THURSDAY, JANUARY 18, 1872

THE SOLAR ECLIPSE

SURELY if eclipse expeditions had their mottoes, that of the expedition of this year should be *per mare per terram;* for it has been *per mare per terram* in our case with a vengeance! Probably when we return, the curious individuals who total up in the *Times* the aggregate number of years those people have lived whose deaths are there recorded, will, in asking us for our autographs, beg also a detailed statement of the number of miles each of us has travelled in the performance of our duty. I fear it will be very difficult to give the information; and if the temperature in the shade be wanted too, the thing will be perfectly hopeless; for, thank goodness, we took the precaution to bring no thermometers; had we done so and looked at them, it might have been all over with us. Let me point my remarks. A week ago I was at Bekul, having travelled I know not how many thousand miles by sea, and having scarcely set foot on land for a month. We were in the jungle, the heat was burning, some of us had fever, and it was opium which enabled me at all events to get through the eclipse, for it was that memorable day just a week ago. Since then, by night and by day, Dr. Thomson, Captain Maclear, and myself, have been— I seek a word, wafted is too weak, jolted is too strong, for some parts of our journey, though ridiculously lacking in expression for others—well, conveyed from Bekul, now in men-carried conveyances, the cunning bearers with their plaintive moaning, by no means unmelodious, keeping step, giving us an idea of the tremendous labour they were undergoing, and reminding us of a certain journey which we must all make once; now on men's shoulders, now in bullock bandy, speed about two miles an hour, thanks to a brutal breach of contract, which has upset my plans terribly, now in Indian railway carriages, average speed ten miles an hour, temperature of carriage at noon unknown, and lastly in the horse transit of the Madras Carrying Company. Oh! that their carriages were as good as their arrangements and the speed of their horses; and, now, here I am shivering, surrounded by hoar frost, with a soupçon of a difficulty of breathing in this higher air after the dense atmosphere of the jungles, but all the same in an earthly paradise with hedges of roses although it is mid-winter, the whole place a perfect garden. I am at Ootacamund, at an elevation of some 7,000 feet with an Australian fauna; and within a few hours I hope to see Janssen, who is still here; Tennant, Herschel, and Hennessy I have unfortunately missed, owing to the breach of contract already referred to.

We can all of us, or nearly all of us, afford to laugh now at any inconveniences we have suffered; for of the eleven who landed at Galle nine have seen the eclipse, some of us perhaps as an eclipse has never been seen before. Unfortunately, to the regret of all, Mr. Abbay and Mr. Friswell, who were among the best prepared for doing good work, and were at a station at which everybody said cloudless weather was certain, found themselves on the 12th in a storm of cloud and mist, which obscured the sun for, I believe, the whole day. With this exception the telegrams from all the English parties have been sent regularly, while we have all been thankful to learn from the telegrams which Dr. Janssen and Colonel Tennant have had the great courtesy to send me, that they too saw the eclipse well, as also did Mr. Pogson, as I gather from the newspapers, but of course the details of their observations are still unknown to me. Hence, I can only give the facts observed by the party at Bekul and Poodocottah; Prof. Respighi, who observed at that station, having joined me at Pothanore, the station on the Madras Railway, at the foot of the hills which we ascended yesterday from 4 30 A.M. till 1 P.M.

But before I say a word about the observations themselves, it is incumbent upon me to express our deep obligations to the supreme Madras and Ceylon Governments for the magnificent manner in which they have aided us. Nothing could be more complete than the arrangements at Bekul made by the collector, Mr. Webster, and his assistant, Mr. McIvor, both for the work to be done and the comfort of those who had to do it. The same must be said for the Poodocottah party, where not only the collector, Mr. Whiteside, but the Rajah did everything in their power, the latter loading the observers with presents when they left. We have at present heard only of the discomforts of the Manantoddy party, and it is clear that here the local arrangements were in strong contrast to those elsewhere. The Ceylon parties, who parted from the main body at Galle, have doubtless been well looked after; as Captain Fyers, the Surveyor-General of the island, accompanied and aided them in their observations.

This brings us to another part of the arrangements. The Ceylon party had the unreserved use of the Government steamer the *Serendib*, to take them from Galle to their places of observation, Jaffna and Trincomalee, both on the coast, and the accommodation on board was perfect. The Indian parties proceeded to their various destinations, or the ports on the coast nearest to them, in the Admiral's flag-ship the *Glasgow*, which, however, could not remain to bring them back, a circumstance which has given rise to very considerable inconvenience and great risk for the instruments, which are now scattered all along the line, to be sent to the coast and from the coast to Bombay or Galle, as circumstances may determine. This of course was not to be helped, and we must hope for the best, especially as all the parties have done their utmost in superintending their repacking, and handing them over in perfect condition to the different Government officers who accompanied each party. Still, although it was not to be avoided, the withdrawal of the ship has been the unfortunate circumstance in the arrangements. Nothing could exceed the kindness of the Admiral, who vacated his own quarters to give us room, of Captain Jones, who took the warmest interest in our proceedings, and helped the arrangements greatly, and by the officers of the ship generally. Without the equal kindness of Mr. Webster at Bekul, the step from the Admiral's cabin into the jungle hut would have been a seven-league one.

As the mail, the first available one after the eclipse, leaves this place to-day, I must lose no more time in recording preliminaries. I will therefore at once state the general arrangements of the parties, and what I at present know of the observations. The stations and observers as finally arranged were as follows :—

Bekul—Analysing Spectroscope, Capt. Maclear and Mr. Pringle.
Polariscope, Dr. Thomson.
Photography, Mr. Davis.
Manantoddy—Analysing Spectroscope, Mr. Friswell.
Integrating Spectroscope, Mr. Abbay.
Poodocottah—Spectroscope, Professor Respighi.
Sketches of Corona, Mr. Holiday.
Jaffna—Integrating Spectroscope, Capt. Fyers and Mr. Ferguson.
Polariscope, Capt. Tupman and Mr. Lewis.
Photography, Captain Hogg.
Trincomalee—Spectroscope, Mr. Moseley.

Besides these observers, we had at Bekul the valuable assistance of General Selby, commanding the troops in Canara and Malabar (for whose help in supplying guards' tents, &c., the friends of Science cannot be too thankful), Colonel Farewell, Judge Walhouse, and others, in sketching the Corona. At all stations, of course, most precious help in various ways was given by all present who volunteered for the various duties, though some of them lost a sight of the eclipse in consequence. Among those who helped in this way at Bekul were Mr. McIvor, Mr. Pringle, Captain Bailey who timed the eclipse, Mr. Cherry, and Captain Christie, the Inspector of Police, whose presence there turned out to be of the most serious value, for the natives seeing in the eclipse the great Monster Rahoo devouring one of their most sacred divinities, not only howled and moaned in the most tremendous manner, but set fire to the grass between our telescopes and the sun to propitiate the representative of the infernal gods. Captain Christie with his posse of police stopped this sacrifice at the right moment, and no harm was done.

Now for the observations. Perhaps I may be permitted to begin with my own, as at the present moment I know most about them. I determined to limit my spectroscopic observations to the spectrum of a streamer, and to Young's stratum, thereby liberating a number of seconds which would enable me to determine the structure of the undoubted corona with a large refractor, to observe the whole phenomena with the naked eye, and through a train of prisms with neither telescope nor collimator, and finally with a Savart and biquartz. I found the 120 seconds gave me ample time for all this, but owing to a defect in the counterpoising of my large reflector, which disturbed the rate of my clock, I missed the observation of the bright line stratum (assuming its existence) at the first contact. At the last contact Mr. Pringle watched for it and saw no lines.

Having missed this, I next took my look at the corona. It was as beautiful as it is possible to imagine anything to be. Strangely weird and unearthly did it look—that strange sign in the heavens! What impressed me most about it, in my momentary glance, was its serenity. I don't know why I should have got such an idea, but get it I did. There was nothing awful about it, or the landscape generally, for the air was dry and there was not a cloud. Hence there were no ghastly effects, due generally to the monochromatic lights which chase each other over the gloomy earth, no yellow clouds, no seas of blood—the great Indian Ocean almost bathed our feet—no death-shadow cast on the faces of men. The whole eclipse was centred in the corona, and there it was, of the purest silvery whiteness. I did not want to see the prominences then, and I did not see them. I saw nothing but the star-like decoration, with its rays arranged almost symmetrically, three above and three below two dark spaces or rifts at the extremities of a horizontal diameter. The rays were built up of innumerable bright lines of different lengths, with more or less dark spaces between. Near the sun this structure was lost in the brightness of the central ring.

But from this exquisite sight I was compelled to tear myself after a second's gazing. I next tried the spectrum of a streamer above the point at which the sun had disappeared. I got a vivid hydrogen spectrum, with 1474 (I assume the point of this line from observation) slightly extended beyond it, but very faint throughout its length compared with what I had anticipated, and thickening downwards, like F. I was, however, astonished at the vividness of the C line, and of the continuous spectrum, for there was no prominence on the slit. I was above their habitat. The spectrum was undoubtedly the spectrum of glowing gas.

I next went to the polariscope, for which instrument I had got Mr. Becker to make me a very time-saving contrivance—a double eye-piece to a small telescope, one containing a Savart and the other a biquartz. In the Savart I saw lines vertical over everything—corona prominences, dark moon, and unoccupied sky. There was no mistake whatever about this observation, for I swept three times across and was astonished at their unbrokenness. I next tried the biquartz. In this I saw wedges, faintly coloured here and there; a yellowish one here, a brownish one there, with one of green on each side the junction, are all the colours I recollect. Then to the new attack—the simple train of prisms which, the readers of NATURE know, Professor Young had thought of as well as myself; its principle being that, in the case of particular rays given out by such a thing as the chromosphere, or the sodium vapour of a candle, we shall get images of the thing itself painted in that part of the spectrum which the ray inhabits, so to speak, we shall see an image for each ray, as if the prisms were not there. What I saw was four exquisite rings, with projections where the prominences were. In brightness, C came first, then F, then G, and last of all 1474! Further, the rings were nearly all the same thickness, certainly not more than 2′ high, and they were all enveloped in a line of impure continuous spectrum.

I then returned to the finder of my telescope, a $3\frac{3}{4}$ inch, and studied the structure of the corona and prominences. One of the five prominences was admirably placed in the middle of the field, and I inspected it well. I was not only charmed with what I saw, but delighted to find that the open-slit method is quite competent to show us prominences well without any eclipse. I felt as if I knew the thing before me well, had hundreds of times seen its exact equivalent as well in London, and went on to the structure of the corona. Scarcely had I done so, however, when the signal was given at which it had been arranged that I was to do this in the 6-inch Greenwich refractor. In this instrument, to which I rushed, for Captain Bailey had just told us that we had "*still* 30 seconds more"—which I heard mentally, though not with my ears, as "*only* 30 seconds more"—the structure of the corona was simply exquisite and strongly developed. I at once exclaimed, "like Orion!" Thousands of interlacing filaments varying in intensity were visible, in fact I saw an extension of the prominence-structure in cooler material. This died out somewhat suddenly some 5′ or 6′ from the sun, I could not determine the height precisely, and then there was nothing; the rays so definite to the eye had, I supposed, been drawn into nothingness by the power of the telescope; but the great fact was this, that close to the sun, and even for 5′ or 6′ away from the sun, there was nothing like a ray, or any trace of any radial structure whatever to be seen. While these observations were going on, the eclipse terminated for the others, but not for me. For nearly three minutes did the coronal structure impress itself on my retina, until at last it faded away in the rapidly increasing sunlight. I then returned to the Savart, and saw exactly what I had seen during the eclipse, the vertical lines were still visible!

Captain Maclear has promised to forward to you himself an account of his observations. I need only here therefore refer to their extreme value, adding what I should have stated before, that I saw the bright lines at the cusps, as he was so good as to draw my attention to them. I am however not prepared to say that they were visible through a large arc of retreating cusp.

Dr. Thomson confined his observations to the polariscope, using the Savart. He states that his observations were identical with my own.

Mr. Davis's photographic tent was below the cavalier in which our telescopes had been erected; and immediately after the observations I have recorded were over, I went down to see what success had attended his efforts. I was hailed when half-way there with the cheering intelligence "five fine photographs," and so they are, those taken at the beginning and end of the eclipse being wonderfully similar, with, I fancy, slight changes here and there; but on this point I speak with all reserve until they have been examined more carefully than the time at our disposal has permitted, and until they have been compared with those taken at Ootacamund, Avenashi, and, I hope, at Jaffna and Cape Sidmouth.

This exhausts the principal work done by the Bekul party, with the exception of the sketchers with General Selby at their head, who have recorded most marked changes in the form of the outer corona, and Mr. Webster, who was so good as to photograph the eclipse from a fort some eight miles away, with an ordinary camera, and obtained capital results.

Next a word about the Poodocottah, the other fortunate Indian party. Prof. Respighi has promised to send his results to you with this. About Mr. Holiday's labours I know nothing, except that he has obtained three sketches.

Concerning the Ceylon parties I give you a verbatim extract from the telegrams. From Jaffna: "Exceedingly strong radial polarisation, 35′ above the prominences; corona undoubtedly solar to that height, and very probably to height of 50′." From Trincomalee Mr. Moseley informs me that he carefully watched for Young's bright line stratum, and did not see it, and that 1474 was observed higher than the other line.

This is the sum total of the information which has at present reached me. It is clear there are discordances as well as agreements, the former being undoubtedly as valuable as the latter. It remains now to obtain particulars of all the observations of all the parties, before a final account can be rendered of the eclipsed sun of 1871. This, of course, will be a work of months; but if all goes well, I trust to obtain information shortly of the outlines of the work done by the Indian observers and M. Janssen, as I am now remaining in India for that purpose, and this I will communicate to NATURE by the earliest opportunity. In the meantime I hope the good people at home will think we have done our duty, and that all the members of the Government Eclipse Expedition of 1871 will soon be safely with them to give an account of their work.

J. NORMAN LOCKYER

Ootacamund, Dec. 19, 1871

CAPTAIN MACLEAR'S OBSERVATIONS

LONG before this, no doubt, you have heard of the success of the expedition, but you must be anxious to hear more of the details, and what the observations really were. When I last wrote to you from Point de Galle,* the expedition had arrived there on November 27th in the *Mirzapore*, and was about to proceed to the different stations selected. The Ceylon sections left on the 28th in the Colonial steamer *Serendib*, placed at our disposal by the Government. She was to leave Messrs. Moseley and Ferguson at Trincomalee, and then proceed to Jaffna, with Captain Fyers, R.E., Captain Tupman, R.M.A. and Mr. Moseley. We have since heard of the safe arrival of these gentlemen at their stations, and, by telegraph, of their successful observations on December 12th.

The Indian parties left Galle on the 28th in H.M.S. *Glasgow*, flag-ship of Admiral Cockburn, who kindly gave us his cabin accommodation. With a fair wind we made sail, and arrived at Beypore on the night of the 1st December. The next morning we landed Signor Respighi and Mr. Holiday to go by train to Poodocottah, and then we left for Cannanore where Messrs. Abbay and Friswell were disembarked to make their way across country to their station at Manantoddy. They had a troublesome and fatiguing journey to perform, with heavy instruments, which however they safely accomplished in three days, and we can only heartily regret that their labours were not recompensed by fine weather on the morning of the eclipse. At Cannanore we were fortunate enough to enlist the services of General Selby, commanding the troops; he came across to Bekul, and rendered good aid in making some valuable sketches of the corona during the eclipse.

We left Cannanore on the 3rd, and with the strong tide that sometimes runs up that coast, were only six hours in reaching Bekul. We found that Mr. McIvor, assistant collector, and Mr. Pringle, engineer, had arrived that morning from Mangalore, on the part of the Indian Government, had prepared the travellers' bungalow for our reception, and had cleared the keep of an old fort erected by Tippoo which would make a capital observatory. The bay is open and shelving, but there

* See NATURE, vol. v. p. 163.

was little surf, and on the morning of the 4th, instruments and all were safely landed and carried up to the fort.

Our voyage in the *Glasgow* had been uneventful; but I cannot take leave of her without speaking of the kindness and assistance we received from Captain Jones and all on board, and we were truly sorry that the duties of the station did not allow them to remain and give us that aid, which, with the interest that all took in the work, would have been so invaluable.

