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THURSDAY, FEBRUARY 2, 1871

SPECTROSCOPIC OBSERVATIONS OF THE AMERICAN ECLIPSE PARTY IN SPAIN

THE complete and accurate account of the observations of the American Eclipse party in Spain, under the charge of Prof. Winlock, which was given by Prof. Langley in NATURE two weeks ago, renders any further report of our spectroscopic work almost unnecessary; and yet perhaps a somewhat fuller statement with reference to one or two points may not be wholly superfluous.

Of the four spectroscopes employed, two were what might be called *analysing*, and two *integrating* instruments. The analysing instruments are designed to study separately the spectrum of different portions of the prominences and corona, only a small part of the object being examined at a time; the integrating instruments, on the other hand, deal with the entire mass of light received from the whole luminous body, without distinction of parts. In the first class of instruments, a distinct image of the object is thrown upon the slit by the object-glass of a large telescope, each point in the slit receiving light from only one point in the object. In the second class no image is thrown upon the slit, every point of which receives light from every point in the object; and if a telescope is used at all (as was the case with Mr. Pye's instrument), it must have a field of view large enough to include the whole object, and must have its eye-piece adjusted for distinct vision of a star—*i.e.*, in such a manner that the rays from a star shall be parallel when they leave the eye-piece. In this case the telescope increases the angular diameter and area of the object, and consequently the amount of light received, without otherwise at all changing the conditions.

Prof. Winlock's instrument and my own were of the former class. Prof. Winlock had a spectroscope of two prisms attached to an equatorial of $5\frac{1}{2}$in. aperture, and about 7ft. focus. My own instrument had (during the totality) the dispersive power of seven prisms, and was attached to an equatorial of $6\frac{4}{10}$in. aperture, and $8\frac{1}{2}$ft. focal length.

The instruments of Messrs. Abbay and Pye were of the integrating kind. Mr. Abbay's had the collimator and observing telescope and two of the prisms which belonged to my old five-prism instrument. It was provided with the means of comparing the observed spectrum directly with the spectra of hydrogen, sodium, magnesium, and iron.

Mr. Pye's instrument was much smaller, but as its prism was made of the extra-dense flint, its dispersive power was very nearly the same as Mr. Abbay's; and the addition of a small telescope in front of the slit, magnifying about $2\frac{1}{2}$ times, and thus increasing its light about six-fold, made it, I think, fully the equal of the other in power and efficiency.

Professor Langley has so well stated what we saw, that it is not necessary to repeat it; but I cannot refrain from putting on record that the sudden reversal into brightness and colour of the countless dark lines of the spectrum at the commencement of totality, and their gradual dying out, was the most exquisitely beautiful phenomenon possible to conceive, and it seems to me to have considerable theoretical importance. Secchi's *continuous spectrum* at the sun's limb is probably the same thing modified by atmospheric glare; anywhere but in the clear sky of Italy so much modified indeed as to be wholly masked.

I wish at this time to call special attention to the evidence which we obtain as to the extent of the self-luminous corona, or "leucosphere," as it has been recently named,* by combining the indications of the two classes of instruments.

By my direction Mr. Pye recorded the brightness of the lines which he saw during totality on an arbitrary scale from 10 down. These are his numbers, C 8·5, D_3 5·5, 1474 10, F 3. I suppose the actual amount of light of each kind would be roughly proportional to the squares of these numbers, for we seem instinctively to call one luminous object twice or thrice as bright as another when it would give the same light at twice or thrice the distance.

If so, the numbers representing the relative amounts of light would stand C 72, D_3 30, 1474 100, and F 9, neglecting fractions.

Now, in the analysing spectroscope the case is very different, and it is difficult to make an accurate estimate; but I think those who have been accustomed to observe both C and 1474, would admit that their ratio of brightness is something the same as that between a first and fifth magnitude star; *i.e.*, C is at least 25 times and perhaps 50 times as bright as 1474. Even during the totality, 1474 can hardly be called conspicuous in an analysing instrument, while C blazes like a red Sirius. It seems necessary, therefore, to assume that the area which emits the 1474 light is to the area which gives C, roughly in the proportion of 100×25 (or 50) to 72—that is to say, the angular area of the self-luminous corona is from 35 to 70 times as great as that of the red stratum of hydrogen and prominences combined. I suppose these taken together would be about equivalent to a ring 15″ high surrounding the sun, and this would make the self-luminous corona equivalent to another ring from 8′ to 16′ high.

Of course I am aware that the numerical data of this calculation are very uncertain, and I have therefore neglected all considerations of shading and inequality of illumination. But the principle is, I think, correct, and it has this advantage. The presence of a light cloud or haze does not sensibly affect the result, because the calculation is based solely on the ratios between lights of two different kinds in the two different instruments, and these ratios would not be seriously affected unless the cloud absorbed one kind of light more than the other.

With the analysing spectroscope alone the case is entirely different; a light cloud or haze vitiates everything. Thus some of the observers, favoured with a less clear sky than we at Xeres, saw the C and F lines even on the moon, undoubtedly by reflection from thin clouds. I saw myself the C line as far as 6′ or 7′ from the sun, far above any possible hydrogen atmosphere.

Therefore, although Prof. Winlock and myself both saw the 1474 line to a distance of more than 16′ from the sun, I should not dare to lay much stress on that observation as showing the true limits of the self-luminous coronal matter. I base my belief that the limit of 15′ or 20′ is reached by it in

* This name seems inadmissible, except as one of the sub divisions of the chromosphere.—ED.

some of its angular prolongations, more upon the observations of 1869, when the sky was exceptionally clear, than upon anything seen at this time; and yet all the observations of this year, so far as I can see, accord well enough with the idea.

The two faint lines which I saw last year between D and 1474, and which I thought might also be corona lines, were not seen this year by any one, so far as I can learn. I certainly saw two such lines last year, but I was not then at all positive about their belonging to the corona (I have felt somewhat annoyed by finding them put on the same footing as 1474 in several publications of the last year or two), and my present impression is that they were two of the faint iron lines that often appear in protuberances in that portion of the spectrum.

The question has been raised whether the corona line *exactly* coincides with the 1474 dark line in the solar spectrum. The difference, if any (and I have not found the slightest reason to suspect the least want of coincidence in observations with the whole dispersive power of 13 prisms), is less than $\frac{1}{10}$ of one division of Kirchhoff's scale.

Just before the totality began, I placed the slit of my spectroscope exactly tangential to the sun's limb at the point which would be last covered, and brought the 1474 line, already bright, as is usually the case at the base of the chromosphere, exactly to the cross hairs. After the totality had fairly begun, I moved the equatorial in right ascension until the slit was more than 16′ east of the sun's limb, and the line remained continually visible, though of course growing fainter as the distance from the sun increased. There is not the slightest possibility of mistake, nor of error beyond the limit named, *i.e.*, $\frac{1}{10}$ of one division of Kirchhoff's scale.

And now a few words in relation to the nature of the Corona. It seems to me to be a complex phenomenon, made up of at least four, perhaps five different elements; and in the main I concur with the views put forth by Mr. Lockyer in a recent number of NATURE, with the exception that I should be disposed to assign a greater relative importance to the truly solar portion of the phenomenon than he appears to do.

1st. We have, I think, surrounding the sun, beyond any further reasonable doubt, a mass of self-luminous gaseous matter, whose spectrum is characterised by the green 1474 line. The precise extent of this it is hardly yet possible to consider as determined, but it must be many times the thickness of the red hydrogen portion of the chromosphere: perhaps, on an average, 8′ or 10′, with occasional horns of twice that height. It is not at all unlikely that it may even turn out to have *no upper limit*, but to extend from the sun indefinitely into space.

2nd. This region undoubtedly reflects to us a certain amount of the ordinary photospheric sunlight. This reflected light is of course polarised radially to a considerable extent. Its spectrum ought to show the ordinary dark lines, but they are partly masked in the manner Mr. Lockyer has so happily explained, and partly by the faintness of the spectrum.

3rd. Our own atmosphere, even when clearest, must apparently extend this corona, both outwards, and inwards upon the moon's disc. Since, however, the inner edge of the coronal ring is far the brightest, the inward extension of the corona should be most marked, except at the very beginning or end of totality, and I have no doubt it is: that is to say, at the middle of totality the illumination of the moon's disc gives a somewhat exaggerated measure of the effect of our own atmosphere in extending the corona outwards. Accordingly, I am disposed to think the effect of the atmosphere (when clear) is a very subordinate one, since in 1869 the light upon the moon's disc was only very trifling compared with that even a whole degree from the sun. This atmospheric light would also be polarised radially. Its spectrum would be mainly that of the chromosphere, prominences, and "leucosphere" combined, a discontinuous bright line spectrum.

4th. There must be a large subjective element, for two even skilled observers, standing side by side, describe phenomena differing in very essential points.

5th. I am somewhat inclined to think with Oudemans (see his paper published in NATURE Nov. 10) that possibly *cosmical dust* between us and the moon may play an important part. Assuming a light cloud of such matter, one or two hundred thousand miles above the earth's surface and of great thickness, it becomes easy to account for the straight dark streaks, the varying form (if it does vary), and many other puzzling phenomena of the corona-phenomena which can hardly be produced by portions of our own atmosphere deeply immersed in the lunar shadow, but which, I own, seem to me now less aurora-like and less certainly solar than they did a year ago. I do not see how optical tests by polariscope and spectroscope could discriminate between the effects of such a cloud and those of our own atmosphere. C. A. YOUNG

POPULAR NAMES OF BRITISH PLANTS

The Popular Names of British Plants. By R. C. A. Prior, M.D., F.L.S. Second Edition. (Williams and Norgate.)

THERE are many botanists who know little of the English names of Plants; and there are many who know these intimately, yet are not botanists. Both classes will welcome this comprehensive volume: and those who possess neither a philosophical nor a popular knowledge of the subject, will yet find abundant interest in a book, which is the work of an accurate scholar and philologist, as well as of a scientific botanist.

Most interesting, and perhaps least expected, is the light which these names throw upon the history of early civilisation. Many of them date from a period antecedent to the European settlement of the Aryan race, and enlighten us as to the habits of our remote ancestors some thousand years ago. We discover from them that the men who continuously advanced through many countries, from the confines of India to the British Islands, were no race of savages, but a comparatively civilised community: that they understood letters; that they had a knowledge of the useful metals; that they possessed the principal domestic animals; that they cultivated the oak, the beech, the birch, the hawthorn, the apple; grew wheat, barley, oats, rye, beans; built timber houses and thatched them; hedged their fields and fenced their gardens.

In a later class of names, which betray the intercourse of our forefathers with Roman cultivation and Grecian poetry, many a strange piece of myth or history lies embalmed. Thus the *Centaury* is the plant with which the Centaur Chiron healed the wound received from the poisoned arrow of Hercules. The beautiful and rare wild *Pæony* recalls Pæon, the physician-god of Homer, who healed the bellowing Ares when smarting from the spear of Diomed. The *Juno's Rose*, or tall white lily, preserves a story curiously transformed in later times from the Pagan to the Roman Catholic Mythology. The *Treacle-mustard* (Erysimum cheiranthoides) is the "Theriacum," invented as an antidote by the Emperor Nero's physician, and reappearing long afterwards as an ingredient in the Orvietan, or Venice Treacle. Mediæval tales and legends in abundance find illustration from the same source. The *Carline Thistle* is the herb which miraculously healed a pestilence that attacked the army of Karl the Great. The sea-loving *Samphire* is corrupted from Saint Pierre, the fisherman Apostle. The *Flower-de-Luce*, or Iris, was the device of Louis VII. The *Filbert* commemorates the horticultural skill of king Phillibert. The *Herb Robert* cures a disease named after Duke Robert of Normandy, The *Margarette*, or Daisy, owes its name to St. Margaret of Cortona, known to the readers of Mrs. Jameson.

More than one curious superstition is embalmed in well-known names. The *Celandine* is an altered form of the Greek "Chelidon," a swallow, because with its yellow juice the swallows were supposed to restore sight to their blinded young ones. The *Hawk-weed* records a like belief respecting the Hawk. The *Fumitory* (fume-terre) was thought to be produced without seed by vapours rising from the earth.

Many names, as *Wound-wort*, *Tetter-wort*, *Pile-wort* *Nipple-wort*, have survived the ancient faith in their medical efficacy. And here comes in the strange principle of nomenclature, known as the "Doctrine of Signatures." Where the external appearance of a plant resembled any disease, or any part of the human body liable to a disease, such resemblance was taken as an indication of its especial healing virtue. The spotted leaves of the *Lung-wort* must be a specific, it was thought, for tubercular disease of the lungs; the scaly pappus of *Scabious* for cutaneous eruptions; the hard stony seeds of the *Gromwell* for stone in the bladder; the throat-like corolla of the *Throat-wort*, or Canterbury bell, for sore throats; the knotty tubers of *Scrophularia* for scrophulous glands The pretty *Toad-flax* of our walls and hedges owes its name to a strange mistake. Believed in early times to be a cure for a complaint known as "Buboes," it received the Latin name of "Bubonium." A confusion between Bubo and "Bufo," Latin for a Toad, gave birth to its present name; and stories were not long wanting that sick or wounded Toads had been seen to eat of it and to recover health.

Similar distortions occur in names not medically expressive. *Apricot*, connected ordinarily with the adjective "Apricus," sunny, is a corruption through the Spanish and Italian of "Præcoqua," early; having been looked upon as an early peach. *Sweet William* is transformed from "Æillet," a little eye; *Pink* is shortened from the Low German "Pinksten," Whitsuntide, a name due to the season of its blossoming. *Gilliflower* comes through *Giroflée* from "Caryophyllus," a clove. *Carnation* is from "Coronation," its flowers being used in chaplets. *Marigold* is Marsh-gold; *Cowslip* is "Hose-flap," applied originally to the large flannelly leaf of the Mullein; *Dame's Violet* from "Damascene" Violet; the blunder being kept up, as is often the case, in the Latin "Hesperis matronalis." *Hip* is the same word with *Jujube*; *Hawthorn* is "Hedge-thorn;" *London Pride* is named not from the smoky metropolis in which it thrives, but from a Mr. London, who introduced it. *Snap-dragon* is "Snout-dragon." *Daffodil*, more properly *Daffadowndilly*, is a combination of "Sapharoun" or "Saffron-lily" with "Asphodelus," the old English "Affodilly." With the taste for alliteration often shown in popular names, the Sapharoun-lily, blending with Affodilly, became by a mutual compromise Daffadowndilly, whence Daffodilly and Daffodil. *Peach* (Persicus), *Damson* (Damascenus), *Shalot* (Ascalonicus), and *Spinach* (Hispanicus), retain in their names dim memories of the lands of their birth. But the most curious instance of blundering is the *Jerusalem Artichoke*. It is a sun-flower, not an artichoke; but its tubers resemble the artichoke in flavour. From its Italian name "Girasole," turn-to-the-sun, came "Jerusalem;" and by a further quibble the soup which is made from it is called "Palestine soup."

Of the miscellaneous names many are equally interesting. The *Laburnum*, closing its petals at night like a tired labourer; the *Campion*, which crowned the champion of the tournament; the *Lady's Bedstraw*, recalling the days when mattresses were not, and when this fragrant Galium was used for ladies' couches; the *Heartsease*, with its many amorous synonyms; the *Shamrock*, concerning whose botanical identity no two Irishmen can agree; the *Lavender*, or Washerwoman, scenting freshly washed linen; the *Ozier*, from the oozy beds in which it grows, are a few out of countless specimens. The philologist may delight to trace the root of *Apple* from the Zend and Sanscrit "Ap" to the Latin "Pa" in "Padus," "Po" in "Pomum" and "Poto." Those familiar with Hans Andersen will read with fresh enjoyment the tale of the "Wild Swan," when they learn that the *Nettles*, which the Princess had to weave into shirts, are derived from the verb "ne," to spin or sew. On the other hand, some pretty time-honoured traditions are ruthlessly swept away. The *Narcissus* is referred, not to the enamoured Grecian youth, but to the Sanscrit "Nark" or Hell; and the protesting lover of the classics is reminded "that Proserpine was gathering Narcissi long before Narcissus was born." The *Fox-glove*, is not, as we had loved to think, like the *Troll-flower* and *Pixie-stool*, the "Folks'" or Fairies' "Glove"; but the Foxes' *Glew*, or Tintinnabulum, with its ring of bells, hung on an arched support. Worst of all, the charming story of the Forget-me-not, current in every European language, is a later legend framed to meet a name already extant. For the original use of the name, its first curious transference of allusion, its final attachment to a river-side flower, and the narrative of the drowning lover, which readily grew up to adorn it, we must refer our readers to Dr. Prior's book. They will be surprised to find a treatise on Botany poetical, and a Dictionary light reading. They will learn how much a technical subject may be enlivened by varied accomplishments in him who treats it; they will see that in the Science of Botany, as in every other Science, the widest culture brings about the most telling and effective teaching.

