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THURSDAY, JANUARY 26, 1871

## *PHYSICAL LABORATORIES*

IT is well known that chemistry can be taught far better by a laboratory in which the student performs the various experiments, than by any system of lectures. Now, although for many years physicists have been in the habit of instructing their special students and assistants in this way, yet it is only recently that the same plan has been tried with large classes in physics. One of the first institutions to attempt this method, in America at least, was the Massachusetts Institute of Technology in Boston; and as I find many colleges here establishing physical laboratories, I trust that our experience may prove of some interest. The great difficulty is to enable twenty or thirty students to perform the same experiment without duplicating the apparatus, and to avoid the danger of injury to delicate instruments. Our plan is this:—Two large rooms (one nearly a hundred feet in length) are fitted up with tables, supplied with gas and water, somewhat like a chemical laboratory. On each is placed the apparatus prepared for a single experiment, which always remains in this place, thus avoiding the danger of breaking it in moving. A full written description is also given of each experiment, pointing out the proper precautions to avoid error or breakage. Near the door is an indicator or board containing the names of the experiments, and opposite each is placed a card bearing the name of the student. When the class enters the laboratory, they go to the indicator, and each member notices what experiment is opposite his name; he then goes to the proper table, reads the description, and performs it. He next reports his results to the instructor in charge, and if they are correct, his card is moved to some unoccupied place, and he proceeds as before. Care is taken that the number of experiments shall exceed that of students, and there is therefore no delay. The instructor in the mean time is enabled to pass from student to student and to see that no errors are committed. As quantitative work is far more valuable than qualitative, most of our experiments are of the former kind, and the student learns to measure physical constants and to verify laws numerically. For example, in one experiment a steel bar is supported on knife edges, and a weight is applied at the centre. The flexure is then measured by a micrometer screw, the exact point of contact being determined by including the screw and bar in the circuit of a battery and galvanometer. After making a number of experiments with various weights, the student constructs a curve in which ordinates represent deflection, and abscissæ weights applied. The law of elasticity shows that this curve should be a straight line, and the close agreement is convincing proof to the student of its correctness. In the same way the law of the conjugate foci of lenses is tested, and the observed curve compared with that deduced from theory. Some experiments are introduced to accustom the student to general methods of research, such as the computation of probable error by least squares, various forms of interpolation, &c. The graphical method is largely used, as it at the same time enables the student to take in all his observations at one glance, while the instructor can constantly tell how carefully the work has been done. For the microscope a few objects are selected to show certain general methods of using this instrument, as one requiring a diaphragm, a second oblique illumination, and so on. Again, the student views by polarised light such objects as unannealed glass, crystals, designs in selenite, and studies the effects produced by various agencies. By thus handling the instruments he acquires a facility in using them and comprehension of their construction which he could never obtain from lectures. The excellence of the work done by many of the students led to the hope that valuable results might be attained by assigning to different students the experiments in a research, taking care that each should be repeated several times by different individuals. These results, if concordant, would be much more conclusive than those obtained by a single experimenter, since they would be free from all personal bias. In this way some interesting results have been attained on the foci of lenses placed obliquely, the flow of air through straight and curved tubes, and other similar subjects. Photometry and electrical measurement seem especially suited to this purpose, and the application of the latter subject to submarine cables would be both interesting and instructive to the student. During the winter time of 1869 and 1870 about sixty students worked in one laboratory, so that the experiment was tried on a sufficiently large scale to enable us to speak with confidence of its success. We found the system described above worked well, the students were interested in the subject, and obtained results of considerable accuracy. The loss by breakage was exceedingly small, and the current expenses insignificant compared with a chemical laboratory, since there is but little consumption of the materials employed.

There are now in America at least four similar laboratories either in operation or preparation, and the chances are that in a few years this number will be greatly increased. The value of a knowledge of physical manipulation is becoming daily better appreciated, and it is evident that instruction of this kind can be properly given only in a physical laboratory.

EDWARD C. PICKERING

## *SCIENCE TEACHING IN PRIVATE SCHOOLS*

A WRITER of the early part of last century defined a philosopher as one "whose trade was to do nothing, and to speculate upon everything." While philosophers were so lightly esteemed, it is no matter of surprise that philosophy was little cared for as a part of education. But such a definition as the above would not now be generally accepted even by the unscientific public. All are beginning to see that it is to Science they are indebted for so many of the comforts and advantages of civilisation, yet to the many is Science a mystery and closed book. And one great cause of this we believe to be, because it is not taught in our schools.

We purpose, in the present article, to speak only of private schools. If a visitation were to be made of such schools in England, we venture to say that very few, comparatively, would be found, in which Science, in any of its branches, is made a subject of regular education. The boys of most schools would be classed by the masters under three heads, classical boys, mathematical boys, and good-for-nothing boys. This last class exists mainly because the proper food for them has not been provided, they are allowed to starve for lack of it, and grow up as men with stunted and impoverished intellects; they have not been educated, the powers of their minds have not been drawn out by the fit means, and they pass through the world as animated failures.

Let Science work side by side with Classics and Mathematics—not usurp their places—in the work of education, and the good-for-nothing class will be very sensibly diminished, if indeed it be not entirely done away with. But how is this to be done? In the first place, gradually; in the second place, zealously; in the third place, thoroughly. Gradually, because it is a new thing, and a large proportion of our private schoolmasters have had no regular training in science themselves. Zealously, because if a teacher be not himself interested in what he teaches, he can have no assurance of success, and no encouragement from his pupils. Thoroughly, because a thing worth doing at all is worth doing well.

This is the spirit in which the work must be done, but what are the special means? Are boys to read about Science merely, or are they to touch and handle Science for themselves? It is doubtless a good thing to read about the truths of Science and their experimental illustration; it is a better thing to see those truths illustrated and proved by another; but it is by far the best thing to experiment upon and prove the truths by one's self. There is nothing that comes home so much to a boy's mind as an experimental proof. He may read of the dual character of electricity, and may get some vague ideas on the subject; as soon, however, as he takes two sticks of sealing-wax, suspends one, rubs both, and brings one near to the other, he, as it were, discovers for himself that the same bodies electrified by the same means repel one another, and on experimenting with glass and sealing-wax sticks he learns something of attraction, and is naturally led on from experiment to experiment until the powers of his mind become quite drawn out, or educated in the pleasurable pursuit of the subject. In the Science-teaching of boys, then, practical demonstrations must play an important part. Let reading, hearing lectures, and attending classes, and individual experimentation, be the working tools. A lecture of itself is but a poor tool, it produces an effect for the time, but in many cases no very permanent good. A lecturer must also be a teacher out of lecture hours.

As a commencement of Science-teaching in schools we commend the following plan to the notice of Science-teachers. Let one or two evenings in the week be set apart for lectures on some branch of Science, Experimental Physics, Botany, or Geology. Each lecture to last *not more* than an hour, and to be experimentally illustrated in the way best suited to the subject, always bearing in mind that the simplest experiments, or those most easily imitated by the pupils, are the best. Let the pupils be encouraged to take notes, and let the lecturer sum up in a concise form at the end of the lecture the main points established, which may be written in the form of memoranda on a black board and copied by the pupils. A day or two after the lecture let him hold a conversational class, the attendance optional. He will then briefly run over the matter of the last lecture, find out by questioning what points are not thoroughly understood, re-explain or even re-experiment, and endeavour to leave each mind with a perfect understanding of fundamentals. On a third evening let him give a series of simple—not needlessly puzzling—examination questions, and look over each boy's answers with himself alone, if possible, in order to give an opportunity for a yet more thorough explanation of any difficult point suited to the individual capacity of each. Instead of a string of questions, a subject for an essay in connection with the lectures might occasionally be given. Private reading of text-books should always be encouraged.

Now, as to the results of a system of this kind, taking such a subject as Experimental Physics, it will be found that the lectures are always looked forward to with no ordinary pleasure, and are listened to with no ordinary attention. At the conversational classes the way in which such subjects teach boys to think is often clearly seen; they ask most puzzling questions, yet natural ones, and in many cases seek to go far deeper into the subject than the lecturer had at first any idea of leading them. This general interest incites them to read and perform such simple experiments for themselves as are within their power.

Generally speaking it is not the high classical or even the mathematical boys that have excelled in Science learning, but precisely those who before occupied no prominent place in the school, had no special gift for classics or mathematics, and were considered, more or less, good-for-nothings. And here it is important to remember that a person may have a mathematical mind without being a mathematician.

While such subjects as Chemistry and Physics claim, perhaps, the highest position as a means of scientific education, it is important to vary the programme as much as possible without treating any superficially. Thus Astronomy, Geology, Physiology, and Botany have strong claims. It is certainly most deplorable to think that even now in many of our private schools the pupils are being tacitly taught that the world was made in six days, and that man is but some 6,000 years old. They might as well learn that there are but four elements—earth, air, fire, and water. We look with confidence for better things in the future.

## *COHN'S CONTRIBUTIONS TO THE BIOLOGY OF PLANTS*

*Beiträge zur Biologie der Pflanzen.* Herausgegeben von Dr. Ferdinand Cohn. Erster Heft. Mit sechs zum theil farbigen Tafeln. (Breslau, 1870. London: Williams and Norgate.)

THIS is the first part of a new periodical established primarily for the publication of the results of the observations made at the Botanico-Physiological Institute at Breslau. The part contains five papers on different microscopic algæ and fungi, and their pathological effects. In subsequent numbers it is intended to give priority to botanical observations which relate to biological questions, or which are more or less connected with practical natural science, medicine, agriculture, &c. It is hoped that the publication may fill the place formerly occupied by Karsten's "Botanische Untersuchungen."

We propose here to give a short account of the contents of the present number. The first paper (by Dr. J. Schroeter) is on the parasites of the genus *Synchytrium*. The *Synchytria* form a small group of parasitic cryptogams, remarkable for their peculiar mode of development and the absence of any mycelium. They produce small swellings, or "galls," on the plants in which they occur, but in comparison with other parasites cause little injury or distortion. The genus was established by De Bary and Woronin in 1863 for the reception of the plant now known as *Synchytrium Taraxaci*. Several other species have since been discovered, the number of those described by Dr. Schroeter in the present paper amounting to eleven. The plants were divided by Woronin into two groups. In the first the protoplasm of the cells is white, and the zoospores pass at once into the condition of resting spores. In the second group the protoplasm is reddish, and the zoospores, attaching themselves to living plants, produce spherical masses of zoosporangia until the close of the vegetative period, when they produce resting spores. Dr. Schroeter proposes three groups, which he calls *Eusynchytrium*, *Chrysochytrium*, and *Leucochytrium*. The first corresponds to Woronin's second group, the second to Woronin's first group, and the third comprehends certain *Synchytria* which agree with those of Woronin's first group except in having white protoplasm. Dr. Schrœter describes the species with great care, and in considerable detail, and the descriptions are for the most part illustrated by excellent plates. He considers the genus to be one of wide distribution. With regard to its systematic position, there has never been any doubt that *Synchytrium* belongs to the *Chytridiaceæ*, but whether the latter are fungi or algæ is not so well settled. The conclusion at which the author has arrived is that they should be placed as a distinct family amongst those *Palmellaceæ* which produce zoospores, being nearly related to *Hydrocytium*, *Codiolum*, and their allied genera.

In the second paper ("Ueber die Fäule der Cactusstämme") MM. Lebert and Cohn describe a new species of *Peronospora*, which was most destructive to a collection of cactuses, affecting particularly *Cereus giganteus* and *Melocactus nigrotomentosus*. The epidermis of the cactus was not materially altered, but the cellular tissue beneath became entirely rotten, the decay beginning with the inter-cellular substance: so that the parenchymatous cells were easily separable from one another. The contents of these cells became brown, and the cell-membrane almost entirely dissolved, the cactus thus exhibiting an internal state of decay similar to that seen in diseased potatoes. The fungus is closely allied to the potato blight, *Peronospora infestans*, which it resembles in having the hyphæ only slightly ramified, not dichotomous and therefore producing but few conidia, as well as in its delicate mycelium (which has no suckers) and in the swellings underneath the large, beaked conidia. The authors suspect that the fungus must have been imported with the cactuses from America.

The third paper, by Dr. Cohn, is entitled "Ueber eine neue Pilzkrankheit der Erdraupen," and contains a detailed account of a fungoid disease affecting the caterpillars of *Agrotis segetum*. The skin turns black, a coal-black pigment appears in the blood, and the caterpillar becomes a wrinkled and brittle mummy. Upon examining the interior the whole cavity of the body, except the intestinal canal, is full of a black tinder-like substance, consisting of very large, dark brown spores, which are globular, but sometimes wrinkled at the surface, so as to present a crenate outline under the microscope. Contemporaneously with the colouring of the blood, cylindrical or curved tubes (*Schläuche*) are seen, which become divided by septa, and form rows of cells which separate from one another and appear in the blood as free globular or oval cells. These Dr. Cohn calls *gonidia*. These gonidia germinate shortly before the death of the grub, and produce a mycelium which displaces the inner organs, except the intestinal canal and the tracheæ, and fills the hollow of the body. This mycelium produces partly new gonidia and partly the dark spores already mentioned. The latter are probably resting-spores, as it seems from Dr. Cohn's observations that they do not germinate until the spring. The fungus (to which the author gives the name *Tarichium*) appears to be generically the same as Fresenius's *Entomophthora*, and to be closely allied to *Empusa*. Dr. Cohn thinks it not improbable that *Empusa* and *Tarichium* are stages of development of the same fungus; that *Empusa* may be the conidial form of a fungus of which *Tarichium* represents the teleutospores. If so, *Empusa* bears the same relation to *Tarichium* as *Oïdium* does to *Erysiphe*, *Uredo* to *Puccinia*, and (perhaps) as the *epi*phytal conidia of *Peronospora* to the *endo*phytal oospores.

We have but little space to notice the two remaining papers. One is by Dr. Schroeter, and relates to a disease affecting *Pandanus*, which was observed many years ago by Sinnig, the Inspector of the Botanic Garden at Poppelsdorf, and which has since been investigated by Bouché, at Berlin. In the spring of the present year it attacked a splendid specimen of *Pandanus odoratissimus*, Jacq., in the gardens at Breslau; the branches decayed, and it was found necessary to cut off the crowns, afterwards the branches themselves, and eventually the greater part of the entire plant, leaving only a portion of the stem and a single branch. A strict investigation afforded no ground for supposing that the disease had been produced by cold, drip, or other causes which would suggest themselves to cultivators, but a fungus was discovered identical with one described by Leveillé as long ago as 1845, which occurred in the Botanic Garden at Paris upon *Pandanus*, and which he called *Melanconium Pandani* (Ann. d. Sciences Nat. ser. iii. t. 3, p. 66). The *Melanconium* in the present case was accompanied by a *Nectria*, which Dr. Schroeter considers to be the *Nectria Pandani*, Tul., and conidia were also observed, the mycelia of which exhibited the forms of *Tubercularia*, *Stilbum*, and *Verticillium* or *Penicillium*. The author considers it extremely probable that the *Melanconium*, the *Nectria*, and the conidia are produced from the same mycelium, the fungus thus exhibiting the following forms of fruit:—1. Grey-green conidia (*i.e.* the *Melanconium* fructification). 2. Colourless conidia, the supports of which assume three different forms, viz. (*a*) *Tubercularia*, (*b*) *Stilbum*, (*c*) the form of such moulds as *Verticillium* or *Penicillium*. 3. Spores in asci formed in orange-red perithecia seated on a *Stroma*.

Dr. Schroeter concludes that there can hardly be a doubt that the fungus is the direct cause of the progress and of the destructive operation of the malady, and that it is probable, though not so certain, that the spores germinate in the healthy stem, and actually produce the disease.

The last paper contains a description by Dr. Cohn of a plant which he has discovered in well-water at Breslau, and to which he has given the name of *Crenothrix polyspora.* The genus is new and is closely allied to *Chamæsiphon*, being intermediate between that and *Lyngyba.* It would take too much space to describe the plant at length, for the particulars of which we must refer to the paper, in which will be found, moreover, some interesting observations upon the microscopical analysis of well-water in general. The *Crenothrix* was first noticed in water from a well at Breslau, in a part of the town notorious for the prevalence of typhus. It has been found also in other wells of bad reputation, but whether it has any injurious effect upon the health of the dwellers in the neighbourhood of the wells in question Dr. Cohn cannot venture to say.

It will be seen, from what has been said, that the periodical under notice is well deserving the attention of botanists and physiologists, and from the reputation of its editor there is every reason to hope that the scientific interest of future numbers may equal that of the present one.

F. CURREY

## OUR BOOK SHELF

*Matheran Hill, its People, Plants, and Animals.* By J.Y. Smith, M.D. (Edinburgh: Maclachlan and Stewart.)

