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THURSDAY, MARCH 14, 1872

LA SEINE*

IN carrying out the great works for the improvement and embellishment of Paris under the late Empire, all incidental discoveries of objects relating to art, history, and science, were systematically investigated, recorded, and preserved, instead of being left to the chance and uncertain description of casual and independent observers. In a liberal and enlightened spirit the Municipality of Paris and the Préfet of the Seine (M. Haussmann) established a proper organisation and a staff (*Service des fouilles et des substructions*) to follow up such discoveries, to take plans of old works, to preserve all art treasures or objects of scientific value; to note, in fact, and to investigate everything of interest. Men eminent in several departments were consulted, and engaged to draw up reports with full illustrations of the discoveries. By these judicious measures, the knowledge of the topography, antiquities, and archæology of Old Paris has been greatly advanced. Works of the Roman, Gallic, and Mediæval periods have been brought to light, surveys and plans made, and the more important specimens preserved *in situ* or in the public museums.

To M. Belgrand, the eminent and able engineer for the water supply and drainage of Paris, was deputed the work of recording all the geological and some of the archæological facts discovered during the construction of the large works on which he was engaged.

Paris up to the last few years had been supplied with water from local sources (river, canal, and wells), but as these were found insufficient and of indifferent quality, it was determined to seek for other and better sources of supply at a distance, and some large springs in the chalk district, respectively distant sixty and eighty-four miles from Paris, were eventually selected by M. Belgrand, and their waters were brought to Paris by means of aqueducts on a high level. In carrying out this great work, M. Belgrand made himself intimately acquainted with the hydrography of the Basin of the Seine. He explored every valley, and determined the *régime* of every important river. The result of the first part of the inquiry appeared in a valuable series of tables, showing the connection between the rainfall and the discharge of each river—the extent and nature of the floods, and the geological character of the ground with reference to the range and extent of the permeable and impermeable strata, and which he illustrated by a specially coloured map. In connection with the construction of the aqueducts, M. Belgrand had also to ascertain the nature of the surface and the contours of the hills and great plains along which he carried them, and to examine the many pits whence the materials for construction were obtained. This geological investigation led to the discovery of many interesting specimens, and further suggested many theoretical inquiries relating to the origin of the present surface, and to the *régime* of the old Seine during the later geological periods. The result of the inquiry is embodied in the three handsome quarto volumes before us—one of 255 pages of text, with 106 pages of introduction, descriptive of the country and giving the theoretical views; a second containing plates of fossils, of flint implements, and pit sections; and a third with extended coloured sections and a monograph by M. Bourguingnat of the shells found in the Drift beds.

Paris stands on Tertiary strata, from beneath which, at a distance of some miles, the chalk crops out and forms a belt many miles in width. These formations constitute a table land having a height of 100 to 200 feet along the sea coast of Normandy, and rising from 500 to 600 feet inland in Champagne. This district is traversed by the Seine and its tributaries, flowing in comparatively narrow valleys cut deep into the table land; while, on the extended upland plains thus formed, there rise, here and there, ranges of hills of Fontainebleau Sands or other later Tertiary strata. The strike of these hills is in a direction entirely distinct from that of the hill slopes flanking the river valleys and forming part of the present river-system. The latter range in various directions—north, north-east, south, and south-east—in accordance with the direction of the tributaries of the Seine until they join that river, the main channel of which has, from Montereau to the sea, a general direction south-east to north-west. M. Belgrand found that the hills on the plains nearly all ranged in this one given direction, or approximately from south-east to north-west, with intervening valleys having the same direction. Numerous such ridges, none being of any great length and all narrow and having this definite trend, are found to extend over the whole plateau area uninfluenced by the more tortuous deeper river-valleys which intersect the same area at various angles to their course. The river-valleys are covered with gravel formed of the *débris* of the rocks through which the present rivers flow, while the plateau valleys and plains are free from such *débris*, but are covered with a uniform layer of red clay or loam. Whence M. Belgrand concludes that the two systems of valleys have a different origin. He contends that it is not possible to have a true river channel without having more or less drifted gravels formed by the constant action of running water and by floods, and therefore that these higher valleys could not have been formed by river action, while at the same time their rectilinear and special bearing indicates that their formation is due to one common and independent cause.

M. Belgrand considers that the only explanation which will account for the phenomena presented by these higher-level valleys and hills, is the rapid and transient passage of a large body of water over the surface; and as the excavation of these higher valleys took place after the formation of the Fontainebleau Sands and of the Calcaire de Beauce (Miocene), and before the Pliocene period (for the *Elephas meridionalis* of the valley of the Eure shows that the land had then emerged), and as also, according to M. Elie de Beaumont, the elevation of the main chain of the Alps took place at the same period, M. Belgrand connects the two events and supposes that the sea of the Pliocene deposits of the Alpine area was thereby displaced and that it swept over this northern portion of France, denuding the softer portions of the strata and leaving narrow ridges of the harder portions

* *Le Bassin Parisien aux Ages Antéhistoriques.* Par M. Belgrand, Inspecteur-Général des Ponts and Chaussées, Directeur des Eaux et des Egouts de la Ville de Paris. (Paris: Imprimérie Imperial.)

all trending south-east to north-west (or in the direction from the Alps), standing out, on the denuded high plains, as monuments of its passage. M. Belgrand points out that where the Tertiary strata have presented a resistance which the waters could not overcome, the high-level valleys formed by the diluvial waters are, in such cases, fronted in the opposite range of hills, against which the mass of waters impinged, by a deep bay cut by the current in those hills, and that the waters thus checked in their course were turned off at acute angles, until they reached the main channel of the Seine, tending thereby to form secondary or tributary valleys, which, when the deluge had passed, contributed, with the Seine valley, to form the present lines of river drainage. Such volumes of water as we have depicted would, he argues, have swept the higher channels and plains clear of *débris*, leaving the denuded area covered merely with the silt thrown down from muddy waters, and depositing the coarser *débris* in the middle and lower range of the deeper channels through which the present rivers afterwards took their course. In support of this hypothesis, he shows that, whereas the basin of the Seine is now drained by the one river and its tributaries, the diluvial waters held their course straight across that basin and debouched in five main channels—one, marked by the hills of Montmorency and Satory, took the course of the Seine below Montereau to the sea, but in a more direct and broader line; the second took the course shown by the hills of Villers-Cotterets, thence across the present valley of the Oise, along the valley of the Pays de Bray, to the sea at Dieppe; the third followed in part the course of the Aisne, and then by the line of the Somme valley to the sea; and the fourth and fifth by those of the valleys of the Aulthie and Cauche. M. Belgrand accounts for the radidity and force of this cataclysm in the belief, which he shares with M. Elie de Beaumont, that the elevation of the Alps took place rapidly and suddenly.

But there was a second elevation of the Alps, at a later geological period, and which, according to M. Belgrand, may have produced a second deluge, not by the displacement of the sea, for then there were only lakes on the north-western side of those mountains, but by the sudden melting of the snow on that great range; and our author again adopts the views of M. Elie de Beaumont on this subject. This distinguished geologist propounded in 1847 the theory that that last convulsion of the Alps was accompanied by an enormous disengagement of those gases to which has been attributed the formation of the Dolomites and Gypsum beds of that chain, and that this caused the accumulated snows to melt in a very brief period of time (*un instant*). At the same time, according to the same authority, the Pliocene lakes of "La Bresse" were raised and drained. Thus, suggests M. Belgrand, this second convulsion might have caused another diluvial wave to pass over the basin of the Seine—an hypothesis also advanced by M. Elie de Beaumont, who speaks of "the probable concourse in this off-throw flood (*déversement*) towards the north-west, of the waters of the great lake of La Bresse, in the production of the diluvial phenomena observed in the neighbourhood of Paris."

We are disposed to agree with our author in the opinion, which we have elsewhere expressed, that the original contour of the surface with its higher valleys and hills, is due to a cause different from that which excavated the present river valleys—that it preceded and is independent of it—but we cannot agree with him as to the nature of that cause. Without going far into the argument, we may mention that the well-known fact of the gravel found in each tributary of the valley of the Seine, consisting of the *débris* of those rocks only through which that tributary flows, while in the Seine valley are found the *débris* of all the tributaries, together with its own and no more, is, it seems to us, a conclusive argument against the passage of a body of water from one great basin to another—against the flow of such a body of water from the Alps across the Jura, the great plains of the Doubs and the Soane, the southern prolongation of the Vosges, and, over the separating water-shed formed by the lower hills of Burgundy, to the Seine basin, and so to sea on the northern shores of France. Such a cataclysm must surely have spread the *débris* of the strata destroyed in its course north-westward along the tract over which it flowed. Some remains of the rocks of Switzerland, of those of the Vosges and of Burgundy, must surely have been detected in the course of its passage. How can the author account for the large blocks and abundant *débris* of the Seine valley—which blocks and *débris* he considers as originally due to this cataclysmic action—and yet overlook the almost necessary consequence of the introduction of some foreign elements into the Seine Basin, whereas none such exist. Not only is the *débris* of each great basin restricted to its own rocks, but even each tributary river valley has its own special rock *débris* and no other. M. Belgrand remarks, it is true, of the Somme Valley, which lies on the line of his third great diluvial water channel, and which prolonged south-east passes across the Oise valley and up that of the Aisne, that some *débris* of the older rocks of the latter areas have been found in the chalk valley of the Somme. But we must confess we have never found a trace of such a mixture, and we have particularly examined the Drift of those areas with a view to the determination of this point. At the same time the water-shed between the two valleys is so low that their complete separation in old times appears to us more remarkable than their present independence, and we can quite conceive the possibility of the Oise waters, when that river flowed at its higher level, passing at periods of flood into the valley of the Somme, and so carrying some small amount of *débris* across the present water-shed, especially as so good an observer as M. Buteux is referred to as the authority for this fact. If there, however, it is evidently quite the exception, and may be accounted for as just suggested.