W. TUCKWELL

OUR BOOK SHELF

A Ride through the disturbed Districts of New Zealand, together with some Account of the South Sea Islands. Being Selections from the Journals and Letters of Lieut. the Hon Herbert Meade, R N., edited by his Brother. With Maps and Illustrations from the Author's Sketches. (Murray, 1870.)

THE title-page sufficiently describes this book, which is illustrated by some nice woodcuts, and several coloured lithographs of less merit. There is a good description of the Geysers of New Zealand, and of the state of the native insurrection in 1865; with some exciting narratives of attacks on Papuan cannibals in the New Hebrides. A good-sized house, built in a lofty tree and used as a fort, was seen in one of the Solomon Islands. One cannot but regret that the opportunities possessed by our officers on the Pacific Station for investigating the little-known natural history of the islands, are so seldom utilised. The author of this book often shoots, but hardly seems aware that his game could be of any other use than for food. The only natural history passage in the book is the following, dated Upolu, Samoan Islands :—" Saw a very rare bird, the Dodunculus, native name, which is peculiar to this island. It has the feet of a pigeon, beak of a hawk," &c., &c. Dodunculus! native name!! A. R. W.

Metals, their Properties and Treatment By C. L. Bloxam, Professor of Chemistry in King's College, London; Professor of Chemistry in the Royal Military Academy, and in the Department of Artillery Studies, Woolwich. Pp. 296. (London: Longmans, Green and Co., 1870.)

THIS is one of the text-books of science which are being edited by Mr. T. M. Goodeve and published by Messrs. Longmans. The series is intended to supply a want that has long been felt of exact and complete works on mechanical and physical science for the use of schools, and for the self-instruction of working men. A difficulty must have been experienced by many who are engaged in teaching science, when asked to recommend a small and inexpensive text-book, which may at the same time be so simply and clearly written as to be useful to those who have not had a scientific education, and who have not the advantage of being able to attend long courses of lectures. Many popular books on scientific subjects have been written, but they are not unfrequently somewhat inaccurate; difficult questions being often omitted, or, what is worse, treated in a superficial manner which is likely to mislead the student, inducing him to believe that these questions are very simple, and deluding him with the notion that he knows all about them. He is thus frequently disappointed at a subsequent period by finding that on studying the subject more minutely, it is much more complex than he at first imagined, and that many of the simple ideas which he had carefully fixed in his mind have to be discarded, and new ones acquired.

The book opens with an introductory section on the properties and treatment of metals, containing many useful tables, such as specific gravities, fusing points, conductivity, &c. The more common metals used in the arts are alone discussed, so as not to introduce unnecessary complication. The remaining sections of the book treat of iron and steel, copper, tin, zinc, lead, silver, gold, mercury, platinum, palladium, antimony, bismuth, aluminium, magnesium, and cadmium. The last six being far less important than the others, are very shortly described, and only occupy twelve pages.

Each section commences with a description of the ores of the metal under consideration, their composition being given, and also the per-centage of metal present. This is followed by the methods of treating the ores in order to extract the metal, chemical reactions being written in words without formulæ, so hat no preliminary knowledge is necessary. The mechanical treatment of the reduced metal is then detailed, and its useful applications in the pure condition or in the form of alloys. The book is profusely illustrated with good woodcuts, and is written in an extremely interesting manner which cannot fail to attract the attention of the student. This, together with the trustworthiness of its contents and its low price, will render the treatise extremely useful for scientific instruction. If the remaining text-books of the series possess all the advantages which are presented by this one, the thanks of teachers and students of science will be due to the editor and publishers for their undertaking.

Odd Showers: or, Explanations of the Rain of Insects, Fishes, and Lizards; Soot, Sand, and Ashes; Red Rain and Snow; Meteoric Stones; and other Bodies. By Carribber. (London: Kerby and Son, 1870.)

THIS little book is stated on the title-page to be " intended chiefly for young persons;" but others will, doubtless, gain information from it, as to the causes of the sudden appearance of swarms of insects and other animals, and showers of rain tinged with various colours, with respect to which so many popular errors are afloat. The writer derives his experience from a long residence in Canada, and one explanation of so-called "showers of blood" is new to us, that it is caused by the exudation of a crimson fluid by various chrysalides when passing into the imago state. The writer states that, on one occasion, twenty-eight chrysalides of *Vanessa antiopa*, the Camberwell Beauty, which he had preserved in a small room, underwent transformation in a single day in July; when the walls and floor were bespattered with a bright crimson-coloured substance resembling blood, as to give the appearance of a regular shower of the fluid.

LETTERS TO THE EDITOR

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

Natural Science at Cambridge

IN NATURE for January 12, 1871 (p. 209), there appeared an article headed "Natural Science at Cambridge," which has the air of having been promulgated *permissu* (if not *auctoritate*) *superiorum*. It is extremely gratifying to read the list of exhibitions and scholarships founded or proposed to be founded in certain Colleges in this University, but the concluding sentence of the article has struck me as having been penned by one with whom the wish was father to the thought. It is said that "most of the Colleges are understood to be willing to award Fellowships for merit in Natural Science equivalent to that for which they are in the habit of giving them for Classics and Mathematics." Now this is entirely at variance with my own opinion on the subject, formed on a somewhat wide acquaintance with members of various colleges; and I would beg of the writer to be good enough to inform the public through your columns, first, how many Fellowships have solely and actually been awarded for merit in Natural Science, and, secondly, which of the sixteen colleges, besides Trinity, have absolutely declared that a Fellowship shall be the reward of great proficiency in Natural Science. I need not say how glad I shall be to find that my opinion is erroneous. M. A.

Cambridge, Jan. 28

Yellow

IT was not from any experiments of my own, but on the authority of Helmholtz, that I asserted the identity of brown with a dark yellow or orange. He found that the pure red and yellow of the spectrum gave the various shades of brown when seen by the side of more brilliantly lighted white surfaces. (Physiologische Optik, p. 281.) There is therefore nothing in the nature of the colour to exclude complete saturation, although it may well happen that most of the browns we ordinarily see fall somewhat short of it.

In NATURE of Jan. 26, Mr. Munro calls attention to the great brilliancy and saturation of many natural yellows as accounting for the difficulty of resolving them into their components. It is, no doubt, quite true that a full yellow could not be compounded of such reds and greens as we come across in daily life, but it is equally certain that a drab or dilute yellow could be; and yet no one recognises the fact by his unaided senses, or thinks it anything but strange and unlikely when told of it. And after all, can it properly be said that natural yellows are more saturated than other colours? That they approach more nearly the corresponding tints in the spectrum is admitted; but is that test a fair one? It seems to me that the homogeneous yellow itself must be considered as dilute when brought into comparison with the nearly primary red and green.

I have another difficulty in accepting Mr. Munro's explanation. A suitable mixture of any red, green, or blue will give a neutral grey. All four come within our every-day experience; but such a result seemed to Goethe, soon after Newton proved it, a paradox of paradoxes, and I believe to unsophisticated minds it seems so still.

Mr. Munro has ingeniously shown from the colour equations that there is no more primary blue in my blue disc than about 2½ as much as in the red *plus* 1⅓ as much as in the green—a conclusion which seems somewhat startling. In choosing the coloured papers and cards for the discs, I had great difficulty in finding a green that was even tolerably good, and the one that I finally used reflected large quantities of blue light. I had some thought of trying a green silk disc, whieh was of a much better colour, but feared errors depending on the different character of the surface.

It is not hard to see a reason for the comparative scarcity of good greens. To obtain a good red orange or yellow by means of absorption, all that is necessary is to cut away the spectrum above a certain point; for a good blue, the rays standing below a given one in refrangibility must be got rid of; but in order to isolate a green in anything like purity, the absorbing agent must hit off *two* points of the spectrum, removing all below one point and all above the other. The result is, that while nearly saturated yellows and reds abound—the scarlet of the geranium is almost perfect—hardly a good green is to be met with. The best I know is a mixture, prepared by adding bichromate of potash to a strong solution of sulphate of copper. The addition of a little chloride of chromium to remove the yellow more effectually is perhaps an improvement. If Mr. Munro would care to see the colours which I used for the discs, I should be very happy to send him samples.

Terling Place, Witham, Jan. 29 J. W. STRUTT

Comets' Tails

MAY there not be a connection between the colour of the sky and the tails of comets?

Suppose a comet to be surrounded with a wide-spread transparent atmosphere, holding in suspension matter as finely divided and as invisible as the sky-matter of our earth; and suppose the more condensed, but still transparent nucleus to act as a lens, throwing a beam of light upon the otherwise invisible mist of that atmosphere, could not most of the phenomena of those puzzling tails be thus accounted for?

Leicester, Jan. 28 FREDERICK T. MOTT

Ocean Currents

MR. LAUGHTON'S letter in the latest number of NATURE shows that the suggestion of a probable influence of differences of atmospheric pressure on ocean currents has not been stated with sufficient clearness.

That a hydrostatic equilibrium would exist under a permanently unequal distribution of the pressures over the ocean, as Mr. Laughton argues, is not to be doubted. But is there any such permanent arrangement?

Granting that the high-pressure area of the trade-winds is nearly constant, varying little from day to day, and only expanding and contracting its limits gradually in summer and winter, the pressures to northward of this over the Atlantic are by no means so steady. Though they give on the average of the year, or even of the month, a *lower* pressure, yet the daily observations show that the pressures over wide areas may vary considerably, rising at one time to equal that of the Trade-wind patch, at another falling very much below it. Quoting from the daily barometric curves registered in the Quarterly Report of the Meteorological Office (for April—June, 1869), "On the 13th of May in that year, the seven stations whose weather is there recorded (Valencia, Armagh, Glasgow, Aberdeen, Falmouth, Stonyhurst, and Kew) show simultaneously a barometric pressure of 30·1 inches." On the 6th of May these stations had severally reported a pressure of only 28·9 inches. A difference of pressure equal to that of 16 inches of water had taken place over the entire extent of the British Isles, and probably over a much greater space.

The system of synchronous observation is not yet sufficiently extended to enable any positive statement of the general direction of the movement of low pressure areas across the North Atlantic to be made; but there is a strong probability that such depressions travel in a direction continuing the path taken by the hurricanes of the West Indies, since the majority of barometric hollows reach the British Isles from west or south-west. Just as the waters are forced to rise into the central low pressure of a hurricane (to the extent of several feet) and to follow its path, so, in a less degree, does it seem probable that the movement of extended and less violent depressions may influence the ocean currents.

The rate of progress of such depressions appears to be in an inverse ratio to their extent and depth. A West Indian hurricane moves onward at a rate of only fifteen miles an hour (Buchan's Meteorology, p. 269), and the highest speed of European storms, according to the same authority, is forty-five miles an hour; but that minor depressions may travel with much greater velocity is shown by reference to the quarterly report before noticed, where is the record of a hollow of from three to four tenths of an inch, which passed over the British Isles on the 24th of February, 1869, at the rate of ninety miles an hour.*

Either in causing a considerable change in the level, or in rapidly moving over it, from near an area of constantly high pressure, through a region where the average pressure diminishes, such depressions must surely influence the surface of the ocean, and either aid or retard its currents. That a difference in level of four inches in 1,800 miles can scarcely under any circumstances give rise to a current of twenty miles a day (the word hour in Mr. Laughton's letter is probably a typographical error) is also clear, but the temporary difference in level may be much greater than this within a much shorter distance. The average rate of the north equatorial current, moreover, in the Trade-wind region, is shown by the pilot charts of the Admiralty to be only from ten to twelve miles a day, and it is only claimed for difference of atmospheric pressure that it has some small share in aiding the formation of the current in question.

KEITH JOHNSTON, JUN.

Insulation of St. Michael's Mount, Cornwall—When did it occur?

THIS is a very interesting question, and the reader will be enabled to judge for himself, presently, whether there are not sufficient historical facts on record to enable us to answer—"In the eleventh century."

Domesday Book, date 1086, in the part relating to "Cornvalge" (*i.e.* Cornwall), at p. 2, has the following, which I have translated from the abbreviated Latin—"*The Land of St. Michael.*—Keiwal holds the church of St. Michael. Brismar was holding it in the reign of King Edward. There are two hides which never paid the Danish tax [nunquam geldaverunt]. The land is 8 carucates. There is 1 carucate with 1 villan, and two bordarii, and 10 acres of pasture. Value 20 shillings. Of these 2 hides, Earl Moriton took away 1 hide, value 20 shillings." And accordingly, at p. 11 of Domesday Book, there appear in the descriptive list of the many estates of Earl Moriton, corresponding particulars of the 1 hide which he had taken away.

Now, in the first place, Domesday Book gives no reason whatever for believing that, at its date, St. Michael's Mount was an *Island*, neither does Magna Britannia, vol. 1, p. 309, where the Mount is called *Mychel-stop*, or Michael's place. And in every case, while "annoting" those holding possessions in "Cornvalge," there is an entire absence in Domesday Book of any mention of island or islands on any of the coasts of Cornwall, just as if there had been then no islands on those coasts of sufficient extent to be worthy of mention. On the other hand, it is the

* The Meteorological Report shows that in January, 1869, 12 depressions reached the British Isles from W.-ward; in February 10, also from W.; in March 8 (5 from W.); in April 7 (5 from W.); in May 8 (7 from W.); and in June 2 from W.

custom in Domesday Book *when any place is an island, to call it so.* For example, in vol. i., folio 75, "Dorsete" [Dorset], we have—"The land of the King. The King holds the island which is called Porland [*i.e.* Portland]. King Edward held it in his life." And again, same vol., fol. 396—"Hanteschire [*i.e.* Hampshire]. These lands below written lie in the isle of Wit" [*i.e.* Wight].

In short, the Mount could not have been an island in 1086, because it then contained at least eight times as much land as it does at present, probably connecting it with the mainland, from which it is even now only one-third mile distant. The truth, Sir Henry Ellis says, seems to be that a hide, a yardland, a knight's fee, &c., contained no certain number of acres, but varied in different places at different times. He says there are "four virgates in each hide, and thirty acres to make a virgate." The elementary acre was 40 perches by 4 perches, as now. And, accordingly, the 8 carucates would amount to the respective numbers of acres mentioned in the last column of the following table at the respective times:—

Periods	Acres in a Carucate	No. of Carucates	Total Acres
Carucate temp. Richard I.	60	8	480
do. do.	100	8	800
do. Edward I.	180	8	1440
do. 32 Edward III. (Oxon)	112	8	896
do. Middleton	150	8	1200

The hide is generally supposed to have been 120 acres. Now, taking the smallest of these measures, we shall have for the two hides 240 acres. The present area of the Mount, however, is only 30 acres, so that there are 210 acres missing. How can we account for them except by supposing that the Mount extended further, perhaps in every direction? The hide of land taken by Earl Moriton was given to Mont S. Michel,* on the French coast where Normandy joins Britanny, in 1085.

I have before me "Two of the Saxon Chronicles parallel," edited and commented on by the Rev. John Earle, formerly Professor of Anglo-Saxon, Oxford, from which the two following remarkable passages are extracted for the respective years 1014 and 1099.

