THURSDAY, AUGUST 22, 1878

BRITISH BARROWS

British Barrows. A Record of the Examination of Sepulchral Mounds in various Parts of England. By William Greenwell, M.A., F.S.A. Together with *Description of Figures of Skulls, General Remarks, Pre-historic Crania, and an Appendix.* By George Rolleston, M.D., F.R.S. (Oxford: Clarendon Press, 1877.)

THE pre-historic inhabitants of Europe are now exciting an interest in the minds of thoughtful men which, twenty years ago, would have seemed impossible, and which can no longer be ignored by the historian. The story of man in Great Britain is rapidly being unfolded, principally by the careful and scientific exploration of the various remains which are eloquent of the condition of things that passed away before the art of letters was known in the north; and, among those who have been mainly instrumental in bringing this about, Mr. Greenwell will ever deserve a foremost place. He has devoted years of patient labour to the accumulation of facts; and in the present work he records the results of the examination of upwards of 230 burial mounds, the greater number being in the wolds of eastern Yorkshire. He has also had the advantage of the aid of Prof. Rolleston, by whom the human remains—fortunately now safe from dispersal, in the Oxford Museum—have been classified and described, in the latter part of the book.

The general results of the exploration are thrown into an introduction of 130 pages, carefully written, and highly suggestive of new lines of thought. The barrows vary in size and shape very much as the graves and tombs in our own graveyards, where the rich man's memory is preserved by the large mausoleum, while the poor man's resting-place is marked merely by the little mound of earth, soon to be lost in the general surface. Those in the Yorkshire wolds are either circular or "long," the former being the more abundant, and are frequently surrounded by a ramp or a ditch. In some cases this was within the base of the barrow, and very generally it was incomplete. "This very remarkable feature," writes Mr. Greenwell, "in connection with the inclosing circles, is also found to occur in the case of other remains which belong to the same period and people as the barrow. The sculptured markings engraved upon rocks, and also upon stones forming the covers of urns or cists, consist in the main of two types—cup-shaped hollows, and circles, more or less in number, surrounding in most cases a central cup In almost every instance the circle is imperfect, its continuity being sometimes broken by a duct leading out from the central cup; at other times by the hollowed line of the circle stopping short when about to join at each end. The connection of the sculptured stones, if so they may be termed, with places of sepulture brings them at once into close relationship with the inclosing circles of barrows, and it is scarcely possible to imagine but that the same idea, whatever that may have been, is signified by the incomplete circle in both cases. The rings of gold and bronze, of various shapes, some of which in their construction show that the penannular form is not caused by the requirements of their use, appear to represent the same incomplete circle. In fact, if some of the gold rings were figured upon stone, they would appear in the very similitude of the circular rock sculptures." Our author suggests that it may have been intended to prevent the exit of the spirits of those buried, though in that case it is hard to see why the spirit should not have found its way out through the opening. It seems more probable that if the barrow represented the hut inhabited by the living that the circle round it would represent the trench, or the inclosure of the hut, and that this would necessarily be incomplete, to allow of access to the habitation.

The dead were buried in the barrows of the wolds very generally in the condition and clothing in which they died, the proportion of cases of inhumation to those of cremation being as 301 to 378, or about 80 per cent. In all probability both customs were carried on simultaneously, as was the case in ancient Rome, where, however, inhumation was mainly confined to the lower classes. Where inhumation had been practised the body was buried in the crouching posture in which life had departed, and which would be natural where the sleeping place was not well protected against the cold, and the covering was scanty. This interpretation, due to the ingenuity of Mr. Evans, is most likely true.

The burnt and broken bones of various animals used for food in the barrows are probably the remains of funeral feasts held at the time of the interment, or from time to time afterwards, or they may be the remains of food offered to the dead. Splinters and various manufactured implements of flint, and fragments of pottery, also occur sometimes in great abundance, and probably symbolise some religious idea. Fragments of flint were used in interments at least as late as the fourth and fifth centuries after Christ in this country; for they were found in considerable quantities inside the oaken coffins in the Romano-British cemetery, referable to the above date, explored at Hardham, Sussex, in 1866.

Where cremation was practised the funeral pile was sometimes kindled upon the spot, which was afterwards occupied by the barrow, but at other times the ashes of the dead were collected and deposited somewhere else. In several barrows curious perforated vessels of pottery, or "incense cups," were met with, which may have been used to convey the sacred fire to the pile. The ashes of the dead were placed in urns sometimes highly ornamented, and those things which delighted the dead most or were most useful to him, were deposited in the tomb. Flint scrapers, flakes, arrow-heads, beads, hammer-axes, celts, domestic pottery, and a few bronze articles. The number of objects buried in each barrow varied according to the wealth of the dead and the estimation in which he was held by the survivors.

The animal remains in these barrows prove that the ancient inhabitants of the Wolds were no rude savages living mainly on the chase. They possessed flocks and herds, consisting of well-known domestic breeds—the small Celtic shorthorn, now represented by the mountain cattle of Wales and Scotland, the pig, the horned sheep or goat, the horse and the dog, the two last being the rarest. They also ate venison of the stag (*Cervus elaphus*). Their place in the archæological scale of culture is fixed by the few and simple forms of bronze articles in the barrows, the simple wedge-shaped axe, and the short broad dagger, in association with various articles of stone, coupled with the absence of the higher bronze types, such as the sword. They belong to the early bronze age. The absence of the sword is also noticed in the tumuli of France, referred by Mr. Chantre to the same horizon. At this time the knowledge of bronze was gradually finding its way northwards from the Mediterranean centres, and the simpler forms preceded the more complex and elaborate.

Nor are we left in doubt as to the ethnical relations of these ancient Yorkshiremen. Prof. Rolleston's elaborate examination of the crania and skeletons reveals the fact that the two types—the small long-headed or "Iberian," and the tall robust round heads, or "Celtic," which have been traced by Thurman, Huxley, Busk, and myself, from Scotland to the Mediterranean, and from the Rhine to the Pillars of Hercules, occur in the round barrows side by side in intimate association. The former of these, "the Silurian" of Prof. Rolleston, is considered in this work as the older of the two. According to Dr. Thurman it was dormant in Britain in the Neolithic age, at the close of which it was invaded by "the Celtic" or "the Cimbric" of Prof. Rolleston. The truth of this view is confirmed by the fact that the dead of these two races rest peacefully together in the round barrows of the wolds referable to the early bronze age. In concluding this review it remains merely to say that this valuable work fills a void in the archæological record of Great Britain, and that it contains a larger mass of accurately observed facts than any book hitherto published relating to the Bronze Age in this country.

W. Boyd Dawkins

THE ECLIPSE OF THE SUN

The following letter appears in the *Daily News* of Tuesday, from its Special Correspondent, dated Manitou, Foot of Pike's Peak, Colorado, August 2:—

Since the eclipse I have had no easy time of it. To gain accurate information concerning the work done along the long north and south line, on which, as your readers already know, the stations were located, it was necessary to return over the crest of the Rocky Mountains at Sherman (8,200 feet) down to Cheyenne, and then go as far south, by the Colorado Central line, as Colorado springs, passing Denver on the way. Manitou, whence this letter is written, is about six miles from the latter place on the old and famous Ute trail, a road much improved in these later days for the needs of the enormous traffic connected with the mines at Leadville. The place itself is lovely—so lovely, indeed, that although it is over 2,000 miles from the eastern seaboard, the hotels and baths (iron and alkaline) formed but a nucleus for a large encampment, tents dotting the slopes in all directions. The newly-published geological map of this region with which Prof. Hayden, with his wonted generosity and forethought, had supplied some of the astronomers, enabled them to revel with more or less success in the wonders of the famous "Garden of the Gods," and Glen Eyrie, where the rocks put on everything they can in the way of uncommon arrangement and colour. Vertical beds of limestone and sandstone 300 feet high, and not many yards thick, even at the base—each spire excelling the other in some glorious colour, including all those we see in an English sunset—is a sight never to be forgotten, even if Pike's Peak, with its patches of snow, were not seen through such gigantic and weird portals.

Here, then, we met the Pike's Peak and the Las Animas parties, all rejoicing over their success. Two of the former—General Myer, the creator and chief of the wonderful meteorological signal surface, and Prof. Cleveland Abbe—have suffered somewhat from their sudden transference from the plains to a height of above 14,000 feet. The latter, indeed, had to be transferred on a litter, by the General's order, to a station at mid-height before the eclipse. But the journey to Manitou does not exhaust my wanderings. Much was expected from Prof. Holden's party at Central City, easily reached from Golden City by one of the most wonderful railways in the world. To Central City, therefore, I went up to a point called Black Hawk. The railroad is filched from the bed of a stream at the bottom of a true cañon. Here rail and river together are not fifteen yards broad, and precipitous cliffs 1,000 feet high shut out the light. Here, again, the cliffs recede, the river bed widens, and miners—American and Chinese—are engaged in extracting the precious gold from the turbid water and its bed, some of the miners making in this way 1,500 dollars a week. From Black Hawk to Central City there is no possible approach except up the side of a steep hill. The railway accepts the condition, and zigzags up—now engine, now cars, foremost—till the nest of stamping mills and smelting furnaces at Black Hawk looks like a speck below.

The party at Central City had been as fortunate as the rest in the matter of weather, and their station, on the top of Teller's Hotel, in the middle of this strange mountain community, had proved a very convenient one. After my last letter to you, I became acquainted with the fact that Mr. Burnham, so well known for his work on double stars, and whose eye seems to be keen as was formerly Dawes's among us, was going to observe the eclipse on behalf of the *Chicago Times*—a piece of newspaper enterprise worthy of imitation. I tried to find him when I passed through Denver to Prof. Young's camp, but was unsuccessful, though I was fortunate enough to see Prof. Young himself, whose party was second to none either in *personnel* or in their means at the disposal for securing good results.

My telegram to you, immediately after the eclipse, was necessarily based upon the most meagre materials, and laid emphasis upon those parts of the combined attack which were most strongly represented at the northern stations. Still, now that the complete information is before me, I have very little to alter, so singularly and exceptionally do the observations support each other in the main; and I say in the main, because on some points there is enough disaccord to make the astronomers already look forward to the next eclipse. Without going too much into detail I will go over the ground in the light obtained by personal communication with the chiefs of the different parties, with the single exception of Prof. Hall, who is a most skilful and competent observer, and to whose opinion, as the discoverer of the satellites of Mars, the greatest weight is to be attached. The corona was much less brilliant than usual. Those who have observed the greatest number of eclipses are strongest on this point. The contrast, perhaps, is most striking between this eclipse and that of 1871 observed in India. Now that this fact is recognised, the naturalness of it is apparent to everybody. We know that the sun's activity and the various meteorological and magnetical conditions on our own earth which depend upon or are connected with it wax and wane every eleven years or so. This is termed the sun-spot period. Thus the sun was very active in 1871, and it will be again very active in 1882. It is very sluggish now, and it will be sluggish again in 1889. In 1871 we had many spots, many prominences, many magnetic storms and auroræ, heavy rainfall, and, let me add, no famines to speak of. Associated with these we had a large corona. This year there are no spots, the prominences are rare, the magnets were never so quiet, there are no auroræ, and we are passing through a famine period. Associated with these we had a small corona. Hence it is that the astronomers agree that this year another connecting link has been added to the chain which binds together the solar changes.

On another point mentioned in my telegram the evidence is overwhelming. Since the use of the spectroscope in this branch of inquiry the spectrum of the corona has been observed to be a mixed one—that is, bright lines as well as a continuous spectrum have been traced in it. In 1869 and 1871 these lines were very bright to the eye—much brighter than the continuous spectrum, while in 1875, when the spectrum was photographed for the first time, the record showed the lines to be much brighter than the continous spectrum. Now, as it generally happens that when in our laboratories we study gaseous spectra there is a faint continuous spectrum accompanying the lines, it was thought that the spectrum of the solar corona might have a gaseous origin exclusively. This year's observations have quite dispelled this idea, for the two elements of the corona spectrum, to which attention has been drawn, instead of varying directly, vary inversely. The continuous spectrum has been seen and photographed by itself, without any bright lines. Now for the interpretation of this hieroglyphic language. The gases which were high up in the sun's atmosphere at the last epoch of maximum sun-spots (1871), have almost entirely retreated to a lower level; as these gases are to a large extent carriers of heat from the interior to the exterior of the sun, the exterior is cooler in their absence, and indeed cool enough to allow it to hold in suspension a larger percentage of solid and liquid particles to which the continuous spectrum is due. A natural suggestion touching these particles is that they consist of meteoritic matter, and all the phenomena to which attention has been drawn can be explained by supposing that these meteorites surround the sun, and that at the minimum sun-spot period they can exist lower down in consequence of the reduced temperature of the sun's atmosphere at that time. If this suggestion, which is the one in favour out here, be endorsed we have a partial cause of reduced solar radiation at that time. I cannot quit this part of the subject without referring to the wealth of astronomical resources which have been brought to bear upon it. Paper astronomers have been now for a long time, I doubt not, very lucratively employed in proving that it was impossible to do exactly what has been done. Dr. Draper, who ranks deservedly high among solar observers, bears off the palm by the strength of his attack. He used a camera of six inches aperture and of only twenty-one inches focal length, and, by means of a Rutherfurd grating two inches square, obtained a photograph of the corona and two of its spectra with the same instrument. The plates were exposed during the whole of totality, and it is encouraging for future work to know that much less exposure would have sufficed to secure the precious records, although the continuous spectrum recorded is the most difficult to obtain. Prof. Harkness used an instrument of nearly equal power, though of slightly different arrangement. The smallest instruments employed—ordinary portrait cameras with a grating in front—also gave the same result, though, of course, on a much smaller scale.

Next comes the endorsement of the fact that the coronal light is due partly to solar light—that all the light it sends to us is not its own. Prof. Barker, with Dr. Draper's party at Rawlings, though he saw no bright lines, saw dark ones in abundance; and Prof. Morton got distinct traces of radial polarisation, thereby endorsing the result obtained in 1871. The object of these observations by the polariscope is to determine whether and in what plane light is reflected; and for the light to be reflected to us by particles in the sun's atmosphere we must imagine our eye, the centre of the sun, and the particle to lie in one plane. If, further, we imagine an infinite series of particles surrounding the sun, we shall have an infinite series of these planes, and each radius of the sun will lie in one of them. I have been particular in stating this, because Prof. Hastings, a great authority in optical matters, declares that the reflection takes place at right angles to these radial planes; in other words, he declares that the polarisation is tangential and not radial. As has been well remarked, this result is easily explained by supposing ice crystals to be present in the sun's atmosphere, and no other solution lies on the surface. I believe no one is more astonished than Prof. Hastings at his observation; and here again we see the necessity of relaxing no efforts and letting slip no opportunity of garnering the observations with which eclipses alone supply us.

Finally, as to the construction of the corona. Numberless records of this, both visual and photographic, have been secured, and as usual, though there is almost perfect agreement as to the structure of the lower portion, the rays and streamers have been very variously observed. The upper and lower portions of the sun were graced by the most exquisite tracery, bending over right and left like plumes of ostrich feathers, the intervals between them being of a delicate blue. Near the solar equator the structure was not so obvious. Still, it was there, for Prof. Bass saw it come out *and pulsate* after he had fixed his eye on one portion for two minutes. The structure was distinctly less filamentous than in 1871. With regard to the streamers there appears to have been two sets—one along the ecliptic, giving rise to the appearance of a wind vane seen by one set of observers; another, at right angles to this, seen by another set. Neither of these systems of streamers was visible in a telescope, though the base of the ecliptical ones appears on the photographs. On this point the greatest weight must be given to the observers on Pike's Peak, 14,147 feet high. Most fortunately the weather there as elsewhere was superb, and the corona was seen as it was never seen before. The clearness of the sky in this region when the weather is good is simply wonderful. The Milky Way seems to have deep holes in it, and the individual stars shine out. Even at Rawlings Prof. Watson could see the satellites of Jupiter with the naked eye. At Pike's Peak General Myer saw the corona for fully five minutes after totality was over, while in India, in 1871, the much brighter corona was seen at sea-level for only three minutes after the eclipse was over. Prof. Cleveland Abbe, who, as already stated, observed below the Peak, saw the ecliptic streamers extending to a distance of twelve solar diameters, while he saw nothing of the north and south ones, pretty conclusive evidence in favour of their subjectivity, and from their appearance he has little doubt of their being meteor streams. Prof. Newcomb saw the ecliptic streamers almost equally well at Separation by the help of a novel contrivance. He had a disc erected on a high pole at some distance from his telescope so that he could momentarily cover the dark moon and corona and observe the external phenomena with the naked eye. In this way he saw the streamers extending nearly as far as Prof. Abbe did. Still he explains them differently. He considers that they indicate the true zodiacal light. No doubt there are difficulties surrounding both suggestions, and here again we see the need for future thought and work. The regions in which the various parties were located enabled many of the connected phenomena to be observed as they had never been before. From Pike's Peak, with its horizon of 150 miles on all sides, the shadow of the moon sweeping rapidly through the air was a very tangible thing and seemed to be solid enough to sweep all before it. It was noticed on the air and on the very buttresses of the Peak that the boundary of the shadow was strongly coloured in the prismatic order. The wide extent of Alcali Plains round Separation and Preston—plains broken by nothing save the wonderful avenue of telegraph poles along the railway, and the solitary water tank and telegraph operator's house, enabled these and the associated phenomena to be seen well there also, and there were just sufficient clouds above the eastern horizon to bring out into strong relief the retreat of the shadow through the air, while during the eclipse the leaden light on the desolate plains gave rise to an intense feeling of loneliness and weirdness. The daring genius of Edison has left its mark on this eclipse. So soon as he had completed his tasimeter he saw its applicability to eclipse work in determining the presence of heat waves in the radiation from the corona. I had the rare privilege of seeing the great inventor at work gradually increasing the sensitiveness of his wonderful instrument with the most consummate knowledge of principles and contempt for elaboration, until at length during the eclipse he was rewarded by seeing the speck of light on the attached Thomson galvanometer give a decided swing from its zero on the dark moon when the image of the corona was brought on the fine slit in the plate which shielded the tasimeter from its surroundings. The instrument was too new to succeed in other hands, even those of Prof. Young; but he was not to be beaten. Driven from one instrument he took up another—the thermo-electric pile—and was rewarded by finding a heat line in the ultra-red. This opens out another new line of work in future eclipses; and so science advances. Rumour here says that one distinguished astronomer remained away because he considered eclipses "played out." What a lesson he has learned ere this touching the need of that receptivity to which I referred in my former letter!

Mindful of your space I must here conclude my statement of the more salient solar phenomena observed, the detailed discussion of which will occupy the astronomical world for some time to come. I shall have been very unfortunate and unworthy of my post if I have not succeeded in convincing your readers that several important advances have been made, and that the work done on the eclipse of 1878 will rank high in astronomical records. I have not yet, however, quite finished the story I have to tell. Students of solar physics may congratulate themselves that the gravitational astronomers will in the future insist upon having their finger in the eclipse pie. In my telegram I was enabled to announce the position of the body observed by Prof. Watson near the sun. Since the telegram was dispatched the matter has been seriously discussed by Prof. Holden and others, and little doubt remains that a new major planet has been discovered, if, indeed, Vulcan has not been refound. Prof. Watson's work has been acknowledged on all hands to be a veritable *tour de force*. The Naval Observatory instructions contained a map of all stars near the sun down to the seventh magnitude. Prof. Watson determined to review all these, and provided his equatorial with paper circles on which to mark the difference of right ascension and declination between any body not marked on the chart and the sun. After the eclipse, and before the instrument was dismounted, Prof. Newcomb and Mr. Lockyer saw the precious record *in situ*. It was here at Manitou that Prof. Watson again reviewed his work, and despatched a telegram to the Smithsonian Institution corroborating the one I had previously sent to you.

The English observers are full of appreciation of the reception they have met with from their American *confrères*. Prof. Thorpe and Dr. Schuster were the guests of Prof. Hall's party at Las Animas. Mr. Lockyer, invited by General Myer to Pike's Peak, by Dr. Draper to Rawlings, Prof. Newcomb to Separation, and Prof. Wright to Las Animas, decided for Rawlings, where Dr. Draper placed all the resources of his observatory at his disposal. A thing unknown in England—that is, a travelling railway photographic car—being, however, placed at the disposal of the astronomers by its proprietor, Mr. Silvis, it was decided at the last moment to establish another station, and on the morning of the eclipse Prof. Watson and Mr. Lockyer proceeded to Separation, a station on the Union Pacific Railway, between the eclipse camps there and at Preston.

The *Times* correspondent, writing from Pueblo, South Colorado, July 31, states that Dr. Schuster brought out with him a couple of fluorescent eye-pieces, with a view of re-determining the position of the lines he observed in 1875; but a too-confiding faith in the tender mercies of the baggage-men in charge of the instruments led to the utter destruction of one of the eye-pieces, while the other was so injured that it was impossible to get it into order in time for the eclipse. Indeed, nearly all the instruments, in spite of most careful packing, suffered more or less damage, either during their canter—for such it can only be called—along the Western railroads, or at the hands of the "baggage-smashers" who took charge of them at the depôts. The baggage arrangements of American railroads are doubtless perfect in theory; but the practical application of them is simply ruinous to scientific apparatus.

The newspapers are filled with allusions to the phenomenon; its effect on animals, on the colours of objects, and on their visibility, was noted everywhere. The darkness was far from being so great as was anticipated; the decrease of temperature was, however, very considerable. At La Junta it fell from 96° to 80°; at the time of last contact it had again risen to 93°. In Pueblo the fall was as great as 23½°—*i.e.*, from 103° to 79·5°.

The following interesting and amusing account of the eclipse observations of Prof. Lewis Swift, by a reporter of the *Rochester Democrat*, appears in the *New York Tribune*.

Prof. Lewis Swift returned from Denver, Col., Saturday evening, bringing splendid trophies of his skill as a searcher of the heavens. That his discovery was not duly reported by the Associated Press is chiefly owing to Prof. Swift's modesty in heralding the results of his labours and his desire to carefully determine the significance of his observations before making public announcement. Yesterday afternoon we visited Prof. Swift at his pleasant home in Ambrose Street, and learnt from him the story of his visit to Denver and observations of the total eclipse. Prof. Swift said:—"Up to the time of the eclipse the prospects for a clear day were very poor. The nights were clear, but it was cloudy, and rainy every afternoon. Sunday afternoon there was a clearing storm, with hail and drenching rain. Up to Sunday afternoon an unprecedented amount of rain had fallen in the region. On Monday morning there was not a cloud in the sky, and all predicted a clear day, and we had it. The final preparations were made as rapidly as possible. A. C. Thomas had arrived at the eleventh hour from Chicago with a telescope and spectroscope attachment. Prof. Hough had brought a small telescope with fine lines stretched across the object-glass, and a micrometer eyepiece, for the purpose of measuring the corona. I had secured from the Mayor the services of a police-officer to keep the grounds about the instruments clear from spectators. According to Washington predictions the eclipse was to commence at 4h. 7m. 50·4s. According to Prof. Colbert's calculations it would begin at 4h. 10m. 50s., Washington time—a difference of three minutes. Prof. Colbert's prediction, was proved to be very nearly correct. About half an hour before the eclipse was to begin our party took their positions. I was about twenty feet to the south of Prof. Hough, who was in a group consisting of himself, Prof. Colbert, and Prof. Easterday, who used the telescope for observing the corona. To the west was a class of fifteen young ladies from Denver, instructed by Prof. Colbert to sketch the corona. South-east of my position Mr. Thomas was stationed.

