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THURSDAY, JANUARY 25, 1872

THE SOLAR ECLIPSE

ACCOUNT OF OBSERVATIONS MADE AT POODOCOTTAH

THE spectral observations of recent total eclipses of the sun had plainly demonstrated the existence of an incandescent gaseous stratum or atmosphere, surmounting the chromosphere or stratum of hydrogen which envelops the body of the sun, but they had not sufficed to determine its true conformation and extent. This question, therefore, constituted one of the principal problems remaining to be solved by observations of the eclipse of the 12th of December, 1871.

The slit-spectroscope applied to large telescopes doubtless affords the best means of verifying the existence, in the circumsolar regions, of this gaseous stratum, which may be termed the superior chromosphere, and of determining the materials of which it is composed; but from the shortness of the time available in an eclipse, the spectroscope can furnish only partial and local results, insufficient, therefore, to reveal the true structure, form, and dimensions of this upper chromosphere.

Preceding observations having shown that the light of the solar corona is composed for the most part of a small number of elementary rays differing considerably in refrangibility, it appeared to me that the form and dimensions of the higher chromosphere might be much more conveniently studied by means of a large prism fixed in front of the object-glass of the telescope, whereby the several chromatic images of the corona would be distinctly formed in the focal plane. If the prism has but little dispersive power, and the eye-piece does not magnify too much, all the chromatic images of the corona may in this manner be observed simultaneously in the same field, and their form and dimensions directly investigated.

Towards the end of the year 1868, a small flint-glass prism was made for me by Signor Merz, of Monaco, to be fitted to the object-glass of the equatorial belonging to the Observatory of Campodoglio, for observations on the spectra of the stars; and this apparatus, in consequence of the dispersion of the prism, and the goodness of this prism and of the object-glass, was found to be admirably adapted for observing the eclipse in the manner just described.

The dispersion of the prism from the lines C to H of Fraunhofer is about 32′; the free aperture of the object-glass is $4\frac{1}{3}$ French inches; the field of the telescope is about 1°, with a magnifying power of 40.

My conviction of the great advantages which would be afforded by this instrument in the observation of the approaching eclipse, induced me to carry it to India for that purpose; and I was glad to learn that Mr. Lockyer, the chief of the expedition, had in like manner resolved to observe the corona by means of a spectroscope without a slit, being persuaded that this would be the most convenient method of solving the questions relating to the corona itself. With this instrument, then, I prepared to observe the eclipse, proposing to myself the following problems:—

1. To ascertain whether, just before the beginning, and at the end of totality, the solar spectral lines are reversed —a phenomenon observed by Prof. Young in the eclipse of 1870.

2. Amidst the several chromatic images of the prominences, to observe especially whether the image given by the yellow line D^3 coincides with that of the lines of hydrogen gas.

3. To define the form and dimensions of the chromatic images of the corona.

The day before the eclipse, I delineated, by means of the direct-vision spectroscope applied to the telescope, the profile of the solar disc, in order to ascertain the state of the chromosphere at the several parts of the limb, and the protuberances existing there. But the picture did not come out with sufficient exactness, in consequence of the cloudy state of the sky, and the strong wind which prevailed throughout the day. This picture, however, clearly showed that both on the eastern and on the western limb, at the point where contact would take place between the lunar and solar discs in the total eclipse, the chromosphere was in that abnormal condition which is generally observed in the neighbourhood of solar spots.

The number of the prominences was, however, rather small, and their dimensions moderate; conditions which appeared to me to be favourable for the examination of the corona.

From the 5th to the 11th of December, the state of the sky at Poodocottah was somewhat variable; and generally, in the early hours of the day, great masses of mist and cloud predominated in the east, leaving but little hope in favour of our station for observing the eclipse. On the morning of the 12th, indeed, the sky was almost wholly covered with dense masses of mist and cloud, completely obscuring the sun till 7h. 53m., at which time the eclipse had already begun. Soon after this the sun was again covered with thick clouds, but fortunately they began to break a few minutes before totality, when the bright disc of the sun was already sufficiently reduced, and when consequently the time for observation was rapidly approaching.

To verify the phenomenon of the reversal of the spectral lines at the extreme edge of the sun, I had arranged the plane of dispersion at right angles to the edge at the point of second contact.

At thirty seconds before totality, the spectral image of the luminous crescent was already sufficiently weakened to allow of its observation by the naked eye without a dark glass; and it was then that the principal dark lines of the solar spectrum came out distinct, and even more strongly marked than before, and curved parallel to the bright edge of the sun; but a few seconds before totality these lines disappeared completely, and the spectrum became continuous, without however exhibiting, just before totality, the reversal of the lines, although I was watching most intently for this phenomenon. I would not, however, be understood as denying altogether the reversal of the lines, for it is not impossible that a thin film of mist, or the bright atmospheric light at that time diffused over the spectrum of the solar limb, may have concealed the bright lines.

At the very instant of totality, the field of the telescope exhibited a most astonishing spectacle. The chromosphere at the edge which was the last to be eclipsed—surmounted for a space of about 50° by two groups of prominences, one on the right the other on the left, of the point of contact—was reproduced in the four spectral lines, C, D^3, F and G, with extraordinary intensity of light and the most surprising contrast of the brightest colours, so that the four spectral images could be directly compared and their minutest differences easily made out.

In consequence of the achromatism of the object-glass, all these images were well defined, and projected in certain coloured zones, with the tints of the chromatic images of the corona. My attention was mainly directed to the comparison of the forms of the prominences on the four spectral lines, and I was able to determine that the fundamental form, the skeleton or trunk, and the principal branches, were faithfully reproduced or indicated in the images, their extent being, however, greatest in the red, and diminishing successively in the other colours down to the line G, on which the trunk alone was reproduced. In none of the prominences thus compared was I able to distinguish, in the yellow image D^3, parts or branches not contained in the red image C.

Meanwhile the coloured zones of the corona became continually more strongly marked, one in the red corresponding with the line C, another in the green, probably coinciding with the line 1474 of Kirchhoff's scale, and a third in the blue perhaps coinciding with F.

The green zone surrounding the disc of the moon was the brightest, the most uniform, and the best defined. The red zone was also very distinct and well defined, while the blue zone was faint and indistinct. The green zone was well defined at the summit, though less bright than at the base; its form was sensibly circular and its height about 6′ or 7′. The red zone exhibited the same form and approximately the same height as the green, but its light was weaker and less uniform. The height of the green zone was estimated by comparison with the moon's diameter, and from the observed distance of the spectral lines of the prominences.

These coloured zones shone out upon a faintly illuminated ground, without any marked trace of colour. If the corona contained rays of any other kind, their intensity must have been so feeble that they were merged in the general illumination of the field.

Soon after the middle of the total eclipse, there appeared on the eastern limb, at about 110° from the north point, a fine group of prominences formed of jets rather low but very bright, some rectilinear, others curved round the sun's limb, and exhibiting the intricate deviations and all the characters of prominences in the neighbourhood of solar spots. The brightness and colour of these jets were so vivid as to give them the appearance of fireworks.

The spaces between some of these jets were perfectly dark, so that the red zone of the corona appeared to be entirely wanting there. Perhaps, however, this was only an effect of contrast due to the extraordinary brightness of the neighbouring jets. I have thought it right to refer to this peculiarity, because the appearance of interstices, or dark spaces, between prominences of considerable brightness, is often observed by means of the spectroscope, independently of total eclipses.

The want of an assistant to note the time, and to write down the observations as they were made, occasioned me some loss of time, and the end of the total eclipse was already at hand before I was aware of it.

The green and red zones were well developed at the western as at the eastern limb, while the blue remained faint and ill-defined. Soon after the appearance of the chromosphere at the western edge, there was suddenly projected on the spectrum of the sun's limb, which then appeared beyond that of the moon, a stratum of bright lines, separated by dark spaces; but I could not determine whether they were due to a general or partial reversal of the spectral solar lines, or to a simple discontinuity in the spectrum, since they were too soon immersed in a flood of light, which put an end to the totality of the eclipse.

About half an hour after the total eclipse, the sun was obscured by clouds, so that I was unable to observe the end of the partial eclipse.

Later in the day, when the sky had become sufficiently clear, I observed with the spectroscope the state of the chromosphere, and of the protuberances existing upon it; but in consequence of the cloudy state of the sky, the violent wind which prevailed, and the shortness of the time at my command, the picture was not sufficiently distinct and detailed.

L. RESPIGHI

THE ZOOLOGICAL RECORD FOR 1870

The Zoological Record for 1870; being Vol. VII. of the "Record of Zoological Literature." Edited by Alfred Newton, F.R.S. (London: published by John Van Voorst, for the Zoological Record Association, 1871.) Pp. 523.

THE "Record of Zoological Literature" is already so well known to, and so well appreciated by, all students of zoology, that we need only remind our readers of the fact that, after five volumes had been published by Mr. Van Voorst, under the editorship of Dr. Günther, the publisher found it impossible to continue its publication, the actual yearly loss being something very considerable. It is true that the British Association for several years contributed 100*l.* towards this loss, and that three of the Recorders contributed, during the years that the British Association was so liberal, an equal sum out of their own pockets. Still, the expenses of such a work are so great, and the number of copies sold so small, that we were not surprised at Mr. Van Voorst's decision, nor to find that the present editor was compelled to look to the co-operation of zoologists generally to attain its continued publication; and it speaks much, not only for his energy, but also for the personal esteem with which he is regarded, that he could obtain in so short a time upwards of eighty friends who should guarantee 400*l.* between them towards any loss that might accrue on this, the seventh volume. While we do not pretend to be in the councils of the committee of the Zoological Record Association, nor have we received even so much as a hint on the subject from the secretary, yet we may venture to express our belief that the members, while they will have the consciousness of having furthered the publication of this work, will not have to pay very much more for the seventh volume than they had for each of the previous six.

Dr. Günther and M. E. von Martens are the only two of the original Recorders who take part in the production of this volume. Prof. Newton's section is taken by Messrs. Sharpe and Dresser; the Insecta are recorded by Messrs. Rye, Kirby, Verrall, M'Lachlan, and Scott; the Arachnida and Myriapoda are noticed by Mr. Cambridge; and the Worms and concluding orders by Mr. E. R. Lankester and Prof. Traquair. The editor stands up bravely in his preface for his staff, and we think he has a very good right to be proud of the work done by his assistants; though we somewhat fail to perceive "the new and perhaps improved modes of treatment" that he refers to.

In proceeding to offer a few friendly criticisms on this work, we would in the first place remark that both editor and Recorders deserve not only the thanks of the Association, but of all zoologists, for the excellent way in which they have accomplished their very difficult tasks, and that we trust that one and all of them will consider our comments as meant for suggestions, and not for fault-finding.

The two most novel features in the volume are "The List of Abbreviated Titles of Journals quoted," and "An Index to the Genera and Sub-genera Recorded as New." As to the List, until we looked over it, we confess that we had an idea that there was some law that guided one in abbreviating the title of a journal. The reader may, perchance, have looked over that corner of the journals of some of the Continental societies in which are recorded the various works sent to them in exchange; and if so he must have smiled to have seen the oftentimes funny attempts made to abbreviate the titles of the British societies. We promise him that, if ever he smiled on such occasions, he will smile still more when he just reads through the "concise forms of citation" given in the "Record," pp. 7—11; and he will, we think, exhaust his patience before he finds out on what principle these concise forms have been chosen. "Ibis" stands for "The Ibis;" while "J.F.O." stands for "Journal für Ornithologie." "P.L.S." stands, not for "Philip Lutley Sclater," as for a moment, in our innocence, we thought, but for "Journal of the Proceedings of the Linnean Society." While the "Journal of the Linnean Society" is very likely to be quoted in the future pages of the "Record," we fancy the "Proceedings" of the society—at least since 1867—will never more be referred to. Of course, any symbol might serve to indicate the journal of a society; but it is rather hard to compel a reader or a consulter of the "Record" to learn off some five pages of such before he can get along. The other novelty supplies a very great need, and one that we believe was often urged on the editor of the first series. The list of names of Genera and Sub-genera occupies in all but five pages, and we would suggest that a little additional space would, in future years, be well spent in indicating where, when, and by whom any of these names had been used before. In the present instance a symbol is affixed to some of the names, indicating that the name to which it is affixed has been used before. But the list has not been properly, or even very carefully, scrutinised for this purpose. On just reading it over, and without referring to such valuable indices as those published each year by the Zoological Society of London, or without pausing at names as familiar as household words to a botanist, we quote the following:—Argyritis, Hein.; Brachyleptus, Mots.; Cadmus, Theob.; Ceratophora, Hein.; Chelaria, Hein.; Dorvillia, Kent; Eucharia, Boisd.; Eurypus, Semp.; Euteles, Hein.; Gonia, Hein.; Helleria, Czern.; Lamprotes, Hein.; Lucina, Wlk.; Pephricus, Pasc.; Perideris, Fieb.; Plicatella, Sdt.; Pœcilia, Hein.; Psammobates, Günth.; Rhinosia, Hein.; Thysanodes, Ramb.; Trichocyclus, Günth.; Trinella, Gray; Zetobora, Wlk.; as names all in previous use, not to say that a query might well be affixed to such as Cephalobares, Camb., as being too near to Cephalobarus, Schönk; and if Ceratonia, Rond., is pronounced to resemble too closely Ceratomia, Harr., which, however, we do not quite see, then is there not greater danger of Euplecta, Semp., being confounded with Euplectus, Kirby? It is quite possible that some of these names may, though once used, have since fallen into disuse; and it is very probable that others in the list, unnoticed by us, may have been in use before. To be certain about this would take more time than is at our disposal; but we feel quite sure enough has been said to induce the editor to extend this valuable portion of the "Record," and to make it more exact in the next volume.

May we venture also to say that to certain zoologists who are in some measure ignorant of the mysteries of the Bird Regions, however important from an educational point of view the present arrangement of this part of the Record may be, it would be more generally useful if the titles of papers were all thrown into one series. This would at all events avoid the trouble of cross references, which savour too much of a library catalogue. When we come to the Mollusca, we find a novel practice which, as far as we can find, is not attempted among the Birds, and which we could not fancy being adopted by the Recorder of the other Vertebrates—viz., of not giving the pages on which the descriptions of new species are to be found. This is certainly a most mistaken economy of space, and very materially detracts from the value of these portions of the Record, for one great use of the Record is to enable one to quote an exact reference to a species the history of which one may be quite familiar with, and yet not have the volume containing that history at hand. There is, however, no uniformity in the matter in the present Record, and the Recorders that sin most in this result are those of the Mollusca, Crustacea, Arachnida, and among the Insecta, the Recorders of the Lepidoptera and of the Diptera.

We have been very much struck by the excellent way in which the Records of the Arachnida and Insecta have been executed, save that they too often quote from reprints. Mr. Cambridge and Mr. Rye's portions are quite models of such work. While we acknowledge the thoroughness of the work to be found in the Record of the Neuroptera and Orthoptera, we regret to see the criticisms on Mr. Walker's Catalogues, on p. 451. It is, we take it, not the province of a Recorder to indulge in such criticism, however well deserved it might be; and there are many who will remember how damaging such kind of remarks, made by a certain gentle entomologist, were to the Insect portion of Leuckart's "Bericht."

In his Record of the Vermes, Mr. Ray Lankester has neither done himself nor his subject justice. His mode of arrangement is novel and without precedent; but he has forgotten to give the number of pages to which each memoir extends, and, stranger still, he overlooks quoting the new genera or species described, and this notably in the case of the last work of the illustrious Claparède, and again in the case of Van Beneden's memoir, where we are told, simply enough, that "a number of new and little-known cestoid and other parasitic worms are described and figured." A whole page is taken up with a list of the Annelids referred to in a paper by Prof. Grube, but the list is quite useless, as it wants the remarks as to their synonymy.

Prof. Traquair's portion of the Record appears to have been very well executed. We wish he had given us the list of the Echinoderms from the Dutch East Indies, as described by Herklots. It would have been much more valuable than the list given of very common species from the East Frisian coast; and although we notice an omission of a paper or two among a group (the Cœlenterates) somewhat familiar to us, yet this portion of the volume leaves very little to be desired.

No one individual could write an exhaustive criticism on such a work as this Zoological Record. We have not even attempted it. The moment the volume reached us we cut its pages, and in noting its contents the remarks that we have now made recurred to us; but in addition to these there was also present to us the thought of how much we owed for the successful publication of this work to its accomplished editor and his well-qualified and trusty staff of friends. E. P. W.