"1,014. And on this year, on Saint Michael's-Mass-Even, came that great sea-flood through widely this land, and ran so far up as never before not did, and submerged many towns, and mankind innumerable number," p. 151.

"1,099. This year also, on St. Martin-Mass-Day, sprang up that exceeding sea-flood, and so much to harm did, as no man not remembered that it ever afore did, and was that same day a new moon," p. 235.

Nobody seems to contend that the coasts of Cornwall have simply been abraded and cut back by the action of the sea during the Christian period. Nor does anyone appear to doubt that there has been a *subsidence.* Now there are 210 acres of land missing since 1086, and in 1099, thirteen years *after*, we have a record of a catastrophe which would fully account for the loss! Supposing, whilst travelling in a foreign country, we were to come suddenly upon a city in ruins, when we had personally experienced shocks of earthquake a short time before. How could we doubt that the earthquake was the cause of the ruins, and how can we doubt that the remarkable event of 1099 caused the loss of land by subsidence and the Insulation of the Mount?

Sir Henry de la Beche (Report on Cornwall, &c., p. 417, *et seq.*) says "submarine forests are so common that it is difficult not to find traces of them in the district at the mouths of all the numerous valleys which open upon the sea, and are in any manner silted up." . . . . It is well known that abundance of roots and stumps of trees have been found *in situ* for some distance on the south of the Mount. And no one seems to doubt that "the Mount was formerly five or six miles from the sea, and enclosed with a very thick wood," and that it was called in the Cornish language "the hoar rock in the wood."

I have before me the very interesting Prize Essay by Mr. John E. Thomas, F.G.S., written for the National Eisteddfod, held at Chester, 1866, upon the "Encroachment of the Sea between the River Mersey and the Bristol Channel."† Prof. Ramsay, who had the adjudication of the prize, says, "The result is so good, that I think this part of the Essay well worthy of the prize of 10*l.* and a medal." Mr. Thomas does not look further back than the year 520 for his sinkings in Cardigan Bay (p. 13). A perusal of the whole tract of twenty-four pages will well repay gentlemen who take an interest in this particular department of geology.

Jersey, Jan. 23 R. A. PEACOCK

* Penny Cyclop. Art. CORNWALL.

† Published by Messrs. Spon, Charing Cross, price 1*s.*

Measurement of Mass

PROF. EVERETT'S first letter contains the statement "Deschanel, in accordance with what has been till recent years an almost universal custom, employs a variable unit of force, and as depending upon this, a variable unit of mass, so that the number denoting the mass of one and the same body is diminished as the body is carried from the equator to the poles, and would increase up to infinity if the body fell to the centre of the earth."

I wished to point out that in making the standard pound a unit of force, by defining the place where it is to be used, we do not adopt a unit of mass which is variable, since if we take three times as much matter as gravitates with the unit force, we shall obtain the same mass at whatever point of the earth's surface the comparison is made. I cannot see that the adoption of this method necessitates any filing or loading of weights to suit change of latitude, since we invariably employ an ordinary balance and not a spring balance to effect our weighings.

Whenever it does become necessary to compare gravitating forces, we are obliged to fall back upon the use of the pendulum, whether we adopt the old system of standards or the new.

As a philosophical theory I am perfectly ready to admit that the standard pound is most appropriately considered as a standard of mass, but the employment of this standard in a text-book for the use of beginners seems calculated to lead to confusion.

If we refuse to commit ourselves to the absurdity of comparing the quantity of one kind of matter with the quantity of another kind of matter, I hardly see how *mass* is to be defined except by means of weight, and without, for the moment at least, employing weights as measures of force.

The assumption of a hypothetical force of gravity not dependent on latitude seems to stand on the same footing as the employment of a *mean solar day*; it is convenient, leads to no confusion, and is not unphilosophical. W. M. W.

Mount Etna

IT may interest the observers who have lately been in Sicily to hear that since their departure there has been a sad falling off in the appearance of Etna. The grand wreath of steam that used to roll out of the crater at such stately leisure that you could hardly detect any movement without close attention, suddenly ceased about three days ago, and left nothing more than a tiny wisp of smoke, rather suggestive of a cottage chimney than a volcano. I call it smoke because the colour became decidedly darker than it used to be, and the manner of its dissipation is different. Formerly, after issuing from the crater it used to assume true cloud forms, and lie about the mountain exactly like clouds: now, it diffuses itself as a thin veil over the sky; sometimes being traceable in a streak as far as the coast of Calabria. Its volume is perhaps a thousandth part of what it was last week. It issues in a distinctly spiral form, the wreath oscillating apparently from side to side of the crater; and sometimes there are little puffs of extra size, whilst at others the wreath is nearly sundered.

The date of the change cannot be very precisely given, although I have watched the mountain at all hours of the day for a week past, in hopes of getting a correct outline of it for pictorial purposes. The clouds only cleared off completely yesterday; but I observed that a change in the wreath had taken place as early as Friday the 13th January. The weather up to the 15th has been outrageously squally and rainy, but is now superb. Last night there was a magnificent display of zodiacal light, considerably brighter than we saw it at Augusta before the Eclipse, and distinctly traceable up to the zenith; the apex of the cone reaching to within about 10° of the Pleiades. It was brightest about 7 P.M., but was still visible at 10 P.M., when clouds shut it out.

Taormina, Sicily, Jan. 17 JOHN BRETT

Note on Chromosphere Lines

A VERY small but very bright prominence on the N.W. limb of the sun, observed at Xeres Dec. 21st, gave in addition to the ordinary protuberance bright lines the following—one below C at 656 Kirchhoff's scale precisely corresponding to an iron line in the solar spectrum—also 3 at 1601, 1605, and 1607, the reversal of a well-known group of *Chromium* lines. The latter I believe are new in prominence spectra.

C. A. YOUNG

Eozoön Canadense

I HAVE just observed that in your number for December 22 a correspondent revives some of the old but often refuted objections to the organic nature of Eozoön. As the mail closes in a few hours, and I have lectures in those hours, I cannot reply by this opportunity; but shall crave a small share of your space next week to show that the objections stated are unfounded; and to state what is now being done here in further illustration of this ancient and veritable fossil. J. W. DAWSON

M'Gill College, Montreal, Jan. 16

THE battle for the *Eozoön Canadense* may be left to Messrs. Carpenter, Jones, King, Rowney, and other eminent microscopists, but perhaps an outsider may be pardoned if he asks some anomalies to be explained.

In the Ophytes of Bennabeola the mountain group in Connemara, or rather Yar-Connaught, Mr. Sandford proved the existence of the *Eozoön Canadense*, and his opinion was backed up by Mr. R. Jones and also, if I remember rightly, by Dr. Carpenter. There are acres upon acres of limestone in that country of the same age, and some of them on the same geological horizon as the Ophytes, Ophicalcytes, Ophimagnesytes and Ophidolomytes; yet, in no plaec, except where Ophyte or one of its varieties exist, has the Eozoönal structure been found. Furthermore, when the West Galway Ophytes are followed in depth they graduate into a Schistose-dolomyte that may be micaceous, felsitic, or quartzitic, and contains more or less calcyte; yet in these dolomytes there is no trace of the Eozoonal structure.

These rocks of Yar-Connaught are said to be of Lower Silurian (Cambro-Silurian) age, by Sir R. I. Murchison, Prof. Harkness, and other eminent geologists. In other parts of the world will be found square miles upon square miles of rocks, of the same geological age, often having inliers of limestones, yet in them there is no *Eozoön Canadense*, it only being found in a peculiar rock (pseudomorph dolomyte) in this small tract of Lower Silurian rocks, in Yar-Connaught.

Yar-Connaught, Jan. 23 G. H. KINAHAN

IF my previous letter, as alleged by Dr. Carpenter, exhibits a complete misapprehension of the state of our knowledge of the above fossil, I cannot plead in extenuation a want of familiarity with the arguments he again brings forward in support of the organic theory. Had he, instead of explaining away imaginary difficulties, addressed himself to those that really exist, his reply would have possessed greater value. Let us examine how my objections have been met.

Firstly, then, Dr. Carpenter cannot affirm that any specimen of Eozoön has been obtained from unaltered rocks. He can go no further than to say that his best specimens are from rocks that have undergone the *least* metamorphic change. Thus it appears after all, that it is only a question of *degree* in metamorphism; and when we consider that Logan, Dawson, Sterry Hunt, and himself, in their original papers, constantly alluded to these Eozoönal rocks as crystalline, highly crystalline, of serpentine marble, &c., we are enabled to judge of the value of the diminutives "little" and "least," now used when it becomes necessary to the argument to soften down these expressions. Sir W. Logan, who is an authority on the subject, says: *—"Any organic remains which may have been entombed in these limestones would, if they retained their calcareous character, be almost certainly obliterated by crystallisation, and it would only be by the replacement of the original carbonate of lime by a different mineral substance, or by an infiltration of such a substance into all the pores and spaces in and about the fossil, that its form would be preserved." It would be strange indeed if, during the millions of years since the deposition of the Laurentian limestones, they had undergone no change, and notwithstanding Sterry Hunt's depositional views, the consensus of opinion is in favour of serpentine itself being a product of alteration.

* "Geological Journal," No. 81, p. 48.

Had Dr. Carpenter pointed out where serpentine pyroxene or loganite had been found in unaltered rocks, instead of dwelling upon the internal casts of foraminifera distinguishing the Greensand formation, his remarks would have been more relevant to the subject. These casts, it is well known, are in glauconite, a hydrous silicate of protoxide of iron and potash. Whether or not the silicates replacing the sarcode bodies of the foraminifera dredged up by Capt. Spratt in the Ægean, are the result of precipitation from sea water, caused by the decomposition of the sarcodic substance, is quite immaterial to the argument; but if, as is assumed, the chambers of Eozoön were filled in the same manner with serpentine, and this chemical reaction was necessary to its precipitation, how are we to account for the serpentine investing huge blocks of pyroxene, and the solid bands of the same mineral intercalated in the limestone? If, therefore, I admit the possible infiltration of certain silicates into the *body* of Eozoön—did such an animal ever exist—it is no help to those who favour the organic hypothesis. I have, however, neither affirmed nor denied such a possibility, as it is entirely outside of my line of argument.

As regards *hydrothermal* action, which it appears is objected to if called in to aid my theories, I may say it is a matter of indifference what the *agency* be so long as the *alteration* is proved.

It would take up too much of your space for me to go into the details of the "canal system," "nummuline layer," "chamber casts," "Stolon passages," "pseudopodial tubules;" and such is unnecessary, as Profs. King and Rowney have pretty well exhausted the subject, and, to my mind, have conclusively proved the existence of identical forms of purely mineral origin. If, as is alleged, the canal system always crosses the cleavage planes, and is never between them, such would appear to be correlative mineral phenomena, and tells against the organic hypothesis. I object, however, to a question of such wide bearing being settled solely on the authority of Dr. Carpenter as a microscopist. If others are wrong, let him demonstrate the fact, which his great experience will more readily enable him to accomplish.

If I have misconstrued the following passage into an admission which he now repudiates, I am ready to make ample apology; perhaps, however, he will explain to what the term "elsewhere" refers. After combating the notion that the *nummuline* layer can be precisely parallel in a purely mineral production, he says he is "prepared to maintain the organic origin of Eozoön on the broad basis of cumulative evidence afforded by the combination in every single mass of an assemblage of features which can only be *separately* paralleled elsewhere."*

Such is Dr. Carpenter's unbounded faith in Eozoön—though every hypothesis attempting to bring it into the category of organic beings is beset with difficulties—that he would not be surprised to find it existing now in the deep-sea bottom. There is, he says, no *a priori* improbability in such an event happening, and indeed there is not, for the persistence of types is one of the most remarkable of zoological facts. But as the area in which Eozoön is to be found is enlarged, and the duration of its time lengthened, our difficulties increase. If the infilling material of the chamber casts is due to substitution during decomposition, or to direct deposition as suggested by Sterry Hunt, there is no possible reason why we should not find Eozoön in some of the immense masses of unaltered limestone which still exist. I repeat that it has never yet been found in such rocks, but always in those that have been metamorphosed. If again serpentine is not a product of alteration, why do we not find it in unaltered rocks? The inference is obvious, they are correlative phenomena, and therefore Dr. Carpenter must pardon me if I decline at present to adopt his views. Still I am open to conviction, and will freely admit my error when, after some of his deep-sea dredgings, he brings home the modern Eozoön fossilised with a silicate, and when, in addition, it is discovered in an unaltered limestone fossilised with serpentine pyroxene or loganite.

T. MELLARD READE

Blundellsands, Liverpool, Jan. 9

* Geological Journal, vol. xxii., p. 22

The Eclipse Expedition

HOW about the Eclipse Expedition, which, I presume, you helped to sanction? I informed the public that it would prove a complete swindle, and so it has turned out. As long as such professional liars as the Astronomical Society are allowed to gull the nation, what chance is there of arriving at the truth?

JOHN HAMPDEN

Scientific Nomenclature

THE Kakapo or Night Kaka of New Zealand, *Strigops habroptilus*, described in NATURE at p. 190 as the *ground* parrot, is called the owl-parrot by Mr. Wood in the current number of the "Student." So long as both names are given there is no confusion, but it is otherwise if a full description is omitted; and I have to suggest that it is very desirable to adopt a uniform usage upon all occasions.

It is curious to notice the analogy between the words *psittakos* of ancient Greece and the *Kaka* of aboriginal New Zealand; as the Greek word has been traced to a Sanscrit origin it would seem that the New Zealand word must have originated since the Aryan descent upon India. A. H.

NATURAL SCIENCE AT CAMBRIDGE

THE following Lectures in Natural Sciences will be delivered at Trinity, St. John's and Sidney Sussex Colleges during the Lent Term, 1871. On Electricity (for the Natural Sciences Tripos,) by Mr. Trotter, Trinity College, in lecture room No. 11, on Tuesdays, Thursdays, Saturdays, at 10, commencing February 4. On Electricity and Magnetism (for the special examination for the B.A. degree), by Mr. Trotter, Trinity College, in lecture room No. 11, on Mondays, Wednesdays, Fridays, at 10, commencing Wednesday, February 1. On Chemistry, by Mr. Main, St. John's College, on Tuesdays, Thursdays, Saturdays, at 12, in St. John's College, Laboratory, commencing Tuesday, January 31. Instruction in Practical Chemistry will also be given. On Geology, by Mr. Bonney, St. John's College: (1) Palæontology, on Wednesdays and Fridays, at 9, commencing Wednesday, February 1; (2) Lyell's Principles of Geology, on Tuesdays and Thursdays, at 9, commencing Tuesday, January 31; (3) Elementary Lectures on Tuesdays and Thursdays at 11, commencing Tuesday, January 31. On Structural and Morphological Botany, by Mr. Hicks. Sidney College, in the College Laboratory, on Mondays, Wednesdays, Fridays, at 10, commencing Wednesday, February 1. On Physiology, by the Trinity Prælector of Physiology (Dr. M. Foster), at the New Museums, on Wednesdays, Thursdays, Fridays, at 11, beginning Wednesday, February 1. The Physiological Laboratory will be open for pratical instruction in Physiology daily.

It may be remembered that a year ago we pointed out some defects in the prospectus issued for the intercollegiate teaching of Natural Science by Trinity and St. John's Colleges, Cambridge. We are glad to find that, as will be found from the above statement, these have been rectified, and that by the appointment by Trinity College of Dr. Michael Foster as Prælector of Physiology, and by combining with Sidney College, and so availing themselves of the services of Mr. Hicks of that College, who obtained the first place in the Natural Sciences Tripos, as lecturer on Structural and Morphological Botany, the staff has been greatly strengthened, and the prospect of thorough teaching proportionately increased. The lectures are open to members of the other colleges upon payment of a small fee.

*OCEANIC VERTEBRATES**

SO far as concerns their distribution, animals may be divided into two classes, the tenants of the land and fresh waters, and the inhabitants of the ocean. In the one case their boundaries depend upon the form and extent of continents past and present; on the other, upon the corresponding limits of the ocean.