"When arranging my instrument I made the post very low, exciting much comment by my companions. I told them I intended to lie on the ground during the observations, this being a position I had found the easiest in my experience of twenty years' comet-seeking. I spread a carpet on the earth and had a great advantage over the other members of the party, who were obliged to assume constrained positions, which tended to unsteadiness of vision. Seated by me was Daniel Drummond, with an accurate time-piece, set by the chronometer a few minutes before the first contact. E. D. Smith, an old acquaintance, whom I met in Denver, recorded the time of each event as I called it. Mr. Drummond is an experienced engineer, and counted the seconds with great accuracy.

"As I arranged my telescope for the first event the wind was blowing in fitful gusts from the south-east, shaking our instruments. To prevent my instrument from swaying I tied a long stick to it, about a foot above the eyepiece, the other end being braced against the ground and free to move only in one direction. This was a blunder to which I owe the discovery of a stranger, which I am inclined to think is Vulcan. As the sun moved the eye end of the telescope moved to the east. The stick would not allow any backward movement, and when I attempted to observe the sky to the east of the sun I could not. This confined my area of vision to a small distance west of the sun. But to return to the observation. My observation of the first contact was four seconds later than the observation of Prof. Colbert. The following is my record of events by Washington time:—

	h.	m.	s.
First contact	4	11	18
Bailey's beads	5	20	22
Beginning of totality	5	20	38
Corona first seen	5	23	17
End of totality	5	23	26
End of eclipse	6	26	35

"The watch was one second slow of the chronometer at the first contact, two seconds at the end of totality, and four seconds at the end of the eclipse. Before the eclipse began I had made up my mind to observe the general phenomena, the corona, protuberances, and Bailey's beads for about half a minute at the beginning of totality. I designed a minute and a half for a search for Vulcan, and the remainder, some forty odd seconds, to observe the phenomena at the end of totality.

"About one minute after totality two stars caught my eye about three degrees, by estimation, south-west of the sun. I saw them twice, and attempted a third observation, but a small cloud obscured the locality. The stars were both of the fifth magnitude, and but one is on the chart of the heavens. This star I recognised as Theta in Cancer. The two stars were about eight minutes apart. There is no such configuration of stars in the constellation of Cancer. I have no doubt that the unknown star is an intra-Mercurial planet, and am also inclined to believe that there may be more than one such planet. In 1859 the French astronomer Lescarbault claimed that he had seen an intra-Mercurial planet crossing the sun's disc. He related his discovery to Leverrier, who became a firm believer in the existence of such a planet. The perturbations of Mercury's orbit demand such a planet as Leverrier named Vulcan. The star I saw may have been the same that was seen by Prof. Watson, who was located at Rawlings, Wy. T.

"I possessed a comet eye-piece of very flat and large field and distinct to the very edge. It was made in this city, and to it and my blunder in failing to untie my instrument I owe my success. Prof. Colbert, of our party, also searched for Vulcan, but his field was not large. I saw but two protuberances, and those just at the end of totality. The advancing moon uncovered them. I had a view for at least two seconds of the sun's chromosphere at the same time. The chromosphere, by my measurement, is 2,000 miles in thickness. It is a layer of red-hot hydrogen surrounding the sun. The protuberances are projected from it.

"The corona was unusually extensive. It had never been seen so far extended. The greatest prolongation was in the direction of the moon's path across the sun, and as drawn by some of the parties extended on each side of the sun to a distance of more than three million miles. The pencils of light were radial mostly, though some of them were curved. I came away so quickly from Denver that I did not learn of the success of the other parties. In comparing notes with our party, Prof. Hough agreed with me in the measurement of the chromosphere. This measurement is made by calculating the time it takes the moon to pass over it. I learned of Prof. Watson's discovery the day after the eclipse. I have not seen him since he made the observation."

OUR ASTRONOMICAL COLUMN

WATSON'S SUSPECTED PLANET.—At the instance of M. Mouchez, the director of the Bureau des Calculs of the Observatory at Paris, M. Gaillot, who so long assisted Leverrier in the formation of his planetary tables, has examined how far the position of the object seen by Prof. Watson will accord with the more probable of the orbits which Leverrier inferred for a hypothetical planet, from the observations of suspicious spots in transit over the sun's disc. It may be remembered that their discussion led to a general formula, which was thus expressed by Leverrier; ν being the heliocentric longitude of the planet, k an indeterminate which might have values positive or negative, but necessarily whole numbers, and j the number of days reckoned from the beginning of the year 1750 :—

$$\nu = 139^{\circ}{\cdot}94 + 214^{\circ}{\cdot}18\,k + (10^{\circ}{\cdot}901252 - 1^{\circ}{\cdot}972472\,k)\,j$$
$$+ (-5^{\circ}{\cdot}3 + 5^{\circ}{\cdot}5\,k)\cos.\nu.$$

M. Gaillot has found that, of the four possible orbits retained by Leverrier, corresponding to $k = -2, -1, 0$, and $+1$, respectively, the first agrees the closest with the observation. With this value of k the diurnal motion is $14^{\circ}{\cdot}8462$, the semi-axis major 0·164, and the period of revolution 24·25 days—less than the period of the sun's rotation. When the question of eccentricity is introduced, it is remarked that in the preferable orbit it is already very considerable, and comparable with that of the orbit of Mercury, and it is easy to demonstrate, to use M. Gaillot's words, "qu'il peut y avoir identité absolue entre la position observée et la position prévue." In fact, he finds that the agreement will be perfect if the eccentricity is assumed 0·14, and the longitude of the perihelion 74°. With regard to the inclination of the orbit to the ecliptic, M. Gaillot, from further consideration, supposes it may not exceed 7°. He notes that the most serious objection which opposes itself to the identification of the object observed, with a planet moving in the orbit indicated by Leverrier's formula, is that we should see but a very small part of the disc illuminated, and without denying that there is reason in this objection, M. Gaillot adds that Prof. Watson describes "as being of the fourth magnitude, a star the diameter of which may be comparable with that of Mercury, and which, in superior conjunction, may appear of the first magnitude." He further remarks that while it is not possible to decide with certainty upon the identity of

Prof. Watson's planet with that of which Leverrier has indicated the track, he believes he has shown that there is no incompatibility between the observed and hypothetical objects. If only one such planet exist between Mercury and the sun M. Gaillot points out that, in order to account for the accelerated motion in the perihelion of Mercury, its mass must be nearly equal to that of the latter—an inference drawn from Leverrier's table in vol. v. of the Paris *Annales*. An ephemeris extending to September 1 is appended to M. Gaillot's communication in the *Comptes Rendus* of August 5. Remarking that the assumed sidereal period of Prof. Watson's planet is 24·25 days, the synodical period is nearly twenty-six days, and accordingly we find by the ephemeris that the body should pass nearly at the same distance in longitude and latitude from the sun on August 24. But considering that this must hold during the next revolution whatever the period of any possible intra-Mercurial planet may be, it may be suggested that the most effectual plan of search will be to watch daily the vicinity so indicated with our larger instruments beyond the period at which the hypothetical planet should pass according to M. Gaillot's ephemeris. To set the equatorial it will be sufficient to subtract 9m. 50s. from the sun's right ascension at the proposed time of search, and to add to the sun's N.P.D. a quantity varying from 23′ on August 22, to 17′ on September 10.

A COMPANION OF α LYRÆ.—On several occasions during the last ten years, to our knowledge, attention has been directed to a star near α Lyræ in the *n. f.* quadrant, and suspicion of variability entertained, from the observer not having distinctly remarked it previously. An inquiry on the same point was lately addressed by a correspondent to Prof. Winnecke. The star is on an angle of about 42°, distance 139″. In October, 1870, it was a full magnitude fainter than the well-known Herschelian companion. Possibly some reader interested in the variable stars may be able to say if there is any reason to include the more distant star in this class of objects. In due course the direction of the proper motion of the large star will bring it immediately upon this *comes*, supposing there be no physical connection.

SCHMIDT'S "CHARTE DER GEBIRGE DES MONDES."—We hope next week to give some account of this most laborious and valuable work, which has been produced, through the liberality and scientific spirit of the Prussian government, in a style and with a perfection of arrangement that reflect the highest credit on all concerned. Probably no astronomical work could possess a greater degree of interest for amateurs generally, and—considering the attention paid to the examination of the moon's surface in this country—to British amateurs especially.

GEOGRAPHICAL NOTES

THE Arctic exploring ship *Alert* is being again fitted out for active duty, under the command of her old captain, Sir George Nares. She is intended for a voyage of surveying service principally in the South Pacific. Her first work will be an examination of the inner water leading from the Straits of Magellan to the Gulf of Peñas, along the seaboard of Chili; from this she will stretch across the South Pacific Ocean towards Fiji adding (*en route*) as far as practicable to our knowledge of the hydrography of the Low Archipelago, Society and Friendly Islands. After a few months spent in the neighbourhood of Fiji and in an examination of dangers lying in the track of navigation between that group and the Colony of New Zealand, she will, for the latter part of her voyage, be employed off the North Western Coast of Australia, principally in ascertaining the positions of, and as far as necessary charting, the various reefs and islets lying off the Australian continent, and between it and the ports of the Dutch Indies, at many of which reefs, &c., traffic has been for some time increasing in the search for trepang, pearls, and guano.

THE *Mittheilungen* of the Vienna Geographical Society, Nos. 6 and 7, contains a valuable "Culture-Map" of Asia Minor, exhibiting in a satisfactory manner the various zones of vegetation which mark that region recently brought into such intimate relations with this country. The map is by A. v. Schweiger-Lerchenfeld, who contributes also the explanatory text. Dr. Ziegler describes the important works carried on during 1877-8 by the Swiss correspondents of the Society, and Prof. Schmick contributes a paper on Ocean Currents.

FROM America we have No. 2, 1878, of the always interesting *Bulletin* of the American Geographical Society. A paper on "Japan, Geographical and Social," by the Rev. W. E. Griffis, contains the results of much research, as well as of personal observation, and is an important contribution to our knowledge of that country. Dr. Wright Hawkes discusses in an able and unprejudiced manner "The So-called Celtic Monuments of Britanny," his conclusion being that the evidence as to their origin is very conflicting. Mr. Jess Young, who was astronomer to Giles's trans-Australian expedition, gives an account of the results of his observations while crossing the great Australian desert.

THE Geographical Society of St. Petersburg intends to publish Karl Ritter's works in Russian in celebration of his jubilee.

WE learn that a new branch of the Russian Geographical Society, independent of those of Orenburg and Western Siberia, will shortly be opened at Tashkent.

WE have received from Williams and Norgate a neat and well-executed map of Cyprus, by Kiepert of Berlin, upon a sufficiently large scale to show distinctly the chief features of the island.

NOTES

PROF. MENDÉLEEFF is to be absent from his post in the St. Petersburg University for a year for the purpose of visiting Western Europe, where he will devote his time to the preparation of a large work on aëronautics. The work will contain a historical sketch of the subject, and expound its present condition from a scientific point of view.

THE appearance is announced of a biography of the late Prof. von Baer, by Dr. Stida, Professor in the Dorpat University. The autobiography of Baer appeared some years before his death, but embraced only his childhood and youth. The work of Dr. Stida is chiefly devoted to the scientific life of Baer, and contains a complete review of his works.

WE are glad to see that the *Times* is beginning to recognise the national importance of science-teaching in schools, and the necessity for our legislators being able to estimate the bearings of the various problems in physical science which are involved in the measures that come before them, in which the national welfare is involved. In a leading article on the meeting of the British Association the *Times* says that "We are living in a time when legislation is busy with physical matters, and is likely to become more so. The tendency of unscientific persons, especially when they are politicians, is to ignore the certainties which physical science furnishes, and hence to suppose that legislation about physical matters may properly be conducted upon a basis of compromise, like legislation about matters of opinion. It is very important that people who are not scientific themselves, and who never will be, should yet possess enough scientific knowledge to understand the difference which separates questions on which compromise is proper or expedient from those in which it would be fatal to the attainment of the desired result." The *Times* seems to us inclined to rate too highly the educational influences of the British Association on our legislators and on the general public. The rational conclusion to be drawn from the admissions in the passage we have quoted is that there ought to be some kind of scientific council which Government could consult in regard to measures involving questions of science. The British Association has done good service in helping to draw the attention of the public to the real nature of science, but it can never be a substitute for such a council as that which the most eminent of our men of science maintain would be the only effectual guarantee for enlightened legislation in scientific matters, and the establishment of which was recommended by the Duke of Devonshire's Commission. It is gratifying to find that the *Times* admits that "physical science affords an admirable means of mental training in schools," and we trust the day is not distant when it will be placed on a footing of equality with other branches of education. From another leader on Prof. Huxley's address we may infer that this chief representative of average British opinion has advanced from its obstinate opposition to the doctrines associated with the name of Darwin; indeed, the tone of the article to which we refer seems even more "advanced" than the address which was the occasion of it. It is comforting to find that, in scientific matters, at least, a better spirit is beginning to pervade the paper which is both an index and a leader of middle-class public opinion.

THE time of meeting of the German Naturalists and Physicians at Cassel has been changed from September 18–24 to September 11–18. This change has been caused by the fact that during the time originally fixed a series of military manœuvres are to take place which will cause a great influx of military men into Cassel, the lodging resources of which, it is feared, would not be commensurate with the requirements of a simultaneous meeting of the men of science and of war.

THE meeting of the French Association will not take place at the Palais des Beaux Arts, as it was originally contemplated, but in the rooms of the Lycée Saint Louis. The session begins to-day.

A SOMEWHAT curious discussion has been recently occupying the attention of the Paris Academy of Sciences. It originated in the publication by M. Berthelot, in the *Revue Scientifique* of July 20, of a series of laboratory notes by the late Claude Bernard, on alcoholic fermentation, in which the great physiologist came to conclusions opposed to those so long advocated by M. Pasteur. It was natural that M. Pasteur should endeavour to weaken the force of M. Bernard's experiments and conclusions. In the *séance* of July 22 he drew attention to the publication of the notes, and explained their apparent opposition to his theory by the statement that Bernard's method of working was to proceed to the investigation of every theory as if its opposite were true, thus submitting every point to a crucial test. He declared his intention of taking up Bernard's series of experiments and working them out from the latter's standpoint, in the confidence that the result would be entirely in favour of his (Pasteur's) theory of fermentation. At the next sitting of the Academy M. Pasteur stated that he had found that many changes had been made for typographical and other reasons in the notes as they appeared in the *Revue* as compared with the original MSS., though these changes do not seem materially to alter the sense of the "Notes." M. Berthelot rejoices that M. Pasteur intends to repeat Bernard's experiments, and whatever may be the result, science is likely to be a gainer. There at present the matter rests.

WE notice the death in Jena, on July 25, of the well-known botanist Prof. Christian Eduard Langethal. He was born at Erfurt in 1806. After completing his botanical studies at Jena he was for some time assistant to the famous agriculturist Prof. Schulze, and in 1835 teacher of natural sciences at Eldena. In 1839 he was called to a professorship at Jena, which he occupied up to the date of his death. Prof. Langethal was the author of several standard works, among others, "Geschichte der deutschen Landwirthschaft" (1846-56); "Lehrbuch der landwirthshaftlichen Pflanzenkunde" (1841-45; fifth edition, 4 vols. 1876); "Beschreibung der Gewächse Deutschlands" (1858); "Terminologie der beschreibenden Botanik;" and "Flora von Thüringen," issued in conjunction with Schenk and Schlechtendal.

THE model of the Gauss monument has just been finished by Prof. Schaper. The design is made after an original portrait of Gauss, in the possession of Göttingen University, and the monument itself is now to be executed in bronze at the atelier of Herr Gladenbeck, of Berlin. It will be erected at Brunswick, the birthplace of the great mathematician.

THE Royal Academy of Sciences at Munich has just elected to membership Prof. Krehl, of Leipzig, and Mr. Charles Darwin.

MEASURES are being taken for the foundation of a geological institute at St. Petersburg, which shall accomplish for the Russian empire what the Imperial Institute at Vienna has done for Austria. At present geological work is attempted only at the instigation of mining companies and the learned societies, and the want of unity in the efforts made for the development of Russian geology has long been painfully felt.

THE French Minister of Public Instruction has authorised the director of the newly created Central Meteorological Bureau to hire a hotel in the Rue de Varenne, in a populous part of Paris, where no observations can be taken, in which to establish the offices of his administration, which, within a few days, will be removed from the observatory. It is fair to state that Admiral Mouchez will not discontinue the taking of meteorological observations with the instruments established by Arago. It is pretty certain that the magnetical instruments established at a great expense by Leverrier will not be removed at a period so important in the history of terrestrial magnetism. We can state that new observers will be paid for that purpose, if necessary.

THE Meteorological Commission of Vaucluse again this year ascended Mont Ventoux. M. Mascart, the new director of the Meteorological Central Bureau, was one of the party, having come from Paris for that purpose. Simultaneous observations were taken at Orange, Carpentras, Avignon, and Apt. But the principal object of this scientific excursion was to determine the best manner of erecting the contemplated observatory at the top of this mountain, which is 1,919 metres above the level of the sea, and 1,692 above Apt, the nearest meteorological station.

THE Giffard captive balloon has become a great favourite amongst the visitors to Paris, as well as the Parisians themselves. The greatest number of ascents in one day has been seventeen, and the money taken has been 6,000*l*. in eighteen days, including three in which ascents could not take place, owing to the boisterous state of the weather. The charges are one franc for witnessing the ascents, and twenty francs for ascending.

THE Lords of the Committee of Council on Education have determined to award bronze medals to students who obtain a first-class in honours in any subject of science at the May examinations.

IN an Appendix to the Annual Report (for 1878) of the Lyceum of Valparaiso, Mr. Edwyn C. Reed gives an account of the progress he has made in commencing a Museum of Natural History for this Institution, which, if we understand rightly, is not intended to be simply for the instruction of the scholars of the Lyceum, but for the City of Valparaiso generally. The new Museum does not appear to be well off for funds, but Mr. Reed has many friends and correspondents in this country who will be able to serve him by exchanges.

THE second marine excursion of the Birmingham Natural History and Microscopical Society to the Island of Arran, in the month of July last, proved most successful. Twenty-eight members, including six ladies, formed the party, who travelled by Pullman cars and family carriage by Midland railway, going by night and returning by day. A small steam yacht—the *Lizzie*—was chartered for a week. There were also botanical and geological excursions daily to the many interesting parts of the island. The results of the dredgings were most satisfactory, and a beautiful series of specimens was taken, including *Luidia fragillisima* and two or three Nudibranchs new to the locality. The towing-net—on an improved principle, devised by Mr. Henry Allport—was also used most successfully, and many interesting forms of marine life taken, notably *Bipinnaria* and *Pluteus*. The examination of these and other microscopic objects in the evenings in the ladies' drawing-room proved a great attraction. Preliminary reports have been made to the society and the specimens exhibited:—General, by Mr. Edmund Tonks, B.C.L., president, and Mr. Sam. Timmins, F.R.S.L.; Botanical, by Mr. John Morley, hon. sec.; Dredging Arrangements, Mr. John F. Goode; and Marine Zoology, by Mr. W. R. Hughes, F.L.S. The Geological Report, by the Rev. George Deane, D.Sc., F.G.S., was deferred. A full account of the proceedings will appear in an early number of the *Midland Naturalist*. The excursion extended from July 19 to 27, and a most interesting and enjoyable week was passed. A resolution was unanimously passed suggesting to the society that the next excursion should be to Falmouth.

WE have on several occasions drawn attention to the interesting department in the Paris Exhibition devoted to school equipments, and it is hopeful to find that in connection therewith, a *Times* Paris correspondent has discovered the success of the French in their efforts to impart to the nation a technical education. "One of the most interesting and instructive departments of the Exposition," the *Times* correspondent writes, "is that devoted to the illustration of the working and results of the system of French popular education both in Paris and in the Provinces. The foreign visitor who observes with admiration throughout the country the evidences of the general artistic and technical skill of the French workmen of every class will see in this Educational Department of the Exposition the key to the secret of that success. When will the corporation of London be able to match the interesting educational results here displayed by the sister municipality of Paris?"

UNDER the presidentship of Lord Hardwicke the opening meeting of the thirty-fifth annual congress of the British Archæological Association began on Monday at the ancient town and port of Wisbech.

ZOOLOGISTS will be glad to know that the "Rules for Zoological Nomenclature," drawn up by the late H. E. Strickland, F.R.S., at the instance of the British Association, have been reprinted. The "Notes" were prepared after consultation with many zoologists, British and foreign, and are now brought out under the care of Mr. P. L. Sclater. The publisher is Mr. Murray.

PROF. BROCA opened the International Congress of Anthropology at the Paris Exhibition by a short address, in which he pointed out the necessity of rigid observation as the only means of obtaining trustworthy data on which to build the science.

WE are glad to learn that the French balloon service has not een disorganised by the resignation of Col. Laussedat. The new head of the service is General Farr, who distinguished himself in the last Franco-German war in the northern part of France.

DR. ERNST, of Carácas, writes us that by an oversight in his note on the earthquake of Cúa (NATURE, vol. xviii. p. 130) the direction of the shock was not given. It came from E.N.E., or more exactly E. 15° N.

THE article on the Elasmotherium from which our description was obtained (vol. xviii. p. 387) appeared in No. 23 of the Russian journal *Niwa* for 1878, p. 411.

THE additions to the Zoological Society's Gardens during the past week include a Lion (*Felis leo*), a Patas Monkey (*Cercopithecus ruber*) from West Africa, presented by Mr. W. H. Wylde, s. s. *Agra*; a common Paradoxure (*Paradoxurus typus*) from India, presented by Mr. Edwin Etty Sass; two Common Buzzards (*Buteo vulgaris*), European, presented by Master Valentine Marks; two Herring Gulls (*Larus argentatus*), European, presented by Mr. Thomas Landseer; a Crested Ground Parrakeet (*Calopsitta novæ-hollandiæ*) from Australia, presented by Mrs. Parker; a many-zoned Hawk (*Melierax polyzonus*) from East Africa, presented by Mr. C. H. Fisher; a Copper-head Snake (*Cenchris centortrix*) from Pittsburg, U. S. A., presented by Dr. F. Painter, F.Z.S.; a Macaque Monkey (*Macacus cynomolgus*) from India, two Barbary Apes (*Macacus inuus*) from North Africa, two Beautiful Parrakeets (*Psephotus pulcherrimus*) from Australia, thirteen Greek Land Tortoises (*Testudo græca*) European, deposited; a Bladder-nosed Seal (*Cystophora cristata*) from the North Atlantic, six Common Kingfishers (*Alcedo ispida*) British Isles, purchased.

THE BRITISH ASSOCIATION

DUBLIN, *Tuesday*.

SINCE we last wrote the British Association week has come and gone with even more than its wonted rush and whirl of incessant engagements. That the meeting has been a successful one in every respect is unquestionable. Though no very original papers have been read or any great sensation excited in any section, as at Plymouth, by the telephone, yet the meetings have been above the average in general interest, the evening lectures and entertainments having been specially attractive, and the attendance extremely good. This meeting will always be memorable for the splendid address of the president, Dr. Spottiswoode.