OUR BOOK SHELF

I. *Earthquakes, Volcanoes, and Mountain-building*, three articles published in the "North American Review," 1869—1871. By J. D. Whitney. 8vo, pp. 107. (University Press, Cambridge, United States, 1871.)

II. *Historical Notes on the Earthquakes of New England*, 1638—1869. By William T. Brigham, A.M., A.A.S. 4to, pp. 28. (Boston, 1871.)

THE first of these works is a small volume containing three reviews, or essays, as they might be more correctly termed, reprinted from the "North American Review," and written by the well-known geologist Mr. Whitney, formerly director of the Geological Survey of California. They are well worthy of perusal, not only from the easy, somewhat popular style in which they are written, but more especially from their containing a tolerably fair summary of the opinions held by most of the later scientific writers who have treated of the phenomena of earthquakes, volcanoes, and mountain-building, as it is here termed, drawn up by one who is evidently well-read in the literature of these subjects.

To give in its turn a summary of the author's opinions as far as we are able to understand them from a perusal of these three essays, we might state, in the first place, that he lays considerable stress on the geographical data, which show that the area within which the greater earthquakes have been mainly confined is also to a great extent coincident with that of the greatest displays of active volcanic forces; and on the observations showing the action which the moon, or rather of the sun and moon combined, exert on the number and intensity of earthquakes, which, if accepted, indicate an internal condition of fluidity in our globe; he believes both in the chronological succession of volcanic rocks, and in their having proceeded from some common or connected source within the earth, but does not agree with those who regard the access of water as the great agent in volcanic cataclysms; disbelieving (in opposition to some elaborate calculations to the contrary) that the force capable of being developed by steam at such immensely high temperatures, could be sufficient to account for the phenomena of ejection; and although admitting the proximity of volcanoes in general to the sea, points out that some of those in South and North America are situated inland, several hundred miles distant from the ocean.

Regarding the differences in texture between the granitic rocks and those of recent volcanic origin as due mainly to the different conditions of our globe in the early periods in which they were erupted, Mr. Whitney protests against the hypothesis, so much brought forward of late, that the former are merely sedimentary deposits, brought within the action of, and softened or liquefied in, some unaccountable way by internal heat, and with respect to the origin of mountains, regards the external action of rain and rivers, now so all-absorbing in the minds of most English geologists, as altogether secondary to more powerful internal forces, believing, whilst mountain-building is to a great extent the result of an antagonism between subsiding and stationary masses of the earth's crust, that in all the great chains of mountains we have ample proof that this is at the same time accompanied by the intrusion of eruptive rocks from below, as a necessary consequence.

The second *brochure* by Mr. Brigham is reprinted from the memoirs of the Boston Society of Natural History; it appears to be the first part of a more lengthy communication to the Society, and is entitled "Volcanic Manifestations in New England;" it is an apparently exhaustive catalogue of all the principal earthquakes which have taken place, or rather been recorded, since the discovery and settlement of the country until the commencement of last year, bearing evidence of much industry, and appearing to be a valuable contribution to the records of American Seismology. D. F.

Astronomische Tafeln und Formeln. Herausgegeben von Dr. C. F. W. Peters, Assistant der Sternwarte in Altona. (Hamburg: W. Mauke, 1871; London: Williams and Norgate.)

A USEFUL collection of auxiliary astronomical tables compiled by the son of the well-known editor of the *Astronomische Nachrichten*. It brings under one cover many tables for which the computer has ordinarily to resort to different books; and in some cases the tables are exhibited in a more expanded form than that in which they are usually printed. It contains copious tables for converting time into arc, sidereal into solar time, hour and minute intervals into decimals of the day, refraction and hypsometric tables, tabular data referring to the figure of the earth, tables of squares and trigonometrical functions, and many others for facilitating the reduction of astronomical observations. It has also a collection of formulæ in common request, goniometrical, trigonometrical, and astronomical. The collection is based upon, and is in many respects closely similar to that made by Schumacher in 1822, and which was re-edited and enlarged by Warnstorff in 1845. Dr. Peters has, however, added many new tables, and modernised others where necessary. We could wish that a little more care had been bestowed upon the printing; the figures on some of the pages are very indistinct, and would tease a computer sorely. The defect is not accidental to a single impression of the work, for two copies have come before us, and in both the same pages are faulty. J. C.

LETTERS TO THE EDITOR

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

Zoological Statistics and the Hudson's Bay Company

AMONG the "Notes" in NATURE of December 28, there is one in which mention is made of the great dearth of martens imported into London this last season from Hudson's Bay, also of the death of 3,000 Indians from small-pox in the Saskachewan district. It is then added "that martens that are not killed, and Indians that die, mean reduced dividends to the Hudson's Bay shareholders and traders."

Having lived a good many years in the Hudson's Bay Territory, perhaps you will permit me to mention a curious circumstance which I noticed, in illustration that martens may abound yet comparatively very few be killed.

In all parts of the fur country east of the Rocky Mountains, where there is timber, hares (*Lepus americanus*), or "rabbits," as they are commonly, but wrongly, called, are found in greater or less numbers, and they congregate in certain favourite localities. The Indian pitches his tent near one of these places, and by setting snares (which his wife and children attend to), easily supplies himself and family with food, whilst the skins of the hares are worked up into most comfortable blankets.

The hunter all the while is trapping the marten and other fur-bearing animals that assemble to prey upon the poor rabbits, and is thus enabled to secure without much labour a large and valuable stock of furs, chiefly martens.

The hares are, however, liable to a very fatal epidemic,* which usually attacks them when they have become very numerous, and they gradually die off, so that in two or three years there is scarcely one to be seen. This scarcity continues for a couple of winters or so, after which the hares again begin to increase, so that at periods of eight or ten years they are at their maximum.

During this dearth of hares, the Indian has to go to a fishery, or is obliged to travel about in search of buffalo, deer, or other game as a means of support, and has little time for trapping the marten; and if he had the time, he would still be under great disadvantage, for the marten, lynx, and fisher have also to scatter themselves all over the country to pick up a precarious living on lemmings, partridges, and other odds and ends, instead of feasting in luxury and ease, as they do, on the hares when abundant. Thus, when hares are numerous, many marten skins are obtained, when hares are few marten skins are also few, not necessarily because martens are scarce, but that they are difficult to get.

The death of even 3,000 prairie Indians in one season, however injurious it might be to the trade of the Hudson's Bay Company in other kinds of furs, would not particularly affect the number of marten skins obtained.

I may here record a striking instance of the efficacy of vaccination as a preventative of small-pox. Nearly forty years ago this dreadful disease spread like a scourge from the Missouri river all over the prairies, being carried by bands of horse-stealers from one tribe to another; for these amiable "children of nature" no sooner heard of any of their neighbours being attacked by the terrible disorder, than parties went immediately to rob the sufferers of their most valuable property. They got the horses, but they also caught the disease, and many hundreds died. The Crees, a tribe of many thousands, having nearly all been vaccinated by the Hudson's Bay Company's officer in charge of the district, escaped with the loss of only two of their number.

JOHN RAE

* It is quite as fatal in its effects as the grouse disease, and the causes are little known The hares are found sitting in their forms dead. The Indians say they can tell when the disease is about to commence by a peculiar growth found in the abdomen.

Ripples and Waves

THE article by Sir William Thomson upon Ripples and Waves in the November part of NATURE, which has just reached me, reminds me of a little capillary wave, the examination of which used to be a source of amusement to me some years ago; and as I have never seen any description of it, my observations may not be without interest to some of your readers.

I had long noticed this little wave, winding about, like a hair upon the surface, amongst the eddies which formed in a deep river below a considerable fall, which I used to frequent; but I first got an insight into its nature in a very different situation. I was in a canoe in a sheltered bay, with just enough wind overhead, without any ripple on the water, to make my canoe drift broadside on at the rate of, perhaps, half-a-mile an hour, when I saw my little wave formed about three feet in advance of the canoe. Being in the neighbourhood of a marsh the water was very impure, and the behaviour of the little particles floating in it attracted my attention. Any objects reaching to the depth of from an eighth to a quarter of an inch below the surface passed on to the canoe unaffected by it; but smaller particles were suddenly agitated on passing the wave, and after getting a few inches within it, they were arrested at distances varying with their size, the larger ones penetrating farther than the smaller ones. If the wind died away the wave was maintained at a greater distance from the canoe, and it was still perceptible at a distance of fully eight or nine feet from it, after which it became fragmentary and disappeared. If the pace of the canoe increased, the wave came nearer to it, and the particles, which had been brought to rest at various intervals according to their sizes, were driven up together, forming at last a sort of scum in advance of the canoe. If the wind increased suddenly, the wave disappeared, and the slightest ripple on the surface obliterated it at once; but if the wind freshened very gradually, the wave approached nearer and nearer, becoming at the same time more strongly defined, until it came to within about nine inches from the canoe, and was maintained there under its lee, even after there was breeze enough to make a considerable ripple outside. If pressed beyond that, ripples of quite another character would form just in advance of the wave, and it would break up, and the canoe would pass over the scum which had collected within it.

With this clue as to its nature, I frequently examined the wave in the situations where I had first seen it. Wherever there was any impediment to the stream, as a tree stretching out into it from the bank, there was the little wave ahead of it, at distances from the impediment varying with the force of the current. In the spring, when the water was high, a good deal of foam would be brought down from the falls above, and would collect against these obstructions, but always leaving an inch or two of clear water within the wave. Upon clearing away the foam the wave would soon again be formed, and the next patch of foam which came down would experience a little jerk, as it passed the wave, and penetrate a few inches within it, when it would be arrested, and there would start out from underneath it little particles of sawdust, or other substances, which had been entangled in it, and would range themselves beyond it, in the order of their sizes. Presently more foam would come down, pushing on what had arrived before, till soon there would be an accumulation of it, as at first.

Where the wave was found winding about amongst the eddies there was no solid obstacle, but only one stream meeting another, and it was not at first sight easy to distinguish which was the front and which the back of the wave. The accumulation of scum, however, on one side showed this, and much more so the behaviour of the wave itself, according to the side from which you approached it. If you came down upon it with the stream, with your canoe broadside on, no effect was produced on the wave; but if you passed over it, it was almost immediately reformed on the other side. But if you approached it from the other side, you pushed it on before you; and by careful handling I have often succeeded in detaching a portion of the wave, and carrying it on before me for ten or fifteen yards; whilst after awhile another would be formed in the same place. Sometimes, where the water boiled up from below, there would be an irregular circular patch, surrounded by one of these waves, which you might drive up till the two sides met; or if you approached it stern on, you would cut the circular patch into two, in which case each would run up rapidly to their centre into a little conical jet, and if your pace was at all rapid there would be a drop projected upwards from it.

The wave is so minute that it was not easy to come to any conclusion as to its shape and size; but from the distorted reflection of an object held above it I satisfied myself that under ordinary circumstances it could not be more than one-twentieth of an inch high, the distortion not extending beyond half-an-inch on each side of the sharp cusp, and that it was convex towards the stream, with a very slight trace of concavity on the side of the obstacle generating it. It seemed as if the wave itself was a little elevated above the surface, and that it sloped back very slowly towards the obstacle. This is in accordance with the description above given of a narrowing circular patch running up to a jet; for, although the motion in that case was too rapid to permit of any precise observation, just before it closed in the patch had the appearance of a little table land elevated above the general surface. Upon one occasion, when a boom had been stretched across the river, running at the time fully five or six miles an hour, the wave was only about nine inches from the boom, against which a dense scum was collected, but still with about an inch of clear water between it and the wave. The wave in this case must have been fully an eighth of an inch high, and on its farther side were a succession of ripples, very much exceeding the capillary wave in height and amplitude, and differing from it in not being cusped, though otherwise imitating its general form.

It would appear, therefore, as if a wedge-shaped film of water were pushed ahead of the canoe, or other obstacle, the lower surface of which must, from the arrangement of the particles arrested, have been of rapidly-increasing curvature. Two difficulties, however, present themselves to this explanation—it is difficult to see how the film could have extended to the wave itself, as no particles, however small, appeared to be arrested within an inch or two of it; and my recollection is that upon the occasion of my first examining the wave driven before my canoe, light objects merely resting upon the water, like thistle down, seemed to be not at all affected by it, but to pass on towards the canoe unimpeded. Such objects, however, are so easily affected by the wind, or even the resistance of the air, that it was not easy to verify the observation.

Some other facts may be mentioned. The depth of the obstruction in the water seemed to have no sensible effect on the wave formed. Whether it was a log a foot through, or an inch board floating on the water, or whether it was the middle of the canoe drawing five or six inches, or the bow and stern barely touching the surface, the effect seemed almost the same. I have often, indeed generally, failed in my attempts to generate a wave with a canoe, and although upon the occasion when I first saw it so formed, I could trace it at fully eight feet from the canoe, I never found such a wave naturally formed at anything like that distance. The explanation appears to be that it requires very even and steady action to generate the wave; but that when once established it can be maintained under circumstances in which it would not be otherwise produced. As I stated before, if you approach it in one direction, you may take a canoe over it and it emerges on the other side unimpaired; the irregular currents of an eddy have no effect upon it except to give it an undulating motion, and I have seen it maintaining its place amongst the standing waves of a rapid when they have been several inches high. I have even raised considerable swells by rocking a canoe close to it, and it rides over them without disturbance; but the slightest ripple caused by the wind makes it disappear in a moment; and if spirits of turpentine be dropped on the water a little above it, the whole wave is instantly obliterated to a distance apparently far beyond that to which the oily film extends. JOHN LANGTON

Ottawa, Canada, Dec. 28, 1871

The Rigidity of the Earth

ALTHOUGH, as he truly says, Sir W. Thomson's arguments for the rigidity of the earth have never been attacked, yet they have undoubtedly been too long ignored; and it is gratifying to see them asserted by their author in NATURE. Allow me, however, to remark on one sentence near the end of his quotation from the "Natural Philosophy," where Mr. Hopkins's observation is given, that the distribution of fluid matter within the earth is "probably quite local." Unless I am mistaken, Mr. Hopkins's opinion was, that its distribution is, as one might say, fortuitous. But, as I have elsewhere observed, the trains of volcanoes which accompany many of the great lines of elevation for enormous distances render the motion of such local distribution of fluid matter highly improbable, unless it be admitted that its presence is due to mountain elevations as a cause. I have suggested that this fluidity may arise from a diminished pressure beneath mountain ranges, owing to their mass being partly supported by the lateral thrust which has upraised them—a supposition which Mr. Scrope had already applied to account for an increased fluidity in the heated rock underlying a volcanic vent, when from any cause the pressure became less.

If any of your correspondents can propose another explanation of this remarkable coincidence compatible with the supposition of a rigid globe, it would be interesting to know it.

Harlton, Cambridge O. FISHER

English Rainfall

IN reply to the letter of Mr. Vernon, in NATURE of the 18th inst., permit me to say that the confusion between the two Seathwaites is *his*, not *mine*. In the article to which he refers there is not a word about either Cockley Bridge or the Valley of the Duddon. His topographical knowledge of the districts is, apparently, as inexact as his manner of reading; for he does not seem aware that "the Stye," of which he speaks, is the name, not of a *place*, but of a *rain-gauge*, in, as I said before, the immediate neighbourhood of Stockley Bridge.

J. K. L.

Circumpolar Lands

IN the last number of NATURE (Jan. 18), Mr. J. J. Murphy asks, "Can any mathematical reason be assigned why the contraction of the earth should be least in the direction of the polar direction? This would account for the rising of the land at the poles."

In the Proceedings of the Literary and Philosophical Society of Liverpool for Nov., 1857, there is a paper on a probable change in the earth's form, in which the rising of the land at the poles is inferred as a necessary result of the cooling and contraction of the earth.

The following is the substance, though not the exact words, of a portion of the paper; the precise words would not be intelligible without a diagram.

If a spheroid of equilibrium, in motion about an axis, contract uniformly in the direction of lines perpendicular to its surface, a new spheroid is produced, having a greater degree of eccentricity, because if equal portions are taken off the two diameters, the ratio of the equatorial to the polar diameter is increased. This is equivalent to a heaping up of matter around the equator in excess of what is due to the velocity of rotation, an increased pressure on the interior, in that region, must be produced, and a consequent transmission of pressure towards the poles. "A change of form is then necessary to restore equilibrium. This may not take place uniformly *per gradum*, for if there be a resistance from a rigid external crust, the force must accumulate until it exceeds the resistance, and thus frequent adjustments *per saltum* may ensue. It is probable, therefore, that the earth's form is undergoing a slow progressive change."

