convincing as that afforded by the series of Bessel. Therefore to Bessel the well-earned medal was given, but the labours of Struve and Henderson received high and honourable mention. I quote from the speech of Sir John Herschel in awarding that medal. He says of Henderson's researches on α Centauri:—

"Should a different eye and a different circle continue to give the same result, we must of course acquiesce in the conclusion; and the distinct and entire merit of the *first* discovery of the parallax of *a* fixed star will rest indisputably with Mr. Henderson. At present, however, we should not be justified in anticipating a decision which time alone can stamp with the seal of absolute authority."

So much for Sir John Herschel's officially expressed opinion. I can state now, and as Henderson's successor I do so with pride and pleasure, that a different eye (that of his able and sympathetic successor, Sir Thomas Maclear) fully confirmed Henderson's result with another circle; and further, that Henderson's result has been still further confirmed by additional researches of which I shall presently speak.

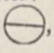
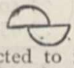
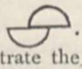
I must now pass over briefly the history of succeeding researches, and indeed it has been so admirably and so recently told within these walls by Dr. Ball that it is quite unnecessary I should enter upon it in detail. The most reliable values arrived at for the parallaxes of the stars of the northern hemisphere are given in the following table, and to these results I shall afterwards refer:—

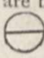
TABLE I.—*Parallaxes of Stars which have been determined in the Northern Heavens with considerable Accuracy*

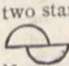
	Magnitude	Proper motion	Parallax
		"	"
61 Cygni	6	5·14	0·50
Lalande 21185 ...	7½	4·75	0·50
α Tauri	1	0·19	0·52
34 Groombridge ...	8	2·81	0·29
Lalande 21258 ...	8½	4·40	0·26
O.Mg. 17415 ...	9	1·27	0·25
σ Draconis	—	1·87	0·25
α Lyræ	1	0·31	0·20
p Ophiuchi	4½	1·0	0·17
α Bootis...	1	2·43	0·13 ?
Groombridge 1830.	7	7·05	0·09
Bradley 3077 ...	6	2·09	0·07
85 Pegasi	6	1·38	0·05

The recent researches referred to in the title of this evening's lecture are some investigations which, in conjunction with a young American friend, Dr. Elkin, who was my guest for two years, I have recently carried out at the Cape of Good Hope.

The instrument employed was a heliometer—my own property—the good qualities of which I had previously tested at Mauritius in 1874 and at the Island of Ascension in 1877.

Now what is a heliometer? It is a telescope of which the object-glass is divided thus , and the two segments so formed can be moved with respect to each other, thus  and .

Here is a model which has been constructed to illustrate the principle of the instrument. You see that when the two segments are brought into what we may call their natural position, thus , that a heliometer differs in no way from an ordinary telescope—its divided lens produces a single image of a point of light, as will be evident from the image of the single artificial disk now on the screen. In optical language, the optical centres of the two segments are in coincidence, and so the images produced by each segment of the lens are in coincidence. But now, if the segments are separated, either segment produces a separate image of the artificial star, and the separation of the images is proportional to the separation of the segments.

Now, to illustrate how this instrument is used in observation, let there be two artificial stars—*a* and *b*. When the optical centres of the segments are in coincidence, we have on the screen—or in the field of view of the telescope—the images of these two stars. By separating the optical centres of the segments thus  we obtain double images of each of the stars *a* and *b*. Now if we turn the direction of the line of motion of the divided segments parallel to the direction of the stars *a* and *b*, and if we separate the lenses sufficiently we can make one of the images of the star *a* coincide with one of the images of star *b*. Similarly if we cross the segments we can bring the second image of star *b* into coincidence with the second image of star *a*, and if we have finely divided scales attached to the slides by which the segments are separated we can read off, in terms of these scales, the amount of this separation, and this separation is obviously twice the angle between the stars *a b*.

There is now upon the screen a photograph from a drawing illustrating the arrangements by which the segments of my heliometer are moved, and showing the scales by which the amount of the movement is measured; and these scales are read off by a powerful microscope from the eye end of the telescope, as in the photograph of the instrument now on the screen.