Little enough is as yet known with certainty about the general distribution of terrestrial animals; about those of the ocean we are still more ignorant. It is, therefore, with great pleasure that we have received Prof. Giglioli's notes on the vertebrated animals which were met with during the voyage of the Italian frigate *Magenta* round the world. The scientific command of this expedition was originally entrusted to Prof. Philippi of Turin. Upon his lamented death at Hong Kong, the author of the present treatise, we believe, succeeded to the post, and is now busily engaged in working out the results obtained by the expedition in every branch of natural history. The present memoir, although founded on observations made during the voyage of the *Magenta*, seems to be only incidentally connected therewith, and to have been prepared with reference to a competition for the Chair of Zoology and Comparative Anatomy at the Royal Institute of Practical Studies in Florence.

Professor Giglioli commences his remarks by treating of the oceanic fishes met with during his voyages. Although it is quite true, according to the popular idea, that the sea is full of fishes, it must be recollected that those that inhabit the mid-ocean are quite distinct from those that swarm round the coasts, and are not nearly so numerous. At the same time, many of them are remarkable for their brilliant colour, and are otherwise of special interest. It is difficult, says Professor Giglioli, to describe the beauty of the *Coryphæna hippuris* when first taken from the water: a thousand different tints of deep azure and golden yellow sparkle over its body, which, however, fade upon death with surprising celerity. Other oceanic fishes are the large *Thersytes*, various species of Tunny, the well-known Pilot fish (*Naucrates*), and the *Echeneis*, concerning which such marvellous tales are told by ancient writers. But, perhaps, the most attractive of all the group to the oceanic traveller are the flying fishes (*Exocetus*). Of this genus six species were met with during the voyage of the *Magenta*, each appearing to have a peculiar district of the ocean assigned to its range.

Of the class of reptiles which Professor Giglioli next speaks of, two orders only have oceanic representatives—namely, the Ophidia and Testudinata. Of the sea-snakes three species were met with belonging to the genera *Hydrophis* and *Pelamis*. This peculiar family of serpents was formerly supposed to be confined to the Indian Ocean; but it has of late years been discovered to extend its range over the Pacific, even up to the Gulf of Panama. Of the marine Turtles likewise three species, all well known, were observed.

The class of birds, which follows third in Prof. Giglioli's memoir, is much better represented on the so-called "desolate" ocean. Members of four large families of this class frequent the seas traversed by the *Magenta*, which were chiefly those belonging to the southern hemisphere. These are the Penguins (*Spheniscidæ*), the Petrels (*Pro ellariidæ*), the Gulls (*Laviaæ*), and the Pelicans (*Pelicanidæ*). A fifth great oceanic family, the Awks (*Alcidæ*), replaces the penguins in the Arctic Seas, and was not met with by Prof. Giglioli. The most abundant of all oceanic birds are, of course, the petrels and albatrosses, of the family Procellariidæ, many of which pass by far the greater part of their lives in mid-ocean. Upwards of forty species of this group are enumerated as having been encountered during the circumnavigation of the *Magenta*, amongst which are several supposed to be new to science, and which are provided with new names accordingly.

The mammals of the ocean, which the present memoir lastly treats of, belong to three very different orders: the Cetaceans, Seals, and Sirenians. Of these the first alone pass their whole existence in the salt sea. All the marine Carnivores, so far as we know, habitually resort to land, or at all events to ice, which in polar regions serves the same purpose, and of the few existing members of the Sirenia, one at least is rather an inhabitant of fresh water than of salt. Prof. Giglioli's observations are chiefly confined to the Cetaceans, of which thirteen or fourteen

* Note intorno alla distribuzione, della Fauna Vertebrata nell'oceano, presse durante un viaggio intorno al Globo, 1865—68, dal Professore Enrico Hillyer Giglioli. Firenze, 1870, 8vo. 96 pp.

species were met with in various parts of the ocean traversed by the *Magenta*.

At the end of his memoir, Professor Giglioli gives a kind of journal of his voyage, containing the approximate latitude and longitude of the *Magenta* upon each day of her circumnavigation, and the various species of vertebrates observed or obtained, in parallel columns. Further assistance in tracing the distribution of oceanic life is afforded by the concluding chart, in which the track of the vessel is exactly delineated, and the names of the principal animals met with on each spot are likewise given.

It will be thus seen that even an oceanic voyage round the world, without counting the foreign lands touched upon, affords ample opportunities for the student of nature who has the use of his hands and eyes. In sending a single frigate on such a voyage, the poverty-stricken government of Italy does not hesitate to put on board a band of scientific observers. Does Mr. Childers do the same, when he sends his flying squadron round the world composed of the largest and most expensive ships which wealthy England can produce? We are ashamed to say he does not. Any application, even, for a free passage for a naturalist on such an occasion, would receive the stereotyped refusal, and the answer that "my lords" had no funds to devote to such purposes, and no space to spare.

P. L. S.

EARTHQUAKES AT FIUME DURING THE YEAR 1870

THE following list of earthquakes at Fiume during the year 1870 is sent by a correspondent at that town, to whom it was furnished by Prof. E. Stahlberger, of the Naval Academy, together with an extract from the Journal of Meteorological Observation kept at that Institution, condensing the remarks, &c., of the original, and omitting such details as are of mere local interest, as well as descriptions of the apparatus used in marking direction. The year 1870 is not to be taken as a fair specimen of the frequency of earthquakes in this place. It was decidedly an exceptional year, both in this respect and with regard to the weather, which was unusually changeable throughout the whole twelve months, and during the autumn and December was marked by an abnormal amount of rain. About the time of the December full-moon a large halo of broad bands was noticed by our correspondent and others at about 8 P.M. On the 20th of May, at about 9 P.M., a very faint Aurora Borealis was visible.

No.	Day.	Time.	Duration.	Direction.	Remarks.
1	Feb. 28	0.22 P.M.	About 4 sec.	Apparently N.N.E. to S.S.W.	This beginning of the series of earthquakes came so unexpectedly that no apparatus was in readiness for marking the exact direction. The shock was violent; the oscillations succeeded each other with great rapidity.
2	Mar. 1	8.57 P.M.	5 sec	N.N.W. to S.S.E.	An alarmingly violent shock, the most violent that has occurred here for many years. Its commencement was sudden; it was accompanied by hollow, roaring sounds; the oscillations extremely rapid. Of all the shocks during the year, this was the most remarkable. It produced disastrous effects on the village of Clana, situate inland 2½ hours distant from Fiume. Out of the 150 houses there, 40 were rendered uninhabitable, and the rest were more or less injured. The walls mostly fell outwards, and no lives were lost.—(N.B. Not many walls fell, but very many were cracked the whole way down, and the houses left in so unsafe a condition that they were uninhabitable. I saw the village myself some time after the catastrophe, but before the repairs and rebuilding were made.—A. M. SMITH)
3	Mar. 2	About 1.15 A.M.	....	N.W. to S.E. or N. to S.	Slighter as to violence.
4	Mar. 4	2.45 A.M.	5 sec.	N.W. to S.E.	Oscillations very rapid. On the preceding evening, at 7.5, a slight vibration of the earth was perceptible, lasting, with interruptions, two minutes. The same phenomenon was again observed at 11.14, about three hours therefore before the actual earthquake
5	April 28	3.25 A.M.	....	......	Two distinct shocks, separated by an interval of two or three seconds.
6	May 9	4.16 A.M.	....	......	Vibrations or tremblings were observed on April 29, at 7.30 A.M. and 2.28 P.M., and on May 4, at 2.30 A.M.
7	May 10	2.51 A.M.			
8	do.	9.19 A.M.			
9	do.	4 5 P.M.			
10	do.	5 56 P.M.	....	Vertical	This was one of the more violent shocks, and was also felt at sea.
11	May 11	1.30 A.M.	....	N.W. to S.E.	Slight.
12	do.	2.15 A.M.	....	do.	do.
13	do.*	2.50 A.M.	....	Vertical	Violent. It consisted of two distinct movements, separated by an interval of a few seconds, and was preceded by a thunder-like sound.
14	do.	4.15 A.M.	....	do.	Violent.
15	do.	4.30 A.M.	....	N.W. to S.E.	Slight.
16	do.	5 0 A.M.	....	do.	do.
17	do.	9 15 A.M.	....	do.	do.
18	do.	9 50 A.M.	....	do.	do.
19	do.	11.38 A.M.	....	do.	do.
20	do.	2.18 P.M.	....	do.	do.
21	do	During the night; time not specified	....	do.	do.
22	May 13	9 5 A.M.	....	......	do.
23	do.	11.19 P.M.	....	......	do.
24	May 14	0.50 A.M.	....	......	do.
25	May 16	10 (?) P.M.	....	......	A slight shock, preceded by a hollow groaning noise some short time previously.
26	do.	10.25 P.M.	....	......	Of short duration, but of some violence, consisting of two distinct shocks.
27	May 18	10.57 P.M.	....	......	Two slight shocks, following close on one another.
28	May 19	9.26 A.M.	....	......	Slight.
29	May 21	1.5 A.M.	....	......	Slight, preceded by a rolling noise of long duration, at 1.45 P.M. vibration,
30	May 23	8.25 P.M.	....	......	Slight.
31	do.	About 10.45 P.M.	....	......	do.
32	June 2	0.27 A.M.	3 sec.	......	Somewhat violent.
33	Sept. 25	5.43 A.M.	4 sec.	......	Slight, with loud thunder-noise.
34	Oct. 13	4.30 P.M.	....	......	Slight.
35	Oct. 17	8.0 P.M.	....	......	A double shock, with loud noise.
36	Dec. 16	1.50 A.M.	....	N.E.	Moderately violent.

* On this day, between 2.50 and 9.15 A.M., there were 27 insignificant movements not specified in the list.—A. M. S.

THE GENESIS OF SPECIES*

IT is a remarkable illustration of the apparently fitful manner in which our knowledge of Nature increases, that the event which has probably been more fruitful than any other during the present century in inducing practical advances in the study of Natural History, was the promulgation of a pure theory, the publication, namely, by Mr. Darwin and Mr. Wallace, of the doctrine of the Origin of Species by means of Natural Selection. We say a pure theory, because the genesis of a new species is a phenomenon which never has yet, and probably never will, come consciously under the cognizance of man. We see forms of animal and vegetable life die out before our eyes, but their birth is not within our ken. As Mr. Darwin has pointed out, even should a new species suddenly arise, we have no means of recognising it as such. As a matter of fact, new plants and animals are constantly being discovered in all parts of the globe. Even in our own small and well-searched island, the additions within the last twenty years of more or less conspicuous flowering plants to our native flora are not inconsiderable; but no naturalist suggests any other interpretation of this, than that either they have been overlooked before, have been recently introduced from other countries, or that the seeds have been buried for ages in the soil. None the less, however, does it seem possible, or even probable, that we may eventually arrive at a correct solution of the problem by a rigorous induction from known facts.

So recently as the date of the publication of the first edition of the "Origin of Species," in 1859, Mr. Darwin wrote, "The great majority of naturalists believe that species are

FIG. 1.—Leaf Butterfly in Flight and Repose.
The lowest apparent leaf on the stem is in reality the under side of the wing of the same butterfly which is represented in the upper part of the picture.

immutable productions, and have been separately created;" and the statement has been repeated in subsequent editions. We think, however, that it is impossible, at the present time, to sustain the correctness of this assertion. A writer in the "Botanische Zeitung"† has recently shown that there is some reason to believe that Linnæus himself, in later years, considerably modified the rigidity of his adherence to the doctrine which he laid down so decisively in his earlier writings:—"*Species tot numeramus quot diversæ formæ in principio sunt creatæ.*"‡ During the eighteenth and the first half of the present century, however, it was only a few naturalists of more than ordinarily keen powers of reasoning and prodigious knowledge of Nature—Lamarck, Buffon, Geoffroy St. Hilaire, Owen (in his earlier writings), and some others—who were bold enough to enunciate the theory that species have been created by a process of evolution from earlier closely-allied forms of life. Since the publication of the "Origin of Species," we may say that almost the whole body of the younger naturalists of this country and of Germany—Von Baer, Huxley, Spencer, and Haeckel leading the way after Darwin and Wallace—have given in their adhesion to the doctrine of Evolution. It is only within the last twelvemonth that the evolutionists can claim so great an accession to their strength as that distinguished systematist, the President of the Linnean Society.*

* "On the Genesis of Species." By St. George Mivart, F.R.S. (London: Macmillan and Co., 1871.)

† Botanische Zeitung, Sept. 9, 1870.

‡ Linn. Phil. Bot. Aphor. 157, p. 99.

* Bentham, "On the Species of Cassia," a paper read before the Linnean Society in 1870, but not yet published.

Along with this theory, these two writers introduced another, of the *modus operandi* by which this evolution is mainly or entirely effected, that of a process of Natural Selection from spontaneous variations. This doctrine was supported by an enormous array of facts, and by a brilliancy of argument which caused it at first to be as eagerly and generally adopted as the other. During the last few years, however, it may be said that a reaction has been setting in in an opposite direction, and attention has been widely called to difficulties in the way of the full adoption of the theory of Natural Selection, at first overlooked, the force of some of which has been admitted, with his usual candour, by Mr. Darwin himself. Some of these objections were pointed out by Mr. J. J. Murphy in

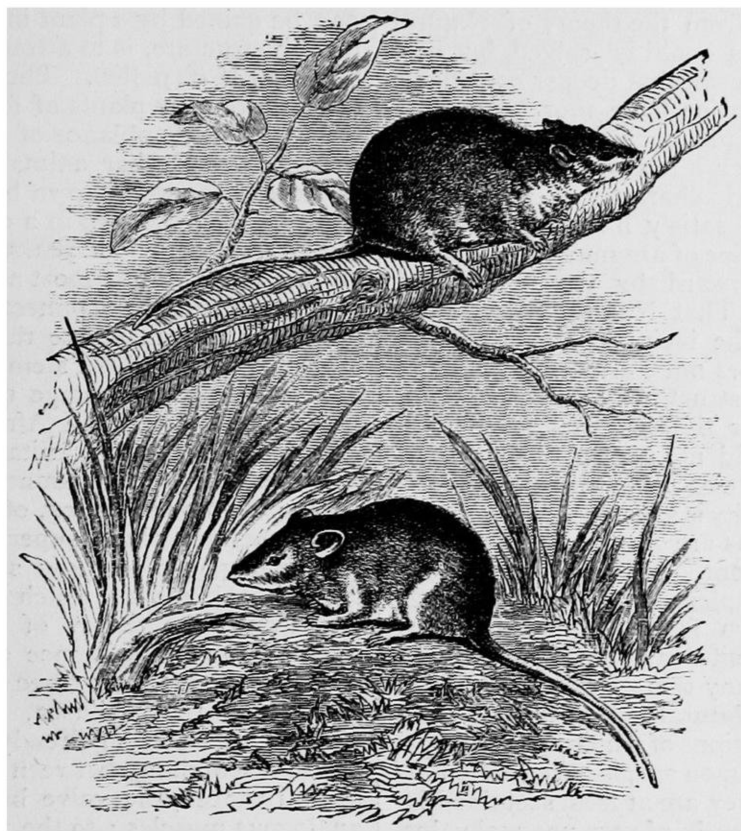

FIG 2.—Upper Figure—Antechinus minutissimus (*implacental*).
Lower Figure—Mus delicatulus (*placental*).