Bekul is an out-of-the-way place, twenty-five miles from Mangalore, from which place all our supplies had to be carried on the backs of coolies; this did not, however, prevent several gentlemen, interested in our proceedings, coming out to join us.

Our party consisted of four who came out from England, viz., Mr. Lockyer, Dr. Thomson, Mr. Davis, and Commander Maclear, besides Messrs. McIvor and Pringle, to whose foresight and care we are very much indebted for our success. It was further strengthened by Mr. Webster, collector at Mangalore, who took some valuable photographs during the eclipse, by General Selby from Cannanore, and several others, making our numbers up altogether to eighteen. Our bungalow was about a mile from the fort, of which the highest bastion in the inner rampart had been selected to mount the equatorials; it was in a most commanding position about eighty feet above the sea, and overlooking a vast extent of country. Just below us, in a well-sheltered spot, Mr. Davis fixed his camera and dark chamber.

The day of our landing the heat of the sun was terrible, and we had to wait till the cool of the afternoon before we could proceed to work. That night, however, a great advance was made, the bases of the equatorials were up, and all ready for the tubes, and a "chuppa," or awning of palm leaves erected to protect them from the night dews and midday sun. The next seven days were employed in getting our instruments perfectly adjusted and in practising with them. The weather left nothing to be desired, except that the sun would take his revenge out beforehand and strike down with such force as to render it impossible to work in the middle of the day. Only one morning was cloudy, and then not to an extent that would have interfered with observations. At night the stars shone with great brilliancy, and we had great delight in observing the clusters and nebulæ, pity we could not have remained longer to make spectroscopic observations of the latter in such a clear atmosphere.

The morning of the 12th dawned bright and clear, only a few small clouds to be seen near the western horizon, a light breeze from the N.E. All were early at their stations watching anxiously the appearance of the sun, which rose over the distant hills about half-an-hour before the commencement of the eclipse. But now I shall speak only of my own observations; Mr. Lockyer has already given the account of those made by himself.

The instrument I used was a double equatorial of two 6-inch refractors mounted on the same base, one at either end of the declination axis. To one was attached a 6-prism spectroscope from Kew, lent by Mr. Spottiswoode, of great dispersive power. To the other was fixed a spindle bar, carrying an erecting eye-piece, and a 7-prism direct vision spectroscope, either of which could be swung at pleasure into the focus of the object glass; the two tubes had been carefully made parallel, so that the same object was viewed in both telescopes. The 6-prism was worked nearly the whole of the time by myself, and the direct vision by Mr. Pringle, who had practised with it constantly during the last few days. I add the observations made by him. At the commencement of the eclipse the slit of the 6-prism was placed tangential to the point of contact, that of the direct vision radial, width such that the absorption lines were very distinct, but not too fine. No change was observed from the ordinary solar spectrum. Keeping the slit for the next quarter of an hour tangential to the northern cusp, C was very bright the whole length; F bright, but thin. The slit was then placed radial to the cusp, and four bright lines near C (besides C itself) became visible, one on the direct side within 10 units Kirchhoff, and three on the red side within 20 units, the length of all five varying, but not together the average being about $\frac{1}{8}$ the height the visible spectrum.

At 6h. 51m. M.T., twenty-five minutes after contact, on a large prominence, C lengthened to half height of spectrum; nine minutes afterwards cusp was at another prominence, the positions of these must have been about N. 13°, and nearly north.

At 7h. 8m. M.T. I watched with the direct vision radial and, besides the Hyd. and "near D" lines, observed another bright line a little more refrangible than the air band between *b* and F. At 1830 Kirchhoff it was very faint, and soon disappeared; soon after this I saw F line double about the same height as usual, $\frac{1}{8}$ spectrum.

At 7h. 23m. M.T., having returned to the 6-prism radial to the cusp, I observed the Hyd. D, E and *b* very plain; several lines then began to come into view, as near as I could judge all the iron lines from halfway between D and E to beyond *b*. These kept on brightening and more lines coming in. I called Mr. Lockyer to look at the phenomenon, and we watched it together for two or three minutes until it became time to take position to observe totality. During these two or three minutes the cusp must have passed from about N. 38° E. to N. 70° E. or further, and the lines were not lost sight of till I moved the telescope and placed the slit tangential to the point where the light would disappear, keeping it there with R.A. movement. On looking through the spectroscope the field was full of bright lines, the light just enough to let me distinguish the positions from the well-known solar lines.

As totality came on the light decreased, and the lines increased exceedingly, rapidly in number and brightness, until it seemed as if every line in the solar spectrum was reversed; then they vanished, not instantly, but so quickly that I could not make out the order of their going, except that the Hyd. D, *b*, and some others between D and *b*, remained last. Then they vanished, and all was darkness. I then unclamped, and swept out right and left, but saw nothing; then went to the direct vision, but saw nothing; placed the telescope on the moon's limb by the eye-piece, then put in the spectroscope, but the light was not sufficient to show any spectrum; pointed the telescope carefully, first on the dark moon, and then on a bright part of the corona, but no spectrum. I then looked at the corona with the naked eye, saw a bright glory around the moon, stellar form, six-pointed, something like the nimbus painted round a saint's head, extending to a diameter and a half. Looked through the finder, and saw the same form, but very much reduced in size and brilliancy; then examined with the 6in. and eye-piece, and saw nothing but a bright glow round the moon, not much more than the height of the big prominence plainly visible in the S.E. quarter. The last thirty seconds had now arrived, and, as previously arranged, Mr. Lockyer took my place at the 6in., while I again looked through the 6-prism spectroscope to record anything that might be visible, but I saw nothing. As the spectroscope was not on the sun's limb at the re-appearance of the light, I cannot state what took place.

During the remainder of the partial eclipse I watched the northern cusp as the moon uncovered the sun, and several times I saw distinctly the four bright lines near C; but saw nothing else worth recording.

The colour of the corona appeared to me a light pinkish white, very brilliant. I saw no streamers. The rest of the sky and everything around had a bluish tinge.

I will now give an extract from Mr. Pringle's report. He was observing with the direct-vision spectroscope attached to the other 6-inch telescope, and with myself watching the northern cusp, slit radial:

"Until 6h. 47m. (mean time) bright lines C, near D, and F, of uniform brightness, and varying but slightly from normal height. At that time F brightened, C remained bright, line near D very faint. At 6h. 54m. all the lines lengthened to some four or five times their normal height, showing a prominence at the cusp. For the next ten minutes lines varying but little. At 7h. 4m. a large prominence at cusp; bright lines lengthening some eight or nine times their normal height. At 7h. 4m. 30s. a bright line appeared on the more refrangible side of F, and close to it, F lengthening considerably, and bending towards the red. All the before-mentioned lines were now bright, F longer than the rest, and remaining bent, the line near it being one-third its length. At 7h. 13m. observed three bright lines at *b*, visible only at the extreme point of the cusp. Half a minute before totality, turned the slit tangential; but the slit not being exactly at the same place as that of Commander Maclear's, both refractors working by the same slow-motion screw [this was owing to the sway of the bars carrying the spectroscope when it was being turned.—J. P. M.] I failed to obtain any results at the moment of totality. I then observed at the 6-prism just quitted by Commander Maclear, whilst that gentleman, observing at the direct-vision spectroscope, swept out from the sun on one side, then brought the finder on the dark moon, and thence swept out from the sun on the opposite side. During this time nothing whatever was visible in the spectroscope. I next observed with the naked eye: corona appeared radial, of a purplish white colour, brightest near the body of the moon; no very long rays perceptible. On holding the head sideways, rays of corona remained permanent, showing none to be due to defect of vision. Next observed corona through $2\frac{1}{2}''$ finder of refractor. Structure well-defined, wavy, nebulous, permanent. Remarked a curiously-curved portion of corona, divided by a partial rift from an oblique ray. I should imagine the corona to extend about $7'$ beyond the sun, but did not accurately estimate the distance whilst observing. When thirty seconds of totality remained, I went to finder of equatorial reflector; structure of corona not so apparent with higher power. Several prominences visible; one of large size, structure similar to that of corona. At about twelve seconds before end of totality, a perceptible brightening along the edge of the moon on the side of appearance; a few seconds before end of totality, I went to one prism corona spectroscope attached to $7\frac{1}{4}''$ reflector. At the end of totality a considerable number of bright lines flashed in (what proportion of the whole I cannot say, perhaps a third). The line near D noticeably bright; continuous spectrum faintly visible a moment before the sun's limb showed. After totality observed at finder, the summit of a large prominence opposite the point of sun's re-appearance visible for several seconds after totality."

During the afternoon I tried to make an accurate sketch of the prominences on the sun's disc, but clouds came on, and I was prevented. It was not worth while keeping the instruments up another day for the purpose, so we commenced, and in two days they were safely packed for Bombay.

The rumours that our presence gave rise to among the natives were very amusing. First we heard that part of the sun was about to fall, and the wise men had come to the East to prevent it. Then when the formidable-looking instruments were seen mounted on the fort, they thought there was a war, and we were engineers going to put the fort in order to prevent a landing. This was strengthened by the fact that the *Glasgow* practised at a target before returning to Ceylon. This gave place to a flood about to descend, and all the Europeans were coming to the high ground to escape it.

When the eclipse commenced the usual shouting and beating of tom-toms went on, but a cordon of police prevented an invasion of the Observatory, and only a confused noise from below reached us.

J. P. MACLEAR

S.S. *Indus*, January 6, 1872

MORSE ON TEREBRATULINA

The Early Stages of Terebratulina septentrionalis. By Edward S. Morse, Ph. D. (Boston Society of Natural History, vol. ii.)

MR. MORSE is one of the band of New England naturalists who have lately been making themselves known to us through that excellent periodical the *American Naturalist*, and who have shown themselves determined to take advantage of the opportunities offered to them by the presence on their sea-board of such zoological treasures as Limulus and Lingula. Mr. Morse obtained Terebratulina in abundance in the harbour of Eastport, Maine, and gives in this paper an account of the change in the form of the shell and the "arms" during development of this Brachiopod from a scarcely visible speck onwards. The changes are illustrated in two plates containing outline figures, and as far as Mr. Morse has observed consist firstly in the passage of the shell from a flat and shorter form to the elongated and convex shape with which we are familiar. Further, the arms were found to commence as a series of ciliated tentacles placed around the mouth, and as nearly as possible identical with the lophophor of such a Polyzoon as *Pedicellina*. At first but six of these tentacles are seen; these increase in number, whilst the lophophor takes on its horse-shoe shape; and finally by the development of the free ends of the two sides of the horse-shoe the great Brachiopodian arms are produced. This is very interesting, and confirms *a priori* notions. At the same time we must dissent from the stress which Mr. Morse lays on the affinities of structure of Brachiopoda and Polyzoa, in so far as he wishes to separate these two widely from the Mollusca, and join them to a group which he calls Vermes. The Vermes have never been accurately defined, and are in fact at present, as Carl Gegenbaur (whom Mr. Morse cites) fully admits, one of those classficatory lumber-rooms, which are so convenient from time to time in the progress of zoological science. Whilst we fully admit the close affinities of the Polyzoa and the Brachiopoda—now long recognised by all zoologists—we cannot overlook the very strong affinities of these to the true Mollusca. Even a hasty study of the embryology of the Mollusca is sufficient to bring under one's eyes larval forms of various classes bearing many of the characteristics of the Polyzoa on the one hand, and of certain Vermes on the other. The early condition of the gill-plates in some Lamellibranchs is only to be compared to the tentacula of the Molluscoidan lophophor, though presenting so large a shifting in some relations. Rather than detach the Molluscoida (with regard to the Tunicata there are a variety of new facts and considerations which require long discussion) from the Mollusca to place them in the lumber-room Vermes—we should prefer to put the whole of the Mollusca along with them there—a proceeding at present useless, but which would express a truth which Mr. Morse does not seem to admit, though it is indicated by Gegenbaur, and accepted also by Huxley, namely, that there are close genetic ties between the group Mollusca (including Molluscoida), and certain so-called Vermes, such as the Turbellaria, Archi-annelida, &c.

In a paper published prior to this, Mr. Morse has spoken of the Brachiopoda as a division of Annelida, on the ground of certain resemblances between Lingula and Annelids. We are not sure whether Mr. Morse adheres to this startling proposition, or whether it was due to the intensity of the impressions produced by his study of living Lingulæ, which must have been exceedingly interesting. By the way, we may mention that Semper has also studied living Lingula. That there is a fundamental community of organisation between Lingula and Annelids we are, as stated above, not indisposed to believe, but that this can be expressed advantageously by making the Brachiopoda a division of Annelida, or that such a classification would be anything more than reactionary exaggeration, we cannot for a moment suppose. Mr. Morse attaches importance in this regard to the setæ of Lingula, and equal or perhaps more importance to the red colour of the blood. The discovery of red-coloured blood in Lingula is interesting, because in all probability it is due, as in vertebrates and all other causes where it is really red, to the presence of hæmoglobin, and is another instance of the exceptional appearance of this chemical principle in the blood of an animal whose nearest congeners do not possess it. We should be very glad of confirmation with the spectroscope of the supposed existence of hæmoglobin in the blood of Lingula. But how can Mr. Morse suppose that this red blood, or hæmoglobin-bearing blood, is a character of the slightest classificatory importance? A great number of Annelids do not possess the vascular system at all, which in others carries this red blood; in some the fluid in that vascular system is coloured green by chlorocruorin, in others the hæmoglobin is present in the perivisceral fluid, which is in most Annelida colourless. Certain Mollusca have blood coloured red by hæmoglobin (Planorbis) as deeply and brightly as that of any lob-worm, so again have some Crustacea and Insect larvæ. The presence or absence therefore of hæmoglobin in the blood of Lingula is a matter of complete indifference as far as the relations of that animal to the Annelida are concerned.

We are much interested by a reference in Mr. Morse's paper on Terebratulina to some observations which he has made on the development of Lingula, observations which we hope before long to see published. From these he states that he is led to believe that the supposed Discina larva figured by Fritz Müller might equally as well be that of a Lingula. Some further information about this remarkable larval form will be very welcome.

Mr. Morse apologises for the undetailed character of his drawings, and for the absence of information in his paper upon the development of Terebratulina *ab ovo*—a great desideratum—by the fact that when he went to Eastport to study the development of Terebratulina he had a microscope with him which he found to be utterly inadequate to the purpose. Since this is an error which is easily remedied, we trust that Mr. Morse will soon return to the attack, if he has not yet already done so, duly armed.

E. RAY LANKESTER

LETTERS TO THE EDITOR

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

The Solar Eclipse

IT does not happen more than once in a lifetime to see such a glorious and magnificent sight as that from which I have just returned; that is, the total eclipse of the sun. I have seen many eclipses before, but never anything to equal this. I was engaged to go with the Morgans to the top of the hill to see it. Got up at six, and found it a lovely morning; rode up to Morgan's, about half a mile, carrying with me glasses, smoked glass, and sun hat. Got there before seven, and found eclipse already begun. Got our two mirrors and watched the hole in the sun grow bigger and bigger. It began from the top, and we all went off to the highest point on the hill, from whence we could see all Ooly and the mountains round. When the eclipse got so far, the cold on the mountain grew much greater, the grass was so wet that no one's boots kept it out, the feet and hands grew cold, and with your back to the sun the light over the country was like twilight, or the earliest dawn. Gradually the lower streak got thinner and thinner, until at last there shone a light like the famous lime-light, and in a moment or two that went out and the sun was totally concealed; many stars were visible, the whole country looked dark—that is, half dark, like moonlight—the crows stopped cawing, and for two minutes and a half the total eclipse lasted, a sight I shall never forget, and then the lime-light again appeared at the bottom rim of the sun, and gradually more and more of him appeared, the crows began again at once, and the cocks began to crow, the shadow now was inverted, and by degrees got smaller, until at nine o'clock the eclipse was over. I cannot but suppose that the scientific men must have had grand opportunities of observation, and that to-day's pencil will carry home many a description. Anything more beautiful, more sublime, or more perfect, it would be impossible to conceive.