There is now on the screen a photograph of a drawing of the most perfect heliometer in the world, recently made by Messrs. Repsold of Hamburg for the Observatory of Yale College, New Haven, U.S. That instrument is now under the charge of my young friend, Dr. Elkin, of whom I have already spoken. If then we wish to observe the angle between two stars, it is only necessary to separate the segments of the object-glass by the required amount, to rotate the tube till the line of section of the object-glass is in the line joining the stars, to direct the axis of the telescope to a point in the heavens midway between the two stars under observation, and then we shall find in the field of view the two stars the angle between which we wish to measure. Then by slow and delicate changes in the distance of the optical centres of the segments, whilst the images of the stars are made to pass and repass through each other—thus—we are able to exactly adjust the angular distance of the segments to correspond truly with angular distance of the stars.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The Museums and Lecture-Rooms Syndicate have recommended the immediate erection of a new lecture-room for physiology, with large additions to the rooms for practical physiology and to the work-rooms adjoining the Comparative Anatomy Museum, at an estimated cost of 7500*l*. These are to be carried along Corn Exchange Street. A work-room for the large class of Elementary Biology is also recommended to be built as an additional story above the Museum of Mineralogy, at a cost of about 2500*l*. It is also recommended that 1000*l*. be laid out in the purchase of microscopes.

The Board of Biology and Geology have modified their report respecting demonstrators and lecturers in Animal Morphology, on learning that the General Board of Studies cannot support their former proposals owing to the financial state of the University. They now ask for a lecturer on Vertebrates at 100*l*. and one on Invertebrates at 50*l*., together with a demonstrator at 150*l*., to be appointed by the Senior Lecturer in Animal Morphology.

The Rev. J. Venn, of Gonville and Caius College, has been approved for the degree of D.Sc.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 15.—"Some Experiments on Metallic Reflection. No. V. On the Amount of Light Reflected by Metallic Surfaces. III." By Sir John Conroy, Bart., M.A. Communicated by Prof. G. G. Stokes, Sec.R.S.

On Silvered Glass Mirrors.—The first set of photometric determinations were made with a silver film deposited on a flat and well-polished glass plate 76·5 mm. long and 51 mm. wide.

The glass plate was weighed before and after being coated with silver, and the weight of the film was found to be 0·0035 grm.; assuming the density of the silver to be 10·62, that being the value for silver finely divided by precipitation given in "Watts's Dictionary," vol. v. p. 277, the thickness of the film calculated from the area and weight was 0·00008447 mm.

The film appeared opaque by ordinary daylight, but when examined with sunlight was seen to be slightly transparent and of a deep blue colour.

The photometrical determinations were made in exactly the same way as those with the speculum metal and steel mirrors (*Proc. Roy. Soc.* vol. xxxvi. p. 187), and the observations were about as concordant as those contained in the Tables I. and II. of the paper giving an account of the experiments.

Two complete series of observations were made with light polarised in, and perpendicularly to, the plane of incidence, and the results are given in Tables I. and II.

The angles of incidence are given in the first column, the percentage amount of light reflected in the second and third, the means of the two sets of observations in the fourth, and the amount of light which ought to have been reflected according to Cauchy's formulæ in the fifth.

TABLE I.—*Silver Film, with Light Polarised in the Plane of Incidence*

Angle of incidence	Observed			Calculated
	A	B	Mean	
30°	96·74	98·39	97·56	96·66
40	97·06	97·13	97·09	97·01
50	98·35	99·67	99·01	97·45
60	97·06	98·40	97·73	98·02
65	100·0	99·04	99·52	98·29
70	99·02	98·41	98·71	98·61
75	99·02	99·05	99·03	98·94

TABLE II.—*Silver Film, with Light Polarised Perpendicularly to the Plane of Incidence*

Angle of incidence	Observed				Calculated
	A	B	C	Mean	
30°	89·25	86·30	—	87·77	95·74
40	90·69	87·02	—	88·85	95·30
50	89·31	87·09	—	88·20	94·66
60	85·10	86·41	—	85·75	93·75
65	86·09	85·0	—	85·54	93·22
70	86·33	86·50	83·85	85·56	92·73
75	83·91	87·55	86·17	85·88	92·50

The principal incidences and the principal azimuths were determined, and the means of two sets of eight observations each are given in Table III.

The values of the principal azimuths are higher than any obtained before in the course of these experiments, whilst those of the principal incidences are nearly the same as those obtained with the silver plate polished with rouge (*Proc. Roy. Soc.* vol. xxxi. p. 493), but considerably in excess of the determinations previously made with silver films.

TABLE III.

	Principal incidence	Principal azimuth
	75° 38′	44° 07′
	75 36	43 40
Mean ...	75 37	43 53

The calculated and observed values for the light polarised in the plane of incidence agree very fairly, the calculated values being slightly the lowest.