THIS is perhaps the first attempt that has been made to give a comprehensive account of the natural history of any particular spot in our East Indian possessions, and we welcome this little book as a sample of what may be accomplished by residents there in the midst of their official occupations. The undertaking is worthy of all praise, and, as far as it goes, it is a valuable contribution to the ethnology and natural history of India, and will no doubt be the groundwork for further research, and lead to other similar works.

The hill of Matheran is within twenty miles of the coast of Bombay, it is basaltic, and rises in the centre of a vast plain to the height of about 2,600 feet, it is precipitous on all sides, flat on the summit, which has an area of about five square miles, and is clothed with luxuriant vegetation; it is, in short, one of the most charming spots in India. In less than three hours one can be transported from the heat, dust, and noisy traffic of Bombay to what appears to be another world, where the body is refreshed and invigorated by the pure mountain air, and the spirit soothed by the beauty of verdant foliage, the cheerful music of feathered songsters, scenery grand and picturesque, and the general repose of nature To the naturalist Matheran opens a grand volume to him, the little plateau has an air of enchantment, and he has spread out before him in the most attractive form objects which will supply him for even years with delightful and instructive occupation without the toil and exposure of long journeys. To such Dr. Smith's account of the more prominent objects to be met with will therefore be a valuable boon, and, indeed, to all lovers of nature who may visit the place. Let the home botanist imagine himself in a pretty little cottage on Matheran, with no less than seventy-five flowering trees and shrubs within a mile of his residence; not to mention climbing plants, creepers, herbs, parasites, and ferns in abundance. As the author is now returning to the East, we hope he will have opportunities of extending his interesting observations, especially on the birds and insects. To the geologist, Matheran is an object of interesting study; he will there see that curious rock called laterite or iron stone clay, the nature of which has been so much disputed, capping the great basaltic formation (to use a homely phrase) like the sugar on a Christmas cake, and if he extends his observations to the north and south, he will find the same capping on other hills, thirty to sixty miles distant, while no trace of it exists in the interval between. To what bold speculations does this fact give rise? Did this laterite once cover the whole country as with a mantle, and are present appearances due to a vast denudation of hard trappean rock 2,000 to 3,000 feet deep?

We are glad to find natural history included among the subjects for Indian Civil Service examinations, for hitherto this class of Europeans in India have contributed comparatively little to our knowledge of that wonderful land "where all, save the spirit of man, is divine."

*A Voyage Round the World.* By the Marquis de Beauvoir. 2 vols. (Murray, 1870.)

THIS is the gossiping journal of a young companion of the Duc de Penthièvre, son of the Prince de Joinville, often amusing and spirited, but of little permanent value. We have the usual exaggerations of a novice in the tropics. At Batavia he speaks of "this torrid temperature of 104° in the shade," a degree of heat never experienced by the present writer during many years' residence in those regions. The author's scientific attainments may be estimated by his account of his visit to the Melbourne Museum, when he makes Prof. MacCoy speak in this fashion:—"The stratum of alluvial soil covering the crust of primitive rocks, which formed round the earth while it was still in a liquid and incandescent state, possesses the same specific type of animal life that characterises the ancient strata of Wales, Sweden, and North America. Then come soils identical with those of these countries, schist and fossil rocks; thus Canada, Scotland, and the province of Victoria have all passed through the same form of existence at this remote period." The countries described are Australia, Java, Siam, and Canton, and the whole journey occupied about six months.

A. R. W.

*The Student and Intellectual Observer.* A Quarterly Journal of Science, Literature, and Art. Vol. 5. (London: Groombridge and Sons.)

THE volume now before us is in every way worthy of the reputation of its predecessors. Four papers on poison are contributed by Mr. F. S. Barff, and Dr. Carpenter contributes two interesting papers on the "Deep Sea," the first on its physical, and the second on its biological condition. The author's experience in these matters, owing to his connection with recent explorations, make the papers the more interesting, because they are the words of an actual and accurate observer. Dr. Collingwood also gives a very readable paper on a kindred subject, "The Sargasso Sea and its Inhabitants," in which the Sargassum or Gulf-weed comes in for a good share of attention, being, as it is, the home of multitudes of Polyzoa, Polyps, Crustacea, Molluscs, and similar creatures. Mr. Shirley Hibberd talks about Cycads under the very misleading title of "Sago Palms." At one time the Sago of commerce was supposed to be the produce of the Cycads, but now we know that the bulk of this useful article is yielded by two or more species of *Sagus*, true palms; it is, to say the least, advisable that an old term proved to have been wrongly given, should not be perpetuated. The author, however, does attempt to qualify its use in the following sentence:—"By 'Sago Palms' is to be understood the great group of gymnospermous plants, of which the Cycads and their allies are representatives, a group possessing powerful morphological relations, and, of course, a correspondence within certain limits in all their biological characteristics." The volume contains many other interesting papers in various branches of science, and we conclude this short notice by wishing well to an old-established monthly in its new quarterly form.

## 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 Isolation of St. Michael's Mount

MR. PENGELLY, in a letter addressed to you, and printed in your journal of Jan. 12, 1871, complains of my having ascribed to him a belief in the extreme antiquity of the Cornish language which he does not hold now, and which he did not hold at the time when he delivered his first lecture "On the Isolation of St. Michael's Mount," at the meeting of the British Association at Birmingham in 1865. He declines to be responsible for any notices or report of his lecture that I may have seen in some newspaper or journal.

All I can say in my defence is that even while the meeting at Birmingham lasted, I received not only newspaper reports, but letters from friends who had heard the lecture, and who asked me in great dismay whether it was possible that a Cornish name, such as *Cara clowse in cowse*, meaning "the hoar rock in the wood," could have existed in so called prehistoric times.

The question discussed at the meeting, so far as I could understand it from letters and the official short report in the Transactions of the British Association, was this, whether St. Michael's Mount was severed by encroachment or subsidence. Those who held the former view required 20,000 years, those who held the latter were satisfied with a smaller number of years, though I could not find out exactly what that number was. Both parties maintained that Cornish must have been spoken in Cornwall before the severance of the Mount took place, because only before that severance could the Mount have been called *Cara clowse in cowse*, "the hoar rock in the wood."

What I wanted to show was simply this, that neither party could properly avail itself of the linguistic argument, whether for positive or negative purposes. If the Mount was severed 20,000 years ago, it would not follow from the name *Cara clowse in cowse*, "the hoar rock in the wood," that Cornish was spoken at that distant time; nor would it in the least follow from that name that the severance could not have taken place until Cornwall was occupied by Celtic speaking tribes. The linguistic and geological evidence can in no wise be brought to bear upon each other.

If I said that "Mr. Pengelly has somewhat modified his former opinion," all I meant was that in his seeond paper he himself seems much less inclined to trust to the linguistic and legendary evidence. But if in his letter to you he says that the point of his argument was that the hypothesis of insulation by encroachment without subsidence could not be admitted because it led to an untenable philological conclusion, this shows that the old leaven is still at work. If the facts which I collected in my essay on the Insulation of St. Michael's Mount are right, that hypothesis would lead to no untenable philological conclusion whatever, for the simple reason that the name *Cara clowse in cowse*, or "hoar rock in the wood," referred originally to Mont St. Michel, in Normandy, if not to Mons Garganur in Apulia, and does not occur in Cornwall before the 16th, or possibly the 15th century of our era.

If I have in any way misrepresented the exact geological reasoning of Mr. Pengelly, all I can do is to plead the ignorance of a layman, and to ask his forgiveness.

Oxford, Jan. 23 MAX MÜLLER

### Earth-Currents

IN Mr. W. H. Preece's communication concerning the earth-currents which occurred on the 24th and 25th of last October in England, published in your issue of the 3rd of November, just come to hand, he says: "This is only a sample of what occurred simultaneously all over England, and probably the globe."

The following few extracts from the Log of the Madras-Bombay Lines show what was taking place out here:—

"Oct. 24, 22 hours, working Bellary with great difficulty. Severe lightning taps on instrument every now and then." "Oct. 25, 8 hours, strong earth-currents at times.—17 hours to 17·45 hours, very strong earth-currents.—22 hours, working Bombay with frequent stoppages, owing to strong earth-currents and failure of signals." Such is the character of the log throughout the 24th and 25th of October.

The direction of the Madras-Bombay Lines (nearly east and west) would account for the fact of the earth-currents being so strongly pronounced in them.

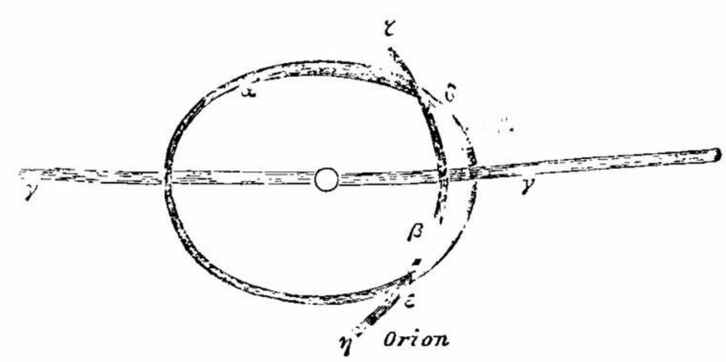
R. S. BROUGH,
Assistant Superintendent of Government Telegraphs in India

### Lunar Bows

A REMARKABLE phenomenon was visible at Liverpool from 7.30 to 7.45 P.M., on Wednesday, 4th inst.

The moon was nearly full at an altitude of 45° or 50°, just above Orion, the sky was covered with a slight mist sufficiently dense to obscure all stars except those of the first magnitude, though here and there some of the lesser were visible.

There appeared three lunar bows or halos—$\alpha$ $\beta$ $\gamma$ in sketch. was nearly but not quite a perfect circle, having a slight tendency to an oval; it was complete. $\beta$ was an excentric cutting $\alpha$ at points

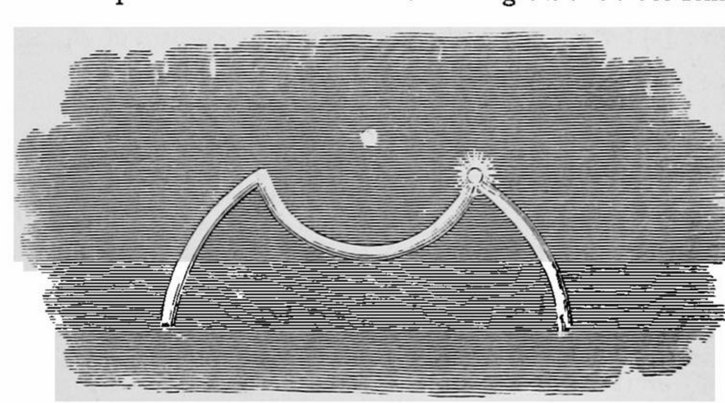



$\delta$ and $\epsilon$, and was incomplete, only having about 90° of its circle visible, being lost to sight at $\zeta$ and $\eta$. $\gamma$ had the zenith for its centre, cutting the moon; its circle was complete, intenser in the western sky and dimmer at its nearer proximity to the moon, thus forming a belt round the whole heavens, cutting off the upper portion.

I venture to consider these interesting sights worthy of a space in NATURE, as they took up so large a space in the heavens.

Liverpool F. J. J.

ON Wednesday evening, January 4th, while the frost still lasted, there appeared, about 7.25 P.M., two brilliant halos, of which a sketch is forwarded. Neither was perfect in outline. The greater spread out literally from the smaller, and made a circuit of the heavens, so that if an observer stood with his back to the moon, there was seen facing him in the N.W an inverted lunar rainbow—to all appearance. This enormous arc scarcely seemed a part of a circle on account of the great size: the zenith



was about its centre. After meeting the smaller halo it was scarcely visible, but it produced would have passed through the moon's disc. At the point of intersection towards the east, a faint paraselene appeared.

When this singular sight was seen, there was but little cloud in the sky. The blue was, however, rather turbid. The prismatic colours were tolerably distinct in the inner arc. The diameter of the large circle was about 90°. There has been a considerable amount of rain and snow since the appearance.

SAMUEL BARBER

Aigburth, Liverpool, Jan. 9

## Yellow

In the paper on colour read by Mr. Strutt before the British Association (Nature, Jan. 19) two things are stated as requiring explanation (p. 238), both of which, it seems to me, are explained by one of the results furnished by such experiments as his own.

The first is the difficulty usually found in recognising the demonstrated fact that yellow is a compound colour. The other is that we generally distinguish different kinds of yellow more strictly than of other colours. "A dark yellow or orange . . . . suggests its colour so little as to be called by a new name (brown), while a dark blue is blue still;" upon which I must observe that we thus distinguish degrees of impurity rather than degrees of darkness, and that an impure yellow is called brown when it is dark, and drab when it is light.

Both things, however, are explained if it is true that *natural yellows differ less from the nearest colours of the spectrum than other natural colours do.* For, in the first place, the consequence will be that there will be many yellows in nature which we could not compound by our ordinary reds and greens, and we therefore find it difficult to imagine it could be compounded by any red and green. Secondly, there will be a greater relative range, so to speak, of yellowness, which we shall naturally subdivide according to degrees of purity and brightness. It may be added that, so far as brightness is concerned, the greater maximum brightness of yellow would act in the way last described.

But is it true that our yellows differ less from the nearest colours of the spectrum than our other colours do? I certainly think this is the result of the experiments, but I will only show that it is the case with the pigments employed by Mr. Strutt. He says, indeed, that "the most saturated yellow can be compounded of red and green." So it may in the spectrum (Maxwell, *Phil-Trans.*, 1860, p. 57—84); but Mr. Strutt's yellow cannot be compounded of Mr. Strutt's red and green. On the contrary, we see from his last "calculated" equation but one, that his yellow had to be diluted with nearly two-thirds as much of his white (the brighter of the two, I presume) before it could be matched by his red and green.

We can test the matter more closely. Denoting Mr. Strutt's red, green, and blue by $r$, $g$, $b$, and the "primary" red, green, and blue of the spectrum by $R$, $G$, $B$ (Maxwell, *ubi supra*, p. 74), we may thus express the former in terms of the latter:—

$$\left.\begin{array}{l} r = LR + mG + n'B \\ g = l'R + MG + nB \\ b = lR + m'G + NB \end{array}\right\} (1);$$

where we know thus much about $L$, $m$, &c.; first that they are all positive, or else extremely small; secondly that, among them, the large letters must denote comparatively large quantities, and the small letters small quantities. Now, by the second "calculated" equation of the second batch, we find that Mr. Strutt's yellow is

$$\frac{1}{57}(128 \cdot 4\, r + 63 \cdot 5\, g - 49\, b);$$

and, if we substitute in this expression the expressions given above (1) for $r, g, b$, we shall have, for the coefficient of $B$,

$$\frac{1}{57}(128 \cdot 4\, n' + 63 \cdot 5\, n - 49\, N)$$

Now this must not be negative, or else must be very small in comparison with the coefficients of $R$ and $G$ (Maxwell, *ubi supra*, Tables VI. and IX.). Therefore $N$ cannot be greater than $2 \cdot 6\, n' + 1 \cdot 3\, n$; that is to say, there is no more "primary" blue in Mr. Strutt's blue than about $2\frac{1}{2}$ as much as in his red, *plus* $1\frac{1}{3}$ as much as in his green. It is true that blue is rather a dark colour in pigments; but so it is in the spectrum; and $N$ measures, not a quantity of colour simply, but its ratio to the "primary" blue of the spectrum. Either the yellow was very pure, or the red and green very impure; and, if Mr. Strutt provided himself with good representatives of natural colours, this proves my point.

C. J. Monro

Jan. 20

## The Primary Colours

I have been greatly interested in reading Mr. Strutt's curious experiments on colour in the last number of Nature. I am glad to see that he is able to assume as proved the theory that green and not yellow is the middle primary. The true position of green is well illustrated in Mr. W. Benson's "Principles of the Science of Colour," both by argument and by diagrams. There is, however, one piece of evidence which seems to me conclusive as against yellow, but which I have not seen noticed.

When a solid body is gradually heated to incandescence, the light given out is first *red*, then *orange*, afterwards *yellow*, and finally *white*. If yellow were a primary, it would be impossible for it to appear in this series, which is formed upon the basis of the first primary, red, by successive additions of more and more rapid vibrations. Every colour in the series except the first must be a compound. If the heat is not sufficient to generate the most rapid vibrations which the eye can appreciate, white light is not given off at all, the series terminates with the yellow. The light of a glowing coal, without flame, in an ordinary fire, rarely passes beyond the yellow stage, and such light yields to the prism abundance of red and green rays, but scarcely a trace of blue or violet.

But, if red is the first primary, and green the second, which is the third? Shall blue still sit upon the throne on which Newton placed him, when his brother yellow is deposed? I think his position has become extremely precarious, and that he would be wise to abdicate with dignity before he is ignominiously turned out as a usurper.