The proceedings have been very fairly and fully reported in the local papers. Is it too much to hope that, ere long, the Association may see its way to utilising the energy now invariably displayed by the provincial press in giving to its members an early, full, and revised report of the proceedings of the meeting in a more convenient form than in the columns of a newspaper? We are aware that this question has often been mooted, and indeed it has not been untried, but for its permanent success a well-considered scheme is necessary, and doubtless the local press, as well as the sectional secretaries (more especially if some honorarium were attached to one secretary in each section) would gladly lend their aid. If we may judge by the constant inquiries for such a report that reach our ears at each meeting of the Association, it would seem to be a widely-felt want. Our own columns have, to a large extent, been opened to meet this need; but it is obvious that the constant pressure upon our space only permits a partial report of the proceedings, whilst the discussions upon the papers, often extremely valuable, have no permanent record, even in a condensed form.

There is another point upon which we may venture to say a word or two, and that is, the desirability of giving a little more attention to the fact that a large majority of those present at the meetings of the Association have had no special scientific training, and to whom, therefore, almost the whole of the sectional proceedings are one continuous riddle. Would the actual usefulness of the Association to scientific men be in any way lessened by devoting a certain portion of the proceedings of each section at a specified time to papers specially prepared for an audience, not of specialists, but simply of intelligent men and women, and to reports on the progress of the various branches of science, couched in untechnical language? Would not such a course be a distinct gain in the diffusion of science, and in its permanent effect upon the town where the annual meetings are held; and would it not enable the general public to give a more hearty and sympathetic support to the scientific worker?

Let us now put on record a brief epitome of the principal events in the meeting which has just closed. On Wednesday night, August 14, Dr. Spottiswoode delivered his presidential address to a crowded audience, but, unfortunately, in a room only suited to the strongest lungs. The concert-hall of the Exhibition Palace was the only room available for the purpose, and the Association were fortunate in having chosen lecturers with such powerful voices as Mr. Romanes and Prof. Dewar, who subsequently delivered their discourses in the same place.

The first lecture, by Mr. Romanes, on Animal Intelligence, was admirably delivered and excited general interest by its lucid and masterly exposition of the psychological affinities between the mind of man and that of the lower animals.

The lecture by Prof. Dewar, on "Dissociation, or Modern Ideas of Chemical Action," delivered on the 19th, will be memorable for its magnificent display of experimental illustrations; the prodigious labour involved in the preparation for the successful execution and for rendering visible to an immense audience (as was the case) a multitude of novel and delicate experiments, generally seen only on a small scale, but here magnified to astonishing proportions, can only be known to those who have gone through a like, though lesser, toil.

Numerous *conversazioni* and private entertainments must leave upon the minds of the members a not unjust impression of the gaiety and hospitality of the citizens of Dublin in their corporate as well as in their private capacity. First there came an unusually brilliant *soirée* given by the Royal Dublin Society, many of the objects of interest in which were reproductions lent by the South Kensington Museum from the valuable loan collection of 1876. To the untiring exertions of Mr. J. H. Wigham, of the firm of Messrs. Edmundson, both on this occasion as well as on others, the hearty thanks of the Association are due. All the powerful machinery in actual use in first-class lighthouses for the purposes of illumination, as well as for signalling in fogs, was erected for the use of the Association in the spacious premises of the Royal Dublin Society, at the sole expense of the firm Mr. Wigham has made so well known.

On Friday evening the Rector and Vice-Rector of the Catholic University gave another *conversazione*. The brilliancy of the electric lights, derived from a Gramme machine, exhibited here on this and on subsequent occasions, delighted everyone. The phonograph was exhibited in the physical lecture-hall, songs were sung and poetry recited by this instrument with the most perfect success. In one instance a song which had been sung into the phonograph by a well-known Dublin vocalist was turned on, after the lapse of an hour, in the presence of a new audience, who had not heard the original, and not only were the words and the melody easily recognised, but those who were familiar with the style of the singer were able at once to tell whose voice it was that came forth thus wonderfully from the rotating cylinder. In another hall, specially darkened for the purpose, Mr. Spottiswoode's vacuum tubes were exhibited, to great advantage by means of a splendid induction coil made by Mr. H. Yeates, of London. It was shown, too, how the stratification of the electric light, in these tubes, may be unravelled by the aid of a rotating mirror, the character of the stratification changing most remarkably with every change in the strength of the current. These experiments were greatly facilitated by the use of a new form of Bunsen's bichromate of potash battery, lately devised by Dr. Molloy, Professor of Physics in the Catholic University, for general use in the laboratory. This battery, which consists of six large cells, is always ready for immediate use, and is equal in power to at least eight quart cells of Grove's battery.

The great hall of the University was laid out with refreshments on one side, while on the other a varied and most successful series of interesting and brilliant experiments, illustrating the phenomena of heat, light, sound, and electricity, offered an intellectual feast even more attractive to the great body of the visitors. Upwards of 700 guests were received during the evening, but, owing to the admirable arrangements, all were able to make their way through the several halls without any inconvenient pressure. Amongst the guests we noticed most of the distinguished English and foreign *savans*, the President of the Association and Mrs. Spottiswoode, Sir John and Lady Lubbock, Sir Joseph Hooker, Prof. Roscoe, Prof. Gladstone, &c.

We understand that it is to Dr. Molloy, the genial and accomplished Professor of Physics at, and Vice-Rector of the Catholic University, the Association is indebted for one of the pleasantest evenings during this meeting.

On Monday the President and Fellows of the Royal College of Surgeons gave a reception to the chief members of the Association, and on Tuesday evening an interesting *soirée*, open to the Association generally, was given by the Royal Irish Academy. The Lord Lieutenant and the Duchess of Marlborough were present both on Thursday evening at the Royal Dublin Society and on Tuesday at the Royal Irish Academy.

Saturday, as well as Thursday, was devoted to excursions, particulars of which we gave in a former impression.

At the general committee on Monday, Mr. Sorby, and other delegates, attended to invite the Association to meet at Sheffield, an invitation which was cordially and unanimously accepted. Dr. Allman was chosen president, and Swansea was fixed upon as the place of meeting for 1880. The officers of the Association were re-appointed, and the election of Mr. J. E. H. Gordon as assistant general secretary was confirmed. The retirement of Mr. Griffith from this post which he has so long and admirably filled, caused universal regret. We hope it may not be considered impertinent if, in conclusion, we also express our sense of the great loss which the Association has sustained in the resignation of Mr. Griffith, who, in spite of the many demands upon his time and patience, has combined a systematic and energetic discharge of his duties with the most calm and courteous demeanour during the fifteen years that he has filled the arduous post.

To-day the University of Dublin conferred the honorary degree of LL.D. on the following members of the Association:—Mr. W. Spottiswoode, Prof. H. J. S. Smith, Dr. Janssen, Prof. Maxwell Simpson, Prof. E. Roscoe, Prof. Alexander W. Williamson, Mr. Evans, Sir John Lubbock, Sir J. D. Hooker, Prof. W. H. Flower, Prof. Huxley, Sir Wyville Thomson, and Prof. J. Thomson.

(*By Telegram.*)

DUBLIN, *Wednesday.*

The attendance at the concluding general meeting has been considerably above the average, the total number present being 2,577, of which 1,959 had either Association or ladies' tickets, whilst 110 new members joined. The total sum received by the treasurer for tickets was 2,605*l*. Last year the attendance was 1,229, and the sum taken 1,268*l*.

The following grants were made :—

	£
Mathematics and Physics.	
Cayley, Prof.—Calculation of Factor Tables for the Fifth and Sixth Millions (re-appointed)	150
Sylvester, Prof.—Tables of Fundamental Invariants of Algebraic Forms	50
Forbes, Prof. G.—Observation of Atmospheric Electricity at Madeira (renewed)	15
Haughton, Rev. Dr.—Tables of Sun-Heat Coefficients	30
Joule, Dr.—Determination of Mechanical Equivalent of Heat (renewed)	65
Forbes, Prof.—Instrument for Detecting Presence of Fire-damp in Mines	30
Ayrton, Mr.—Specific Inductive Capacity of a good Sprengel Vacuum	40
Glaisher, Mr.—Luminous Meteors	20
Gill, Mr. D.—Improvements in Astronomical Clocks ...	30
Chemistry.	
Roberts, Mr.—Composition and Structure of some of the less known Alkaloids (re-appointed)	25
Wallace, Dr.—Development of Light from Coal-gas of different qualities (re-appointed)	10
Adams, Prof. W. G.—Electrolysis of Metallic Solutions and Solutions of Compound Salts...	20
Geology.	
Evans, Mr. John.—Exploration of Caves	50
Hull, Prof.—Circulation of Underground Waters (re-appointed)	15
Godwin Austen, Mr.—Kentish Boring Exploration (renewed)	100
Evans, Mr. John.—Kent's Cavern Exploration (re-appointed)	100
Evans, Mr. John.—Record of Progress of Geology	100
Haughton, Rev. Dr.—Fermanagh Caves Exploration (re-appointed...	5
Close, Rev. Maxwell.—Miocene Flora of Basalt of North of Ireland	20
Biology.	
Spence-Bate, Mr. C.—Marine Zoology of South Devon re-appointed)	20
Stainton, Mr.—Record of Zoological Literature	100
Foster, Dr. M.—Table at the Zoological Station, Naples (re-appointed)	75
Brooke, Sir Victor.—Illustrations for a Monograph on the Mammoth	17
Sclater, Mr. P. L.—Natural History of Socotra	100
Rolleston, Prof.—Exploration of Bone Caves in South Wales (partly renewed)...	50
Fox, General Lane.—Exploration of Ancient Earthworks (re-appointed)	25
Fox, General Lane.—Excavations at Port Stewart, and elsewhere in the North of Ireland	
Economic Science and Statistics.	
Farr, Dr.—Anthropometric Committee (re-appointed) ...	50
Mechanical Science.	
Thomson, Sir W.—Datum Level of the Ordnance Survey (re-appointed)	10
Froude, Mr. W.—Instruments for Measuring the Speed of Ships (renewed)...	50
Napier, Mr. G. R.—Steering of Screw Steamers (re-appointed)	10
Thomson, Sir W.—Tidal Observations in the English Channel (re-appointed)	10

REPORTS.

Report of the Committee, consisting of Prof. Cayley, Dr. Farr, Mr. J. W. L. Glaisher, Dr. Pole, Prof. Fuller, Prof. A. B. W. Kennedy, Prof. Clifford, and Mr. C. W. Merrifield, appointed to consider the Advisability and to estimate the Expense of constructing Mr. Babbage's Analytical Machine, and of printing Tables by its means. Drawn up by Mr. Merrifield.—We desire in the first place to record our obligations to General Henry Babbage for the frank and liberal manner in which he has assisted the Committee, not only by placing at their disposal all the information within his reach, but by exhibiting and explaining to them, at no small loss of time and sacrifice of personal convenience, the machinery and papers left by his father, the late Mr. Babbage. Without the valuable aid thus kindly rendered to them by General Babbage it would have been simply impossible for the Committee to have come to any definite conclusions, or to present any useful report.

We refer to the chapter in Mr. Babbage's "Passages from the Life of a Philosopher," and to General Menabrea's paper, translated and annotated by Lady Lovelace, in the third volume of "Taylor's Scientific Memoirs," for a general description of the analytical engine.

The Report then, in Section I., contains an account of the general principles of calculating engines, and proceeds :—

II. *Special Characteristics of Mr. Babbage's Analytical Engine.*—1. *The Mill.*—The fundamental operation of Mr. Babbage's analytical engine is simple addition. This and the other elementary rules of subtraction, multiplication, and division, and all combinations of these, are performed in what is called "*the mill.*" All the shifts which have to take place, such as changing addition into subtraction by throwing a reversing train into gear, or the shift of the decimal place, carrying and borrowing, and so forth, are effected by a system of rotating cams acting upon or actuated by bell-cranks, tangs, and other similar devices commonly used in shifting machinery, sometimes under the name of clutches or escapements. These clutches and bell-cranks control the purely additive and carrying processes effected in the additive trains described in the note to Section I., and, *being themselves suitably directed*, secure that the proper processes shall be performed upon the proper subject-matter of operation, and duly recorded, or used, as may be required.

2. *The Store.*—A series of columns, each containing a series of wheels, constitutes the store. This store, which may be in three or more dimensions, both receives the results of operations performed in the mill, and serves as a store for the numbers which are to be used in the mill, whether as original or as fresh subjects of operation in it. Each column in the store corresponds to a definite number, to which it is set either automatically or by hand, and the number of digits in this number is limited by the number of wheels carried on the shaft of the column. The wheels gear into a series of racks, which can be thrown into or out of gear by means of the cards.

3. *Variable Cards.*—All the numbers which are the subject of operation in the mill, whether they are the result of previous operations therein, or new numbers to be operated upon for the first time, are introduced to it in the form of Jacquard [1] cards, such as are used in weaving. One set of wires or axes transfers the numbers on these cards to the subject of operation in the mill, exactly as similar cards direct which of the warp threads are to be pushed up, and which down, in the Jacquard loom. The mill itself punches such cards when required.

4. *Operation Cards.*—A different set of cards selects and prescribes the sequence of operations. These act, not upon the number wheels of the mill or store, but upon the cams and clutches which direct the gearing of these wheels and trains. Thus, in such an operation as $(a\,b + c)\,d$, we should require :—

1st, four variable cards with the numbers a, b, c, d.

2nd, an operation card directing the machine to multiply a and b together.

3rd, a record of the result, namely, the product $a\,b = p$, as a fifth variable card.

4th, an operation card directing the addition of p and c.

5th, a record of the result, namely, the sum $p + c = q$, as a sixth variable card.

[1] In a letter written by Mr. Babbage to Arago in December, 1839, the following explanation of the use of these cards is given. It probably conveys the idea in the fewest words possible. It is only necessary to add that their twofold employment embodies the separation of the symbols of operation from those of quantity. "You are aware that the system of cards which Jacquard invented are the means by which we can communicate to a very ordinary loom orders to weave any pattern that may be desired. Availing myself of the same beautiful invention, I have by similar means communicated to my calculating engine orders to calculate any formula, however complicated ; but I have also advanced one stage further, and I have communicated through the same means orders to follow certain laws in the use of those cards, and thus the calculating engine can solve any equations, eliminate between any number of variables, and perform the highest operations of analysis."

6th, an operation card directing the machine to multiply q and d together.

7th, a record of the result, namely, the product $q\,d = p_2$, either printed as a final result or punched in a seventh variable card.

III. *Capability of the Engine.*—It has already been remarked that the direct work of the engine is a combination and repetition of the processes of addition and subtraction. But in leading up to any given datum by these combinations, there is no difficulty in ascertaining tentatively when this datum is reached or about to be reached. This is strictly a tentative process, and it appears probable that each such *tentamen* requires to be specially provided for, so as to be duly noted in the subsequent operations of the machine. There is, however, no necessary restriction to any particular process, such as division; but any direct combination of arithmetic, such as the formation of a polynomial, can be made to lead up to a given value in such a manner as to yield the solution of the corresponding equation. In any such process, however, it is evident that there can be only (to choose a simile from mechanism) one degree of freedom; otherwise the problem would yield a locus, indeterminate alike in common arithmetic, and as regards the capabilities of the machine. The possibility of several roots would be a difficulty of exactly the same character as that which presents itself in Horner's solution of equations, and the same may be said of imaginary roots differing but little from equality. These, however, are extreme cases, with which it is usually possible to deal specially as they arise, and they need not be considered as detracting materially from the value of the engine. Theoretically, the grasp of the engine appears to include the whole synthesis of arithmetic, together with one degree of freedom tentatively. Its capability thus extends to any system of operations or equations which leads to a single numerical result.

It appears to have been primarily designed with the following general object in view: to be coextensive with numerical synthesis and solution, without any special adaptation to a particular class of work, such as we see in the difference engine. It includes that, *à majori*, and it can either calculate any single result or tabulate any consecutive series of results just as well. But the absence of any specialty of adaptation is one of the leading features of the design.

Mr. Babbage has also considered the indication of the passage through infinity as well as through zero, and also the approach to imaginary roots. For details upon these points we must refer to his "Passages from the Life of a Philosopher."

IV. *Present State of the Design.*—The only part of the analytical engine which has yet been put together is a small portion of "the mill," sufficient to show the methods of addition and subtraction, and of what Mr. Babbage calls his "anticipating carriage." It is understood that Gen. Babbage will (independently of this report) publish a full account of this method. No further mention of it will therefore be made here.

V. *Probable Cost.*—Without attempting any exact estimate, we may say that it would surprise us very much if it were found possible to obtain tenders for less than 10,000*l.*, while it would pretty certainly cost a considerable sum to put the design in a fit state for obtaining tenders. On the other hand it would not surprise us if the cost were to reach three or four times the amount above suggested.

Section VI. refers to *Strength and Durability*, and VII. to *Probable Utilisation of the Analytical Engine.*

VIII. *Possible Modification of the Engine.*—Without prejudging the general question referred to us as to the advisability of completing Mr. Babbage's engine in the exact shape in which it exists in the machinery and designs left by its inventor, it is open to consideration whether some modification of it, to the sacrifice of some portion of its generality, would not reduce the cost and simplify the machinery so as to bring it within the range of both commercial and mechanical certainty. The "mill," for example, is an exceedingly good mechanical arrangement for the operations of addition and subtraction, and with a slight modification, with or without store-columns, for multiplication. We have already called attention to the imperfection of the existing machines, which show weakness and occasional uncertainty. It is at least worth consideration whether a portion of the analytical engine might not thus be advantageously specialised so as to furnish a better multiplying machine than we at present possess. This, we have reason to believe, is a great desideratum both in public and private offices, as well as in aid of mathematical calculators.

Another important desideratum to which the machine might be adapted without the introduction of any tentative processes (out of which the complications of the machinery chiefly arise) is the solution of simultaneous equations containing many variables. This would include a large part of the calculations involved in the practical application of the method of least squares. The solution of such equations can always be expressed as the quotient of two determinants, and the obtaining this quotient is a final operation, which may be left to the operator to perform by ordinary arithmetic, or which may be the subject of a separate piece of machinery, so that the more direct work of forming the determinant, which is a mere combination of the three direct operations of addition, subtraction, and multiplication, may be entirely freed from the tentative process of division, which may thus be prevented from complicating the direct machinery. In the absence of a special engine for the purpose, the solution of large sets of simultaneous equations is a most laborious task, and a very expensive process indeed, when it has to be paid for, in the cases in which the result is imperatively needed. An engine that would do this work at moderate cost would place a new and most valuable computing power at the disposal of analysts and physicists.

Other special modifications of the engine might also find a fair field for reproductive employment. We do not think it necessary to go into these questions at any great length, because they involve a departure, in the way of restriction and specialisation, from Mr. Babbage's idea, of which generality was the leading feature. Nevertheless, we think that we should be guilty of an omission if we were to fail to suggest them for consideration.

IX. *General Conclusions, and Recommendation.*—1. We are of opinion that the labours of Mr. Babbage, firstly on his Difference Engine, and secondly on his Analytical Engine, are a marvel of mechanical ingenuity and resource.

2. We entertain no doubt as to the utility of such an engine as was in his contemplation when he undertook the invention of his analytical engine, supposing it to be successfully constructed and maintained in efficiency.

3. We do not consider that the possibilities of its misuse are any serious drawback to its use or value.

4. Apart from the question of its saving labour in operations now possible, we think the existence of such an instrument would place within reach much which, if not actually impossible, has been too close to the limits of human skill and endurance to be practically available.

5. We have come to the conclusion that in the present state of the design of the engine it is not possible for us to form any reasonable estimate of its cost, or of its strength and durability.

6. We are also of opinion that, in the present state of the design, it is not more than a theoretical possibility; that is to say, we do not consider it a certainty that it could be constructed and put together so as to run smoothly and correctly, and to do the work expected of it.

7. We think that there remains much detail to be worked out, and possibly some further invention needed, before the design can be brought into a state in which it would be possible to judge whether it would really so work.

8. We think that a further cost would have to be incurred in order to bring the design to this stage, and that it is just possible that a mechanical failure might cause this expenditure to be lost.

9. While we are unable to frame any exact estimates, we have reason to think that the cost of the engine, after the drawings are completed, would be expressed in tens of thousands of pounds at least.

10. We think there is even less possibility of forming an opinion as to its strength and durability than as to its feasibility or cost.

11. Having regard to all these considerations, we have come, not without reluctance, to the conclusion that we cannot advise the British Association to take any steps, either by way of recommendation or otherwise, to procure the construction of Mr. Babbage's analytical engine and the printing tables by its means.

12. We think it, however, a question for further consideration whether some specialised modification of the engine might not be worth construction, to serve as a simple multiplying machine, and another modification of it arranged for the calculation of determinants, so as to serve for the solution of simultaneous equations. This, however, inasmuch as it involves a departure from the general idea of the inventor, we regard as lying outside the terms of reference, and therefore perhaps rather for the consideration of Mr. Babbage's representatives than ours.

We accordingly confine ourselves to the mere mention of it by way of suggestion.

Third Report of the Committee, consisting of Dr. Joule, Prof. Sir W. Thomson, Prof. Tait, Prof. Balfour Stewart, and Prof. Maxwell for the Determination of the Mechanical Equivalent of Heat.—Dr. Joule has published a paper giving *in extenso* the experiments summarised in the last two reports, in the *Philosophical Transactions* of the Royal Society, where was published his former paper in 1850. The new result which confirms the old one, gives 772·55 foot pounds as the equivalent at the sea-level, and the latitude of Greenwich of the heat which can raise a pound of water, weighed *in vacuo*, from 60° to 61° F. of the mercurial thermometer where the permanent freezing point is called 32°, and the permanent boiling point of water under a barometrical pressure of 30 inches of mercury raised to 60° F. is 212°. The work at present in hand is a more accurate investigation of the true position of the freezing and boiling points of the thermometer when cleared from the effects of the imperfect elasticity of the glass of which they are constructed. The correction to the above equivalent which may thus accrue, is not expected to be of considerable amount.

Report of the Committee appointed for the purpose of inquiring into the possibility of establishing a "Close Time" for Indigenous Animals.—The Committee being dissatisfied with certain points in the Report of the Scottish Fishery Commissioners, addressed a letter to the Home Secretary, calling attention to the following points:—

"I. That conclusions Nos. 2 and 3 of the Commissioners, viz., that 'Legislation in past periods has had no appreciable effect,' and that 'Nothing that man has yet done, and nothing that man is likely to do, has diminished, or is likely to diminish, the general stock of herrings in the sea,' if correct, are absolutely contradicted by conclusion No. 13, which recommends that 'The Sea-Birds Preservation Act, protecting gannets and other predaceous birds which cause a vast annual destruction of herrings, should be repealed in so far as it applies to Scotland.'

"II. That conclusion No. 1, stating that 'The herring-fishery on the coast of Scotland as a whole has increased and is increasing,' clearly shows that there can be no necessity for the step recommended in conclusion No. 13 as above cited.

"III. That conclusion No. 13 seems to have been arrived at from exaggerated or incorrect information, as will appear from the following considerations:—The number of gannets on Ailsa is estimated ('Report,' p. xi.) at 10,000, and a yearly consumption of 21,600,000 herrings is assigned to them; while the Commissioners assume that there are '50 gannets in the rest of Scotland for every one on Alisa,' and on that assumption declare that the total destruction of herrings by Scottish gannets is more than 1,110,000,000 per annum. This is evidently a miscalculation; for, on the premises, this last number should be 1,101,600,000, a difference of more than 8,000,000.