With regard to the ingenious suggestion of M. Belgrand that some south-east and north-west valleys of the tablelands are faced on the opposite side of intersecting river valleys by a bay in the hills due to the violence of the checked diluvial waters, such for example as the amphitheatre in the hills on the west of the River Ecolle between Milly and Moigny and again at Soissy, it is to be remarked that such amphitheatres exist equally on the opposite or lee side of the hills towards La Ferté-Aleps and Maisse; and, further, that, in the same Tertiary area beyond the intersecting range of hills between the Ecolle and the Essonne (which according to M. Belgrand's views should have acted as a breakwater), the south-east and north-west ridges again resume between the valleys of the Essonne and the Eure.

After the contour of the surface produced by this cataclysm, and by which M. Belgrand considers that all traces of any previous river courses must have been obliterated, the Seine and its tributaries began to flow at an elevation estimated by him of from 80 to 100 feet above the present level. This he proves, as we have already done, by the occurrence of the remains of land mammalia and of river and land shells in beds of Drift at that elevation above the Seine on some of the hills near Paris. This part of M. Belgrand's work is admirably illustrated, both by general and local sections, and contains valuable lists of the mammalian remains, in the determination of which he had the advantage of the high authority of the late M. Ed. Lartet. Here again we cannot, however, agree with him in his *modus operandi*. The great boulders of sandstone, meulière, granite, &c., found in the valley gravel of the Seine, are attributed by M. Belgrand in the first place to removal to the line of the Seine valley by diluvial action, and subsequently to their drifting along the valley channel by the river action during floods of the Quaternary period, and he gives some remarkable instances of the power of water to remove large blocks, and of the rate at which such blocks move. When, however, it is considered that the granitic rocks of the Morvan have been transported some 150 miles, and other rock boulders in proportion, that the angles of many of the large blocks of sandstone and of meulière constantly show little wear, and that they are dispersed irregularly and at various levels, some imbedded in soft clays, and others in sand or fine gravel and that these latter are often twisted and contorted, we can only explain the phenomena by the action of river ice and transport thereby.

M. Belgrand, on the other hand, shows that a prolonged and steady fall of rain, even if not very heavy, during the winter, now produces great floods—that such rivers as the Yonne and Cure flowing over impermeable strata are subject to sudden and great freshets after a heavy but short fall, whereas the Marne and Seine flowing over permeable strata have their floods retarded, but, at the same time, rendered more permanent by the rainfall having to pass through the strata and delivered in springs. He also shows that when the permeable strata become saturated by long-continued rains, they act as impermeable strata, and that the floods follow close on the rainfall besides being long maintained, so that in the remarkable and long wet winter seasons of 1658 and 1802 the Seine rose at Paris in the one case 29 feet, and in the other 24½ above its ordinary low level, and the floods in the last case lasted three months. M. Belgrand considers that this state of things was a normal condition during the Quaternary period, and he sees reason to believe that the rainfall at that period must have been very much greater than at present.

The ordinary low-water discharge of the Seine at Paris is 75 cubic metres per second; but during these great floods it rose to 2,400 and 2,000 cubic metres. M. Belgrand gives a list of eight such floods in the last two centuries, during which the discharge was above thirty times greater than the ordinary low-water discharge. In rivers flowing over more impermeable strata the difference is still greater; and he mentions that in the Loire at Orleans it has amounted to as much as 400 times, or 25 : 10,000. We may take the width of the Seine valley during the high-level gravel period at six kilometres, and during the low-level gravel period at about two kilometres; and M. Belgrand estimates that the river in flood had in the first instance a sectional area of 60,000 square metres, and in the second of 40,000 metres; and, calculating the flow at a given rate per second, the discharge, as compared with that of the present river, would be as under:—

Discharge per second of the Seine at Paris in the present period and during floods in past periods:—

		Extreme rise of river. Metres	Discharge of river. Cubic Metres
Present River	low water	8·81	75
	flood-water		2,400
Old River during the Quaternary period	low level stage 20 high level stage 13		27,000 to 60,000

Large as these Quaternary period quantities are, M. Belgrand thinks that there are cases of recent occurrence to prove that it is possible to realise them. He mentions a flood following on a heavy rainfall in the valley of the Armançon, a small river flowing over impermeable strata, with a basin of only 1,490 square kilometres, which had its discharge raised for a short time to 800 cubic metres per second; and he infers that under like conditions of rain and impermeability (by saturation and otherwise) the Seine, with its basin of 78,600 square kilometres, might have its discharge raised to 42,444 cubic metres, showing, that notwithstanding the size of the old river channels, the area drained during a period of greater rainfall would have sufficed for the necessary water supply.

In confirmation of this larger and more permanent supply of water, M. Belgrand instances the presence of the Hippopotamus, the remains of which are found at several places in the Seine basin as well as in that of the Somme, and which would have required for its existence larger and fuller rivers. He also derives a further argument in the presence of this animal, against a prolonged and severe winter cold, which he considers would have been fatal to it. M. Belgrand, nevertheless, argues that the presence of the Reindeer indicates the six summer months temperature of Scandinavia, not exceeding in the mean 8° centigrade; but with such a summer temperature we hardly see how he can avoid the three months' winter temperature of the same latitude or of 4·6 per cent. A still more extreme winter temperature is in fact indicated by the presence of the Musk Ox and the Marmot. It is to be observed also that the Reindeer at that time lived as far south as the Pyrenees, and that the physical condition of the drift deposits are, as we have before shown, strictly in accordance with a very low winter temperature. As the Hippopotamus is an extinct species, we do not know how far it may, like the extinct Elephants and Rhinoceroses, have been adapted to live in a severe climate. M. Belgrand's work is full of interesting details of the distribution of these and the other Quaternary animals, not only over the Seine Basin, but in some cases over the whole of France. He gives also a monograph with figures, by M. Bourguingnat, of all the mollusca of this age found in the Seine Basin. This well-known conchologist makes out that out of a total of 76 there are 38 new species which he considers as extinct, a conclusion which we expect English conchologists will hardly be prepared to agree with, as they have detected no extinct species in these deposits, and find only a few which are not local--a view in which we also believe most French conchologists join. The author considers that the same mammalian fauna is common to both the high-level and the low-level gravels. In one main point, however, do these gravels differ. In those of the high-levels of Montreuil and Bicêtre no Human remains, no Flint Implements, have been found, whereas, in those of the low-levels of Clichy, Grenelle, &c., above 5,000 flints, more or less worked, are stated to have been found by a single collector. Besides these works of early Man, M. Belgrand states that human bones, skulls, and entire skeletons, have been found in these lower gravels; but it seems to us that much of this evidence requires confirmation.

The Quaternary period of the Seine Basin is coeval, in M. Belgrand's opinion, with the Glacial period, and he considers that it was brought suddenly to a close with the low-level gravels. To this Quaternary period the peat deposits immediately succeed, owing, as the author ingeniously suggests, to the suddenly diminished rainfall leaving the rivers clearer and under conditions favourable for the growth of peat, which he shows never takes place in river valleys subject to frequent and heavy floods, but always in valleys where springs abound, and the floods are few and not turbulent.