If the kingdom of light is really divided into three principalities, is not violet the rightful heir to the third throne? Violet is said to be a mixture of blue and red. But how should red make its appearance at the wrong end of the spectrum? If it has no definite limit, but gradually thins out from its own place to the other extremity of the spectrum, then the whole of the other colours must be more or less affected by it, and red must be the only true primary among them. If it is said that the red in violet is clearly recognised by the eye, I think it may be answered that this is only because we have been taught to think of it as a compound, and that we might just as truly say that we can see yellow in green or orange in red.

Leicester, Jan. 21 Fredk. T. Mott

## Utilisation of Sewage

While heartily thanking your reviewer for the very valuable suggestions which he has given me with regard to the second edition of my "Digest of Facts relating to the Treatment and Utilisation of Sewage," I wish to point out a slight oversight which he has made.

He says, "Mr. Menzies' name, however, has somehow or other slipped out of his pages (159, 169) where he treats of this improvement on the older plans for sewage."

The fact is that page 145 has somehow or other escaped the reviewer's critical eye. On that page the following sentence occurs:—"Some of these towns, then, it will be seen, are provided with sewers much upon the plan which Mr. Menzies has the credit of having first brought prominently forward; that is to say, with impervious pipes for the sewage properly so called, and brick drains for the surface and storm water, the former being laid deeply and the latter being superficial."

I should be very sorry to have it thought that I had forgotten "to emphasise the name of the Windsor Sanitarian."

W. H. Corfield

## Ocean Currents

The differences of barometric pressure to which Mr. Keith Johnston refers (Nature, Jan. 19, p. 227) have a well-ascertained geographical existence, but his suggestion that they may originate or direct the Ocean Currents is clearly inadmissible. The high pressure over a large patch of the North Atlantic to the south or south-west of the Azores—and similarly in each of the other oceanic basins—is there permanently; and whatever disturbance might be produced by it was produced once for all when the high pressure was first formed. It would then displace a certain quantity of the water over which it rested, would thrust it out, and keep that particular part of the ocean at a slightly lower level than that over which the pressure of the air was not so great. But having done this, the adjacent bodies of water would be in hydrostatic equilibrium, and the high pressure could not continue to thrust water out towards the place of low pressure. My meaning may be at once illustrated by putting one end of an open glass tube into a basin of water, and partially exhausting the air inside it. The adjacent surface is thus exposed to a higher pressure than the surface inside the tube, and a certain portion of the water is immediately thrust from the place of greater to the place of less pressure; the column of water inside the tube is raised until the weight of the excess balances the difference of pressures. At that height it remains, and no further movement takes place, so long as the relative pressures remain the same. A fluctuation of the pressures will give rise to alternate ingoing or outgoing currents, but a continuous stream in one direction can only be produced by a continuous increase or decrease of one or the other pressure.

But independently of the equilibrium, which must, so far as the pressure is concerned, be established, the difference of level caused by these differences of pressure is extremely trifling. The barometric difference between the patch of high pressure in latitude 30° and the equator is about $\frac{3}{10}$ths of an inch, or equivalent to a column of water 4 inches in length. A difference in level of 4 inches in 1,800 miles can scarcely under any circumstances give rise to a current of twenty miles an hour.

J. K. LAUGHTON

Royal Naval College, Portsmouth, Jan. 23

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IT is singular that diversity of opinion should still exist as to whether ocean currents are due to the impulse of the winds, or to difference of specific gravity. That ocean currents are not caused by difference of specific gravity between the waters of equatorial and polar regions can be proved, as the amount of force from this cause acting on the ocean to produce a current, can be readily calculated.

Assuming, which is not the case, that difference in saltness between the water of equatorial and polar regions does not in any wad tend to neutralise the effect resulting from difference of temperature, in other words, that the sea in polar regions is as salt as the sea in equatorial regions, it can be shown that the force resulting from the difference of temperature, tending to produce a current towards the poles, amounts to only $\frac{1}{1820000}$ of that of gravity. For example, the force impelling a cubic foot (64lb.) of sea water at the surface of the ocean towards the poles is scarcely equal to the weight of one-fourth of a grain.* A force so infinitesimal, acting on a fluid even so perfect as water, can produce absolutely no motion. M. Dubuat found by direct experiment that it requires a force four times greater than the above to produce even sensible motion.

Ocean currents are due alone to the impulse of the wind. In the latter half of my paper on the Cause of Ocean Currents, which will shortly appear in the *Philosophical Magazine*, I hope to be able to show that the objections to this theory are founded upon misconceptions regarding the way in which winds produce the great system of oceanic circulation.

JAMES CROLL

* *Philosophical Magazine* for October 1870, p. 249.

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## Dr. Frankland's Experiments

IN last week's NATURE Dr. Frankland describes some experiments, apparently under the impression that they were similar to one (No. 20) published by me in NATURE, No. 36, p. 200. The results which he obtained were in reality totally different from those which I obtained, although those who read Dr. Frankland's communication are lead to believe that the results were almost wholly similar. The inference to be drawn from what he has written is that we differ merely as regards the interpretation of the nature of what was seen.

Dr. Frankland says he made use of tubes of "hard Bohemian glass," and that, on examining them "when they came out of the digester, it was evident that the *interior walls of the glass tubes had been corroded* by the enclosed fluid." After a time the "liquid in all the tubes became more or less turbid, and, in some cases, a small quantity of a light flocculent precipitate subsided to the bottom." After five months two of the tubes, which exhibited "the greatest turbidity," were selected for examination, and the "flocculent sediment" in the tubes was more especially subjected to a careful microscopical examination. This scrutiny was conducted by Professors Frankland and Huxley and Mr. Busk. Dr. Frankland then says: "So far as the optical appearances presented by the sediment go, they may be appropriately described in the terms which Dr. Bastian applied to the matter found by him in a solution of like composition, and similarly treated."

Now, that any real similarity did exist, I feel most strongly inclined to doubt, because the solution examined was not similar in constitution to my own, and because no such "flocculent sediment," as that to which Dr. Frankland alludes, ever existed in my flask.

In the experiment of mine to which reference is made, the precise quantities of carbonate of ammonia and phosphate of soda employed are not known. In this first experiment the ingredients were not weighed, although, subsequently, solutions have been prepared for me of the strength which Dr. Frankland names.

Then, although my tube with its contained solution was exposed to the same temperature as that employed by Dr. Frankland, its internal walls were not in the least corroded, and no "flocculent sediment" appeared in the solution. And, in addition, two other tubes which were prepared for me by Dr. Frankland (which did contain solutions of the same strength as those which he employed) have not had their transparency in the least impaired, although they were submitted to precisely the same temperature; neither have they shown a trace of the "flocculent sediment" previously mentioned. Seeing, however, that in one experiment (May 11th), with a solution of the same strength, a tube of English glass was employed by Dr. Frankland's assistant, and that the internal walls of this tube were corroded; and seeing, moreover, that a "flocculent sediment" did form also in this particular tube of mine, I cannot help fancying that Dr. Frankland may be mistaken as to the nature of the glass employed in his experiments. If, as in this experiment of mine, it was really English glass instead of hard Bohemian, almost the whole of the small quantity of phosphoric acid originally in the solution would probably have been deposited in the form of an insoluble phosphate of lead, and thus the character of the solution would have been entirely changed.

In my previously published experiment the fluid was examined at the end of thirty days. "When this flask was received from Dr. Frankland, the fluid was somewhat whitish and clouded. During the last ten days a thin pellicle had been seen gradually accumulating on its surface, and in the latter four or five days this increased much in thickness, and gradually assumed a distinct mucoid appearance. The fluid itself was tolerably clear, though an apparent turbidity was given by the presence of a fine whitish deposit on the sides of the glass. When the flask was opened the reaction of the fluid was found to be *neutral*. Portions of the *pellicle* were at once transferred to a glass microscope slip," &c. (NATURE, No. 36, p. 200.) In portions of this pellicle were found the "*five spherical or ovoid spores*," upon the finding of which alone I laid any stress as indicative of the presence of living things. The presence of mere particles having a movement indistinguishable from Brownian movements, has never been adduced by me as evidence that living things had been evolved in a solution, although the representations of others would lead the public to believe that I have done so.

In the face of these differences, therefore, I was somewhat surprised at the intimations contained in Dr. Frankland's letter. He believes, and would lead your readers to believe also, that the microscopical appearances presented by the "flocculent sediment" and *débris* of corroded glass obtained from his tubes were similar to the microscopical appearances of a *pellicle* obtained by me from a tube in which there had been no corrosion. This, however, I am the less inclined to believe, because I also have had the opportunity of examining a flocculent sediment and *débris* of corroded glass from a tube previously referred to, which was opened on October 21, and in which also no living things were found. Microscopical specimens of the "pellicle" and of the "sediment" are now in my possession.

Perhaps I may venture to recommend Dr. Frankland to destroy the other two tubes which are corroded, as being worthless, and to hope that, in any future experiments, he will subsequently expose his fluids to a somewhat higher temperature, and also, before immersing his experimental tubes in any fluids, that he will thoroughly satisfy himself as to the transparency of such fluids to the actinic or chemical rays of light. We are informed that his tubes were "exposed to bright diffused daylight, and sometimes to sunlight," but any amount of exposure to light would be more or less useless if strong sulphuric acid and strong carbolic acid are as black to the chemical rays of light as nitrite of amyl and other fluids have been shown to be. Dr. Frankland makes no statement concerning this very important point.

H. CHARLTON BASTIAN

20, Queen Anne Street, W., Jan. 22

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## The Tails of Comets, the Solar Corona, and the Aurora, considered as Electric Phenomena

MY attention has been called to a rudely worded attack by a certain Mr. Bedford, Phil.D., on Professor Reynolds, of Owens College. This is not the first time Mr. Bedford has offended in this way. Prof. Reynolds has not seen Mr. Bedford's pamphlet.

*I have.* A copy was sent to Messrs. Groombridge in support of certain claims on my views about the stars. Let me hasten to assure Prof. Reynolds that, as he surmises, the views expressed in this very scarce treatise bear not the remotest resemblance to his.

I read several weeks ago Prof. Reynolds' interesting paper, the views expressed in which are, in a general way, similar to those I advocated in a paper entitled "Strange Discoveries respecting the Aurora" in *Frazer's Magazine* for February 1870. As it was quite clear to me, however, that Prof. Reynolds' views had been formed quite independently, it seemed wholly unnecessary to comment on that resemblance. I could only rejoice that so competent an authority should have been led to conclusions agreeing in general so satisfactorily with those I had deduced; and also, be it noted, with the results of the observations made on the recent eclipse. RICHARD A. PROCTOR

### Browning's Spectroscope

A LETTER from Mr. Browning, in the number of NATURE for December 15th, has just come to my notice, and seems to require a word from me. I regret exceedingly that he should have supposed that I intended to imply that he had committed any impropriety in employing in his own automatic combination an arrangement of Mr. Rutherfurd's from a spectroscope which was not automatic. I did not "go out of my way" in making the allusion, but only stated what I supposed to be a fact, in order to show that the proposed arrangement of radial bars was good and practicable, having already been endorsed by most eminent authority.

At the time when the article was written, Mr. Browning had but recently published the account of his instrument, and, of course, I knew nothing about its earlier history.

On the other hand, a full description of Mr. Rutherfurd's arrangements with an illustrative figure had appeared in *Silliman's Journal* in March 1865, more than four years earlier. This article is dated December 10, 1864, and will be found in the Journal referred to: vol. xxxix., p. 129.

Possibly the tone of my allusion may have been unintentionally affected by the fact that I supposed that Mr. Browning had seen this article. In common with many other Americans, who have spoken to me about it, I thought it singular that, in describing his own instrument, he made no reference to Mr. Rutherfurd, and am very happy to find him blameless in the matter. At the same time, I think he has no ground of complaint against me for referring to the arrangement as "first devised by Mr. Rutherfurd, and since adopted by Mr. Browning;" although, if I were to write the sentence again with my present knowledge of the facts, I should put it quite differently.

Let me add also that, having seen the instrument to which Mr. Lockyer refers in his note, I cheerfully concede to him the priority in respect to the use of an elastic spring, and the half prism at the beginning of the train, as well as the idea of sending the light twice through the train by a right-angled prism at its extremity. As he has never published an account of his instrument, however, I suppose I can hardly be held blameworthy for re-inventing it, and publishing it myself. Without one unkindly feeling the words of the old poet still sometimes come to mind, "*Pereant qui ante nos nostra dixerint.*"

The magnetic record at Greenwich shows a well-marked disturbance of all the elements precisely simultaneous with the eruption observed on the sun's disc September 28th. The declination was affected to the extent of five minutes of arc, and the disturbance was compounded of two waves, following each other, and partly superposed, probably corresponding to the ejection of the two masses of protuberance-matter which are shown in the figures. C. A. YOUNG

London, Jan. 21, 1871

### St. Mary's Hospital

I SEE in this week's NATURE the announcement that Dr. Wood has been appointed Lecturer on Chemistry at St. Mary's Hospital Medical School. This is an entire mistake; no appointment has yet been made, since Dr. Russell will continue to hold the post until the end of the Winter Session. The vacancy has not therefore actually occurred yet, although it will be declared shortly, and a fresh appointment made in due course.

W. B. CHEADLE,
Dean of St. Mary's Hospital Medical School

Jan. 20

[We were misled in making the announcement referred to above by our contemporary the *British Medical Journal.*—ED.]

## IMPROVEMENT OF GEOMETRICAL TEACHING

A CONFERENCE was held at University College, London, on Tuesday, the 17th inst., to take this subject into consideration, and to form an Association for the improvement of geometrical teaching throughout the United Kingdom.

Previous to the meeting a large number of head and mathematical masters and others interested in the subject had given in their adhesion to the principles upon which it was proposed to form the Association. These included representatives of the following important schools:—Winchester, Eton, Harrow, Rugby, Charterhouse, Christ's Hospital, Marlborough, Wellington, Clifton, Uppingham, Sherborne, Birmingham, Dulwich, University College School, London, Repton, Durham, Manchester, King William's College, Isle of Man; Tiverton, Taunton, Leeds, Huddersfield, Nottingham, Yarmouth, Windermere, Mill-hill School, Middlesex, Middle Class School, Cowper Street, Middle Class School, Bedford, the majority of whom were present at the Conference. The movement was further supported by Dr. Hirst, F.R.S., of London University, Mr. W. Spottiswoode, F.R.S., president of the London Mathematical Society, Mr. C. W. Merrifield, F.R.S., Principal of the Royal School of Naval Architecture, South Kensington, and others.

Dr. Hirst, the president, took the chair, and resolutions were passed bearing upon the organisation and future working of the Association. It was proposed to invite the mathematicians of the country to prepare syllabuses of elementary geometry, embodying their views of the principles which should be adopted in any new text-book which is to supersede Euclid. Further particulars may be obtained by application to Mr. R. Levett, honorary secretary, King Edward's School, Birmingham.

## A HINT TO ELECTRICIANS

MR. MANCE'S method for measuring the internal resistance of a single galvanic element or battery, communicated to the Royal Society at its meeting of last week, and the modifications of Wheatstone's bridge suggested by myself for finding the resistance of a galvanometer coil from the deflection of its own needle, supply desiderata in respect to easy and rapid measurement, which have been long *felt* by telegraph electricians and *needed* by other scientific investigators and by teachers of science. Year after year the latter, in their arrangement of batteries, electrodes, and galvanometers, have darkly and wastefully followed the method which from workmen we learn to call rule of thumb; while the former, with admirable scientific art, measure every element with which they are concerned, in absolute measure. How many physical professors are there in Europe or America who could tell (in millions of centimetres per second) the resistance of any one of the galvanometers, induction coils, or galvanic elements which they are daily using? How many of them, in ordering an electro-magnet, require of the maker that the specific resistance of the copper shall not exceed 16,000 (gramme-centimetre-seconds)? How many times have eight Grove cells been set up to produce a degree of electro-magnetic effect which four would have given, had the professor exacted of the instrument-maker the fulfilment of a simple and inexpensive scientific condition, as submarine telegraph companies have done in their specifications of cables? If every possessor of an electro-magnet were to cut a metre off its coil, weigh the piece, measure its resistance, and send the result to NATURE, and if every maker of Ruhmkorff coils would do the like for every coil of copper wire designed for his instruments, a startling average might be shown. And what of the items? I venture to say that (provided the instruments of the great makers are not excluded) specific resistance above 30,000 would not be a singular case. I could tell something of galvanometers of 1869, comparable only to submarine cables of 1857. I refrain:—but let makers of galvanometers, Ruhmkorff coils, and electro-magnets beware; surely NATURE will find them out if they do not reform before 1872. W. THOMSON

## THE GAUSSIAN CONSTANTS OF TERRESTRIAL MAGNETISM

I THINK you will be doing good service to the cause of Natural Science by giving insertion in your valuable pages to the following translation of a notice which appeared in No. 1,825 of the *Astronomische Nachrichten* (Vol. 77, p. xi), on the subject of Prof. Petersen's re-computation of the Gaussian Constants of Terrestrial Magnetism, in aid of which the British Association at their last meeting voted a grant of money. It has been communicated to me by Prof. Erman of Berlin, who, in reference to the grant in question, writes as follows:—"This new act of British generosity would in other times have scarcely needed a special mention, being equalled by so many former ones of the same kind; but in the present moment, when the raging war makes petty jealousies spring up between our two befriended [friendly] nations, it is a most sacred duty to publish the fact of two Prussians having found in England a most generous and most wanted help for their scientific endeavours."