GEORGE HAMILTON

Queen's College, Liverpool, Jan. 21

The Kiltorkan Fossils

MR. BAILY's letter needs only a word or two from me.

I must protest against my reference to an error made by Mr. Baily being considered a "personal attack" upon him, or an "accusation" against him. Has Mr. Baily ever consulted a systematic work which did not contain corrections of the real or supposed errors of former workers? And did he consider such corrections as "personal attacks"?

On two points Mr. Baily has misunderstood or misread the plain statements of my letter:—1. I did not say that his drawing in "Explanation of Sheets 187, &c.," was made on the spot at Kiltorkan, but that it was a drawing of the fossil he had named *Sagenaria Veltheimiana*; 2. The qualifying phrase, "coal measure," was used, as it often is, as the equivalent of "carboniferous." How Mr. Baily could make it mean anything else perplexes me; seeing the Upper Carboniferous beds have no connection with the question. To have used it in the limited sense he suggests, and elaborately argues against, would have been absurd.

The remainder of Mr. Baily's letter is occupied with reference to private letters as evidence in the case. That written by Mr. Baily to Prof. Heer confirms the statement I made at the Geological Society, and repeated in your pages; but, in as far as it declares that the specimens sent to Prof. Heer from Kiltorkan were named *S. Bailyana*, it differs from the statement made by Prof. Heer at the Geological Society, who, on the evidence of these fossils, included *S. Veltheimiana* among the Kiltorkan fossils, and never mentioned *S. Bailyana*!

The reference to the other private letters is equally unhappy; for Mr. Baily is quite wrong in supposing my "accusation" was made because I could not persuade him to join me in work. My letter, if he will look at it again, bears a date some time after the "accusation" was made. And if at the same time he will read his reply, he will find that the reason he gave for declining to work with me is somewhat different from those he records in your pages. But the fact is, the letters have nothing whatever to do with my declaration, now more than ever confirmed by Mr. Baily's letter, that his giving to the Irish Lepidodendroid plant the name of a carboniferous species misled Prof. Heer. If Mr. Baily's letter indicates the "facts" contained in his paper, I can only conclude that it was the patriotism of your reporter that induced him to characterise them as "strong." W. CARRUTHERS

Condurango

I HAVE read in No. 104 (October 26, 1871) of your scientific and highly-interesting journal, a few words on "Condurango," the new Ecuadorian plant that has lately called so much general attention in Europe and America to its supposed properties of curing cancer.

The want of exactitude in the description of the plant will doubtless give an erroneous idea of it to your readers, and with the desire of effacing such errors as those published in the "Andes" of Guayaquil, and in Bogota by Mr. Buyon, to whom you make reference, allow me to present to you and your readers the botanical description of the Condurango twining plant, very useful, indeed, in some rheumatisms and secondary syphilitic disorders, but of very doubtful medicinal properties in cancer, so far as my own experience goes.

The Condurango belongs to the order *Asclepiadaceæ*, 3rd tribe, which corresponds to *Aclepiadeæ veræ*; 1st division *Astephanus*, whose characters are that the limb of the corolla is without scales, and the stamens without appendage or corona.

This division comprehends only five genera, viz., *Mitostigma*, *Astephanus*, *Hæmax*, *Hemipogon*, and *Nantonia*. In none of these genera can the Condurango be classed.

The genus *Mitostigma*, as a distinguishing character, has two long filaments at the end of the stigma, and this is not the case in Condurango. The genus *Astephanus* has the sepals acute, the corolla subcampanulate, and the stigma elongated; characters that do not belong to the Condurango. The genus *Hæmax* has the divisions of the corolla hooded, and other characters not observed in the Condurango. The genus *Hemipogon* has the sepals of the calyx acute, hard, and with a curved extremity. The corolla is campanulate, which is not the case in Condurango. The genus *Nantonia* has the sepals striated and curved, which also is not the case in Condurango.

The flowers of the Condurango have a calyx of five divisions, obtuse, ovate, and villose in their inferior part, and of quincuncial præflorescence. The corolla is rotate, of five divisions, lanceolate, hairy at the base on the inside, and somewhat fleshy, with a membranous margin. Its æstivation is imbricated. The stamen has no appendage or corona; the anthers are terminated by a membrane, and the pollen-masses are elongated and suspended. The stigma is pentagonal and conical. The flowers are numerous, and disposed in umbelliferous inflorescence.

As aforesaid, the Condurango forms a new genus. It is absurd to speak of Condurango as if it were the same as *Mikania huaco*.

In the importance of the subject I hope to find ample apology for asking room in your columns for these few lines.

A. DESTRUGE

Guayaquil, Ecuador, Dec. 13, 1871

Ocean Currents

IT appears to me that the numerical data adduced by Mr. Croll in his letter (NATURE, Jan. 11) disprove his conclusions.

The doing of 9 foot-pounds of work upon a pound of water should give it a velocity (in feet per second) of

$$\sqrt{2 \times 32 \times 9} = 24;$$

and the doing of one foot-pound of work upon a pound of water should give it a velocity of eight feet per second. These are much greater than the observed velocities, so that a margin is left for friction.

The following passage in Mr. Croll's letter also calls for some remark:—"But it must be borne in mind that the deflecting power of rotation depends wholly on the rate at which the body is moving. If difference of specific gravity be regarded as the impelling cause of any current, the deflecting power of rotation will certainly be infinitesimal."

The deflecting force does indeed vary directly as the velocity of the body acted on; but the curvature of path which the deflecting force tends to produce, is proportional to the quotient of the deflecting force by the square of the velocity, and therefore varies inversely as the velocity. In latitude 45°, a velocity of a foot per second would give a radius of curvature of less than two miles. Here, then, again, there is a wide margin left for resistance. The expression for the radius of curvature in feet, supposing that there are no resistances, is

$$\frac{6850\, v}{\sin \lambda},$$

λ being the latitude, and v the velocity in feet per second.

Belfast, Jan. 13 J. D. EVERETT

Mock Sun

I THUS name the phenomenon I am about to describe, but without regard to scientific accuracy. Last evening, a little before sunset, I observed a dark bank of clouds couched on the horizon, just beneath the sun, and a long mass of cirro-stratus above him. A band of light, of about half his width, stretched up and down to the clouds. This remained visible, with remarkable changes, till 25min. after the sun's total immersion. On his disappearance the band gradually widened (or seemed to do so), and assumed the form of a table flower-vase, *i.e.*, bulged at the base and cylindrical above. At ten minutes after sundown the band, which had been about 10° in length, stretched to 20°, being superposed on the cirro-stratus, where it was rose-coloured, the bulged portion being orange. At twenty minutes after sundown a slight collapse occurred, and the band almost disappeared, the bulged portion becoming an orange disc, just like a second sun setting in fog. Soon afterwards this became elongated, and the band reappeared, stretching over an arc of 40°. A few minutes later all disappeared. I witnessed this beautiful phenomenon from a carriage on the L. and N.W. Railway, on both sides of Blisworth. C. M. INGLEBY

Edgbaston, Jan. 20

Solar Eruptions and Magnetic Storms

AT a recent meeting of the Astronomical Society a paper was read by Mr. Ranyard, in which some suggestions were put forward concerning the possibility of accounting for the solar prominences on the supposition that they may be caused by the projection of matter from a lower level, and that such an uprush into and through the layers above, emerging into the lighter envelope of the chromosphere, might lift before it a cone of compression of the gaseous matter, producing an elevation on the surface, visible to us as a prominence. And the solid particles or masses thus projected might form meteorites, the shape of the prominence being afterwards modified by other causes.

This theory, offering as it does a possible account of the genesis of prominences and meteorites, appears to contain the germ of another hypothesis respecting the cause of the connection between solar eruption and terrestrial magnetism.

If it be legitimate to suppose that in and near the photosphere we have a circuit of conducting matter (viz. incandescent metallic vapours), according to well-known facts any cause tending to effect an unequal distribution of heat, and at the same time a want of homogeneity of structure, such as a difference of pressure or density, would establish thermo-electric currents in such a circuit.

Now such a difference would arise from an upward burst of matter from below the photosphere. If, therefore, the prominences have their origin at great depths below the photosphere, we may expect currents of considerable intensity to circulate round the equatorial region of the sun. In the equatorial region rather than in any other, because it is there that the greatest disturbance is manifested, as shown by observations on the limits of spots and prominences; and, therefore, *there* that the necessary *differences* of temperature are most likely to occur, the effects of such currents being to create secondary or reduced currents in the adjacent layers, and, if of sufficient intensity, in the earth itself.

Provided that this be so, this supposition will suffice to reconcile some observed facts. Secchi has deduced,* in treating of the periodical variations of the magnetic elements, the law that "The annual disturbances are at a maximum at the equinoxes, and at a minimum at the solstices."

Knowing then that the plane of the sun's equator passes through the earth on June 11th and Dec. 12th, and that therefore the equator as seen from the earth presents its widest ellipse in March and September, it follows that such thermo-electric currents, if they exist, are able to exert their maximum inductive effect on the earth at or near the equinoxes.

The case is analogous to the experiment in which terrestrial magnetism is made to cause induced currents in a closed circuit rotated round an axis at right angles to the magnetic meridian.

In this case the ring is placed successively in positions variously inclined, but always keeps its plane perpendicular to the meridian, and the maximum induced current then occurs.

Similarly, solar equatorial currents would produce *their* maximum effect when the plane of the sun's equator has its aspect most nearly in the direction of the earth, and although any

* De La Rive's Electricity, tom. iii. p. 780.

variations in the intensity of these solar currents may be followed by a disturbance in the terrestrial magnetism at any time, yet such disturbance should be at a maximum at the equinoxes (as is the case by Secchi's law), because then the sun is most favourably situated for causing such effects.

In this hypothesis the source of the earth's permanent magnetism is not included, but simply the cause of the close connection between solar eruption and the disturbance of the terrestrial magnetic elements.

F. A. FLEMING

Mechanism of Flexion and Extension in Birds' Wings

UNDER the above heading in your issue of January 18, 1872, Dr. Elliot Coues describes the peculiar movements made by the bones of the wing of the bird in flexion and extension. It may interest some of your readers to know that those movements were minutely described and elaborately illustrated in a paper by Dr. J. Bell Pettigrew, communicated to the Linnean Society in June 1867, and published in vol. xxvi. of the Transactions of that body.

MILLEN COUGHTREY

Edinburgh University, Jan. 22

Elisée Reclus

A MEMORIAL addressed to the "Commission des grâces," sitting at Versailles, and most influentially signed by many of the leading scientific men in London, was presented at Versailles on the 3rd inst.

It is an appeal for commutation of sentence of deportation passed on Elisée Reclus, the well-known French geographer, author of "La Terre," an admirable popular work on physical geography (now being introduced as an English work* by Messrs. Chapman and Hall), and various other books.

A paragraph having appeared in several of the daily papers announcing that M. Reclus's sentence had been already commuted to simple banishment, I regret to state that he is still a prisoner at Versailles, although it is hoped the appeals made in his favour may produce the desired result.

The petition to the Commission in favour of Elisée Reclus was signed by the president of the Geological Society (Mr. Prestwich), Sir Charles Lyell, Bart., Mr. G. Poulett Scrope, Profs. Owen, Ramsay, Williamson, Duncan, Atkinson, Morris, Rupert Jones, Tennant, Messrs. Evans, Forbes, Gwyn Jeffreys, Drs. Carpenter, Richardson, and many others.

A second petition signed by Sir Henry Rawlinson, Sir John Lubbock, Bart., Mr. Darwin, and other men of eminence, was addressed to M. Thiers in favour of Elisée Reclus.

Surely the time for an amnesty has arrived.

British Museum, January 23

H. WOODWARD

* Sections I. and II. of this work are already published; Sections III. and IV. are now in the press.

NOTES ON MICROSCOPY

MOUNTING IN GLYCERINE.—It is often found desirable to mount very thin objects in glycerine, for which no special cell is requisite, and in which the thickness of a cell would be a disadvantage. To accomplish this was often a work of difficulty, since the presence of the smallest amount of glycerine outside the thin glass cover prevented the adhesion of the luting by means of which the cover was to be secured to the slide. Since the introduction of gum dammar dissolved in benzole to the attention of microscopists, this disadvantage has almost wholly vanished. It is now comparatively easy to mount such objects in the following manner. A small drop of glycerine, just enough for the purpose, is let fall in the centre of an ordinary cleaned slide, the object is then placed in the glycerine, having been previously soaked in benzole if any difficulty was likely to be experienced on account of contained air; a cover (say three quarters square) of thin glass is placed over the object and pressed down, taking care that the object remains in the centre; a wire clip then applied holds the cover in its place. If too much glycerine has been used, blotting-paper or a camel-hair pencil will remove all that issues beyond the edge of the cover. If too little, the addition of more at one edge will supply the deficiency, and the superfluous remainder may be wiped away. Thus secured by the clip the edges of the cover may be painted round with gum dammar in benzole, and when dry and firm (in a day or two) the clip may be removed, and the surface of the slide carefully washed to remove any trace of glycerine. The clip may be replaced, and a second thin coating of dammar laid over the first, or old gold size may be used instead. When this is dry "papering" the slide in the usual way helps to provide against accident. The advantages derived from the use of this method are chiefly the facility with which the cover is attached, notwithstanding the presence of a trace of glycerine on the slide and cover, which it is not easy to avoid; and, so far as the experience of two years can vouch, freedom from leakage afterwards, especially when covered with paper. This plan succeeds best with objects as thin as the minute spores of fungi, delicate hairs, &c., and a one-eighth objective may be employed in their examination.

THE ASCI IN PEZIZA.—Having left a specimen of *Peziza humosa* for a long time in water until it became quite soft and pulpy, I was curious to examine it in such condition, and found that the hymenium presented a singular appearance. All the paraphyses had become dissolved into a granular mass, retaining still some of their original colour. Amongst these the asci were free, and there were some free sporidia. In their normal condition the asci are cylindrical, and the sporidia are arranged in a single series, but in the present case the asci had become perfectly spherical, from the absence of all lateral pressure, and the sporidia were clustered in the centre. The line of the external surface of the asci was very distinct amongst the orange-tinted granular mass, and the eight sporidia could be counted within. There could be no doubt of the presence of an investing membrane, but of a much more elastic nature than has been supposed. This fact seems to suggest the probability that more or less lateral compression in the hymenium may influence the character of the asci, and that cylindrical, or clavate and elliptical asci, indicate more or less of lateral pressure during development.

SACRED THREAD.—The sacred thread, or at least one kind of thread held sacred to religious purposes by the Brahmins in India, is derived from the stem of a species of water lily—some say the *Nelumbium speciosum*, others *Nymphæa edulis*. At any rate under the microscope it exhibits a mass of spirals, unwinding in ribbons of four or five threads laterally united. There is no trace of cells mixed up with it, and the spiral threads are as clean as if they had been removed with special care for microscopical purposes.

HOP MOULD.—A new mould has made its appearance during the past autumn on the spent hops so common about Burton-on-Trent. It formed large dense patches of a bright salmon colour, sometimes several inches in length and breadth, upon the sombre hops, and could not have escaped notice had it appeared in previous years. The structure of this mould seems to be closely allied to that of *Oïdium*, whilst in many respects it reminds one of *Sporendonema casei*. The creeping mycelium gives rise to branched threads, which become divided into strings of oval conidia or spores. The mould refuses to develop itself artificially, so that the mode in which the beaded spores were produced was not absolutely determined. Directly the threads come in contact with fluid of any kind they are resolved into a mass of oval cells or spores. Specimens of this mould have been published and distributed in Cooke's "Fifth Century of British Fungi" under the name of *Oïdium aurantium*, a rather unfortunate specific name, since another member of the same genus which appeared nearly simultaneously on the Continent has been called *Oidium aurantiacum*.

M. C. C.

HUXLEY'S MANUAL OF THE ANATOMY OF VERTEBRATED ANIMALS*

THIS long-expected work will be cordially welcomed by all students and teachers of Comparative Anatomy, as a compendious, reliable, and, notwithstanding its small dimensions, most comprehensive guide in the subject of which it treats.