For light polarised perpendicularly to the plane there is considerable difference between the two sets of numbers, the calculated values being considerably the highest.

As has already been stated, the silver film was, to some extent at least, transparent, and it was found that when a Nicol was held between the eye and the silvered glass, and sunlight was incident obliquely upon the film, the brightness and colour of the transmitted light varied with the position of the Nicol; the image of the sun being brightest when the short diagonal of the Nicol was in the plane of incidence, and darkest and of a deep blue colour when the long diagonal was in that plane. Hence it would appear that at oblique incidences light which is polarised perpendicularly to the plane of incidence penetrates to a greater depth in the film than that polarised in the plane—a result that is in accordance with the conclusion drawn from the experiments with silver films already referred to, and one that may account for the difference in the observed and calculated intensities of light polarised perpendicularly to the plane of incidence reflected by the silver film.

In order to ascertain whether the difference between the observed and calculated results was really due to this cause or not, a thicker film was prepared by depositing a second coating of silver on a freshly-prepared film.

The same glass plate was used; the silver weighed 0·0072 grm., and its thickness was therefore 0·0001737 mm., or as nearly as possible double that of the single film.

The thick film was not absolutely opaque, as the disk of the sun on a clear day could just be seen through it, but it transmitted much less light than the film previously used.

Tables IV. and V. give the results of two series of observations made with it, and also the theoretical amount of light which should have been reflected, calculated from the values of the principal incidence and principal azimuth given in Table VI.

TABLE IV.—*Double Silver Film, with Light Polarised in the Plane of Incidence*

Angle of incidence	Observed			Calculated
	A	B	Mean	
30°	97·24	97·39	97·31	97·04
40	98·27	98·87	98·57	97·35
50	98·62	101·10	99·86	97·74
60	98·97	99·62	99·29	98·22
65	100·0	99·25	99·62	98·45
70	100·0	100·0	100·0	98·79
75	99·31	99·62	99·44	99·06

TABLE V.—*Double Silver Film, with Light Polarised Perpendicularly to the Plane of Incidence*

Angle of incidence	Observed			Calculated
	A	B	Mean	
30°	98·77 100·60	100·40	99·92	96·21
40	97·60	97·50	97·55	95·82
50	98·20	96·28	97·24	95·24
60	97·62	95·67	96·64	94·43
65	95·88	95·68	95·78	93·94
70	94·20	93·11	93·66	93·48
75	94·03	93·77	93·90	93·26

TABLE VI.

	Principal incidence	Principal azimuth
	75° 50′	43° 52′
	75 45	44 07
Mean ...	75 47	44 0

The values of the principal incidence and azimuth are slightly higher than those obtained with the thinner film, and therefore the percentage amount of light which, according to theory, should be reflected by the silver, is also higher.

The tables show that both for light polarised in and perpendicularly to the plane of incidence the observed intensity exceeds the calculated intensity, in the former case by about 1, and in the latter by about 2 per cent., except at incidences of 30° with light polarised in the plane, and 70° and 75° for light polarised perpendicularly to the plane, for which angles the observed and calculated intensities agree closely.

These results appear to confirm the general conclusion arrived at in the former paper, that, although the received formulæ for metallic reflection are approximately correct, they are not a complete expression of the facts of the case.

Zoological Society, May 20.—Sir Joseph Fayrer, F.R.S. vice-president, in the chair.—Mr. W. T. Blanford, F.R.S., exhibited and made remarks on a series of horns of the Wild Sheep of the Pamir, *Ovis polii*, Blyth, which had been obtained by the Hon. Charles A. Ellis, F.Z.S., from the Pamir district during his recent journey to Yarkand.—Mr. R. Bowdler Sharpe exhibited and made remarks on a second specimen of the new European Nuthatch (*Sitta whiteheadi*) recently discovered by Mr. Whitehead in Corsica.—Dr. J. G. Garson exhibited and made remarks upon a specimen of *Lithodes maia*, the Northern Stone-Crab.—Mr. Frank E. Beddard, F.Z.S., read the first of a series of papers on the Isopoda collected during the voyage of H.M.S. *Challenger*. The present communication treated of the genus *Serolis*, sixteen species of which were represented in the specimens obtained during the expedition. Of these nine were described as new. The author also gave a short account of the geographical distribution of the genus, and pointed out some of its peculiar structural points.—Mr. Gwyn Jeffreys, F.R.S., read the eighth part of his papers on the Mollusca of the *Lightning* and *Porcupine* Expeditions. It included the families Aclidæ, Pyramidellidæ, and Eulimidæ, with seventy-five species. Two genera and twenty-three species were described by the author as new to science.—Prof. Jeffrey Bell read the fourth of his series of papers on the Holothurians. The present communication gave an account of the structural characters of the Cotton-Spinner (*Holothuria nigra*), and especially of its Cuvierian organs.—Mr. F. Day read a paper on races and hybrids among the Salmonidæ, in continuation of a former communication made to the Society, and continuing an account of the experiments made by Sir James Gibson-Maitland in the hybridisation of Salmonidæ in the ponds at Howietown.—A communication was read from Mr. R. Collett, C.M.Z.S., containing the description of some apparently new Marsupials obtained by Dr. Limholtz in Northern Queensland. These were described as *Phalangista archeri*, *Ph. herbertensis*, *Ph. lemuroides*, and *Dendrolagus limholtzi*.