Mr. Petersen's calculations are progressing in a very desirable manner, and he hopes fully to bring them to their end (D.V.). J. F. W. HERSCHEL

Collingwood, Jan. 21

TRANSLATION

"We learn by a communication from Prof. Erman that M. Petersen, of Kiel, has undertaken to extend his great work on Terrestrial Magnetism, so as to afford for the whole earth, and for the epoch 1829, a *Fundamental Determination of the Potential Constants*, which, according to laws yet unknown, are subject to secular variation. From the knowledge of these fundamental values so obtained by the researches of Erman and Petersen, will then come to be securely connected, as a second step in advance, the determination of the laws of secular change. Since, however, the material obstacles to so laborious a work, with whatever personal devotion, would have proved insurmountable without public aid, it becomes our duty most gratefully to announce that such aid has been granted from the same quarter which afforded it to the earliest portion of this undertaking. The British Association for the Advancement of Science, at its last annual meeting, has appointed a committee, consisting of Sir J. Herschel and Prof. A. Erman, for the purpose of engaging M. H. T. R. Petersen to prosecute the continuation of his computations of the constants in question for 1829, so as to embrace all observations not included in the previous calculations, and to this end has placed a sum of 50*l.* at their disposal.

"In pursuance of this object Prof. Erman addresses to the readers of this notice his request for the communication of citations of, or references to, works and treatises or essays in which may be found recorded measured values for any station of the globe, of the magnetic *declination*, *inclination*, and *intensity* during any portion of the last ten years, as also any researches on the annual variations of these elements at determinate stations. Of course it is not meant to call for even an approximately complete catalogue of works of this kind, to furnish which would of itself require no small amount of labour. But many astronomers [and others] must have access to a variety of journals, accounts of travels, records of measures and observations, &c., which may not have come under the notice of Messrs. Erman and Petersen, notices of which, communicated to the Editor of the *Astronomische Nachrichten* * in the form of a letter, with a postscriptum or memorandum such as:—Magnetic Observations for 18 . . are to be found in . . . Volume . . . page . . are requested."

* Prof. Dr. C. A. F. Peters, Direktor der Sternwärte in Altona.

## ACCOUNT OF THE AUGUSTA ECLIPSE EXPEDITION

IN consequence of the unfortunate wreck of the *Psyche* on a sunken rock on the coast of Sicily, about nine miles north of Catania, the arrangements of the Sicilian Expedition were considerably modified. Catania was made the headquarters of the expedition, and the garden of the Benedictine Monastery was given up by the authorities of the city to the English and American observers. It was finally arranged that Prof. Roscoe should take charge of the Etna Expedition, and I was asked by Mr. Lockyer to take charge of the Expedition to Augusta. Mr. Brett, Mr. Burton, Mr. Clifford, Mr. Ranyard, Mr. Samuelson, and myself formed the party.

It was also arranged with Mr. Ranyard at Catania, that on the morning of the 22nd, he and another of our party should drive some miles up from Augusta in the direction of the hills of Carlentini, to observe the Eclipse. At Augusta we were to live in camp, and Colonel Porter, with a body of sappers, had been landed there by the *Pysche* on her way to Naples.

Mr. Brett and Mr. Ranyard went first to Augusta to make arrangements with Colonel Porter for our encampment and observatory, and they met with every assistance from the Syndic of the City of Augusta, and were very kindly received by the Italian astronomers, among whom were Prof. Cacciatore, Prof. Donati, Father Secchi, and Father Denza, who were stationed inside the fort. Our encampment, and a wooden observatory sixty feet long, were pitched on the southern slopes of the glacis of the fort, with a full view of the sea to the east.

I cannot speak too highly of the way in which Colonel Porter exerted himself to make all arrangements satisfactory and complete, and even to introduce elements of comfort into our camp life; and the energetic way in which his men carried out his instructions is beyond all praise. Up to Monday the 19th, the terraces of the Monastery at Catania were made the general practising ground, and those who were to observe for polarisation, except Mr. Ranyard, who was at Augusta, tested and compared their instruments for rapidity of correct observation, and for delicacy.

For my own telescope I had two eye-pieces, one with plates of double-rotating quartz, and the other with a Savart polarimeter. When the polarisation was not very strong, I found the polarimeter more delicate than the bi-quartz for detecting the plane of polarisation, and with it I was able to measure the amount of polarisation readily. On observing the same points with Mr. Griffiths, who also used a Savart polarimeter, we found that in from ten to fifteen seconds we could determine the plane and amount of polarisation, and in some cases we found that our readings for both were absolutely identical.

At about 6.30 on Monday evening, and again soon after 7 o'clock, when Mr. Clifford and I were on the sea on our way to Augusta, we saw a brilliant display of the zodiacal light, consisting of brilliant pink streamers, stretching up perpendicularly to the horizon, the planet Jupiter being just on the most brilliant streamers. Towards the north and round the horizon there were also streamers and a faint hazy light, and the sky became covered with a pinkish mauve colour. One of these displays was also seen by the rest of our party at Augusta.

As the evening grew darker, there was strong phosphorescence on the sea. The drops scattered by the oar as it struck the water glowed with phosphorescent light, and the forms of the eddies, caused by the bending of the oar, were distinct and brilliantly illuminated.

At Augusta, through the kindness of Prof. Cacciatore and Father Denza, I was able to obtain the latitude and longitude, as well as the local times of the different phases of the Eclipse :—

| | | |
|---|---|---|
| Latitude of Fort Augusta | $37^\circ$ 14′ 0″·6 | N. |
| Longitude ,, ,, | $1^h$ 0′ 52″·2 | E. of Greenwich. |
| Local time of beginning of totality | $2^h$ 1′ 56″·7 | |
| ,, middle ,, | $2^h$ 2′ 52″·1 | |
| ,, end ,, | $2^h$ 3′ 47″·5 | |
| Making the duration of totality | 1′ 50″·8 | |

The barometer fell from the morning of the 20th to the morning of the 22nd, then rose a little, and again began to fall about 12 o'clock, and was lowest about the time of totality. On the evening of the 21st the Italian astronomers reported to us the bad state of the weather throughout Italy and Sicily, the wind being westerly, and that a sirocco was expected everywhere. From about 2 o'clock in the night there was a heavy storm of wind and rain, with thunder and lightning, and our tents were in danger of being blown away. By 6 o'clock the rain had ceased, and the wind moderated, but there were still frequent flashes of lightning on the eastern horizon ; in half an hour thick clouds had again covered the sky, and we had rain. The wind again became violent, and swept away the clouds, but the weather did not look promising.

According to previous arrangement, Mr. Ranyard started in a carriage to go up to the hills, and Mr. Samuelson accompanied him, and they took two sappers with them.

When the moon had entered about one-third of her diameter Mr. Brett, with his 8·5 inch reflector, observed the corona round the limb of the sun as a hazy light most brilliant nearest the sun's limb, and the limb of the moon could be traced on this corona for about 2′ from the cusps. On these points Mr. Burton confirmed Mr. Brett's observations. Soon after a sudden chill was felt, and there was a sudden change in the light. About three minutes before totality there were brilliant and very remarkable patches of red and yellow light on the cloud to the right of and below the sun. Mr. Burton describes them as bows, apparently concentric with the sun. During the morning Mr. Burton had been able to indicate the positions of some of the most remarkable prominences, but the stormy wind prevented him from mapping them accurately. Father Secchi had also kindly sent us the positions of those which he had observed.

Just before totality, Mr. Burton saw and made a diagram of a prominence at the lower horn, and saw several lines in the spectrum of the chromosphere between D and E. Using his large telescope Mr. Brett was able to make a sketch of the corona during the totality.

On account of the cloud, Mr. Burton was able to make only one of the four observations he had hoped to make on the Corona with his five-inch equatorial and spectroscope. At the beginning of totality, placing his slit tangential and very near to the east limb, but not on a prominence, a bright line was distinctly seen in the spectrum, very near E, and a little less refrangible. The line was less defined than the hydrogen lines of the prominence. No dark lines were seen on the Corona.

At the end of totality, Mr. Burton had a momentary glimpse of the Corona, but had no time to get the telescope on it before the totality was over. With regard to the amount of light, he says that it was sufficient to see a pencil diagram at a distance of two feet from the eye. I can confirm him in the view that the darkness was not intense, and have no doubt that the diffusion of light by the cloud gave us more light than we should otherwise have had. Venus and some stars were seen.

Colonel Porter had kindly volunteered to make a sketch of the Corona, but the cloud prevented him from obtaining any satisfactory result.

I did not see the Corona at the beginning of totality with my telescope. As the band of sunlight became exceedingly thin, and at the instant of its disappearance broke up into sections, I could not decide whether the lunar mountains had pierced the rim of light, or whether the dense cloud coming over the moon had cut out certain portions of the rim before obscuring the whole.

After this, I could detect nothing of the disc of the moon for a full minute, then the cloud became thinner, and I found that by slowly moving the telescope I had kept the moon in the centre of the field. At the top and bottom, the limb was visible, but no light was seen outside it at these points. I saw light of the Corona near the point of beginning of totality covering some 20° of the limb, and also a trace of light near the point of emergence. I could not perceive any colour on these portions of the Corona, nor could I detect any difference of colour on the two plates of my bi-quartz, the line of division of which was at right angles to the sun's path, *i.e.*, inclined at 15° to the vertical. The moon was again observed, and again I detected light near the point of emergence, and placed the line of division of my bi-quartz radial to the moon, having the light in the centre of the field, but I could detect no trace of colour on the two parts of the crystal, showing that the bi-quartz was not sufficiently delicate to detect the polarisation under such unfavourable circumstances. The rim then became continuous, and the totality was over.

Mr. Clifford observed polarisation on the cloud to the right and left and over the moon, in a horizontal plane through the moon's centre, and found the plane of polarisation to be inclined at from 15° to 20° to the vertical towards the west. At his last observation, which was on the moon, when it could be seen near the end of totality, he determined the plane of polarisation to be vertical.

The comparisons made at Catania, as well as a comparison of my observations with Mr. Clifford's, seem to show that bands, rather than a difference of shades of colour, should be employed to detect delicate polarisation.

Mr. Ranyard had a very clear view at Villasmunda, although it was raining during the totality, and made *three* observations, two of which he described to me as agreeing with what should be observed in the case of radial polarisation.

Mr. Samuelson and the two sappers made independent rough drawings of what they saw, and their drawings agree well as to the Corona and the positions of the rays. Mr. Samuelson also used a Nicol's prism, with Savart's bands, to determine the polarisation on the sky at three points, and at two of these points found the plane of polarisation vertical.

I have not yet seen the details of Mr. Ranyard's or of Mr. Samuelson's reports.

Although the other successful observers of the Eclipse in Sicily were not attached to the Augusta expedition, of which Mr. Lockyer had put me in charge, I may add that at Syracuse the weather was favourable, and Mr. Griffiths was able to determine the plane and amount of polarisation at different points of the Corona ; also, that Messrs. Brothers and Fryer were able to take some good photographs of the Corona, one of which is very remarkable for its clear definition of the Corona and of the rays extending out to a distance of two diameters from the moon's limb. This photograph and a careful sketch of the Corona by Mr. Watson, one of the American observers, show a very remarkable agreement, and prove the existence of the remarkable dark cusps on the Corona.

At Augusta, two bright lines were seen in the spectrum of the Corona by Father Denza, one of the Italian astronomers, but no dark lines have been seen.

We experienced every kindness from the Italian astronomers at Augusta and from the authorities ; and, on the day after the Eclipse, we were invited by the Syndic of the City to meet the Italian Astronomers at a grand public dinner given by the City in honour of our visit.

King's College, Jan. 16 W. G. ADAMS

## *FLOWER'S OSTEOLOGY OF THE MAMMALIA* *

PROF. FLOWER'S "Introduction to the Osteology of the Mammalia" is a thoroughly satisfactory addition to English anatomical literature. It supplies a much-felt want, and combines the rarely united qualities of com-

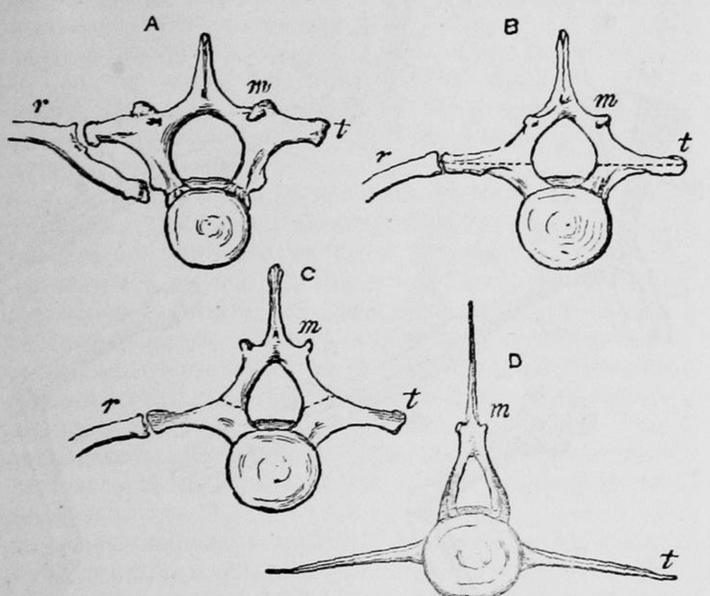


FIG. 1.—Anterior surface of vertebræ of Dolphin (*Globiocephalus melas*), ⅙. A fifth thoracic; B seventh thoracic; C eighth thoracic; D first lumbar: *r* rib; *m* metapophysis; *t* transverse process. The dotted lines indicate the position of the neuro-central suture.

pleteness with brevity; and, while thoroughly scientific, is remarkable for its clearness and simplicity of expression.

Of convenient size for the pocket of the student, it consists of three hundred and thirty-seven pages of excellent letter-press, and is illustrated by one hundred and twenty-

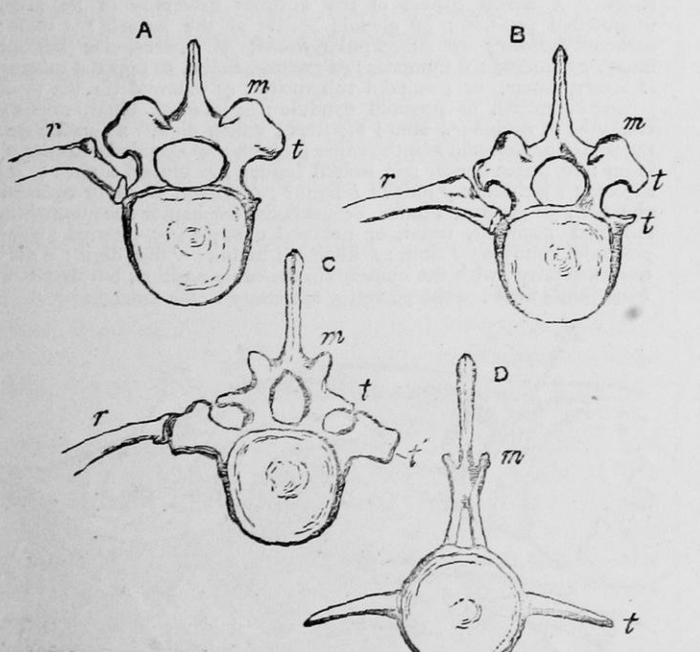


FIG. 2.—Anterior surface of vertebræ of Sperm Whale (*Physeter macrocephalus*), $\frac{1}{14}$. A eighth thoracic; B ninth thoracic; C tenth thoracic; D fifth lumbar; *r* rib; *m* metapophysis; *t* upper transverse process; *t'* lower transverse process.

six woodcuts, nearly all original. These are exceedingly well drawn, and make the book attractive in appearance (though it is to be regretted that they have somewhat suffered in the printing), while the subjects chosen have evidently been selected with great care as regards their utility in illustrating the text.