Upway House, Mercara, Coorg, Ooly, Dec. 12, 1871 R. N. TAYLOR

The Rigidity of the Earth

I HAVE been urged from several quarters to defend my argument for the rigidity of the earth against attacks which are supposed to have been made upon it. It has, in fact, never been attacked to my knowledge, and I feel under no obligation to defend it. There is, I believe, a general impression that grave objections to it have been raised by M Delaunay, and it seems that even in this country some geological writers and teachers, in their reluctance to abandon the hypothesis of a thin solid crust, enclosing a wholly liquid mass, hastily concluded that all dynamical arguments against it had been utterly overthrown by Delaunay.

In point of fact Delaunay made no reference at all to the tidal argument, and clearly was unaware that I had brought it forward when he made his communication on the "Hypothesis of the interior fluidity of the terrestrial globe,"* to the French Academy, three years and a half ago, objecting to Hopkins's argument founded on precession and nutation, and merely quoting me as having expressed acquiescence. On this subject I say nothing at present, except that ten years ago, before I expressed (in my first communication of the tidal argument to the Royal Society) my assent to Hopkins's argument from precession and nutation, I had thought of the objection to this argument since brought forward by Delaunay, and had convinced myself of its invalidity. But I hope to be able on some future occasion to return to the subject, and to prove that any degree of viscosity, acting in the manner and to the effect described by Delaunay, must in an extremely short time abolish the distinction between summer and winter. My reason for writing to you at present is that I see in Mr. Scrope's beautiful book on Volcanoes (just published as a second edition) a sentence ("Prefatory Remarks," page 24), written on the supposition that the tidal argument had been brought forward for the first time at the recent meeting of the British Association in Edinburgh. I therefore take the liberty of suggesting to you that a reprint of the short abstract of my tidal argument, which appeared in the Proceedings of the Royal Society, for May 16, 1862, might not be inappropriate to your columns. I ought, however, to inform you that the tidal argument was carefully re-stated in the first volume of the treatise on Natural Philosophy, by Prof. Tait and myself, published in 1867, but as the volume is at present out of print, you may not consider this objection fatal to my proposal.

Glasgow University, Jan. 9 WILLIAM THOMSON

Abstract of Paper on the Rigidity of the Earth, by Prof. Sir William Thomson, F.R.S., received April 14, 1862

The author proves that unless the solid substance of the earth be on the whole of extremely rigid material, more rigid for instance than steel, it must yield under the tide-generating influence of sun and moon to such an extent as to very sensibly diminish the actual phenomena of the tides, and of precession and nutation. Results of a mathematical theory of the deformation of elastic spheroids, to be communicated to the Royal Society † on an early occasion, are used to illustrate this subject. For instance, it is shown that a homogeneous incompressible elastic spheroid of the same mass and volume as the earth, would, if of the same rigidity as glass, yield about $\frac{2}{5}$, or if of the same rigidity as steel about $\frac{1}{3}$ of the extent that a perfectly fluid globe of the same density would yield to the lunar and solar tide-generating influence. The actual phenomena of tides (that is, the relative motions of a comparatively light liquid flowing over the outer surface of the solid substance of the earth), and the amounts of precession and nutation, would in one case be only $\frac{3}{5}$ and in the other $\frac{2}{3}$ of the amounts which a perfectly rigid spheroid of the same dimensions, of the same figure, the same homogeneous density, would exhibit in the same circumstances. The close agreement with the results of observation presented by the theory of precession and nutation, always hitherto worked out on the supposition that the solid parts of the earth are perfectly rigid, renders it scarcely possible to admit that there can be any such discrepancy between them as 3 to 5, and therefore almost necessary to conclude that the earth is on the whole much more rigid than steel. But to make an accurate comparison between theory and observation, as to precession, it is necessary to know the absolute amount of the moment of inertia about some diameter; and from this we are prevented by the ignorance in which we must always be as to the actual law of density in the interior. Hence the author anticipates that the actual deformation of the solid earth by the lunar and solar influence may be more decisively tested by observing the lunar fortnightly and the solar half-yearly tides.* These tides, it may be supposed, will follow very closely the "equilibrium theory" of Daniel Bernouilli for all oceanic stations, and the author suggests Iceland and Teneriffe as two stations well adapted for the differential observations that would be required.

The earth's upper crust is possibly on the whole as rigid as glass, more probably less than more. But even the imperfect data for judging referred to above render it certain that the *earth as a whole must be far more rigid than glass*, and probably even more rigid than steel. Hence the interior must be on the whole more rigid, probably many times more rigid, than the upper crust. This is just what, if the whole interior of the earth is solid, might be expected when the enormous pressure in the interior is considered, but it is utterly inconsistent with the hypothesis held by so many geologists that the earth is a mass of melted matter enclosed in a solid shell of only from 30 to 100 miles' thickness. Hence the investigations now brought forward confirm the conclusions arrived at by Mr. Hopkins, that the solid crust of the earth cannot be less than 800 miles thick. The author indeed believes it to be extremely improbable that any crust thinner than 2,000 or 2,500 could maintain its figure with sufficient rigidity against the tide-generating forces of the sun and moon, to allow the phenomena of the ocean tides and of precession and nutation to be as they are.

Extract from Thomson and Tait's "Natural Philosophy."

"§ 832. . . . . All dynamical investigations (whether "static or kinetic) of tidal phenomena, and of precession and "nutation, hitherto published, with the exception referred to "below, have assumed that the outer surface of the solid earth "is absolutely unyielding. A few years ago, for the first time, "the question was raised: Does the earth retain its figure with "practically perfect rigidity, or does it yield sensibly to the de- "forming tendency of the moon's and sun's attractions on its "upper strata and interior mass? It must yield to *some* extent, "as no substance is infinitely rigid. But whether these solid "tides are sufficient to be discoverable by any kind of observa- "tion, direct or indirect, has not yet been ascertained. The "negative result of attempts to trace their influence on ocean "and lake tides, as hitherto observed, and on precession and "nutation, suffices, as we shall see, to disprove the hypothesis "hitherto so prevalent, that we live on a mere thin shell of solid "substance enclosing a fluid mass of melted rocks or metals, "and proves, on the contrary, that the earth is much more rigid "than any of the rocks that constitute its upper crust."

"§ 833. The character of the deforming influence will be "understood readily by considering that if the whole earth were "perfectly fluid, its bounding surface would coincide with an "equipotential surface relatively to the attraction of its own "mass, the centrifugal force of its rotation and the tide generat- "ing resultant of the moon's and sun's forces, and their kinetic "reactions. Thus there would be the full equilibrium lunar and "solar tides; of $2\frac{1}{2}$ times the amount of the disturbing deviation "of level if the fluid were homogeneous, or of nearly twice "this amount if it were heterogeneous with Laplace's hypotheti- "cal law of increasing density. If now a very thin layer of "lighter liquid were added, this layer would rest covering the "previous bounding surface to very nearly equal depth all round, "and would simply rise and fall with that surface, showing only "infinitesimal variations in its own depth, under tidal influences. "Hence had the solid part of the earth so little rigidity as to "allow it to yield in its own figure very nearly as much as if it were "fluid, there would be very nearly nothing of what we call tides "—that is to say, rise and fall of the sea relatively to the land; "but sea and land together would rise and fall a few feet every

* *Comptes Rendus* for July 13, 1868.

† Communicated August 22, 1862, and read November 27, of same year "Dynamical Problems regarding Elastic Spheroidal Shells and Spheroids of incompressible Liquids."

* High tide, as far as the influence of either body is concerned, is produced at the poles, and low average water at the equator, when its declination, whether north or south, is greatest, and low water at the poles and high water at the equator, when the disturbing body crosses the plane of the equator.

"twelve lunar hours. This would, as we shall see, be the case "if the geological hypothesis of a thin crust were true. The "actual phenomena of tides, therefore, give a secure contradiction "to that hypothesis. We shall see, indeed, presently, that even "a continuous solid globe of the same mass and diameter as the "earth, would, if homogeneous and of the same rigidity as glass "or as steel, yield in its shape to the tidal influences three-fifths "as much or one-third as much as a perfectly fluid globe; and "further, it will be proved that the effect of such yielding in the "solid, according as its supposed rigidity is that of glass or that "of steel, would be to reduce the tides to about $\frac{2}{5}$ or $\frac{2}{3}$ of what "they would be if the rigidity were infinite."

"§ 834. To prove this, and to illustrate this question of elastic "tides in the solid earth, we shall work out explicitly the solu- "tion of the general problem of § 696 for the case of a homo- "geneous elastic solid sphere exposed to no surface traction; "but deformed infinitesimally by an equilibrating system of forces "acting *bodily* through the interior, which we shall ultimately "make to agree with the tide generating influence of the moon "and sun. . . . ."

"§ 847. We intend in our second volume to give a dynamical "investigation of precession and nutation, in which it will be "proved that the earth's elastic yielding influences these pheno- "mena in the same proportionate degree as it influences the "tides. We have seen already that the only datum wanted for a "comparison between their observed amounts and their theoreti- "cal amounts on the hypothesis of perfect rigidity, to an accuracy "of within one per cent., is a knowledge of the earth's moment "of inertia about any diameter within one per cent. We have "seen that the best theoretical estimates of precession hitherto "made, are in remarkable acordance with the observed amount. "But it is not at all improbable that better founded estimates of "the earth's moment of inertia, and more accurate knowledge than "we yet have from observation, of the harmonic of the second "degree in the expression of external gravity, may show that "the true amount of precession (which is known at present with "extreme accuracy) is somewhat smaller than it would be if "the rigidity were infinite. Such a discrepancy, if genuine, "could only be explained by some small amount of deformation "experienced by the solid parts of the earth under lunar and "solar influence. The agreement between theory on the hypo- "thesis of perfect rigidity, and observation as to precession and "nutation, are, however, on the whole so close as to allow us to "infer that the earth's elastic yielding to the disturbing influence "of the sun and moon is very small—much smaller, for in- "stance, than it would be if its effective rigidity were no more "than the rigidity of steel."

"§ 848. It is interesting to remark that the popular geological "hypothesis, that the earth is a thin shell of solid material, "having a hollow space within it filled with liquid, involves "two effects of deviation from perfect rigidity, which could in- "fluence in opposite ways the amount of precession. The com- "paratively easy yielding of the shell must, as we shall see in "our second volume, render the effective moving couple, due "to sun and moon, much smaller than it would be if the whole "interior were solid, and on this account must tend to diminish "the amount of precession and nutation. But the effective "moment of inertia of a thin solid shell containing fluid, "whether homogeneous or heterogeneous, in its interior, would "be much less than that of the whole mass if solid throughout; "and the tendency would be to much greater amounts of pre- "cession and nutation on this account. It seems excessively "improbable that the defect of moment of inertia due to fluid in "the earth's interior, should bear at all approximately the same "ratio to the whole moment of inertia, that the actual elastic "yielding bears to the perfectly easy yielding which would take "place if the earth were quite fluid. But we must either admit "this supposition, improbable as it seems, or conclude (from "the close agreement of precession and nutation with what "they would be if the earth were perfectly rigid) that the defect "of moment of inertia, owing to fluid in the interior, is small in "comparison with the whole amount of inertia of the earth "about any diameter; and that the deformation experienced by "the earth from lunar and solar influence is small in compari- "son with what it would be if the earth were perfectly fluid. "It is, however, certain that there is some fluid matter in the "interior of the earth; witness eruptions of lava from vol- "canoes. But this is probably quite local, as has been urged "by Mr. Hopkins, who first adduced the phenomena of pre- "cession and nutation to disprove the hypothesis that the solid "part of the earth's mass is merely a thin shell."

The Kiltorcan Fossils

I HAVE just seen Mr. Carruthers' letter in your number of January 4th, to which I beg leave to reply.

In this communication it now appears that Mr. Carruthers' former remarks in the discussion upon Prof. Heer's paper were intended as a personal attack upon me; as he now states that on me alone rests the credit of misleading Prof. Heer by my erroneous determination of the Kiltorcan plant.

I have no hesitation in acknowledging to having referred the Kiltorcan plant in question to *Sagenaria veltheimiana*, and I think it very possible I may even now be correct. I will however now state the reason for my afterwards adopting Professor Schimper's name in preference. When that gentleman was in Ireland he spent some time in the examination of the Kiltorcan fossils, and did not then object to my determination of the species; it was afterwards, on my sending him a collection, that his further study of these fossils and comparison with the original species (of which I had only seen figures) enabled him to announce to me what he believed to be the distinctive characters in relation to the fruit which accompanied it, of those I had named *Sagenaria veltheimiana;* these fossils in his letter to me he referred to *Sagenaria*, and afterwards in his work "Traité Paléontologie Végétale," to *Knorria* under the name of *K. bailyana*. In the meantime I had read my report on these fossils at the British Association, and naturally adopted the generic name first applied to it by Prof. Schimper, which I afterwards corrected to *Knorria*, on his authority, in my "Figures of British Fossils," as Mr. Carruthers states.

In my letter to Professor Heer (June 1870) accompanying the specimens which I was requested to send him for his comparison with the Bear Island flora, I named those from Kiltorcan *Sagenaria bailyana* in accordance with Prof. Schimper's determination, whilst others from Tallow Bridge, co. Waterford, which he specially wished to see, I still referred to *S. veltheimiana*. I made him aware of Prof. Schimper's views on these plants, stating distinctly that they were originally referred by me to *S. veltheimiana*, but that Prof. Schimper, in consequence of his being enabled to compare the fruit accompanying it with that of the true *S. veltheimiana*, had arrived at the conclusion that it could not be that species, and therefore he had named it as a distinct one. Under these circumstances I cannot see how Mr. Carruthers can charge me with misleading Prof. Heer, who had the whole facts, with examples of the specimens from both localities, to draw his own conclusions from; with his acknowledged powers of discrimination, surely he was fully competent to judge for himself as to their correct identity.

The amount of Mr. Carruthers' knowledge on the subject about which he writes, is evidenced from his intimation that the fossil figured by me in the explanation to Sheet 187, &c., of the Irish Survey maps, is from Kiltorcan (co. Kilkenny), whereas it was sketched by me, on the spot, at Tallow Bridge (co. Waterford), where the section exposed exhibited a profusion of these plants in various conditions and stages of growth. The character of the rock in which they occur is totally different from that at Kiltorcan, the former being a grey shale, corresponding with the Lower Carboniferous shales, the latter a fine-grained greenish sandstone; neither has any of the associated Kiltorcan fossils, including the fish which are of typical Devonian or Old Red sandstone genera, ever been found at Tallow Bridge. I did however state in this memoir my belief that the *S. veltheimiana*, as identified by me at Tallow Bridge, was similar to the Kiltorcan plant in question, and also that it corresponded with the so-called *Knorria* of the Marwood beds, N. Devon.

With reference to Mr. Carruthers' announcement that *Sagenaria veltheimiana* is a "coal measure plant," I may remark that it is a particularly abundant fossil, occurring in various conditions, but seldom, if ever, met with in the typical coal series of Great Britain; I have identified it from the sandstones of the lower coal measures in the North of Ireland, as well as at various localities in the Lower Carboniferous shales of the Counties of Cork and Kerry. On the Continent, especially in Germany, it appears to be still more universal, and has been recorded under various names by fossil botanists, as Dr. H. R. Goeppert, in his "Fossile Flora der Silurischen der Devonischen," &c., mentions more than twenty synonyms for this species; moreover the same author states its occurrence to be "In der Kulmgrauwacke, dem Kohlenkalke und in der *jungsten Grauwacke*." Dr. F. Unger and Dr. H. B. Geinitz, the latter of whom personally inspected the collections from Kiltorcan and Tallow Bridge, also mentions similar lower geological horizons at which it occurs; and Dr.

W. P. Schimper in the work before cited places it in Lepidodendron as a characteristic plant "des formations houillières inférieures (grauwacke culm) correspondant au calcaire carbonifère." It is therefore evidently more characteristic of the Lowest Carboniferous than of the coal measures; the older of these formations being considered by Sir Charles Lyell "as equivalents of the Lower Carboniferous, and were even formerly referred to the Devonian group."

I believe enough has now been said to show the part I took in misleading this eminent Professor, and I will leave those interested to judge between the merits of Mr. Carruthers' or Prof. Heers' classification, but in conclusion I must request to be allowed to state that prior to this gentleman's accusation against me, he made me a proposal to help him out of his controversy with Prof. Heer, and to "join him in a memoir to describe and figure the valuable materials I had collected;" this I had to decline, because it would not only have interfered with my official duties, but might also have drawn me into a discussion in which I had no interest, besides the probability of its committing me to what may prove to be erroneous opinions.