Geological Society, May 14.—Prof. T. G. Bonney, F.R.S., president, in the chair.—John Ruscoe was elected a Fellow of the Society.—The following communications were read:—On the pre-Cambrian rocks of Pembrokeshire, with especial reference to the St. David's district, by Dr. Henry Hicks, F.G.S., with an appendix by Thomas Davies, F.G.S. The author in this paper gave further detailed evidence in addition to that already submitted by him, to show that the Geological Survey Map of the district of St. David's and of other parts of Pembrokeshire is incorrect in some of its most essential features, and inaccurate in very many of its petrographical and stratigraphical details. Some new areas in South Pembrokeshire were also referred to. He replied also to the criticisms contained in the paper by the Director-General of the Survey, read last year before the Society, and indicated that Dr. Geikie had completely misunderstood the sections and the order of succession of the rocks at St. David's. He pointed out that the views so elaborately worked out by the Director-General to show the evidence of metamorphism in the rocks, were based on the entirely false supposition that the granitoid rocks were intrusive in the Cambrian rocks, and that the felsites were merely peripheral masses. He showed, by producing abundant fragments of the granitoid rocks and of the felsites from the basal Cambrian conglomerates, that the granitoid rocks were the very oldest rocks in the district, and that they must undoubtedly be of pre-Cambrian age. He proved, from microscopical evidence, that the rocks supposed to have been altered by the intrusion of the granitoid rocks, were in the condition in which they are now found before the Cambrian rocks were deposited, and, moreover, that the supposed concretions in the porcellanites and conglomerates, claimed to have been due to metamorphism, had turned out, on microscopical evidence, to be actually fragments of old pre-Cambrian rhyolites inclosed in the sediments. It was shown also that at the points indicated by the Director-General, where the evidences of intrusion were supposed to be seen, there was not the slightest change of a metamorphic character induced in the sedimentary rocks in contact with the granitoid rocks. The only difference that could possibly be recognised in them by the aid of the microscope was such as is well known to be the result of crushing when in the neighbourhood of faults. Indeed there was the clearest evidence possible to show that the junctions were merely fault junctions. The supposed fold in the Pebidian rocks, the author stated, was impossible if petrological evidence was of any value. The author also produced many facts to show that the conglomerates at the base of the Cambrian constantly overlapped the different members of the series which he claimed to be of pre-Cambrian age, and that the unconformity was very marked and to be clearly seen in many coast sections. The conglomerates were shown also to contain well-rolled pebbles of all the series included under the names Dimetian, Arvonian, and Pebidian, as proved by careful microscopical examination of the fragments by Mr. T. Davies and himself. An appendix by Mr. Davies, describing the microscopic character of the rocks, accompanied the paper.—Note on a specimen of iron amianthus, by the Rev. J. Magens Mello, M.A., F.G.S. The accompanying specimen was found at the bottom of one of the Wingeworth iron-furnaces, near Chesterfield, and was given to the author by Mr. Arthur Carrington, one of the owners. The furnaces have been lately blown out for repairs, and in the mass of slaggy refuse at the bottom a thin layer of the curious product known as iron amianthus was interposed between the sand and the iron refuse. The red sand at the bottom of the furnace was converted in its upper part into a compact, hard, white sandstone an inch or two in thickness, and upon the top of this the iron amianthus occurred in snow-white fibrous masses, the fibres radiating in a concentric manner, and forming more or less botryoidal concretions, somewhat resembling hæmatite in appearance, and separated by extremely thin plates or septa of iron, by which the entire mass is divided into irregular prisms of about half an inch in diameter.