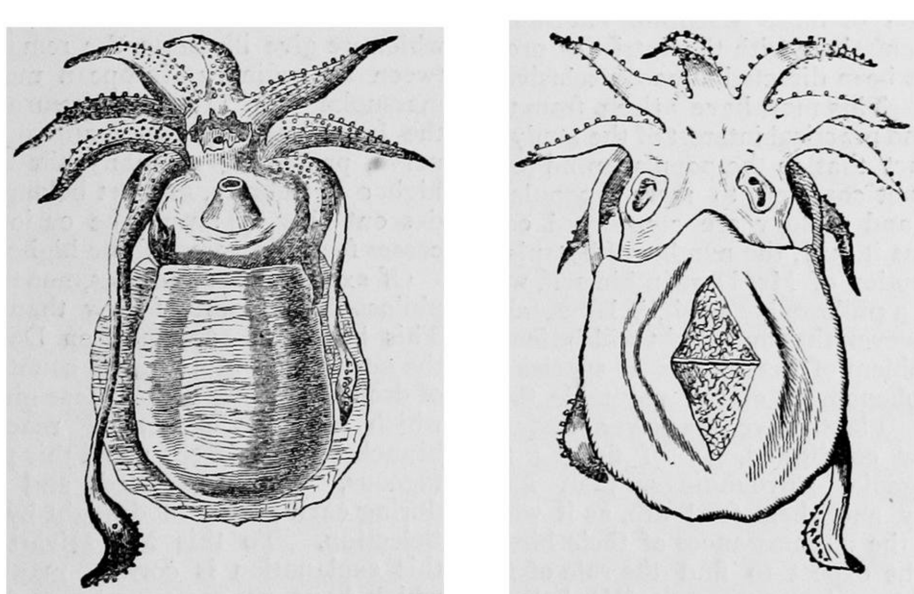

FIG. 3.—Cuttle-fish. Ventral and Dorsal Aspect.

his elaborate essays, entitled "Habit and Intelligence," published in 1869; on others Mr. Mivart dwells in the volume now before us, the most recent contribution to this department of literature.

A theory may be true, and yet may not be adequate. To take an illustration from the realm of mind. The believer in the doctrine of innate ideas will admit that the greater number of our conceptions are the result of habit, imitation, and training; he believes, however, that there are others which cannot thus be adequately accounted for, and which are innate and independent of subsequent education. In the same manner Mr. Mivart and those who think with him freely admit the potency of Natural Selection to have produced the greater number of the specific forms and organic structures we see around us; for the production of others they believe that it can be proved to be inadequate, and that we must look to some other innate principle for their formation. Mr. Mivart supports his arguments with so much cogency of reasoning, so great a knowledge of anatomical structure, and so complete an acknowledgment of the strength of his opponents' position, that they cannot be disregarded by any one interested in the subject. His objections are the more deserving of careful consideration, inasmuch as he states that he was himself by no means disposed originally to dissent from the theory of Natural Selection, if only its difficulties could be solved, but that he has found each successive year that deeper consideration and more careful examination have more and more brought home to him the inadequacy of Mr. Darwin's theory to account for the preservation and intensification of incipient specific and generic characters. It behoves, therefore, every Darwinian to satisfy himself that either Mr. Mivart's premisses or his line of argument is unsound.

The objections brought forward by the author are summed up as follows :—(1.) That Natural Selection is incompetent to account for the incipient stages of useful structures. (2.) That it does not harmonise with the co-existence of closely similar structures of diverse origin. (3.) That there are grounds for thinking that specific differences may be developed suddenly instead of gradually. (4.) That the opinion that species have definite though different limits to their variability is still tenable. (5.) That certain fossil transitional forms are absent which might have been expected to be present. (6.) That some facts of geological distribution supplement other difficulties. (7.) That the objection drawn from the physiological difference between "species" and "races" still exists unrefuted. (8.) That there are many remarkable phenomena in organic forms upon which Natural Selection throws no light whatever, but the explanations of which, if they could be attained, might throw light upon specific origination. If these objections are not new, they are at least sustained by new arguments. They are evidently of very unequal value. The third is very difficult of proof or disproof. The fifth may be true in our present state of knowledge, but would be very unsafe by itself as the basis of an argument. The first, second, and eighth are of greatest value, and are those which Mr. Mivart has most closely worked out.

Hitherto the attention of those scientific naturalists who have concerned themselves with the intricate problems of organic life, has been directed almost exclusively to the animal kingdom. This may have arisen from the greater attractiveness and practical interest of the study of zoology, or from the fact that in the popular mind (and we fear the error is not confined to mere "popular" writers) natural history and zoology are considered convertible terms. Be this as it may, the number of botanists, with the illustrious exception of Mr. Darwin himself, who look on their science in a philosophic spirit, is lamentably small. We believe, however, that more light will be found to be thrown on the problem of the genesis of species by a consideration of the phenomena of the vegetable than of the animal kingdom. Plants have less power of adapting themselves to new conditions, or of finding for themselves more congenial surroundings, than have animals. Their locality and their food are, as it were, prescribed for them by the circumstances of their birth; here, therefore, we might expect to find the rule of the survival of the fittest to reign supreme. We believe, however, it would be very difficult to substantiate any instances of species of plants being supplanted by other closely allied species, similar to those well-authenticated in the case of the rat and the cockroach. Plants when first artificially introduced into a new country undoubtedly frequently spread with extraordinary rapidity, to the destruction of weeds belonging to native races; but this is evidently not the mode in which species have supplanted one another in a state of pure nature.

Under Mr. Mivart's first head, he deals with the subject of Mimicry, contending that Natural Selection is incompetent to account either for the first or last stages of such wonderful instances of protective resemblance as that represented in our illustration. As this subject has been so recently discussed in these columns, we need not dwell upon it further than to remark, that we think the author could have supported his case with arguments of even greater force, had he extended his observations to the vegetable kingdom. The only object which it has been conjectured can be gained by a plant imitating a different species or a foreign structure, is to attract insects to assist in the distribution of its pollen. The most remarkable instances of the imitation by plants of foreign objects is in the case of the curious resemblance of the flowers of certain orchids to insects and other animals. One of the most singular of these is the well-known bee-orchis. But, as Mr. Mivart remarks, Mr. Darwin, in a course of observations extending over a series of years, has never seen a bee alight on this orchis. The most noteworthy resemblances again of plants *inter se* unconnected with organic affinity, are not in the flowers, where they might be useful, but in the leaves, or in the whole stem and foliage. It is difficult to conjecture any advantage that is gained by the close resemblance between an African Euphorbia and a South American Cactus, the imitation being carried out in the most extraordinary manner throughout the vegetative organs, the flowers being, of course, totally unlike.

But besides these superficial resemblances, there are also analogies of organic structure in different classes of the animal kingdom, which Mr. Mivart holds to be equally opposed to the theory of Natural Selection. He refers especially to the existence of the higher organs of sense, as the eye, in at least three distinct and independent lines of descent, the Mollusca, the Annulosa, and the Vertebrata, an objection already pointed out by Mr. Murphy; to the resemblance between the shells of certain Mollusca and Crustacea, the valve being moved in each case by analogous muscles; to the analogy between the different families of Marsupials and the different orders of Placental Mammals; and to numerous other instances. These might be supplemented in the vegetable kingdom by the similarity in the mode of opening of the anthers in Berberidaceæ and Lauraceæ, or the extraordinary resemblance of certain Conifers to flowerless plants. The wood-cuts which we give illustrate the remarkable resemblance between an ordinary European mouse and an Australian marsupial (Fig. 2); the structure of the cuttle-fish with the brain, cartilaginous cranium, and complex auditory nerve, presenting so many similarities to those of the higher Vertebrata, and yet belonging to a different line of descent (Fig. 3); and the curious bird's-head-like processes found in some of the higher Polyzoa (Fig. 4.)

Of exceptional structures, none is more interesting in a philosophical point of view than the neck of the giraffe. This has been explained on Darwinian principles from the occurrence in its native country of occasional periods of drought, during which those giraffes only have survived which had the power of reaching somewhat higher branches of the trees; and this peculiarity, being advantageous, was propagated, and continually augmented during each period of drought by the process of Natural Selection. To this Mr. Mivart objects, firstly, that if this explanation is correct, many other African animals, which have no greater power of endurance or of migration than the giraffe, ought to have elongated necks; and secondly, that in the intervals between the droughts the long neck would be a positive disadvantage, as requiring a greatly increased size and strength of muscles to support it, and would, consequently, be lost before the next drought set in. To take another instance of the commencement of an organic structure which is universal in all the higher classes of animals; there is scarcely anything more inexplicable than the separation of the sexes, if we suppose animals with distinct sexes to have originated by the process of Natural Selection from those simple forms which propagate by cell-multiplication or spontaneous fission.

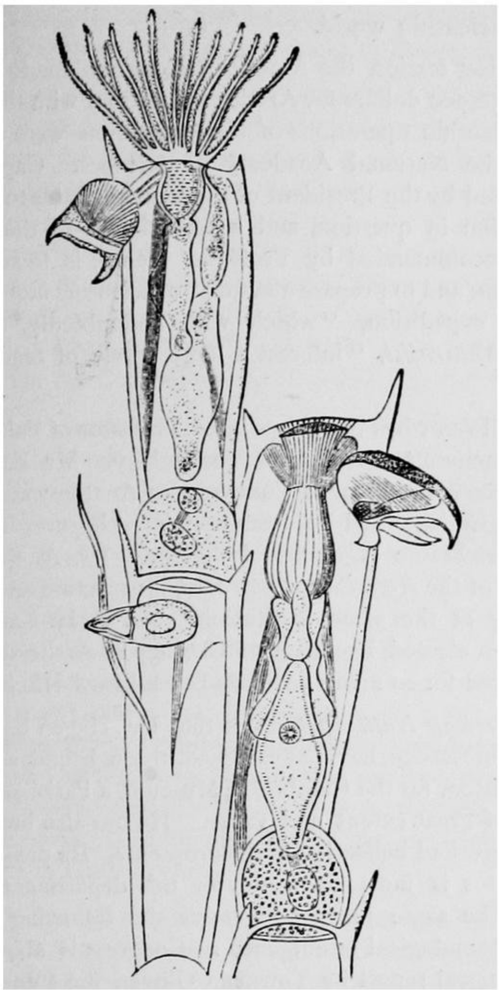

FIG. 4—A Polyzoon with Bird's-head Processes.

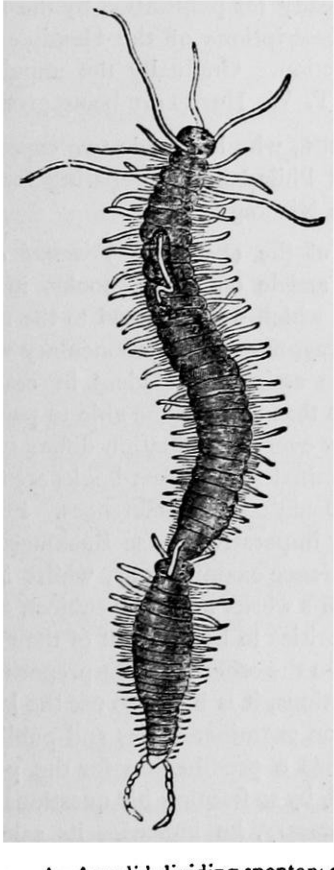

FIG. 5.—An Annelid dividing spontaneously.
(A new head having been formed towards the hinder end of the body of the parent.)

We have not space to enumerate all our author's arguments, for which we must refer our readers to the volume itself, wherein even those who differ from his conclusions will find a mine of physiological information and ingenious speculation. Nor can we do more than allude to the theological portion of the work, wherein he ably defends the doctrine of Evolution against both the *odium theologicum* and the *odium antitheologicum.*

The present state of the argument we take to be this :—The theory of Natural Selection, in the hands of Mr. Darwin and Mr. Wallace, afforded a simple, a beautiful, and a valid solution of the origin of a large number of the organic phenomena by which we are surrounded ; by many disciples of Mr. Darwin it has been assumed, perhaps too rashly, as adequate to account for the entire evolution of all the existing forms of animal and vegetable life from one or a few primordial germs. To this idea, so seductive in its very simplicity, a number of more or less cogent objections have now been urged. It is possible that on still closer examination, these objections will be found to break down ; but in the meantime we must suspend our judgment ; and in order to save defeat, the next move must be made by the advocates of Natural Selection, a *primâ facie* case against them having at all events been made out. Mr. Mivart has no counter theory to propose, beyond a belief that there exists in all organic life an innate power analogous to intelligence, which controls their actions as reason does those of men. Should the inquiries which are now being energetically pursued on every side result in our acquiring more accurate knowledge of such a force, it will be safe to predict that to it will then be ascribed a more easy and natural solution of many phenomena which we are now forced to attribute to Natural Selection.

ALFRED W. BENNETT

NOTES

THE Royal Commission on Scientific Instruction and the Advancement of Science is about to resume its sittings, and meetings will be held at 6, Old Palace Yard, Westminster, on Thursday, February 9 ; Friday, February 10 ; Monday, February 13 ; Tuesday, February 14, at 11.30 A.M.

IN the recent changes which have taken place in the Royal Mint by the deaths of Professors Graham and Miller, Dr. Stenhouse, F.R.S., lost his position as Non-resident Assayer to the establishment. Several of our leading chemists, including Sir Benjamin Brodie, and Professors Williamson, Frankland, and Odling thought that this was an opportunity for recognising the services which Dr. Stenhouse had rendered to chemistry by his numerous researches, and they requested Dr. Lyon Playfair, M.P., to bring his claims before the Premier. We have now the gratification to announce that the representation has been successful, and that Her Majesty has directed that a pension on the Civil List of 100*l.* should be given to Dr. Stenhouse "in consideration of his scientific attainments."

THE shelling of Paris has been disastrous to M. Desnoyers, the librarian of the Museum, who lost his son by it. The celebrated Abbé Moigno, editor of *Les Mondes*, was slightly hurt by the breaking of a glass, from an explosive shell. The private apartment of M. Milne-Edwards was visited by a shell which has done much harm to the valuable furniture. Another shell penetrated into the Gallery of Zoology and caused much damage to the glass-work ; and again another into the Gallery of Mineralogy, in the very place where M. Daubrée performs his experiments on artificial meteors ; but happily it had exhausted its force, and did but little harm. Many instances of this description have occurred, where shells were sent to their utmost range, and did not explode under such circumstances. The reverse has however been observed, for one shell falling in the Seine with great force, the water offered resistance like a solid body, and explosion took place : the effect was curious. A shell fell in the courtyard of the Collège de France when M. Levavasseur was lecturing on political economy, and the young professor was not disturbed by the projectile, portions of which broke through the windows. He continued lecturing, saying, "Those who fear such things have only to leave the place."

THE addition to the building for the Museum of Comparative Zoology at Cambridge, at an expense of upwards of 60,000 dollars, is rapidly going on. Prof. Agassiz has returned to Cambridge with restored health, and with new plans for the enlargement of his museum. Prof. O. C. Marsh, of Yale College, has just returned with his party from the Rocky Mountains.

THE *Indépendance Belge*, in its number for January 9, publishes an extract from the *Comptes-rendu de l'Académie des Sciences*, in which M. Elie de Beaumont describes the experiments used in directing the course of the balloon which fell at Rheims. In accordance with what was said in one of our recent numbers, M. Elie de Beaumont, who had witnessed the ascent, explained to his colleagues that the wind was bearing to the East, but that owing to the deviation the balloon would reach Switzerland! He was, however, most unfortunate. The captain of the directing balloon arrived recently at Lille. Experiments directed by M. Mangon have proved that a balloon 90,000 cubic metres measurement requires forty-two foot-pounds to move with a rapidity of a yard per second, which is a very important result for future experiments. The mass of gas and balloon is about three tons.

MR. J. HOPKINSON, the Senior Wrangler of 1871, is a native of Manchester. He was educated at Owens College; and at Easter 1867 obtained the Senior Mathematical Scholarship at Trinity, after open competition. In 1868 he also obtained the Sheepshanks Astronomical Scholarship. In 1867 he was awarded the exhibition in Chemistry and Natural Philosophy, at the first examination for the B.Sc. degree at the London University; and at the second examination for the same degree in the following year he carried off the Scholarship in Mathematics and Natural Philosophy. Mr Jas. W. L. Glaisher, the Second Wrangler, is the son of Mr. Jas. Glaisher, F.R.S., director of the Meteorological Department at the Greenwich Observatory. He received his education at St. Paul's School, where he gained the first mathematical prizes in 1865, 1866, and 1867. On leaving school he entered Trinity College with a Senior Campden Exhibition. He is the author of a paper read before the Royal Society in March 1870, on the numerical value of the sine-integral, cosine-integral, and exponetical-integral.