To praise or to criticise the work of so accomplished a master of his favourite science would be equally out of place. It is enough to say that it realises in a remarkable degree the anticipations which have been formed of it; and that it presents an extraordinary combination of wide, general views, with the clear, accurate, and succinct statement of a prodigous number of individual facts. The extreme brevity, indeed, takes one in some degree by surprise; and it is only on repeated reading that one feels assured that the facts exposed have been stated with sufficient fulness.

It is a wholesome and encouraging sign of the scientific literature and teaching of the day, that men of the highest eminence devote a portion of their time to the composition of elementary manuals or short guides in their respective sciences. The abuses to which such short manuals are subject are well known, and have been often commented on; and they are no doubt serious when leading to the formation of imperfect knowledge and the exclusion of more extended study. The objections, however, have weight chiefly as applied to the inferior class of such treatises, which, certainly, have too much abounded in this country. A thoroughly good manual, even though strictly elementary, besides forming the first secure basis of correct knowledge, excites a desire for fuller reading, and serves at later periods for useful revisal of more complete information; while its small size obviously places it within the reach of many whose means do not enable them to become possessed of larger treatises, and has thus considerable influence in extending the study of the branch of science to which it is related.

Nor is Prof. Huxley's manual so very short as might at first be supposed from the unpretending form given to it; but rather the abundance of facts is surprising which the author has contrived to compress into the space, without any loss of that clearness and comprehensiveness of statement for which he is so well known. The amount of printed matter, indeed, is very nearly the same as that comprised in the portion devoted to vertebrate animals in the second edition of Gegenbaur's "Outlines," the most approved recent German elementary treatise on Comparative Anatomy.

It is also deserving of note that there is an entire absence of speculation and theory, as well as of any vague generalities. The words "teleology," "design," "type of organisation," "descent," "natural selection," "genesis of species," find no place in this manual, which deals simply with ascertained facts and principles. In most instances, where uncertainty prevails, the grounds of doubt are stated, or the subject is altogether omitted; but on the whole, as is perhaps right in a manual, the author leans to the side of positive statement of his own views, when he has made up his mind on any disputed point.

So much for the manner of the book. As regards the matter, it may be said that, while it presents a masterly and decided statement of the great principles of Vertebrate Morphology, the most characteristic and important feature which pervades the whole, is the constant reference of all anatomical description and zoological distribution to the facts and laws of organogenesis, as ascertained from the observation of fœtal development. This is well known to be one of the great merits of Prof. Huxley's researches and writings, and he has made it truly the key-note and whole tenor of the manual, so as assuredly to prove one of its most valuable qualities in its future influence on the study of Comparative Anatomy.

The first two chapters of the manual, extending to one hundred and eleven pages, are devoted to an exposition of the general organisation of the Vertebrata, as exhibited in the skeleton (endoskeleton and exoskeleton), the muscular system, the nervous system with the organs of sense, the alimentary canal including the teeth, the sanguiferous and lymphatic systems, the respiratory organs, and the renal and reproductive organs. This is premised by a statement of the distinctive characters of the vertebrate organisation, in which the double cavity of the body, neural and visceral, is taken as the most marked basis of distinction between vertebrate and invertebrate morphology; and a clear short sketch is added of the most prominent phenomena of fœtal development.

The remaining six chapters contain a systematic exposition of the classification, organisation, and distribution of the several classes of vertebrate animals, under the three provinces of 1, Ichthyopsida, 2, Sauropsida, and 3, Mammalia; thus recognising the important approximations now established between Fishes and Amphibia under the first, and between Reptiles and Birds under the second of these provinces. In each class the position and organisation of extinct and fossil animals is also given.

The third chapter begins with the statement of the anatomical characters of the three great provinces; after which the organisation of fishes is described under an arrangement which is a modification of Johannes Müller's in the following groups, viz., 1, Pharyngobranchii (Amphioxus); 2, Marsipobranchii (the Myxines and lampreys); 3, Elasmobranchii (the sharks and rays); 4, Ganoidei (Lepidosteus, sturgeons, &c.); 5, Teleostei (osseous fishes); and 6, Dipnoi (Lepidosiren, transitional).

In Chapter 4 the structure of the class Amphibia is similarly given, under the following distribution—viz., I. Saurobatrachia, including, 1, Proteida (Siren, Axolotl, &c.), 2, Salamandrida (newts, &c.); II. Labyrinthodonta; III. Gymnophiona (Cæcilia, &c.); and IV. Batrachia (Anura, frogs and toads).

In Chapter 5, after giving the distinction between Reptiles and Birds as included under the province of Sauropsida, the class Reptilia is distributed under the following groups—viz., I. Chelonia; II. Plesiosauria; III. Lacertilia; IV. Ophidia; V. Ichthyosauria; VI. Crocodilia; VII. Dicynodontia; VIII. Ornithoscelida (Megalosaurus, Iguanodon, &c., transitional; IX. Pterosauria (Pterodactyles); and the comparative osteology of these groups is described.

In Chapter 6 Birds are distributed, and their Osteology is described under the following classification—viz., I. Saururæ (Archæopterygidæ, the metacarpals not ankylosed together); II. Ratidæ, including birds with more or less rudimentary wings, and in which the sternum is without a keel; III. Carinatæ, the large tribe in which the sternum is keeled, including four groups, viz., 1, Tinamomorphæ (Tinamous), 2, Schizognathæ, (the Plovers, Gulls, Penguins, Cranes, Hemipods, Fowls, Sand Grouse, Pigeons, Hoazin); 3, Ægithognathæ, (the Passerines, Swifts and Woodpeckers); 4, Desmognathæ (the Birds of Prey, Parrots, Cuckoos, Kingfishers, Anserinæ, Flamingoes, Storks, Cormorants).

In Chapter 7 the Muscles and Viscera of the Sauropsida are described together.

Chapter 8 (180 pages) is devoted to the Mammalia, distributed in three great groups, as follows:—

I. Ornithodelphia (1, Monotremata).

II. Didelphia (2, Marsupial animals).

III. Monodelphia, divided provisionally into twelve orders as follows—3, Edentata, 4, Ungulata, 5, Toxodontia, 6, Sirenia, 7, Cetacea, 8, Hyracoidea, 9, Proboscidea, 10, Carnivora, 11, Rodentia, 12, Insectivora, 13, Cheiroptera, 14, Primates. The first of these twelve orders is separated

* "A Manual of the Anatomy of Vertebrated Animals." By Thomas H. Huxley, LL.D., F.R.S. (London: J. and A. Churchill. 1871.)

from the rest by the absence of middle incisor teeth, the next four (4, 5, 6, 7) being reputed nondeciduate, the 8th, 9th, and 10th presenting a zonary placenta, and the remaining orders a discoidal placenta.

It was not to be expected that Professor Huxley should have here departed from the placental classification for which he has elsewhere shown so much favour.

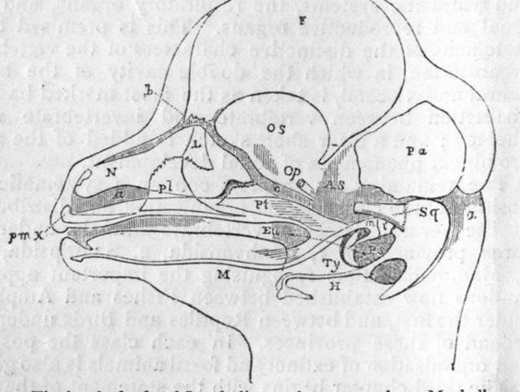

FIG. 1.—The head of a fœtal Lamb dissected so as to show Meckel's cartilage, *M*; the malleus, *m*; the incus, *i*; the tympanic, *Ty*; the hyoid, *H*; the squamosal, *Sq*; pterygoid, *Pt*; palatine, *pl*; lachrymal, *L*; premaxilla, *pmx*; nasal sac, *N*; Eustachian tube, *Eu*.

But however important the distinctions established upon that basis may be in themselves, it may fairly be doubted how far characters derived from parts which do not belong to the permanent organisation of the adult animal, the application of which is not yet fully known in one or two orders, and in which, too, there is much of a transitional nature, are preferable to signs of a more marked and easily observable kind deducible from other parts of the organisation.

In the description of structure all these orders are referred to; but in several of them particular familiar ani-

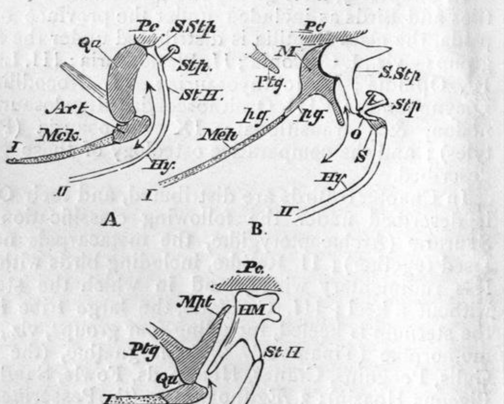

FIG. 2.—Diagram of the skeleton of the first and second visceral arches in a Lizard (A), a Mammal (B), and an Osseous Fish (C).
The skeleton of the first visceral arch is shaded, that of the second is left nearly unshaded. *I.* First visceral arch. *Mck.* Meckel's cartilage. *Art.* Articulare. *Qu.* Quadratum. *Mpt.* Metapterygoid; *M.* Malleus; *p.g.*, Processus gracilis. *II.* Second visceral arch. *Hy.* Hyoidean cornu. *St. H* Stylohyal. *S.* Stapedius. *Stp.* Stapes. *S. Stp.* Supra-stapedial. *HM.* Hyomandibular. The arrow indicates the first visceral cleft. *Pc.* The periotic capsule. *Ptg.* The pterygoid.

mals are happily selected for the fuller illustration of the more important systems; as for example, the horse, pig, dog, rabbit, hedgehog, seal, ox, porpoise: thus suggesting to the student the means by which a more practical and thorough knowledge of the organisation may be obtained by actual observation, than by the mere description of varieties in a wider series of animals less within his reach.

In regard to the order to be followed in so extensive a range of description as the comparative anatomy of any large tribe of animals involves, it may be remarked that, however interesting in a physiological point

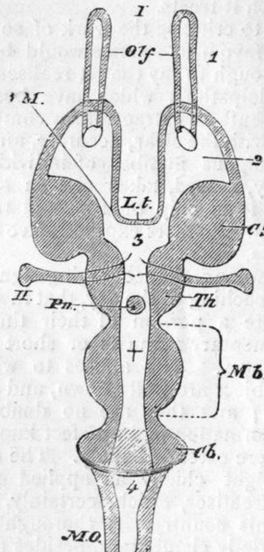

FIG. 3.—Diagrammatic horizontal section of a Vertebrate Brain. The following letters serve for both this figure and Fig. 4:—*Mb*, Mid-brain. What lies in front of this is the fore-brain, and what lies behind, the hind-brain. *L. t.* the lamina terminalis; *Olf*, the olfactory lobes; *Hmp*, the hemispheres; *Th.E*, the thalamencephalon; *Pn*, the pineal gland; *Py*, the pituitary body; *FM*, the foramen of Munro: *CS*, the corpus striatum; *Th*, the optic thalamus; *CQ*, the corpora quadrigemina: *CC*, the crura cerebri; *Cb*, the cerebellum; *PV*, the pons varolii; *MO*, the medulla oblongata; *I.* olfactorii; *II.* optici; *III.* point of exit from the brain of the motores oculorum; *IV.* of the pathetici; *VI.* of the abducentes; *V-XII*, origins of the other cerebral nerves. 1, olfactory ventricle; 2, lateral ventricle; 3, third ventricle; 4, fourth ventricle; +, *iter a tertio ad quartum ventriculum*.

of view may be the description of the variations of form and structure in the different organs taken separately in the whole series of animals, the results of this mode of teaching and study are inferior to those obtainable from the method of description of the whole organisation in successive groups or individual animals, as regards pro-

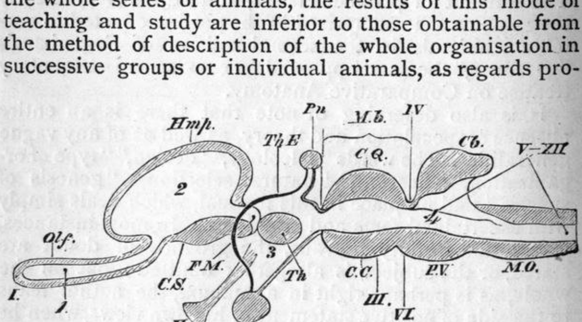

FIG. 4.—A longitudinal and vertical section of a Vertebrate Brain. The letters as before. The *lamina terminalis* is represented by the strong black line between *FM* and 3,

gress in morphological attainments, the determination of zoological affinities, and their application to the solution of the great biological problems of the day.

The latter part of this chapter treats of the Primates, which are divided into—1, the Lemuridæ, 2, the Simiadæ, and 3, the Anthropidæ. The Simiadæ are thrown into three groups, viz., 1, Arctopithecini, or marmosets; 2, Platyrrhini, or American monkeys; and 3, Catarrhini, or monkeys of the Old World, including two sub-groups, viz., *a*, Cynomorpha (with ischial callosities), and *b*, Anthropomorpha. In this last the author recognises with certainty as distinct the genera Hylobates or Gibbons, Pithecus or Orang, and Troglodytes or Chimpanzee, and is inclined to separate Gorilla as a fourth genus.

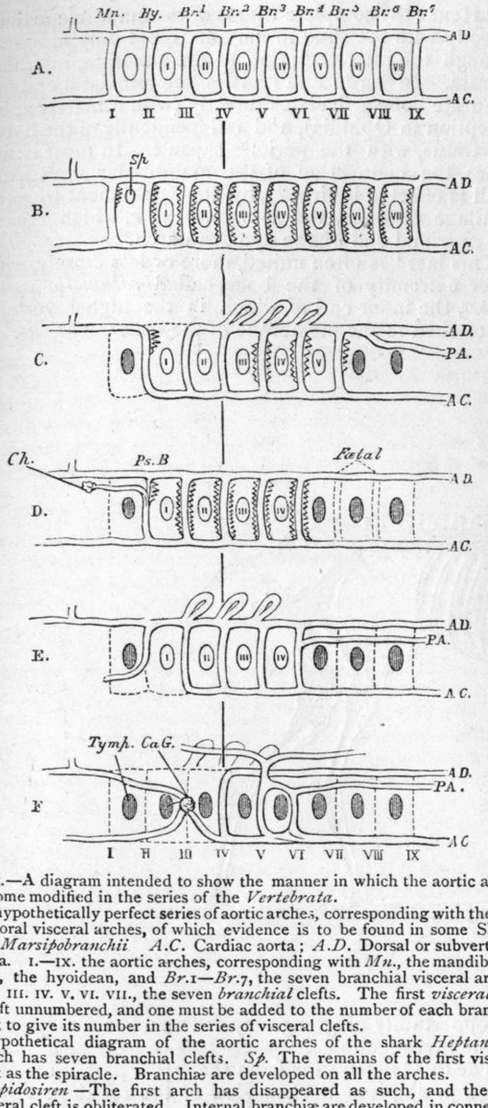

FIG 5.—A diagram intended to show the manner in which the aortic arches become modified in the series of the *Vertebrata*.

A. A hypothetically perfect series of aortic arches, corresponding with the nine postoral visceral arches, of which evidence is to be found in some Sharks and *Marsipobranchii* *A.C.* Cardiac aorta; *A.D.* Dorsal or subvertebral aorta. I.—IX. the aortic arches, corresponding with *Mn.*, the mandibular; *Hy.*, the hyoidean, and *Br.*1—*Br.*7, the seven branchial visceral arches. I. II. III. IV. V. VI. VII., the seven *branchial* clefts. The first *visceral* cleft is left unnumbered, and one must be added to the number of each branchial cleft to give its number in the series of visceral clefts.

B. Hypothetical diagram of the aortic arches of the shark *Heptanchus*, which has seven branchial clefts. *Sp.* The remains of the first visceral cleft as the spiracle. Branchiæ are developed on all the arches.

C. *Lepidosiren*.—The first arch has disappeared as such, and the first visceral cleft is obliterated. Internal branchiæ are developed in connection with the second, fifth, sixth, and seventh aortic arches; external branchiæ in connection with the fourth, fifth, and sixth. *P.A.* the pulmonary artery. The posterior two visceral clefts are obliterated.