* "An Introduction to the Osteology of the Mammalia." By W. H. Flower, F.R.S., Hunterian Professor of Comparative Anatomy and Physiology. (London: Macmillan and Co.)

The first chapter contains a very short account of the classification of the Mammalia, accompanied by an interesting diagram "intended to exhibit the relationships which appear to exist between the different groups of the Mammalia." The small size of this diagram hardly affords space enough to express fully the degrees of affinity between the different groups. It is owing to this, perhaps, that the Carnivora are separated from the Ungulata by a less interval than that which divides them from the Insectivora, and that the Hominina are but very slightly more approximated to the Simiina than are the latter to the Lemurina, although the structural difference between the last-

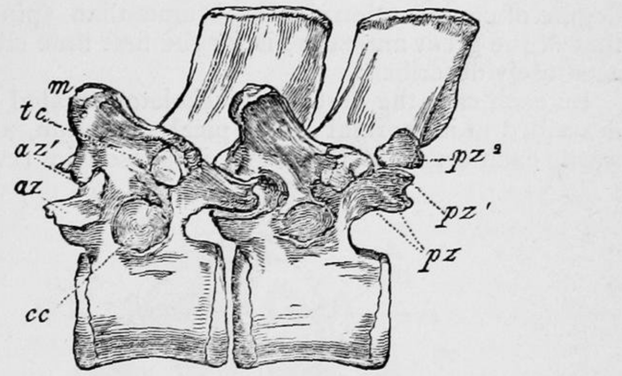


FIG. 3.—Side view of twelfth and thirteenth thoracic vertebræ of Great Anteater (*Myrmecophaga jubata*), ⅔. *m* metapophysis; *tc* facet for articulation of tubercle of rib; *cc* ditto for capitulum of rib; *az* anterior zygapophysis: $az^1$ additional anterior articular facet; *pz* posterior zygapophysis; $pz^1$ and $pz^2$ additional posterior articular facets.

named groups and all the higher primates are so great that Professor Flower himself hesitates "whether they should be associated with the monkeys, or should constitute a distinct order by themselves." Nevertheless, the diagram is very instructive, and well expresses the more important relationships existing between the groups as far as their affinities have been demonstrated, or shown to be probable by the present state of zootomical science.

Thus the distinctiveness, yet close affinity, between the ordinary Carnivora and the seals is made evident, as also

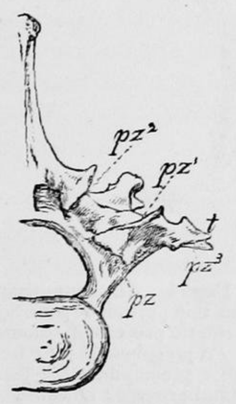


FIG. 4.—Posterior surface of second lumbar vertebra of Great Anteater, ⅔. *t* transverse process; *pz* posterior zygapophysis; $pz^1$, $pz^2$, and $pz^3$, additional posterior articular facets.

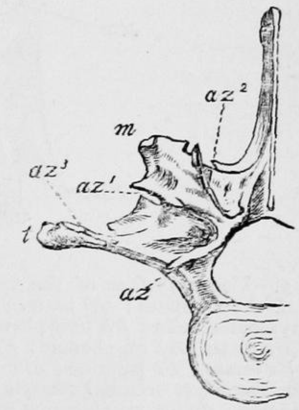


FIG. 5.—Anterior surface of third lumbar vertebra of Great Anteater. ⅔. *t* transverse process; *m* metapophysis: *az* anterior zygapophysis: $az^1$, $az^2$ and $az^3$, additional anterior articular facets.

the remoteness of the Sirenia from the Cetacea, and the approximation of the former to the Ungulata.

The complex relationships of the subordinate groups of hoofed beasts are also well exhibited, and though, perhaps, some objection might be made to the position of the anomalous little group, Hyracoidea, it would be difficult to put it in any other spot not also open to criticism.

A laudable desire not to increase too much the bulk of his volume has, doubtless, induced the author to make his chapter on Classification so very brief. It is, nevertheless, to be hoped that, in the next edition, it may be made at least as long again; as, in its present condition, the student can hardly find in it all the information necessary for the comprehension of the other parts of the work.

Thus, in describing the skull, the family names *Cebidæ* and *Hapalidæ* are used, as also *Ursidæ*, *Procyonidæ* and *Mustelidæ*, though nothing is said as to these groups either in the text of the first chapter, or in its explanatory diagram. The addition of three more pages would do away with this imperfection.

After the introductory chapter on Classification we have the skeleton as a whole, the vertebral column with its several regions, the sternum, ribs, skull, shoulder girdle, arm and hand, pelvic girdle, leg and foot, successively described in eighteen different chapters. The maximum degree of complication in the Mammalian spine (*i.e.*, in that of the great ant-eater) is for the first time clearly and accurately described.

In each case the part of the skeleton treated of is first described in its normal and typical condition, and afterwards each order of mammals is passed in review, and

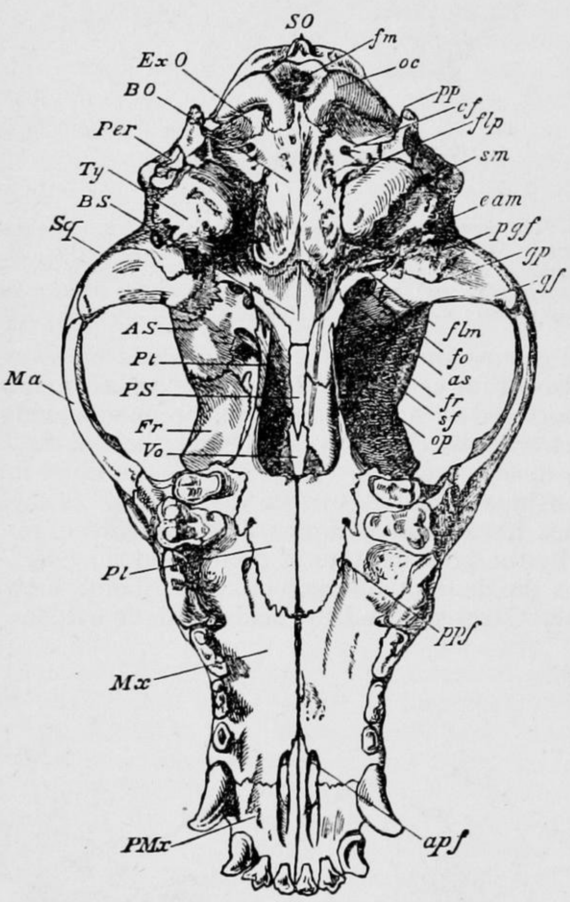


FIG. 6.—Under surface of the cranium of a Dog. ½. *SO* supraoccipital; *ExO* exoccipital; *BO* basioccipital; *Per* mastoid portion of periotic; *Ty* tympanic bulla; *BS* basisphenoid; *Sq* zygomatic process of squamosal; *Ma* malar; *AS* alisphenoid; *Pt* pterygoid; *PS* presphenoid; *Fr* frontal; *Vo* vomer; *Pl* palatine; *Mx* maxilla; *PMx* premaxilla; *fm* foramen magnum; *oc* occipital condyle: *pp* paroccipital process; *cf* condylar foramen; *flp* foramen lacerum posterius; *sm* stylo mastoid foramen; *eam* external auditory meatus; *pgf* postglenoid foramen; *gp* postglenoid process; *gf* glenoid fossa; *flm* foramen lacerum medium; *fo* foramen ovale; *as* posterior opening of alisphenoid canal; *fr* foramen rotundum and anterior opening of alisphenoid canal; *sf* sphenoidal fissure or foramen lacerum anterius; *op* optic foramen; *ppf* posterior palatine foramen; *apf* anterior palatine foramen.

the leading modifications which such part presents in each ordinal group are stated.

Thus, with regard to the skull, we have first a chapter on the skull of the dog as a type—an admirable description, and a model for writers on kindred subjects from its completeness, its clearness, its thoroughness, and its simplicity. Next, we have a chapter on the skull as it exists in the orders Primates, Carnivora, Insectivora, Chiroptera, and Rodentia. After this follows a chapter on the skull in the Ungulata, Hyracoidea, and Proboscidia; then one on the same part in the Cetacea and Sirenia, and finally one on the skull in the Edentata, Marsupialia and Monotremata. This last chapter may hereafter be judiciously expanded. The skulls of the Echidna and Ornithorhynchus alone might well take up half the space which is here allotted to these three orders.

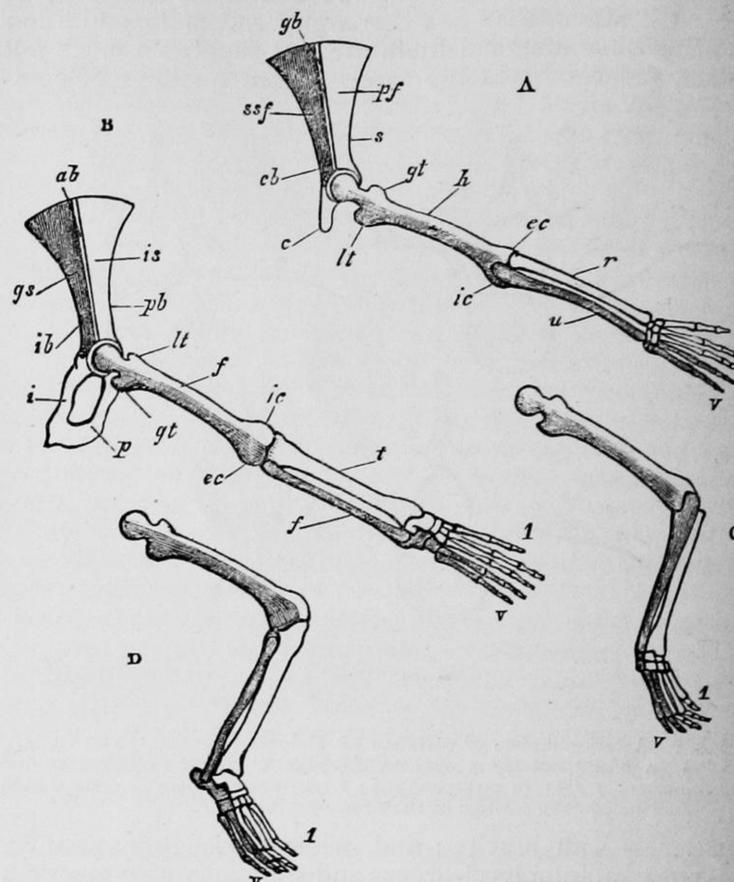


FIG. 7.—Diagrammatic representation of the positions of the limbs of Mammalia. The preaxial border is left light, the postaxial border shaded, in all the figures. Limbs of the right side are represented in all cases. A dorsal aspect of the anterior extremity in its primitive unmodified position; *gb* glenoid border of the scapula; *s* spine; *cb* coracoid border; *ssf* subscapular fossa; *pf* postscapular (infraspinal) fossa; *c* coracoid; *h* humerus; *gt* greater, radial, or preaxial tuberosity; *lt* lesser, ulnar, or postaxial tuberosity; *ec* external (in the modified position), radial, or preaxial condyle; *ic* internal, ulnar, or postaxial condyle; *r* radius; *u* ulna; I pollex; V fifth digit. B dorsal aspect of the posterior extremity in the same position; *ab* acetabular border of the ilium; *pb* pubic border; *ib* ischial border; *gs* gluteal surface; *is* iliac surface; *i* ischium; *p* pubis; *f* femur; *lt* lesser, tibial, or preaxial trochanter; *gt* greater, fibular, or postaxial trochanter; *ic* internal (in the modified position), tibial, or preaxial condyle; *ec* external fibular, or postaxial condyle; *t* tibia; *f* fibula; I hallux; V fifth digit; C the anterior extremity, with the humerus in the same position, but the elbow and wrist joints bent; D the posterior extremity in the same position.

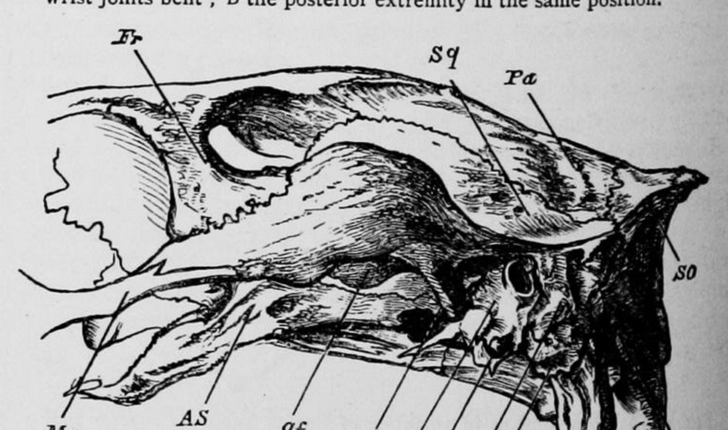


FIG. 8.—Side view of the posterior part of the skull of a Horse, ⅓. *Fr* frontal (the line points to the postorbital process); *Sq* squamosal; *Pa* parietal; *SO* supraoccipital; *ExO* exoccipital; *oc* occipital condyle; *pp* paroccipital process; *Per* mastoid portion of periotic; *pt* post-tympanic process of squamosal; *th* tympanohyal; *Ty* tympanic; *pg* postglenoid process of squamosal; *As* alisphenoid (the line points to the plate of the bone which bridges over the alisphenoid canal); *Ma* malar.

The chapters on the hand and foot (Manus and Pes) are excessively instructive and interesting; the better to understand the conditions presented, the author steps beyond the limits of the class Mammalia and exhibits the condition of the manus in a water tortoise (*Chelydra serpentina*). At the end of the work is a chapter on "The Correspondence between the Bones of the Anterior and Posterior Extremity and the Modifications of the Positions of the Limbs," and this is the only speculative and theoretical portion of the book. All the rest is a plain and clear statement of observed facts.

Some may be disposed to regret that there is not more "theory" in the work, but the reviewer believes that, considering the scope and object of the treatise, the introduction of theoretical views would be a blemish rather than a gain.

The work is addressed to students who are in earnest and really want to learn "Comparative Anatomy." Do such need to be attracted or encouraged by brilliant or startling theories, or will, on the other hand, they follow teaching which is simply the clear expression of actual facts? To this question it may be replied *solvitur ambulando*. Last year Professor Flower delivered in the theatre of the Royal College of Surgeons the very lectures which are now being reviewed, and the attendance of students was most remarkable, not only as to numbers who came but also as to their perseverance and constancy in attending.

The book is eminently a student's book. Obscurities of expression and unnecessary technicalities are carefully avoided; and any youth who provides himself with the bones of a dog, and reads it patiently, referring with care to his dog-bones all the while, may become a respectable osteologist. While if such a youth has access to a museum like that of the College of Surgeons, he may, with this work in his hand, lay the foundation of a really good knowledge of comparative anatomy; for every anthropotomist and teacher of human anatomy will admit how readily a knowledge of "soft parts" is acquired by one who thoroughly "knows his bones."

An excellent exercise for a student would be to take the skull of a rabbit and a pig, and write out a detailed description of each, modelled on the author's description of that of the dog.

Although the work is of such moderate bulk, yet owing to its convenient arrangement, and the thorough knowledge of his subject which the author professes, it is possible, by referring to it, to get an answer to almost any ordinary question respecting the bony structure of any mammal.

Professor Flower's new human bone, the *tympanohyal* (first described by him at the Liverpool meeting of the British Association), is shown to exist in other mammals, being very large, *e.g.* in the horse and the sheep.

Amongst so great a mass of facts, it is impossible but that there should not be some inaccuracies, without the slightest discredit thereby accruing to the author; for the laws of animal structure being but empirical, a "black swan" is at any moment liable to turn up and falsify the simplest and most useful generalisation. Thus it may turn out that the generalisation, "the *Simiina* are remarkable in never having an ossified stylohyal," is not strictly accurate, but the author having examined hundreds of skulls and never having found one so furnished, was amply justified in making the assertion. Any attempt to depreciate a work by carping criticism upon details of that kind can result only in discredit to the critic himself.

In conclusion, the reviewer is of opinion that Prof. Flower's "Osteology" should be warmly recommended to all students of comparative anatomy; while even to many advanced zoologists it will prove a work not only of much interest, but one conveying substantial, important, and trustworthy information.