The latter part of the work is occupied with a minute account of formation of gravel beds, and of the position of the Organic Remains, showing how all the features of those deposits are to be accounted for by ordinary river action, and that the mammalian remains are abundant precisely at those very places where a river with strong currents and numerous eddies would leave them. He endeavours to account also for the fact of all the bones of the larger animals being found in the coarser bottom beds of gravel, by the circumstance that these coarser beds were formed in those deeper water-channels along which only the larger carcases could have floated, and which were afterwards surmounted by those upper beds of sand and finer gravel, which he considers to be due to silting up (*alluvionnement*) of the channel where the river had changed its course to another channel. The brick earth or Loess is ascribed by him, as by English geologists, to river floods. But instead of considering it, as we do, to be produced by successive floods at all the various levels of the river, from the high to the low level, M. Belgrand admits but two levels, the high and the low, and that owing to a sudden elevation of the land, the excavation between these two levels was produced at once without intermediate stages. Consequently, he considers that the height of the Loess above these two levels marks in each case the rise of the flood waters. This, we think, is a weak point in his argument. According to his view, which he illustrates by a section, showing the range of the Loess up the hill slopes, he concludes that the floods of the low-level stage of the river rose, notwithstanding the width of the valley, to a height of 66ft., and during the high-level stage, to a height of 43ft., which give very much larger sectional areas for the river in flood than is otherwise necessary, and such as we conceive the area drained would have been insufficient to fill even with greatly larger rainfall. For, although the discharge of the Armançon may in a particular case of heavy rainfall have been so large as when multiplied by the whole area to give two-thirds of the required supply, still it is perfectly well known that the discharge by the main river never equals the sum of all its tributaries, and the discharge of the Seine at Paris on that occasion actually only rose to 1,250 cubic metres per second. There are besides beds of gravel on the slopes of Clichy towards Paris, and again on the slopes leading to Charenton distinct beds of gravel at intermediate levels, though of limited extent.

Thus, M. Belgrand ascribes the gravel beds and the Loess of the Seine Basin to old river action, referring the red loam alone of the higher plains above to diluvial causes, in opposition to the view usually received in France, according to which all these Drift beds are divided into the three diluvial deposits of *Diluvium gris*, *Diluvium rouge*, and *Limon* or *Loess*. As we have already expressed very similar views respecting the commonly accepted classification, we cordially agree with the author on this point.

The illustrations forming the second volume constitute a very interesting exhibition of the art of Photo-lithography. The execution varies a good deal, and there are plates which, though valuable for their truthfulness, are rather indistinct. Some of the representations of the Flint Implements are excellent. The work is somewhat large and costly; but as a copious record of facts, an ingenious statement of theory, and a reliable representation of specimens, this work of M. Belgrand will be greatly valued by all those who feel an interest in the remarkable phenomena connected with the present configuration of the country, the distribution of life during the Quaternary period, and especially with the evidence bearing on the Antiquity of Man.

J. P.

OUR BOOK SHELF

The Discovery of a New World of Being. By George Thomson. (Longmans, Green and Co., 1871.)

THE world discovered by this psychological Columbus is the "world of spirits," although he "disclaims all connection with so-called Spiritualists—a sect of modern times," whom he somewhat ungenerously "believes to be either dupes or knaves." Mr. Thomson believes that man consists of two "personalities," an animal personality or body, and a personality he calls spirit, which is the "knowing and conscious we," and which he believes to be as distinct from and as capable of being at almost any moment abstracted from the former as steam is from a steam-engine. Indeed, this latter phenomenon takes place every time the body "goes to sleep," to use the vulgar phrase; for Mr. Thomson believes that the "animal life never sleeps, and cannot sleep, and that to say or think that it, or any other life, can sleep, in the popular sense of the word, is the most glaring absurdity that ever has had possession of the human mind." "What is meant properly by sleep," he goes on to say, "is simply the abstraction or withdrawal of the influence of a being, a spirit, from a being, an animal, the leaving of a servant to itself, from the influence of its lord and master." Mr. Thomson explains the phenomenon of dreaming to be the struggles of this "being, a spirit," to get out of and back into the house of its servant, the body. The frequently unpleasant consequences of a late supper might have led Mr. Thomson one step further, and suggested to him the probable habitat of the spirit when embodied. How brimful of meaning to Mr. Thomson, then, must be Shakespeare's well-known utterance—"We are such stuff as dreams are made of." The particular merit which he claims for himself as a discoverer is, that he has realised to himself this spirit-world "predicted of old to be in existence," become conscious of himself as a "spirit in the world of spirits," clearly distinct, "in rounded belief," as he puts it, from that other entity, the body; and he declares that any one may make this awful discovery for himself if he only has "faith," shuts himself off from the outer world, and ponders long enough and with sufficient intensity. If our author is really in earnest—and we cannot but think he is—in trying to fathom the mystery of life and of consciousness, we recommend him to approach the subject unprejudicedly from the side of physiology; for so long as a psychologist concerns himself with the phenomena of his "inner consciousness" alone, and neglects the facts of his "outer man," his work is less than half done, and he is as likely to succeed in arriving at the whole truth as Columbus would have been in discovering America, had he contented himself with studying charts and staring longingly across the Atlantic for forty years.

On the Elevation of Mountains by Lateral Pressure; its Cause, and the Amount of it, with a Speculation on the Origin of Volcanic Action. By Rev. O. Fisher, M.A., F.G.S., &c. (From the Trans. of Camb. Phil. Soc. Vol. xi. part iii.)

THIS paper is of considerable interest as bearing upon the question of the internal condition of the earth. Mr. Fisher is of opinion that the elevation of mountain chains and the phenomena of volcanoes can both be accounted for on the hypothesis that the earth is solid. He conceives that "if a sufficient loss of heat has happened since the stratified rocks were formed, to cause a slight diminution in the volume of the earth, then the outer layer will have become too large, and will have had to accommodate itself to the reduced spheroid; and the lateral pressure caused by the resulting failure of support will have given rise to those foldings which have produced mountain ranges;" and an attempt is made by the author to "estimate the lateral pressure which would arise in the outer strata of the earth under such circumstances." Referring to the results obtained by Archdeacon Pratt in India, which seem to show that the density of the earth's crust beneath mountain chains is less than in other places, the author thinks this is only what might have been expected upon the supposition that the elevation of these mountains is due to lateral pressure; for it is evident that the strata would to some extent be supported by the lateral pressure which upheaved them. Here then, he thinks, may be the origin of volcanoes:—Diminished vertical pressure will enable the interior layers of the crust to pass into a state of fusion, and, "if from an independent cause a partial passage towards the surface is opened for molten rock containing highly heated water, the fluid will convey to a level where the resistance is less the pressure existing at a lower depth, and the force necessary to complete a passage to the surface may be furnished by the pressure of the molten rock and by the steam contained within it." But, although Mr. Fisher believes that the elevation of mountain chains and the phenomena of volcanoes are both of them the result of the same fundamental causes, yet, he thinks, it would certainly be a mistake to regard elevation as the consequence of volcanic action. He does not see how subterraneous lakes of molten matter can account for the elongated form which trains of volcanoes like those of the Andes affect; nor how such lakes should have shifted about from one region to another at different geological epochs. His theory, however, offers an explanation of the elongated form assumed by chains of volcanoes—the shifting of volcanic activity to different regions at successive periods—the spasmodic character of volcanic action, and other volcanic phenomena.

J. G.

LETTERS TO THE EDITOR

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

The Placental Classification of Mammals

A REMARK made by Prof. Allen Thomson on this subject in a late number of NATURE induces me again to draw attention to some objections I offered to the placental classification in a review of Prof. Rolleston's "Forms of Animal Life" (NATURE, vol. i., p. 81). If this system fails to satisfy so sound a critic and so accomplished an anatomist as Dr. Thomson, there must be some serious deficiencies in it. No doubt De Blainville did good service in calling attention to the wide distinction of Marsupials and of Monotremes from other mammals; but his names, *Ornithodelphia* and *Didelphia*, are inappropriate, and even misleading, and the skeletal characters of these two groups furnish quite as important, and far more available, means of diagnosis.

It admits of question whether the divisions of the higher mammals, according to the same system, are the most natural, even if the placenta were the best organ by which to define them. It is true, as Prof. Huxley observes, that the singularities which ally the elephant with the Rodentia have been a matter of common remark since the days of Cuvier, but the placental classification requires us to find still more singular ties between the elephant and the Carnivora. On the other hand the Carnivora lead down by the seals to the true Cetacea, a line of connection broken by the placental arrangement; which is equally opposed to the more doubtful analogy of the whales with the Ruminants. And the third order with deciduous zonary placentation, the isolated genus *Hyrax*, whatever may be thought of its relations to Rodentia on the one hand and to Ungulata on the other, has at least more likeness to either than to elephants and cats. Again, the different placentation of Edentata may be held only an additional proof of the looseness of an order held together chiefly by negative characters, but if we break it up, shall we obtain a more natural or convenient arrangement by placing the sloths with the Ruminants, *Manis* with Cetacea and Perissodactyla, and *Orycteropus* with Primates?

No doubt embryological characters are justly regarded as the most important for revealing true affinities between animals. But the tenacity of hereditary transmission, which gives them this value, does not appear to belong to placental structure. The placenta is more a maternal than a fœtal organ, especially as to its deciduate or non-deciduate character, and should rather rank with organs like the mammæ than with the yelk-sac and the amnion.

There are, moreover, many practical objections to the placental classification. The opportunities of obtaining knowledge on the subject are few, the investigation is not always easy, and it cannot be readily verified by subsequent observers.