"But, more than this, supposing the figures at the outset are right, it appears to the Close-Time Committee that the succeeding assumption of the Commissioners must be altogether wrong; at any rate, there is no evidence adduced in its support, and some that is contradictory of it.

"The number of breeding-places of the gannet in the Scottish seas has long been known to be five only, as, indeed, is admitted by one of the Commissioners (Appendix No. 2, p. 171; and the evidence of Capt. M'Donald, which is quoted in a note to the same passage, while estimating the Ailsa gannets at 12,000 in 1869 (not 1859, as printed), puts the whole number of Scottish gannets at 324,000 instead of 510,000, which there would be at the rate of 50 in the rest of Scotland for one on Ailsa, according to the Commissioners' assumption.

"Moreover, 50,000 of these 324,000 birds, or nearly one-sixth, are admitted by this same Commissioner to be 'of great value to the inhabitants' of St. Kilda, and, indeed, they are of far greater value to them than any number of herrings, since it is perfectly well known that the people of St. Kilda could hardly live without their birds; therefore this 50,000 must be omitted from any estimate of detriment. Deducting, then, 50,000 from Capt. M'Donald's 324,000, we have 274,000, and these, at the Commissioner's estimate, would consume 600,060,000 herrings instead of the 1,110,000,000 alleged by the report, and, therefore, nearly 200,000,000 fewer than the Commissioners' estimate of the annual take of the Scottish fisheries (800,000,000)—25 per cent. less instead of 37 per cent. more.

"Hitherto the supposition of the report, that the gannets frequent the Scottish seas all the year round, has been followed; but the Close-Time Committee begs leave to observe that, as a matter of fact, these birds are not there in force for more than half the year.

"This, then, will require another abatement to be made. Not to exaggerate the case, the Committee assumes them to frequent these waters seven months, or seven-twelfths of a year. This will make their annual capture of herrings 350,350,000, instead of the more than 1,110,000,000 of the Commissioners, being nearly 700,000,000, or much less than one-third, fewer.

"IV. That in all the evidence received and published by the Commissioners only two witnesses allege that any harm has resulted to the fisheries from the Sea-Birds Protection Act. Of these, the first, Robert M'Connell, presented a petition from the fishermen of Girvan, in which it is stated (p. 145) that 'no legislation is called for or required;' while another witness from the same place, John Melville (a fishery officer), declares (at p. 146) that 'The fishery has very much increased this last year. Recent years have also shown a gradual increase. The increase is partly due to the increased machinery and partly to the increase in the number of herrings.'

"The second witness unfavourable to the Act, John M'William (an Inspector of Poor), speaks (pp. 147-49) only from personal knowledge acquired between 1833 and 1853, when he ceased to be a fisherman, and not from any recent experience. He can therefore scarcely be held competent to give an opinion of his own as to whether the Sea Birds Protection Act (passed in 1869) has injured the fisheries. Another witness recommends the repeal of this Act; but he, Hugh MacLachlan, expressly states (p. 143) that he 'thinks the cause of the decrease [in the numbers of herrings taken] is the catching immature fish;' and the remedy he proposes is the adoption of a strict Close Time.

"V. That, on the other hand, the utility of sea-birds in pointing out the situation of shoals of herrings and other fish is not only generally notorious, but is even admitted in the Report (pp. 57 and 175).

"VI. That if the Sea-Birds Act be repealed on the grounds alleged for Scotland, its repeal for England and Ireland must logically follow; and this Committee trusts that no steps may be taken to repeal the Act for Scotland."

In view of any proceedings which may be taken in the Session of 1879 in regard to the recommendations of the Scottish Herring-Fishery Commissioners, as well as on general grounds, the Committee urges its reappointment.

SECTION A.—MATHEMATICAL AND PHYSICAL.

Note on the Pedetic Action of Soap, by Prof. W. Stanley Jevons.—Since the publication in the *Quarterly Journal of Science* for April, 1878, of my paper on Pedesis, or the so-called Brownian movement of microscopic particles, it has been suggested to me that soap would form a good critical substance for experiment in relation to this phenomenon. It is the opinion of Prof. Barrett, and some other physicists, that the movement is due to surface tension, whereas, I believe that chemical and electromotive actions can alone explain the long-continued and extraordinary motions exhibited by minute particles of almost all substances under proper conditions. Soap considerably reduces the tension of water in which it is dissolved, without much affecting (as is said) its electric conductibility. If, then, pedesis be due to surface-tension, we should expect the motion to be killed, or much lessened when soap is added to water.

Having tried the experiment, I find that the result is of the opposite character to what Prof. Barrett anticipated. With a solution of common soap the pedetic motion becomes considerably more marked than before. I have observed this result not only with china clay and some other silicates, but also with such comparatively inert substances, as the red oxide of iron, chalk, and even the heavy powder of barium carbonate. The last-named substance, one of those which we should least expect to dance about of its own accord, gave a beautiful exhibition of the movement when mixed with a solution of about 1 per cent. of soap, and viewed with a magnifying power of 500 or 1,000 diameters.

The correctness of this result was also tested by observing the suspending power of solutions of soap-solution compared with water. If a little china clay be diffused through common impure water, that, for instance, of the London Water Companies, the greater part of the clay will soon be seen to collect together in small flocks and fall to the bottom in two or three hours, the water being almost clear. However, if about 1 per cent. be dissolved in the water, the behaviour of the clay is quite different. The larger particles soon subside, but the smaller ones remain diffused through the liquid for a long time, giving it a milky appearance, quite different from the flocky and grainy appearance of the common water; if 1 per cent. of sodium carbonate be dissolved in common water, and china clay be mixed therewith, the subsidence of the clay is still more rapid, owing, as I have explained, to the increase in the electric conductivity of the fluid, and the consequent decrease of pedesis. But I now find that if soap be added at the same time, pedesis is not destroyed but considerably increased, and the clay remains a long time in suspension, two or three days at least.

These facts give a complete explanation of the detergent power of soap. It has long seemed to me unaccountable that for cleansing purposes the comparatively neutral soap should be better than the alkaline carbonate by itself; we are told that the alkali is but feebly combined with the stearic or other fatty acids. But why combine it at all if we need only the alkaline power of the base? The fact is that the detergent action of soap is due to pedesis, by which minute particles are loosened and diffused through the water so as to be readily carried off. Pure rain or distilled water has a high cleansing power, because it produces pedesis in a high degree. The hardness of impure water arises from the vast decrease of pedesis due to the salts in solution. Hence the inferior cleansing power of such water. If alkaline salts be added, dissolved in water, it becomes capable of acting upon oleaginous matter, but the pedetic power is lessened, not increased. But if soap be added also, we have the advantage both of the alkali dissolving power, and of the pedetic cleansing power. At the same time we have a clear explanation why silicate of soda is now largely used in making soap; for I have shown, in the paper referred to, that silicated soda is one of the few universal substances which increase the pedetic and suspensive power of water.

I believe that the detergent power of soap and water is one of the many important phenomena which may be explained by the study of pedesis, and I propose to follow up the investigation of this movement in regard to the several substances which tend to increase it.

Motions produced by Dilute Acids on some Amalgam Surfaces, by Robert Sabine.—The author finds, when a drop of very dilute acid is placed upon the clean and newly filtered surface of a rather rich amalgam of some metal which is positive to mercury, that the drop does not lie still as it would do upon pure mercury, but sets itself into an irregular jerky motion. This is the case with copper, zinc, antimony, tin, and lead amalgams. But if instead of these amalgams those of platinum, gold, and silver are used—these latter metals being negative to mercury—the drop of acid water lies quite still. The acids tried were sulphuric, hydrochloric, oxalic, and acetic, which behaved similarly but in different degrees. When the experiment is made in an atmosphere of oxygen the movements upon the amalgams of the positive metals are increased; but in hydrogen, carbonic acid, nitrogen, and coal-gas the motions are instantly arrested.

The author concludes that the motions result from an alternate play of deoxidation of the mercury underneath the acid by electrolysis, due to the currents of small floating particles of the positive metal causing the drop to contract, and of oxidation of the surface outside the acid-drop causing it to re-expand.

On Certain Phenomena accompanying Rainbows, by Prof. Silvanus P. Thompson.—The author narrated several instances of rainbows seen chiefly in Switzerland, where radial streaks of light devoid of colour were observed within the primary and without the secondary bow. The explanation suggested was as follows:—The wedge-shaped radial streaks are beams of sunlight, which become visible by diffuse reflection from particles of matter in their path, just as the apparently divergent beams of sunrise or sunset become visible. These "beams" being practically parallel to one another, appear to converge in the point exactly opposite to the sun by perspective, or, in fact, just as the parallel beams of sunset appear divergent. Since the rainbow has for its centre the point opposite the sun, such beams must have positions radial with respect to the bow. They resemble, therefore, the *rayons du crépuscule* occasionally seen in the east at sunset; they had never been observed crossing the dark span between the primary and secondary bows. A similar phenomenon of rays might sometimes be seen in sunlight, when the shadow of the observer fell upon a slightly turbid lake or river.

SECTION B.

CHEMICAL SCIENCE.

Opening Address by the President, Prof. Maxwell Simpson, M.D., F.R.S.

My position here is a highly honourable, but by no means a comfortable one. Naturally you expect to hear from me something new about the science which occupies the attention of this section, and I have the miserable feeling that I must disappoint you. How can I possibly find a fact in chemistry with which you are not already acquainted? If, in order to cater for you, I go to France, Germany, Russia, or America, I find the abstractors of the Chemical Society have been there before me, and have swept everything of value into their journal. Chemists are now kept perfectly acquainted with the progress of science in every part of the world, and therefore the *raison d'être* of this address, so far as announcing the discoveries of the year is concerned, has passed away. I therefore propose instead of giving you a concentrated essence of the last twelve numbers of the *Journal* of the Chemical Society, to bring before you the claims of this science to a place in general education, and the claims of original research to a place in the curriculum for higher degrees in our universities.

I have been devoted to chemistry all my life. It has been my business and my pleasure. The longer I live the more deeply am I impressed with the advantages to be derived from its study, and I am anxious that these advantages should be shared by the rising generation.

Whether we take into account the value of the knowledge acquired, the discipline of the intellectual faculties in acquiring that knowledge, or the effect on the character, surely we have a right to give the study of this science a prominent place in our schools and colleges. It would be difficult to over-estimate the value and extent of the knowledge we derive from chemistry. Without it we can know nothing about the air we breathe, the water we drink, or the food we eat; we cannot understand the processes of combustion, respiration, fermentation, putrefaction, or the endless chemical changes which are continually in operation around us, and which affect our lives for good or for evil. In a word, the whole of the phenomena of nature must for ever remain to us, more or less, an inscrutable mystery.

Again, is it not desirable that we should have some acquaintance with the chemical arts, from which we derive so many of our comforts and luxuries? Should we not know something of the arts of photography, dyeing, metallurgy—something of the manufacture of glass and china, and of the thousand beautiful things that are constantly in our hands? Not only is the knowledge we obtain from chemistry very considerable in itself, but it furnishes us with a key which enables us to unlock vast stores of knowledge contained in several other sciences—these are, physics, geology, mineralogy, physiology, and I may now add, astronomy. Physics and chemistry are so intimately connected that it is difficult to say where the one begins and the other ends. The help that chemistry gives to physics is shown by the numbers of chemists who have distinguished themselves as physicists. I may mention a few belonging to our own time—Andrews, Bunsen, Faraday, Frankland, Graham, Guthrie, and Regnault.

With regard to mental discipline, the mind of the student is exercised in both the inductive and deductive methods of reasoning. His original faculties are stimulated by the consciousness that he can in many cases readily test the worth of his ideas by experiment. With inexpensive apparatus and a good balance, the intelligent student can make out for himself some of the laws and many of the facts of the science, and it may be, also add to them. He glides insensibly from the known to the unknown. Indeed his spirit of inquiry demands, in most cases, to be curbed rather than spurred. Some students are constantly finding out new methods of analysis or discovering the precious metals in impossible places.

The readiness with which we can cross over into the *terra incognita* of chemistry and make little explorations there, constitutes, in my opinion, the great charm of this science, and, to a great extent, its value as an educational agent. What I wish to insist upon is that the student of chemistry can reach the field of original work sooner than the student of most other sciences. Once he commences original research the development of his intellectual faculties rapidly progresses. His imagination is daily exercised in propounding new theories and devising experiments in order to ascertain their truth or falsehood. And what more valuable intellectual training can there be than the habit of subjecting our ideas to the test of inexorable experiment? In the world outside chemistry we are, alas! too ready to take things for granted. The chemist's motto is *prove all things*. The ancients adopted a different method: they assumed certain principles and reasoned from them. They therefore did little in science.

Chemistry promotes in a remarkable manner accuracy, thoroughness, and circumspection. An organic analysis requires six weighings: if any one of these is inaccurate, the results are worthless. A qualitative test carelessly applied may cause us, in a research, to waste months in the pursuit of a phantom or Will-o'-the-Wisp which can have no corporeal existence. If we have to employ absolute alcohol in our experiments, we must not be satisfied with going through the ceremony of making it absolute, but we must assure ourselves that it *is* absolute. Unless we are sure of every step in our research, our results become doubtful, and therefore of no value.

On the circumspection, also, of the original worker large demands are made. The avenues by which error may creep in and vitiate his results are very numerous. These he must foresee, and endeavour to close up. Laboratory work teaches us to use our senses aright, sharpens our powers of observation, and prevents us from reasoning rashly from appearances. It also promotes manual dexterity, and trains the hands to work in subordination to the head.

Perhaps in no other science is the student so deeply impressed with the order and economy of nature, the immutability of her laws, and the exactness of her operations. These impressions will, no doubt, in after life impart seriousness to his character, and save him from the adoption of many a wild theory.

I come now to the effect of original work on the character. Many virtues are necessary to the chemist—courage, resolution, truthfulness, and patience. He is often obliged to perform experiments which are attended with great danger, and no man can hope to fight long with the elements without carrying away many a scar. Sometimes fatal accidents occur. Many years ago Mr. Hennel, of the Apothecaries' Hall, London, lost his life by the explosion of a fulminating powder which he was preparing for the East India Company. And many of us recollect the sad death of young Mr. Chapman, a distinguished chemist whom I had the pleasure of knowing, who was literally blown to atoms while working in the Hartz Mountains on a new dynamite which he had himself discovered. I must tell the ladies, however, that accidents are not always so disastrous, but that often one may escape with merely the loss of an eye. But the chemist must not be discouraged by fear of accident, neither must he be disheartened by the temporary failure of his experiments, nor at the slowness of his processes. Bunsen was obliged to evaporate forty-four tons of the waters of the Dürcheim springs in order to obtain 200 grains of his new metal, cæsium. It took Berthelot several months to form, by a series of synthetical operations, an appreciable quantity of alcohol from water and carbon, derived from carbonate of baryta. Many years ago, in the laboratory of Wurtz—my honoured master—a poor student, whom I knew, was carrying from one room to another a glass globe which contained the product of a month's continuous labour, when the bottom of the globe fell out, and the contents were lost. Nothing daunted, he recommenced his month's work, and brought his research to a successful issue.

Above all things, the chemist must be *true*. He must not allow his wishes to bias his judgment or prevent him from seeing his researches in their true light. He must not be satisfied that his results appear true, but he must believe them to be true; and having faithfully performed his experiments, he must record them faithfully. He may often be obliged to chronicle his own failures and describe operations that tell against his own theories, but this hard test of his truthfulness he must not shrink from.

But I must not weary you with the virtues of the chemist. If I have succeeded in showing that the pursuit of this science tends largely to develop the intellect and discipline the character, I think I have done something for chemistry. We are told by Bishop Butler that "habits of virtue acquired by discipline are improvement in virtue, and improvement in virtue must be advancement in happiness."

I am glad to see that the importance of original research as a part of higher education is at last beginning to be recognised in this country. The Royal University Commission at Oxford has recently recommended that candidates for the higher degrees in science shall in that university be required in future to work out an original investigation. In Germany, where education has been so long and so well understood, original work has been, for at least the last half century, a *sine quâ non* for a degree. Another admirable rule exists in that country, the adoption of which in Great Britain might go far to wash out the stain from our islands, of not having contributed our fair quota to the advancement of human knowledge. It is this—the Germans make a point of securing invariably that their scientific chairs shall be filled by men who have already distinguished themselves by their discoveries. The professor, on his appointment, naturally desires to continue his investigations, and endeavours to secure, and usually succeeds in securing, the assistance of his pupils. This is a mutual advantage. The professor is able to do more work for science, and the student, on his part, learns to conduct for himself an original investigation. Hence there is always a rising generation of original workers in Germany, who turn out papers more or less meritorious with the rapidity of a Walter's press. They are stimulated by the hope of one day arriving themselves at a professor's chair, the path to which they are well assured is only through the toilsome field of original work. But I must not wrong the German student by the implication of a purely selfish motive in his work. His labour is one of love, and his ambition, for the time at least, is bounded by the desire to *do something* for science. And from a multitude of such enthusiasts the great professors come. Great mountains are only found in mountainous countries.

I find myself insensibly led to speak of the encouragement of research in this country, and, although it has been very largely discussed in scientific circles, I will venture to add a few words. To promote original work here, I believe it is indispensable that our professors should be well paid. It would save them from the necessity of supplementing their incomes by commercial analyses, and thus enable them to devote their spare time to original work. And to secure that they shall have spare time, I would like to see in every laboratory a competent assistant, who would be able occasionally to take up the professor's lectures, should he be engaged in important work. There are many around me who know how very exacting original investigation is, and how necessary it is, at times, to be able to work on without interruption, bits and scraps of time being of no value. I am glad to see that the Oxford Commission also recommends the appointment of well-paid assistants. Well-paid professorships and well-paid assistantships would be attractive prizes for our students to work up to; and if it were clearly understood that the only way to these prizes was through original investigation, we should very soon have an army of zealous and competent workers.

The plan of appointing a staff of original workers unconnected with teaching has been proposed; but I do not approve of it. The original worker is, as a rule, the best teacher, and the rising generation of students should not be deprived of the advantage of his instruction. Moreover, as I said before, the professor may be greatly assisted by his pupils.

No doubt the Government Grant Fund does a good deal for science, but the field of its operations is, under present conditions, limited. Professors, as a rule, are so occupied with teaching that they cannot avail themselves of the fund; and of those students who might be competent and willing, very few can afford to do so. Instead of trusting to the precarious and insufficient support of the fund they must endeavour to settle themselves permanently in life.

It is much to be regretted that the universities of Oxford and Cambridge, with such splendid revenues at their disposal, should contribute so little to the advancement of physical science. I hope the day is not far distant when the Fellowships—or at least a few of them—which now go to reward young men for merely passing a good examination, shall be given *without examination* to men who shall have advanced human knowledge in any department. At present, a Fellowship of 250*l*. or 300*l*. a-year, lasting ten or twelve years, and in some cases for life, may be obtained on showing proof of a good memory—or, at most, a capacity for assimilating other men's ideas. To make discoveries—to follow out a new train of thought, and establish it by experiments specially devised to that end, has been left not only without reward, but almost without recognition, in our two principal seats of learning. Is it to be so always? The world at large, ignorant as it is, has a sounder instinct on this subject, and the man who makes the humblest addition to the stock of knowledge in the world rarely fails to receive the world's respect and honour.

The suggestions I have ventured to make could not, of course, be well carried out, unless the government take into its own hands the appointment to all scientific chairs. Of this I think I see indications. I believe that sooner or later the government will assume the supreme direction of education in this country. It has already taken primary education under its control, and quite recently, here in Ireland, intermediate education to a great extent. And does the appointment of so many university commissions not show a disposition on the part of the government to assume the direction of higher education also?

SECTION C.—GEOLOGY.

The Origin and the Succession of the Crystalline Rocks, by Prof. J. Sterry Hunt, LL.D., F.R.S.—As a preliminary to a statement of the results of many years of study of the crystalline rocks in North America, the author proceeded to consider the question of their origin, which is still a subject of debate between plutonists and neptunists. The crystalline silicate rocks naturally divide themselves into three groups, namely, those indigenous stratified formations which have been called primitive or primary, those masses to which, from their relations to contiguous rocks, geologists assign an exotic origin, and in accordance with a generally-accepted theory, have agreed to call igneous or plutonic; and a third and distinct group of rock-masses which, though like the last, clearly posterior to those encasing them, are now, by most geologists, admitted to be of aqueous origin. This third group includes metalliferous lodes and various other crystalline veinstones, and is conveniently designated endogenous. It is not always easy to distinguish between the rocks of these three groups; there are not wanting those who have assigned an igneous origin to metalliferous lodes, and many still confound endogenous granitic veins with the mineralogically similar plutonic granites. In like manner the distinction between the latter and the stratified granitoid gneisses is frequently not very apparent. That the movement of flow in extravasated plutonic rocks may give to their constituent minerals a stratiform arrangement, is a fact of which both exotic granites and doleritic dykes and masses afford illustrations. Moreover, the arrangement due to successive depositions upon the walls of a fissure may give to an endogenous mass a structure which simulates that of a sedimentary rock, and imparting to granitic veinstones a resemblance to gneiss; while a laminated structure sometimes results from the arrangement of the crystals developed in a cooling mass. Hence there are not wanting those who include under the head of plutonic rocks not only the clearly marked exotic granites, dolerites, and diorites, but the granitoid gneisses, the massive bedded greenstones, and likewise the more schistose rocks with which these gneisses and greenstones are often so intimately associated that it is difficult to separate them. According to those who hold this plutonic view, the crystalline rocks represent the igneous crust of the globe, and their frequent stratiform structure is due to agencies in great part anterior to the production of sedimentary rocks. In opposition to this view is that of the neptunist, who, starting from the fact that the elements of an aqueous sediment may, through the action of chemical and crystallogenic forces, pass into new combinations and acquire a new structure, argues not only that all indigenous crystalline rocks have had an aqueous origin, but that the exotic masses themselves represent the last stages of this process of alteration or metamorphosis of sedimentary beds.

Further inquiry into the chemical and lithological composition of the crystalline rocks, however, brings to light difficulties in the way of both of these hypotheses. To begin with the plutonist view, volcanic rocks, both ancient and modern, are more or less nearly related in composition to the gneisses and the stratified greenstones, but we seek in vain among undoubted volcanic or igneous rocks for the chemical representatives of the masses of serpentine, olivine, steatite, chlorite, quartzite, magnetite, oligist, and limestone, which appear in the primary formations, and have, all of them, by geologists of the school in question, been regarded as of igneous or plutonic origin. To account for the presence of such rocks among the more or less feldspathic aggregates—chiefly gneisses and greenstones—which make up the greater portion of the crystalline formations, three hypotheses have been imagined by plutonists. According to the first of these the earth's interior is a reservoir from which, at times, have been ejected not only basic and acidic feldspathic rocks, but molten masses of olivine, iron-oxyde, quartz, and limestone. Other geologists of this school have sought to account for the presence of some of these exceptional rocks by a process of so-called segregation, which would assimilate them to endogenous masses. The chemical and geognostical difficulties in the way of both of these hypotheses have, however, led to their general rejection for the third, which supposes these rocks to have been formed by a subsequent local alteration of portions of the ordinary plutonic rocks.