Dublin, Jan. 10 WM. HELLIER BAILY

Circumpolar Lands

IN NATURE of December 28 there is an interesting letter endeavouring to show that the land everywhere about the North Pole down to lat. 57° is rising. We know less about the South Polar regions, but there are active volcanoes in the Antarctic Continent, and Darwin has shown in his work on volcanic islands that the land and sea-bottom are rising. This appears to be at least a remarkable coincidence.

The earth must be cooling by the escape of the central heat in volcanic eruptions and hot springs, and by slow upward conduction through the strata. As it cools it must contract. Can any mathematical reason be assigned why the contraction should be least in the direction of the polar diameter? This would account for the rising of the land at the poles. J. J. MURPHY

English Rainfall

IN NATURE of the 11th inst. your reviewer, "J. K. L." (p. 201), makes a mistake in stating that the greatest English rainfall takes place at Cockley Bridge, Seathwaite. The greatest fall takes place at the Stye and on the north side of Stye Head, Seathwaite, Borrowdale; whereas the Cockley Bridge named by your reviewer is Seathwaite, Valley of the Duddon, and many miles from the place of greatest fall. He has evidently confounded the two Seathwaites. A reference to Mr. J. G. Symons' annual rainfall returns willl show that the Seathwaite named is the one in Borrowdale. G. V. VERNON

Wanted, a Government Analyst

I AM a grocer in a small way in a country place, so that I retail almost all that comes under the name of food; and I am very desirous that all should be unadulterated and worth its price, as far as a fair profit will allow. But how am I to ensure this, even supposing I possessed the requisite knowledge and appliances? Time would be wanting to carry out a systematic analysis, and the ordinary "rule of thumb" tests are not a match for the increasing cleverness of "manufacturing chemists." It only remains to send samples to some known food analyst; but here the expense becomes a barrier, when the dealings dependent on it are on a small scale. Is there (or, if not, ought there not to be?) some Government functionary to whom samples could be sent for testing, at a charge to just cover necessary expenses? After reading a very sad article on "Artificial Milk," in your paper of Dec. 15, I feel emboldened to ask whether, either of yourself or through any of your readers, you could assist me to render practical a feeling I am sure you must sympathise with. For obvious reasons, I ask you to receive in strict confidence the name and address I have given to show the genuine nature of my application. GROCER

Earthquakes in Celebes

I WISH to contribute to the list of earthquakes and eruptions in your journal the following, all of which I have witnessed:—

1871

May 1 . Eruption of a volcano on the Island Camiguin, south of the Philippine Islands.

June 13 . Earthquake in Kakas, at the Lake of Tondano in Minahassa, North Celebes, $7\frac{1}{2}$ P.M. This shock was at the same time felt throughout Minahassa.

July 15 . Earthquake at Gorontalo, North Celebes, Bay of Tomini, $12\frac{1}{2}$ P.M. and $10\frac{1}{2}$ P.M.

,, 19 . Earthquake at Gorontalo, $12\frac{1}{4}$ A.M., heavy.

August 7. Eruption of the volcano of Ternate. This eruption had not ended August 23. Most of the inhabitants of Ternate ran away. Stones and ashes were thrown as far as Halmaheira.

,, 19. Earthquake at Gorontalo, 5 A.M.

,, 25. Seaquake at Gorontalo, 3 P.M.

,, 31. Earthquake at Gorontalo, 1 P.M., very strong, vertically.

In the month of August there were at Gorontalo a series of earthquakes, all of which I did not notice in my diary, some of them very severe, shocks so severe and numerous have not been experienced for years at that place. I do not doubt that they were in connection with the long-continued eruption of the volcano of Ternate in the same month.

Some years ago there was communicated to the Paris Academy, from South America, the fact that permanent magnets lose their magnetism during earthquakes. I will not discuss here the theoretical point of view of the question. During my whole stay in the northern part of Celebes I have always hung up a magnet, with a maximum weight attached to it, but never, not even during the severe earthquakes of Gorontalo, has the weight fallen down. I therefore doubt the fact.

Earthquakes are felt throughout the northern part of Celebes, on the coasts of the Bay of Tomini, at the Togian Islands in the Bay of Tomini; whereas in the southern part of Celebes, for instance at Macassar, earthquakes are scarcely ever felt or only very slight ones. The geological structure of the southern part of Celebes differs entirely from that of the northern.

I enclose a list of earthquakes observed at Gorontalo from 1866-70 by Mr. Riedel.

List of earthquakes at Gorontalo (N. lat. 0° 29′ 42″, W. long. 23° 2′ 50″) between the year 1866 and 1870:—

Year.	Month.	Day.	Hour.	Direction.	Direction of the Wind.
1866	February	18	1 p.m.	E.—W.	N.W.
	April	5	$7\frac{1}{2}$ p.m.	—	E.S.E.
	April	6	$10\frac{1}{2}$ a.m.	E.—W.	—
	June	20	$6\frac{1}{2}$ a.m.	E.—W.	E.S.E.
	September	5	$8\frac{1}{2}$ a.m.	—	S.E.
	December	2	$3\frac{1}{2}$ p.m.	E.—W.	W.
1867	February	26	$11\frac{1}{2}$ p.m.	E.—W.	W.
	March	22	$4\frac{1}{2}$ p.m.	—	N.W.
	March	30	9 p.m.	—	—
	April	22	10 a.m.	E.—W.	N.W.
	May	17	3 p.m.	E.—W.	S.E.
	June	26	$8\frac{1}{2}$ p.m.	E.—W.	S.E.
	July	26	8 a.m.	—	S.E.
	August	27	2 a.m.	E.—W.	S.E.
	September	14	$10\frac{1}{2}$ p.m.	E.—W.	S.E.
	December	23	10 p.m.	E.—W.	W.
1868	April	7	$9\frac{1}{2}$ p.m.	E.—W.	W.N.W.
	May	27	$6\frac{1}{2}$ p.m.	E.—W.	S.S.E.
	June	13	$9\frac{2}{2}$ p.m.	E.—W.	S.S.E.
	July	27	$11\frac{1}{2}$ a.m.	E.—W.	S.E.
	September	4	$9\frac{1}{2}$ p.m	—	S.E.
	November	18	$6\frac{1}{2}$ a.m.	E.—W.	S.E.
1869	March	3	$10\frac{1}{2}$ a.m.	E.—W.	S.E.
	May	3	7 p.m.	E.—W.	N.W.
	August	22	$9\frac{1}{2}$ p.m.	E.—W.	S.E.
	November	17	$4\frac{1}{2}$ p.m.	E.—W.	W.N.W.
1870	April	7	$12\frac{1}{2}$ a.m.	E.—W.	W.S.W.
	July	12	$5\frac{1}{2}$ a.m.	E.—W.	W.S.W.
	August	28	$3\frac{1}{2}$ a.m.	E.—W.	W.S.W.

I am now going to the southern parts of the Philippine Islands, and in the following year to New Guinea. A short communication about my travels in Celebes will be found in *Petermann's Geographische Mittheilungen.*

Macassar, Celebes, Nov. 10, 1871 A. B. MEYER

ELECTROPHYSIOLOGICA:

SHOWING HOW ELECTRICITY MAY DO MUCH OF WHAT IS COMMONLY BELIEVED TO BE THE SPECIAL WORK OF A VITAL PRINCIPLE

III.

2. *In continuation of the question—How in muscular action electricity may do much of what is commonly believed to be the work of a vital principle.*

CONNECTED with the history of electrotonus as exhibited in these experiments* are also other facts which must not be overlooked in this attempt to trace out the workings of electricity in muscular action—facts which show that the departure of contractility and the arrival of rigor mortis are considerably retarded by both forms of electrotonus. Left to itself, the gastrocnemius of the frog loses its contractility and passes into the state of rigor mortis in a time varying with the season and from other causes from 6 to 12 hours; but not so when left to the action of electrotonus. In this latter case, indeed, the contractility may remain for 18, 24, or 36 hours—for a longer time in anelectrotonus than in cathelectrotonus—and even then there may still be no signs of rigor mortis. Once, where anelectrotonus was kept up steadily all the time, and where contractility lingered for 36 hours, the muscles were still limber at the end of 48 hours. No doubt, before exact conclusions can be drawn in these matters more experiments are wanted, many more; but it is not necessary to wait for these in order to be certain that the departure of contractility, and the arrival of rigor mortis, are considerably retarded by the action of both forms of electrotonus. And it is simply to the bare fact that attention is now directed.

What then? Do these facts bear upon what has gone before, and, if so, how?

The facts are obvious. In anelectrotonus and cathelectrotonus alike there are—suspension of the tetanus caused by feeble faradaic currents, elongation of muscle, exalted contractility, together with considerable retardation in the time at which contractility passes off and rigor mortis comes on. In anelectrotonus and cathelectrotonus the parts, muscle and nerve alike, are charged with a charge larger in amount than that which is natural to them—a positive charge in anelectrotonus, a negative in cathelectrotonus. The facts, indeed, are strangely in keeping with the premises. Only let it be supposed that the artificial charge acts upon the dielectric sheaths of the fibres as the natural charge has been supposed to act, but in the contrary direction, that is from without to within instead of from within to without, the charge imparted to the outside inducing the opposite charge on the inside, and all the rest follows. The artificial charge is larger in amount than the mutual charge, and hence the increased elongation of the muscular fibres, the compression arising from the natural attraction of the two opposite elements of the charge keeping up a state of elongation proportionate to the amount of the charge. Hence, also, the suspension of the tetanus by electrotonus, for if the charge elongates the fibres it is easy to see that another of its actions may be that of suspending or antagonising muscular action. And hence again the increased contractility, for, according to the premises, contraction, happening under these circumstances, will be greater because the elasticity of the muscle has freer play at the discharge. In these matters the artificial charge plays the same part as the natural charge, only more energetically, nothing more. And not less so, as it would seem, in the action exercised upon the passing off of contractility and coming on of rigor mortis. Contractility passes off and rigor mortis comes on in the ordinary course of things, because the muscle loses its natural electricity. Contractility passes off and rigor mortis comes on more slowly in electrotonus because the artificial charge associated with this state can take the place and do the work of the natural charge. This is all. Indeed, so far, the whole electrical history of muscle would seem to point to the view which led to the experiment with the elastic band, and to show that living muscle is kept in a state of elongation by the presence of an electrical charge, and that contraction is nothing more than the action of the fibres, by virtue of their elasticity, when liberated by discharge from the charge which kept them elongated previously—ordinary muscular contraction differing from rigor mortis in this only, that the charge which prevents contraction is suddenly withdrawn, and immediately replaced, in the former case, and gradually withdrawn, and not replaced, in the latter case.

Upon this view, also, it is possible to get a glimpse of the reason why contraction is more antagonised by anelectrotonus than by cathelectrotonus; and why contractility is slower in passing off, and rigor mortis slower in coming on, under the former state than under the latter. In anelectrotonus the artificial charge of the parts, muscle and nerve alike, is positive, and, being so, the sheaths are positive externally, and (by induction) negative internally, the manner of charging, which, there is reason to believe, is natural to the muscle. In cathelectrotonus, on the other hand, the opposite state of things obtains. Here the artificial change is negative, not positive. Here, consequently, the charging of the sheaths is negative on the outside and positive on the inside—a state of things which is not natural to the fibres, or which is only met with exceptionally, when these fibres are upon the point of passing into the state of rigor mortis. In anelctrotonus, therefore, the natural charge may co-operate with the artificial charge in a way in which it cannot do in cathelectrotonus; and which, without further comment, it is easy to see may explain in some degree why contraction is more antagonised by anelectrotonus than by cathelectrotonus; and why contractility passes off and rigor mortis comes on more slowly under the former condition than under the latter.

As I have shown elsewhere,* the whole electrical history of muscle is in keeping with this view. The charges obtained from the common friction machine act in the same way as those associated with electrotonus. Everywhere, the question is not of polarisation and of changes in direction of a continuous current, but simply of charge and discharge. Everywhere it is charge preventing, and discharge permitting, action. In a word, the whole electrical history of muscle would seem to show that electricity may have much to do in what is commonly believed to be the work of contractility and tonicity, and that the way in which this work is done is that which is here pointed out.

Against this view, however, sundry objections may be urged. It may be said that the phenomena of muscular action in muscles with sheathed fibres cannot be explained after this fashion. It may be said that the proof of charge during rest and discharge during action is little more than a matter of imagination. It may be said that the force of the natural electricity of muscle is inadequate as force. But, in reality, these objections, when fairly looked into, prove to be of little value.

No doubt the fibres of involuntary muscles differ from those of voluntary muscles in having no proper sheaths. Instead of having those sheaths, indeed, they are made up of cells, mostly fusiform in shape, imbedded in a sort of homogeneous plasm or matrix; and these cells, there is reason to believe, are the contractile elements of the fibres. Still it is not easy to allow the force of any objection arising in this fact, for may it not be that the walls of these contractile cells, which, like the sheaths of the fibres of voluntary muscle, in the main consist of the material of elastic tissue, behave in the way the sheath is supposed to behave under the charge and discharge, that a charge developed on the

* See NATURE, Jan. 11, 1872.

* "Dynamics of Nerve and Muscle." Macmillan.

inside of these walls induces the opposite charge on the outside, that the walls elongate under the compression arising from the mutual attraction of these charges, and shorten when this charge is discharged, because their elasticity is then left free to come into play? Nay, may it not be that this action of the cell membrane is not excluded in those long voluntary muscles in which the fibres seem to be made up of several cells or fibres over-wrapping at their ends, rather than of a single sheathed fibre? And, certainly, this idea is not contradicted by facts remaining in the background; for, as will be seen in due time, these go to show that the walls of all cells and fibres are affected electrically in the same way as that in which the sheath of the fibre of voluntary muscle is supposed to be affected. So that, after all, the phenomena of rest and action in sheathless muscular fibres may supply no valid objection to the view which has been taken of these phenomena as presented in muscular fibres with proper sheaths.

And surely the evidence supplied by the new quadrant electrometer is a sufficient contradiction to the objection that the charge during muscular rest and the discharge during muscular action are mere matters of imagination, for this evidence shows unequivocally that there is a charge during this state of rest and a discharge during this state of action. It is not a question of inference merely, such as it might be if the evidence supplied by the galvanometer were alone available; for here, as has been pointed out, the current during rest, and the comparative disappearance of this current during action, may in reality point to charge and discharge when traced to their causes: it is a question of simple fact. Moreover, the anatomical and physiological analogies existing between the muscular apparatus and the electrical apparatus in the torpedo and the phenomena of secondary contraction, make it more than probable that muscular action is accompanied by a discharge analogous to that of the torpedo. Like the nerves of the muscle, the nerves of the electric organs originate in the same track of the spinal cord, and terminate in the same manner. Like the muscles, the electric organs are paralysed by dividing their nerves. Like the muscles, the electric organs, after being thus paralysed, may be made to act by pinching the nerve below the line of section. Like the muscles, the electric organs are thrown into a state of involuntary action by strychnia. Like the muscles, the electric organs cannot go on acting without intervals of rest. And lastly, the nerves of the electric organs, like the nerves of the muscles, when somewhat exhausted, respond in the same curiously alternating way to the action of the "inverse" and "direct" current, if only discharge be taken as the equivalent of contraction. In a word, these analogies may be said almost to necessitate the conclusion to which Matheucci was led in regarding them, namely this—that muscular action is accompanied by a discharge of electricity analogous to that of the torpedo. And certainly this conclusion is borne out rather than contradicted by the phenomenon of secondary contraction which is exhibited in a prepared frog's leg, when, after laying its nerve upon the muscle of another such limb, contraction is produced in the latter limb; for here the only sufficient explanation would seem to be that offered by Becquerel, namely this—that contraction happens in the first limb because its nerve is acted upon by an electrical discharge developed in and around the muscles of the second limb during action—a discharge which may not indirectly show that there was a charge to be discharged during the previous state of rest. In a word, the evidence, direct and indirect, must surely suffice to show that the idea of charge during rest and discharge during action is something more than a mere matter of imagination.