But the most important objection to De Blainville's system is, that the perishable nature of the structures on which it is based renders it impossible to apply the criterion to fossil animals. It will probably be long before we shall have any notion of what a Sirenian placenta is like; it is only lately that we have learnt what is the real placentation of so common a creature as the rat, but we shall certainly never have the remotest idea of that of a megatherium, a zeuglodon, or a rhytina. So that if it be admitted—and surely no one will deny—that any classification of animals which is to be more than a mere aid to the memory, must include all known forms, recent or fossil, it follows that neither placenta, nor brain, nor any other soft part, can be of more than subordinate value in classification. On the other hand, it may be fairly maintained that there is no group of mammals, and scarcely one of the other Vertebrata, of undisputed importance, which cannot be completely defined by the characters of the skeleton.

It is, I venture to think, rather the authority of such illustrious names as Gegenbaur and Huxley than its own merits which have recommended the placental classification of mammals. If we regard the object of classification to be the setting forth of true genetic relationships, all characters must be included, and among them the placenta has no claim to be a primary index of affinity. And if we only seek for the most practically convenient way of arranging Mammalia, it is to the bones and teeth, rather than to the maternal organs of generation, that we must look.

P. H. PYE-SMITH

Potential Energy

WHILE on the subject of Thomson and Tait's Natural Philosophy, I should like to call attention to the definition of Potential Energy, given in Art. 273, p. 189.

I think it will be found that this definition gives the wrong sign, because the potential energy in any configuration is the amount of work the forces of the system perform in *returning* to the zero configuration, the ideal position of stable equilibrium.

Thus when a spring is stretched or compressed the potential energy is measured by the kinetic energy which is generated by the work done by the elastic force of the spring by the time the spring has returned to its unstretched condition. With this change of sign the definition now agrees with that given in Art 484.

Infinite distance being taken as the zero configuration, the potential energy is a positive quantity for such forces as electric and magnetic forces.

With this zero the potential energy for gravitating particles is negative, which is expressed by saying that the exhaustion of potential energy is positive, because as the particles approach their kinetic energy increases, and their potential energy suffers exhaustion and diminishes.

In Art. 485 we read, "The potential at any point, due to any attracting or repelling body or distribution of matter, is the mutual potential energy between it and a unit of matter placed at that point. But in the case of gravitation, to avoid defining the potential as a negative quantity, it is convenient to change the sign. Thus the gravitation potential at any point, due to any mass, is the quantity of work required to remove a unit of matter from that point to an infinite distance."

Although the gravitation potential has had its sign changed, nevertheless the potential at any point P for gravitation and for electric and magnetic forces, is defined in the same way as the sum of the quotients of every portion of the mass divided by its distance from P.

This is the Potential Function of Green, usually called by the name given by Gauss, the Potential, and is the function which satisfies Laplace's equation.

The gravitation potential is the old force-function of Sir W. Hamilton and Jacobi, such that its rate of increase in any direction is the resolved part of the force in that direction on the unit of mass.

The potential, defined as the potential energy in the unit of mass is of opposite sign to the free function; its rate of decrease in any direction is the component force in that direction.

These perplexing changes of sign arise from the fact that in gravitation we have only one kind of matter, the particles of which naturally attract; hence the potential energy is negative, or it diminishes as the particles approach; it is, therefore, convenient to make a change of sign.

In the general case of which electrical and magnetical phenomena may be taken as the type, like particles repel, unlike attract, and the potential energy increases as the particles approach.

These definitions and conventions of signs are, of course, in accordance with those given by Thomson and Tait; the proper signs and names are given also in Briot's "Théorie Mécanique de la Chaleur," but in all the other French books there is great confusion; for instance, in the "Théorie Mécanique de la Chaleur" of Verdet, the potential goes by Green's name, the potential function, but has its sign changed, while the potential energy is called the potential, after Clausius. This also seems to be the nomenclature adopted by the Germans.

It is very necessary that all doubt as to the meaning and value of these important functions should be set at rest; the system adopted in Thomson and Tait's "Natural Philosophy" leaves nothing to be desired.

A. G. GREENHILL

St. John's College, Cambridge, March 6

Development of Barometric Depressions

I LEAVE to those who are equal to it the task of reconciling and discussing "J. K. L.'s" propositions in reference to Indian meteorology, which appear to be these:—1, "The rainfall in the Himalayas" (instanced by him in proof that rainfall is not the cause of depression), "probably causes a very great depression" (meaning, I now suppose, the great Asiatic depression really due to the rarefaction of the air in Central Asia); 2, "but certainly not any currents such as I have described" (viz., currents in accordance with Buys Ballot's Law, having the lowest pressure on their left); 3, "the circuit of the wind in the region of the Himalayas is, so far as we know, in exact accordance with Ballot's Law."

My complaint was that the critic had ignored, not, of course, Part II. of my book, but certain propositions in Part I., as "distinctly enunciated" as those on which he comments, and inseparable from them, though not yet fully discussed.

I will now close, as far as my part is concerned, a discussion, for the opening of which I was responsible, but which has, contrary to my intention, become rather personal than scientific. The question, however, really at issue between us I believe to be one of some interest in meteorology. "Does the fact that precipitation in certain cases, and especially in the warmer regions of the globe, fails to produce baric depression, disprove, or render improbable, the theory (based on substantial evidence) that the depressions which occur in Western Europe are results of precipitation?"

March 10 W. CLEMENT LEY

A Safety Lamp

THE article in this week's NATURE on "Foul Air in Mines, and how to live in it" calls to mind a contrivance made use of by the watchmen of Paris in all magazines where explosive or inflammable materials are stored, and suggests the idea that the same may possibly be of service to our miners.

The Paris *Figaro* says, "Take an oblong vial of the whitest and clearest glass, put in it a piece of phosphorus about the size of a pea, upon which pour some olive oil, heated to the boiling point, filling the vial about one-third full, and then seal the vial hermetically. To use it, remove the cork, and allow the air to enter the vial, and then re-cork it. The whole empty space in the bottle will then become luminous, and the light obtained will be equal to that of a lamp. As soon as the light grows weak its power can be increased by opening the vial and allowing a fresh supply of air to enter. Thus prepared the vial may be used for six months."

4, Moreton Place, S.W. B. G. JENKINS

Beautiful Meteor

I ENCLOSE a description of meteor, apparently of unusual brilliancy, recently seen by my assistant at Parsonstown, thinking that it may perhaps be interesting to some of your readers.

Carlton Club, London, March 12 ROSSE

"Observed an intensely brilliant meteor. It was first seen in the region about Lepus, whence it moved with a slow and steady motion across the heavens to the S.E. horizon, where it gradually disappeared in a bank of cloud at about $9^h\ 5^m\ 19^s$, Greenwich mean time, having occupied seven or eight seconds in moving over 50° of a great circle. The time given may be a few seconds wrong, as it was noted by an ordinary watch. The head was intensely brilliant, of a bluish white colour, and lighted up the whole sky.

"Its brightness was maintained during its entire visibility, and may have been as great as the moon at quadrature. Apparent diameter of the head 42′. It was followed by a very narrow tail about 3° in length and of a reddish hue. It did not leave any phosphorescent train behind it, but at the latter part of its course it threw out some reddish luminous masses, that gradually faded away. Its apparent course was in a great circle through β Canis Majoris to a point near the S.E. horizon, in azimuth S. $28\frac{1}{2}°$ E., and altitude $8\frac{1}{2}°$. For β Canis Majoris the azimuth was S. 20° 52′·4 W., and altitude 16° 43′·3.

"Observatory, Birr Castle, March 8"

WHILE travelling last night, at about twenty minutes to nine o'clock, as we were descending a tolerably high hill, about 5 miles from this city, our road leading S.S.W., I found myself very favourably circumstanced for seeing a beautiful meteor which was visible for probably forty seconds. It appeared first as if approaching from the W.S.W. about 40° or 50° above the horizon, unusually large and bright, and leaving a long train of bright spots behind. After a few seconds it seemed extinguished, but in a moment or two flashed out again still brighter, apparently passing due E., at a height of about 25° or 30°, through Eridanus, Lepus, Canis Major, and Argo, and much slower than at first. While passing under Orion two protuberances burst out, giving it the appearance of an arrowhead, or rather a bird flying, as it appeared to have a tail which at the end was a fine smoke colour: it now occupied the space of 1½° or 2°. Passing behind a cloud below Regulus it disappeared.

Waterford, March 9 JAMES BUDD

"Whin"

CAN you or any of your readers furnish a probable etymology of the word *whin?* Over all the north of England and south of Scoland basalt is so called. Here we have the *whin*-sill or stratiform basalt—*whin*-dykes, or geological fissures filled with basalt. The vocabularies in treatises on geology give no derivation of this prevalent mining term. In Scotland *whin* seems to typify the hardest mineral known. Burns makes Death say in "Hornbuik," "I micht as weel hae tried a quarry o' hard *whin* rock." Surely a satisfactory root for the word in question can be found in Celtic, Old Norse, Danish, or Anglo-Saxon! The Old Norse "fors" is found in the names of several local waterfalls, as for instance "High Force" in Teesdale. At this "force" the river Tees is precipitated over a whin-stone cliff, 80ft. high.