From acknowledged cases of alteration or replacement in mineral species which result in pseudomorphs, and from the more frequent cases of envelopment and of isomorphism, which have been taken for examples of pseudomorphism, it was argued that many species are capable of being changed into others by the loss or addition of certain elements, so that the resulting body often contains no portion of its original constituents. Extending this view from single crystals to rock-masses, it was maintained that different portions of an igneous or plutonic formation, whether basic or acidic, might be transformed into serpentine, chlorite, or limestone. These changes were supposed to depend on the action of water, which, aided by heat, was regarded as the efficient agent in the local alterations of plutonic rocks. At the same time the adjacent sedimentary strata were supposed to share in these changes, thus giving rise to what have been called contact-formations. In their latest form these doctrines have been well set forth by von Lasaulx and by Knop. This third hypothesis, then, proposes to account for the presence of various exceptional varieties of rock among ordinary plutonic formations by supposing that limited portions of these have, at different times, been the subject of very unlike chemical processes, resulting in their complete change into new forms of rock by what has been called pseudomorphic alteration, or metamorphosis. As, however, such a conversion involves a change not only of form but of substance, it has been more properly designated a metasomatosis.

We have next to consider the neptunean view as ordinarily expounded. This, while it accounts by sedimentation for the stratiform arrangement of the crystalline rocks and explains the existence therein of beds of iron ores and limestones, still presents many of the difficulties which are encountered in the plutonist view. If, as most neptunists maintain, the great crystalline series have been derived from the alteration of uncrystalline ones, which were not only similar to those of palæozoic and more recent times, but are, in fact, portions of these which in adjacent regions are still known to us in their original unchanged condition, how are we to explain the genesis of the feldspathic and hornblendic rocks which predominate in these crystalline formations? The sandstones and shales from which, in this view, they are supposed to be formed, could never, by themselves, give rise to the rocks in question, since they are deficient in the alkalis, and to a greater or less extent in the other bases required for the production of the constituent silicates. To explain their origin, therefore, it becomes necessary to admit the introduction of these various bases from without, and to suppose a series of metasomatic processes more wonderful than those imagined by the plutonist. The latter, by his hypothesis, has already at hand feldspathic and hornblendic rocks which are to be the subjects of metasomatosis, while the neptunist has only the products of their decay.

In either hypothesis, we have to account for the presence, in the primary formations, of beds and interstratified masses of a great number of exceptional silicated rocks very distinct in composition from any mechanically-formed sediments, including not only silicates like serpentine, olivine, steatite, chlorite, pinite, garnet, epidote, and hornblende, but of pure orthoclase, as well as of triclinic feldspars. Each of these species would require, for its production from any ordinary igneous or aqueous rock, a separate and independent metasomatic process—involving the addition of certain elements and the abstraction of others—until the whole heterogeneous crystalline series was complete. The author illustrated these views by examples from recent writers, and concluded that the hypothesis of metasomatosis, as maintained both by plutonists and neptunists, supposes the operation in solid rocks of processes of circulation, absorption, elimination, selection, and aggregation scarcely to be equalled in the economy of highly-organised beings, and not easily imagined in the masses of the mineral kingdom.

Certain geologists suppose the existence of two classes of crystalline stratified rocks: the one neptunean, and consisting of altered portions of palæozoic or more recent sediments, and the other—more ancient—which may be either neptunean or plutonic in origin. The history of geology gives many examples of crystalline formations which have been, in turn, assigned to various geological horizons from the cainozoic to the base of the palæozoic, but have since been found to belong to a pre-palæozoic period. In the opinion of the author we have no good and sufficient reason for believing in the present existence of any uncrystalline representative of these crystalline formations, or of any such formation which is not pre-Silurian, if not pre-Cambrian, in age. There are, however, many examples of local alterations of later sediments by hydro-thermal action, which has developed in these many crystalline minerals identical with those found in the more ancient rocks. The advocates of the neptunean hypothesis have, for the most part, sought for the origin of the crystalline rocks in sediments of a later date, of which the uncrystalline representatives are still to be found. There are, however, reasons for believing that in eozoic, or pre-Cambrian times, there prevailed chemical activities, dependent upon greater subterranean temperature, different atmospheric conditions, and abundance of thermal waters, and that under these circumstances were deposited the materials for the crystalline rocks. There have not been wanting those who have sought in similar hypothetical conditions for the origin of these rocks. De la Beche, in 1834, imagined them to be chemical deposits, due to the action of the heated ocean upon the earth's primeval crust before the dawn of life.

The author's researches into the composition and structure of the crystalline rocks, conjoined with his studies of the chemistry of natural waters, led him, in 1860, to reject the hitherto received view of the epigenic or metasomatic origin of serpentine, steatite, chlorite, and similar rocks, and to maintain their derivation from silicates formed by chemical processes and deposited in the water of lakes or seas. This view he soon after extended to the various other exceptional rocks found in crystalline formations, which it was in 1864 asserted, had been "formed by a crystalline and molecular re-arrangement of silicates generated by chemical processes in waters at the earth's surface." In elucidation of this view the author referred to the insoluble silicates now separated in the evaporation of many natural waters, to the formation from the earliest times to the present of deposits of serpentine, sepiolite, glauconite, and of aluminous silicates allied to chlorite, which are found either forming beds or filling the cavities of various marine organic forms from the foraminifers of to-day to the crinoids of palæozoic time, and the eozoon of the Laurentian. The formation in modern times of crystalline zeolites and quartz in thermal waters was also cited in illustration of this view of the generation of various mineral silicates by causes now in operation, which, it is believed, were far more active in eozoic times. This was not, as had been already suggested by others, a process confined to a seething primeval ocean before the advent of life, but was continued through long ages under varying chemical conditions, and was contemporaneous with the deposition of successive strata of limestone and detrital matters. The argillaceous portions of these, it is conceived, may have taken part in the reactions with thermal waters.

We have thus, in the opinion of the author, a reasonable mode of accounting for the origin of the various rocks of the crystalline formation, and a consistent and complete neptunean theory, which does not invoke the aid of metasomatosis. It has, since it was proposed eighteen years ago, met with the approval of many whose studies have made them the fittest judges of its reasonableness. Among those who have either formally given their adhesion to it, or have enunciated similar views, may be mentioned the names of Delesse, Renard, Gümbel, Credner, Alphonse Favre, and Gastaldi.

The chemical activities concerned in the production of the various silicates have doubtless suffered gradual change and diminution through the successive ages of eozoic time, from which have resulted mineralogical and lithological differences in the crystalline terranes. Each of these includes quartzites and limestones, in which latter certain silicates, such as serpentine, hornblende, and micas, are occasionally found. It is in those aluminiferous rocks, which are without lime or magnesia, that are seen the essential and characteristic differences dependent, as long ago pointed out by the author, upon a decrease in the proportion of alkalis. As we pass from the older to the younger of the eozoic terranes, the feldspar, orthoclase, and albite, become partially or wholly replaced by silicates like muscovite, damonite, and paragonite, and finally by andalusite, fibrolite, cyanite, and pyrophyllite.

The author alluded briefly to the changes by which the ancient aqueous deposits were transformed into crystalline stratified rocks by what Gümbel has designated diagenesis, as distinguished from their supposed origin by epigenesis or metasomatic change. The question of the relation of the indigenous crystalline rocks to the endogenous and exotic masses included in them was noticed, the author alluding to the hypothesis, which he has elsewhere maintained, that the source of all exotic or eruptive rocks is to be found in the displacement or extravasation of ancient deposits of neptunean origin.

Coming to the second division of his subject, the author asserted that the study of the crystalline rocks of North America shows the existence of several distinct groups or terraces.

The Laurentian, which is the most ancient, includes in its lower part a mass of unknown thickness of granitoid gneiss, often hornblendic (Ottawa gneiss), succeeded—perhaps unconformably—by what has been called the Grenville series, consisting of similar gneisses and hornblendic rocks with intercalated quartzites and iron ores. These two divisions make up together the lower Laurentian of Logan, of which the thickness in Canada may greatly exceed 20,000 feet.

The Norian, which is the upper Laurentian or Labradorian of Logan, rests unconformably upon the Laurentian, and is remarkable for a great development of rocks composed chiefly of labradorite or related plagioclase feldspars, which have been called labradorite-rock or norite. The interstratified gneisses, quartzites, and limestones of the Norian are not unlike those of the Laurentian. This series, which abounds in great beds of titanic iron-ore, has a great volume which may exceed the thickness of 10,000 feet assigned to it by Logan.

The Laurentian is in many parts unconformably overlaid by the Huronian series, which is characterised by a great development of greenstones, generally hornblendic, with epidote, chlorite, steatite, serpentine, and soft hydrous mica-schists, often called talcose, besides argillites, quartzites, and limestones, generally magnesian. It abounds in metalliferous deposits, including magnetic and specular iron-ores, chrome, and sulphurets of copper, iron, and nickel, and has had assigned to it in different regions a thickness of from ten to twenty thousand feet.

In many parts of North America there exists a great development of rocks characterised by the predominance of orthofelsite or petrosilex, often becoming a quartziferous porphyry. This, which is apparently the *hälleflinta* of Sweden, was regarded as eruptive until, in 1869, the author showed it to be a stratified series with some associated quartzites and schists, and then included it in the lower part of the Huronian. Hitchcock, who has since studied these rocks in New Hampshire, has called them lower Huronian. From their absence in many localities at the base of the typical Huronian, it is conjectured that they may belong to a more ancient and distinct series.

The Montalban or White Mountain series is characterised by micaceous gneisses, generally called granites, which pass into quartzose and feldspathic mica-schists, often abounding in garnet, staurolite, fibrolite, and cyanite. Great masses of dark green gneissoid hornblendic rock, very distinct from the Huronian greenstones, abound in the Montalban, which also includes beds of a peculiar olivine rock, besides quartzites and crystalline limestones. This series abounds in endogenous granitic veins, containing muscovite, beryl, tourmaline, apatite, and oxide of tin. It probably equals the Huronian in thickness, and is supposed to overlie it.

The Taconian series includes a great volume of characteristic mica-schists, often quartzose, but seldom distinctly feldspathic, and frequently consisting in large part of damonite, or of pyrophyllite. Some of these, like the schists of the Montalban, include garnet and chiastolite. They are associated with quartzites and with dolomites and limestones, all of which are also frequently micaceous. Associated with these are found serpentines and granular hornblendic rocks of a peculiar type, very unlike those of the preceding groups and much less crystalline. The quartzites are in large part detrital rocks. This series, which yields the statuary marbles of North America, has a thickness of about 5,000 feet and is the lower Taconic of Emmons. It is found reposing alike on the Laurentian, Huronian, and Montalban, and is overlaid, in apparent unconformity, by the upper Taconic, which is identical with the Quebec group of Logan. This, which consists of many thousand feet of sandstones and argillites, with some limestones, includes among its strata organic forms belonging to various divisions of the Cambrian up to the Arenig. The Taconian, although containing an undescribed linguloid shell and a so-called scolithus, is by the author considered provisionally as distinct from the Cambrian. It has yielded in Ontario, besides scolithus, the Eozoon Canadense, and may perhaps be regarded as the connecting link between the eozoic and palæozoic ages.

The upper Taconic, or so-called Quebec group in Eastern North America, is separated by a stratigraphical break from the succeeding portion of the Cambrian, the Bala group (Trenton, Utica, and Loraine), while, on the contrary, the supposed discordance in the regions just mentioned at the summit of the latter, corresponding to the division between Cambrian and Silurian in Wales, appears to have been based on a misconception. There is, however, an important palæontological break at this horizon, connected with a great deposit of barren detrital rocks, which marked the close of the Cambrian period, and the author records his opinion that the name of lower Silurian, as well as that of Siluro-Cambrian, which he, with others, has applied to the Bala or upper division of the Cambrian is to be rejected as being historically incorrect and as tending to perpetuate false views of the palæontological relations between these and the succeeding rocks.

The early advocates in North America of the metamorphism of palæozoic rocks taught, in the first place, the stratigraphical equivalence of the upper Taconic (or lower and middle Cambrian) with the upper Cambrian, and further maintained that these rocks had suffered various degrees and various kinds of metamorphism, as the result of which they had assumed, in different areas, the characters of the Taconian, the Montalban, and Huronian, and the Laurentian; the lithological differences between these several series being regarded as marks of the greater or less alteration which, it was supposed, these uncrystalline Cambrian sediments had undergone. Other geologists have imagined portions of these same crystalline formations in North America to be altered strata of Silurian, Devonian, and even of triassic age.

The great groups of eozoic rocks already described constitute, however, in the author's opinion, as many great stratified series which, before the Cambrian time, existed in their present crystalline condition, and had been successively subjected to the accidents of uplift, contortion, and denudation, so that the newer eozoic groups were, at the beginning of the paleozoic period, distributed irregularly over the floor of fundamental Laurentian gneiss. These various crystalline groups are found, with a singular persistence and uniformity of lithological character, from Alabama to Newfoundland, along the Atlantic belt, and thence westward through Canada to the great lakes, and beyond, in the vast regions of the Cordilleras to the Pacific slope.

The author had some years since pointed out the remarkable similarity between these various crystalline groups of North America and the crystalline rocks of the British Islands, and had lately been able, by new observations, to confirm his conclusions. Among the crystalline formations of Donegal he had indicated representatives of Laurentian, Montalban, and Huronian, and the latter he had recently observed largely developed in Argyleshire and Perthshire. To the Huronian also he refers the green schists of Anglesea and Carnarvonshire, in both of which regions the orthofelsite or hälleflinta series at the base of the Huronian (the so-called porphyries), and likewise the more ancient gneisses, are well represented. He would, however, leave this subject to his friend Dr. Henry Hicks, who has so happily mastered the obscure problems of the pre-Cambrian geology of Wales. The studies of Gastaldi and others enable us to assert that similar series of ancient rocks occur in the same order in the Alps; and we infer that the chemical and physical conditions which presided over the production of the crystalline stratified rocks were world-wide.

SECTION D.

BIOLOGY.

Department of Anatomy and Physiology.

ADDRESS BY R. MCDONNELL, M.D., F.R.S., VICE-PRESIDENT OF THE SECTION.

SINCE this Association met twelve months ago the science of physiology has suffered an irreparable loss. In February last Claude Bernard died in the sixty-fifth year of his age. He was interred with a degree of pomp never in this country, and rarely even in France, accorded to men of science. His country showed how justly and how highly they estimated the merit of a man who, gentle, unobtrusive, modest, by the greatness of his genius and the brilliancy of his many discoveries shed a lustre on the land which gave him birth. It was my privilege to have been at one time a pupil of this illustrious physiologist. It will be my pride if I can show to a thoughtful and cultivated audience, such as I have now the honour to address, that the discoveries of my honoured master, although of necessity made by experiment on animals, have added much to that stock of knowledge which has conferred the greatest benefits upon mankind. In an address like this—limited to a short time—it would not be possible to give a detailed account of the work accomplished by Bernard. To do so would be to give a history of the progress of physiology for the last thirty-five years. His researches were so extended, and some of his discoveries so vast, that by comparison they seem to make others appear small, as the gigantic Californian pine seems to dwarf a goodly-sized oak which grows alongside it. Hence, we speak of Bernard's less important researches—of his minor discoveries, although of sufficient magnitude to have seemed great if made by another. Of these I cannot speak at length, yet some of my hearers will know that the services which Bernard has rendered to science by his researches on the pneumogastric nerves, the fifth pair, the chorda tympani, the facial, &c., are not small. Assuredly, the same may be said for his observations on "recurrent sensibility;" on the blood pressure and the gases of the blood; on the variations of colour of this fluid, according to the active or passive condition of the functions of the organ traversed by it; on the variations of temperature during these conditions of functional activity or inactivity; on the elective elimination by the glands of substances introduced into the economy, or of those which, as morbid products, accumulate in the system as the result of certain morbid states; on the special character and action of the varieties of the salivary secretions; upon the influence of the nervous centres on the secretion of saliva; on the electric phenomena manifested in nerve and muscle; on albuminuria connected with lesions of the nervous system; and (notably in its important practical bearings on uræmia) on the modifications of the secretions of the stomach and intestines after arrest of the elimination of urea through the natural channels. Claude Bernard, in truth, left his mark deeply on every aspect of physiology on which he touched. His discoveries, however, as regards the functions of the pancreas, of the liver, and concerning the vaso-motor system of nerves, are those on which his fame will ever chiefly rest.

Dr. McDonnell then dwelt in detail on the importance of Bernard's researches from a medical point of view.

Department of Anthropology.

The business of this department was opened by the following address from Prof. Huxley.

When I undertook, with the greatest possible pleasure, to act as a lieutenant of my friend the president of this Section, I steadfastly purposed to confine myself to the modest and useful duties of that position. For reasons, with which it is not worth while to trouble you, I did not propose to follow the custom which has grown up in the Association of delivering an address upon the occasion of taking the chair of a section or department. In clear memory of the admirable addresses which you have had the privilege of hearing from Prof. Flower, and just now from Dr. M'Donnell, I cannot doubt that that practice is a very good one; but I would venture to say, to use a term of philosophy, that it looks very much better from an objective than from a subjective point of view. But I found that my resolution, like a great many good resolutions that I have made in the course of my life, came to very little, and that it was thought desirable that I should address you in some way. But I must beg of you to understand that this is no formal address. I have simply announced it as a few introductory remarks, and I must ask you to forgive whatever of crudity and imperfection there may be in the mode of expression of what I have to say, although naturally I shall do my best to take care that there is neither crudity nor inaccuracy in the substance of it. It has occurred to me that I might address myself to a point in connection with the business of this department which forces itself more or less upon the attention of everybody, and which, unless the bellicose instincts of human nature are less marked on this side of St. George's Channel than on the other, may possibly have something to do with the large audiences we are always accustomed to see in the anthropological department. In the Geological Section I have no doubt it will be pointed out to you, or, at any rate, such knowledge may crop up incidentally, that there are on the earth's surface what are called *loci* of disturbance, where, for long ages, cataclysms and outbursts of lava and the like take place. Then everything subsides into quietude; but a similar disturbance is set up elsewhere. In Antrim, at the middle of the tertiary epoch, there was such a great centre of physical disturbance. We all know that at the present time the earth's crust, at any rate, is quiet in Antrim, while the great centres of local disturbance are in Sicily, in Southern Italy, in the Andes, and elsewhere. My experience of the British Association does not extend quite over a geological epoch, but it does go back rather longer than I care to think about; and when I first knew the British Association, the *locus* of disturbance in it was the Geological Section. All sorts of terrible things about the antiquity of the earth, and I know not what else, were being said there, which gave rise to terrible apprehensions. The whole world, it was thought, was coming to an end, just as I have no doubt that, if there were any human inhabitants of Antrim in the middle of the tertiary epoch, when those great lava streams burst out, they would not have had the smallest question that the whole universe was going to pieces. Well, the universe has not gone to pieces. Antrim is, geologically speaking, a very quiet place now, as well cultivated a place as one need see, and yielding abundance of excellent produce; and so, if we turn to the Geological Section, nothing can be milder than the proceedings of that admirable body. All the difficulties that they seemed to have encountered at first have died away, and statements that were the horrible paradoxes of that generation are now the commonplaces of schoolboys. At present the *locus* of disturbance is to be found in the Biological Section, and more particularly in the anthropological department of that Section. History repeats itself, and precisely the same terrible apprehensions which were expressed by the aborigines of the Geological Section, in long far back time, is at present expressed by those who attend our deliberations. The world is coming to an end, the basis of morality is being shaken, and I don't know what is not to happen if certain conclusions which appear probable are to be verified. Well, now, whoever may be here thirty years hence—I certainly cannot—but, depend upon it, whoever may be speaking at the meeting of this department of the British Association thirty years hence will find, exactly as the members of the Geological Section have found, on looking back thirty years, that the very paradoxes and conclusions, and other horrible things that are now thought to be going to shake the foundations of the world will by that time have become parts of every-day knowledge and will be taught in our schools as accepted truth, and nobody will be one whit the worse.

The considerations which I think it desirable to put before you in order to show the foundations of the conclusions at which I have very confidently arrived, are of two kinds. The first is a reason based entirely upon philosophical considerations, namely, this—that the region of pure physical science, and the region of those questions which specially interest ordinary humanity, are apart, and that the conclusions reached in the one have no direct effect in the other. If you acquaint yourself with the history of philosophy, and with the endless variations of human opinion therein recorded, you will find that there is not a single one of those speculative difficulties which at the present time torment many minds as being the direct product of scientific thought, which is not as old as the times of Greek philosophy, and which did not then exist as strongly and as clearly as they do now, though they arose out of arguments based upon merely philosophical ideas. Whoever admits these two things—as everybody who looks about him must do—whoever takes into account the existence of evil in this world and the law of causation—has before him all the difficulties that can be raised by any form of scientific speculation. And these two difficulties have been occupying the minds of men ever since man began to think. The other consideration I have to put before you is that, whatever may be the results at which physical science as applied to man shall arrive, those results are inevitable—I mean that they arise out of the necessary progress of scientific thought as applied to man. You all, I hope, had the opportunity of hearing the excellent address which was given by our president yesterday, in which he traced out the marvellous progress of our knowledge of the higher animals which has been effected since the time of Linnæus. It is no exaggeration to say that at this present time the merest tyro knows a thousand times as much on the subject as is contained in the work of Linnæus, which was then the standard authority. Now how has that been brought about? If you consider what zoology, or the study of animals, signifies, you will see that it means an endeavour to ascertain all that can be studied, all the answers that can be given respecting any animal under four possible points of view. The first of these embraces considerations of structure. An animal has a certain structure, a certain mode of development, which means a series of stages in that structure. In the second place, every animal exhibits a great number of active powers, the knowledge of which constitutes its physiology; and under those active powers we have, as physiologists, not only to include such matters as have been referred to by Dr. M'Donnell in his observations, but to take into account other kinds of activity. I see it announced that the Zoological Section of to-day is to have a highly interesting paper by Sir John Lubbock on the habits of ants. Ants have a polity, and exhibit a certain amount of intelligence, and all these matters are proper subjects for the study of the zoologist as far as he deals with the ant. There is yet a third point of view in which you may regard every animal. It has a distribution. Not only is it to be found somewhere on the earth's surface, but palæontology tells us, if we go back in time, that the great majority of animals have had a past history—that they occurred in epochs of the world's history far removed from the present. And when we have acquired all that knowledge which we may enumerate under the heads of anatomy, physiology, and distribution, there remains still the problem of problems to the zoologist, which is the study of the causes of those phenomena, in order that we may know how those things came about. All these different forms of knowledge and inquiry are legitimate subjects for science, there being no subject which is an illegitimate subject for scientific inquiry, except such as involves a contradiction in terms, or is itself absurd. Indeed, I don't know that I ought to go quite so far as this at present, for, undoubtedly, there are many benighted persons who have been in the habit of calling by no less hard names conceptions which our president tells us must be regarded with much respect. If we have four dimensions of space we may have forty dimensions, and that would be a long way beyond that which is conceivable by ordinary powers of imagination. I should, therefore, not like to draw too closely the limits as to what may be contradiction to the best established principles. Now, let us turn to a proposition which no one can possibly deny—namely, that there is a distinct sense in which man is an animal. There is not the smallest doubt of that proposition. If anybody entertains a misgiving on that point he has simply to walk through the museum close by in order to see that man has a structure and a framework which may be compared, point for point and bone for bone, with those of the lower animals. There is not the smallest doubt moreover that, as to the manner of his becoming, man is developed, step by step, in exactly the same way as they are. There is not the smallest doubt that his activities—not only his mere bodily functions, but his other functions—are just as much the subjects of scientific study as are those of ants or bees. What we call the phenomena of intelligence, for example (as to what else there may be in them, the anthropologist makes no assertion)—are phenomena following a definite causal order just as capable of scientific examination, and of being reduced to definite law, as are all those phenomena which we call physical. And just as ants form a polity and a social state, and just as these are the proper and legitimate study of the zoologist, so far as he deals with ants, so do men organise themselves into a social state, and though the province of politics is of course outside that of anthropology, yet the consideration of man, so far as his instincts lead him to construct a social economy, is a legitimate and proper part of anthropology, precisely in the same way as the study of the social state of ants is a legitimate object of zoology. So with regard to other and more subtle phenomena. It has often been disputed whether in animals there is any trace of the religious sentiment. That is a legitimate subject of dispute and of inquiry; and if it were possible for my friend Sir John Lubbock to point out to you that ants manifest such sentiments he would have made a very great and interesting discovery, and no one could doubt that the ascertainment of such a fact was completely within the province of zoology. Anthropology has nothing to do with the truth or falsehood of religion—it holds itself absolutely and entirely aloof from such questions—but the natural history of religion, and the origin and the growth of the religions entertained by the different kinds of the human race are within its proper and legitimate province. I now go a step farther, and pass to the distribution of man. Here, of course, the anthropologist is in his special region. He endeavours to ascertain how various modifications of the human stock are arranged upon the earth's surface. He looks back to the past, and inquires how far the remains of man can be traced. It is just as legitimate to ascertain how far the human race goes back in time as it is to ascertain how far the horse goes back in time; the kind of evidence that is good in the one case is good in the other; and the conclusions that are forced on us in the one case are forced on us in the other also. Finally, we come to the question of the causes of all these phenomena, which, if permissible in the case of other animals, is permissible in the animal man. Whatever evidence, whatever chain of reasoning justifies us in concluding that the horse, for example, has come into existence in a certain fashion in time, the same evidence and the same canons of logic justify us to precisely the same extent in drawing the same kind of conclusions with regard to man. And it is the business of the anthropologist to be as severe in his criticism of those matters in respect to the origin of man as it is the business of the palæontologist to be strict in regard to the origin of the horse; but for the scientific man there is neither more nor less reason for dealing critically with the one case than with the other. Whatever evidence is satisfactory in one case is satisfactory in the other; and if any one should travel outside the lines of scientific evidence, and endeavour either to support or oppose conclusions which are based upon distinctly scientific truths, by considerations which are not in any way based upon scientific logic or scientific truth—whether that mode of advocacy was in favour of a given position, or whether it was against it, I, occupying the chair of the Section, should, most undoubtedly, feel myself called upon to call him to order, and to tell him that he was introducing considerations with which we had no concern whatever.