THE Delegate Government of Bordeaux has established a Scientific Commission, presided over by M. Marié Davy, the meteorologist, and having amongst its members the Professor of Analysis at the Polytechnic School of Paris, M. Silberman, Head of the Physical Laboratory at the Collège de France, and some local scientific celebrities.

M. LEVERRIER is said to be hiding in a country place near Marseilles, assuming a false name, and living like a private man with one of his intimate friends.

M. THÉNARD, a member of the French Institute, who had retreated to his home on the Côte d'Or, was taken prisoner and sent to a fortress in Germany as a hostage. The President of the French Institute has protested against the arrest. M. Thénard is the son of the celebrated Professor of Chemistry, and belongs to the agricultural section. He is a landed gentleman of considerable property.

THE elephants and other animals of the same description at the *Jardin des Plantes* were sacrificed to the necessities of the war. Members of the French Institute were present on the spot in order to witness the effects of the shots on the huge brutes, and some parts of the body were set apart for careful dissection. M. Milne-Edwards has taken advantage of the circumstance to prepare a paper, which must have been already presented to the French Institute, and which will create a sensation in the scientific world.

DURING last session the American Congress made an appropriation of 50,000 dollars for Arctic exploration, with the promise that the scientific operations of the expedition were to be prescribed by the National Academy of Sciences. Captain Hall was appointed by the President of the United States to command the expedition in question, and a commission of the National Academy, recommended by Professor Henry, is to act in concert with him, and to prepare a manual of scientific inquiry for the use of the expedition, "which will, undoubtedly," says the *American Naturalist*, "interest a large circle of readers when published."

MR. A. HYATT has been appointed Professor of Palæontology at the Massachusetts Institute of Technology. Mr. E. S. Morse has been chosen Professor of Comparative Anatomy and Zoology at Bowdoin College, and has been appointed lecturer in the same branch at the Maine Agricultural College. Dr. A. S. Packard, jun., editor of the *American Naturalist*, is to lecture on Economic Entomology at the same institution. Mr. B. H. Emerson has recently been elected Professor of Geology at Amherst College, the chair filled for so many years by Dr. Edward Hitchcock, sen.

THE *American Naturalist* states that Dr. Hagen has recently returned from Europe, having purchased, through funds supplied by a lady in Boston, for the Cambridge Museum, a Parisian collection of weevils of great extent and value. He has also brought over his own unrivalled collection of Neuroptera. Its presence in the United States is most fortunate for this department of entomology. The same journal gives us the following additional items of entomological intelligence:—Congress is about to print an entomological report by Townend Glover, the entomologist of the Agricultural Department. It will form an exceedingly useful work, and will deserve the widest circulation. Mr. P. R Uhler, of Baltimore, has ready for publication by the Boston Society of Natural History, descriptions of the Hemiptera of the Harris entomological collection. Gradually the unpublished results of the labours of Dr. T. W. Harris are being given to the public.

MR. J. A. M'NIEL, who has made two expeditions to Central America, is now in Philadelphia preparing for a third archæological excursion to Nicaragua.

IN the number of the *Quarterly Journal of Education* for January is a short article on school books, in which occur the following remarks, which we commend to the notice of those interested:—"The vast majority of elementary works are written specially as aids to assist the student in cramming a certain specified subject, so that he shall be able to pass some particular examination. Now every examination differs to a certain extent from any other examination, and text-books are therefore required to point out and elucidate these differences. For example, one is told that it is almost imperative to use Buckmaster's Chemistry in preparing for the science examinations, whilst Miller's, Williamson's, Gill's, or Barff's works upon the subject, although admitted by competent authorities to be the best of their kind, are kept in the background. On the other hand, in preparing for the London University examinations, it is better to use the latter works. . . . Frequently, again, an examiner writes and publishes a text-book, and, of course, he has a predilection for the peculiarities of his own offspring; and, by so framing his questions that a knowledge of *his* book is necessary, he increases its sale, and thereby the balance at his banker's." Without subscribing to the inuendo contained in the last sentence, we think the *Journal* here points out a weak place in our examination-system. Great complaints are made—and these do not come from rejected candidates—that the mode in which questions are sometimes put at these examinations renders them impossible to be answered clearly by any student who has not been trained in the particular school of the examiner, though he may possess a competent knowledge of the subject. This is a great evil, and ought to be looked into.

WE have received the first four numbers of what seems likely to be a very useful series of publications issued by authority of the University of New Haven, Connecticut, and termed "The University Series." Two of these are reprints of the well-known English treatises :—Prof. Huxley "On the Physical Basis of Life," and Dr. Jas. H. Stirling "As Regards Protoplasm, in reference to Prof. Huxley's Essay." The remaining two comprise Prof. Cope's essay "On the Hypothesis of Evolution, Physical and Metaphysical ;" and Prof. G. A. Barker of Yale College "On the Correlation of Vital and Physical Forces."

A RECENT number of *L'Illustration Horticole* contains an interesting paper on the Botanic Gardens of Kew, by M. André, prefaced by some details regarding similar establishments in Europe. From this it appears that the first was established at Padua in 1545, followed by that of Pisa ; those of Leyden and Leipzig date respectively 1577 and 1579. The Montpellier garden was founded in 1593, that of Giessen in 1605, of Strasburg in 1620, of Altorf in 1625, and of Jena in 1629. The Jardin des Plantes was established in 1626, and the Upsal Garden in 1627 ; that of Madrid dates from 1763, and that of Coimbra from 1773. At the end of the eighteenth century, according to Gesner, more than 1,600 kindred establishments existed in Europe. England comes late in the list, the Oxford Garden not having been founded until 1632, and long remaining the only one in the kingdom.

WE learn from the *Scottish Naturalist* that the work of fitting up cabinets for the reception of the Natural History collection in the Paisley Museum is rapidly approaching completion ; and that the opening is expected to take place shortly. The geological and botanical specimens are mostly, if not all, British, and will form a valuable reference collection for students. The large and valuable reference library will contain a choice selection of scientific works.

THE revenue cruiser *Moccasin*, according to the *Technologist*, has been supplied with a new marine drag, that is, one of those substitutes for an anchor which will, it is said, effectually prevent a ship's drifting even in the heaviest weather. The success of experiments made and reported to the Treasury department has caused a contract to be made with a New York firm for the construction of several of these marine drags ; and it is the intention to supply all vessels in the Revenue service with them. To vessels off a lee shore, in stormy weather, the marine drag must be a most desirable protection against shipwreck.

SINCE the American publishers abandoned the reprinting of the *Chemical News*, our American friends, says the New York *Technologist*, "have no resource but to subscribe for the original London edition. We do not regret this. What they lose in cost they more than gain in neatness and accuracy, and also in the fact that they do not have to wait a month for their news."

MR. ANDREW MURRAY has published in the *Field* some remarks upon eight samples of honey which have been forwarded by M. de Solsky, of the Agronomic Museum of St. Petersburg, to the South Kensington Museum. The honey was produced by bees fed in districts where there was a great preponderance of the following plants :—*Reseda odorata* (mignonette), *Tilia parvifolia*, *Dracocephalum moldavicum*, *Carduus nutans*, *Fagopyrum esculentum* (buckwheat), *Epilobium angustifolium*, *Echium vulgare*, each plant being represented by one of the samples. The eighth consisted of honey flavoured by the herbs of the Steppes of Central Russia, and this was the best of all in taste. Next came that from the mignonette, then that of the lime-tree ; the buckwheat honey being beyond question the worst.

ONE of the most interesting and important trees of Sumatra is the Camphor-tree, *Dryobalanops camphora*. This camphor attracted the attention of the earliest voyagers, and was then, as it is now, an important article of commerce with China and Japan, the people of those countries attributing to it extraordinary virtues and paying a high price for it. The tree grows to a height of 100 or 130 feet, and forms a trunk 7 to 10 feet in diameter. The quantity of camphor contained in the trunks is very unequal, the young trees appear to contain little or none. It is said that, on an average, about nine trees are required to produce 100 lbs. weight of crystallised camphor. It is obtained by cutting down the tree and dividing the wood into small pieces, in the divisions of which the camphor is found. It differs in the form of its crystals from the camphor of commerce, is harder, more brittle, and does not so readily condense. Great quantities are used by the Bataks for the preservation of the corpses of their chiefs. The trees are spread over a portion only of Sumatra and Borneo, and generally occur in localities into which commerce and civilisation have as yet but little penetrated. Notwithstanding the continued destruction of the trees, for the sake of procuring the camphor, no means are taken for the future preservation of the species. This camphor is seldom seen in this country, except in museums. The Chinese eagerly buy it in preference to the ordinary camphor—their own produce—which they send in such large quantities into the European markets.

THE habit of branching among palms, though constant only in the Doum palm (*Hyphæne thebaica*) of Egypt, is not uncommon in other genera, and notably amongst Palmyras (*Borassus flabelliformis*) and Cocoa Nuts, (*Cocos nucifera*), figures and descriptions of which have been published in the Linnean Society's Transactions. According to a correspondent in Ceylon, branching Cocoa Nuts, Palmyras, and Arecas, are to be seen there in plenty, besides other curious freaks of the Cocoa Nut, such, for instance, as the growth of two or three trees from one nut. In the Racket Court at Colombo, we are told that five trees are now flourishing (or at least were at the time the letter was written) which have proceeded from the same nut. Originally there were eight in all, but three have died, probably by the nourishment being drawn away from them for the sustenance of the other five.

THE use of the fruits of *Taughinia venenifera* in Madagascar as an ardent poison is of great antiquity, and is one of the still remaining superstitious customs ardently believed in by savage nations. The system of administering the poison has been often told, but the following account from a private letter of an eye-witness differs in so many points from those we have before read that we give it entire. The fruit was taken, bruised, and boiled whole. A fowl was boiled, and the broth set aside. Three pieces of the skin of the fowl were cut and put into the broth ; a cupfull of poison was first administered, followed by another of the broth containing the three pieces of skin. If vomiting did not speedily set in, the poison soon killed, but if it did, it was kept up by constant exhibition of the broth and warm water until the three pieces of skin were ejected. Should the skins obstinately remain, it was held as evidence of guilt, and another dose of the poison was administered.

SOME discussion has taken place in Indian circles on the authenticated, but unprecedented, fact of a tigress having been shot near Octacamund, by Colonel Christie, while she was forty feet from the ground in a tree. Many observers state that a tiger when in danger, and at the foot of a tree, does not take to a tree, and in the inundations only gets on to the lower branches. A leopard does take to a tree.

HEAT SPECTRA

I NOTICE in NATURE, vol. i., p. 28, an account of some very important researches on Heat Spectra, made by the late Professor Magnus; and I am gratified to think that some observations which I made on the subject in the years 1858 and 1859 were confirmed by this eminent German philosopher.

In a paper read before the Royal Society of Edinburgh in 1858, I showed that rock-salt absorbs heat radiated from rock-salt in larger quantity and more powerfully than other kinds of heat; and also that the amount of absorption of rock-salt for heat increases with the thickness of the absorbing plate. These are the fourth and fifth results of Professor Magnus. His next result is very interesting, namely, that the high diathermancy of rock-salt does not depend on its small absorptive power for the different kinds of heat, but on the fact that it only radiates, and consequently only absorbs, heat of one kind; while almost all other bodies at the temperature of 150° F., emit heat which contains only a small fraction or none of those rays which are given out by rock-salt.

Certain experiments which I made in 1859 lead me to think that Professor Magnus was quite justified in his conclusion that the heat radiated and absorbed by rock-salt is a peculiar kind of heat. These experiments, which are described in the Transactions of the Royal Society of Edinburgh, are as follows. I tested the quality of the heat radiated by rock-salt at 212° F. by transmitting it through three different screens :—

α A screen of mica.
β One of mica split by heat.
γ One of glass.

It was found that a mica screen, which passed about 31 per cent. of ordinary lampblack heat, passed only 18 per cent. of rock-salt heat; or if we call the proportion of black heat passed by the mica 100, that of rock-salt heat will be 58. Again, it was found that while 20 per cent. of lampblack heat passed through a screen of split mica, the proportion of rock-salt heat transmitted through the same screen was only 15¼ per cent. These numbers are to one another as 100 to 76.

Lastly, with respect to a glass screen, calling the proportion of lampblack heat which passed 100, that of the rock-salt heat which passed the same screen was 57.

On these results I remark as follows:—It is already well known that rays of great refrangibility, or small wave length, pass through glass or mica more readily than those of an opposite character. The difficulty with which rock-salt heat penetrates these substances as compared with ordinary heat, might therefore lead us to infer that the wave length of this heat is greater than that of ordinary lampblack heat.

If we now look to the relative transmission of the two descriptions of heat through mica split by heat, we see that the facility of transmission is yet in favour of ordinary heat, but not so strikingly as with a screen of common mica. This will be seen from the following table :—

Nature of Screen	Transmission of Ordinary Heat at 212°	Transmission of Rock-salt Heat at 212°
Mica	100	58
Mica split by heat	100	76

Compare this with the following table, deduced from the results given by Professor Forbes :—

Nature of Screen	Transmission of Heat from Blackened Brass at 700° F.	Transmission of Black Heat at 212° F.
Mica ·015 inch thick	100	52
Mica split by heat	100	64

From a comparison of these two tables, it will be seen that, as tested by the two substances, mica and mica split by heat, rock-salt heat at 212° F. bears to ordinary heat of that temperature a relation similar to that which ordinary heat at 212° bears to heat at 700°; that is to say, that just as heat of 212° has a greater wave-length than heat of 700°, so rock-salt heat at 212° has a greater wave-length than ordinary heat at that temperature. And the *surface stoppage* produced by splitting the mica, telling most powerfully upon heat of high temperature, or small wave-length, while the *stoppage* by *substance* is in the opposite direction, we see how the one effect tends, to a certain extent, to neutralise the other, rendering the proportions of different kinds of heat passed by split mica more nearly alike than those passed by ordinary mica. In connection with these remarks I may state that neither the radiation nor the absorption of a plate of rock-salt is sensibly influenced by roughening its surface with emery paper.

All these experiments concur in showing that heat from rock-salt possesses very great wave-length, and probably heat from a thin plate of this substance, at a low temperature, may be found to possess a greater wave-length than any other description of heat which can be exhibited.

The observations of Professor Magnus with respect to the nature of the heat from potassium chloride are very interesting; unfortunately I did not make any experiments on this substance, but I did on some others in the shape of powder.

I found that the comparative radiation at 212° was as follows :—

From lamp black	100
Alum in powder	100
White sugar	98·7
Sulphate of potash	88·1
Nitrate of potash	86·7
Table salt	83·1

The experiments of Prof. Magnus on the reflection of heat are also of the very greatest importance, and they strengthen the evidence (already overwhelming) in favour of that view which regards light and heat as varieties of the same agent differing in nothing except wave-length.

BALFOUR STEWART

ON THE METHOD OF ASSAYING SILVER AS CONDUCTED IN THE INDIAN MINT*

BY DR. H. E. BUSTEED, OFFG. ASSAY MASTER

THE method of assaying silver, as now in use in H.M.'s Indian Mints, is one peculiar to them; it was introduced into the Calcutta Mint about the year 1850, and thence extended in course of time to those of Bombay and Madras.

It has been favourably reported on and described more or less in detail as an official duty by various assay officers to local Mint authorities in India; but beyond this it would appear that no attempt has been made towards giving publicity to the practical working of the process, or to making generally known the laboratory details of this method of assay.