Physical Society, May 24.—New member, Mr. F. C. Phillips, electric engineer.—Prof. W. G. Adams took the chair while the President, Dr. Guthrie, gave a brief summary of his recent researches on eutectic alloys, that is alloys of low fusing point. The complete research will be published in the Society's *Proceedings*. Dr. Guthrie showed by means of tables and curves of results that mixtures of water and nitre, nitre and nitrates, &c., behaved in the same way as fusible alloys, such as alloys of lead and bismuth. On cooling down the alloy or mixture, the ingredient present in richer quantity crystallised out. There seemed to be no definite molecular proportions in these alloys. A "tetra-eutectic" alloy of bismuth 47·38, tin 19·97, lead 19·36, cadmium 13·29 per cent., was exhibited by the author, which fused at 71°, or in boiling alcohol. Rose's fusible metal melts at 93°. Results were given of the behaviour of mixtures of water and the aniline salts, salicylate, oxalate, &c.; also of water and tri-ethylamine, and other members of the ammonia group. Dr. Guthrie's observations tended to show that fusion and solution were of the same nature. He pointed out their bearing on mineralogy and geology, and inferred that water in igneous rocks was there from the first, and not by infiltration, as some suppose.—The President then took the chair, and Dr. W. H. Stone exhibited a simple, cheap, and portable galvanometer for hospital use, made of a boxwood cylinder with coils wound round it, and a needle with mirror, inserted into a test-tube, and pushed into the hollow of the cylinder. The needle is made dead-beat by putting paraffin oil into the tube. He also exhibited a Kohlrausch metre bridge for alternating currents, a telephone playing the part of indicator. Dr. Stone employs it for measuring the resistance of the human body, which he finds to be less than 1000 ohms. With high-tension currents it appears lower than with low-tension currents. Another metre bridge of the kind with a longer wire (3 m. in this case as compared with ½ m. in the other) was also shown in connection with a sledge induction-coil, by which the power of the current can be regulated to suit the patient. Dr. Stone stated that the body acts more like a solid than a liquid conductor. Mr. Glazebrook said he had used a similar plan with a telephone to measure the resistance of electrolytes; but found the telephone too sensitive from induction, though in Dr. Stone's work this objection might not apply. Prof. G. Forbes stated that the telephone had been applied in a similar way to comparing capacities. With regard to the danger from currents, Prof. Ayrton said the E.M.F. of the railway current at Bushmills was 250 volts, and pointed out that very intermittent currents were more dangerous than fairly continuous ones. Dr. Stone thought that with good skin contact (as with salt and water) this E.M.F. would be dangerous. Mr. Lecky instanced the reported death of a horse at Bushmills by a shock.—A new speed indicator, especially for marine engines, was exhibited by Mr. W. T. Goolden and Sir A. Campbell of Blythswood. Its action depended on the rolling of a disk on a cone, the disk traversing a screw driven by the engine-shaft. The disk forms the nut of the screw, and rotates in an opposite direction to the latter. Its position on the screw depends on the surface velocity of the cone, which is kept turning at a uniform rate by clockwork. In travelling, the disk makes a series of electric currents which indicate its position on a set of dials detached. Recording apparatus can be added. The apparatus was made by Mr. A. Hilger.—Mr. W. Baily exhibited a similar device, in which the cone was replaced by a circular plane or disk. He had invented this independently, and it had the advantage of giving a zero position to the rolling disk, though the cone was the more compact arrangement. The idea of using a screw in this manner was suggested by Mr. Shaw of Bristol some three years ago.