Nor can it be fairly urged that the force of the natural electricity of the muscle is too feeble to produce the results attributed to it. On the contrary, after what has been said respecting the analogies between muscular action and the action of the electrical organs of the torpedo, it is quite fair to suppose that the force of the discharge in muscular action, instead of being feeble, may be equivalent to that of the torpedo; and that the reason why it cannot be detected in the same way may be that it is short-circuited, and so mainly out of reach, within the body.

3. *How in nervous action electricity may do much of what is commonly believed to be the work of a vital principle.*

There is good reason to believe* that the electrical law of nerve-fibre differs in no wise from that of muscular fibre.

There are also similarities between the principal structural elements of the nervous system from which it would appear that what holds good of one part of this system electrically may hold good of the other parts also. Nay more, there is in these facts reason for believing that what holds good of nerve-tissue generally may hold good of muscle also, for the typal element of nerve and muscle is evidently one and the same.

Looking at the different parts of the nervous system—ganglionic cells, and the peripheral nerve-organs—and at muscle-cells and fibres, it is easy to trace the same structural plan.

Central ganglionic cells, as seen in the ganglia of the sympathetic system, and in other small ganglia of the kind, consist of a round, oval, or pyriform mass of soft translucent, granular substance, with which two or more nerve-fibres communicate, and of an enclosing capsule formed of a transparent membrane with attached or embedded nuclei. The central granular substance, with which the nerve-fibres communicate, and the investing capsule, are unmistakeable in the ganglionic cells of the minute ganglia, but not so in the brain and spinal cord. In the brain and spinal cord there is the same central substance, but the proper cell wall is doubtful. Moreover, the central substance, instead of being a round, oval, or pyriform mass, with which the nerve-fibres are connected at one point only, branches out into several processes, which seem to be continuous with the nerve-fibres. At the same time, these cells and fibres are surrounded and supported by connective tissue, called reticulum by Kölliker, and neurologia by Virchow—a tissue which, as Dr. Sharpey points out, "is not merely an open mesh-work, but consists of fine laminæ formed of a close investment of finest fibrils, disposed as membranous partitions and tubular compartments for supporting and enclosing the nervous bundles;" so that, in the brain and spinal cord, as in the smaller ganglia, there is good reason for believing that the structure of the ganglionic cell is virtually the same, namely, a central granular mass, with which nerve-fibres are connected, and a membrane, with nuclei, investing this mass.

The peripheral nerve organs, of which the principal forms are three in number—the end-bulbs, the touch-corpuscles, and the Pacinian bodies—agree in having (1) an inward part or core of soft, translucent, finely granular matter, in which one or more nerve-fibres end by bulbous, or knobbed extremities; and (2) an outer investing capsule of ordinary connective tissue, with nuclei. In the end-bulbs and touch corpuscles this capsule is simple; in the Pacinian body it is made up of many concentric layers, from forty to sixty in number, with nuclei, these layers, "encasing each other, like the coats of an onion, with a small quantity of pellucid fluid included between them," being strung together where the nerve passes through. The structural plan is still that of the ganglionic cell—a central mass of granular matter, with which nerve fibres are intimately connected, and an investing capsule, simple or complex, as the case may be; and this would seem to be the plan of all the peripheral parts of the nervous system without exception, for it is a question

* See NATURE, Jan. 4, 1872.

whether nerves do ever *terminate* in plexuses or meshes of any kind.

The fibre of voluntary muscle is said to consist of a large number of extremely fine filaments enclosed in a transparent, homogeneous, elastic (the composition agrees with that of elastic tissue), tubular sheath, called the sarcolemma or myolemma, in which are nuclei, called muscle-corpuscles. It might, however, be more correct to say that this fibre consists of a mass of soft granular matter (the granules being the *sarcous elements* of Bowman), agreeing in the main with the granular core of the ganglionic cells and peripheral nerve-organs, enclosed in the sheath which has been described; for the contents of the fibre, instead of splitting up longitudinally into filaments, may split up horizontally into discs—may split either way or any way, in fact, as they would do if they were made up, neither of fibrils nor discs, but of granules which may, as it happens, aggregate into fibrils or discs. The fibre of involuntary muscle, on the other hand, is made up of elongated fibre-cells, connected together by a homogeneous, transparent uniting medium, without any sarcolemma. Each of these fibre-cells has a core of finely granular matter, sometimes arranged so as to form imperfect fibrils, and of a distinct cell-membrane, with nuclei, the shape of the cell being fusiform, with ends sometimes pointed, sometimes truncated, sometimes simple, sometimes branched. The cell-membrane in reality takes the place of the sarcolemma, for each cell is nothing more or less than a rudimentary fibre. Indeed, in long voluntary muscles there are fibres which seem to partake somewhat of the character of voluntary and somewhat of the character of involuntary fibres—fibres which, instead of running continuously from one end of the muscle to the other, are made up of several elongated fusiform cells, overlapping each other at the ends, and which therefore may consist of cell-membrane and sarcolemma both. Nor is the connection of the nerves with the muscular fibres or cells peculiar. Beale and Kölliker think that the nerves belonging to voluntary muscle end in meshes of pale fibres outside the sarcolemma. Rouget, Kühne, and others are of opinion that this ending is in peculiar organs—motorial end-plates continuous with the axis-cylinder of the nerve, oval or irregular in shape, within the sarcolemma and between it and the proper muscular substance, the primitive nerve-sheath fusing with the sarcolemma, and one end-plate being devoted to each muscular fibre. And thus it may be that the muscular fibre or cell may agree in structure with the ganglionic cell, and the peripheral nerve organ, in having a soft granular core, with which one or more nerve-fibres are connected, and an investing membrane of connective tissue with one or more nuclei. It may be, indeed, that the muscular fibre and cell are only varieties of the peripheral nerve-organ.

The nerve-fibres by which these several bodies—ganglionic cells, peripheral nerve organs of various kinds, and muscular fibres and cells—are connected together, are of two kinds, the tubular, which are white with dark borders, and those which are grey, pale, non-medullated or gelatinous. The white or tubular fibres, when quite fresh, appear perfectly homogeneous like threads of glass, but afterwards, when coagulation has taken place, they are found to consist of an axis, or primitive band, as it is called, a white medullary coating strongly refractive of light, and giving them the appearance of having dark borders, and an outer membranous sheath or tube, with nuclei in it, agreeing in composition with elastic tissue, and being analogous to the sarcolemma. The grey, pale, gelatinous fibres would seem to consist of the axis or primitive band of the others, with obscure sheaths in which are nuclei, but without medullary coating. They belong chiefly to the ganglionic system, but not exclusively; at all events the finer subdivisions of the white dark-bordered nerves of the other systems are found to have lost their dark borders, and to have become undistinguishable from those which have no dark borders naturally. In nerve-fibres, therefore, as in nerve-cells, there would seem to be a central core, and a membranous investment containing nuclei; and, all things considered, the connection of these fibres with ganglionic cells, with peripheral nerve-organs, and with muscular fibres and cells, would appear to be by one and the same method, the axis or primitive band being continuous with the central soft granular core of the central and peripheral elements of the nervous system, and of the muscular fibres and cells (for with so many points of analogy it is difficult not to believe with Rouget, Kühne, and others who agree with them in this matter), the primitive sheath, when there is one, being continuous with the membranous investment of this core, neurilemma, sarcolemma, or other, as the case may be.

Instead of being peculiar, therefore, the voluntary muscular fibre may be no more than a modified form not only of the contractile cell of the involuntary muscular fibre, but also of the nerve-fibre, and of the central and peripheral cell-elements of the nervous system. The same type of structures is to be traced out in each case. There is in each case the same central, granular, soft, substance, but slightly changed protoplasm probably, in the molecular change of which an electrical change may originate. There is in each case outside this central substance a membrane which may become charged leyden-jar-wise as the neurilemma and sarcolemma are supposed to be charged. And, therefore, it is not altogether begging the question to conclude that in each case one and the same electrical law may bear rule.

And certainly the adoption of this idea is calculated to elucidate much that is obscure in the structure and action of the nervous and muscular systems.

Upon this view a use is found for the contents and walls of the fibres and cells of which the nervous and muscular systems are made up. The contents are wanted for the generation of the charge; the walls are wanted for receiving and holding this charge. Their leyden-jar office, indeed, explains why it is that the nervous and muscular systems should be made up of cells and fibres.

Upon this view one use is found for the nucleus in the walls or sheath of cell or fibre. The nucleus may represent the spot at which the development of this wall or sheath is arrested—the spot at which the original, moist, *conducting* protoplasmic matter is not transformed by drying, or in some other way, into *non-conducting* wall or sheath, and, therefore, as I think, the nucleus may have a very definite function to fulfil. As I think, indeed, the case may be this: that the molecular changes in which the charge of the cell or fibre originates (those in the contents of the cell or fibre) depend upon the continual ingress of fresh and egress of used-up aërated matter; that this ingress and egress is, not through the wall or sheath anywhere or everywhere, but only through the nucleus; that the one charge not wanted for charging the inner surface of the wall or sheath may escape to earth through the nucleus; and that the channel of the discharge which happens when the cell or fibre passes from the state of rest into that of action may also be through the nucleus. Without such opening as may be supposed to exist in the nucleus, indeed, it is difficult to understand how the cell or fibre should be charged and discharged; and thus, upon the view in question, a use is found (not the only use, of course), for the nuclei present in the walls of the cells and in the sheaths of the fibres of the nervous and muscular systems.

Upon this view, too, the infinite number of these cells and fibres may in some degree be accounted for. For may it not be that each cell and fibre acts as a condenser to every other cell or fibre, so that a charge or discharge which is feeble without being multiplied becomes anything but feeble when multiplied? And may not this function of a condenser be the one function of the Pacinian bodies?

Other cells and fibres have other functions as well; these bodies may have this one function only. They may, in fact, be rudiments of the electric organs of the torpedo, with a sphere of action, not without the body, but within it. And this may be the reason why these bodies are placed on the trunks of nerves at points where it may be supposed that special means are wanted for keeping up the requisite degree of elastic tension, their use in this case being analogous to that of an ordinary leyden condenser in connection with a telegraph wire conveying a minimum amount of electricity.

Nor does this view fail to elucidate in some degree the way in which nerves tell upon muscle and react upon each other. Let the contents of the muscular fibre or cell be connected with the contents of the corresponding ganglionic cell by the axis cylinder of the nerve, and a charge or discharge in the nerve centre must tell upon the muscular tissue, just as in the case of two leyden jars with their inner coatings connected by a conductor, the charge or discharge of the one involves corresponding changes in the other. Let the case be that of a sensory peripheral cell and a central ganglionic cell, similarly connected, and a charge or discharge in the former will involve a charge or discharge in the latter, the discharge producing sensation. The case is simply that of a leyden battery, with all possible space economised by making the conductors, where they may, do the work of the jars. The case is plain as regards the charge, for the molecular charges are ever at work by which it is kept up and renewed; and the case is not altogether obscure even as regards the discharge, for it may well be that discharge happens when the charge increases until it overleaps the barrier of insulation presented in the dielectric walls of the fibres and cells—a result which, for want possibly of a sufficiently insulating barrier somewhere, happens more easily than it ought to do in the case of involuntary nervous action, such as is seen in convulsion, neuralgia, and the rest.

Viewed in this way, too, it is easy to see that the nervous system may do its work, not by discharge only, but by charge also. It is easy to see that the discharge may be all that is wanted to cause contraction; indeed, ac cording to the premises, all that is wanted for this purpose is that the charge which kept the muscular fibre elongated should be discharged, and the fibre so left to the play of its own natural elasticity. It is easy to see, also, that discharge may be the mechanical agent which may call the various nerve-centres into action—by shaking the veil which separates the visible from the invisible in the higher mental processes, perhaps. And for charge no less than discharge it is also easy to see that there may be a definite work to do—a work of which the end is, not to cause action in the muscles and in the various nerve-centres, but to prevent it. Indeed, after what has been said, it is to be supposed that all nerves, through their electricity, have during rest an action which Pflüger supposes to be peculiar to certain nerves only, and to which he gives the name of *inhibitory*.

And here opens out a question of paramount interest.

It has been seen that the electric law of nerve and muscle is one and the same. It has been seen that the state of contraction in muscle is antagonised by the presence of a charge of electricity in muscle—that a state of actual elongation is produced by the action of this charge. It has been seen, not only that the state of contraction is antagonised and a state of elongation set up by the presence of the natural charge of electricity in muscle, but that more marked changes of the same kind are produced by the action of an artificial charge of electricity, provided this charge be greater in amount than the natural charge. The facts, indeed, are calculated to justify the notion that the degree of elongation produced by the conjoint action of the charge belonging to the muscle itself and the charge imparted to the muscle from its nervous system is greater than that produced by the action of the former charge singly; or, in other words, that the charge imparted to the muscle by its nervous system may cause a degree of elongation in the muscle which is over and above that caused by the charge belonging to the muscle itself—which surplus may have much to do in explaining rhythmical action in hollow muscles.

Take the case of a hollow muscle—a capillary vessel, for example. This vessel has its special nervous system, vasomotor nerves, efferent and afferent, vasomotor centre; and the question is as to how this system acts upon the vessel. May it be that a charge of electricity is continually being developed upon the cell-walls and fibre-sheaths of this system by the action of the oxygen of the blood and other causes upon the contents of the cells or fibres; and that this development goes on until, the bounds of insulation being overpassed, discharge happens? May it be that the muscular fibres forming the walls of the vessel elongate, and in so doing cause the vessel to dilate as long as this charge is imparted to them? May it be that the vessel passes from the state of dilatation into that of contraction when the discharge of this charge happens, in consequence of the muscular fibres being then liberated from the condition of extra-elongation caused by the charge imparted to them from the nerves, and so left to the play of their natural elasticity? May it be that thus there are diastolic and systolic changes in the vessel by which the blood is alternately drawn into and driven out of the vessel, changes which may supply the key to the mystery of "capillary force"? Nay, more; may it not be that the diastolic and systolic movements of the heart itself may have to be explained in the same way? To all these questions I answer, unhesitatingly, yes, it may be so. Indeed, after what has been said, the only explanation which seems to be called for concerns the movements of the auricles of the heart, and this is easily given: for, as it seems to me, the auricles must be looked upon chiefly as cisterns formed of dilated veins, and their movements chiefly as passive consequences of the movements of the ventricles, the systole of the auricles being little more than the passive falling-in of the auricular walls upon the blood being suddenly sucked away by the ventricular diastole, the diastole of the auricles being little more than the passive bulging-out of the auricular walls, caused at one and the same time by the stream of blood which is ever flowing in from the valveless openings of the great veins, and by a forcing back of this stream, consequent upon the sudden closure and recoil of the auriculo-ventricular valves at the moment of the ventricular systole. In this way the seemingly diastolic and systolic movements of the auricles must alternate with the true diastole and systole of the ventricles, and, at the same time, the absence of valves at the opening of the great veins into the auricles is accounted for—an absence altogether inexplicable if the auricular systole had to play the *active* part in the circulation which is played by the ventricular systole. And much to the same effect may be said of rhythmical movements in other hollow muscles, the chief difference between one such movement and another being perhaps this—that contraction follows upon dilatation more slowly in consequence of the cell-walls and fibre-sheaths of the special nervous systems being constructed differently as regards the capacity for quick charging and discharging; but these hints must suffice for what might be said upon this subject.

Nor can it be urged as an objection to this view of nervous action—the only objection which may be urged, so far as I know—that the state of action in nerve-fibre is unattended by the contraction which attends upon action in muscular fibre. The electrical law of nerve and muscle being one and the same, it might be expected, perhaps, that this particular difference should not exist; but this difficulty, if it be one, is soon disposed of. Thus, the success of the experiment with the elastic band depends upon the band being of a certain thickness, and upon the weights being so adjusted as to balance without overbalancing its elasticity. Failing these conditions charge and discharge may not tell in causing elongation and contraction. And, therefore, the absence of perceptible elongation and contraction in the nerve-fibre under the charge and discharge may be simply owing to the fact that the thickness and stretching of the neurilemma have not been adjusted for the production of these results. Besides, it is by no means certain that there are not in some nerve-fibres slight changes which are strictly parallel to the elongation and contraction witnessed in muscular fibres.

In a word, there seems to be good reason for believing that in nerve as in muscle electricity may have to do much of what is commonly regarded as the special work of an inherent vital principle.