WM. R. BELL

Laithkirk Vicarage, Mickleton, March 12

CUCKOO AND PIPIT

SEVERAL well-known naturalists who have seen my sketch from life of the young cuckoo ejecting the young pipit (opposite p. 22 of the little versified tale of which I send a copy)* have expressed a wish that the details of my observations of the scene should be published. I therefore send you the facts, though the sketch itself seems to me to be the only important addition I have made to the admirably accurate description given by Dr. Jenner in his letter to John Hunter, which is printed in the "Philosophical Transactions" for 1788 (vol. lxxviii., pp. 225, 226), and which I have read with pleasure since putting down my own notes.

The nest which we watched last June, after finding the cuckoo's egg in it, was that of the common meadow pipit (Titlark, Mosscheeper), and had two pipit's eggs besides that of the cuckoo. It was below a heather bush, on the declivity of a low abrupt bank on a Highland hill-side in Moidart.

At one visit the pipits were found to be hatched, but not the cuckoo. At the next visit, which was after an interval of forty-eight hours, we found the young cuckoo alone in the nest, and both the young pipits lying down the bank, about ten inches from the margin of the nest, but quite lively after being warmed in the hand. They were replaced in the nest beside the cuckoo, which struggled about till it got its back under one of them, when it climbed backwards directly up the open side of the nest, and hitched the pipit from its back on to the edge. It then stood quite upright on its legs, which were straddled wide apart, with the claws firmly fixed half-way down the inside of the nest among the interlacing fibres of which the nest was woven; and, stretching its wings apart and backwards, it elbowed the pipit fairly over the margin so far that its struggles took it down the bank instead of back into the nest.

After this the cuckoo stood a minute or two, feeling back with its wings, as if to make sure that the pipit was fairly overboard, and then subsided into the bottom of the nest.

As it was getting late, and the cuckoo did not immediately set to work on the other nestling, I replaced the ejected one, and went home. On returning next day, both nestlings were found, dead and cold, out of the nest. I replaced one of them, but the cuckoo made no effort to get under and eject it, but settled itself contentedly on the top of it. All this I find accords accurately with Jenner's description of what he saw. But what struck me most was this: The cuckoo was perfectly naked, without a vestige of a feather or even a hint of future feathers; its eyes were not yet opened, and its neck seemed too weak to support the weight of its head. The pipits had well-developed quills on the wings and back, and had bright eyes, partially open; yet they seemed quite helpless under the manipulations of the cuckoo, which looked a much less developed creature. The cuckoo's legs, however, seemed very muscular, and it appeared to feel about with its wings, which were absolutely featherless, as with hands, the "spurious wing" (unusually large in proportion) looking like a spread-out thumb. The most singular thing of all was the direct purpose with which the blind little monster made for the open side of the nest, the only part where it could throw its burthen down the bank. I think all the spectators felt the sort of horror and awe at the apparent inadequacy of the creature's intelligence to its acts that one might have felt at seeing a toothless hag raise a ghost by an incantation. It was horribly "uncanny" and "grewsome." J. B.

The University, Glasgow

* "The Pipits," illustrated by Mrs. Hugh Blackburn (Glasgow: Maclehose, 1872).

DR. G. E. DAY

IN a former number, under the date of February 8, we had the painful duty of announcing the death, at the age of fifty-six, of Dr. George Edward Day, F.R.S., Emeritus Chandos Professor of Medicine in the University of St. Andrews, which took place at Torquay on January 31, 1872. Most of his earlier friends had probably heard of the sad accident which reduced him to a state of bodily helplessness, and which darkened his latter years; but few of those who remembered him only as the genial witty Cantab, overflowing with life and spirits, and as the brilliant medical student at Edinburgh, carrying everything before him in class-room and debating hall, or later, as the active untiring President of the Medical Examinations at St. Andrews, would have supposed him capable of the cheerful resignation with which he submitted to his enforced exclusion from all participation in active, professional, and social life.

The story of Dr. Day's life is a sad record of brilliant expectations suddenly wrecked, and long continued struggles against irreparable calamities.

As the eldest son of a wealthy country gentleman of good position, his fortune seemed assured from his birth; but the failure of the Swansea Bank in 1825, when he was scarcely ten years old, ruined his father, and led to his removal to the house of a widowed grandmother.

In 1834, after some preparation under a private tutor, he went up to Cambridge with the reputation of an able mathematician, and a good classical scholar. At the University he worked splendidly by fits and starts, but the period between 1834 and 1837 does not belong to the working era of Cambridge, and George Day's natural love of fun and the fascination of his manner combined to render his society especially attractive to his comrades, and the result was, that he came out as low as twelfth among the wranglers of his year.

On leaving Cambridge he resolved to adopt medicine as his future profession, and went to Edinburgh, where he at once took his place among that brilliant band of young men who reckoned John Goodsir, Edward Forbes, and many others of similar promise amongst their ranks. On leaving Edinburgh he at once came to London, and taking a house at the West End, attempted to establish himself as a pure physician. During these eight or nine years of his London life, Dr. Day laboured on with unwearying industry and patience, lecturing at the Middlesex and other metropolitan medical schools, writing for reviews, translating from German, and turning his versatile talents and his special knowledge of physiological chemistry to account in every way. The result of this heavy strain was a threatening of brain disease, which, according to the verdict of his medical advisers, could only be warded off by complete rest and cessation from the cares in which he was immersed.

At that moment the death of an old friend, Dr. John Reid, opened the prospect to him of obtaining the Chair of Medicine at St. Andrews. His success in this probably saved his life, for the removal from the turmoil of a struggling London career to the comparative ease of the Scottish University arrested the threatenings of disease, and enabled him to recover some of his old vigorous tone. During the 13 years that Dr. Day held the Chair of Medicine at St. Andrews, from 1850 to 1863, he made it his special duty to promote the honour and further the interests of the University by raising the character of medical degrees; and so successfully did he accomplish this task, that the discredit which had belonged in former days to the M.D. degree of St. Andrews was completely effaced under his presidency of the Examining Board. A new system of stringent *vivâ voce* and written examinations was then inaugurated, which justified those who graduated in his time in regarding their attainment of the M.D. degree of St. Andrews as a professional honour of which any man might be proud.

In 1857 Dr. Day's prospects of a more prosperous future than he had as yet been able to look forward to were completely destroyed by the accident to which we have already referred, and which befell him in the course of a vacation tour in the English Lake District. On a bright morning at the end of the August of that year, he had set forth from his hotel at Patterdale in full vigour and strength, bent on "learning a new wrinkle about Helvellyn," as he himself expressed it, by making his way to the summit along a recently opened path. He made the ascent as he had designed, but instead of returning by the same track, he struck off in the direction of the white lead mines; and while walking along what he mistook for a miner's path, the ground gave way under him, and he fell into what proved to be a horizontal chimney or culvert, constructed to carry off the sulphurous, arsenical, and other gases, whose deposits had proved injurious to the sheep grazing on the hill side. He was rescued after three hours of anxious suspense, but the proximate results of that accident were dislocation of the right elbow and two fractures of the same arm, the upper one in the surgical neck of the bone of the humerus, which never united. The subsequent effects were the complete destruction of his general health, which obliged him in 1863 to give up the Chair of Medicine at St. Andrews and retire from active life. A removal to the milder climate of Torquay had little effect in arresting the train of symptoms which year by year marked the progress of disease, and were, it is conjectured, the result of a jar to the spine sustained by his accident on Helvellyn, which had, in truth, proved to him the beginning of the end.

And such was the checkered career of this man of brilliant promise, unflinching bravery of spirit, clear judgment, and tender heart. Disappointed again and again, he always met his troubles manfully, and turned them to good account for himself or others. We have given no list of the various honours which he attained in his profession, or of his literary works, for the detailed reports of these particulars are contained in the various obituary notices which have appeared of Dr. Day in the medical and other journals, to whose pages, as well as to our own, he was a frequent contributor.