I have occupied your attention for a considerable time; yet there is still one other point respecting which I should like to say a few words, because some very striking reflections arose out of it. The British Association met in Dublin twenty-one years ago, and I have taken the pains to look up what was done in regard of our subject at that period. At that time there was no anthropological department. That study had not yet differentiated itself from zoology, or anatomy, or physiology, so as to claim for itself a distinct place. Moreover, without reverting needlessly to the remarks which I placed before you some time ago, it was a very volcanic subject, and people rather liked to leave it alone. It was not until a long time subsequently that the present organisations of this Section of the Association was brought about; but it is a curious fact, that although proper anthropological subjects were at the time brought before the Geographical Section—with the proper subject of which they had nothing whatever to do—I find that even then more than half of the papers that were brought before that section were, more or less distinctly, of an anthropological cast. It is very curious to observe what that cast was. We had systems of language—we had descriptions of savage races—we had the great question, as it then was thought, of the unity or multiplicity of the human species. These were just touched upon, but there was not an allusion in the whole of the proceedings of the Association at that time to those questions which are now to be regarded as the burning questions of anthropology. The whole tendency in the present direction was given by the publication of a single book, and that not a very large one—namely, "The Origin of Species." It was only subsequent to the publication of the ideas contained in that book that one of the most powerful instruments for the advance of anthropological knowledge—namely, the Anthropological Society of Paris—was founded. Afterwards the Anthropological Institute of this country, and the great Anthropological Society of Berlin came into existence, until it may be said that now there is not a branch of science which is represented by a larger or more active body of workers than the science of anthropology. But the whole of these workers are engaged, more or less intentionally, in providing the data for attacking the ultimate great problem, whether the ideas which Darwin has put forward in regard to the animal world are capable of being applied in the same sense and to the same extent to man. That question, I need not say, is not answered.

It is a vast and difficult question, and one for which a complete answer may possibly be looked for in the next century; but the method of inquiry is understood; and the mode in which the materials are now being accumulated bearing on that inquiry, the processes by which results are now obtained, and the observation of these phenomena leads to the belief that the problem also, some day or other, will be solved. In what sense I cannot tell you. I have my own notion about it, but the question for the future is the attainment, by scientific processes and methods, of the solution of that question. If you ask me what has been done within the last twenty-one years towards this object, or rather towards clearing the ground in the direction of obtaining a solution, I don't know that I could lay my hand upon much of a very definite character—except as to methods of investigation—save in regard to one point. I have some reason to know that about the year 1860, at any rate, there was nothing more volcanic, more shocking, more subversive of everything right and proper, than to put forward the proposition that as far as physical organisation is concerned there is less difference between man and the highest apes than there is between the highest apes and the lowest. Now my memory carries me back sufficiently to remind me that, in 1860, that question was not a pleasant one to touch on. The other day I was reading a recently-published valuable and interesting work, "L'Espèce Humaine," by a very eminent man, M. de Quatrefages. He is a gentleman who has made these questions his special study, and has written a great deal and very well about them. He has always maintained a temperate and fair position, and has been the opponent of evolutionary ideas, so that I turned with some interest to his work as giving me a record of what I could look on as the progress of opinion during the last twenty years. If he has any bias at all it is one in the opposite direction to which my own studies would lead me. I cannot quote his words, for I have not the book with me, but the substance of them is that the proposition which I have just put before you is one, the truth of which no rational person acquainted with the facts could dispute. Such is the difference which twenty years has made in that respect, and speaking in the presence of a great number of anatomists, who are quite able to decide a question of this kind, I believe that the opinion of M. de Quatrefages on the subject is one they will all be prepared to indorse. Well, it is a comfort to have got that much out of the way. The second direction in which I think great progress has been made is with respect to the processes of anthropometry, in other words, in the modes of obtaining those data which are necessary for anthropologists to reason upon. Like all other persons who have to deal with physical science, we confine ourselves to matters which can be ascertained with precision, and nothing is more remarkable than the exactness which has been introduced into the mode of ascertaining the physical qualities of man within the last twenty-five years. One cannot mention the name of Broca without the greatest gratitude; and I am quite sure that when Prof. Flower brings forward his paper on cranial measurements on Monday next you will be surprised to see what precision of method and what accuracy are now introduced, compared with what existed twenty-five years ago, into these methods of determining the physical data of man's structure. If, further, we turn to those physiological matters bearing on anthropology which have been the subject of inquiry within the last score of years, we find that there has been a vast amount of progress. I would refer you to the very remarkable collection of the data of sociology by Mr. Herbert Spencer, which contains a mass of information useful on one side or the other, in getting towards the truth. Then I would refer you to the highly interesting contributions which have been made by Prof. Max Müller and by Mr. Tylor to the natural history of religions, which is one of the most interesting chapters of anthropology. In regard to another very important topic, the development of art and the use of tools and weapons, most remarkable contributions have been made by General Lane Fox, whose museum at Bethnal Green is one of the most extraordinary exemplifications that I know of the ingenuity, and, at the same time, of the stupidity of the human race. Their ingenuity appears in their invention of a given pattern or form of weapon, and their profound stupidity in this, that having done so, they kept in the old grooves, and were thus prevented from getting beyond the primitive type of these objects and of their ornamentation. One of the most singular things in that museum is its exemplification—the wonderful tendency of the human mind when once it has got into a groove to stick there. The great object of scientific investigation is to run counter to that tendency.

Lastly, great progress has been made in the last twenty years in the direction of the discovery of the indications of man in a fossil state. My memory goes back to the time when anybody who broached the notion of the existence of fossil man would have been simply laughed at. It was held to be a canon of palæontology that man could not exist in a fossil state. I don't know why, but it was so; and that fixed idea acted so strongly on men's minds that they shut their eyes to the plainest possible evidence. Within the last twenty years we have an astonishing accumulation of evidence of the existence of man in ages antecedent to those of which we have any historical record. What the actual date of those times was, and what their relation is to our known historical epochs, I don't think anybody is in a position to say. But it is beyond all question that man, and not only man, but what is more to the purpose, intelligent man, existed at times when the whole physical conformation of the country was totally different from that which characterises it now. Whether the evidence we now possess justifies us in going back further, or not—that we can get back as far as the epoch of the drift is, I think, beyond any rational question or doubt; that may be regarded as something settled—but when it comes to a question as to the evidence of tracing back man further than that—and recollect drift is only the scum of the earth's surface—I must confess that to my mind the evidence is of a very dubious character.

Finally, we come to the very interesting question—as to whether, with such evidence of the existence of man in those times as we have before us, it is possible to trace in that brief history any evidence of the gradual modification from a human type somewhat different from that which now exists to that which is met with at present. I must confess that my opinion remains exactly what it was some eighteen years ago when I published a little book which I was very sorry to hear my friend, Prof. Flower, allude to yesterday, because I had hoped that it would have been forgotten amongst the greater scandals of subsequent times. I did there put forward the opinion that what is known as the Neanderthal skull is, of human remains, that which presents the most marked and definite characteristics of a lower type—using the language in the same sense as we would use it in other branches of zoology. I believe it to belong to the lowest form of human being of which we have any knowledge, and we know from the remains accompanying that human being, that as far as all fundamental points of structure were concerned, he was as much a man—could wear boots just as easily—as any of us, so that I think the question remains pretty much where it was. I don't know that there is any reason for doubting that the men who existed at that day were in all essential respects similar to the men who exist now. But I must point out to you that this conviction is by no means inconsistent with the doctrine of evolution. The horse, which existed at that time, was in all essential respects identical with the horse which exists now. But we happen to know that going back further in time the horse presents us with a series of modifications by which it can be traced back from an earlier type. Therefore it must be deemed possible that man is in the same position, although the facts we have before us with respect to him tell in neither one way nor the other. I have now nothing more to do than to thank you for the great kindness and attention with which you have listened to these informal remarks.

SECTION E.

GEOGRAPHY.

Opening Address by the President, Prof. Sir C. Wyville Thomson, F.R.S.

In doing me the honour to select me to preside over this section on the present occasion, the Council of the British Association have doubtless had in view the part which it has been my privilege to take in contributing to the physical description of the earth, as director of the civilian scientific staff on board H.M.S. *Challenger*. I will not, therefore, apologise for following the example of several of my more immediate predecessors in leaving to others the subject of topographical geography which I have never made a special study, and directing your attention for the short time at my disposal to advances which have been made of late years in certain directions in the application of the physical and natural sciences to the illustration of the general condition of the earth.

Before doing so, however, I must refer to the great geographical event of the year which has passed since the geographical section of the British Association last met—the return of the African explorer, Henry Moreland Stanley. As the graphic account which Mr. Stanley has given of his journey "through the dark continent" is in all our hands, and as we may hope to have an opportunity during this meeting of hearing something further of his adventures from the great traveller himself, it will not be necessary for me at present to enter into any details either with regard to the course taken by his expedition or to the brilliant results which it has achieved. It is, however, incumbent upon us in this place to acknowledge once more the flood of light which Stanley has thrown upon the geography of Central Africa, and to express our wondering admiration of the iron will and the daring intrepidity which carried him through these long years of labour and difficulty and danger. Although, in reading Stanley's narrative, we may be forced to regret some of the dark scenes by which his terrible march was chequered, still no one who has not himself had some dealings with savages can fully understand how entirely the action of a leader, solely responsible for the lives of his party, must be guided in every emergency by considerations which he alone is in a position to weigh.

During the last few years a factor, so altered in its proportions that it has appeared almost new, has entered into the calculations of the naval executive departments of all the maritime powers; and in harmony with the rapid advance of natural knowledge and the widening recognition of its practical value, many opportunities, hitherto too often lost, have been taken advantage of. Latterly almost all special expeditions, whether despatched avowedly with the object of extending the boundaries of science, or for hydrographic purposes, or for training naval cadets, or for drilling the inmates of a penitentiary, or pioneering commercial enterprise, as in the case of Capt. Wiggins' late excursions to the mouth of the Yenisei, have been supplied more or less fully with the means of scientific observation, and have been in many cases accompanied by observers trained in one department or another of physical research.

I will simply name among many such equipped and instructed expeditions of these later days, the splendid circumnavigating voyage of the Austrian frigate *Novara*, under the command of Admiral von Wüllerstorf-Urbair. The report of the scientific results of this expedition has been published by the Austrian Government in eighteen beautifully illustrated volumes, and the completion of this work, after seventeen years of heavy labour, was one of the scientific events of the year 1877. The voyage of the Italian corvette *Magenta* round the world, so well chronicled by Prof. Enrico Hillyer Giglioli; the very important sounding voyages of Capt. Belknap, in the American surveying ship *Tuscarora*; the *Hassler* expedition, the last crowning effort with which the elder Agassiz closed a long and brilliant career devoted to the study and illustration of nature, and the many scientific explorations undertaken from year to year by the officers of the American coast survey, with the co-operation of the younger Agassiz and Count Pourtales; the tentative cruises of the British gunboats *Lightning* and *Porcupine*, culminating in the *Challenger* expedition; the scientific voyage round the world of the German frigate *Gazelle*; the several expeditions sent out by different powers to observe the transit of Venus; the German Arctic expedition, under Capt. Koldewey; the several Swedish expeditions, so rich in zoological results, to the Spitzbergen Sea, under the guidance of Otto Torell, Nordenskjöld, and others; the exhaustive researches into the conditions physical and biological of the North Sea, by the North Sea Commission, under the direction of Dr. H. A. Meyer; the voyage of the *Tegethoff*, which Lieut. Payer has rendered ever memorable by his thrilling story of disaster, success, and heroism; the Arctic voyage of the *Alert* and the *Discovery*, of which Sir George Nares has just published the semi-official narrative, a simple and charming account of almost superhuman effort and insuperable obstruction, which it is impossible to read without a feeling of regret that the devoted little band had attempted what was so hopeless, and at the same time a conviction that if their task had been practicable by human skill and bravery, it must certainly have been accomplished. But although this expedition of necessity failed in its main object, that of reaching the Pole, the additional information which we gain from Capt. Nares' volume and from the more popular sketch of the voyage by Capt. Markham on the physical condition of the Arctic Sea is in the highest degree valuable. I must also mention the very important cruises in connection with the Norwegian Department of Fisheries, which, through the skilled labours of Prof. Mohn and Prof. G. O. Sars, annually contribute largely to our knowledge of the distribution of temperature, of the course of the ocean currents, and of the range of animal life in the North Atlantic. I observe in a letter from Prof. Mohn, dated from Hammerfest on the 10th of last month, that the expedition of the past year has had a successful cruise to Bear Island, where she has left letters for the Dutch Arctic schooner the *Willem Barentz*, and has made many important temperature observations. Prof. Mohn speaks highly of the service rendered by Negretti and Zambra's new reversing thermometer. This is a most ingenious instrument, so constructed that by a simple mechanical arrangement the temperature may be registered at any given depth, irrespective of any number of zones of temperature, higher or lower, through which the instrument may have passed in descending. In the *Challenger* we felt greatly the want of such a thermometer, for although generally throughout the ocean the temperature of the water falls steadily from a surface maximum to a minimum at the bottom, in the Arctic and Antarctic Seas—where a special interest attaches to the vertical distribution of temperatures—the coldest layer is frequently, as in Prof. Mohn's observations, on the surface; and a warmer belt intervenes between it and a bottom-stratum, probably in many cases of intermediate temperature. With the ordinary deep-sea registering thermometer the temperature of the lowest layer cannot be ascertained with certainty. We had Negretti and Zambra's earlier instrument on the reversing principle on board during the latter part of our cruise, but through some defect in construction we did not find its indications trustworthy for great depths. I always believed the plan of construction of this instrument to be good, and I am very glad to find from Prof. Mohn's report that this defect has now been entirely overcome.

It follows from the nature of these many and varied enterprises that the department of geographical science to whose progress they have most specially contributed is the physical geography of the sea; and the special appliances with which they have been provided have been principally instruments for determining the temperature of the water at different depths, the depth of the sea and the nature of the sea-bottom, and, in some few cases, the distribution of the deep-sea fauna. It is of course impossible for me in so short a time even to sketch their several lines of investigation, or to attempt to assign to each its share in the general advance of knowledge; I think it may be better that I should give an outline of some of the conditions of the regions to which they refer by the light of their combined results. I am aware that in taking this course I shall be forced to face questions on which there has been some controversy; and I can only say that I will avoid the controversial aspects of such questions as far as possible, and merely describe as shortly as I can the condition of things as they appear to me.

The General Ocean Circulation.—It was pointed out long ago by Sir Charles Lyell that many of the most marked phenomena of the present physical condition of the globe depend upon the fact that the surface of the world is divided into two hemispheres, one of which contains nearly the whole of the dry land of this world, while the other is almost entirely covered by water. The centre of the land hemisphere is somewhere in Great Britain, and the centre of the water hemisphere, which includes the southern sea, the South Pacific, whatever antarctic land there may be, Australia, and the southern point of South America, is in this neighbourhood of New Zealand. With a full knowledge of the absolute continuity of the ocean we have hitherto been too much in the habit of regarding it as composed of several oceans, each possibly under special physical conditions. All recent observations have, however, shown us that the vast expanse of water which has its centre in the southern hemisphere is the one great ocean of the world, of which the Atlantic with the Arctic Sea and the North Pacific are merely northward extending gulfs; and that any physical phenomena affecting obviously one portion of its area must be regarded as one of an interdependent system of phenomena affecting the ocean as a whole.

Shallow as the stratum of water forming the ocean is—a mere film in proportion to the radius of the earth—it is very definitely split up into two layers, which, so far as all questions concerning ocean movements and the distribution of temperature is concerned, are under very different conditions. At a depth varying in different parts of the world, but averaging perhaps 500 fathoms, we arrive at a layer of water at a temperature of 40° F., and this may be regarded as a kind of neutral band separating the two layers. Above this band the temperature varies greatly over different areas, the isothermobathic lines sometimes tolerably equally distributed, and at other times crowding together towards the surface, while beneath it the temperature almost universally sinks very slowly and with increasing slowness to a minimum at the bottom.

The causes of natural phenomena, such as the movements of great masses of water, or the existence over large areas of abnormal temperature conditions, are always more or less complex, but in almost all cases one cause appears to be so very much the most efficient that in taking a general view all others may be practically disregarded; and speaking in this sense it may be said that the trade-winds and their modifications and counter-currents are the cause of all movements in the stratum of the ocean above the neutral layer. This system of horizontal circulation, although so enormously important in its influences upon the distribution of climate is sufficiently simple. Disregarding minor details, the great equatorial current driven from east to west across the northerly extensions of the ocean by the trade-winds, impinges upon the eastern coasts of the continents. A branch turns northwards and circles round the closed end of the Pacific, tending to curl back to the North American coast from its excess of initial velocity; and in the Atlantic, following a corresponding course, the Gulf Stream bathes the shores of Northern Europe, and a branch of it forces its way into the Arctic basin, and battling against the palæocrystic ice, keeps imperfectly open the water-way by which Nordenskjöld hopes to work his course to Behring's Strait. The southern deflections are practically lost, being to a great extent, though not entirely dissipated in the great westerly current of the southern anti-trades.

One of the most singular results of these later investigations is the establishment of the fact that all the vast mass of water, often upwards of 2,000 fathoms in thickness, below the neutral band, is moving slowly to the northward; that in fact the depths of the Atlantic, the Pacific, and the Indian Oceans are occupied by tongues of the Antarctic Sea, preserving in the main its characteristic temperatures. The maintenance of a low temperature while the temperature of the floor of the ocean must be higher, and that of the upper layers of the sea greatly higher, is in itself a conclusive proof of steady movement of the water from a cold source; and the fact that the temperature of the lower layers of water, both in the Atlantic and the Pacific, is slightly but perceptibly raised to the northward, while the continuity of every layer with a corresponding layer in the southern sea can be clearly traced, indicates the southern position of that source.

The immediate explanation of this very unexpected phenomenon seems simple. For some cause or other, as yet not fully understood, evaporation is greatly in excess of precipitation over the northern portion of the land-hemisphere, while over the water-hemisphere, and particularly over its southern portion, the reverse is the case; thus one part of the general circulation of the ocean is carried on through the atmosphere, the water being raised in vapour in the northern hemisphere, hurried by upper wind currents to the zone of low barometric pressure in the south, where it is precipitated in the form of snow or rain, and welling thence northwards in the deepest channels on account of the high specific gravity dependent on its low temperature, it supplies the place of the water which has been removed.

The cold water wells northwards, but it meets with some obstructions on its way, and these obstructions, while they prove the northward movement, if further proof was needed, bring out another law by which the distribution of ocean temperature is regulated. The deeper water sinks slowly to a minimum at the bottom, so that if we suppose the temperature at a depth of 2,000 fathoms to be 36° F., the temperature at a depth of 3,000 may be, say, 32°. Now, if in this case the slow current meet on its northward path a continuous barrier in the form of a submarine mountain ridge rising to within 2,000 fathoms of the sea-surface, it is clear that all the water below a temperature of 36° will be arrested, and, however deep the basin beyond the ridge may be, the water will maintain a minimum of 36° from a depth of 2,000 fathoms to the bottom. In many parts of the ocean we have most remarkable examples of the effect upon deep-sea temperature of such barriers intersecting cold indraughts, the most marked instance, perhaps, a singular chain of closed seas at different temperatures among the islands of the Malay Archipelago; but we have also a striking instance nearer home. Evaporation is greatly in excess of precipitation over the area of the Mediterranean, and consequently, in order to keep up the supply of water to the Mediterranean, there is a constant inward current through the Straits of Gibraltar from the Atlantic; I need not at present refer to an occasional tidal counter-current. The minimum temperature of the Mediterranean is about 54° F. from a depth of 100 fathoms to the bottom. The temperature of 54° is reached in the Atlantic at the mouth of the Straits of Gibraltar at a depth of about 100 fathoms, so that in all probability future soundings will show that the free water-way through the Straits does not greatly exceed 100 fathoms in depth.

The Depth of the Sea, and the Nature of Modern Deposits.—It seems now to be thoroughly established by lines of trustworthy soundings which have been run in all directions, that the average depth of the ocean is a little over 2,000 fathoms, and that in all probability it nowhere exceeds 5,000 fathoms. Depths beyond 4,000 fathoms are rare and very local, and seem to be usually pits in the neighbourhood of volcanic islands. In all the ocean basins there are depressions extending over considerable areas where the depth reaches 3,000 fathoms or a little more, and these depressions maintain a certain parallelism with the axes of the neighbouring continents.