D. A Teleostean Fish.—The first aortic arch and first visceral cleft are obliterated, as before. The second aortic arch bears the pseudo-branchia (*Ps. B.*), whence issues the ophthalmic artery, to terminate in the choroid gland (*Ch.*). The next four arches bear gills. The seventh and eighth arches have been observed in the embryo, but not the ninth, and the included clefts are absent in the adult.

E. The Axolotl (*Siredon*), a perennibranchiate amphibian. The third, fourth, fifth, and sixth aortic arches, and the anterior four branchial clefts, persist. The first visceral cleft is obliterated.

F. The Frog.—The three anterior aortic arches are obliterated in the adult. The place of the third, which is connected with the anterior external gill in the Tadpole, is occupied by the common carotid and the *rete mirabile* (carotid gland, *Ca.G.*) which terminates it. The fourth pair of aortic arches persist. The fifth and sixth pair lose their connection with the subvertebral aortic trunk, and become the roots of the cutaneous and pulmonary arteries. The first visceral cleft becomes the tympanum, but all the others are obliterated in the adult.

An interesting synopsis is given of the anatomical peculiarities of these animals, and of the circumstances in which they most differ from, or resemble, man. Among these the author has inadvertently overstated the proportion of the volume of the brain of the orang and chimpanzee to that of man, when he rates it at about half the minimum size of the normal human brain. Taking thirty-three ounces as the lowest weight of the latter consistent with a natural condition in the adult male, the brain of the orang and chimpanzee may be stated at a third of that weight.

At p. 487 this subject is summed up as follows:—"Of the four genera of the Anthropomorpha, the gibbons are obviously most remote from man, and nearest to the Cynopithecini.

"The orangs come nearest to man in the number of the ribs, the form of the cerebral hemispheres, the diminution of the occipito-temporal sulcus of the brain, and the ossified styloid process; but they differ from him much more widely in other respects, and especially in the limbs, than the gorilla and chimpanzee do.

"The chimpanzee approaches man most closely in the character of its cranium, its dentition, and the proportional size of the arms.

"The gorilla, on the other hand, is more man-like in the proportions of the leg to the body, and of the foot to

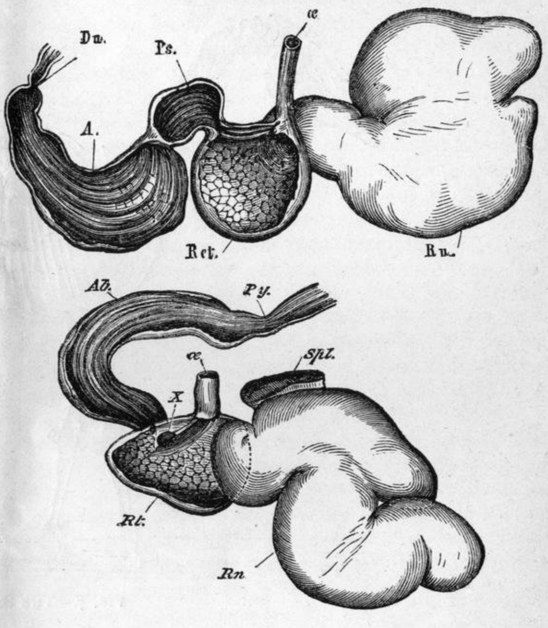

FIG. 6.—A, the stomach of a Sheep. B, that of a Musk-deer (*Tragulus*). *œ*, œsophagus; *Rn*, rumen; *Ret.*, reticulum; *Ps.*, psalterium; *A.*, *Ab* abomasum; *Du.*, duodenum: *Py.*, pylorus.

the hand; further, in the size of the heel, the curvature of the spine, the form of the pelvis, and the absolute capacity of the cranium."

The work is concluded with a brief statement of the characteristics of the human organisation. Among these the superior size of the head of the male infant at birth might perhaps have received a more prominent place. The short statement of variations in structure connected with difference of race is of peculiar interest. The various races of mankind are placed in two groups according to the character of the hair, viz., *a*, the Ulotrichi (crisp or woolly-haired), who are almost all dolichocephali, and *b*, Leiotrichi (straight-haired), who are distributed in four sets, viz., 1, Australioid, 2, Mongoloid, 3, Xanthochroic, or blue-eyed whites, and 4, Melanochroic, or dark whites.

It will now be proper to place before the reader some illustrations, taken from the "Manual," of Prof. Huxley's mode of treatment of individual topics.

The first of these which is selected (Fig. 1) relates to the intricate but deeply interesting subject of the homology of the *os quadratum* of birds and reptiles, a bone which was compared by Cuvier to the tympanic bone of mammals, but which more lately, in consequence of the embryological researches of Reichert and Rathke, was held to correspond rather with the incus,—a view in which Prof. Huxley formerly concurred. Later observations, however, (detailed in a paper published in the Proceedings of the Zoological Society for 1869) have led him to alter his opinion, and to form the opinion that the *os quadratum* may, with the greatest probability, be regarded as representing the malleus.

In explaining this morphological point, Prof. Huxley refers as follows (at p. 27) to the osteogenetic process connected with the formation of the lower jaw and hyoid bone, or mandibular and hyoid arches.

"Two ossifications commonly appear near the proximal end of Meckel's cartilage, and become bones moveably articulated together. The proximal of these is the quadrate bone found in most vertebrates, the malleus of mammals; the distal is the *os articulare* of the lower jaw in most vertebrates, but does not seem to be represented in mammals. The remainder of Meckel's cartilage usually persists for a longer or shorter time, but does not ossify. It becomes surrounded by bone, arising from one or several centres in the adjacent membrane, and the ramus of the mandible thus formed articulates with the squamosal bone in mammals, but in other vertebrata is immoveably united with the *os articulare*.

"Hence the complete ramus of the mandible articulates directly with the skull in mammals, but only indirectly, or through the intermediation of the quadrate, in other vertebrata. In birds and reptiles, the proximal end of the quadrate bone articulates directly (with a merely apparent exception in Ophidia), and independently of the hyoidean apparatus, with the periotic capsule. In most if not all fishes, the connection of the mandibular arch with the skull is effected indirectly, by its attachment to a single cartilage or bone, the *hyomandibular*, which represents the proximal end of the hyoidean arch."

This last "is often united, more or less closely, with the outer extremity of the bone, called *columella auris*, or *stapes*, the inner end of which, in the higher vertebrata, is attached to the membrane of the *fenestra ovalis*."

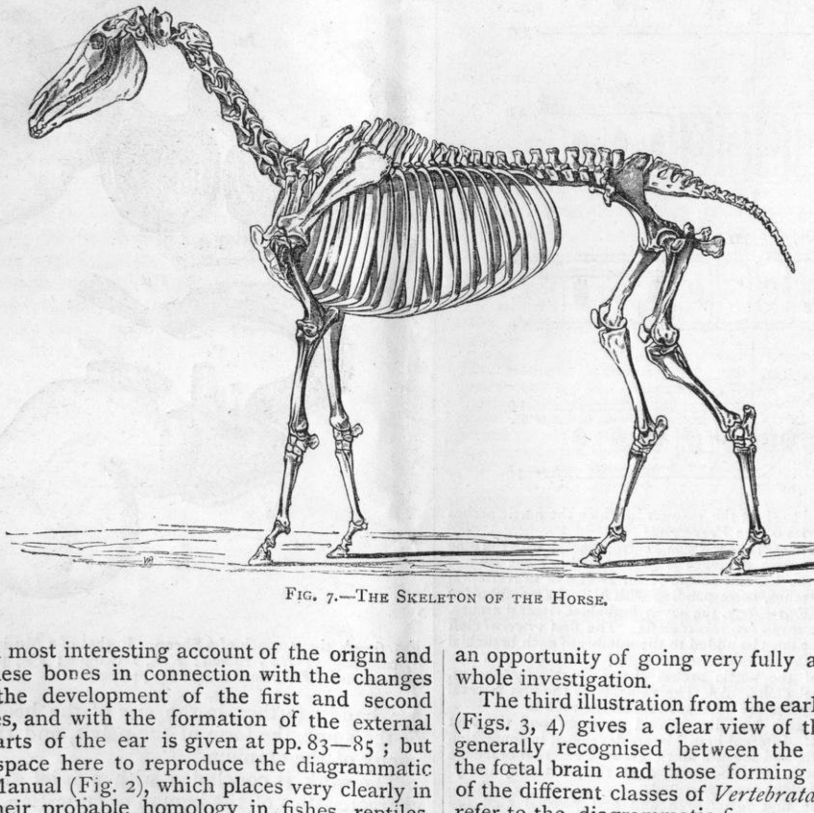

Fig. 7.—The Skeleton of the Horse.

A fuller and most interesting account of the origin and relations of these bones in connection with the changes occurring in the development of the first and second visceral arches, and with the formation of the external and middle parts of the ear is given at pp. 83—85; but there is only space here to reproduce the diagrammatic figure of the Manual (Fig. 2), which places very clearly in comparison their probable homology in fishes, reptiles, and mammals.

The main point on which the question hinges seems to be the separate connection ascertained to exist between the periotic capsule and the two rods contained respectively in the first and second visceral arches; the proximate part of the first becoming the *quadrate bone*, or *malleus*; that of the second becoming the *incus* in the part above the attachment of the *stapes* to the rod, and *stapedius* muscle below; while the *stapes* itself, or *columella auris*, is an offset, as it were, from the second rod proceeding to the *fenestra ovalis*. The subject, however, is one of so great difficulty, especially as connected with the existence and relations of the tympanic bone in birds and reptiles, to the proof of which the researches of Peters have been directed, that it would be hazardous to attempt any decision of the question at issue without an opportunity of going very fully and minutely into the whole investigation.

The third illustration from the earlier part of the Manual (Figs. 3, 4) gives a clear view of the relations now very generally recognised between the rudimentary parts of the fœtal brain and those forming the adult encephalon of the different classes of *Vertebrata*; and it is enough to refer to the diagrammatic figures, with their description.

The exposition which follows of the modifications in the form and organisation of the brain in different animals, together with the comparative views of the brains of the rabbit, pig, and chimpanzee, in figs. 21 and 22 of the Manual, is most instructive and worthy of attentive study.

The next illustration extracted from the Manual (Fig. 5) is diagrammatic, like the preceding ones, being intended to explain the changes by which, in fishes and amphibia, the permanent arterial vessels nearest the heart are derived from the common typical aortic arches, which, in the fœtus of all vertebrate animals, surround the first part of the alimentary canal.

The illustration might advantageously be extended to show the parallel changes in reptiles, birds, and mammals; these, however, may be better given from the ventral than from the lateral aspect.

The figure here inserted of the skeleton of the horse (Fig. 7) is a very good example of the wood engraving, in which, notwithstanding the small scale, there is remarkable clearness of detail; and the succeeding figures, representing several details of the osteology of the same animal, are all to be commended for beauty and delicacy of execution.

The illustration given in Fig. 6 is one in explanation of the structure of the stomach of the ruminant, in connection with which the following statement of recently-established points regarding rumination may be quoted (p. 381):—

"1. Rumination is altogether prevented by paralysis of the abdominal muscles, and it is a good deal impeded by any interference with the free action of the diaphragm.

"2. Neither the paunch nor the reticulum ever becomes completely emptied by the process of regurgitation. The paunch is found half full of sodden fodder, even in animals which have perished by starvation.

"3. When solid substances are swallowed, they pass indifferently into the rumen or reticulum, and are constantly driven backwards and forwards, from the one to the other, by peristaltic actions of the walls of the stomach.

"4. Fluids may pass either into the paunch and the reticulum, or into the psalterium, and thence at once into the fourth stomach, according to circumstances.

"5. Rumination is perfectly well effected after the lips of the œsophageal groove have been closely united by wire sutures.

"It would appear, therefore, that the cropped grass passes into the reticulum and rumen, and is macerated in them. But there is no reason to believe that the reticulum takes any special share in modelling the boluses which have to be returned into the mouth. More probably, a sudden and simultaneous contraction of the diaphragm and of the abdominal muscles compresses the contents of the rumen and reticulum, and drives the sodden fodder against the cardiac aperture of the stomach. This opens, and then the cardiac end of the œsophagus, becoming passively dilated, receives as much of the fodder as it will contain. The cardiac aperture now becoming closed, the bolus thus shut off is propelled by the reversed peristaltic action of the muscular walls of the œsophagus into the mouth, where it undergoes the thorough mastication which has been described."

In connection with this it may be remarked that fuller illustration by figures of the organs of digestion, circulation, and respiration in different animals seems desirable in the Manual.

Of the 110 woodcuts contained in the Manual, two-thirds are original, while the remaining third (37) are borrowed from other authors, whose names are mentioned in the preface.

For so complex a subject as the osteology of the skull, as well as perhaps in several other parts, some extended table of the bones, with the letters of reference employed throughout the work, would afford considerable assistance to the student.

It might also be advantageous in an elementary work of this kind to have added select references to works for fuller study, and a glossary of (at least unusual) terms.

In concluding this notice we repeat that the Manual is in every way worthy of its learned author, and calculated to be extremely useful in promoting the study of Comparative Anatomy and Zoology on sound principles. The work cannot fail soon to go to a second edition, when the author will have considered the expediency of such additions as we have ventured to suggest, or of others of which he approves, and which he has doubtless been deterred from including in the present work from the desire to bring it within as narrow a compass as possible. We may also express the hope that the publishers have made arrangements for the speedy publication of a similar Manual of the Anatomy of the Invertebrate Animals.

ALLEN THOMSON

NOTES

M. JANSSEN has addressed to the French Academy of Sciences the following letter, on the principal consequences to be drawn from his observations on the solar eclipse of 12th December last; it is dated Sholoor, December 19, 1871:—"I had the honour," he says, "of sending you on the very day of the eclipse a few lines to inform the Academy that I had observed the eclipse under an exceptional sky, and that my observations led me to assume a solar origin for the Corona (see NATURE, vol. v. p. 190). Immediately after the eclipse I was obliged to busy myself with the personal and material arrangements for my expedition into the mountains, and hence I have been unable to complete any detailed account, but I take advantage of the departure of this courier to give some indispensable details as to the announced results. Without entering into a discussion, which will form part of my narrative, I shall say, in the first place, that the magnificent Corona observed at Sholoor showed itself under such an aspect that it seemed to me impossible to accept for it any cause of the nature of the phenomena of diffraction or reflection upon the globe of the moon, or of simple illumination of the terrestrial atmosphere. But the arguments which militate in favour of an objective and circumsolar cause, acquire invincible force when we inquire into the luminous elements of the phenomenon. In fact, the spectrum of the Corona appeared in my telescope, not continuous, as it had previously been found, but remarkably complex. I detected in it, though much weaker, the brilliant lines of hydrogen gas, which forms the principal element of the protuberances and chromosphere; the brilliant line which has already been indicated during the eclipses of 1869 and 1870, and some other fainter ones; obscure lines of the ordinary solar spectrum, especially that of sodium (D); these lines are much more difficult to perceive. These facts prove the existence of matter in the vicinity of the sun; matter which manifests itself in total eclipses by phenomena of emission, absorption, and polarisation. But the discussion of the facts leads us still further. Besides the cosmical matter independent of the sun which must exist in its neighbourhood, the observations demonstrate the existence of an excessively rare atmosphere, with a base of hydrogen, extending far beyond the chromosphere and protuberances, and deriving its supplies from the very matter of the latter—matter which is projected with so much violence, as we may ascertain every day. The rarity of this atmosphere at a certain distance from the chromosphere must be excessive; its existence, therefore, is not in disagreement with the observations of some passages of comets close to the sun."

WE earnestly call the attention of all men of science who may have influence with the French Government, to the letter on behalf of Elisée Reclus by Mr. H. Woodward, which will be found in another column.

WE have to record the death of the Rev. Canon Moseley, F.R.S., on Saturday last in his 71st year. Born in 1801, he went to St. John's College, Cambridge, where he graduated seventh wrangler in 1826. He was for a time Professor of Natural Philosophy and Astronomy at King's College, London, and was afterwards appointed one of Her Majesty's Inspectors of Schools, and was a member of the Ordnance Select Committee. Canon Moseley was well known for his writings on various physical subjects, in particular on the phenomena connected with the freezing of water, and the molecular constitution of glacial ice.

THE *Photographic News* notices the death of one of the most eminent continental photographers, Johannes Grasshoff, of Berlin, at the early age of thirty-six. At the recent exhibition of the Photographic Society in Conduit Street, his studies were among those most admired in the whole collection, and not least his group of thirty different pictures from one and the same model. Like some others of the most successful photographers, his education was that of an art student, and he was known as a clever painter before he became a skilful photographer.