OCEAN CURRENTS

A NEW interest seems now to be taken in Ocean Currents, and much is being said and written upon the subject. In the investigation of this subject it is very important that we should understand well all the forces and agencies concerned in the production and maintenance of the currents, and that we should consider well all the principles, and theories based upon hypothetical forces, which have come down to us from preceding generations, however plausible and however much sanctioned by high authority they appear to be. As in the case of the winds, so also in ocean currents, the modifying force arising from the earth's rotation has a very important bearing, and should be well understood. There are certain erroneous views in connection with this force, which have come down to us from preceding generations, and which are contained in text-books, and are being taught in colleges and schools, which are liable to have, and do have, a mischievous bearing upon this subject. These are the more dangerous because they appear to have received at least the tacit sanction of past ages, so that almost any one is liable to adopt them without much consideration. Prof. Colding has in this way been unsuspectingly let into error in his recent paper on ocean currents. We are all familiar with the usual explanation of the trade-winds contained in text-books, which assuming that a particle of air at the equator, at rest relatively to the earth, and consequently having a lineal velocity in space of about 1,000 miles per hour, is forced to move toward the pole, it will, on arriving at the parallel of latitude where the earth's surface has a velocity of only 900 miles, still have its velocity of 1,000 miles per hour in the case of no friction, and consequently have a relative velocity of 100 miles per hour, and on arriving at the parallel of 60°, will still have its initial velocity of 1,000 miles, and consequently have a relative velocity of 500 miles per hour. But this is at variance with a fundamental and well-established principle in mechanics. The force in this case is a central force, or at least the compound perpendicular to the earth's axis can be neglected, since it can have nothing to do with any east or west motion. This being the case, the principle of the preservation of areas must be satisfied, and consequently the particle of air, when it arrives at the parallel where the earth's surface has a velocity of 900 miles, must have a velocity of more than 1,000 miles, and a relative velocity of more than 200 miles per hour, and on arriving at the parallel of 60°, where the earth's surface has a velocity of 500 miles, it must have a velocity of 2,000 miles, and consequently a relative velocity of 1,500 miles, instead of 500 miles per hour. Adopting thoughtlessly, and very naturally, the erroneous principle which is usually taught, that a particle of air or of water in moving toward or from the pole, tends to keep its initial lineal velocity relative to space, Prof. Colding estimates the amount of deflecting force due to the earth's rotation, eastward when the particle is moving towards the pole, and westward when moving from the pole, and the result is, that his force is just one half of what it really is. Consequently, all the results based upon his estimated amount of this force should be doubled. Prof. Colding has also entirely neglected one component of the force due to the earth's rotation. It has been shown by Prof. Everett, and also by the writer, that when a body moves east or west, there is also a similar deflecting force due to the earth's rotation, exactly equal to the former. Prof. Colding has, therefore, taken into account only the one-fourth part of the whole force. If he had taken in this latter component of the force also, and resolved it in the direction of the line of motion and perpendicular to it, as he did the former, he would have found that the parts in the direction of motion, arising from both components, exactly cancel one another in all cases, and that the resultant of both components is a force perpendicular to the direction of motion. This force then tends only to change the direction of the motion, and never to accelerate or retard it, in whatever direction it may be. Prof. Colding's result, therefore, that the velocity of the current is accelerated by the earth's rotation, when moving in certain directions, and retarded in others, is erroneous.

It is known that there are two theories with regard to the cause of Ocean Currents: the one, that they are caused by the winds acting upon the ocean, the other, advocated by Dr. Carpenter, that they are caused by a difference of density of the ocean between the equator and the poles, due to a difference of temperature. The tendency of both theories is in the same direction, and the currents, no doubt, are in some measure due to the forces belonging to each theory. The history of the former theory, and the high authority which can be appealed to in its support, are well known, but we have reason to think that the forces, and the effects of them, in the former theory, are quite subordinate to those of the latter. The well-known explanation of the Gulf Stream by the former theory assumes that there is a heaping up of the water of the ocean in the Gulf of Mexico by the action of the trade winds, sufficient to change the sea-level enough to cause the observed current passing through the Strait of Florida. But the trade winds cannot have much effect in causing a heaping up of the water on the coast of Mexico, since the force is applied to the surface merely, and tends to produce only a surface current, while all the great body of the water, except a little of the surface, is free to flow back. It is true there must be a slight change of sea-level to give rise to a force sufficient to overcome the resistances to this under tow, but these are extremely small since the velocity of this under tow, including all the great depth of the ocean, except the superficial westward current, is very small. That the merely superficial part of the equatorial current is mostly caused by the trade-winds may be true, but the Gulf Stream, which is not directly acted upon, except by the very gentle south-west winds, and which is not merely a surface-current, must be mostly accounted for by the other theory. Let us now see what can be learned upon this subject from observation. Instances of a great change of water-level in shallow canals have been cited to show the influence of the wind in causing a heaping up of the water at the one end; but the water in these cases being very shallow, the force may be regarded as applied somewhat to the whole body of the water, and the under counter-current is thus prevented, but the case is very different in a deep ocean. It is well known from the discussion of tidal observations that the influence of the wind in changing the sea-level is very small. If the force of the trade winds causes a higher sea-level in the Gulf of Mexico, we know that the west winds in higher latitudes must cause a similar rise of sea-level on the west coast of Europe, for the sum of the moments with reference to the earth's axis, of the forces, west between the tropics and east in higher latitudes, must exactly balance each other. If the explanation of the Gulf Stream requires that the level of the Gulf of Mexico should be raised about twelve feet, as shown by Prof. Colding, then there must be about an equal change of level on the west coast of Europe, if these changes are caused by the winds; for although the extent of coast receiving the west winds may be greater than that receiving the east winds, yet this is counterbalanced by the circumstance that the force of the west winds acts at a less distance from the earth's axis, which requires that they should be stronger. If, then, the west winds cause a change of sea-level on the coast of Europe, say of ten feet, then any change in the force of these winds at different seasons must cause a very perceptible change of sea-level. Now, we know that the force of the west wind on the Atlantic Ocean is considerably greater in the spring than the autumn. There should therefore be a corresponding difference in the mean level of the sea, and this mean level on the coast of Europe should be greatest in the spring. But the discussion of the tidal observations made at Brest, shows that the mean level of the sea, after being corrected for the barometer and a very small astronomical term affecting the mean level, is about four inches lower in the spring when the winds are strongest than in the autumn when they are weakest. (Proceedings of the American Academy of Sciences and Arts, vol. vii. p. 32.) The discussion, likewise, of the tides of Boston Harbour gives a similar result, except that the range of the monthly means is still less, being less than three inches. (U.S. Coast Survey Report for 1868.) These results should receive the attention of those who maintain that great changes of sea-level are caused by the winds.

In a paper by the writer, published in *Silliman's Journal* (second series, vol. xxxi. p. 45) there are several pages given to the subject of ocean currents, in which it is maintained that the principal agency in their production is difference of temperature of the sea-water between the equator and the poles. The principal effects of the earth's rotation are there given, which are too numerous to be recited here. In addition to the results there given, the following additional thought may be given here as being perhaps new. As the surface-water flows toward the poles the deflecting force of the earth's rotation presses it toward the east. In like manner as the water below flows toward the equator, there is a similar force pressing it toward the west. These forces are small, but they must nevertheless cause a gradual rising of the cold water at the bottom on the American coast, and this, perhaps more than the Greenland current, causes cold water there. The Gulf Stream of warmer water cuts its way through this cold water gradually rising from the bottom, and hence the cold walls observed by the U.S. Coast Survey.

Mr. Croll seems committed to the wind theory, and is unwilling to admit that the theory advocated by Dr. Carpenter can have even a subordinate effect. His principal argument is based upon an experiment of M. Dubuat. I know not under what circumstances this experiment was made, but of course it was with a comparatively shallow canal or stratum of water, and the result is no doubt correct for the depth of water with which the experiment was made. A much less force on each particle of a large body of water is sufficient to overcome the cohesion of the particles, and produce motion than upon a small one, just as a small drop of water remains suspended to a twig, while the same force of gravity causes a large one to drop off. The case therefore of the ocean is very different from that of a shallow canal. As Mr. Croll insists that Dr. Carpenter's experiment, to be applicable to the case, should have been made with a canal 120 feet long, and only one inch deep; so it might be insisted that M. Dubuat's experiment, to be applicable to Mr. Croll's case, should be made with a canal or body of water three or four miles deep. But there is no necessity for us to make any such experiments, for nature is performing the experiment regularly every six hours, and all that we have to do is to observe. The attraction of the moon changes the level due to the attraction of the earth alone, and puts the ocean, as it were, upon an inclined plane with a gradient of about two feet in the distance of a quadrant, and the water slides down, causing a rising of the tide at one place and a falling at another; and in six hours this gradient is reversed, and the reverse motion of the water follows, thus causing the regular ebbing and flowing of the tides. If M. Dubuat's experiment were applicable to the ocean, the moon could not cause a tide at all unless its mass were about fifteen times greater.