4. *How in maintaining the "tone of the system" electricity may have to do much of what is commonly regarded as the special work of a vital principle.*

After what has been said little remains to be added under this head. The conclusion arrived at is that each perfect fibre and cell of living muscle and nerve (and, by implication, every living fibre and cell), is a charged leyden-jar while at rest. It is that the membranous portion of the fibre or cell is at this time compressed by the mutual attraction of the two opposite charges disposed leyden-jar-wise upon its two surfaces. It is that the effect of this compression is to elongate the fibre or cell by squeezing out this membrane lengthwise. What then? May it be that this compression, this squeezing out, is sufficient to account for what is called the "tone of the system"? This state, no doubt, is indefinite enough, but it becomes more definite when viewed in this way—so definite, in fact, that here also, in the maintenance of the "tone of the system" that is to say, electricity may have to do much of what is commonly believed to be the work of a vital principle.

5. *How in certain processes of growth electricity may do much of what is commonly regarded as the special work of a vital principle.*

A cell or fibre is at first a mass of protoplasm without any investing membrane. Later, this membrane makes its appearance, and how is this? Is it that the surface of the protoplasmic mass, except at the part or parts where the nucleus is afterwards met with, hardens by desiccating, or dying, or changing in some other way, and, so hardening, acquires dielectric properties? Is it that the molecular changes ever going on in the protoplasmic matter beneath this crust, develope a charge on the inside of this crust, which, acting inductively, leads to the development of the opposite charge on the outside? Is it that the compression arising from the mutual attraction of these opposite charges, causes the crust to stretch out every way, and so separate from the underlying protoplasmic mass, leaving thereby in some instances a vacuole, which may be filled with a thin liquid or even air? Is this the way in which the sarcolemma and neurilemma, the cell-walls, and all membranes more or less analogous to them, may be formed? After what has been said such an idea is by no means improbable. Nay, such an idea may be looked upon as the natural consequence of the premises. And if so, then electricity may have to do much of what is commonly believed to be the work of a vital principle in these phenomena of growth, as well as in the various processes which have been already passed in review, and upon which so much has been said as to leave only room now for these bare hints of what might be said upon the subject.

C. B RADCLIFFE

MERCURY PHOTOGRAPHS

AN entirely novel method of photographic printing has just been discovered by M. Merget of Lyons. Although akin in some respects to the daguerreotype process, it differs essentially therefrom in the fact that exposure to light is not necessary to the formation of every separate image. It is difficult indeed just now to apply any distinguishing name to M. Merget's invention, for the methods hitherto discovered—and the number of these has, we all know, increased of late beyond all calculation—are all of them divisible into two very distinct classes. Thus we have those processes broadly termed chemical, in which every print is secured by the aid of light, as for instance, the nitrate of silver and carbon methods; and those again where a matrix, or printing block, having been prepared, the copies are struck off in the ordinary lithographic or printing press; photographs prepared in this latter manner are usually termed photo mechanical prints. M. Merget's invention partakes singularly enough of the nature of both classes; for while the prints are undoubtedly formed by chemical action, the question of light is of no moment at all, and the manipulations involved are to some extent of a mechanical nature.

The experiments of Faraday upon the diffusion of gases will be remembered by many, and it was the results arrived at by that distinguished philosopher that incited M. Merget, the Professor of Physics at the Faculté des Sciences of Lyons, to take up the investigation he has so successfully carried through. Faraday had already found out that the vapour of mercury acted very sensibly upon gold-leaf, and the first task undertaken by M. Merget was to discover whether this same action also took place upon other metals or their compounds. The investigation, it should be stated, was designed to be of a purely theoretical nature, and was not undertaken, in the first instance at any rate, with a view of working out any practical processes such as may eventually result from the research. The principal points discovered by M. Merget may be thus summarised:—

1. The vaporisation of mercury is a continuous phenomenon; that is to say, the metal emits vapour at all times, even at a very low temperature, and when in a solidified form.

2. Mercury vapour may be condensed upon certain substances, such as carbon, platinum, &c., without these latter being chemically affected.

3. Mercury vapour will pass with exceeding facility through porous bodies, such as wood, porcelain, &c.

4. The salts of all precious metals when in solution are very sensitive to the action of mercury vapour, which has the effect of rapidly reducing them.

The most sensitive to mercury of the precious metal salts are nitrate of silver and the soluble chlorides of gold, palladium, and iridium, and paper prepared with any of these forms at once a most delicate test for the volatile metal; but the solutions must contain some hygrometric body to prevent complete desiccation, so that the surface coated with them will always remain in a moist condition. To demonstrate how exceedingly sensitive this test-paper is to mercury, we may state that its contact with any body containing but a slight trace of amalgam suffices to darken the surface, while it is affirmed that any workman who has been employed for some time in a looking-glass or other similar factory, may produce an impression of his hand by simply laying the same upon a sensitive surface of this kind, the minute traces of mercury in the pores of the skin being amply sufficient to bring about a reduction of the salt, and to produce consequently an imprint of the fingers. In the same way a section of wood exposed to mercury vapours, and afterwards pressed in contact with a sheet of sensitive paper, prints off upon the surface all the rings and markings it possesses, the mercury being deposited in the pores of the wood in a more or less condensed form.

In the event of nitrate of silver being used for preparing the paper, it is necessary, obviously, to exclude the light, as otherwise a reducing action will be already set up by solar means alone, but with the salts of palladium or platinum no such action need be feared. According to the kind of metallic salt employed, so the tint of the impression varies, but in most cases an intense black may be obtained where the action has proceeded far enough.

Having described M. Merget's discoveries thus far, it is easy to guess how that gentleman employs them in the carrying out of a photographic process. An ordinary glass negative, possessing an image which has been formed by the deposition of silver particles, is prepared in a suitable manner to protect it from injury by contact with the mercury (such, for instance, as coating it in some way with platinum or carbon particles), and the picture is then exposed to the action of mercury vapour. The vapour condenses, in a more or less concentrated form, upon the image—in the same way, pretty well, as it becomes deposited upon, and develops, the latent image in the daguerreotype process—and subsequently the plate thus treated is brought into contact with the sensitive paper. The consequence is that the minute particles of mercury deposited all over the image exercise a reducing action upon the salts on the surface of the paper, and a print results of the original photograph, poss ssing the same gradation of tint as the original. Indeed, when nitrate of silver is employed for sensitising the paper, the photograph secured is in every respect similar to that produced by light in the ordinary silver printing process, and the picture is forthwith toned and fixed in the same way, in fact, as one of these; in the one case, however, the reduction of the silver salts has been brought about by mercury vapour, while in the other light alone has been the reducing agent. Impressions obtained by means of platinum and palladium salts need simply to be washed in water in order that they may be permanently fixed. These latter, in truth, are so indestructible and inalterable that they cannot be destroyed except by a chemical agent which would at the same time radically injure the paper, or other basis, upon which they rest.

This process of photography is not yet in such an advanced state as to be of any practical importance; but, nevertheless, it is certainly one of the most ingenious and interesting discoveries made of late in this branch of Science. The great advantage it possesses is that of printing without the aid of light, and yet producing prints with detail and half-tone dependent upon delicate chemical reaction—such rare gradation being secured as our present light-printed pictures (silver and carbon prints) alone possess. A mechanical printing process could, of course, easily be worked out from these data, if considered desirable; and, indeed, it is by no means improbable that this will be the most successful way of applying the discoveries in a practical form. But even in the event of no practical use at all being made of the process—for this is indeed questionable—the research, regarded from a purely scientific point of view, is deserving of the highest eulogium.

H. BADEN PRITCHARD

NOTES

IN another column will be found full details of the observations of the Total Eclipse of December 12, made at Bekul, by Mr. Norman Lockyer and Captain Maclear. In future numbers we hope to give similar reports from the observers at the other stations. The weather was very favourable at all the stations, with only one exception.

M. JANSSEN writes as follows to the French Academy of Sciences, under date Sholoor, Neelgherry, 12th of December, 1871, 10 A.M.:—"I have just observed the eclipse, only a few moments since, with an admirable sky, and whilst still under the emotion caused by the splendid phenomenon of which I have just been a witness, I address a few lines to you by the Bombay courier, who is to start instantly. The result of my observations at Sholoor indicates without any doubt the solar origin of the corona, and the existence of matter beyond the chromosphere.' And in a letter to M. Faye, written half an hour later, he says:—"I have just seen the corona, as it was impossible for me to do in 1868, when I was entirely occupied with the spectrum of the protuberances. Nothing can be finer, nothing more luminous, with peculiar forms which exclude all possibility of a terrestrial atmospheric origin. The spectrum contains a very remarkable brilliant green line already indicated; it is not continuous, as has been asserted, and I have found in it indications of the obscure lines of the solar spectrum (especially D). I believe the question whether the corona is due to the terrestrial atmosphere is settled, and we have before us the prospect of the study of the extra-solar regions, which will be very interesting and fertile."

PROF. HUXLEY'S friends, and the scientific world generally, will learn with great regret that he has been compelled to relinquish all work for the present, his medical advisers having ordered him complete rest for two months, for which purpose he has just started for Egypt. There is every prospect that at the end of the time he will return to his old work with renewed vigour.

THE Regius Professorship of Physic in the University of Cambridge has become vacant by the resignation of Dr. Bond, who has held the office since 1851.

THE Council of the College of Preceptors has arranged for the delivery of a series of three lectures to the members of the college and their friends, on the teaching of science in secondary schools. The first lecture of the series, "On Teaching Physics," was delivered at the rooms of the College, 42, Queen Square, on Saturday evening, the 13th instant, by Professor G. C. Foster, F.R.S.; the second, "On Teaching Mechanics," was delivered yesterday (Wednesday) evening by Prof. W. G. Adams; and the third, "On Teaching Botany and Geology," is to be delivered on Monday evening, 22nd inst., by Mr. J. M. Wilson, of Rugby. The point mainly insisted on by Prof. Foster in his lecture, was the necessity, in order to make the study of Physics of much use as a training for the mind, that the pupils should not only see, but actually make experiments for themselves, so that the principal facts and phenomena discussed may be known to them as matters within their own experience.

A SERIES of lectures will be delivered in Gresham College, Basinghall Street, by Mr. E. Symes Thompson, M.D., F.R.C.P., as follows:—Thursday, January 18, 1872, On the Digestive Organs in Health and Disease (continued from last course); Friday, January 19, 1872, On the Blood Vessels; Saturday, January 20, 1872, On the Pulse.

AT the first Anniversary Meeting of the Anthropological Institute, held January 15, Sir John Lubbock, Bart., M.P., F.R.S., president, in the chair, the president delivered an address, and the officers and councils to serve for 1872 were elected as follows:—President—Sir John Lubbock, Bart., M.P., F.R.S.; Vice-Presidents—Mr. W. Blackmore, Prof. Busk, F.R.S., Dr. Charnock, Mr. John Evans, F.R.S., Mr. George Harris, Prof. Huxley, F.R.S.; Director—Mr. E. W. Brabrook; Treasurer—Mr. J. W. Flower; Council—Mr. H. C. Bohn, Captain R. F. Burton, Mr. James Butler, Mr. A. Campbell, M.D., F.R.S., Mr. Hyde Clarke, Mr. J. Barnard Davis, M.D., Mr. Robert Dunn, Mr. David Forbes, F.R.S., Colonel A. Lane Fox, Mr. A. W. Franks, Sir Duncan Gibb, Bart., M.D., Mr. Joseph Kaines, Mr. Richard King, M.D., Mr. A. L. Lewis, Mr. Clements R. Markham, Captain Bedford Pim, R.N., Mr. F. G. Price, Mr. C. Robert des Ruffières, Mr. Spottiswoode, V.P.R.S., Mr. C. Staniland Wake.

MR. SAMUEL SHARPE has presented the sum of 4,000*l.* to University College towards the building fund, and Mr. J. Pemberton Heywood has given a donation of 1,000*l.* towards the same object. The executors of the late Mr. Felix Slade have given 1,600*l.* towards the cost of the fine-art buildings and to provide casts and other appliances for the students.

AT a recent session of the Council of University College, it was decided to admit ladies attending the class of political economy to compete for the prizes and also for the Hume and Ricardo Scholarships awarded for proficiency in that science.

THE young Hippopotamus, which we announced as having been born on Tuesday last week at the Garden of the Zoological Society, died the following day. The body has been sent for dissection to Prof. Humphry at Cambridge. We may hope therefore to hear more of him in the pages of the *Journal of Anatomy and Physiology*, which is edited by the professor.

WE are informed that the next number of the *Quarterly Journal of Science* will contain a detailed account by the editor of the scientific principles involved in the A B C Sewage Company's process, of which, according to the *Times*, Mr. Crookes, F.R.S., has accepted the scientific direction.

THE first part is just published at Leipzig of a new edition of Pritzel's "Thesaurus Litteraturæ Botanicæ," or index of works on the various branches of botany, published in all languages, from the earliest times. As it is more than twenty years since the publication of the last edition, the additions are very numerous.

THE President of the Medical Society of the county of New York, Dr. Abraham Jacobi, has placed in the hands of its treasurer 400 dols., to be awarded for the best essay on "A History of the Diseases of Infancy and Childhood in the United States, and of their Pathology and Therapeutics." Competitors will send their essays in English, with motto attached, and address of the writer, with the same motto, in a sealed envelope, to the present Secretary of the Society, Dr. Alfred E. M. Purdy. 123, East Thirty-eighth Street, on or before January 1, 1873 The committee are authorised by the society to withhold the prize if the essays submitted should not merit it.

DR. J. W. FOSTER, President, and Mr. William Stimpson, Secretary of the Chicago Academy of Sciences, have issued a circular informing the scientific world of the extent of the losses suffered by the Institution through the calamitous fire in that city. These comprise, besides a very large number of other collections of great value, the Audubon Club collection, consisting of very finely mounted specimens of the game birds and mammals, both of America and of Europe and Asia, about 400 in number; the State collection of Insects, recently purchased by the State of the heirs of the late State Entomologist, Mr. B. D. Walsh, for 2,000 dols., but of great scientific value from the number of types it contained; the splendid series of specimens illustrative of the natural history of Alaska, collected in 1865-69 by Bischoff and the naturalists of the W. U. Telegraph Expedition; the Smithsonian collection of Crustacea, undoubtedly the largest alcoholic collection in the world, which filled over 10 000 jars, and contained the types of the species described by Prof. Dana and other American authors, besides hundreds of new species, many of which were described in manuscripts lost by the same fire; the Invertebrates of the U.S. North Pacific Exploring Expedition, collected in great part in Japanese seas by the secretary in 1853-56, which besides Crustacea, included in the last item, embraced great numbers of Annelides, Mollusca, and Radiata, most of which remain as yet undescribed, except in manuscripts also lost; the collection of the marine shells of the coast of the United States, made by the secretary and his correspondents during twenty years of dredgings and general research on every part of the coast from Maine to Texas; nearly every species was illustrated by specimens from every locality in which it occurs, not only on our own shores, but on those of Europe and the Arctic Sea, and in the Tertiary and Quaternary formations, showing the effect of climatic influences, geological age, &c.; this collection embraced about 8,000 separate lots of specimens; the deep-sea Crustacea and Mollusca, dredged in the Gulf Stream by M. Pourtales, of the U. S. Coast Survey, in the years 1867, '68, and '69, which had been placed in the hands of the secretary for description; the results of the deep-sea dredgings in Lake Michigan, by the Academy in 1870 and 1871, the work of the latter year having been performed by Mr. J. W. Milner; the Arctic collections of the late Director of the Academy, Robert Kenicott, made during the years 1859-61. The general collection contained about 2,000 mammals, 30 mounted skeletons, including two mastodons, an African elephant, sea otter, elephant-seal, &c., 10,000 birds, 1,000 nests of eggs, and a great quantity of eggs without nests, 1,000 reptiles, 5,000 fishes, including many large sharks and rays, 15,000 species of insects and other articulates, 5,000 species of shells, with immense numbers of duplicates, 1,000 jars of molluscs in alcohol, 3,000 jars or "lots" of radiates, including several hundred corals, 8,000 species of plants, 15,000 specimens of fossils and 4,000 minerals. In Archæology there were about 1,000 specimens, all American; and the Ethnological collection embraced a very fine series of the clothing and implements of the Esquimaux of Anderson River, collected by Robert Kenicott and his Arctic friends, and presented by the Smithsonian Institution. The Academy desires to announce that although now laid prostrate by the terrible disaster it has suffered, it will soon rise to refill its place among its sister institutions. The trustees have determined to build up again the material interests of the Institution, notwithstanding the terrible losses which they, in common with all of its patrons, have suffered. The publication of its Transactions will soon be resumed. The Academy would therefore take this opportunity to appeal to its correspondents for the donation of their own publications of the past few years, to replace those lost, for which it was also indebted to their generosity.