It will be recollected what a warm discussion was raised in the French Academy of Sciences before the late war by the proposal to enrol Mr. Darwin among its corresponding members. The proposal was at that time postponed, but his name has now been placed first on the list for the forthcoming election of a Corresponding Member in Zoology, and he will, therefore, no doubt receive the honour. His supporters are MM. Milne-Edwards, Quatrefages, and Lacaze-Duthiers.

At the meeting of the Royal Geographical Society on Monday evening last, Lieutenant Dawson, R.N., was introduced as the leader of the party organised to attempt the relief of Dr. Livingstone. Mr. W. O. Livingstone, a son of the explorer, born in the neighbourhood of Lake N'gami, is to accompany the party. An application to the Treasury for a grant of money to aid the expedition has been unsuccessful. Should this decision be a final one, the undertaking must therefore depend entirely on private subscriptions; but we are happy to see that the subject is already being warmly taken up in many of the larger towns in the country, and the sum of 1,700*l.* was announced as having been raised by Monday evening last. Since then a public meeting has been held at Glasgow, at which 200*l.* was subscribed, and one will probably be held in London, under the auspices of the Lord Mayor.

The subscription raised as a Memorial Fund to the late Mr. Alder of Newcastle now amounts to about 300*l.* This is considerably less than the amount it was thought might have been raised, though sufficient to carry out in a limited form the original suggestions as to its appropriation. The Committee recommend that it should be invested in the names of trustees, and should serve as the foundation of a Scholarship in Zoology, or other branch of Biology, bearing Mr. Alder's name, in the New College of Physical Science in Newcastle; the transfer to be coupled with such stipulations as to the teaching of Biological Science as may be agreed upon.

The editor of *Les Mondes* calls attention to the manner in which scientific chairs have been disposed of in France, not so much with the object of "finding men to fill the vacant places as places for the *protégés* or favourites of the moment." On the death of M. d'Archiac, the chair of palæontology in the Museum of Natural History at Paris was given to M. Lartet, a palæontologist of world-wide renown, but too advanced in years and of too feeble health to permit him to give a single lesson. On the death of M. Lartet, although there are a large number of good palæontologists in France, it was all but decided, from motives of private convenience and patronage, to abolish the chair, its maintenance being secured by a majority of two votes only. The appointment has now been made to the professorship of M. Albert Gaudry, late assistant to Prof. d'Archiac, and author of "La Géologie et la Paléontologie de l'Attique," an appointment which will give general satisfaction.

The *Engineer* states that the French Government, impressed by the want of thorough geographical instruction, have under consideration a plan for a Geographical Institute, on a scale which has never before been attempted. The proposed Institute is to include all the means and accessories of geographical education in its widest acceptation—books, maps, charts, globes, instruments, collections of natural objects, &c.—and to include a staff of professors and teachers of the highest grades. The naval depôt of charts and plans will form one of the departments of the new Institute, which promises to be of eminent service, not only to France, but to the whole of Europe, for, should it be established on the scale proposed, there is little doubt that it will give an impulse to geographical study throughout the civilised world.

The Massachusetts Society for Promoting Agriculture will award on the 1st of March next two prizes of 300 dols. and 200 dols. respectively to the two best establishments in the State for the culture of fishes for food, all competitors for which must send in their names and addresses to the secretary of the Society, Edward N. Perkins, 42, Court Street, Boston. The committee of award will consider the number of species of fishes cultivated, the number of individuals, and their size and condition, the number of eggs hatched in the establishment, and of young reared from them, the neatness and economy of the establishment, and the excellence of the fixtures.

Dr. Stimpson, the secretary of the Academy of Sciences of Chicago, left Baltimore in the steamer of the 15th of December for Key West, for the purpose of making explorations and collections in the Florida waters, partly with the object of replacing that portion of the collection of the Chicago Academy lost by the fire. It is expected that he will take charge of the dredging operations of the United States Coast Survey steamer *Bibb*, while she is employed in selecting a line for the submarine cable which is to be laid for the International Cable Company between Cape San Antonio, Cuba, and some point on the coast of Yucatan.

We learn from the *Gardeners' Chronicle* that among the disastrous losses occasioned by the Chicago fire, the very valuable Entomological Collection of the late Dr. Walsh was totally destroyed. The *Canada Farmer* states that after the death of the eminent entomologist, the collection became by purchase the property of the State. It was not only very extensive, but the specimens were arranged and labelled with great care and accuracy; and it will be many years before another can be collected to replace it.

The first number of the Journal of the Anthropological Institute of New York, an institution newly organised upon the base of the former Ethnological Society of that city, is published. In the change the scope of the society has been greatly enlarged, and many of the difficulties attendant upon the maintenance of the old organisation have been obviated. Several papers of more or less interest are to be found in this first number, and there is little doubt that the new society will occupy a prominent place in advancing knowledge in the world.

Mr. Stephen T. Olney, a well-known botanist, resident at Providence, Rhode Island, has just published a list of the Algæ of Rhode Island, as collected and prepared by himself. In this he enumerates twenty-four species of melanosperms, or olive-coloured algæ; forty-four of rhodosperms, or red algæ; and twenty-five of the chlorosperms, or green algæ, making ninety-three species in all. The remaining forms, principally microscopic, enumerated by him, and including zygnemaceæ, desmideæ, and diatomaceæ, bring the number up to 189. Of most of these Mr. Olney possesses duplicates, which he will be happy to dispose of in exchange.

The second volume of the "Annals of the Dudley Observatory," edited by its director, G. W. Hough, has just made its appearance, and consists of a report of the meteorological observations made at the observatory from 1862 to 1871. Its value is enhanced by its embracing the hourly records of the barometer (automatically printed) for a continuous period of five years, made by means of a very efficient apparatus invented by the director, and now used in numerous places, among others, in the office of the Signal Service at Washington. An appendix to the report contains miscellaneous communications upon the galvanic battery, the total eclipse of the sun of August 2, 1869, and the meteoric showers of 1867, &c.; and the whole book must be considered a very valuable contribution to physical science.

Serious apprehensions have been excited at Nantwich in

Cheshire by the repeated landslips which have occurred there. For several winters in succession large surfaces of ground have fallen in, it is supposed on account of the withdrawal of the salt from the salt-mines. The slip which occurred this winter is on the same spot where similar occurrences happened twelve months, two, and four years ago. The pit is about 300 yards in circumference, and about 100 feet deep, and the sides are almost perpendicular. It is feared that if these subsidences continue the town itself will be threatened, and the attention of the Government has been called to them.

MR. W. LAIRD CLOWES, in a letter to a contemporary dated The Cottage, Pinner, Monday, Jan. 8, writes :—"To-night, between 8.15 and 8.30, I noticed three beautifully luminous atmospherical phenomena on the northern horizon. They all took the form of an arc of fire of between 8° and 10° in height, the first two happening within a minute of one another, and the last about eleven minutes after the second. There were a slight breeze and light clouds at the time." This was most probably an aurora borealis, but we have not seen any other account of it.

THE Trinity Board have established an electric light at the South Foreland lighthouse, which is situated between Dover and Deal. It was formally opened on New Year's Day by Sir Frederick Arrow, the Deputy-Master of the Trinity Board, in the place of Prince Arthur, who was prevented from being present. This lighthouse establishes a triangle of electric lights, the other two being at Dungeness and Cape Grisnez

THE accounts furnished by the *Boston Advertiser* from the captains and crews of the vessels of the whaling fleet lately destroyed or ice-bound in the Arctic Ocean concur in describing the presence of peculiar meteorological phenomena during the past season. The prevailing summer wind on the north-west coast of Alaska is from the north, and this works the ice off from the land and disperses it, while the north-westerly winds close it up on the shore. As the ice moves off, the ships generally work up by the land, and in that situation find whales in plenty. By the end of the season, when north-westerly winds are prevalent, the ice becomes so broken up and melted that it has ceased to be an element of danger, and the vessels are compelled to retire to the northward by heavy ice drifting along the coast from the north, and not from a threatened closing in upon the land. But this season the easterly winds were not so strong and constant as usual, and the ice that had gone off from shore returned in a heavy pack, so that it was impossible to get a ship through, or even to hold against it at anchor. The heavy ice-fields are all composed of fresh-water berg-ice, not floe-ice of salt-water. The bergs are not of the immense proportions seen in Greenland seas, but are solid enough to be equally dangerous, many masses being so heavy as to ground in ten fathoms of water.

ON Nov. 15 the town of Oran, the second city in the province of Salta, was destroyed by a series of earthquakes lasting nine hours. Very little life was lost, the first shocks being light. The inhabitants had time to flee to the open camp of Monte Video.

*SCIENTIFIC INTELLIGENCE FROM AMERICA**

ADVICES from Lieutenant G. M. Wheeler, United States Engineers, whose movements during the past year we have had frequent occasion to chronicle, announce his arrival at Tucson about Dec. 4, with the men and animals nearly exhausted. The trip from Prescott to Camp Apache had been very severe, on account of the snow and high winds on the Colorado plateau. During their exploration one party had been sent to the San Francisco mountains, and made the ascent of the principal peak. These mountains consist of three prominences, grouping in the form of a crater, the north-eastern rim being wanting. The principal peak was occupied as a topographical, barometrical, and photographic station. It is believed to be nearly 1,000 feet higher than the peak usually ascended ; and Lieutenant Wheeler was of the opinion that his party was the first to occupy its summit. This, however, was a mistake, as Dr. Edward Palmer, of the Smithsonian Institution, made the ascent in 1870, and obtained a number of new species of plants and insects.—A document which has been for some years in preparation, and toward which much expectation has been directed by agriculturists, has just appeared from the Government press, namely, the Report of the Commissioner of Agriculture upon the Diseases of Cattle in the United States. About the middle of June, 1868, a disease broke out at Cairo, Illinois, among a number of Texas cattle, known as the Spanish fever, or the Texas cattle disease. In consequence of the rapid extension of this disease, very serious alarm was excited, and the services of Prof. John Gamgee, a distinguished English veterinarian, then in the United States, were secured by General Capron, the Commissioner of Agriculture, for the purpose of instituting a careful inquiry as to its cause, course, and methods of treatment. The Professor immediately visited the infected districts in Illinois, and in the spring of 1869 examined that part of Texas on or near the Gulf coast, where the transportation of the native cattle begins. In this last journey he was accompanied by Prof. Ravenel, of South Carolina, a specialist among the fungi, and whose particular object was to determine what part such plants played in the infection. Dr. J. S. Billings and Dr. Curtis, of the army, were also associated in the inquiry, having special reference to the microscopic investigations. A second investigation by Prof. Gamgee, under the authority of the Commissioner of Agriculture, had reference to the subject of pleuro-pneumonia, in the course of which numerous microscopic observations were made by Dr. Woodward, of the Army Medical Museum. Full reports on these various subjects made by the different gentlemen are embodied in the volume referred to, which appears in quarto form, with numerous well-executed plates in chromo lithography. It is also accompanied by a report by Mr. Dodge, the statistician of the Agricultural department, upon the history of this Texas cattle disease, also known as splenic fever, in which the devastations of this peculiar native malady are traced back into the eighteenth century. This report was considered by General Capron as simply preliminary, and further investigations are indicated as important. Among those especially mentioned are inquiries as to the best mode of arresting the contagion, and the proper way of transportation of the cattle northward. He thinks that a general law of the United States, in the interest of public health, of an enlightened humanity, and of the cattle trade, should regulate this traffic, not only throughout the Gulf States, but on the great routes throughout the country.—A valuable document lately issued by the Surgeon-General's Office at Washington, prepared by Dr. G. A. Otis, consists of a report of surgical cases treated in the army of the United States from 1865 to 1871, covering almost every possible variety of injury, whether by gun-shot wounds, lacerations, fractures, dislocations, amputations, &c. The report, which is a quarto of nearly 300 pages, is illustrated in the same excellent style as its predecessors, and the woodcuts are especially worthy of all praise.—Bills have been introduced both in the Senate and House of Representatives providing for the reservation of that portion of the region about the Yellow Stone Lake, in which the wonderful geysers and hot springs occur, to which we have repeatedly called the attention of our readers. The thorough exploration of that country made during the past season by Dr Hayden has enabled him to define the limits within which these natural features occur, and the bill is based upon a plan prepared under his direction. The area proposed to be preserved is about sixty five miles in length by fifty-five in width, and it is suggested that the reservation be placed under the direction of the Secretary of the Interior, who is to be empowered to take such steps as may be required to protect the natural curiosities from injury or destruction. It is highly important that this should become a law at the present session, as the glowing accounts given by Dr. Hayden will cause a great many persons to visit the country during the coming year, and with the natural iconoclasm of the Anglo-Saxon race, there is great danger that the wonderful water basins and formations of sulphur and of calcareous and siliceous rocks will be knocked to pieces for the purpose of securing mementoes of a visit.

* Communicated by the Scientific Editor of *Harper's Weekly*.

THE LAWS OF ORGANIC DEVELOPMENT*

THE discussion of this subject divides itself into two parts, viz.: a consideration of the proof that evolution of organic types, or descent with modification, has taken place; and, secondly, the investigation of the laws in accordance with which this development has progressed.

I.—*On the Proof for Evolution.*

There are two modes of demonstration, both depending on direct observation. One of these has been successfully presented by Darwin. He has observed the origin of varieties in animals and plants, either in the domesticated or wild states, and has shown, what had been known to many, the lack of distinction in the grades of difference which separate varieties and species. But he has also pointed out that species (such, so far as distinctness goes) have been derived from other species among domesticated animals, and he infers by induction that other species, whose origin has not been observed, have also descended from common parents. So far I believe his induction to be justified: but when from this basis evolution of divisions defined by important structural characters, as genera, orders, classes, &c., is inferred, I believe that we do not know enough of the uniformity of nature's processes in the premises to enable us so regard this kind of proof as conclusive.

I therefore appeal to another mode of proving it, and one which covers the case of all the more really structural features of animals and plants.

It is well known that in both kingdoms, in a general way, the young stages of the more perfect types are represented or imitated with more or less exactitude by the adults of inferior ones. But a true identity of these adults with the various stages of the higher has, comparatively, rarely been observed. Let such a case be supposed.

In *A* we have four species whose growth attains a given point, a certain number of stages having been passed prior to its termination or maturity. In *B* we have another series of four (the numbering a matter of no importance), which, during the period of growth, cannot be distinguished by any common, *i.e.*, generic character, from the individuals of group *A*, but whose growth has only attained to a point short of that reached by those of group *A* at maturity. Here we have a parallelism, but no true evidence of descent. But if we now find a set of individuals belonging to one species, and therefore held to have had a common origin or parentage (or still better the individuals of a single brood), which present differences among themselves of the character in question, we have gained a point. We know in this case that the individuals *a*, have attained to the completeness of character presented by group *A*, while others, *b*, of the same parentage, have only attained to the structure of those of group *B*. It is perfectly obvious that the individuals of the first part of the family have grown further, and, therefore, in one sense faster, than those of group *b*. If the parents were like the individuals of the more completely grown, the offspring which did not attain that completeness may be said to have been *retarded* in their development. If, on the other hand, the parents were like those less fully grown, then the offspring which have added something have been *accelerated* in their development.

I claim that a consideration of the uniformity of nature's processes, or inductive reasoning, requires me (however it may affect the minds of others) to believe that the groups of species whose individuals I have never found to vary, but which differ in the same point as those in which I have observed the above variations, are also derived from common parents, and the more advanced have been accelerated or the less advanced retarded, as the case may have been with regard to the parents.

This is not an imaginary case, but a true representation of many which have come under my observation. The developmental resemblances mentioned are universal in the animal and probably in the vegetable kingdoms, approaching the exactitude above depicted in proportion to the near structural similarity of the species considered.

II.—*On the Laws of Evolution.*

Wallace and Darwin have propounded as the cause of modification in descent their law of natural selection. This law has been epitomised by Spencer as the "preservation of the fittest." This neat expression no doubt covers the case, but it leaves the origin of the fittest entirely untouched. Darwin assumes a "tendency to variation" in nature, and it is plainly necessary to do this in order that materials for the exercise of a selection should exist. Darwin and Wallace's law is, then, only restrictive, directive, conservative, or destructive of something already created. Let us, then, seek for the originative laws by which these subjects are furnished—in other words, for the causes of the origin of the fittest.