It has been suggested to the writer that some such attempt now would be not only interesting but useful, as after twenty years' experience of it, the assay offices in the Indian Mint must be in a position to assign its true value to a method which has been used for the assay of an immense importation and coinage of silver bullion. To render it more generally intelligible, and to show wherein the process about to be explained contrasted with those in more general use, Dr. Busteed very briefly adverted to the principles on which those processes depend for their results, omitting details and technicalities. In modern acceptance, the principal duty of an assayer is to ascertain the proportion of the precious metals present in any sample of mixed metal submitted to him for examination, so that from the result of his investigation the proper value may be assigned by calculation to the mass which the sample is supposed to represent.

This the assayer effects by separation of the precious metals from the coarser ones. The most ancient means of effecting this was by the method of *cupellation*. He explained the principle of this method, what skill and experience it required on the part of the operator, and how it still fell short of accuracy in its results.

* From the Proceedings of the Asiatic Society of Bengal.

Its shortcomings led to the invention of another process by Gay-Lussac, known as the volumetric, or humid, method, which is much more accurate, and is now practised very generally on the Continent. Its principles were briefly glanced at. Its introduction, however, into the Indian Mints was not considered desirable by their assay officer for certain reasons, a few of which were given. The method of cupellation, therefore, being not accurate enough for the purposes of buying and selling bullion, and that by the French process being considered not well suited to Indian Mints, it became necessary to look out for and introduce into the Mints of this country a process more likely to answer all the ends in view.

This object was attained by the adaptation and introduction of the process now in use, viz. the "Chloride process of assaying silver.' Hitherto it had never been resorted to, except on a very small scale. Assayers appear to have shrunk from the manifest difficulties of manipulation in collecting, drying, and weighing the precipitated chloride of silver. The credit is due to Mr. James Dodd, a former Assay Master of the Calcutta Mint, of having so simplified, modified, and systematised the details of this method, as to render its application to the assaying of silver on a large scale easy and accurate. The principles, and an outline of the details of the process were then given, an understanding of some of the chief appliances and steps in the manipulations being assisted to by suitable photographs. The system of weights in use, and the quantity of the sample taken for assay, were also explained, as well as the points wherein this system might fairly be considered better suited to a Mint in India than the other methods.

In conclusion Dr. B. alluded to the vast amount of silver bullion which this process enabled the assay officers of the Indian Mints to deal with confidently and accurately during the past fifteen years. In one year alone, that of 1865-66, the importation of silver bullion reached to the immense amount of over fourteen millions sterling, so putting to a crucial test the system of assay used for its valuation.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 12.—"On Fluoride of Silver," Part II., by George Gore, F.R.S. An exhaustive account of the behaviour of argentic fluoride in vessels of platinum, carbon, and various fluorides in contact with chlorine, bromine, and iodine at various temperatures. When argentic fluoride is completely decomposed by chlorine in platinum vessels at a red heat, the reaction agrees with the following equation :—

$$4\,Ag\,F + 4\,Cl + Pt = 4\,A\,G\,Cl,\ Pt\,F_4.$$

Vessels of cryolite and of fluor-spar were found incable of retaining argentic fluoride in a melted state. Other vessels were also made by melting and casting various mixtures of earthy fluorides at a high temperature; and although forming beautiful products, probably capable of technical uses, they were not capable of retaining silver fluoride in a state of fusion. Numerous vessels were also made of seventeen different fluorides by moulding them in the state of clay and baking them at suitable temperatures; these also were found incapable of holding melted fluoride of silver. Argentic fluoride was only superficially decomposed by chlorine at 60° Fahr. during thirty-eight days. When heated to 230° Fahr. during fifteen days in a platinum vessel in chlorine, it was very little decomposed. Chloride of silver heated to fusion in a platinum vessel in chlorine corroded the vessel and formed a platinum-salt, as when fluoride of silver was employed. An aqueous solution of argentic fluoride agitated with chlorine evolved heat and set free oxygen, in accordance with the following equation :—

$$8\,Ag\,F + 8\,Cl + H_2O = 5\,Ag\,Cl + 3\,Ag\,Cl\,O + 8\,H\,F + O,$$

or

$$7\,Ag\,Cl + Ag\,Cl\,O_3 + 8\,H\,F + O.$$

Dry hydrochloric acid gas completely decomposed argentic fluoride in a melted state, but only acted upon it superficially at 60° Fahr. A saturated aqueous solution of argentic fluoride was not precipitated by chloric acid. Perfectly anhydrous fluoride of silver was only superficially decomposed by contact with bromine in a platinum vessel during thirty-six days at 60° Fahr., or during two days at 200° Fahr. At a low red heat in vessels of platinum argentic fluoride was completely decomposed by a current of bromine vapour, a portion of its fluorine being expelled and a portion corroding the platinum and forming an insoluble compound of fluoride of platinum and bromide of silver. In carbon boats at the same temperature the whole of the silver-salt was converted into bromide, the boat being corroded and the fluorine escaping in chemical union with the carbon. The action of bromine on an aqueous solution of argentic fluoride was similar to the action of chlorine. A solution of argentic fluoride yielded copious precipitates both with hydrobromic and bromic acids. Under the influence of a temperature of 200° to 600° Fahr. in closed platinum vessels, iodine very slowly and incompletely decomposes argentic fluoride without corroding the vessels, and produces a feeble compound of argentic iodide, fluorine, and iodine, from which the two latter substances are expelled at a red heat. At a red heat in platinum vessels, iodine produces argentic iodide, and in the presence of free argentic fluoride corrodes the vessels in consequence of formation of platinic fluoride; iodine and fluorine pass away together during the reaction. In vessels of carbon at the same temperature, argentic iodide is formed, the vessels are corroded, and a gaseous compound of fluorine and carbon is produced. By treating an aqueous solution of argentic fluoride with iodine, similar results are produced as with bromine and chlorine; a similar solution yields copious precipitates both with hydriodic and iodic acids. A mode of analysis of iodine is also fully described in the paper. A known weight of iodine was dissolved in absolute alcohol, a strong solution of argentic nitrate of known strength added to it in proportions at a time with stirring until the colour of iodine exactly disappeared. The mixture was evaporated, the free nitric acid expelled by careful heat, and the residue weighed. The residue was then heated to fusion to convert the iodate of silver into iodide, and again weighed. Two experiments of this kind yielded accurate results, and the process was easy and expeditious.

January 19.—"On the Structure and Development of the Skull of the Common Frog (*Rana temporaria*)," by W. Kitchen Parker, F.R.S. At the close of my last paper, "On the Skull of the Common Fowl," I spoke of bringing before the Royal Society another, treating of that of the osseous fish. I was working at the early conditions of the salmon's skull at the time. I was, however, led to devote my attention to another and more instructive type early in the following year; for it was then (January 1869) that Professor Huxley was engaged in preparing his very important paper "On the Representatives of the Malleus and Incus in the other Vertebrata" (see Zool. Proc. May 27, 1869). In repeating some of his observations for my own instruction, it occurred to me to renew some researches I had been making from time to time on the frog and toad. The results were so interesting to us both, that it was agreed for me to work exhaustively at the development of the frog's skull before finishing the paper on that of the salmon. On this account Professor Huxley mentions in his paper (*op. cit.* p. 406) that he leaves the Amphibia out of his demonstration, and that they are to be worked out by me. The amount of metamorphosis demonstrable in the chick whilst enclosed in the egg, suggested a much more definite series of changes in a low, slow-growing Amphibian type. I think that this has been fully borne out by what is shown in the present paper.

The first of the ten stages into which I have artificially divided my subjeet is the unhatched embryo, whilst its head and tail project only moderately beyond the yelk-mass. Another stage is obtained by taking young tadpoles on about the third day after they have escaped from their glairy envelope; a few days elapse between the second and third stages, but a much longer time between the third and fourth, for the fourth stage is the perfect tadpole, before the limbs appear and whilst it is essentially a fish with mixed Chimæroid and Myxinoid characters. Then the metamorphosing tadpole is followed until it is a complete and nimble frog, two stages of which are examined, and then old individuals are worked out, which give the culminating characters of the highest type of Amphibian.

The early stages were worked out principally from specimens hardened in a solution of chromic acid; and the rich umber-brown colour of these preparations made them especially fit for examination by reflected light.

Without going further into detail as to the mode of working my subject out, and without any lengthened account of the results obtained, I may state that the following conclusions have been arrived at, namely, that the skull of the adult is highly compound, being composed of :—

1st. Its own proper membranous sac;

2nd. Of a posterior part which is a continuation, in an unsegmented form, of the vertebral column;

3rd. Of laminæ which grow upwards from the first pair of facial arches, and which enclose the fore part of the membranous sac, just as the "investing mass" of the cranial part of the notochord invests the hinder part;

3rd. The ear-sacs and the olfactory labyrinth become inextricably combined with the outer case of the brain.

And 5th. The subcutaneous tissue of the scalp becomes ossified in certain definite patches; these are the cranial roof-bones. Around the mouth there are cartilages like those of the Lamprey and the Chimera; but these yield in interest to the proper facial bars, which are as follows, namely:—

First pair the "trabeculæ."

Second pair the mandibular arch.

Third pair the hyoid arch.

And fourth to seven pairs; these are the branchials.

These are all originally separate pairs of cartilaginous rods; and from these are developed all the complex structures of the mouth, palate, face, and throat. The pterygo-palatine arcade is merely a secondary connecting bar developed, after some time, between the first and second arches.

Meckel's cartilage arises as a segmentary bud from the lower part of the second; and the "stylo-cerato-hyal," as a similar secondary segment, from the third arch.

By far the greater part of the cranium (its anterior two-thirds) is developed by out-growing laminæ from the trabeculæ, which after a time become fused with the posterior or vertebral part of the skull.

When the tadpole is becoming a frog, the hyoid arch undergoes a truly wonderful amount of metarmophosis.

The upper part, answering to the hyomandibular of the fish (not to the whole of it, but to its upper half), becomes the "incus," and a detached segment becomes the "orbiculare," which wedges itself between the incus and the "stapes." The stapes is a "bung" cut out of the "ear-sac." The stylo-cerato-hyal is set free, rises higher and higher, and then articulates with the "opisthotic" region of the ear-sac; in the toad it coalesces therewith, as in the mammal. The lower part of the hyomandibular coalesces with the back of the pier of the mandibular arch; and the "symplectic" of the osseous fish appears whilst the tadpole is acquiring its limbs and its lungs, and then melts back again into the arch in front; it is represented, however, in the Bull-frog, but not in the common species, by a distinct bone.

This very rough and imperfect abstract must serve at present to indicate what has been seen and worked out in this most instructive vertebrate.

"Modification of Wheatstone's Bridge to find the Resistance of a Galvanometer Coil from a Single Deflection of its own Needle." By Professor Sir W. Thomson, F.R.S. In any useful arrangement in which a galvanometer and a galvanic element or battery are connected through whatever trains or network of conductors, let the galvanometer and battery be interchanged. Another arrangement is obtained which will probably be useful for a very different, although reciprocally related, object. Hence, as soon as I learned from Mr. Mance his admirable method of measuring the internal resistance of a galvanic element (that described in the first of his two preceding papers), it occurred to me that the reciprocal arrangement would afford a means of finding the resistance of a galvanometer coil, from a single deflection of its own needle by a galvanic element of unknown resistance. The resulting method proves to be of such extreme simplicity that it would be incredible that it had not occurred to anyone before, were it not that I fail to find any trace of it published in books or papers, and that personal inquiries of the best informed electricians of this country have shown that in this country at least it is a novelty. It consists simply in making the galvanometer coil one of the four conductors of a Wheatstone's bridge, and adjusting, as usual, to get the zero of current when the bridge contact is made; with only this change of plans, that the test of zero is not by a galvanometer in the bridge itself showing no deflection; but by the galvanometer, the resistance of whose coil is to be measured, showing an unchanged deflection. Neither diagram nor further explanation is necessary to make this understood to anyone who knows Wheatstone's bridge.

Chemical Society, January 19.—Professor Odling, F.R.S., Vice President, in the chair. The following gentlemen were elected fellows:—R. Bannister, H. T. Brown, J. Moss, R. J. Moss, E. Potts. The following papers were read:—"On the action of sulphuric acid on the natural alkaloids," by Henry E. Armstrong. On heating narcotine with sulphuric acid, which had previously been diluted by its own volume of water over the water-bath, and subsequent addition of ammonia to the mixture, a body is obtained which shows at once the properties of dimethylnarcotine, the base which Matthiessen and Wright had obtained from narcotine by means of hydrochloric acid. The reaction takes place, therefore, according to the equation—

$$C_{22}H_{23}NO_7 + H_2SO_4 = C_{21}H_{21}NO_7 + CH_3HSO_4.$$

From this result the author concludes it becomes evident that Gerhardt and Laurent's view, who regarded this body as an amide, must be abandoned. On treating codeine in a similar manner, and dissolving the base obtained in hydrochloric acid, a crystalline hydrochlorate was obtained. An analysis of the product showed it to be hydrochlorate of codeine. The first action of sulphuric acid results, therefore, in the production of an isomeric codeine. By the further action of sulphuric acid, 1 molecule of water is removed from 2 of codeine—then 1 H_2O from 1 codeine, and finally, apomorphine seems to be formed. On this last point, however, further evidence has yet to be awaited. "On the origin of nitrates in potable waters," by Ch. Ekin.—The author found nitric acid in the water of a spring which is very remote from any agency that could impart to it decaying animal matter. On closer examination, he found that the water in question had passed through a fossiliferous stratum. This observation necessitates a modification of the "previous sewage contamination theory."—"On an alkaloid from Cinchona bark, hitherto undescribed," by Dr. Howard. This new alkaloid was obtained from the mother liquors of quinine salts. It is a yellowish oil which cannot be sufficiently purified for analytical purposes. The formula of its platino-chloride corresponds with the formula assigned by Gerhardt to the anhydrous platino-chloride of quinine. Mr. Macleod exhibited an ingenious little contrivance by means of which eudiometer tubes which have lost the outer portion of their discharging wires may yet be made use of.

Mathematical Society, January 12.—Mr. W. Spottiswoode, F.R.S., President, in the chair. The Rev. J. Wolstenholme, M.A., Christ's College, and Mr. R. B. Hayward, M.A., late Fellow of St. John's College, Cambridge, were proposed for election. There was a large attendance of members to hear Prof. Peirce, of Harvard, give an account of the methods made use of in his "Linear Associative Algebra." The President conveyed to the author the thanks of the society for his interesting communication. Other papers communicated were "On Systems of Tangents to place Cubic and Quartic Curves," by Mr. J. J. Walker, M.A., and "On the Order and Singularities of the parallel of an Algebraical Curve," by Mr. S. Roberts, M.A. In the course of the evening Mr. Roberts stated the following construction as being mechanically more convenient than one discussed in a former paper "On the Pedals of Conics:"—In a plane, if a limited straight line, whose length is equal to the distance between the centre of two equal circles, moves with an extremity on each, the locus of any point rigidly connected with the line will consist of a circle, and a bi-circular quartic with a third node.

Entomological Society, January 23.—At the Annual Meeting this day held, Mr. A. R. Wallace, President, in the chair, the following gentlemen are elected to form the Council for 1871.—Messrs. Butler, Dunning, Fry, Grut, Higgins, M'Lachlan, Major Parry, Pascoe, E. Saunders, Stainton, S. Stevens, A. R. Wallace, and Professor Westwood. The following officers for 1871 were subsequently elected:—President, Mr. A. R. Wallace; Treasurer, Mr. S. Stevens; Secretaries, Messrs. M'Lachlan and Grut. Librarian, Mr. Janson. An Address was read by the President, which will be published *in extenso* in the Society's Proceedings.