F. R. S.

## NOTES

So far as the delegates are concerned, the amalgamation of the Ethnological and Anthropological Societies is complete, under the name of the Anthropological Institute of Great Britain and Ireland, and the separate societies have ceased to exist. All property, effects, debts, and liabilities are to be taken over by the new society. The new officials taken from the two societies are: President, Sir John Lubbock (E.), Vice-Presidents, Prof. Huxley (E.), Tylor (E.), Prof. Busk (E.), Dr. Charnock (A.), Dr. Beddoe (A.), G. Harris (A.). Treasurer, Flower (E.). Council.—Blackmore (E), Bohn (E.), Archibald Campbell (E.), Hyde Clarke (E., A.), Boyd Dawkins (E.), Dunn (E.), David Forbes (E.), Col. Lane Fox (E., A.), T. M'K. Hughes (E.), M'Lennan (E.), Pusey (E., A.), Braybrooke (A.), W. C. Dendy (A.), Dr. King (A.), F. G. H. Price (A.), Dr. Harcourt (A.), Des Ruffieres (A.). Director, C. Staniland Wake (A.); Assistant-editor of the Journal, F. W. Rudler (E.). The last meeting of the Ethnological was held on Tuesday, and the last meeting of the Anthropological will be held on Tuesday next, when the old arrangement will cease, and other meetings will be fixed by the new council.

A DETERMINATION to take advantage of any opportunity for making scientific inquiries, is the principal feature of the present crisis among French scientific men. It is carried so far that the French Institute has appointed a committee to inquire into the effects of the shelling as well on the buildings as on the inhabitants. The specialities presented by the wounds have been reviewed in a very able essay, written by Baron Larey, in the *Revue des Cours publiques*, published by Baillière, but which has not appeared regularly. Some other essays on the same subject have been written by different medical men in the same periodical, and in some political papers.

THE increase of the mortality is a feature of the present crisis in Paris. The winter number of deaths is generally 1,000 or 1,200 a week; but in the last few weeks it has amounted to 3,000. This is not owing to any particular epidemic, although the small-pox has destroyed many victims. The larger amount of fatal cases is owing to the exposure, the want of proper food suited for infants and invalids, and the scarcity of fuel. Moral causes have also seriously affected invalids and old persons to a very large extent. Affections of the lungs are prevalent in that portion of the population.

THE large conservatory for *Orchidaceæ* in the *Jardin des Plantes* at Paris has been destroyed by a Prussian shell. The glass-work was broken, and the plants, which are so delicate, were lost without any hope of recovery. The losses are very serious, as the collection had many valuable specimens obtained from the occupation of Mexico, and from the exertions of the French Scientific Commission, which had been sent to America to explore the Empire of the unfortunate Maximilian. M. Chevreuil, the Director of the Museum, has addressed to the Academy of Sciences the following protest:—"The garden of medicinal plants, founded in Paris by an edict of King Louis XIII., dated January 3, 1626, became a Museum of Natural History on the 23rd May, 1794. It was bombarded in the reign of William I., King of Prussia, Count Bismarck being Chancellor, by the Prussian army, on the night of the 8th-9th January, 1871. Until then it had been respected by all parties, and by all national and foreign authorities. Paris, January 9, 1871." The Academy has determined that the protest of M. Chevreuil shall be printed at the head of its reports, and the Committee of Professors of the Museum have decided that a marble monument, with an inscription of the protest, shall be placed in one of the galleries of the building, surrounded with projectiles thrown from the enemy's batteries.

THE Polytechnic School has been opened at Bordeaux. M. Gambetta delivered an inaugural address. The director is M. Serrey, Member of the French Academy; but the larger number of the professors are now besieged in Paris, serving in the ranks of the National Guard. They mostly belong to the artillery. Among them are M. Janin, Professor of Physics; M. Laussedat, Professor of Topography; M. Moutard, Professor of Mathematics; M. Manheim, Professor of Descriptive Geometry.

ARRANGEMENTS for instruction in Practical Chemistry have now been completed at the London Institution, Finsbury Circus, by the opening of the Chemical Laboratory, under the direction of Dr. Henry E. Armstrong, for the reception of students requiring instruction in Analytical Chemistry and the methods of Original Investigation. The evening class for Elementary Chemical Analysis will commence work on Feb. 13, and will meet three times a week, on Monday, Wednesday, and Friday, from 6 to 8 P.M. from February to May. All students must be nominated by a proprietor, and the sons of proprietors have an advantage in the fees charged.

THE Young Men's Christian Association of New York announces a course of popular scientific lectures by Dr. Doremus, on the "Triumphs of Modern Science." "The subject is an interesting one, but we regret," says the *New York Technologist*, "to see that the Association has placed the price of tickets at $1\frac{1}{2}$ dols. per lecture, or 5 dols. for the course of four. At this rate, many young men who would otherwise attend and be interested, will prefer to pay a less amount and visit Booth's, or even the opera-house of Jim Fisk, jun. We presume that the managers will claim that the enormous expenses incurred for chemical and physical illustrations render this high price necessary. This may be true in their case, but it is a pity that they and their lecturer should forget that neither Science itself nor its popularisation depends upon magnificent spectacular effects. It this were the case, then Tweed, Sweeny, and Hall, with their free entertainments given every 4th of July at the expense of the city, can beat any scientific lecturer in the country, and even Fisk, with the gorgeous scenery of his Twelve Temptations, will prove more attractive than Huxley or Tyndall. A good experiment, so extensive as to be visible in all parts of the audience hall, and so pertinent that it gives a perfect illustration of the point under discussion, is a thing that we admire above all things. But the burning of a pound of potassium, merely for the sake of making a blaze and a smoke; the burning of diamonds, which, when performed on the lecture table of a popular audience hall, proves nothing, and is the sheerest mountebankery; the dragging in of an aquarium, merely for the sake of advertising the maker's name; and in short, everything that does not aid the hearer in obtaining clear ideas in regard to the matter in hand, ought to be rigorously excluded. The simplest experiment, performed with the cheapest apparatus, becomes beautiful and interesting beyond any display of fireworks, when it clearly illustrates some great physical truth, while the most gorgeous display will, of itself alone, fail to excite that intellectual interest that is so far superior to mere physical emotion. In the name of Science, therefore, and of that intellectual progress, of which our Young Men's Christian Association should be the promoters and advocates, we protest against a system which degrades the scientific lecture to a level with the performances of Houdin or Signor Blitz." Sensible language this, and deserving of note in this country as well as America.

WE learn from the *American Technologist* that the scientific lectures delivered and to be delivered this session before the American Institute are as follows:—Tuesday evening, December 20, 1870, The Struggles of Science, by George B. Loring, M.D., of Salem, Mass.; Tuesday evening, December 27, 1870, How we stand and walk, by Prof. Burt G. Wilder, of Cornell University, Ithaca, N.Y.; Friday evening, January 6, 1871, The Triumphs of Modern Surgery, by Prof. F. H. Hamilton, of Bellevue Hospital Medical College, New York; Friday evening, January 20, 1871, On Water, by Prof. C. F. Chandler, of Columbia College, New York; Friday evening, January 27, 1871, On Tides and Tidal Currents, and their effects upon Harbours, by J. E. Hilgard, of the U.S. Coast Survey, Washington, D.C.; Friday, February 3, 1871, On Light, by Henry Morton, President of Stevens Institute, Hoboken, N.J. These lectures are free to members of the Institute and their families, and are an evidence of the earnest work which this association is accom plishing in the way of the diffusion of knowledge.

THE centenary anniversary of the birthday of Humboldt was celebrated in Boston by the raising of a subscription to the amount of 7,040 dollars, which has been placed in the hands of the trustees of the Museum of Comparative Zoology as a Humboldt Scholarship for the benefit of young and needy persons engaged in study at the Museum, and the officers of the Museum have formally accepted the trust.

THE wonderful bore of five miles through the Hoosac Mountains goes forward with persistent steadiness, and bids fair to be an accomplished fact in 1874, as promised by the contractors. Some conception of the magnitude of this great work may be formed when it is known that at the west end the workmen have fully one-third of a mile of solid mountain above their heads.

A ZOOLOGICAL Garden in Central Park, New York, has been objected to on the following grounds:—The defect of the Central Park is a lack of breadth and repose. This defect grows out of the natural limitations fixed by the original rocky surface of its site, and from the necessity of providing structurally for the convenience and safety of great throngs of people in a public pleasure-ground that is expected finally to be situated in the heart of a densely populated city. The impracticability of making, in either section of the Park, open spaces of greensward as large as desirable was recognised from the outset, but as much as possible was done to gain ground in this direction, and the central meadow stretches are the result in the upper Park. They supply two connected spaces, each about a quarter of a mile in extent, partially separated by a mass of rock and almost completely surrounded by a border of indigenous trees, which are already beginning to take on umbrageous forms and to cast broad shadows over the now well-established turf. These meadows constitute the only broad space of quiet rural ground on the island which has been left undisturbed by artificial objects, and much labour has been expended to render practicable the preservation of their present general character. A Zoological Garden must be made up to a considerable extent, if not altogether, of small scattered buildings and small fenced yards; it requires little breadth or unity of surface in its site, and it must be adapted to recreation of a completely diverse character from that which this ground has been prepared to serve.

THE busy town of Sheffield, we are glad to see, is waking up to a sense of its position as one of the industrial centres of England. A meeting was recently held to consider the propriety of presenting the Museum of the Literary and Philosophical Society to the town, on condition that a Free Public Museum be established by the Town Council, and a suitable building be erected. The President of the Society, Mr. H. C. Sorby, occupied the chair, and briefly opened the meeting. The Master Cutler, Mr. W. Bragge, then rose and proposed the following resolution, "That upon the establishment by the Town Council of Sheffield of a Free Public Museum and the provision of such accommodation for it as the Council of the Literary and Philosophical Society shall deem suitable, the said Society hereby authorises its Council to transfer to the said Free Public Museum its collections of specimens illustrating natural history, geology, mineralogy, antiquities, numismatics, ethnology, and industrial art, and also such apparatus as is of historical interest, together with the cases containing the said articles, but not its library nor modern scientific apparatus." After some discussion, the resolution was put to the meeting and carried unanimously.

DR. COBBOLD describes, in the *British Medical Journal*, an Entozoön which, if not actually, is practically unknown to the most experienced helminthologists. The *Stephanurus dentatus* s a species of Strongylus which has hitherto been described only by the late Professor Diesing of Vienna in the *Annalen des Wiener Museums*, a scarce book to obtain. It is not fully described in any of the systematic works, and appears never to have been seen by Kuchenmeister or Von Siebold. The interest of this observation consists less in the ready identification of the parasite by Dr. Cobbold, than in the abundance with which, according to Dr. Fletcher of Indianopolis, who forwards the specimen for examination, it is found in the hogs slaughtered in that part of the world. The bearing of this observation on the extension of parasitic disease remains yet to be determined. The suggestions arising out of Dr. Cobbold's observation will, no doubt, be followed out by local inquirers; and we shall expect to hear more of this hitherto rare, and probably interesting, stranger.

AT a recent meeting of the Scientific Committee of the Royal Horticultural Society, Mr. Alfred Smee exhibited some lemons from Sicily attacked by a species of coccus, quite distinct from the well-known coccus of the orange, and apparently an undescribed species. It was stated that nearly the whole of the lemon-crop in Sicily is attacked by this parasite, which renders it almost valueless for the English market. Although the juice is not much affected, the skin is completely spoilt and rendered uncrystallisable; by far the most important use of lemons in this country being of the rind for use by confectioners. The root appears to be at the same time attacked by a fungus.

AT the close of 1870 the numbers of the several classes belonging to the Institution of Civil Engineers were :—16 honorary members, 709 members, 1,010 associates, and 201 students, together 1,936, as against 1,802 at the same date last year, showing an increase at the rate of 7½ per cent. in the twelve months.

IN analysing the statistics of inquests held as Coroner of Central Middlesex, Dr. Lankester points out, in his seventh annual report just prepared, that the proportion of suicides to the population in England and Wales is 1 in 12,000 of the population, while the proportion in Central Middlesex is about 1 in 13,000 of the population. The figures seem to show that of all causes of death suicide is the most constant. The proportion in which the sexes commit suicide is nearly everywhere the same. It may be stated that the proportion of males to females is as five to two. The ages at which suicide is committed are for the seven years nearly the same. One in twelve are young people under 20 years of age; a larger proportion amongst people above 60; and the remainder, four-fifths of the whole, are equally divided amongst people from 20 to 40 years of age. A further analysis of the cases shows that, as a rule, women prefer taking poison and drowning themselves. Of the twenty-three cases of female suicide in 1868-9, six were from poison and ten from drowning. Women seldom cut their throats or hang themselves, whilst, of the sixty-six cases of male suicide, exactly half chose these methods of self-destruction. Men are also more given to jumping out of windows and from the tops of high places.

AN admirable paper on "The Food and Habits of Beetles," by Mr. Townend Glover, appears in a recent report of the Commissioner of Agriculture for Washington. It contains brief descriptions of the beetles which are injurious to vegetable or animal substances, with their common and scientific names, and nearly 200 figures, roughly but characteristically executed. An alphabetical list of the principal substances frequented by beetles is given at the end, with cross references to the first part of the paper.

THE Winchester and Hampshire Scientific and Literary Society, which was formed in 1869, has issued its first report. From this we learn that over 100 members have joined, and that the meetings are frequent and well attended. The papers are not confined to subjects connected with natural history, but embrace archæology and kindred topics. There is a botanical section, of which Mr. Frederick J. Warner is secretary; and we gather from the report that a flora of Winchester is in contemplation. It is perhaps not too much to hope that, with the aid of the Newbury Club, a complete flora of Hampshire may be undertaken by the society. The President is the Rev. C. A. Johns, Mr. A. Angell, jun., acting as the hon. secretary. The meetings are held on the second Monday of each month. The annual subscription is 10*s*.

THE *Scottish Naturalist* states that a work on the Birds of Scotland, by Mr. Robert Gray, Secretary to the Glasgow Natural History Society, is in the press; and that Mr. Howie, Secretary of the Largo Naturalists' Field Club, is drawing up, for publication, a catalogue of the plants of Fifeshire.

A JOURNAL is published in Liverpool, under the title of "Cope's Tobacco Plant, a Monthly Periodical, interesting to the Manufacturer, the Dealer, and the Smoker," from the January number of which we quote the following :—"Huxley and Friends—Attention! An American exchange says:—'Professor Huxley may become a good Christian yet; he smokes now, after forty years' hostility to tobacco.' Now, at cost of losing ground for ourselves, we ask Professor Huxley and his friends—Will they admit that the distinguished gentleman was not a Christian during those forty years when he abstained from tobacco? And that the first step towards his salvation was—smoke? We expect an answer; because, should we be in danger of yielding to the arguments of zealous anti-tobaccoites, we might fall back upon that most infallible of argument-stoppers, the religious sentiment."

A LETTER in the *Gardener's Magazine*, which has just come to our notice, imputes a grave charge of false teaching to the authorities of the South Kensington Museum. The writer says:—"In the Educational Series is a large glass case of British butterflies, conspicuously labelled 'The Gardener's Foes.' The case contains forty-three out of the sixty-six native species. Now, the idea of all these, or even the majority of them, being injurious, is most absurd, and tends to perpetuate erroneous notions, and to exterminate these beautiful creatures from the land." Then follows an enumeration of the species exhibited, and the names of the plants upon which, according to the writer, the insects actually feed, most of them being roadside weeds. We draw attention to this in order that "erroneous notions," on whichever side they may be, may *not* be "perpetuated."

GOLD ore having been discovered in Madagascar, the Government of the island has prohibited the search. If gold is discovered in remunerative quantities, there will be such a rush of Europeans to the country as will dispossess the native inhabitants.

THE African diamond fields are being overdone; they have become the seat of an extensive population. A little canvas town has rapidly sprung up on the banks of the Vaal River. Stores have been opened, the rival proprietors of which advertise their wares in a newspaper devoted to the diamond interest, and printed on the field; and, finally, a music-hall has been started for the amusement of the thousands of diggers, who with their wives and children are now encamped in the "happy valley."

## SCIENCE IN AMERICA

THE following appropriations by the U.S. Congress were made at the session of 1869-70 for the ensuing year, July 1, 1870, to June 30, 1871, in aid of Science, Literature, &c.