PROF. NATHAN SHEPPARD, of the University of Chicago, has written to the papers to state the present position of the University of Chicago, and of the Observatory, which is well known in the astronomical circles of Europe. The buildings fortunately escaped, but the fire has left the University in very serious financial difficulties. Many of the gentlemen upon whom the University, and especially the Observatory, was dependent are so reduced in circumstances as to be unable to meet their engagements. The consequence is that the resources of the University are suddenly and greatly abridged. In fact, its income, aside from its tuition fees, is entirely cut off. The Observatory is the first department to feel this loss. A letter just received from Chicago says it is feared that the eminent director, Prof. Truman H. Safford, would be obliged to leave his post for want of support. This will be sad news to the professional correspondents of Prof. Safford in Europe. When the University was founded, about fifteen years ago, a few public-spirited gentlemen rallied around it, and under their self-sacrificing care it has been housed in a commodious and elegant (although unfinished) building, at a cost, including Observatory, telescope, &c., of about forty thousand pounds; and now has, in all departments of study (preparatory, classical, scientific, and law), twelve professors and about 250 students. In conclusion, the Professor, dating from 77, Upper Thames Street, London, asks any reader who would care to lend a helping hand to do so, and to follow in the wake of a Scotch gentleman who has generously offered to head the subscription list with 50*l.*

INOCULATION has by the Indian Legislature been forbidden in the districts of the twenty-four Pergunnahs, Nuddwa, Burdwan, Hooghly, and Howrah in Bengal.

THE FOUNDATION OF A TECHNOLOGICAL EDUCATION*

TECHNOLOGICAL education is taken up by many writers on the subject at the time when a youth is supposed to enter the School of Technology; and scientific men, as a rule, do not seem to set sufficient stress upon the necessity of laying the foundation for it at a much earlier age. It is not indeed scientific men alone who are interested in this question, but they are the authorities who should speak out upon it, for they alone are competent to pronounce an opinion upon the value of scientific education. It cannot be expected that men who themselves know nothing of science, care nothing for its progress, and recognise none of the obligations under which they lie to it, should favour its introduction into our schools, and thus depart from the stereotyped and antiquated system of education, that brings up our youth but partially fitted or altogether unprepared for a majority of the occupations they are destined to pursue, and exposed at every point to suffer from their own ignorance and the impositions of others. Every one now-a-days should have such a knowledge of scientific principles and methods as will enable him to form a just idea of the value of science, and to distinguish between knowledge and pretence—between science and quackery. The political economist, who has to legislate regarding the natural resources of the country; the capitalist, who invests in their development and manufacture; the lawyer, who has to conduct the numberless suits into which scientific questions enter; the journalist, who claims to enlighten and direct the masses; every one who uses manufactured products liable to adulteration; every one who values his health, or has to consult a medical man or other scientific expert; every father, and, what is still more important, every mother of a family; every youth that is making choice of an occupation for life; or, in other words, every member of a civilised community, ought to be acquainted with the elementary facts and principles upon which all the applications of science are based.

This knowledge, which should thus form an essential feature of general education, is also that which will form the very best foundation for technological purposes. In the first place, it will bring into technological schools a vast amount of excellent material that is now wasted elsewhere; for numbers of youths, with minds well adapted to such pursuits, would take to the practical applications of science, if they knew anything at all of science itself. Nor need there be any fear that the field will thereby be overcrowded; for so long as quacks and pretenders abound there is room for good men, and the difficulty at present is to obtain students who have a natural aptitude, or rather, we should say, an aptitude developed by early education for scientific work.

Secondly, and this is the really important aspect of the case, educators will have to deal with material prepared for their purposes, instead of, as now, receiving it not merely unprepared, but actually warped out of proper condition. For it is not too much to say that a youth who has had a purely academic education, on entering a technological institute has to devote a large portion of his time to mastering elementary ideas and principles, that he should have learned as a child; whilst the erroneous methods of instilling knowledge to which he has been subjected, will be a hindrance to him for years, if not for life. It is but a few days since that a freshman in such an institute gravely asked the writer "if a fish was not an animal," thus displaying, at the age of seventeen, a doubt of the meaning of a term that he should have accurately understood at the age of seven. Of a term, did we write? We mean of a fact; of one of the broadest generalisations of science. Now, what has not such a youth to learn of first principles? How utterly unprepared in the simplest rudiments of knowledge is he for a technological course! But when we come to the system of thought induced by the vicious methods of preparatory study, the case is still worse Here we have the labour of driving practical instruction into the brain of a young man who, after having passed perhaps brilliantly through college, is now laboriously pushing his way through a technological course; he is now nominally near its close, yet three years of steady application have not divested him of the habit of learning by rote on the authority of others. He has no reliance on his own experiences, seeks no explanations by questioning his own reasoning powers, but prefers always to take another's opinion, instead of elaborating a judgment of his own. He is still in fact utterly devoid of the first essentials of self-help in education, so completely have his natural abilities been misdirected in that first course, in which the amount of evil accomplished may be judged by the very brilliancy of his success in it. Such a student will never make a reliable scientific expert. We should not like to trust him even as a druggist's clerk; he should never have entered a technological institute, because he has never had any foundation laid for a technological education.

But in what is such a foundation to consist? and when is it to be commenced? What alterations are to be made in our recognised systems of instruction? Already there are more subjects to be taught than the child has time to learn. We reply, let this education commence in the very infant school; let the methods of instruction be rational, because natural ones; let the subjects be taught in their natural order; and we may very easily teach, or rather "educate," vastly more than we do now. At present beyond mere reading, writing, some mathematics, and something of languages, this child learns absolutely little, and that little superficially. It wastes its time largely in learning the theoretical use of these tools without being made to apply them in building up an education. This is not the way in which the carpenter instructs his new apprentice; if he did, neither would ever reap much benefit from his instruction.

Let the elements of the natural and physical sciences form a part of general education; let physical geography go before political; let the child learn that a history of the world precedes that of man; and at every point let him be familiarised with the intimate dependence between the truths of science and the fact of his own existence. Let these things be taught by a rational method of object teaching, not used to convey desultory information, but as a system of training, whereby the reasoning faculties may be rightly educated, at the same time that the memory is taxed with a stock of useful, because elementary and connected ideas. Let reading and writing sink to their proper rank, as means of education and not as objects of it; and let them, whilst being taught, be used to aid in the acquirement of real knowledge.

This may seem to demand a radical change in our system of preparatory education public and private; but if the technologist wishes to make the most of young minds, he must bend them to his purpose from their earliest years; nor will the community at large, when it understands that its interests in the matter are identical with its own, be averse to the change proposed, which is in accordance with its needs and the progressive spirit of the age. If the advocates of a liberal and enlightened system of popular education in England can succeed in tiding over the shortsighted opposition of sectarianism, as above sketched out, inaugurated there by the aid of its scientific men; the result will be, that the technological schools of Great Britain will be supplied with materials trained from their very infancy in science. Are there no scientific men in the country who will take up the subject here in the same wide-awake spirit?

* By Mr. E. C H. Day, reprinted from the *New York Technologist*.

MECHANISM OF FLEXION AND EXTENSION IN BIRDS' WINGS*

DR. COUES' proposition is, that flexion of the forearm upon the humerus produces flexion (adduction) of the hand upon the forearm, by osseous mechanism alone, and conversely: extension of the forearm causes extension (abduction) of the hand. The point of the article consists in a demonstration of the fact that, in spreading and folding the wing, the radius slides lengthwise along the ulna to a certain extent. Recapitulating certain points in the anatomy of the elbow and wrist. the author shows that this sliding is produced by the relative size, shape, and position of the humeral surfaces with which the radius and ulna respectively articulate; these being such, that in flexion of the forearm the radial surface is nearest the wrist-joint, and in extension the ulnar one; and consequently the two bones of the forearm occupy different relative positions in flexion and extension. In flexion, the radius is pushed forward, and projects somewhat beyond the end of the ulna, impinging upon the radio-carpal bone (scapholunar), and pushing the pinion around the centre of motion of the wrist-joint so that it is more or less flexed. In extension, the reverse motion takes place, from the pulling back of the radius. The proposition is carefully demonstrated, illus-

* Abstract of a Paper read at the Indianapolis Meeting of the British Association for the Advancement of Science, August 1871. By Dr. Elliott Coues. From the *American Naturalist*.

strated with three figures, and likewise shown to be susceptible of ocular proof by direct experiment. Several interesting corollaries are also drawn. Some such mechanism is shown to be an anatomical necessity, from the structure of the wrist-joint, to provide for the extremes of adduction aud abduction that take place in the wrist, without straining the joint. Another obvious purpose subserved is equalisation of muscular power, by relegating a part of the work, that the hand muscles would otherwise have to perform, to the larger flexors and extensor of the upper arm; and an actual saving of a certain amount of muscular effort, this being replaced by automatic movements of the bones themselves. Having seen no account of this mechanism, the author is inclined to think it may be unnoticed.* It is is at any rate a new explanation of the design of the peculiar shape and position of the radial articulating surface of a bird's humerus, far more important than that hitherto assigned—viz., its causing simply the well-known obliquity of flexion of the forearm.

* It is indeed not mentioned in the works of Cuvier, Meckel, Tiedemann, Wagner, and other distinguished authors: but Dr. Bergmann, of Göttingen (*Archiv. fur Anat.*, 1839, 296), speaks of essentially the same thing, although the results of the mechanism are not so fully shown. —*Eds. Am. Nat.*

SCIENTIFIC SERIALS

THE number of the *Geological Magazine* for Dec. 1871 (No. 90) contains an unusual abundance of important interesting papers. The first is an article by Prof. Traquair on the genus of fossil fishes to which Prof. Huxley has given the name of *Phaneropleuron*, with the description of new species (*P. elegans*) from the Lower Carboniferous limestone of Burdiehouse. The author describes some new points in the structure of the type-species of this genus (*P. Andersoni*) from the Devonian yellow sandstone of Dura Den), the most important being that the dorsal fin was in that fish continued as a "dorso-caudal" to extremity of the body as in *Lepidosiren* and *Ceratodus Forsteri*. Prof. Traquair gives a restored outline of *P. Andersoni* in accordance with his views, and also figures of two specimens of his new species.—Mr. T. G. Bonney contributes an interesting paper on a double "cirque" in the syenite hills of Skye, with remarks upon the formation of cirques, in continuation of his paper read before the Geological Society some time since.—From Mr. Carruthers we have descriptions of two previously unknown coniferous fruits from the Gault of Folkestone; one of them a magnificent cone, described and figured under the name of *Pinites hexagonus*; the other a smaller form called *Sequoites ovalis*. To this paper the author has appended a note on the structure of the scales of his *Araucarites sphærocarpus*, with some judicious remarks on the caution which ought to be exercised by the student of fossil plants in determining the affinities of the often fragmentary remains with which he has to deal.—Mr. James Geikie publishes a first paper connected with that apparently inexhaustible subject, the climate of the glacial epoch. In this the author discusses the evidence furnished by the glacial deposits of Scotland with regard to the occurrence of warm interglacial periods, during which all or nearly all the snow and ice may have disappeared from the face of the country.—Mr. A. H. Green's notes on the geology of part of the county of Donegal contain an interesting account of the structure of the county, especially with regard to the relations of the granites and stratified rocks and to the glaciation of the surface.—And lastly, Mr. A. J. Browne, from an examination of the valley of the Yar in the Isle of Wight, throws out the suggestion that that valley and the other river-valleys of the island were originally occupied by continuations of the Hampshire rivers before the excavation of the Solent.—Among the miscellaneous notices we may call attention to an article by Prof. T. Rupert Jones and Mr. W. K. Parker on the Foraminifera from the chalk of Meudon, figured by Ehrenberg in his "Mikrogeologie."

Quarterly Journal of Microscopical Science, January.—"Notes of a Course of Practical Histology for Medical Students." given at King's College, London, by Dr. Wm. Rutherford, F.R.S.E., &c. This paper illustrates the author's method of teaching, the students preparing for themselves the series of specimens of the various tissues. After an enumeration of the tissues so prepared follow some general observations on Examination of Tissues, How to Harden Tissues, How to Soften Tissues, How to make Sections of Tissues, How to render Tissues Transparent, How to Stain Tissues, How to Inject, and How to Preserve Tissues, with notes on cells and cements.—"On the Peripheral Distribution of Non-medullated Nerve-fibres," by Dr. E. Klein. Part II. This is the continuation of the paper commenced in the last number of this journal, and to be concluded in the next. It deals with the Nerves of the Nictitating Membrane and Nerves of the Peritoneum.—"Remarks on Prof. Schulze's Memoir on *Cordylophora lacustris*," by Prof. Allman, F.R.S.; "Size of the Red Corpuscles of the Blood of the Porbeagle, or Beaumaris Shark, *Lamna cornubica*," by George Gulliver, F.R.S. The mean long diameter of the corpuscles measured $\frac{1}{923}$ of an inch, and the short diameter $\frac{1}{1500}$, nearly alike in magnitude to those of the small dog-fish and other Selachii.—"A Note on some Circumstances affecting the Value of Glycerine in Microscopy," by Mr. W. M. Ord. This note suggests that from the action of glycerine on murexide and oxalate of lime, mounted for the microscope, it is impossible not to have some misgivings as to the results of its use in the preparation of tissues for the microscope.—"On Remak's Ciliated Vesicles and Corneous Filaments of the Peritoneum of the Frog," by Dr. E. Klein.—"On the Structure of the Stem of the Screw Pine," by Prof. W. T. Thiselton Dyer. Scalariform ducts were detected by the author in the branches of a *Pandanus*, and crystalline forms of two kinds in the tissues.—"On Students' Microscopes," by Mr. J. F. Payne, with a table of English and foreign microscopes, their features, powers, accessory apparatus, and prices.

Journal of the Quekett Microscopical Club, January.—"Notes on Podisoma," by Mr. M. C. Cooke. After describing the minute structure and mode of germination in these fungi, the author proceeds to detail the experiments of Prof. Oersted, from which it has been supposed that the identity of *Podisoma* with *Ræstelia* has been established. The paper concludes with a critical examination of all the known species, one of which it referred to a new genus, and a different order, under the name of *Sarcostroma Berkeleyi*.—"On the so-called Boring or Burrowing Sponge (*Cliona*)," by Mr. J. G. Waller. The object of this paper is to call in question the burrowing proclivities of the sponges belonging to the genus *Cliona* of which *Hymeniacidon celata*, Bowerbank, is the type. This number completes the second volume of the journal.

SOCIETIES AND ACADEMIES

LONDON

Geologists' Association, January 5.—The Rev. J. Wiltshire, president, in the chair. "On the overlapping of several Geological formations on the North Wales border," by Mr. D. C. Davies, of Oswestry. The author stated that the Geological formations of the district ranged upwards from the Llandeilo to the New Red Sandstone. Attention was directed to the way in which nearly every one of these overlapped the one below, hiding in its course many of the beds, amounting in some cases to 1,000 feet of strata, which at other points were exposed. The overlaps increase as a rule from north to south, except in that of the Bala and Caradoc beds by the Llandovery, which increase in an opposite direction. The author inferred that the conformability of strata at a given point did not necessarily prove the unbroken sequence or complete series of the beds at that point, and also that conformability between either two consecutive beds of the same formation, or between those of two distinct formations, was not to be expected to extend over a large area. Amongst other facts stated in this paper was the important one that coal seams occur in Permian strata in the neighbourhood of Ifton. The President remarked upon the enormous time required for the production of the phenomena described by Mr. Davies. Prof. Morris explained the geological and physical features of the district, and spoke of the high value of the paper.—"Report of the Proceedings of the Geological Section of the British Association at Edinburgh, 1871," by Mr. John Hopkinson, one of the deputation from the Geologists' Association. In this communication the author succinctly stated the more important features of the opening address by the president, Prof. Geikie, and of the many papers read before Section C at the meeting at Edinburgh last year, and gave interesting accounts of the two geological excursions under the direction of Prof. Geikie.—Mr. J. T. B. Ives communicated the interesting fact of an extensive bed of peat occurring under gravel between Finchley and Whetstone.—Fossils from the glacial deposits of Islington cemetery were exhibited by Mr. Caleb Evans.