Ethnological Society, January 24.—Professor Huxley, President, in the chair. The Rev. Dr. Steere read a paper "On the Tribes and Languages of East Africa." The author, who had resided for several years at Zanzibar, described in detail the Swahilis, a mixed race, half negro and half Arab, belonging to the Shafi school of Mahommedans. Many examples of their folk-lore were introduced, and a detailed description was given of the Swahili language. A comparison was instituted between the Swahili, Shambala, Yao, and Nyamwezi languages, which all belong to the great Bântu family. Mr. Hyde Clarke spoke upon this communication.—A paper was read "On the weapons and implements used by the Kaffir Tribes and Bushmen of South Africa," by Dr. Carl L. Griesbach. The author described the primitive method of iron-smelting practised by the Kaffirs, and alluded to the knowledge of certain mixed metals, such as brass, possessed by some of the northern tribes. A description was also given of the native method of gold-washing, carried on in some of the tributaries to the Zambesi. The degraded state of the Bushmen was referred to, and it was remarked that although they are ignorant of iron-working, they yet possess some artistic taste. Among the South African implements attention was directed to the musical instruments, which the author considered to have been derived from the Arabs. Dr. Theophilus Hahn made some philological remarks upon this paper, and gave some illustrations of the Hottentot clicks. The President announced that this was the last meeting of the Ethnological Society as a distinct body, and read the terms of union whereby an amalgamation had been effected between the Ethnological and Anthropological Societies of London, under the common designation of "The Anthropological Institute of Great Britain and Ireland."

EDINBURGH

Royal Physical Society, January 25.—Mr. C. W. Peach, president, in the chair. The secretary exhibited a beautiful specimen of the snowy owl *Strix nyctea*, shot near Baltasound, Shetland, on the 24th of December. It was a female, and the remains of a dunin and a jack snipe were both found in its stomach. The facial disc and legs of the bird were pure white, the rest of the bird was whitish, barred all over with rich brown. Another specimen was seen in the same locality, probably the male bird. A curious specimen of a young rook was exhibited. It was of a uniform dull brown or ash colour, instead of the usual black colour, the bill being lightest coloured, and it was feathered down to the base of the bill.—Mr. G. F. Barbour, of Bonskeid, exhibited to the society a fine specimen of the spotted rail, *Crea porzana*. It was shot on the lands of Preston, near Linlithgow, in a bog on the hillside.—Note on the Nesting of the Kingfisher, *Alcedo ispida*, by Prof. Duns The opening to the nest was twenty-two inches below the surface of the bank, and a little more than four feet above the water. The entrance to the nest was by a rounded passage, two inches wide at the front, increasing a little in size as it reached the oval chamber, or nest proper, and being only twenty-one inches long. The chamber was four inches broad, six long, and four high. After looking at the notices of the kingfisher's nest in the literature of ornithology, I find that the specimen before us sheds light on the following moot points:—1. The passage did not slope upwards, but was horizontal, the bottom being about half an inch below the bottom of the nest. "Instinct," says Montagu, "has taught them to have the entrance to their habitation ascending, by which means the filthy matter runs off." The matter referred to is the thin, watery fœces of the young birds, which soon becomes fetid. In this case the end indicated would be partially gained by the greater thickness of the small bones laid down in the passage than in the nest—the passage being thus brought to the level of the nest, and an imperfect kind of drainage supplied, by which, for a time, the watery fouling would be taken from the surface. 2. The hole was not the old hole of a water rat. 3. There were no traces of withered leaves or grass or feathers in the nest. The bottom was covered with the bones of minnows. The nest proper was perfectly dry, though the passage, especially at the edge of the nest, was wet and fetid. As the bones when disgorged must have been wet, it would appear that the pellets must have been scattered by the birds and left to dry before the eggs were dropped. 4. It is evident from this specimen that the bones of small fishes are as truly the lining of the nest as feathers are of the nests of many other birds.—"Note on the Plaice," *Platessa Vulgaris*, by Prof. Duns.—"Note on *Lithodes Maia*, fem.," by Prof. Duns. The specimen was taken at Elie, Fifeshire, in December last. It is a female. When received it was loaded with spawn, attached to branching tubes, situated beneath the abdominal plates; the size and arrangement of the abdominal plates, the presence and state of the ova, and the light shed by this specimen on the spawning time, of which Bell and others say they know nothing, deserve to be noted. *Lithodes Maia*, though occurring in the Firth, is no doubt one of our rarer crabs.—"Note on *Galathea strigosa*," by Prof. Duns, New College.—Mr. C. W. Peach exhibited *Antholithes* and its fruit (*Cardiocarpon*) with specimens of *Halonia*, *Flabellaria*, and other fossil plants, from the Coalfield near Falkirk.—C. W. Peach exhibited a large collection of fossil plants from the coal at the Cleuch No. 1 pit, and the brickwork near Falkirk, last summer. Amongst them was a series of *Antholithes Pitcairniæ*, some with its fruit, *Cardiocarpon*, attached, this being the first instance of the kind at present known. He stated that *Calamites*, associated with magnificent fronds of *Flabellaria Corassifolia*, were abundant. *Lepidodendron*, *Halonia*, *Ulodendron*, &c. &c., were much rarer; altogether, they showed that the flora of the coal period of Scotland was varied and of great beauty. He added that they were more interesting from the fact that several of them were generically and specifically identical with plants described in his "Acadian Geology," by Principal Dawson, found in the coalfields of Canada and America, even to the minute shells of *Spirorbis* still adhering to the fronds of *Flabellaria*.—"Notice of the Discovery of a new locality, near Edinburgh, of the Lower Carboniferous rocks, having fossils equivalent to the Burdiehouse and Wardie Series," by Mr. D. Grieve. Mr. Grieve read a notice of a new fossiliferous deposit discovered by him in certain shales and sandstones at Lochend, near Edinburgh, and which are situated on the east side of the loch. Mr. Grieve was led to make a search in this quarter from an indication given by Mr. Geikie, ten years ago, in his "Geology of the Neighbourhood of Edinburgh," that a continuity of the shales on the north side of the Calton Hill would likely be found between that place and Lochend, and which indication he had now verified. Mr. Grieve described the shales as belonging to the Lower Carboniferous formation, and as being equivalents of the sandstones and shales of Burdiehouse, Wardie, and Granton. He obtained *Calamites* of larger size and better marked than those found in the other localities stated as being abundant in the sandstone; *Lepidodendra*—a *Lepidophyllum*, *Sphenopteri*, &c. Of fishes he had obtained a beautiful specimen of the genus *Palæoniscus*, also scales, teeth, and spines, besides coprolites, which are abundant; also numerous specimens of a small crustacean, identical with *Cypris Scoto Burdigalense*, or of an allied species.

GLASGOW

Geological Society, January 5.—Mr. E. A. Wunsch, V.P., in the chair. *Carboniferous Fossils*.—Mr. James Thomson read a paper on the occurrence of *Cœlacanthus lepturus* at Newarthill, and *Palæoniscus Wardii* at Possil. He briefly described the scales, fin-rays, and head-plates of *Cœlacanthus* which had been found in a detached form in the neighbouring coal measures, and which the examination of a nearly entire specimen from the Staffordshire coal-field had now enabled him to identify. It occurs in the upper members of the Carboniferous system in Scotland, in a shale overlying the ironstone of the Airdie coal-field. Both with regard to this and the other ichthyolite—the *Palæoniscus*—before them, he remarked that he had had these forms for years in his cabinet, unnamed; and it was only recently that *Palæoniscus Wardii* had been described, and named specifically after its discoverer in the Staffordshire coal-field, Mr. John Ward of Longtown. It is found in the Possil black-band ironstone, which is between four and five hundred fathoms below the position in which it occurs in Staffordshire, thus not only adding another form to the fauna of our Scottish coal-fields, but adding also to our knowledge of its range in time. The lower beds of the Ayrshire coal-field had also yielded some specimens of this fossil. Mr. Thomson then exhibited specimens of *Rhizodopsis sauroides*, *Amphycentrum granulosum*, and *Platysomus parvulus*, from the Staffordshire coal-field, observing that the scales of *Rhizodopsis* had been found in our Scottish coalbeds, but as yet no complete specimen of the fossil had thence been obtained. Mr. Thomson also exhibited specimens of *Oldhamia* from Bray Head, near Dublin. He described minutely the position of the beds in which these fossils are found, and complained that geological references are frequently so vague as to be of little real service to one going over the ground for himself. Two species of this fossil had been discovered, *O. antiqua* and *O. radiata*; and they were generally believed to have been zoophytes allied to the *Sertularia*. Their precise nature, however, is still matter of discussion. They possessed a special interest as being, with the exception now of *Eozoön Canadense*, the oldest distinct traces that had been found of life on the globe. Mr. Thomson further called attention to the wide unconformability presented by the Mountain Limestone near Dublin, resting, as it does, upon the Cambrian rocks on the north, and upon the granite on the south, side of the bay.

PHILADELPHIA

Academy of Natural Sciences, October 11, 1870.—Dr. Ruschenberger, president, in the chair. Mr. Thomas Meehan said he had noticed a singular habit in the common "Stink bug" of gardens, *Reduvius novenarius*, Say, which might lead to some important physiological discoveries by those more closely devoted to entomological studies. Wondering what made some abrasion on the bark of a *Pinus cembra* on his grounds, he was attracted by a female insect of this species near it; and noticed that on the thigh of the middle leg the usual grey colour was of a polished black. Supposing that possibly the insect may have had something to do with the injury to the bark, through which the turpentine was oozing, he waited a few minutes to re-assure the insect—usually timid under observation—that there was no danger. It then went to work to take the turpentine with the heel of the tarsus of the fore leg, and place it on the thigh of the second leg. It took several dozen "heelsful," winding it round the gathering ball on the leg, as one would wind a ball of string. After it had collected together a ball of turpentine about the size of a pin's head, it gently wiped it off with the femora of the hind leg, and applied it to the anus, where it was very rapidly absorbed. It then walked very leisurely to the top of the nearest branch, when it flew away. This was in the end of September. He saw no more of these insects till a week afterwards, when he cut off a small branch on which was another female, and carried it to the pine tree, applying the branch to the stem so that the insect could walk on to it, without much suspicion of human agency in the matter. As soon as it got to the turpentine, it went through the same operation as the other one, taking two doses of it before it walked away; which it did leisurely, and with much apparent satisfaction. Up to this time he had not been able to find a male, so as to ascertain if it also had any similar use for turpentine.

Oct. 25.—"On the Stipules of Magnolia and Liriodendron," by Thomas Meehan. An examination of the stipules of *Magnolia* affords some highly interesting facts; most, or perhaps all of which are known to leading botanists, but which do not appear to be as generally known as they deserve to be; and which facts may have a more intimate bearing on many of the questions connected with the laws of development than is suspected. On the upper point of the scar next the leaf blade are two small articulation points, where the membranaceous stipules finally parted from the leaf. Examining a leaf before these stipules have fallen, the main veins forming the skeleton of the stipules are found connecting with these articuli, and spreading out, diverge downward toward the base of the leaf. I suppose no one of experience in living plants doubts the possibility of the adhesion of some parts and the separation of others, so as to make new parts or organs. If such is desired, I would refer to the *adhesion* of the carpellary leaves by their backs in the capsules of *Staphylia trifolia*; and for *separation* to the pinnate leaf often formed out of an entire blade in *Fraxinus excelsior, heterophylla*, and many other plants with entire leaves which often have pinnate ones amongst them. The author stated his opinion that the stipules of *Magnolia* are not formed like the stipules of most plants, which are perhaps leaf portions which have never been well developed, but rather are the tolerably well developed side pinnules of a trifoliate or deeply auricled leaf, which in an early stage had adnated with the petiole, and by their edges, and thus formed the stipular sheath we see. This ternate division of the leaf is a marked character in Ranunculaceæ, and with this exposition of a ternate type in Magnoliaceæ, its claim to a place in the Ranal alliance, strong as it always has been acknowledged to be, is still more strengthened. It is impossible to suppose that a so closely allied genus as *Liriodendron* should be founded on a different type from *Magnolia*. We shall see that only very slight causes, which we can well understand, have made some of the chief foliar distinctions, and the few which we cannot prove from actual facts, can be made almost certainties from parallel observations. The identity of type will in this way be manifest. There seems to be every evidence short of an actual witnessing of the fact, that the petiole in *Liriodendron* became adnate with the stem, and in this way the two lateral sections (stipules) were brought in contact with the stem with which they united. This would bring them nearer the sources of nutrition, and enable them to assume a more leaf-like and permanent character than if on the petiole. They become rather primary than secondary leaf organs, and this is just what we see them to be. Thus we may assume that *Magnolia* has typically a ternate leaf structure; that the stipules are the two lateral lobes which, by a peculiar process of adnation, became stipular sheaths after having been partially organised as leaf blade; and that *Liriodendron* differs from *Magnolia* only in possessing a greater power of adnation.

BOOKS RECEIVED

ENGLISH.—A Dictionary of Science: G. F. Rodwell, new edition (E. Moxon and Co.).—The Earth, vols. 1 and 2: E. Réclus (Chapman and Hall). —Dr. Bevan on the Honey-bee, new and enlarged edition: W. A. Munn (J. Van Voorst).

AMERICAN.—Theoretical Chemistry, part 1: G. F. Barker, M.D. (C. C. Hatfield, New Haven.)

DIARY

THURSDAY, FEBRUARY 2.

ROYAL SOCIETY, at 8.30.—On Linear Differential Equations: W. H. L. Russell, F.R.S.—Measurement of Specific Inductive Capacity of Dielectrics: J. C. Gibson and T. Barclay.—On the Uniform Flow of a Liquid: Rev. Canon Mosley, F.R.S.
SOCIETY OF ANTIQUARIES, at 8.30.—On Charters relating to Robertsbridge: C. S. Perceval, LL.D., Dir. S.A.
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.
LONDON INSTITUTION, at 7.30.—On the Action, Nature, and Detection of Poisons: F. S. Barff.
ROYAL INSTITUTION, at 3.—Davy's Discoveries: Dr. Odling.

FRIDAY, FEBRUARY 3

GEOLOGISTS' ASSOCIATION, at 7.30.—Anniversary Meeting.
ROYAL INSTITUTION, at 9.—Polarisation of Light: W. Spottiswoode, F.R.S.
ARCHÆOLOGICAL INSTITUTION, at 4.

SATURDAY, FEBRUARY 4.

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

SUNDAY, FEBRUARY 5.

SUNDAY LECTURE SOCIETY, at 3.30.—The Origin, Migrations, and Development of Remarkable Parasites: Dr. Cobbold, F.R.S.

MONDAY, FEBRUARY 6.

ENTOMOLOGICAL SOCIETY, at 7.—On the Early Development of the Sexual Organs in Insects, and its bearing on the Origin of Species: Mr. Lowne.
VICTORIA INSTITUTE, at 8.—Evidence of the Egyptian Monuments to the Sojourn of Israel in Egypt: Rev. B. W. Savile.
LONDON INSTITUTION, at 4.—On the First Principles of Biology: Prof. Huxley. (Educational Course.)
ROYAL INSTITUTION, at 2.—General Monthly Meeting.

TUESDAY, FEBRUARY 7.

ZOOLOGICAL SOCIETY, at 9.—Notes on some points in the Osteology of *Rhea Americana* and *Rhea Darwinii*: Dr. R. O. Cunningham.—On the Arctic collection of Birds presented by Mr. John Barrow to the University Museum, Oxford: J. E. Harting.
ROYAL INSTITUTION, at 3.—Nutrition: Dr. Foster.

WEDNESDAY, FEBRUARY 8.

GEOLOGICAL SOCIETY, at 8.
ROYAL MICROSCOPICAL SOCIETY, at 8.—Anniversary Meeting. Election of Officers and Council.
SOCIETY OF ARTS, at 8.—On Ornamentation considered as High Art: Dr. C. Dresser.
ARCHÆOLOGICAL ASSOCIATION, at 8.

THURSDAY, FEBRUARY 9.

ROYAL SOCIETY, at 8.30.
SOCIETY OF ANTIQUARIES, at 8.30.
LONDON MATHEMATICAL SOCIETY, at 8.
LONDON INSTITUTION, at 7.30.—On the Action, Nature, and Detection of Poisons: F. S. Barff, M A., F.C.S.
ROYAL INSTITUTION, at 3—Davy's Discoveries: Dr. Odling.

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