The origin of new structures which distinguish one generation from those which have preceded it, I have stated to take place under the law of acceleration. As growth (creation) of parts usually ceases with maturity, it is entirely plain that the process of acceleration is limited to the period of infancy and youth in all animals. It is also plain that the question of growth is one of nutrition, or of the construction of organs and tissues out of protoplasm.

The construction of the animal types is restricted to two kinds of increase—the addition of identical segments and the addition of identical cells. The first is probably to be referred to the last, but the laws which give rise to it cannot be here explained. Certain it is that segmentation is not only produced by addition of identical parts, but also by subdivision of a homogeneous part. In reducing the vertebrate or most complex animal to its simplest expression, we find that all its specialised parts are but modifications of the segment, either simply or as sub-segments of compound but identical segments. Gegenbaur has pointed out that the most complex limb with hand or foot is constructed, first, of a single longitudinal series of identical segments, from each of which a similar segment diverges, the whole forming parallel series, not only in the oblique transverse, but generally in the longitudinal sense. Thus the limb of the Lepidosiren represents the simple type, that of the Ichthyosaurus a first modification. In the latter the first segment only (femur or humerus) is specialised, the other pieces being undistinguishable. In the Plesiosaurian paddle the separate parts are distinguished; the ulna and radius well marked, the carpal pieces hexagonal, the phalanges well marked, &c.

As regards the whole skeleton, the same position may be safely assumed. Though Huxley may reject Owen's theory of the vertebrate character of the segments of the cranium, because they are so very different from the segments in other parts of the column, the question rests entirely on the definition of a vertebra. If a vertebra be a segment of the skeleton, of course the skull is composed of vertebræ; if not, then the cranium may be said to be formed of "sclerotomes," or some other name may be used. Certain it is, however, that the parts of the segments of the cranium may be now more or less completely parallelised or homologised with each other, and that as we descend the scale of vertebrated animals, the resemblance of these segments to vertebræ increases, and the constituent segments of each become more similar. In the types where the greatest resemblance is seen, segmentation of either is incomplete, for they retain the original cartilaginous basis. Other animals which present cavities or parts of a solid support are still more easily reduced to a simple basis of segments, arranged either longitudinally (worm) or centrifugally (star-fish, &c.).

Each segment—and this term includes not only the parts of a complex whole, but parts always subdivided, as the jaw of a whale or the sac-body of a mollusc—is constructed, as is well known, by cell-division. In the growing fœtus the first cell divides its nucleus and then its whole outline, and this process repeated millions of times produces, according to the cell theory, all the tissues of the animal organism or their bases from first to last. That the ultimata or histological elements of all organs are produced originally by repetitive growth of simple, nucleated cells with various modifications of exactitude of repetition in the more complex, is taught by the cell theory. The formation of some of the tissues is as follows:—

First Change—Formation of simple nucleated cells from homogeneous protoplasm or the cytoblastema.

Second—Formation of new cells by division of body and nucleus of the old.

Third—Formation of tissues by accumulation of cells with or without addition of intercellular cytoblastema.

A. In connective tissue by slight alteration of cells and addition of cytoblastema.

B. In blood, by addition of fluid cytoblastema (fibrin) to free cells (lymph corpuscles), which in higher animals (vertebrates) develop into blood-corpuscles by loss of membrane, and by cell development of muscles.

* Abstract of paper by Prof. E. D. Cope, read at the Indianapolis meeting of the American Association for the Advancement of Science: reprinted from the *American Naturalist.*

C. In muscles by simple confluence of cells, end to end, and mingling of contents (Kölliker).

D. Of cartilage by formation of cells in cytoblast which break up, their contents being added to cytoblast; this occurring several times, the result being an extensive cytoblast with few and small cells (Vogt). The process is here an attempt at development with only partial success, the result being a tissue of small vitality.

Even in repair-nutrition recourse is had to the nucleated cell. For Cohnheim first shows that if the corner of a frog's eye be scarified, repair is immediately set on foot by the transportation thither of white or lymph or nucleated corpuscles from the neighbouring lymph heart. This he ascertained by introducing aniline dye into the latter. Repeated experiments have shown that this is the history in great part of the construction of new tissue in the adult man.

Now, it is well known that the circulating fluid of the fœtus contains for a period only these nucleated cells as corpuscles, and that the lower vertebrates have a greater proportion of these corpuscles than the higher, whence probably the greater facility for repair or reconstruction of lost limbs or parts enjoyed by them. The invertebrates possess only nucleated blood corpuscles.

What is the relation of cell division to the forces of nature, and to which of them as a cause is it to be referred, if to any? The animal organism transfers the chemism of the food (protoplasm) to correlated amounts of heat, motion, electricity, light (phosphorescence), and nerve force. But cell division is an affection of protoplasm distinct from any of these. Addition to homogeneous lumps or parts of protoplasm (as in that lowest animal, *Protamœba* of Haeckel) may be an exhibition of mere molecular force, or addition as is seen in the crystal, but cell division is certainly something distinct. It looks to me like an exhibition of another force, and though this is still an open question, it may be called for the present *growth force*. It is correlated to the other forces, for its exhibitions cease unless the protoplasm exhibiting it be fed. It is potential in the protoplasm of both protoplasmic animal mass and protoplasmic food, and becomes energetic on the union of the two. So long as cell-division continues it is energetic; when cells burst and discharge the contained cytoblastema, as in the formation of cartilage, it becomes again potential.

The size of a part is then dependent on the amount of cell division or growth force, which has given it origin, and the number of segments is due to the same cause. The whole question, then, of the creation of animal and vegetable types is reduced to one of the amount and location of growth force.

Before discussing the influences which have increased and located growth force, it will be necessary to point out the mode in which these influences must necessarily have affected growth. Acceleration is only possible during the period of growth in animals, and during that time most of them are removed from the influence of physical or biological causes either through their hidden lives or incapacity for the energetic performance of life functions. These influences must, then, have operated on the parents, been rendered potential in their reproductive cells, and become energetic in the growing fœtus of the next generation. However little we may understand this mysterious process, it is nevertheless a fact. Says Murphy, "There is no act which may not become habitual, and there is no habit which may not be inherited." Materialised, this may be rendered—there is no act which does not direct growth force, and therefore there is no determination of growth force which may not become habitual; there is, then, no habitual determination of growth force which may not be inherited; and of course in a growing fœtus becomes at once energetic in the production of new structure in the direction inherited, which is acceleration.

III.—*The Influences Directing Growth Force.*

Up to this point we have followed paths more or less distinctly traced in the field of nature. The positions taken appear to me either to have been demonstrated or to have a great balance of probability in their favour. In the closing part of these remarks I shall indulge in more of hypothesis than heretofore.

What are the influences locating growth force? First, physical and chemical causes; second, use; third, effort. I leave the first, as not especially prominent in the economy of type growth among animals, and confine myself to the two following. The effects of use are well known. We cannot use a muscle without increasing its bulk; we cannot use the teeth in mastication without inducing a renewed deposit of dentine within the pulp-cavity to meet the encroachments of attrition. The hands of the labourer are always larger than those of men of other pursuits. Pathology furnishes us with a host of hypertrophies, exostoses, &c., produced by excessive use, or necessity for increased means of performing excessive work. The tendency, then, induced by use by the parent is to add segments or cells to the organ used. Use thus determines the locality of new repetitions of parts already existing, and determines an increase of growth force at the same time, by the increase of food always accompanying increase of work done, in every animal.

But supposing there be no part or organ to use. Such must have been the condition of every animal prior to the appearance of an additional digit or limb or other useful element. It appears to me that the cause of the determination of growth force is not merely the irritation of the part or organ used by contact with the objects of its use. This would seem to be the remote cause of the deposit of dentine used in the tooth, in the thickening epidermis of the hand of the labourer, in the wandering of the lymph-cell to the scarified cornea of the frog in Cohnheim's experiment. You cannot rub the sclerotica of the eye without producing an expansion of the capillary arteries and corresponding increase in the amount of nutritive fluid. But the case may be different in the muscles and other organs (as the pigment cells of reptiles and fishes) which are under the control of the volition of the animal. Here, and in many other instances which might be cited, it cannot be asserted that the nutrition of use is not under the direct control of the will through the mediation of nerve force. Therefore I am disposed to believe that growth force may be, by the volition of the animal, as readily determined to a locality where an executive organ does not exist, as to the first segment or cell of such an organ already commenced, and that therefore effort is in the order of time the first factor in acceleration.

Effort and use have, however, very various stimuli to their exertion.

Use of a part by an animal is either compulsory or optional. In either case the use may be followed by an increase of nutrition under the influence of reflex force or of direct volition.

A compulsory use would naturally occur in new situations which take place apart from the control of the animal, where no alternatives are presented. Such a case would arise in a submergence of land where land animals might be imprisoned on an island or in swamps surrounded by water, and compelled to assume a more or less aquatic life. Another case which has also probably often occurred, would be when the enemies of a species might so increase as to compel a large number of the latter to combat who would previously have escaped it.

In these cases the structure produced would be necessarily adaptive. But the effect would be most frequently to destroy or injure the animals (retard them) thus brought into new situations and compelled to an additional struggle for existence, as has, no doubt, been the case in geologic history. Preservation, with modifications, would only ensue where the changes should be introduced very gradually. This mode is always a consequence of the optional use. The cases here included are those where choice selects from several alternatives, thus exercising its influence on structure. Choice will be influenced by the emotions, the imagination, and by intelligence.

As examples of intelligent selection the modified organisms of the varieties of bees and ants must be regarded as striking examples of its exercise. Had all in the hive or hill been modified alike, as soldiers, queens, &c, the origin of the structures might have been thought to be compulsory; but varied and adapted as the different forms are to the wants of a community, the influence of intelligence is too obvious to be denied. The structural results are obtained in this case by a shorter road than by inheritance.

The selection of food offers an opportunity for the exercise of intelligence, and the adoption of means for obtaining it still greater ones. It is here that intelligent selection proves its supremacy as a guide of use, and consequently of structure, to all the other agencies here proposed. The preference for vegetable or for animal food determined by the choice of individual animals among the omnivores, which were, no doubt, according to the palæontological record the predecessors of our herbivores, and perhaps of carnivores also, must have determined their course of life, and thus of all their parts into those totally distinct directions. The choice of food under ground, on the ground, or in the trees would necessarily direct the uses of organs in those directions respectively.

Intelligence is a conservative principle, and always will direct effort and use into lines which will be beneficial to its possessor. Thus we have the source of the fittest—*i.e.*, addition of parts by increase and location of growth force directed by the will—the will being under the influence of various kinds of compulsory choice in the lower, and intelligent option among higher animals. Thus intelligent choice may be regarded as the originator of the fittest, while natural selection is the tribunal to which all the results of accelerated growth are submitted. This preserves or destroys them, and determines the new points of departure on which accelerated growth shall build.

Acceleration under the influence of effort accounts for the existence of rudimental characters. Many other characters will follow at a distance, the modifications proceeding in accordance with the laws here proposed, and retardation is accounted for by complementary or absolute loss of growth force.

SOCIETIES AND ACADEMIES

London

Royal Society, January 18.—"Investigations of the Currents in the Strait of Gibraltar, made in August 1871," by Captain G. S. Nares, R.N., of H.M.S. *Shearwater*, under instructions from Admiral Richards, F.R.S., Hydrographer of the Admiralty.

Geological Society, Jan. 10.—Mr. Joseph Prestwich, F.R.S., president, in the chair. "On Cyclostigma, Lepidodendron, and Knorria from Kiltorkan." By Prof. Oswald Heer. In this paper the author indicated the characters of certain fossils from the yellow sandstone of the South of Ireland, referred by him to the above genera, and mentioned in his paper "On the Carboniferous Flora of Bear Island," read before the Society on November 9, 1870 (see Q. J. G. S. vol. xxvii. p. 1). He distinguished as species *Cyclostigma kiltorkense*, Haught., *C. minutum* (Haught.), *Knorria acicularis*, Göpp. var. *Bailyana*, and *Lepidodendron Veltheimianum*, Sternb.—Mr. Carruthers was glad that he had made the observations which he did on Prof. Heer's former paper, as it had caused the Professor to give the reasons on which his opinions were based. He was doubtful whether the success which had attended Prof. Heer's determination of species from leaves justified the application of the same principles to mere stems. He could not accept the difference in size or distance of leaf-scars as a criterion of species, inasmuch as they were merely the result of the difference in the age and size of the parts of the plants on which they were observed. Even Prof. Heer himself had united together specimens presenting greater differences in this respect than those which he distinguished. He considered *Cyclostigma kiltorkense*, *C. minutum*, and *Lepidodendron Veltheimianum* to be founded on different parts of one species. In the Kiltorkan fossils the outer surface of the original stems was often broken up into small fragments, the phyllotaxy on which proved them to be portions of large stems, and not entire branches. As to *Knorria*, it was certainly the interior cast of the stem of *Lepidodendron*, with casts of the channels through which the vascular bundles passed with some cellular tissue to the leaves; and the specimen figured showed that it belonged to a branch similar to that represented as *C. minutum*. He considered that the four supposed species belonging to three genera were only different forms of the same plant.—"Notes on the Geology of the Plain of Marocco, and the Great Atlas." By Mr. George Maw. The author described first the characters presented by the coast of Marocco, and then the phenomena observed by him in his progress into the interior of the country and in the Atlas Chain. The oldest rocks observed were ranges of metamorphic rocks bounding the plain of Marocco, interbedded porphyrites and the porphyritic tuffs forming the backbone of the Atlas Chain, and the Mica-schists of Djeb Tezah in the Atlas. At many points in the lateral valleys of the Atlas almost vertical grey shales were crossed; the age of these was unknown. Above these comes a Red Sandstone and Limestone series, believed to be of Cretaceous age, and beds possibly of Miocene age, which occupied the valleys of the Atlas and covered the plain of Marocco, where vestiges of them remain in the form of tabular hills. The probable age of these beds was determined on the evidence of fossils. The author noticed the sequence of denuding and eruptive phenomena by which the arrangement and distribution of these rocks has been modified, and described the more recent changes resulting in the formation of enormous boulder-beds flanking the northern escarpment of the Atlas plateau, and of great moraines at the heads of the valleys of the Atlas, both of which he ascribed to glacial action. An elevation of the coast line of at least seventy feet was indicated by raised beaches of concrete sand at Mogador and elsewhere, and the author considered that a slight subsidence of the coast was now taking place. The surface of the plain of Marocco was described as covered with a tufaceous crust, probably due to the drawing up of water to the surface from the subjacent calcareous strata and the deposition from it of laminated carbonate of lime. Mr. Ball, as an Alpine traveller who had also visited the Atlas in company with Dr. Hooker and Mr. Maw, offered a few remarks. The plain of Marocco was not, in his opinion, a level, but an inclined plane, rising gradually in height up to the foot of the mountain, so that the base of the boulder ridges was at some height above the level of the plain near Marocco. He did not think that the boulder deposits could be safely attributed to glaciers, but thought rather that they had been carried into and deposited in a shallow sea. He thought also that Mr. Maw had somewhat over-estimated the thickness of some of the boulder deposits; and though there was one instance of an undoubted moraine in one of the higher valleys of the Atlas, yet he could not agree in the view that the glaciation of the Atlas was general. He could not accept such a great thickness of beds as that represented by the vertical shales in Mr. Maw's section. Prof. Ramsay was pleased that the author, though giving so many interesting details, had not assigned any definite age to many of the beds. He agreed with him as to the cause assigned for the great tufaceous coating of the country. He had already assigned the same cause for the existence of certain saline beds, and would attribute the existence of the great coating of gypsum at slight depth below the surface of the Sahara to the same cause. As to the existence of moraines, he was not surprised to find them in the Atlas, as they were already known in the mountains of Granada. As to the escarpments, it was now well known that, as a rule, they assumed a direction approximately at right angles to the dip of the strata; and he felt inclined to consider that the bulk of the mounds at the foot of the escarpment of the Atlas were rather the remains of a long series of landslips from the face of the cliffs than to an accumulation of moraine matter. Mr. D. Forbes commented on the similarity of the rocks to those of the Andes in South America. In the Andes the porphyritic tuffs appeared to belong to the Oolitic age; and the igneous rocks associated with them were of the same date. He thought that, so far as the author's observations had gone, the structure of the Atlas was much the same as that of the Andes. Mr. W. W. Smyth mentioned that in the district to the east of the Sierra Nevada, in the south part of Spain, where there was great summer heat, and also heavy occasional rainfall, the same tufaceous coating as that observed in Marocco was to be found. He had been led to much the same conclusion as to its origin as that arrived at by Mr. Maw. The upper part was frequently brecciated, and the fragments re-cemented by carbonate of lime. Mr. Seeley, though accepting Mr. Etheridge's determination as to the Cretaceous age of the fossils if found in England, could not accept it as conclusive in the case of fossils from Marocco. The genius *Exogyra*, for instance, which ranges through the Secondary to existing seas, might well belong to some other age; and even the fossils presumably Miocene might, after all, date from some other period. Mr. Maw, in reply, stated that he agreed with Mr. Ball as to the rise in the Marocco plain as it approached the Atlas, having taken it in one direction at 400 feet in 25 miles. He pointed out the resemblance between the moraines in the valley of the Rhone and those which he regarded as such on the flanks of the Atlas. As a proof of their consisting of transported blocks, he mentioned the fact that the Red Sandstone rock of which they were composed did not occur in the adjacent escarpments, but was not to be found within seven or eight miles. There was, moreover, a mixture of different materials in the mounds.