Photographic Society, January 9.—Mr. J. R. Sawyer, in a paper entitled "Photography in the Printing Press," gave an account of the history of mechanical photographic printing. He ascribed to Mungo Ponton the discovery of the action of light upon the bichromates when mixed with certain organic bodies, and to Becquerel the first suggestion of employing gelatine and bichromate in conjunction for photographic printing; but to Poitevin is due the honour of having invented photo-mechanical printing. Mr. Sawyer proceeded to describe the improvements which have since been made, referring to the processes of Tessié de Motay, Lichtdruck, Heliotype, &c. He concluded with a description of photo-collographic printing as now practised.—Mr. J. W. Stillman exhibited and described some new Photographic apparatus.—Mr. Henry Whitfield and Mr. R. Phipps were elected members.

Glasgow

Geological Society, Dec. 14, 1871.—Mr. John Young, vice-president, exhibited specimens of coal from a thin seam, intercalated amidst beds of trappean ash at Glenarbuck, near Bowling, He referred to the discovery, by the late Mr. Currie of Bowling, of thin beds of coal amongst the traps of the Kilpatrick hills at Auchintorlie Glen, which clearly established the carboniferous age of these igneous rocks. He also alluded to his own subsequent observation of thin beds of indurated shale, containing fish remains of carboniferous genera, associated with and overlying one of the seams of coal in the same glen. Since then he had found another thin seam of coal cropping out at a high level in beds of trappean ash on the hillside above Glenarbuck, in the same neighbourhood. In the specimens of the coal exhibited, the woody fibre of the plants in a carbonised condition is clearly distinguishable; and although of a very foul quality, and considerably altered by the heat of the traps amongst which it is imbedded, yet it still gives off a little flame in the burning. From the same ash-bed he had also extracted a portion of the stem of a species of *Sigillaria*, and he believed the greater part of the woody structure observed in this Glenarbuck coal was derived from plants allied to *Sigillariæ* and *Lepidodendra*.—Mr. D. Bell submitted portions of the large pitchstone vein at Corriegills, Arran, and of the sandstone in which it occurs, showing that both rocks are much altered along the lines of contact.

Halifax, Nova Scotia

Institute of Natural Science, November 13, 1871.—"On a Lophioid Fish caught off Halifax Harbour," by Mr. J. M. Jones, F.L.S., president. The little Lophioid fish in the Provincial Museum collection was at first sight regarded by the writer as a Gurnard, but on closer examination it was found to be a Lophioid. The description in the paper, with a figure, were submitted to Dr. Theodore Gill, of Washington, who considered that in the description and figure he recognised the young of the *Lophius americanus* or Sea Devil. It was supposed, however, that the description was slightly defective, and that some characteristic features had been unobserved. The writer did not find the desiderated features in the specimen, and was assured that it never possessed them, as the specimen had been brought to the museum while living and unhurt, and was in the finest state of preservation when examined and described. It was very different from any of the young Lophioids described in Günther's Catalogue, and was, therefore, probably a new Lophioid. The writer referred to two fine specimens of *Lophius piscatorius* lately caught in the Halifax Harbour, one of which had a cod fish in its stomach. He could see no reason for the application of the term *americanus* by American naturalists, as the European and American forms are identical.—On Sir W. Logan and Hartley's Geology of the Precarboniferous Rocks underlying the Picton Coal Field, by Rev. D. Honeyman. Sir W. Logan, in his Report on the Picton Coal Field (*vide* Report of Progress from 1866 to 1869, page 7), says: "No evidence was observed by me on McLellan's mountain to show to what epoch these old rocks belong, but masses somewhat similar are noticed by Mr. Hartley on the west side of East River in a position where they have been mentioned in his Acadian Geology by Dr. J. W. Dawson, who considers them to be of Devonian age, and on his authority they will be so distinguished." By the Devonian colouring of Logan and Hartley's map, which accompanies the Report and illustrates it, it would appear that Sir W. Logan intends that the language should apply to a part of pre-carboniferous rocks in the district of Sutherland River as well as the northern part of McLellan's mountain. Now the rocks of the part of McLellan's mountain range indicated belong to the northern part of one of the great anticlinal Silurian series which extends to the south about nine miles is generally metamorphic and non-fossiliferous. The author was, however, fortunate enough to discover the fossiliferous localities in the series, viz., at Fraser's mountain, the southern extremity of McLellan's mountain, and Blanchard, celebrated in Danzer's Eulogy and elsewhere for its iron deposit. In the former he found Middle Silurian fossils in the western side of the anticlinal, and in the other Middle Silurian fossils on the eastern side of the same anticlinal, of one or both of these Sir W. Logan's Devonian Rocks must be the extension and northern terminus. In this series the author found Lower Heldesberg or Upper Ludlow fossiliferous strata overlying the Clinton and Redina fossiliferous of Fraser's mountain, and this is the most recent of the pre-carboniferous rocks of McLellan's mountain. The other part of Sir W. Logan's Devonian area, the Sutherland river containing the Middle Silurian bend which changes the direction of the Silurians, or connects the N. and S. anticlinals of McLellan and Irish mountains with the Silurians to the east, viz., French River, Barney's River, Antigonish, Arisaig, and Lochaber. In this band there are two monoclinal Middle Silurian series: the one commencing in McLellan's mountain, its greenstone forming Blackwood's mountain, the northern extremity of McLellan's mountain range; overlying this to the south is a metamorphic Medina band. Overlying the greenstone of the second monoclinal on the south is a partially metamorphosed band of Medina age, containing abundance of fossils. The lower part overlying the greenstone at St. Mary's Road contains abundance—beds of Orthids and Athyrus. At Sutherland's River Bridge I found indifferently preserved Lingulæ in the same strata.

Paris

Academy of Sciences, January 2.—After the election of officers and the reading of the report for 1871, M. Delaunay communicated a note on the movements of the perigee and node of the moon.—M. E. Vicaire read a note on the temperature of the solar surface, in which he arrives at the conclusion that this temperature is below 3000° C. (= 5432° F.). M. Faye, M. H. Sainte-Claire Deville, M. E. Becquerel, and M. Fizeau, spoke upon this subject, all of them agreeing in opinion with M. Vicaire. Father Secchi, however, in a third note on the solar temperature, maintained his previous estimate of 10,000,000° C. —M. Chasles read a continuation of his theorems relating to the harmonic axes of geometrical curves; General Morin presented a note by General Didion on the expression of the relation of the circumference to the diameter, and on a new function; and M. Chasles communicated a further note by M. Halphen, on the straight lines which fulfil given conditions.—A note on the electrical currents obtained by the flexion of metals, by M. P. Volpicelli, was read, in which the author enlarged and corrected the results obtained by Peltier and De la Rive.—M. W. Fonvielle read an explanation of the appearance, during balloon ascents, of rings which do not exhibit chromatic decomposition.—A letter was read from M. de Bizeau, of Entre-Monts, near Binche, in Belgium, giving the extreme cold at that place on the 8th December, 1871, at −21·5° C. (= −6·7° F.) at half-past 7 A.M.—M. Pasteur presented a note upon a previous communication of M. Trécul on the origin of lactic and alcoholic ferments, in which he stated that he saw nothing in M. Trécul's results to impugn the exactitude of former experiments or the conclusions which he had drawn from them.—M. A. Trécul read a paper, in which he described the cells of beer-yeast becoming mobile like monads.—M. Berthelot communicated a further paper on the state of bodies in solutions, in which he treated of certain salts of peroxide of iron (sulphate, nitrate, and acetate).—M. Balard presented a third note by M. C. Saint-Pierre on the spontaneous decomposition of certain bisulphites (of lead and baryta).—M. Robin communicated a note by MM. Rabuteau and Massul, on the physiological properties and metamorphoses of the cyanates in the organism, in which the authors state as the result of their researches that the cyanates of potassa and soda are not poisonous, and that in the animal economy they give origin to carbonates.—A note by M. S. Jourdain, containing materials towards the history of *Gymnetrus gladius*, was presented by M. Blanchard. The author describes the anatomy of a specimen of this rare fish, which was stranded near Palavas (in Hérault.)—A note on the heat absorbed during incubation by M. A. Moitessier was communicated by M. Balard. The author finds that the specific heat of fecundated is less than that of unfecundated eggs when treated in the same manner, and infers that a portion of the heat absorbed by the former during incubation is transformed.—M, Decaisne presented a note by M. A. F. Marion on the fossil plants of Ronzon in the department of the Haute Loire. The flora of the marly limestones of Ronzon includes only fifteen species belonging to the same number of genera; eleven of the species are said to be new. These belong to the genera *Equisetum*, *Podostachys*, *Myrica*, *Celtis*, *Litsæa*, *Bumelia*, *Myrsine*, *Pistacia*, *Mimosa*, *Echitenium*, and *Ronzocarpum*. The facies of the flora is African or Asiatic.—A note by M. Bleichen on the discovery of *Posidonia minuta* in the Trias of the department of the Gard, and on a deposit of schists containing *Walchia* in the Permian formation of Aveyron, was presented by M. de Verneuil; and a note by M. Sanson on an equine skull from the turbaries of the Somme by M. de Quatrefages. The author of the last-mentioned paper refers the skull obtained by Boucher de Perthes from the ancient turbaries of the Somme to the African variety of the common ass.

January 8.—M. Martin de Brettes presented a memoir on the motion of oblong projectiles in resisting media, and on the explanation of the wounds produced in living creatures by the oblong balls of rifled guns.—M. E. Rolland read a memoir on the effects of variations of work transmitted by machines, and on the means of regulating them.—Three letters from M. Janssen were read, giving an account of the position selected by him at Sholoor, in Neilgherry Hills, for the observation of the solar eclipse of Dec. 12, and a brief statement of his results, the latter will be found in another column.—M. S. Meunier read a note on the transition types in meteorites. In this paper the author indicated certain transitions between the constituents of meteorites analogous to those occurring in terrestrial lithology—namely, between luceite and montrejite, mesminite and canellite, montrejite and limerickite, montrejite and stawoopolite, and between aumalite and tadjerite.—A memoir was presented by M. C. A. Valson on a relation between capillary actions and densities in saline solutions, in which he showed by a table of results that in nearly all cases the amount of capillary action is dependent on the density of the fluid.—M. H. Sainte-Claire Deville presented a note by MM. Troost and P. Hautefeuille on the action of heat upon the oxy-chlorides of silicium.—M. Berthelot read the conclusion of his memoir on the state of bodies in solutions, which related to persalts of iron.—M. S. de Luca communicated some investigations of a complex alum, obtained from the thermo-mineral water of the Solfatara of Puzzuoli; it consists of sulphuric acid combined with alumina, ammonia, protoxide and sesquioxide of iron, lime, magnesia, and potass, with traces of soda and manganese.—A note by M. D. Tommasi on the action of iodide of lead upon some metallic acetates was read.—M. Dubrunfaut presented a note on the combustion of carbon in carbonic acid in presence of water, in which he indicated the importance of the presence of aqueous vapour in many phenomena of combustion. M. Dumas spoke in opposition to to the views of Dubrunfaut.—M. Pasteur communicated a note by M. J. C. de Seynes on the asserted transformations of Bacteria and Mucedineæ into alcoholic ferments; and M. F. Béchamp a paper on the development of alcoholic and other ferments in fermentescible media, without the direct intervention of albuminoid substances.—M. Boussingault presented a note on saccharine matter which appeared in the leaves of a lime tree.—The author stated that the saccharine fluid observed by him was not, as is generally supposed, the production of *Aphides*, but apparently a morbid secretion of the tree; it was found to be identical in saccharine constitution with the manna from Sinai analysed by Berthelot.—M. C. Dareste read a note in which he described the presence of bodies presenting the characters of starch-grains in the testes of birds, before the appearance of the spermatozoids.—M. Decaisne presented a note by M. J. E. Planchon, on the characters and systematic position of the Chinese spiny elm (*Hemiptelea Davidii*); and M. Daubrée some observations by M. H. Magnan, on two recent notes by M. Cayrol, on "The Lower Cretaceous formation of La Clape and Les Corbières."

BOOKS RECEIVED

ENGLISH.—Text Books of Science; Arithmetic and Mensuration: C. W. Merrifield (Longmans).—The Elements of Plane Geometry, 2nd edition: R. P. Wright (Longmans).—Concerning Spiritualism: Gerald Massey (Burns).—Catalogue of Transactions, &c, Radcliffe Library, Oxford.

AMERICAN.—Approved Plans and Specifications for Ports, Hospitals, &c., —Reports on Barracks and Hospitals, &c.—Elements of Chemistry and Mineralogy, Vol. ii.: J. Hinrichs.

DIARY

THURSDAY, JANUARY 18.

ROYAL SOCIETY, at 8.30.—Investigations of the Currents in the Strait of Gibraltar, made in August 1871, by Capt. Nares, of H.M S. *Shearwater*: Admiral Richards, F.R.S.—On the Absolute Direction and Intensity of the Earth's Magnetic Force at Bombay, and its Secular and Annual Variations: C. Chambers, F.R.S.

SOCIETY OF ANTIQUARIES, at 8.30.—On Neolithic and Savage Implements: A. W. Franks, M.A., and Col. A. H. Lane Fox.

CHEMICAL SOCIETY, at 8.

ROYAL INSTITUTION, at 3.—On the Chemistry of Alkalies and Alkali Manufacture; Prof Odling, F.R.S.

LINNEAN SOCIETY. at 8.—On the Anatomy of the American King-Crab (*Limulus polyphemus*, Lat.): Prof. Owen, F.R.S. (*Continued.*)

FRIDAY, JANUARY 19.

ROYAL INSTITUTION at 9.—On the new metal Indium: Prof. Odling, F.R.S.

SATURDAY, JANUARY 20.

ROYAL INSTITUTION, at 2.—On the Theatre in Shakespeare's Time: Wm. B. Donne.

SUNDAY, JANUARY 21.

SUNDAY LECTURE SOCIETY, at 4.—On King Arthur; the legend and its significance in relation to English life, past and present: Sebastian Evans.

MONDAY, JANUARY 22.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

VICTORIA INSTITUTE, at 8.—On the Influence of Colloid Matters upon Crystalline Form: Dr W. M. Ord.

ENTOMOLOGICAL SOCIETY, at 7.—Anniversary Meeting.

LONDON INSTITUTION, at 4. Elementary Chemistry: Prof. Odling, F.R.S.

TUESDAY, JANUARY 23.

ROYAL INSTITUTION, at 3.—On the Circulatory and Nervous Systems: Dr. W. Rutherford, F.R.S.E.

WEDNESDAY, JANUARY 24.

GEOLOGICAL SOCIETY, at 8.—On the Foraminifera of the family Rotalinæ (Carpenter) found in the Cretaceous formations, with Notes on their Tertiary and Recent Representatives: Prof. T. Rupert Jones, F.G.S., and W. K. Parker, F.R.S.—On the Infra-Lias in Yorkshire: Rev. J. F. Blake, F.G.S.—Further Notes on the Geology of the Neighbourhood of Malaga: M. D. M. d'Orueta.

SOCIETY OF ARTS, at 8.—On Improvements in the Process of Coining: Ernest Seyd.

ROYAL SOCIETY OF LITERATURE, at 8.30.—On Excavations at the Site of the Homeric Pergamus: Dr. J. G. Von Hahn.

THURSDAY, JANUARY 25.

ROYAL SOCIETY, at 8.30.

SOCIETY OF ANTIQUARIES, 8.30.

ROYAL INSTITUTION, at 3.—On the Chemistry of Alkalies and Alkali Manufacture: Prof. Odling, F.R.S.

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NOTICE

We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception. Communications respecting Subscriptions or Advertisements must be addressed to the Publishers, NOT *to the Editor.*