Within 300 or 400 miles of the shore, whether in deep or in shallow water, formations are being laid down, whose materials are derived mainly from the disintegration of shore rocks, and which consequently depend for their structure and composition upon the nature and composition of the rocks which supply their materials. These deposits imbed the hard parts of the animals living on their area of deposition, and they correspond in every way with sedimentary formations with which we are familiar, of every age. In water of medium depths down to about 2,000 fathoms, we have in most seas a deposit of the now well-known globigerina-ooze, formed almost entirely of the shells of foraminifera living on the sea-surface, and which after death have sunk to the bottom. This formation, which occupies a large part of the bed of the Atlantic and a considerable part of that of the Pacific and Southern Seas, is very like chalk in most respects, although we are now satisfied that it is being laid down as a rule in deeper water than the chalk of the cretaceous period.

In depths beyond 2,500 or 3,000 fathoms no such accumulations are taking place. The shores of continents are usually too distant to supply land detritus, and although the chalk-building foraminifera are as abundant on the surface as they are elsewhere, not a shell reaches the bottom; the carbonate of lime is entirely dissolved by the carbonic acid contained in the water during the long descent of the shells from the surface. It therefore becomes a matter of very great interest to determine what processes are going on, and what kind of formations are being laid down in these abyssal regions, which must at present occupy an area of not less than ten millions of square miles.

The tube of the sounding instrument comes up from such abysses filled with an extremely fine reddish clay, in great part amorphous, but containing, when examined under the microscope, a quantity of distinctly recognisable particles, organic and inorganic. The organic particles are chiefly siliceous, and for the most part the shells or spines of radiolarians which are living abundantly on the surface of the sea, and apparently in more or less abundance at all depths. The inorganic particles are minute flakes of disintegrated pumice, and small crystalline fragments of volcanic minerals; the amorphous residue is probably principally due to the decomposition of volcanic products, and partly to the ultimate inorganic residue of decomposed organisms. There is ample evidence that this abyssal deposit is taking place with extreme slowness. Over its whole area, and more particularly in the deep water of the Pacific, the dredge or trawl brings up in large numbers nodules very irregular in shape, consisting chiefly of peroxide of iron and peroxide of manganese, deposited in concentric layers in a matrix of clay, round a nucleus formed of a shark's tooth, or a piece of bone, or an otolith, or a piece of siliceous sponge, or more frequently a fragment of pumice. These nodules are evidently formed in the clay, and the formation of the larger ones and the segregation of their material must have taken a very long time. Many of the sharks' teeth to which I have alluded as forming the nuclei of the nodules, and which are frequently brought up uncoated with foreign matter, belong to species which we have every reason to believe to be extinct. Some teeth of a species of *Carcharodon* are of enormous size, four inches across the base, and are scarcely distinguishable from the huge teeth from the tertiary beds of Malta. It is evident that these semi-fossil teeth, from their being caught up in numbers by the loaded line of the trawl, are covered by only a very thin layer of clay.

Another element in the red clay has caused great speculation and interest. If a magnet be drawn through a quantity of the fine clay well diffused in water, it will be found to have caught on its surface some very minute magnetic spherules, some apparently of metallic iron in a passive state, and some of metallic nickel. From the appearance of these particles, and from the circumstance that such magnetic dust has been already detected in the sediment of snow-water, my colleague Mr. Murray has a very strong opinion that they are of cosmic origin—excessively minute meteorites. They certainly resemble very closely the fine granules which frequently roughen the surface of the characteristic skin of meteorites, and from their composition and the circumstances under which they are found there is much to be said in favour of this view. I cannot, however, hold it entirely proved; there can be little doubt, from the universal presence of water-logged and partially decomposed pumice on the bottom, and from the constant occurrence of particles of volcanic minerals in the clay, that the red clay is formed in a great measure by the decomposition of the lighter products of submarine volcanoes drifted about by currents, and finally becoming saturated with water and sinking; and it is well known that both iron and nickel in a metallic state are frequently present in minute quantities in igneous rocks. I think it is conceivable that the metallic spherules may be derived from this source.

So far as we can judge, after a most careful comparative examination, the deposit which is at present being formed at extreme depths in the ocean does not correspond either in structure or in chemical composition with any known geological formation; and, moreover, we are inclined to believe, from a consideration of their structure and of their imbedded organic remains, that none of the older formations were laid down at nearly so great depths—that, in fact, none of these have anything of an abyssal character. These late researches tend to show that during past geological changes abyssal beds have never been exposed, and it seems highly probable that until comparatively recent geological periods such beds have not been formed.

It appears now to be a very generally received opinion among geologists—an opinion which was first brought into prominence by Prof. Dana—that the "massive" eruptions which originated the mountain chains which form the skeleton of our present continents, and the depressions occupied by our present seas date from the secular cooling and contraction of the crust of the earth from a period much more remote than the deposition of the earliest of the fossiliferous rocks, and that during the period chronicled by the successive sedimentary systems, with many minor oscillations by which limited areas have been alternately elevated and depressed, the broad result has been the growth by successive steps of the original mountain chains and the extension of the continents by their denudation, and the corresponding deepening of the original grooves. If this view be correct—and it certainly appears to me that the reasoning in its favour is very cogent—it is quite possible that until comparatively recent times no part of the ocean was sufficiently deep for the formation of a characteristic abyssal deposit.

Time will not allow me even to allude to the interesting results which have been obtained from the determination of the density of sea water from different localities and different depths, and from the analysis of sea water and its contained gases, and perhaps these results have been scarcely sufficiently worked out as yet to afford safe bases for generalisation. I must, however, say a few words as to certain additions which have been made to our knowledge of the two hitherto impregnable strongholds of the frost, the regions round the North and South Poles.

The Arctic Regions.—The question which has of late held the most prominent place in all discussions about the conditions of the Arctic Regions, particularly since the voyage of Dr. Hayes, is whether it is possible that there can be at all times or at any time anything in the form of an open Polar sea. This question seems now to be virtually settled, and in the most unsatisfactory manner imaginable. There can be no doubt that in the year 1871 Count Wilczek, in the schooner *Isbjörn*, found the sea between Novaya Zemlya and Spitzbergen nearly free from ice, and that the same sea presented to Weyprecht and Payer in the following year a dangerous stretch of moving and impenetrable pack. There can be no doubt that in the year 1861 Dr. Hayes gazed over an expanse of open water where, in 1875-76, Capt. Nares studied the conditions of palæocrystic ice. It is evident, therefore, that the Polar basin, or at all events such portions of it as have been hitherto reached, is neither open sea nor continuous ice, but a fatal compromise between the two, an enormously heavy pack formed by the piling up and crushing together of the floe of successive years, in frequent movement, breaking up and shifting according to the prevailing direction of the wind, and leaving open, now here and now there, lanes and vistas of deceptive open water which may be at any moment closed and converted into a chaotic mass of hurtling floe-bergs by a hurricane from another direction. It seems, however, that in certain seasons there is more open water in the direction of Grinnell's Land and Smith's Sound than in others, and that there are also years comparatively favourable for the northward route following the lead of Franz-Josef Land; and there seem now to be only two plans, one nearly as hopeless as the other, to choose between in any future attempt—either to establish several permanent Polar stations, as proposed by Lieut. Weyprecht, and already initiated at one point, so far as preliminaries are concerned, by Capt. Tyson and Capt. Howgate, and to seize the opportunity of running north in early autumn from the station where the sea appears most open, or to run as far north as possible at enormous expense, with a great force of men and abundance of provisions and paraffin oil, and push northwards during the arctic winter by a chain of communicating stations with ice-built refuge huts. It seems possible that in a cold season, with the pack in the condition in which Markham found it in 1876, some progress might be made in this way if it were conceivable that the end to be gained was worth the expenditure of so much labour and treasure.

The Antarctic Regions.—But little progress has been made during the last quarter of a century in the actual investigation of the conditions of that vast region which lies within the parallel of 70° S. Some additional knowledge has been acquired, and the light which recent inquiries have thrown upon the general plan of ocean circulation and the physical properties of ice, have given a new direction to what must partake for some time to come of the nature of speculation.

From information derived from all sources up to the present time, it may be gathered that the unpenetrated area of about 4,700,000 square miles surrounding the South Pole is by no means certainly a continuous "Antarctic Continent," but that it consists much more probably partly of comparatively low continental land, and partly of a congeries of continental (not oceanic) islands, bridged between and combined, and covered to the depth of about 1,400 feet, by a continuous ice-cap; with here and there somewhat elevated continental chains, such as the groups of land between 55° and 95° W., including Peter the Great Island and Alexander Land, discovered by Billingshausen in 1821, Graham Land and Adelaide Island, discovered by Biscol in 1832, and Louis Philippe Land by D'Urville in 1838, and at least one majestic modern volcanic range discovered by Ross in 1841 and 1842, stretching from Balleny Island to a latitude of 78° S., and rising to a height of 15,000 feet. It seems, so far as is at present known, that the whole of the antarctic land, low and high, as well as the ice-cap of which a portion of the continuous continent may consist, is bordered to some distance by a fringe of ice, which is bounded to seaward by a perpendicular ice-cliff, averaging 230 feet in height above the sea-level. Outside the cliff a *floe*, which attains near the barrier a thickness of about 20 feet, and in some places by piling a considerably greater thickness, extends northwards in winter to a distance varying according to its position with reference to the southward trending branches of the equatorial current; and this floe is replaced in summer by a heavy drifting pack with scattered ice-bergs. Navigating the Antarctic Sea in the southern summer, the only season when such navigation is possible, it has been the opinion of almost all explorers, that after forcing a passage through an outer belt of heavy pack and ice-bergs, moving as a rule to the north-westward, and thus fanning out from the ice-cliff in obedience to the prevailing south-easterly winds, a band of comparatively clear water is to be found within.

Several considerations appear to me to be in favour of the view that the area round the South Pole is broken up and not continuous land. For example, if we look at a general ice-chart we find that the sea is comparatively free from icebergs, and that the deepest notches occur in the "Antarctic Continent" at three points, each a little to the eastward of south of one of the great land masses. Opposite each of these notches a branch of the equatorial current is deflected southwards by the land, and is almost merged in the great drift-current which sweeps round the world in the Southern Sea before the westerly anti-trades. But while the greater portion of the Brazilian current, the East Australian current, and the southern part of the Agulhas current are thus merged, they are not entirely lost; for at these points of junction with the drift-current of the westerlies, the isobathytherms are slightly deflected to the southwards, and it is opposite these points of junction that we have comparatively open sea and penetrable notches in the southern pack. But we have not only the presumed *effect* of this transfer of warmer water to the southwards; we were able to detect its presence in the *Challenger* by the thermometer. Referring to the result of a serial temperature sounding on February 14, 1874, with a surface temperature of 29° F. at a depth of from 300 to 400 fathoms, there is a band of water at a temperature of more than half a degree above the freezing-point. That this comparatively warm water is coming from the north there is ample proof. We traced its continuity with a band at the same depth gradually increasing in warmth to the northward, and it is evident that its heat can be derived from no other source, and that it must be continually receiving new supplies, for it is overlaid by a band of colder water, tending to mix with it by convection.

It is, of course, possible that these warm currents may by coincidence be directed towards those notches already existing in a continental mass of land; but such a coincidence would be remarkable, and there is certainly a suggestion of the alternative that the "continent" may consist to so great an extent of ice as to be liable to have its outline affected by warm currents.

In high southern latitudes it seems that all the icebergs are originally tabular, the surface perfectly level and parallel with the surface of the sea, a cliff about 230 feet high bounding the berg. The top is covered with a layer of the whitest snow; now and then a small flock of petrels take up their quarters upon it, and trample and soil some few square yards, but after their departure one of the frequent snow showers restores it in a few minutes to its virgin whiteness. The upper part of the cliff is pale blue, which gradually deepens towards the base. When looked at closely the face of the cliff is seen to be traversed by a delicate ruling of faint blue lines, the lines being more distant from one another above and becoming gradually closer. The distance between the well-marked lines near the top of a berg may be of a foot or even more, while near the surface of the water it is not more than two or three inches, and the space between the blue lines have lost their dead whiteness and have become hyaline or bluish. The blue lines are very unequal in their strength and in their depth of colouring; sometimes a group of very dark lines gives a marked character to a part of a berg. Between the stronger blue lines near the top of the cliff a system of closer lines may be observed, marking the division of the ice by still finer planes of lamination; but in the narrower spaces near the water-line they are blended and lost. The blue lines are the sections of sheets of clear ice; the white intervening bands are the sections of layers of ice where the particles are not in such close contact—ice probably containing some air.

The stratification in all these icebergs is, I believe, originally horizontal and conformable, or very nearly so. In many, while melting and beating about in the sea, the strata become inclined at various angles, or vertical or even reversed; in many they are traversed by faults, or twisted, or contorted, or displaced; but I believe that all deviations from a horizontal arrangement are due to changes taking place in the icebergs themselves.

I think there can be no doubt, from their shape and form, and their remarkable uniformity of character, that these great table-topped icebergs are prismatic blocks riven from the edge of the great antarctic ice-sheet. I conclude, therefore, that the upper part of the iceberg, including by far the greater part of its bulk, and culminating in the portion exposed above the surface of the sea, was formed by the piling up of successive layers of snow during the period, amounting perhaps to centuries, during which the ice-cap was slowly forcing its way over the low land, and out to sea over a long extent of gentle slope, until it reached a depth considerably beyond 200 fathoms, when the lower specific weight of the ice caused an upward strain which at length overcame the cohesion of the mass, and portions were rent off and floated away. The icebergs when they are first dispersed float in from 200 to 250 fathoms; when, therefore, they have been drifted to latitudes of 65° or 64° south, the bottom of the berg, the surface which forced itself glacier-like over the land, just reaches the layer at which the temperature of the water distinctly rises; and is rapidly melted, and the pebbles and land *débris* with which it is more or less charged are precipitated. That this precipitation takes place all over the area where the icebergs are breaking up, constantly and to a considerable extent, is evident from the fact that the matter brought up by the sounding instrument and the dredge is almost entirely composed of such deposits from ice; for diatoms, foraminifera, and radiolarians are present on the surface in large numbers, and unless the deposit from the ice were abundant it would soon be covered and masked by the skeletons of surface organisms.

The curious question now arises, what is the cause of the uniform height of the southern icebergs—that is to say, what is the cause of the restriction of the thickness of the free edge of the ice-cap to 1,400 fathoms? I have mentioned the gradual diminution in thickness of the strata of ice in a berg from above downwards. The regularity of this diminution leaves it almost without a doubt that the layers observed are in the same category, and that therefore the diminution is due to subsequent pressure or other action upon a series of beds, which were at the time of their deposition nearly equally thick. About 60 or 80 feet from the top of an iceberg, the strata of ice a foot or so in thickness, although of a white colour and thus indicating that they contain a considerable quantity of air, are very hard, and the specific weight of the ice is not much lower than that of layers three inches thick nearer the water-line of the berg. The upper layers have been manifestly produced by falls of snow after the berg has been detached.

Now it seems to me that the reduction in thickness cannot be due to compression alone, but that a portion of the substance of the lower layers must have been removed. It is not easy to see why the temperature of the earth's crust, under a widely extended and practically permanent ice-sheet of great thickness, should ever fall below the freezing-point; and it is a matter of observation that at all seasons of the year vast rivers of muddy water flow into the frozen sea from beneath the great glaciers which are the issues of the ice-sheet of Greenland. Ice is a very bad conductor, so that the cold of winter cannot penetrate to any great depth into the mass. The normal temperature of the surface of the earth's crust, at any point where it is uninfluenced by cyclical changes, is at all events above the freezing-point, so that the temperature of the floor of the ice-sheet would certainly have no tendency and fall below that of the stream passing over it. The pressure upon the deeper beds of the ice must be enormous at the bottom of an ice-sheet 1,400 feet in thickness—not much less than a quarter of a ton on the square inch. It seems, therefore, probable that under the pressure to which the body of ice is subjected a constant system of melting and regelation is taking place, the water passing down by gravitation from layer to layer until it reaches the floor of the ice-sheet, and finally working out channels for itself between the ice and the land, whether the latter be subærial or submerged.

I should think it probable that this process, or some modification of it may be the provision by which the indefinite accumulation of ice over the antarctic continent is prevented and a certain uniformity in the thickness of the ice-sheet maintained—that in fact ice at the temperature at which it is in contact with the surface of the earth's crust within the antarctic regions cannot support a column of itself more than 1,400 feet high without melting. It is suggested to me by Prof. Tait that the thickness of the ice-sheet very probably depends upon its area, as the amount of melting through squeezing and the earth's internal heat, will depend upon the facility of the escape of the water. The problem is, however, an exceedingly complex one, and we have perhaps scarcely sufficient data for working it out.

The Fauna of the Deep Sea.—I can scarcely regret that it is utterly impossible for me on this occasion to enter into any details with regard to the relations of the abyssal fauna, the department of the subject which has naturally had for me the greatest interest. Recent investigations have shown that there is no depth limit to the distribution of any group of gill-bearing marine animals. Fishes, which, from their structure and from what we know of the habits of their congeners, must certainly live on the bottom, have come up from all depths, and at all depths the whole of the marine invertebrate classes are more or less fully represented. The abyssal fauna is of a somewhat special character, differing from the fauna of shallower water in the relative proportions in which the different invertebrate types are represented. It is very uniform over an enormously extended area, and in this respect it fully confirms the anticipations of the great Scandinavian naturalist Lovén, communicated to this Association in the year 1844. It is a rich fauna, including many special genera and an enormous number of special species, of which we, of course, know as yet only a fraction; but I do not think I am going too far in saying that from the results of the *Challenger* expedition alone the number of known species in certain classes will be doubled. The relations of the abyssal fauna to the faunæ of the older tertiary and the newer mesozoic periods are much closer than are those of the faunæ of shallow water; I must admit, however, that these relations are not so close as I expected them to be—that hitherto we have found living only a very few representatives of groups which had been supposed to be extinct. I feel, however, that until the zoological results of several of these later voyages, and especially those of the *Challenger*, shall have been fully worked out, it would be premature to commit myself to any generalisations.

I have thus attempted to give a brief outline of certain defensible general conclusions, based upon the results of recent research. Some years ago, certain commercial enterprises, involving the laying of telegraph cables over the bed of the sea, proved that the extreme depths of the ocean were not inaccessible. This somewhat unexpected experience soon resulted in many attempts, on the part of those interested in the extension of the boundaries of knowledge, to use what machinery they then possessed to determine the condition of the hitherto unknown region. This first step was naturally followed by a development of all appliances and methods bearing upon the special line of research; and within the last decade the advance of knowledge of all matters bearing upon the physical geography of the sea has been confusingly rapid—so much so, that at this moment the accumulation of new material has far outstripped the power of combining and digesting and methodising it. This difficulty is greatly increased by the extreme complexity of the questions, both physical and geological, which have arisen. Steady progress is, however, being made in both directions, and I trust that in a few years our ideas as to the condition of the depth of the sea may be as definite as they are with regard to regions to which we have long had ready access.

SECTION G.

MECHANICAL SCIENCE.

OPENING ADDRESS BY THE PRESIDENT, EDWARD EASTON, C.E.

On the Conservancy of Rivers and Streams.

BY the conservancy of rivers and streams I mean the treatment and regulation of all the water that falls on these islands from its first arrival in the shape of rain and dew to its final disappearance in the ocean.

I had at first, in my ignorance, contemplated treating the subject in a still wider manner by referring to the rivers and streams of other countries; but I soon found that the vast extent of the field to be traversed would make it extremely unlikely that I could, with any satisfactory result, attempt the more restricted task which I have now before me.

The question of the conservancy of rivers and streams involves the consideration of their regulation for the following principal purposes:—

1st. For the supply of pure and wholesome water for the domestic and sanitary wants of the population.

2nd. For the supply of water of proper quality and sufficient quantity for industrial purposes.

3rd. For the proper development of water power.

4th. For the drainage and irrigation of land.

5th. For navigation and commerce.

6th. For the preservation of fish.

Until the appointment last year of the Select Committee presided over by the Duke of Richmond, no attempt, as far as I am aware, has been made to grapple with the question as a whole, and the Report made by them omitted to deal with, at least, two of the objects I have indicated as being necessary to the proper dealing with the subject.

The recommendations made in the Report of that Committee were most important, and will, if carried out, remove many of the difficulties which stand in the way of a complete system of conservancy of our rivers.

So much has been written on the engineering details of this subject, by men far better qualified than I am to deal with them, that I shall confine myself to the simple statement of the principles which have been recognised by the chief authorities as essential, and to a few suggestions, which my own experience leads me to think may be of some value. Almost all the great engineers of former generations, who have paid attention to this question, Smeaton, Telford, Rennie, Golborne, Mylne, Walker, Rendel, Stephenson, Jessop, Chapman, Beardmore, and without mentioning names, many of the most eminent now living, have agreed to the following general propositions:—

That the freer the admission of the tidal water, the better adapted is the river for all purposes, whether of navigation, drainage, or fisheries.

That its sectional area and inclination should be made to suit the required carrying power of the river through its entire length, both for the ordinary flow of the water, and for floods.

That the downward flow of the upland water should be equalised as much as possible throughout the entire year; and

That all abnormal contaminations should be removed from the streams.

In carrying out these principles, it is perhaps superfluous to say, that modifications must be introduced to suit the particular phenomena of each river. In some watershed areas it would be easy to construct reservoirs, which would to a great extent equalize the flow and reduce floods. In others, it might be better to control the floods by means of embankments. In others, to have weirs, and sluices, delivering into side channels, parallel to the main stream, with the same object. Sometimes reservoirs, or receptacles, must be made for catching the *débris* brought down by the streams. In fact, every river must be treated as a separate entity. It is therefore necessary that a systematic collection of data relating to rainfall, the geological character of the gathering ground, and the volume of each separate stream, should be made for each watershed area; and this should be carried on for a sufficient length of time to enable a fairly correct estimate to be formed of the behaviour of the river both in time of flood and in time of drought. The establishment of self-acting, tide-registering gauges at several points of every outfall should be insisted on. By these means the whole of the phenomena of a watershed area could be ascertained and recorded, and safe and trustworthy knowledge could be obtained, which would contribute towards the determination, not only of the works which ought to be executed, but of the incidence of the taxation by which the necessary funds should be raised. For instance, it is obvious that where the geological character of a watershed is variable, one portion of it consisting of a permeable stratum, such as chalk or red sandstone, and another portion of an impervious stratum, such as the tertiary clays or the shales of the millstone grit, the same works would not be adapted to each section of the river, nor would it be fair to charge all with the expense according to the same scale of contribution. The former, that is the permeable stratum, is not only, from its absorbent nature, not the cause of floods, but is, by reason of that characteristic, absolutely constituted by nature one of the very works which must be devised by art to mitigate the effects of rainfall on the latter, or impervious stratum.

Bearing this in mind, I have often thought that nature might be usefully imitated in this operation, by passing the surplus rainfall into the permeable strata of the earth by means of wells, or shafts, sunk through the impermeable strata overlying them. This has been done in isolated cases for the drainage of lands, but not for the deliberate purpose of preventing floods and equalizing the flow of rivers.

I also wish to remark that artificial compensating reservoirs may be much more frequently made use of than is generally supposed to be possible, when it is considered that, so long as the dams are constructed in situations where there is no danger of their giving way, it is by no means necessary that they should be watertight, and that, therefore, they can be constructed at a very much smaller outlay. In fact, the purpose would be answered by a series of open weirs, which would collect the water in times of flood and discharge it gradually down the stream.