Linnean Society, January 18.—Mr. Bentham, president, in the chair. "On the Anatomy of *Limulus polyphemus*," by Prof. Owen (continued). The author resumed and concluded the reading of this memoir. The nervous system of *Limulus* appeared to have occupied most attention, and was described in detail. From the fore part of the œsophageal ring, answering to the brain, were sent off the "ocellar," "ocular," "antennular," and "antennal" nerves; the latter supplying the second pair of articulate limbs—the homologues of the "external antennæ" of higher *Crustacea*. From the post- or sub-œsophageal part of the ring, proceeded large nerves to the four succeeding pairs of limbs; and also smaller nerves, having distinct origins, to the chilaria and to the opercular plate-limbs. The neural axis then continues, as a pair of coalesced chords, to the middle of the thoracetron, developing five ganglions supplying the five gill-limbs. Beyond the fifth ganglion the chords separate; each forms a loop resembling a ganglion, beyond which each chord penetrates the base of the "pleon." To this it supplies five dorsal and five ventral nerves before being continued and resolved into a plexus toward the end of the tail and spine. The author remarked that, as the nervous system preceded the tegumentary in the order of development, it might thus manifest evidences of the more generalised segmental type of the pleon, more plainly than had been noticed in the formation of the chitinous walls of that division of the body, in the embryo, in which it first budded forth as a ninth segment of the thoracetron. Details of the organs of the senses, of the digestive, circulatory, respiratory, and generative systems were then given, and illustrated, like the nervous system, by minutely-finished drawings. The heart was elongate, vasiform, included in a pericardial-like sinus: besides an anterior and posterior aortic trunk, there were seven pairs of lateral primary branches. The arteries soon lose their tubular form, and, as they expand, lose likewise much of their fibrous walls, and seem reduced to delicate membranous sinuses which follow the shapes of the parts or interstices along which the blood meanders as it returns by the venous sinuses to the general pericardial one. The most remarkable of the arterial prolongations are those which the author had previously described in his "Lectures on Invertebrata" (8vo ed., 1855, p. 310) as expanding upon, and seeming to form the neurilemma of, the central axis and branches of the nervous system; so that injection of the anterior aorta coats the neurine and demonstrates a great part of the nervous system by its colour. (A drawing showing this effect of fine red injection was exhibited.) Finally the author cited the chief results of the observations of Lockyer, Packard, and Dohrn on the development of the king-crab. There was neither a nauplius stage nor a trilobite stage. A superficial resemblance to trilobites is shown by the absence of the pleon in the embryo king-crab; but the very fact of the late appearance of this terminal division was decisive against any real representative resemblance of the embryo *Limulus* to the trilobites; on the acceptance, at least, of Barrande's observations of the successive and later appearance of the segments of the "thoracetron" in the space between the head ("cephaletron") and "pygidium" (pleon and tail-spine) of the embryos of *Sao*, *Agnostus*, and *Trinucleus*. The author here recalled attention to Newport's observations of the like development of successive segments, anterior to the caudal one, in Iulidæ, and remarked that with other facts noted in the anatomical sections, especially the fusion of the pair of cephalic ganglia, and the short and thick crura connecting these with the subœsophageal mass, giving the condition of that part of the nervous system in *Scorpio* and *Iulus*, *Limulus* manifested in an instructive and interesting way the more "generalised type" of articulate structure, in which arachnidan and myriapodal characters were associated with crustaceous ones. But, in the development of *Limulus*, the pleon, pygidium, or tail-spine was the last to appear, and, at its first budding, looked like a ninth segment of the thoracetron. Packard speaks of indications, transitory indeed, of segmentation of the crust; and such indications, as the author had shown in the anatomy of *Limulus*, were more strongly and lastingly given by the nervous system. The tail-spine belongs to the series of body-segments, and is no mere appendage to the dorsal arc of such. After formifaction and the attractive and repellent forces have produced in the germ-masses the phenomena of segmentation and vegetative repetition, as manifested in the similar and parallel heaps of granules, like bricks for the building, the inherited influences overrule the polaric ones, and operate in differentiating and adaptive lines, speedily showing the embryo *Limulus*, which, like that of *Astacus fluviatilis*, *Palæmon adspersus*, *Crangon maculosus*, *Eriphia spinifrons*, and one may add, all Cephalopods, takes its own course to the full manifestation of its specific characters, agreeably with the nature originally impressed on the germ. There was no divergence to a larval form with a term of active life as such; there was no metamorphosis, either "nauplial" or "trilobitic." Some objected to the king-crabs being called Crustacea; there was more ground, the author thought, for objecting to call them Arachnida or Myriapoda. Characters common to *Limulus* with their allied extinct gill-bearing, well limbed Articulata, have not a class-value. The author could not, at least, raise the Merostomes to an equivalency with, and run them parallel to and alongside of, the rest of the branchiate Condylopods. A class, after all, was an artificial group, a help to the classifier. One may call *Limulus* a Crustacean and yet discern in its anatomy the evidence of its more "generalised structure" than in Malacostraca; its type preceded that of either macrourous or brachyurous Crustacea, and indicates characters subsequently appropriated by and intensified in the air-breathing members of the Apterous Insecta of Linnæus. As compared with its longer-bodied and many-jointed predecessors, *Limulus* itself shows a concentrative specialisation; but vegetative repetition still reigns in the limb-series. "Inner antennules," "outer antennæ," "mandibles," "maxillæ," "maxillipeds," "legs," all work together by their basal joints in subserviency to mastication, and all end in pincers. As compared with modern crabs no structure was more striking and significant than the resistance, so to speak, of the heart in *Limulus* to the concentrative tendencies; it is still the "dorsal vessel," though the body-part containing it has the breadth and shortness of the crab's carapace, in which the heart is shaped to match. In both the neural axis supplying the cephaletral limbs is annular, but in modern crabs the subœsophageal part is defined by distance and concomitantly long and slender from the super-œsophageal or cerebral part. This differentiation had not taken place in *Bellimurus*, *Neolimulus Prestwichia*, and other palæozoic predecessors of *Brachyura*, whose organisation we have to thank their long-lived, lingering representative genus for enabling us to peer into. That such glimpses, with concomitant tracing of the development of the individual *Limulus*, afford us some ground, and that the like work, with persevering quest of its palæozoic fossil allies, may afford more, for guessing at the ways in which a pre ordained plan of derivation by congenital departures from parental form has operated, in originating the various deviations from a common primitive articulate type, is an encouraging faith. That the old ocean should have given the chance conditions of origin of crustaceous sub-classes, orders, genera, species, by natural selection, was not conceivable by the author, who, nevertheless, held the conviction that all forms and grades of Articulata were due to "secondary cause or law," as strongly as when he expressed the same conclusion in regard to the Vertebrata, and termed it "the deep and pregnant principle" evolved in the researches on the general homologies and archetype of their skeletons.

Mathematical Society, January 11.—Mr. W. Spottiswoode, F.R.S., president, and subsequently Prof. Cayley, V.P., in the chair. Major E. Close, R.A., was admitted into the society. Prof. Cayley gave an account of his paper "On the Surfaces the loci of the Vertices of Cones which satisfy six conditions."—Mr. J. W. L. Glaisher stated and illustrated the principal points in his communication "On the Constants which occur in certain summations by Bernouilli's Series.—Mr. W. B. Davis read a paper describing the methods he had used in the construction of tables of divisors, and exhibited tables of factors of numbers consisting of nine and twelve figures. A brief discussion ensued on the subject of this communication.—Mr. Roberts explained some of the results which he submitted to the society in his paper "On the parallel surface of Conicoids and Conics," and illustrated the same by means of a model and drawings of sections of one of the surfaces.

Zoological Society, January 16.—Prof. Newton, F.R.S., vice-president, in the chair.—The Secretary read a report on the additions that had been made to the Society's collection during the month of December, 1871, amongst which was particularly mentioned a young Prince Alfred's Deer (*Cervus alfredi*), born in the Gardens.—A letter was read from Prof. Owen, F.R.S., communicating some particulars received from Dr. Julius Haast, of Christchurch, New Zealand, respecting the finding of the remains of *Aptornis* in the Glenmark Swamp, New Zealand.—Mr. H. E. Dresser exhibited and made remarks on specimens of the eggs of *Reguloides superciliosus* and *Reguloides occipitatis*, collected by Mr. W. E. Brooks in Cashmere.—A communication was read from Dr. G. Hartlaub and Dr. O. Finsch, giving an account of a collection of birds from the Pelew and Mackenzie Islands in the Pacific, to which was added a complete synopsis of the ornithology of this portion of the Caroline group.—A communication was received from Mr. A. Sanders, containing a complete description of the Myology of *Liolepis belli*.—Mr. A. G. Butler communicated a synomic list of the species formerly included in the genus *Pieris*, with references to all others described since the subdivisions of that genus by recent authors.—A communication was read from Mr. John Brazier, of Sydney, N.S.W., giving a list of the *Cypræa* met with on the coast of New

South Wales.—A paper by Mr. A. Anderson was read containing the second portion of his notes on the Raptorial Birds of India.

Chemical Society, January 18.—Dr. Frankland, F.R.S., president, in the chair.—At this meeting Dr. Odling exhibited some very fine specimens of rare metals and their compounds, which had been lent to him by Dr. Richter and Dr. Theodor Schuchardt. Among these was a bar, weighing about seven ounces, of metallic indium; an element discovered a few years ago by Richter, in conjunction with Reich; also some metallic rubidium.—Dr. David Howard then read an interesting paper "On quinicine and cinchonicine and their salts." These alkaloids are prepared artificially, from quinine and cinchonine respectively, by the action of heat on their salts, and are isomeric with them. Quinicine occurs along with the two last-mentioned alkaloids in cinchona bark, being apparently the one which is first formed during the growth of the cinchona plant.

PARIS

Academy of Sciences, January 15.—A note by M. M. Lévy on a property of the focals of surfaces, was presented by M. Bertrand, in which the author puts forward the proposition that any surface and its focal intersect each other at right angles.—A note from M. Catalan, on General Didion's communication concerning the relation of the circumference to the diameter, was read, in which the authorship of similar formulæ is ascribed to Euler.—M. H. Resal communicated a memoir containing equations of the vibratory movement of a circular plate, and M. Serret a note by M. E. Ciotti on the employment of vibrating elastic plates for the realisation of a propeller, in connection with a recent communication from M. de Tastes.—A memoir on the measurement of very high temperatures, and on the temperature of the sun, by M. H. Sainte-Claire Deville, was read. The author maintained that the temperatures which may be produced and measured in the laboratory are not greatly exceeded in nature, and that the temperature of the sun is not far from 2,500—2,800° C. (= 4,532—5,072° F).—M. Delaunay read a note on the secular variations of the mean movements of the perigee and node of the moon.—M. Faye presented a note upon the investigations of Dr. Heis on meteors, which are confirmatory of M. Faye's previous communication as to the different centres of radiation observed in November last.—A letter was read from M. Janssen on the principal consequences which may be drawn at present from his observations of the eclipse of December last. (A translation of this letter will be found in another column.)—M. P. Guyot forwarded a note on a meteor observed at Nancy on the 20th of December last at 10h. 28m. A.M. This meteor passed from Cassiopeia through Perseus towards the Pleiades, near which it exploded, with a bright green light.—M. E. Becquerel presented a report on various memoirs by M. W. de Fonvielle regarding observations to be effected during balloon ascents. M. E. Becquerel also presented a note by M. T. Sidot on the electrisation by friction of metals in sulphide of carbon, and on the decomposition of that body by light. The author finds that certain metals, especially silver, aluminium, and iron, become electrised, and produce sparks when strongly agitated with pure sulphide of carbon, and that the latter, when exposed to the light of the sun, is decomposed, producing a gas and a solid flocculent matter. The same gentleman also communicated a joint note by MM. F. Lucas and A. Cazin containing an account of some experimental researches upon the duration of the electric spark.—Notes by M. Lion and M. Diamilla Müller on the action of ecliptical conjunctions upon the elements of terrestrial magnetism were read. According to the former considerable perturbations were observed at Alençon during the eclipse of the 11th December last.—M. Tarry presented a further note on the movement of recoil of cyclones in equatorial regions.—In a paper on the combustion of carbon by oxygen, M. Dumas showed, in opposition to M. Dubrunfaut, that carbon is combustible in perfectly dry oxygen.—M. Chevreul made some remarks on this paper.—A note by MM. L. Dusart and C. Bardy on the transformation of phenole into alkaloids was presented by M. Cahours. The authors have obtained phenylamine, chloride of phenyle, and diphenylamine by the action of hydrochlorate of ammonia and fuming hydrochloric acid upon phenole.—M. P. Barbier announced his having produced cymene by treating hydrate of essence of turpentine with bromine.—A letter was read from M. V. Meyers on the reaction between sulphur and aqueous vapour in the synthesis of sulphuric acid, and on the preparation of pure zinc by electrolysis.—An important discussion on the vexed question of spontaneous generation was raised by the reading of some reflections concerning heterogenesis by M. A. Trécul. In the discussion MM. Balard, Fremy, and Blanchard took part.—A somewhat cognate matter was also treated by M. A. Béchamp in his paper on the cause of alcoholic fermentation by beer-yeast, and on the formation of leucine and tyrosine in this fermentation.—M. C. Robin presented a note by M. S. Chantran on the fecundation of the crayfish, in which the author describes the impregnation of the ova as taking place after their expulsion from the oviducts.—A note by MM. E. Mathieu and V. Urbain on the gases of the blood, was presented by M. Cahours.

DIARY

THURSDAY, JANUARY 25.

ROYAL SOCIETY, at 8.30.—On the Absolute Direction and Intensity of the Earth's Magnetic Force at Bombay: C. Chambers, F.R.S.—On the Elimination of Alcohol: Dr. Dupré.—On the Action of Low Temperatures on Supersaturated Solutions of Glauber's Salt: C. Tomlinson, F.R.S.

SOCIETY OF ANTIQUARIES, at 8.30.—Miscellaneous Communications on Objects of Mediæval Antiquity.

FRIDAY, JANUARY 26.

ROYAL INSTITUTION at 9.—On the Demon of Socrates: Archbishop of Westminster.

QUEKETT MICROSCOPICAL CLUB, at 8.

SATURDAY, JANUARY 27.

ROYAL INSTITUTION, at 3.—On the Theatre in Shakespeare's Time: Wm. B. Donne.

SUNDAY, JANUARY 28.

SUNDAY LECTURE SOCIETY, at 4.—On Ice, as a Geological Agent: A. H. Green.

MONDAY, JANUARY 29.

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

ROYAL UNITED SERVICE INSTITUTION, at 8.30.—On Modern Ships of War, as illustrated by the Models in the Institution: Nathaniel Barnaby.

TUESDAY, JANUARY 30.

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

WEDNESDAY, JANUARY 31.

SOCIETY OF ARTS, at 8.—On Individual Providence for Old Age as a National Question: G. C. T. Bartley.

THURSDAY, FEBRUARY 1.

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

ROYAL SOCIETY, at 8.30.

SOCIETY OF ANTIQUARIES, 8.30.

LINNEAN SOCIETY, at 8.—On the Classification and Geographical Distributions of Compositæ: The President.

CHEMICAL SOCIETY, at 8.

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NOTICE

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