It should be observed that the undermentioned appropriations are those of the General Government, and not those of the separate States, which, in the aggregate, would far exceed the amount here presented.

| | | $ | $ |
|---|---|---|---|
| *Museums.* | | | |
| National Museum in charge of Smithsonian Institution | | 20,000 | |
| Army Medical Museum | | 5,000 | |
| Agricultural Departmt. Museum | | 8,000 | |
| | | | 33,000 |
| *Botanic Gardens and Greenhouses.* | | | |
| Of the U.S. Capitol | | 35,996 | |
| " " President's House | | 2,500 | |
| " " Agricultural Department | | 38,200 | |
| | | | 76,696 |
| *Agriculture.* | | | |
| Department of Agriculture, Miscellaneous Expenses | | | 138,070 |
| [To this is to be added, items already given,— | | | |
| Botanic Garden and Living Plants | 38,200 | | |
| Museum | 8,000 | | |
| Library | 3,800 | —50,000 | |
| or an aggregate of $188,070.] | | | |
| *Astronomy and Meteorology.* | | | |
| Observations of Eclipse, Dec. 1870, under Coast Survey | | 29,000 | |
| U.S. Nautical Almanac | | 20,000 | |
| National Observatory | | 19,800 | |
| New Telescope for National Observatory | | 50,000 | |
| Telegraphic Notices of Storms | | 50,000 | |
| | | | 168,800 |
| *Surveys, &c.* | | | |
| U.S. Coast Survey | | 703,000 | |
| Survey of Lakes | | 150,000 | |
| " Nicaragua and Tehauntepec Ship Canals | | 30,000 | |
| Military Surveys west of Mississippi | | 100,000 | |
| Prof. Powell's Survey of Colorado of West | | 12,000 | |
| Polar Explorations | | 50,000 | |
| Dr. Hayden's Geological Survey | | 25,000 | |
| Statistics of Mines and Mining | | 10,000 | |
| | | | 1,080,000 |
| *Light-house Establishments* | | | 1,431,207 |
| *Libraries.* | | | |
| Library of Congress | | 36,220 | |
| " of Medical Department, U.S.A. | | 3,000 | |
| " of Agricultural Department | | 3,800 | |
| | | | 43,020 |
| *Education.* | | | |
| U.S. Department of Education | | 14,500 | |
| Wilberforce and Lincoln Universities | | 37,000 | |
| | | | 51,500 |
| *Benevolent Objects.* | | | |
| Life-boat Service on the Coast | | 48,883 | |
| Government Hospital for Insane | | 149,980 | |
| Columbia Institution for Deaf and Dumb | | 40,775 | |
| Columbia Hospital for Women | | 18,000 | |
| National Association for Destitute Coloured Women, D.C. | | 10,000 | |
| National Soldiers' and Sailors' Home, D.C. | | 15,000 | |
| Care of 60 transient Paupers | | 12,000 | |
| | | | 234,635 |
| Total | | | $ 3,316,928 |

## THE INFLUENCE OF INTENSE COLD ON STEEL AND IRON

THERE has recently been a most interesting discussion at the Literary and Philosophical Society, Manchester, on the above subject, the result of which seems to be that we must at once give up the idea that such accidents as the one, for instance, near Hatfield, are due to anything beyond the control of the Railway Companies concerned.

The paper which gave rise to the discussion was by Mr. Brockbank, who detailed many experiments, and ended by stating his opinion that iron does become much weaker, both in its cast and wrought state, under the influence of low temperature; but Mr. Brockbank's paper was immediately followed by others by Sir W. Fairbairn, Dr. Joule, and Mr. Spence, which at once put an entirely new complexion on the matter.

As Dr. Joule's results are the most to the point we may take them first. He says:—

"As is usual in a severe frost, we have recently heard of many severe accidents consequent upon the fracture of the tires of the wheels of railway carriages. The common-sense explanation of these accidents is, that the ground being harder than usual, the metal with which it is brought into contact is more severely tried than in ordinary circumstances. In order apparently to excuse certain Railway Companies, a pretence has been set up that iron and steel become brittle at a low temperature. This pretence, although put forth in defiance, not only of all we know of the properties of materials, but also of the experience of everyday life, has yet obtained the credence of so many people that I thought it would be useful to make the following simple experiments:—

"1st. A freezing mixture of salt and snow was placed on a table. Wires of steel and of iron were stretched so that a part of them was in contact with the freezing mixture, and another part out of it. In every case I tried the wire broke outside of the mixture, showing that it was weaker at 50° F. than at about 12° F.

"2nd. I took twelve darning needles of good quality, 3in. long, $\frac{1}{24}$in. thick. The ends of these were placed against steel props, $2\frac{1}{8}$in. asunder. In making an experiment, a wire was fastened to the middle of a needle, the other end being attached to a spring weighing-machine. This was then pulled until the needle gave way. Six of the needles, taken at random, were tried at a temperature of 55° F., and the remaining six in a freezing mixture which brought down their temperature to 12° F. The results were as follow:—

| Warm Needles. | Cold Needles. |
|---|---|
| 64 oz. broke | 55 oz. broke |
| 65 " " | 64 " " |
| 55 " " | 72 " " |
| 62 " " | 60 " bent |
| 44 " " | 68 " broke |
| 60 " bent | 40 " " |
| Average $58\frac{1}{3}$ | Average $59\frac{5}{8}$ |

"I did not notice any perceptible difference in the perfection of elasticity in the two sets of needles. The result, as far as it goes, is in favour of the cold metal.

"3rd. The above are doubtless decisive of the question at issue. But as it might be alleged that the violence to which a railway wheel is subjected is more akin to a blow than a steady pull; and as, moreover, the pretended brittleness is attributed more to cast iron than any other description of the metal, I have made yet another kind of experiment. I got a quantity of cast-iron garden nails, an inch and a quarter long and ⅛in. thick in the middle. These I weighed, and selected such as were nearly of the same weight. I then arranged matters so that by removing a prop I could cause the blunt edge of a steel chisel, weighted to 4lb 2oz., to fall from a given height upon the middle of the nail as it was supported from each end, 1 1/16in. asunder. In order to secure the absolute fairness of the trials the nails were taken at random and an experiment with a cold nail was always alternated with one at the ordinary temperature. The nails to be cooled were placed in a mixture of salt and snow, from which they were removed and struck with the hammer in less than 5s."

The collective result of the experiments, the details of which need not be given, was that 21 cold nails broke and 20 warm ones.

Dr. Joule adds, "The experiments of Lavoisier and Laplace, of Smeaton, of Dulong and Petit, and of Troughton, conspire in giving a less expansion by heat to steel than iron, especially if the former is in an untempered state. Such specimens of steel-wire and of watch-spring as I possess expand less than iron. But this, as Sir W. Fairbairn observed to me, would in certain limits have the effect of strengthening rather than of weakening an iron wheel with a tire of steel.

"The general conclusion is this: Frost does *not* make either iron (cast or wrought) or steel brittle, and that accidents arise from the neglect of the companies to submit wheels, axles, and all other parts of their rolling stock to a practical and sufficient test before using them."

Mr. Spence in his experiments decided on having some lengths of cast-iron made of a uniform thickness of ½in. square, from the same metal and the same mould.

He writes:—"Two of the four castings I got seemed to be good ones, and I got the surface taken off, and made them as regular a thickness as was practicable.

"I then fixed two knife-edged wedges upon the surface of a plank, at exactly nine inches distance from each other, with an opening in the plank in the intervening space, the bar being laid across the wedges, a knife-edged hook was hung in the middle of the suspended piece of the bar, to the hook was hung a large scale on which to place weights.

"The bar was tried first at a temperature of 60° F.; to find the breaking weight I placed 56lb. weights one after another on the scale, and when the ninth was put on the bar snapped. This was the only unsatisfactory experiment, as 14 or 28lb. might have done it, but I include it among the others. I now adopted another precaution, by placing the one end of the plank on a fixed point and the other end on to a screw-jack, by raising which I could, without any vibration, bring the weight to bear upon the bar. By this means, small weights up to 7lb. could be put on while hanging, but when these had to be taken off and a large weight put on, the scale was lowered to the rest, and again raised after the change was made. I may here state that a curious circumstance occurred twice, which seems to indicate that mere raising of the weight, without the slightest apparent vibration, was equal in effect to an additional weight. 3¾cwts. were on the scale, a 14lb. weight was added, then 7lb., then 4lb., 2lb., 1lb., and 1lb., making 4cwts. and 1lb. This was allowed to act for from one to two minutes, and then lowered to take off the small weights, and replaced by a 56lb., intending to add small weights when suspended, raised so imperceptibly by the screw, that the only way of ascertaining that it was suspended was by looking under the scale to see that it was clear of the rest. As soon as it was half-an-inch clear it snapped, thus breaking at once with one pound less than it resisted for nearly two minutes.

"Six experiments were carefully conducted at 60° F., the parts of the bars being selected so as to give to each set of experiments similar portions of both bars; the results are marked on the pieces. My assistant now prepared a refrigerating mixture which stood at zero, and the bars were immersed for some time in this, and we prepared for the breaking trials to be made as quickly as could be, consistently with accuracy, and to secure the low temperature each bar on being placed in the machine had its surface at top covered with the freezing mixture. *The bars at zero broke with more regularity than at 60°, but instead of the results confirming the general impression as to cold rendering iron more brittle, they are calculated to substantiate an exactly opposite idea, namely, that reduction of temperature,* cæteris paribus, *increases the strength of cast iron.* The only doubtful experiment of the whole twelve is the first, and as it stands much the highest, the probability is that it should be lower; yet, even taking it as it stands, the average of the six experiments at 60° F. gives 4cwt. 4lb. as the breaking weight of the bar at that temperature, while the average of the six experiments at zero gives 4cwt. 20lb. as the breaking weight of the bar at zero, being an increase of strength from the reduction of temperature equal to 3·5 per cent."

Sir W. Fairbairn's evidence is of great importance, for he not only gives facts showing that frost does not affect the tires, but he states the real cause of such accidents as are generally attributed to the frost.

He states:—"It has been asserted in evidence given at the coroner's inquest on the Hatfield accident, that the breaking of the steel tire was occasioned by the intensity of the frost, which is supposed to render the metal brittle, and of which this particular tire was composed. This is the opinion of most persons, but judging from my own experience such is not the fact, and provided we are to depend on actual experiment, it would appear that temperature has little or nothing to do with it. Some years since I endeavoured to settle this question by a long and careful series of experiments on wrought-iron, from which it was proved that the resistance to a tensile chain was as great at the temperature of zero as it was at 60° or upwards, until it attained a scarcely visible red heat. To show that this was the case, and taking, for example, the experiments at 60°, it will be found that the mean breaking weight, in tons, per square inch, was in the ratio of 19·930 to 21·879, or as 1 : 1·098 in favour of the specimens broken at the temperature of zero. The generally received opinion is, however, against these facts, and it is roundly asserted that the strength of iron and steel is greatly reduced in strength at a temperature below freezing. The contrary was proved to be the case in wrought-iron plates, and assuming that steel follows the same law, it appears evident that we must look for some other cause than change of temperature for the late fracture of the tire on the wheel of the break-van of the Great Northern Railway. . . . The immense number of purposes to which both iron and steel are applied, and the changes of temperature to which they are exposed, renders the inquiry not only interesting in a scientific point of view, but absolutely necessary to a knowledge of their security under the various influences of those changes; and when it is known that most of our metal constructions are exposed to a range of temperatures varying from the extreme cold of winter to the intense heat of summer, it is assuredly desirable to ascertain the effects produced by those causes on material from which we derive so many benefits, and on the security of which the safety of the public frequently depends. It was for these reasons that the experiments in question were undertaken, and the summary of results are sufficiently conclusive to show that changes of temperature are not always the cause of failure, as that which occurred near Hatfield on the Great Northern Railway. That such is the fact, I may adduce several accidents of broken tires all of which occurred during the spring and summer months when the temperature was high. One of them occurred on the Lancashire and Yorkshire Railway in the summer of last year when the temperature was 50° to 60° above freezing. I could enumerate others in which the winter frosts had nothing to do with the fractures which ensued."

After referring to some other experiments, Sir W. Fairbairn proceeded: "The danger arising from broken tires does not, according to my opinion, arise so much from changes of temperature *as from the practice of heating them to a dull red heat, and shrinking them on to the rim of the wheels.* This, I believe, is the general practice, and the unequal, and in some cases, the severe strains to which they are subject, has a direct tendency to break the tires. To show how easily this may be effected, let us suppose that a tire, two feet six inches or three feet diameter, is shrunk on to a wheel one-tenth of an inch larger than the tire, it then follows that the tire in cooling must be elongated to that extent, with a strain equivalent to the force of the shrinkage, and calculated to produce that amount of molecular disturbance. It may be more or it may be less, but supposing the strain to be one-half or three-fourths of that which would break the tire, it then follows that the constant action of its irregular motion on the rails must ultimately lead to fracture.* I am not surprised that this should be the case, as most, if not the whole, of railway tires, excepting those on engines and tenders, are not turned, but selected by hand, heated and shrunk upon the wheels with every degree of tension, as suits the convenience of the workman. So long as this process is pursued the public will be exposed to the risk of broken tires. What is required in this description of manufacture is, that the rim of the wheel and the inside of the tire should be *turned to a standard gauge*, accurately calculated to give the required amount of tightness with a larger margin of strength, and this done we should attain greatly increased security to the public, and a great saving in wear and tear—to say nothing of the large sums expended by companies in the shape of compensation for injuries and loss of life."

Here, then, is another potential triumph for more scientific accuracy and more hope for travellers.

* From long-continued action under strain, it has been proved that it is only a question of time when rupture takes place, as repeated increased and diminished changes with the same load ultimately leads to fracture.

## *SCIENTIFIC SERIALS*

*Poggendorff's Annalen der Physik und Chemie*, 1870, *No.* 9.—The contents of this number are:—(1.) "Calorimetric Researches," by R. Bunsen. In the first part of this paper Prof. Bunsen describes the construction and method of using a new calorimeter, in which quantities of heat are measured by the amount of ice at 0° which they are capable of converting into water at the same temperature. The quantity of ice melted is in its turn indicated by the resulting diminution of volume, as shown by the movement of a mercury-column in a graduated capillary tube communicating with the vessel in which the ice is contained. In order to convert the results obtained by this method into absolute heat-units, it is necessary, either that the motion of the mercury-column produced by a known quantity of heat should be ascertained, or that the specific gravity of ice at 0° and its latent heat of fusion should be known. The first of these quantities was found by observing the effect produced by a given weight of boiling water, and the second by a process which may be described as consisting in the application of the principle of the weight-thermometer to measure the change of volume which water undergoes on freezing. From these data the third of the quantities mentioned, or the latent heat of fusion of ice, is readily calculated. Of the numerical results, given in the paper, we will quote only the following:—

| | |
|---|---|
| Specific gravity of ice at 0° C . . . . . | 0·91674 |
| Latent heat of fusion of ice . . . . . . | 80·025 |
| Specific heat of indium . . . . . . . | 0·0570 |
| Specific heat of calcium . . . . . . . | 0·1704 |

One special advantage of this method of calorimetry is that it allows good results to be obtained with very small quantities of material; for instance, for specific heat determinations, from 0·3 gramme to, at the most, 4 grammes is sufficient. (2.) "On the relations between the crystalline form and chemical constitution of some organic compounds," by P. Groth. (3.) "Experimental and theoretical investigation of the figures of Equilibrium of a liquid mass without weight" (Eighth series), by J. Plateau. A translation of this paper, which relates to the conditions of the ready production and of the persistency of liquid films, to the superficial tension of liquids, and to their superficial viscosity, was printed in the *Philosophical Magazine* vol. xxxviii. p. 445 [1869.] (4.) "On the Absorption of Light," by Paul Glan. Among other results, the author finds that the absorbing power of a substance, when it is employed in solutions of different degrees of concentration, increases in a greater ratio than the concentration; also, that the absorbing power of a body in solution is affected by the nature of the medium in which it is dissolved. The experimental results are followed by a mathematical discussion of the mechanism of the absorption of light. (5.) "Additional researches into the behaviour of Vapours in relation to the Laws of Mariotte and Gay-Lussac," by Dr. Hermann Herwig. This paper has reference to an earlier one published in vol. cxxxvii. of *Poggendorff's Annalen.* The author finds that, when the pressure upon a vapour at a given temperature is diminished so far that the vapour obeys Mariotte's law, that is to say, so far that the product of the pressure into the corresponding volume becomes constant, this product bears to the similar product, when the pressure is great enough to cause the vapour to be saturated at the same temperature, a constant ratio which is proportional to the square root of the absolute temperature. In the present paper it is shown that ethylic bromide and carbonic sulphide conform to this law. (6.) "Some analogous Theorems in Photometry and in the Laws of Attraction," by Wilhelm von Bezold. The mathematical law of the inverse square of the distance applying equally to the illumination produced by a luminous point, and to the force exerted by an attracting particle, it follows that the mathematical expressions by which photometrical relations are expressed, will also admit of an interpretation in relation to the action of attracting particles. In this paper the double interpretation of the same formula is pointed out in several important cases. For example:—The author shows that the photometrical analogue of an equipotential surface drawn about several attracting particles, is a surface so placed, relatively to luminous points, whose luminosity is proportional to the masses of the particles, that the illumination of each element of the surface is greater than that of any other element passing through the same point. (7.) "On the Luminosity of Phosphorus," by W. Müller. The author finds that phosphorus vapour is not luminous in the absence of free oxygen; that it is not luminous at ordinary atmospheric temperatures when in contact with pure oxygen of atmospheric pressure, but that it becomes luminous, and at the same time absorbs oxygen, when the pressure is diminished to a certain amount, depending on the temperature, the necessary reduction of pressure being greater when the temperature is lower; and that phosphorus which has been for some time in contact with certain vapours, (notably hydrocarbons), is deprived by them of the property of becoming luminous on the admission of air, although air, mixed with the same vapours, is not thereby deprived of the power of exciting (temporary) luminosity in phosphorus. (8.) "On the Superoxides that can be prepared by Electrolysis," by W. Wernicke. (9.) "On a mechanical theorem applicable to Heat," by R. Clausius. (10) "On the Spectra of negative Electrodes, and of long-used Geissler'sT ubes," by Prof. Edm. Reitlinger and Prof. Moriz Kuhn. (11.) "On the Meissner Lignite modified by contact with Basalt," by Dr. A. von Lasaulx. (12.) "On the analysis of Silicates," by E. Ludwig. Refers chiefly to the precautions required for the accurate separation of silica and alumina. (13.) "On the absorption-spectrum of liquid peroxide of Nitrogen," by August Kundt. On comparing the absorption-spectra of liquid and gaseous peroxide of nitrogen, the author found that the ill-defined black bands in the spectrum of the former coincided in position with strongly-marked groups of lines in the spectrum of the latter. (14.) "On the work done by Gases in Motion, or remarks on the paper so entitled," by Dr. A. Kurz. This is a reply to a criticism by Dr. Boltzman (noticed in NATURE, vol. ii. p. 364) of a previous paper by the author.