The example of our French neighbours in the more general use they make of movable weirs—*barrages*—of various constructions could, I am satisfied, be followed by us with very great advantage in many cases.

The question of water power is one which I think deserves more consideration than it has lately received. It has been the fashion to consider that small watermills are of little or no value, and, in the present state of most rivers and streams, this is to a very great extent true, but only because the supply of water to work them is so variable and uncertain. Sufficient attention has never yet been given to the subject of the amount of compensation water which should be given for the use of riparian proprietors when the watershed areas are dealt with for purposes of water supply. There is a kind of empirical rule acknowledged by most of the eminent water engineers, that one-third of the average flow of three consecutive dry years is a fair equivalent for the abstraction of the water falling on a gathering ground. I am strongly of opinion that, looking to imperial interests, advantage should be taken of every opportunity of dealing with a gathering ground to provide for a much larger proportion of its available water being sent down the streams, so that the natural water power of the country may be properly developed. The extra cost of the necessary works must, as a matter of course, be borne rateably by the interests benefited. It is certain that with the progress of invention many more ways of utilising this power will be discovered. At present, through the medium of compressed air, of hydraulic pressure, and of electro-motors, the great disadvantage of its being only available at the spot where the water runs is overcome, and the power can be transmitted to any distance, and used wherever it may be most conveniently applied.

Sir Robert Kane, in his most valuable and exhaustive work on the "Industrial Resources of Ireland," has given an estimate of the value of the power allowed to escape every year in the shape of floods, and the same calculation might be applied to the sister kingdom. It is probably no exaggeration to say that where running streams exist the power required for estate purposes, on the majority of properties in the United Kingdom, might be obtained by a proper conservation of the natural water resources of those streams.

The consideration I have been able to give to this subject has helped to convince me that, although a vast amount of labour and research has been devoted to it, it is nevertheless one in which "a more systematic direction to scientific inquiry" is urgently needed.

A vast collection of scientific facts exists, but they require arrangement and collation, and future observations should be more strictly classified, so that the bearing of each one, both on the others and on the subject at large, may be properly appreciated with a view to a practical result.

In France this is being done to a very large extent, and an excellent map showing the phenomena of the rivers and streams of that country is now in course of preparation. For many years also very accurate observations of the phenomena of the whole of the basin of the Seine have been taken, and have been centralised (*centralisées*) by that eminent engineer, whose loss all who had the privilege of knowing him, either in his work or in private intercourse, are deploring, M. Belgrand, late Inspector-General of the Ponts et Chaussées, and by his able coadjutor, M. M. G. Lemoine. These observations have been published in the form of diagrams, admirable in their simplicity of design, which show at a glance the bearing of every one of those phenomena on the general character of that river.

In Italy also, where there exists a distinct department having control of the hydraulic works of that country, the same exhaustive system of collation and record has been followed, and the results have been published in a series of tables. In Germany, although the same complete system is not in vogue, its chief river has been the subject of most thorough investigation, the results of which have been published in a beautiful map of the Rhine and its regulating works.

In our own country, as might be expected from the number of engineering works which have been executed, there probably exists an amount of detailed information on special and often minute points which is unsurpassed, and probably unequalled in the world.

But, although as I have said before, a great number of eminent men have treated in an exhaustive manner the phenomena relating to many of the principal rivers of Great Britain and Ireland, yet, as far as I am aware, there has been no attempt to collect and combine these most valuable, though detached fragments of knowledge, so that their relation to one another might be seen, and a general conclusion arrived at. This can only be done by the establishment of a public department analogous to those described as already existing in France and Italy.

When it is considered that many lives are annually sacrificed, either directly by the action of floods, or by the indirect but no less fatal influence of imperfect drainage—when it is remembered that a heavy flood, such as that of last year, or that of the summer of 1875, entailed a monetary loss of several millions sterling in the three kingdoms—that during every year a quantity of water flows to waste, representing an available motive power worth certainly not less than some hundreds of thousands of pounds,—that there is a constant annual expenditure of enormous amounts for removing *débris* from navigable channels, the accumulation of which could be mainly, if not entirely prevented,—that the supply of food to our rapidly growing population, dependent, as it is at present, upon sources outside the country, would be enormously increased by an adequate protection of the fisheries,—that the same supply would be further greatly increased by the extra production of the land when increased facilities for drainage are afforded,—that, above all, the problem of our national water supply, to which public attention has of late been drawn by H.R.H. the Prince of Wales, requires for its solution investigations of the widest possible nature, I believe it will be allowed that the question, as a whole, of the management of rivers is of sufficient importance to make it worthy of being dealt with by new laws to be framed in its exclusive behalf.

I do not wish it to be understood that in suggesting the collection of additional data relating to the phenomena of rivers, I am advocating delay in dealing with the existing state of things until the facts have all been ascertained. On the contrary, I believe that the first step ought to be the establishment of a distinct Water Department, which should at once address itself to the remedying of the evils which are found to be most pressing. The time has long since arrived when the present neglected state of many of our most important streams should be dealt with, and that this was also the conviction of Parliament and of the government is evident, from the appointment of such an influential Committee as that presided over by the Duke of Richmond last session.

A new department should be created—one not only endowed with powers analagous to those of the Local Government Board, but charged with the duty of collecting and digesting for use all the facts and knowledge necessary for a due comprehension and satisfactory dealing with every river, basin, or watershed area in the United Kingdom—a department which should be presided over, if not by a Cabinet Minister, at all events by a member of the government who can be appealed to in Parliament.

The department should have entire charge of, and control over, all estuaries and navigable channels, both because these are used by foreign vessels, and therefore the responsibilities attaching to their preservation are international, and because they must be protected from hostile attack, and on these accounts are essentially imperial property. For the same reason the cost of amending and maintaining them should be defrayed out of the imperial exchequer.

As regards the regulation of the remainder of the watershed area, the conclusions arrived at in the Report of the Duke of Richmond's Select Committee seem to me entirely satisfactory. I cannot do better than give a few extracts from that Report. The Committee say—"That in order to secure uniformity and completeness of action each catchment area should, as a general rule, be placed under a single body of conservators, who should be responsible for maintaining the river from its source to its outfall in an efficient state. With regard, however, to tributary streams, the care of these might be entrusted to district committees acting under the general direction of the conservators, but near the point of junction with the principal stream they should be under the direct management of the conservators of the main channel, who should be a representative body constituted of residents and owners of property within the whole area of the watershed. The Committee go on to say that "means should be taken to ensure the appointment of a conservancy board for each watershed area," but that application should first be made by persons interested in the district, and that then the departmental authorities should send inspectors to make local inquiries and to report upon the "necessities and capacities of the district, and suggest the area and proportions of taxation."

With regard to what is probably the most important point of all, the finding of the money necessary to carry out these recommendations, the Committee advocate the introduction of a new principle of taxation, the soundness of which cannot be questioned. Instead of the principle first introduced by the statute of Henry the Eighth, and observed ever since, of levying taxes in proportion to the direct benefit conferred, the Committee propose that the rates should be distributed over the whole area of a watershed, including not only the lands, but the towns and houses and all other property situate within that area. This is in fact no more than a general application of the law of highways, which in the time of the Romans, according to Justinian, applied equally to waterways. It is perfectly just that every acre, the drainage of which contributes to the flow of the streams and rivers of every watershed area, should, in some proportion or other, contribute also to the cost of maintaining the channels of those streams and rivers in an efficient state. The incidence of the taxation must of course, as has been pointed out, be determined by the circumstances of each particular case, but there is no doubt that the conclusion of the Duke of Richmond's Committee, that "the taxation should be levied on the basis of rateable value," is the only sound, and at the same time practical, way of dealing with this difficulty.

The word "taxation" is not, I fear, generally connected with any idea of profit to the individual taxpayer. But in this case, as I hope in the course of this address I have made clear, the prevention of large present losses, and the advantages gained by an improved system, will give not only a fair but an ample return on the capital expended.

It is my firm belief that an intelligent management of watershed areas would be compatible with an absolute profit to every interest affected; that we have here no question of give and take, but that in this, as in every other case, the laws of nature, under proper and scientific regulation, can be made subservient to the needs of the highest civilisation.

THE PHONOGRAPH AND VOWEL SOUNDS[1]

III.

WE now pass to the general conclusions which may be drawn from our experiments. In the first place it seems clear that vowels do not depend on pitch alone or on the simple grouping of partial tones independently of absolute pitch. Before the constituents of a vowel can be assigned, the pitch of the prime must be named. But on the other hand the pitch of the most prominent partial of the group is not alone sufficient to allow us to name the vowel in which it appears; to do this we also require to be told the relation of the constituent partials to one another.

The sound *ū* consists mainly of one tone generally lying in the region above *a*.

The sound *ō* requires at least two partials; when there are only two important ones these lie in the region between *g* and f'', a region covering nearly two octaves. Indeed the upper limit may extend above f'' with a tenor or woman's voice. Other partials than the prime are reinforced by the mouth-cavity over all this region. This great range is obviously a distinguishing mark of *ō* as compared with *ū*, perhaps *the* distinguishing mark; for when *ū* and *ō* are sung at various pitches, the most prominent partial, first of one letter and then of the other, is highest, and the most prominent partial of both sounds may lie on $b'\flat$, the characteristic tone of *ō*. An *ū* sung on $b\flat$ may even have the tone $b'\flat$ more strongly present in it than an *ō* of the same pitch. When *ū* was sung by voice 1 on $b\flat$ the prominent partial was the second; when *ō* was sung at the same pitch by the same voice, the second partial was still the most prominent. Thus for voice 1 the chief distinction between *ū* and *ō* on this note lay not in the pitch of strongest reinforcement, but in the fact that the prime was larger for *ō* than for *ū*. When voice 5 sang *ū* on $b\flat$ the prominent partial was the prime; when it sang *ō* the prominent partial was the second, the prime being also strong. Thus, for voice 1, the distinction lay in the fact that the prime was much smaller for *ō* than for *ū*. It is obvious here that the ear cannot have been guided by the absolute pitch of the reinforcement to the distinction between *ō* and *ū*. At this pitch the distinction lies in the fact that *ō* contains two strong partials (the prime and second), whereas *ū* contains only one (the prime *or* the second). The argument is not weakened by saying that the *ū* of one voice was not the same as the *ū* of the other. Identically the same it cannot have been; nevertheless, on higher or lower notes the two voices agreed as to the composition of *ū*, and generically the vowels were certainly the

[1] Continued from p. 397.

same. There remains the fact that speakers and hearers were unconscious of any generic change in the vowel *ū* when the pitch of the strongly-reinforced partial changed by a whole octave.

On the other hand, it is equally clear the voice, in singing a given vowel at various pitches, does not simply produce a certain constant group of relative partial tones. Possibly, indeed, the ear might recognise a single tone, especially if very feebly accompanied by higher harmonics, as a kind of *ū* outside the region within which the human voice forms *ū* in that way. Thus Helmholtz, in his *Tonempfindungen*, says that the single tone B♭, when sounded alone, gave a very dull *ū*, much duller than could be produced by the voice. This tone is an octave below the place where voice I ceased to make *ū* by reinforcing the prime. Quite similarly it is conceivable that the group consisting of a prime and its octave might be recognised as *ō* even when produced below the limits within which the human voice does produce this simple harmony in singing *ō*. Our own impression as to the result of running the phonograph slower when it is speaking than when it is spoken to, supports this view, but we do not desire to base any inference on that. Certainly the low *ō* produced in this manner is not the human *ō*.

Moreover, we find a very decided resemblance in the relative constituents of *ō* at a low pitch and *a*° or *ā* at higher pitches. *ō* in the neighbourhood of B♭, and *ā* in the neighbourhood of *f* and *g*, are pretty similarly constituted. Our experiments on *a*° and *ā* are not sufficiently extended to allow any very general conclusions to be drawn, but they are sufficient to show that between certain vowels the main distinction must lie in the absolute pitch of the reinforced group of partial tones.

We are thus brought back to our original statement that in distinguishing vowels the ear is aided by two factors, one depending on the harmony or group of partials, and the other on the absolute pitch of the constituents. It seems not a little singular that the ear should attribute a distinct unity to sounds so dissimilar in their relative and absolute composition as those represented by the curves of Fig. 1.

We are forced to the conclusion already adopted by Helmholtz and Donders that the ear recognises the kind of cavity by which the reinforcement is produced; that although the sounds which issue differ so much that we fail when they are graphically represented and mathematically analysed to grasp any one prominent common feature, nevertheless by long practice the ear is able to distinguish between the different sorts of cavities which are formed in pronouncing given vowels. Something of the same kind may be observed with other sources of sound than the human voice; the resonating cavities of various musical instruments aid greatly in allowing each particular species to be recognised at once, though their effect must be widely different at different parts of the scale. It is, moreover, no mere inference that we recognise the cavity. Prof. Crum Brown's gutta percha bottle, described before, proves that we do, and that it is a group of tones reinforced by a particular kind of cavity that we call a particular vowel. But we have to consider what light the experiments throw on the kinds of cavity that are required for certain vowels. The cavities are clearly distinguished in virtue of two distinct properties: first, the pitch of their maximum resonance or strongest proper tone, and second the range of reinforcement which they are capable of producing. This latter property has, we believe, been hitherto much neglected.

Prof. Crum Brown's bottle proves that a *constant* cavity is capable of producing the constant vowel *ō* over a large range of pitch. On the other hand our experiments with various human voices singing *ō* appear to exhibit a *tuning* of the cavity by which new partials are sometimes introduced somewhat abruptly; and for the sound *ū* it seems certain that the cavity is tuned, that is to say, that the pitch of its proper tone is not the same when the vowel is sung on different pitches. The appreciably strong *fourth* partial in all the duplex *ū*'s of Table VI. may here be noticed as favouring the view that in each of these examples the oral cavity had been adjusted so as to be in unison with the second partial. We may describe the *ū* cavity as an adjustable cavity with a very limited range of resonance, whose effect is to reinforce strongly only one partial lying above *a*. It is possible that this cavity may keep itself constant throughout the very limited range of pitch employed in ordinary speech, but when the range is increased as in singing, a certain tuning seems indispensable.

If we assume that the *ō* cavity is absolutely constant, we must describe it as a cavity capable of reinforcing more or less strongly tones lying anywhere between *g* and *f*″. This cavity

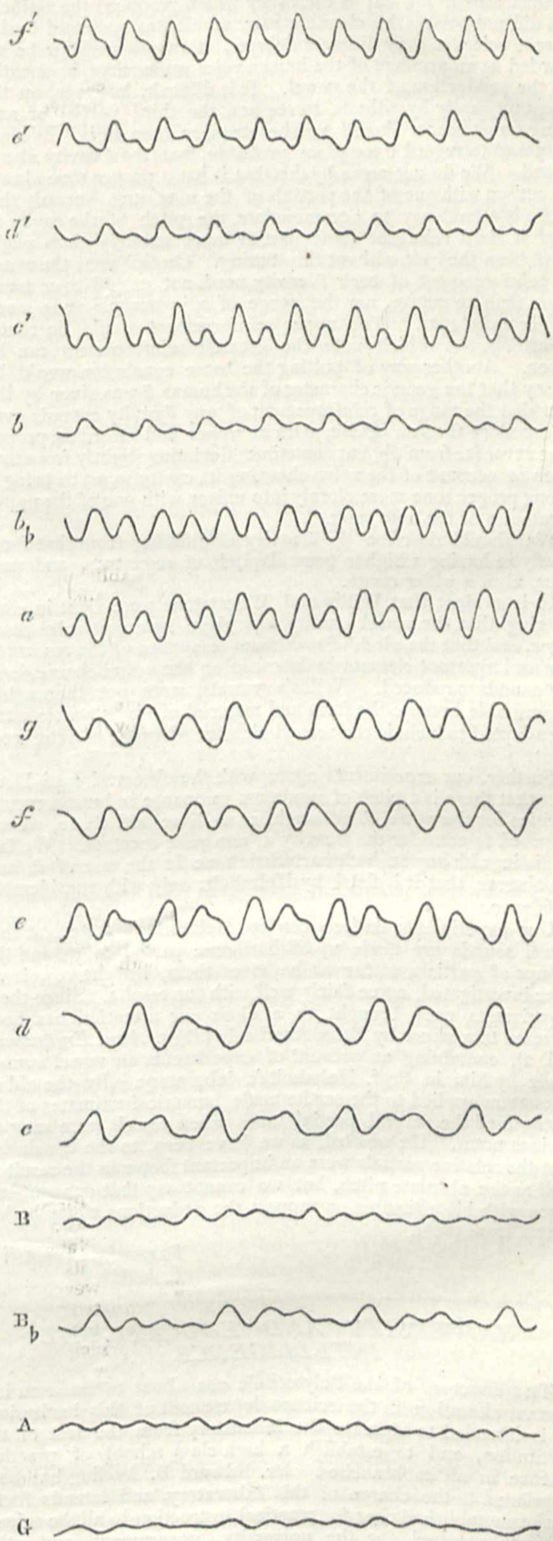

FIG. 1.[1]—*Wave-forms of ō Sung by the same Voice at Various Pitches.*

[1] We reproduce this week the figure which accompanied Profs. Jenkin and Ewing's first paper, on "The Wave-Form of Ō," p. 342. It shows the delicate forms of the curves with greater exactness, and will enable the reader to understand more clearly the value of the conclusions come to by the authors.—ED.

has, in the case of all the human voices investigated, one strong proper tone in $b'\flat$, but as the cavity which produced the artificial ō's did not possess this characteristic, and it was possessed by different voices in very different degrees, it should perhaps be regarded as an accident of the human voice rather than as essential to the production of the vowel. It is difficult, however, on the constant cavity hypothesis, to see how the third partial of an ō sung on or near *a* should not be stronger than it is. We are disposed to regard it as more probable that the ō cavity also is tuned. We do not mean by this that it has a proper tone always in unison with one of the partials of the note sung, but only that there is a tendency to accommodate the pitch of the cavity so that it shall reinforce some partial more strongly than might have been the case without this tuning. On this view the range of reinforcement of each ō cavity need not extend over much more than an octave, nor the range of adjustment over so much as six semitones. The upper reinforcement would lie round about $b'\flat$, on which note the greatest reinforcement can be given. Another way of putting the same conclusion would be to say that the generic character of the human ō was given by the fact that the range of reinforcement of any ō cavity extends over rather more than an octave, with an upper and strongest proper tone never far from $b'\flat$, but sometimes deviating slightly from that pitch on account of the voice choosing its cavity so as to bring a strong proper tone more closely into unison with one of the upper harmonics of the note sung.

We should describe the ā cavity as differing from that for ō chiefly in having a higher general pitch of resonance, and perhaps, also, a wider range.

It is evident that Willis and Wheatstone were right in considering that the vowel quality was given by a particular resonator, and that the pitch of maximum resonance of the resonator was an important element in determining the vowel character of the sounds produced. Willis's vowels were not thoroughly recognisable because the form and material of his resonator were not adapted to include the second element of range of reinforcement.

Further, our experiments agree with the observation of Donders that there is a pitch of maximum resonance in human mouth cavities for the vowel ō, although, as we have said above, we are disposed to consider the ō cavity as not quite constant. We fail to distinguish any such characteristic tone in the case of ū, and we observe that it is fixed by Helmholtz only with considerable diffidence.

Our experiments entirely confirm Helmholtz's statement that vowel sounds are made up of harmonic partial tones, and the groups of partials, so far as he gives them, for the vowels we have investigated, agree fairly well with our results. Since these experiments were brought to a close our attention has been directed to a paper by Felix Auerbach (*Pogg. Ann. Ergänzung*, viii. 2), containing an account of experiments on vowel sounds made by him in Prof. Helmholtz's laboratory. By the aid of resonators applied to the ear he made numerical estimates of the strength of the several partial tones when vowels were sung on various notes. He was led, as we have been, to the conclusion that the relative partials were an important factor in the result as well as the absolute pitch, but we cannot say that our numbers agree with his estimates or support the deductions which he has drawn from them.

FLEEMING JENKIN
J. A. EWING

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE directors of the Polytechnic are about to make an important alteration in the science department of this institution. It is intended to separate the laboratory from the rest of the institution, and to establish a high-class school of practical science in all its branches. Dr. Edward B. Aveling has been appointed to the charge of this laboratory, and intends forthwith to establish classes for practical instruction in all the science subjects required for the university, government, and other examinations.

THE first series of 500 select public teachers in France arrived in Paris on August 15 to visit the Exhibition, at the expense of the government. They are accompanied by 1,000 teachers of the same districts travelling at their own expense, but conveyed at half price by the various railway companies, and boarded in several Paris colleges. They were received at the Sorbonne, in the large hall, by M. Casimir-Périer, the Sub-Minister of Public Instruction, and lectured by M. Levasseur on the teaching of geography. They will be lectured on the teaching of French, of history, and on the organisation of lectures and public libraries. They will leave on Friday, and be succeeded by another set of teachers.

M. BARDOUX has issued a circular intimating that a special financial department has been created for facilitating the building of school-houses in the several French communes. A credit of 60,000,000 francs has been voted by the Chambers, and will be divided amongst the several municipalities that desire to improve or rebuild their public schools, on the condition that each should expend a sum of at least double that taken from the public exchequer.

PROF. A. WOLTMANN, of Prague, has accepted a call to the directorship of the Archæological Institute at Strasburg.

THE first experiment of an educational turn for children, about which we spoke some months ago, has given such good results that a new society is in process of formation at St. Petersburg for a similar purpose. Several eminent teachers of the Russian capital have offered their services to the society, which will yearly send out companies of children on educational travel, as well as parties of young ladies and young men who have finished their studies in secondary schools, or are following the courses of high schools.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, August 12.—M. Fizeau, president.—On the composition of the milk of the cow-tree (*Brosmium galactodendron*), by M. Boussingault. He finds that while the general constitution of the juice of the cow-tree approaches that of milk, the proportions of the various substances are very different.—Observation on the discovery announced by Mr. L. Smith of a new earth belonging to the cerium group, by M. C. Marignac. He does not see any reason for distinguishing the supposed new earth from terbene.—Studies on the placenta of the Aï (*Bradypus tridactylus*, Lin.); the place which that animal should occupy in the series of mammals, by M. N. Joly.—On the fundamental co-variants of a cubo-biquadratic binary system, by Prof. Sylvester.—New process for the analysis of milk, yielding rapidly butter, lactose, and caseine, in one and the same specimen, by M. A. Adam.—M. J. Vinot sent to the Academy a letter addressed to him by Leverrier in September, 1876, in which the late astronomer inferred, from various observations, that there are two intra-Mercurial planets.—On the functions of leaves; function of the stomata in the exhalation and inhalation of aqueous vapours by leaves, by M. Merget.—On the delay of the pulse in intrathoracic aneurisms and in aortic insufficiency, by M. Fr. Franck.—Chemical researches on the division of cyclamine into glucose and mannite, by M. S. de Luca.—On parasitic isopods of the genus Entoniscus, by M. Alf. Giard.—On the changes of colour of *Nika edulis*, by M. S. Jourdain.—Importance of the partition of vegetable cells in the phenomena of nutrition, by M. Max. Cornu.—On the part of stipules in inflorescence and in the flower, by M. D. Clos.—On the fall of avalanches, by M. Ch. Dufour.

CONTENTS