Cambridge, Mass., U.S.A. W. FERREL

FERGUSSON'S RUDE STONE MONUMENTS*

IN Mr. Fergusson's "Handbook of Architecture," published in 1854, one chapter of about fifty pages is devoted to Megalithic, or, as he prefers to call them, Rude Stone, Monuments. Ever since that period he has been collecting materials on this interesting subject, and the result is now before us, in the work which forms the subject

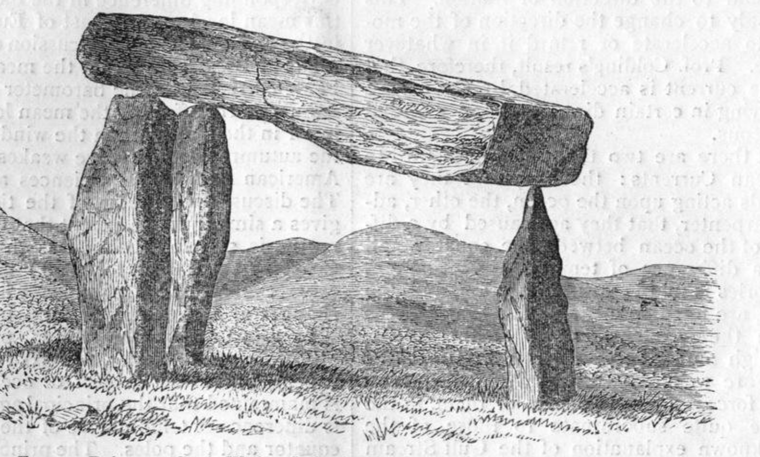

FIG. 1.—Dolmen at Castle Wellan, Ireland. From a drawing by Sir Henry James.

of this notice. In it he confines himself to the classes of monuments indicated in the title, omitting all reference to hut circles, Pict's houses, brochs, and other buildings composed of smaller stones; not because he doubts that they belong to the same period, "but because their age being doubtful also" it would only complicate the

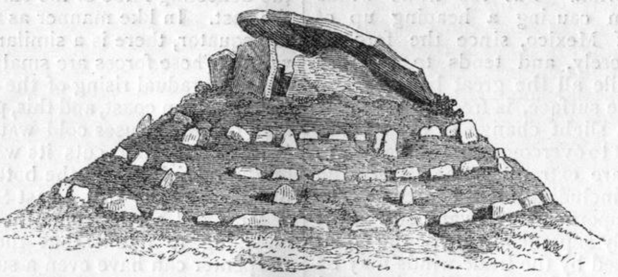

FIG. 2.—Dolmen de Bousquet. From a drawing by E. Cartailhac.

argument to introduce them. He limits himself therefore to tumuli, menhirs or stone pillars, stone circles, avenues, and dolmens. All these we find sometimes singly, sometimes in combination, the tumulus containing a dolmen, being surrounded by one or more stone circles, and surmounted by a menhir. Fig. liii., representing the celebrated

FIG. 3.—Nine Ladies, Stanton Moor. From a drawing by L. Jewitt.

tumulus of New Grange, near Drogheda, gives a good idea of the large barrows; it was originally surrounded by a circle of stones, most of which, however, have disappeared. Fig. 3 represents the stone circle, known as the Nine Ladies on Stanton Moor.

The typical "Dolmen" may be described as a massive

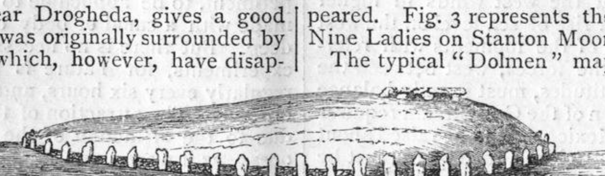

FIG. 4—Long Barrow, Kennet, restored by Dr. Thurnam. *Archæologia*, xii.

stone resting on three supports; the celebrated Kits Coty House, near Maidstone, may be regarded as a typical example. Fig. cvii. represents one at Halskov, in Denmark, raised on a small mound, and surrounded by a circle of stone. Fig. 1, representing a Dolmen at Castle Wellan, Ireland, and Fig. 6, one at Grandmont, in Bas Languedoc, are more ex-

* "Rude Stone Monuments." By James Fergusson, D.C.L., F.R.S. (London: John Murray, 1872.)

ceptional types. Dolmens are sometimes covered by a mound of earth (like the Gib Hill example, excavated by Mr. Bateman), sometimes free, as in the preceding figures. That all the earlier ones were covered, says Mr. Fergusson, "is more than probable, and it may since have been originally intended to cover up many of those which now stand free; but it seems impossible to believe that the bulk of those we now see were ever hidden by any earthen covering."

The tumuli which contain megalithic chambers closely resemble the dwellings even now used by many northern nations, the Siberian Yurt, for instance, consists of a central chamber, generally sunk a little below the surface, built of stones or timber, and heaped over with earth, so as to form a mound. The Tchutski huts are very similar. "They are," says Captain Cook, "sunk a little below the surface of the earth. One of them which I examined was of an oval form, about twenty feet long and twelve or more high. The framing was composed of wood and the ribs of whales, disposed in a judicious manner, and bound together with smaller materials of the same sort. Over this framing is laid a covering of strong coarse grass, and that, again, is covered with earth, so that, on the outside, the house looks like a hillock supported by a wall of stone three or four feet high, which is built round the two sides and one end."

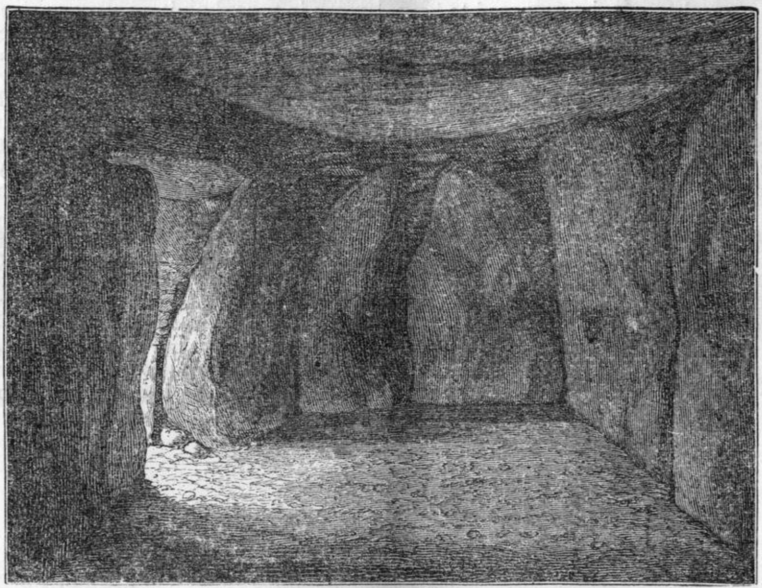

FIG. 5.—View of Interior of Chamber at Uby. From Madsen.

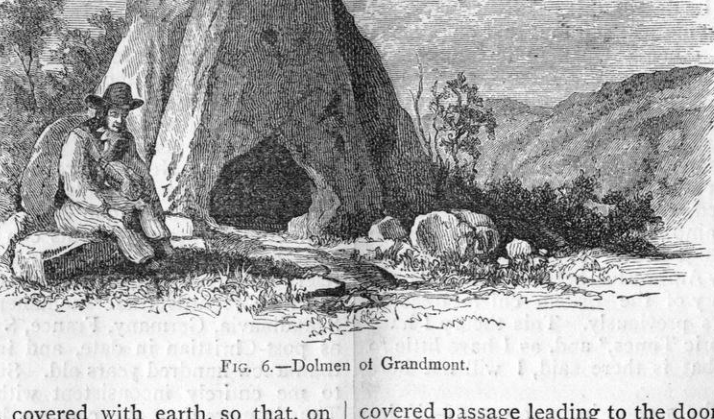

FIG. 6.—Dolmen of Grandmont.

The huts of the Esquimaux and Lapps are built on the same model, and have generally a longer or shorter covered passage leading to the door, the object of which is to keep the cold out of the central chamber. Round the walls of the latter are ranged seats for the inmates, and part of the space is often separated off by partitions. So closely do many of our Northern tumuli correspond to these descriptions, that Nilsson long ago suggested many of them having been originally used as dwelling places, and converted subsequently into tombs. Fig xi., for instance, represents the chamber of a tumulus near St. Helier, in Jersey. Here we have the central room, with partitions, and the passage leading to the door. In some few cases the dead have been found sitting round the sepulchral chamber, with their arms and implements by their side, just as they may be supposed to have sat during life. Fig. 5 represents the chamber of a tumulus at Uby in Denmark. Stonehenge itself (Fig. 8) seems to be constructed on the same model: the mound, however, being absent, or only represented by the encircling ring of earth.