## SOCIETIES AND ACADEMIES

### London

**Zoological Society**, January 17.—Prof. Newton, F.R.S., V.P., in the chair. The Secretary read a report on the additions to the Society's menagerie during the month of December 1870, amongst which were particularly noticed a specimen of the Two-toed Amphiuma (*Amphiuma didactylum*), and an example of Erxleben's Monkey (*Cercopithecus Erxlebeni*). On concluding his report the Secretary called attention to the registers of accessions to and deaths in the Society's menagerie, which lay on the table, and showed, in contradiction to statements recently published by Dr. Gray, that they were faithfully kept up, and that a revised abstract of the former was published every year as an appendix to the Society's "Proceedings."—Mr. Howard Saunders exhibited and made remarks on a series of skins of eagles belonging to *Aquila imperialis*, *A. bifasciata*, and *A. nævioides*.—A letter from Mr. R. Brown was read, recommending the introduction of hogs into countries where poisonous serpents were frequent, in reference to a communication from the Governor of Santa Lucia, read at the last meeting of the Society.—Mr. Jules Verreaux made some remarks on the facility with which the colouring matter in the wings of the Touracous was soluble, and stated that he had observed it washed out in the living birds by heavy rain.—Mr. J. E. Harting exhibited and made remarks on a specimen of Sabine's Snipe, recently killed in Ireland.—Mr. Sclater made some remarks on the amphibians usually called Axolotls, now living in the Society's gardens, and pointed out that if what Prof. Baird had recently stated were correct, these were not the true Axolotl of the lakes of Mexico (*Siredon mexicanum*), but the larval form of a known Salamander—*Amblystoma mavortium*, Baird. Mr. Sclater likewise exhibited a typical specimen of *Ateles variegatus*, Wagner, and pointed out its unquestionable identity with *A. Bartletti*, Gray.—Mr. J. W. Clark read a paper on a fine skull of the Narwhal (*Monodon monoceros*) with two tusks in the Cambridge University Museum, to which were added full particulars as to all the known bidentate skulls of this animal.—A paper was read by Dr. J. C. Cox, C.M.Z.S., containing descriptions of some new species of Australian land-shells.—Prof. Newton exhibited and made remarks on some new and rare birds' eggs, amongst which were those of the Sanderling (*Calidris arenaria*) and Lesser Sheath-bill (*Chionis minor*).—Mr. St. George Mivart pointed out the characters of a new genus of Insectivorous Mammals proposed to be called *Hemicentetes*, founded on the *Erinaceus madagascariensis* of Shaw, to which was added a revised synopsis of the known genera of the order Insectivora.—A communication was read from Mr. A. G. Butler, containing descriptions of some new species of Exotic Lepidoptera.—Mr. G. F. Angas communicatad a list of additional species of marine mollusca to be included in the Fauna of Port Jackson and the adjacent coasts of New South Wales.—Mr. Sclater read some notes on the typical specimens of *Tyrannula mexicana*, Kaup, and *T. barbirostris*, Swainson.—A second communication from Mr. Sclater contained remarks on certain species of *Dendrocolaptidæ*, in the collection of the Smithsonian Institution.

**Geological Society of London**, January 11.—Joseph Prestwich, F.R.S., President, in the chair. The following communications were read:—1. "On the older Metamorphic Rocks and Granite of Banffshire," by T. F. Jamieson, F.G.S. The author indicated three divisions in the metamorphic strata a Banffshire:—At bottom of great thickness of arenaceous beds, more or less altered into quartz-rock, gneiss, and mica-schist; next a series of fine-grained clay-slates, in the midst of which is a bed of limestone; and then again an upper group of arenaceous strata. The author stated that the arrangement of the rocks is very similar to that occurring in Bute and Argyleshire. He remarked that the general texture of the beds is fine-grained, and considered that they were probably deposited in the depths of the sea, off the mouth of a great river, the deposition of the argillaceous strata having taken place during a period of increased depression. The deposition of the beds was said to have probably taken place after the formation of the (Cambrian) Red Sandstone and Conglomerate of the North-west Highlands, or in Lower Silurian times, the river by which the sediment was brought down being supposed to have drained the great Laurentian region to the north-west. After their accumulation the author supposed that "a glow of heat from beneath" approached them, causing expansion and the wrinkling of the mass into folds running from S.W. to N.E. The granites were considered by the author to owe their origin to the fusion and recrystallisation of the arenaceous beds. Prof. Ramsay observed that the general section wonderfully corresponded with that given many years ago by Sir Roderick Murchison, of the Silurian and Laurentian rocks at Cape Wrath, and it seemed to him that the large views originally propounded by Sir Roderick were confirmed by the author. He was glad that the metamorphic origin of granite was supported by Mr. Jamieson, as he had held that view for several years, and he was pleased to find that opinions which had formerly met with so many opponents were constantly gaining acceptance. The fusion of these sedimentary rocks by metamorphic action was not identical with the fusion of lava, but their fluidity might be the same; and if that were the case there could be no difficulty in accepting the possibility of the injection of such fused rocks into crevices and fissures. The crumpling of the beds, however, was due to more extensive causes than those contemplated by the author. The proportion of igneous rock injected into contorted rocks, like those of North Wales, was almost infinitesimal, and the crumpling could hardly be due to mere local causes.—Prof. Ansted referred to what he had observed in the north-west part of Corsica, where about 40ft. of granite were distinctly interstratified between perfectly unmetamorphosed beds of sandstone and limestone, without any alteration at the points of contact, such as would be produced by an igneous rock. He also cited the crumpled strata in the Maritime Alps, in which the granites were parallel with the other beds, and seemed to form part of them. Mr. Carruthers mentioned that the late Prof. Fleming twenty years ago had taught the same doctrine as to the nature of granite as that held by the last speakers.—Mr. David Forbes agreed that the crumpling of the strata was not due to the intrusion of any eruptive rock. He completely disagreed with Prof. Ramsay and the author as to the origin of granite, and maintained that, in the sedimentary rocks traversed by the granite, the requisite ingredients for the formation of granite did not exist. The proportion of felspar in quartzose rocks was infinitesimally small, as compared with that entering into the composition of granite. He could not accept the notion of the heat from the interior approaching gradually to some portion of the surface. Prof. Ramsay, in reply to Mr. Forbes, maintained that some of the slaty rocks of Wales, by extreme metamorphism, would pass into some kinds of granite. As to the conditions of metamorphism of the rocks, this process must have gone on at a time when these older rocks were overlain by a great thickness of more recent beds which have since been removed by denudation. 2. "On the connection of Volcanic action with changes ot Level," by Joseph John Murphy, F.G.S. The author commenced by discussing the chemical theory of volcanic action, which he considered he had disproved. He remarked on the coincidence of volcanic action with elevation of the surface, but stated his opinion that the elevation of one part of the earth's surface and the depression of another, are the results of a movement of subsidence in the following manner:—The interior of the earth is constantly cooling, and as it cools it must contract. But the cold strata of the surface cannot contract in the same proportion; and as they must remain in contact with the core, they are compelled to form folds and ridges. The breaking out of volcanoes is due to the breaking of part of the earth's crust by these foldings. According to the author, "volcanic action is not the cause, but the effect of secular changes of level; and secular changes of level are due to the subsidence of the surface on the interior, as the interior contracts in cooling." 3. "On some points in the Geology of the neighbourhood of Malaga," by Don M. de Orueba. Communicated by Sir R. I. Murchison, F.R.S., F.G.S. After referring to the writings of previous authors upon the geology of the south of Spain, the author noticed a mountain-chain near Antequera, one branch of which, known as the "Torcal," he described as presenting a very singular appearance from the huge blocks of stone of which it is composed. The division of the rock into separate blocks, often of the most fantastic shapes, was attributed by the author to denudation by water. The "Torcal" consists of a compact limestone, generally of a red colour, resting conformably on the east upon a fine-grained white oolitic marble of considerable thickness. At the divisional line between the two formations many Ammonites were said to occur, and three of these were doubtfully identified with *A. giganteus*, *biplex*, and *annulatus*. These species would indicate the deposit to be probably of Portlandian age. The plain of Antequera was considered by the author to consist of Tertiary formations. One of these, at the south of the city, he regarded as analogous to the

"Calcaire grossier." He mentioned indications of the presence in the vicinity of a Miliolitic marble, and of a limestone containing Nummulites. Between Antequera and the Torcal, he noticed a small calcareous deposit containing many forms of *Gryphæa*. The paper was illustrated by photographs of two scenes on the Torcal, and of several species of Ammonites. Prof. Ansted remarked that the condition of the Torcal was similar to that prevailing in many other limestone districts, and was probably due to subaerial denudation. Mr. W. W. Smyth mentioned that he had lately had an opportunity of examining, at Cadiz, a collection of fossils formed by Mr. Macpherson in that district, which also contained specimens of Ammonites. It appeared that there were large tracts in which the rocks appeared almost destitute of fossils, which rendered their classification extremely difficult; and great credit was due to the author for his exertions in a country where unfortunately so little interest was taken in geology. He mentioned that some of these unfossiliferous rocks had been classified as Silurian by some French geologists; but for this there was not the slightest evidence. It appeared far more probable that they were of Jurassic age. Some red beds, which had been called Triassic, were also in all probability Tertiary. Mr. Gwyn Jeffreys, who had examined several collections in Spain and Portugal, stated that he had been much struck with the absence of newer Tertiary fossils, the latest being of Miocene age. These latter presented a tropical aspect, and differed from the mollusca now inhabiting the neighbouring seas. Mr. Blake was not satisfied with the determination of the Ammonites, which appeared to him rather of Cretaceous than Jurassic forms. Mr. Tate observed that the French geologists had determined the existence in Spain of the whole Jurassic series, from the Lower Lias to the Portlandian beds; and, judging from the photographs, he should consider the Ammonites to be Jurassic. Mr. Boyd Dawkins cited the remains of *Rhinoceros etruscus*, procured by the late Dr. Falconer at Malaga, as affording evidence of the presence of Pliocene age in that district. Prof. Duncan mentioned that he had found corals of the genus *Flabellum*, such as were found in the Tejares clays, in recent deep-sea dredgings in the Atlantic, and among specimens brought from Japan.

**Linnean Society**, January 19.—Mr. G. Bentham, president, in the chair. "Historical Notes on the Radix Galangæ of Pharmacy," by D. Hanbury, F.R.S. The introductionof this drug into Europe appears to have been due to the Arabians; its common use in the West does not date earlier than the 15th century. It is an aromatic stimulant, and may be used to replace ginger; but the high virtues ascribed to it by the ancients cannot be sustained. —"On the Vegetation of the Solomon Islands," by Mr. J. Atkin. The writer had spent some months in these little-known islands, chiefly in Christoval, the southernmost of the group, which lies between 10° and 11° S. lat., and between 162° and 163° E. long. The whole group extends for about 300 miles eastwards to Papua, over 4½° of longitude and 4° of latitude. They are mainly of volcanic origin; the low lands consist of coral, which reaches to an elevation of from 300 to 500 feet. Earthquakes are very frequent, almost every month, but not very severe. The nearest active volcano is Tinkalu, 200 miles to the westward. The wet season is in winter, especially the early part of July, when an enormous quantity of rain falls. The temperature is remarkably uniform; the writer had never seen the thermometer below 75° F. or above 88° in the shade, or 132° in the sun; the air is extremely damp. The highest land in Christoval is from 3,000 to 4,000 feet elevation, and is probably granite. The island is entirely covered with vegetation, except near the sea. Grasses are very few. In the forests are very few trees with trunks five feet in diameter. The bush is very thick, and climbers numerous. One Aroid was noticed, and eight or nine Orchids, all epiphytal. Several Zingibers, including the true ginger, native. Three or four species of Pandanus, which are extremely variable. The cocoa-nut and sago palms are native, the latter growing eighty feet high; also the areca-palm, and the betel-nut; the latter is universally chewed. The yam is grown, as well as five other roots probably belonging to the same order. The bread-fruit is abundant; and a variety of mango grows wild, as well as a bitter orange. The leaf of the sago-palm is used for thatch. There is a Cycas thirty or forty feet high, which is sometimes branched. Of ferns the genera most observed were Asplenium and Acrostichum; but no tree-ferns, although they are so abundant in the neighbouring Banks's group. Two Convolvuli were noticed and an Ipomœa; two Hibisci, two Casuarinæ, and two Acacias, a tree and a shrub; also a Begonia, the same species as in Banks's group; and a handsome species of nettle. The men are short, with dark curly hair. They use spears, and sometimes bows and arrows; their canoes are very beautifully ornamented. Animals are comparatively few. Dogs and pigs are abundant, both apparently native; also opossums and a small rat. There are many beautiful birds; the white cockatoo is never seen, though so abundant in islands separated by a channel only fifteen miles broad. Insects are plentiful. Snakes, both land and water, abound, but none are poisonous. Scorpions are numerous but small. Alligators were found, but not abundant. Frogs plentiful; lizards innumerable; one iguana was seen four feet long.—"Note on *Byrsanthus*," by Dr. M. T. Masters, F.R.S. The chief interest of this paper lay in the author's exposition of the relation between the glands and the perfect stamens.

## DIARY

*THURSDAY*, January 26.

Royal Society, at 8.30.—On the Mineral Constituents of Meteorites XII. The Breitenbach Meteorite: Prof. Story-Maskelyne, F.R.S.—On the Organisation of the Calamites of the Coal Measures: Prof. W. C. Williamson, F.R.S.—On Approach caused by Vibration (a Letter to Prof. Guthrie) Sir W. Thomson, F.R.S.

Royal Institution, at 3.—Davy's Discoveries: Dr. Odling.

Society of Antiquaries, at 8.30.—On Remains on the Site of Keynsham Abbey: Rev. H. M. Scarth, M.A.

*FRIDAY*, January 27.

Royal Institution, at 9.—Dr. Odling.

Quekett Microscopical Club, at 8.

*SATURDAY*, January 28.

Royal Institution, at 3.—Laws of Life revealed in History: Rev. W. H. Channing.

*SUNDAY*, January 29.

Sunday Lecture Society, at 3.30.—The Nature of the Earth's Interior: D. Forbes.

*MONDAY*, January 30.

Victoria Institute, at 8.—Archæology: Rev. J. Titcomb.

London Institution, at 4.—On the First Principles of Biology: Prof. Huxley (Educational Course).

*TUESDAY*, January 31.

Royal Institution, at 3.—Nutrition of Animals: Dr. Foster.

Anthropological Society, at 8.—On some of the Racial Aspects of Music: Joseph Kaines, F.A.S.L.

*WEDNESDAY*, February 1.

Society of Arts, at 8.—On the Preservation of Vegetables: O. Buchanan.

*THURSDAY*, February 2.

Royal Society, at 8.30.

Society of Antiquaries, at 8.30.

London Institution, at 7.30.—On the Action, Nature, and Detection of Poisons: F. S. Barff.

Linnean Society, at 8.—Natural History of Deep-Sea Soundings between Galle and Java: Capt. Chimmo, R.N.

Chemical Society, at 8.—On the Development of Fungi in Potable Water: Dr. Frankland.

Royal Institution, at 3.—Davy's Discoveries: Dr. Odling.

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