BERLIN

Physiological Society, May 2.—Dr. Bender gave a short description of a preparation which he exhibited at the end of the meeting. It was an axolotl in the stage of development in which the heart consists of a tube with a sacular expansion at one part, corresponding to the atrium, and then forms a loop, the ventricle, afterwards passing over into a second expansion, the bulb; the animal is in this stage still transparent enough to permit of the movement of the blood through the three chambers of the heart being seen distinctly.—Dr. Herter described the experiments which Dr. Lukjanow had made in his laboratory upon the influence of increased tension upon the absorption of oxygen. The question is of physiological importance because, if it is decided by experiment in the negative, the existence of an optimum amount of oxygen in the air will be proved, which would coincide with normal percentage proportion of oxygen in the air, whereas if the experiment should result in proving that the absorption of oxygen increases with the increase of the oxygen tension, then this oxygen absorption and the consequent oxidations would have to be included in the general combustion processes whose intensity is known to increase with the increased tension of oxygen. The experiments were conducted after the method of Regnault and Reiset. The animals were placed inside a bell-jar, into which the air entered on one side along with an additional quantity of oxygen, which could be varied at pleasure, and from which it was drawn off on the opposite side by a tube which passed into the absorption vessels where the carbonic acid was removed, and the residue was provided with fresh oxygen and led back into the bell-jar. An offset from the air tube allowed of a sample of the expired air being drawn off at any time for analysis. In all fifty animals were experimented upon, which were kept fasting for half a day before commencing the experiment. The oxygen of the inspired air varied between 30 and 90 per cent. The mean result of all the experiments on guinea-pigs, rats, dogs, and cats, was a slight increase of the oxygen absorption, to wit 104 volumes as against 100 absorbed from normal air. Dr. Herter is of opinion that this small increase cannot be regarded as a consequence of the increased oxygen tension, because, in individual animals, the means of oxygen absorption sometimes fell below, and sometimes exceeded the normal amounts, and further, because they did not vary proportionally with the increased tension of oxygen in the air. The small increase of the general mean must be referred to other causes, *i.e.* the movements of the animals during the experiments. No increase of temperature was observed under the increased pressure of oxygen. Further experiments were made upon animals in which one could assume an increased demand for oxygen in consequence of high fever being present, but not even did the animals that had fever take up more oxygen from the air that was charged with a more than normal amount of oxygen. Likewise, animals from whom a large quantity of blood had been withdrawn behaved in exactly the same way as normal animals in presence of the surplus of oxygen. The conclusion to be drawn from all these experiments is that the absorption of oxygen is not an ordinary combustion process, and that the normal composition of the atmosphere contains an optimum percentage amount of oxygen.—Prof. Busch spoke about caries of the teeth which has been so little scientifically investigated, because in studying it the external hurtful processes have been alone considered, whereas the second important factor, the resisting power of the teeth, has been quite overlooked. In regard to the latter, Prof. Busch called attention to the fact that caries of the teeth had been observed in no animal, and that it appeared to be peculiar to man. Caries of the teeth, however, appears not to be a characteristic of civilised man alone, but it has been observed in large collections of skulls even in those of prehistoric time. Some races are more disposed to it than others. For instance, the Celtic, Arabian, and Polish races appear to possess a relative immunity. This is less the case with the Indo-Germanic race. Certain families are particularly predisposed to it. General habit of body has a pronounced influence upon its development, as well as menstruation and pregnancy in women, chlorosis, typhoid, &c. Disposition to caries shows itself even in the developing tooth in the composition of its enamel, which is undulating, whereas teeth with quite smooth enamel have much greater power of resistance. The enamel appears to be the only tissue in the body which is subject to no metabolism, and which remains quite unchanged. Every alteration in it which is caused by external influences, and every defect of the enamel remains during the whole of life, and can never be repaired. Dentine also shows differences in its structure as regards its disposition to caries. The dentine tubes either run regularly close side by side to each other, such teeth having a greater power of resistance; or the dentine tubes branch and surround cellular bodies, or even small air vesicles, such teeth falling an easy prey to caries. If dentine has been decalcified at any place by the action of acids, it undergoes putrefaction under the influence of bacteria which do not seem to belong to any specific species. Dentine is sensitive, although nerve filaments have not as yet been traced into it. Actual toothache does not occur in the course of caries until it has reached the pulp. The inflammation of the pulp is particularly violent and painful, because the tissue is so richly supplied with blood-vessels and nerve-filaments. As the products of inflammation cannot escape, they collect and work their way downwards, where they produce the most painful inflammation of the roots and the periosteum. The chief object of the rational treatment of caries of the teeth consists in the removal of every particle of carious substance out of the diseased tooth and to protect the sound dentine that has been exposed against external injurious influences by covering it with a firm substance which is not attacked by acids: gutta-percha, cement, or gold. Although the dentine is not as unchangeable as the enamel, but manifests, by its becoming firmer or softer, that it is not quite uninfluenced by tissue changes, yet its caries is not an irritative process that the dentine takes an active part in, but a passive process, and consequently the removal of all diseased portions, and the protection of the non-carious part of the tooth by filling with a resistant mass suffices to stay the morbid process completely.

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