In determining the date of particular tumuli, Mr. Fergusson seems to me to attach too much importance to objects found on, or near the surface, and which often have no doubt been accidentally dropped, or belong to secondary interments. Thus he refers to the two objects of iron found at Gib Hill, as if they justified us in ascribing that interesting tumulus to the iron age. But Mr. Bateman, by whom that mound was opened, expressly states that the objects of iron were not found in the central cist, but they belonged to a secondary interment. They throw, therefore, no more light on the date of Gib Hill itself than the fragments of ginger-beer bottles which abound in the area of Stonehenge do on the period to which it belongs. This is a consideration which is of great importance; because the history of these megalithic monuments, the race by whom, and the date at which they were constructed, are most interesting questions of archæology. Although few now regard Stonehenge as a Druidical temple, still archæologists are almost unanimous in regarding it as very ancient; while the class of megalithic monuments they consider to have begun in pre-historic times, and to have continued in out-of-the-way parts down to a comparatively recent period. Mr. Fergusson, on the contrary, is of a different opinion. He endeavours to show that these monuments belong to one period, and to comparatively recent times:—

"However this may be," he says, "I trust that this work may lay claim to being, in one respect at least, a contribution to the cause of truth regarding the much-disputed age and use of these rude stone monuments. It states distinctly, and without reserve, one view of the mooted question, and so openly, that any one who knows better can at once pull away the prop from my house of cards and level it with the ground. If one thing comes out more clearly than another in the course of this investigation, it is that the style of architecture to which these monuments belong is a style, like Gothic, Grecian, Egyptian, Budhist, or any other. It has a beginning, and middle, and an end; and though we cannot yet make out the sequence in all its details, this at least seems clear—that there is no great hiatus; nor is it that one part is pre-historic, while the other belongs to historic times. All belong to the one epoch or the other. Either it is that Stonehenge and Avebury, and all such, are the temples of a race so ancient as to be beyond the ken of mortal men, or they are the sepulchral monument of a people who lived so nearly within the limits of the true historic times that their story can easily be recovered."

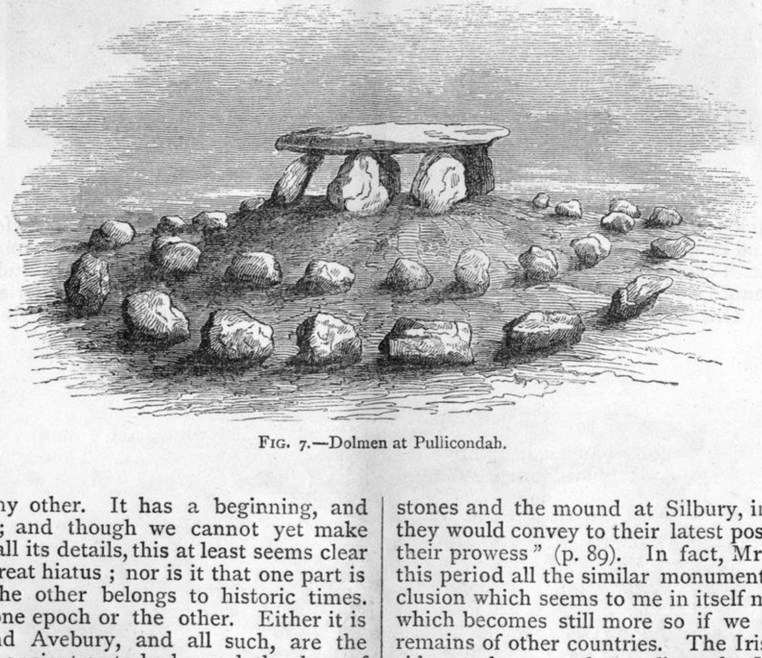

FIG. 7.—Dolmen at Pullicondah.

As already mentioned, the latter is Mr. Fergusson's view. Almost alone among English archæologists, he considers that Stonehenge is part Roman, and believes it to have been erected by Ambrosius, between the years 466 and 470 A.D., in memory of the British chiefs treacherously slain a few years previously. This theory I have discussed in "Pre-historic Times," and, as I have little to alter in, or add to, what is there said, I will not here repeat my arguments.

As regards Abury, the second in importance—if, indeed, it be the second and not the first of these monuments Mr. Fergusson says:—"I feel no doubt that it will come eventually to be acknowledged that those who fell in Arthur's twelfth and greatest battle were buried in the ring at Avebury, and that those who survived raised these stones and the mound at Silbury, in the vain hope that they would convey to their latest posterity the memory of their prowess" (p. 89). In fact, Mr. Fergusson refers to this period all the similar monuments in England, a conclusion which seems to me in itself most improbable, and which becomes still more so if we consider the similar remains of other countries. The Irish examples he considers to be somewhat earlier; the Moytura remains, for instance, being perhaps as early as the first century B.C. As regards the North, he regards the celebrated tumulus of Maes Howe as probably the "tomb of Havard, or of some other of the Pagan Norwegian Jarls of Orkney;" while the Stones of Stennis can hardly, he thinks, "be carried back beyond the year 800," to which period he refers all the megalithic remains in those islands. In short he regards these monuments, whether in Britain, Scandinavia, Germany, France, Spain, Algeria, or India, as post-Christian in date, and in many cases not more than a few hundred years old. Such a conclusion seems to me entirely inconsistent with architectural history. Thus in more than one case we know of early churches, probably belonging to the 10th or 11th centuries, which are constructed over dolmens.

Mr. Fergusson admits that the great tumulus near Sardis (Fig. 1, p. 31) is rightly identified as the tomb of Alyattes, was erected in the sixth century, B.C., and was described by Herodotus; that some of the tumuli on the eastern shores of the Mediterranean are certainly "as old as the thirteenth century, B.C.: that the practice of burying in tumuli must have existed for many centuries before such tombs could have been constructed; and that the age in which they "were erected was essentially the age of bronze: not only are the ornaments and furniture found in the Etruscan tombs generally of that metal, but the tombs at Mycenæ and Orchomenos were wholly lined with it;" a fact which is the more interesting when we remember that all the metallic objects found in the tumuli round Stonehenge were of bronze.

Again, let us consider the class of monuments which consist of a free dolmen standing on a mound, and surrounded by one or more stone circles. This type is very widely distributed. A Danish example has already been given, Fig. 5. Fig. 4 represents the long barrow at Kennet, near Marlborough, after Dr. Thurman; Fig. 2 is the Dolmen de Bousquet in the Aveyron; lastly, Fig. 7 is a similar monument at Pullicondah, near Madras. These tumuli, though differing in detail, are identical in all essential

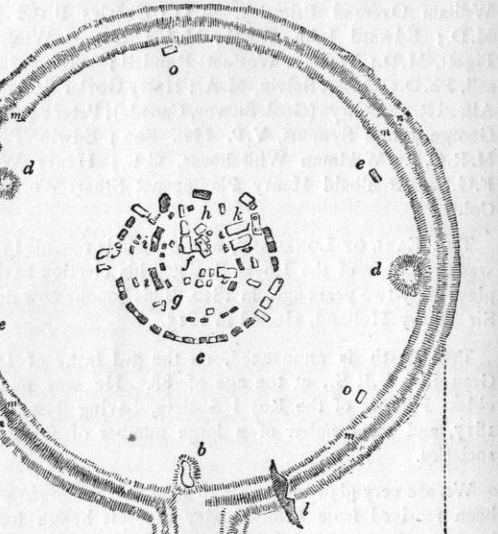

FIG. 8.—General Plan of Stonehenge, from Knight's "Old England."

points. If these monuments all belong to post-Christian times, they must have been erected by very different races of men. Mr. Fergusson, indeed, admits that they are the work of very different races; how then does he account for the remarkable similarity existing between them? He denies that the Celts, Scandinavians, or Iberians were themselves naturally "rude stone builders," and endeavours to remove the difficulty by an explanation which is most important, because it seems to me to involve the practical abandonment of the conclusion, which, as he told us in the preface, is the central feature of his work. This style of art, he says, "seems to have been invented by some pre-Celtic people, but to have been adopted by Celts, by Scandinavians, by British, and Iberian races."

But if Europe was once occupied by a pre-Celtic, megalithic-monument-building race, surely some of our megalithic monuments must be ascribable to that time and race, and we come back therefore to the general opinion of archæologists, that our megalithic monuments belong to very different periods and people, and *not* all to one race or one epoch.

I cannot now enter into the consideration of the dates to which Mr. Fergusson ascribes individual monuments; I doubt whether any belong to so recent a period as he supposes: and can only express my surprise at the certainty and confidence which he feels in his own opinions—a certainty sometimes, however, oddly expressed, as, for instance, when he tells us, speaking of the crosses at Katapur, which he considers to be Christian and contemporaneous with a group of neighbouring dolmens, that "their juxtaposition and whole appearance render escape from this conclusion apparently inevitable."

But while I cannot accept Mr. Fergusson's peculiar theories, I cannot conclude without thanking him for the labour and care with which he has brought together a great number of illustrations, and a vast mass of facts, on this most interesting subject. In a review, one naturally dwells on points of difference, but every one must accord to Mr. Fergusson the credit which, in the following passage from his preface, he claims for himself; though I would venture to add that the unintentional self-criticism in the latter sentence seems to me not inapplicable. "I have," he says, "spared no pains in investigating the materials placed at my disposal, and no haste in forming my conclusions." His conclusions are, I think, in some cases, hasty and untenable; some seem inconsistent with one another; but no one can deny to his work the merit of being a rich and trustworthy storehouse of facts.

JOHN LUBBOCK

THE STUDY AND TEACHING OF MECHANICS

A LECTURE on this subject, being one of the series of lectures at the College of Preceptors on the Teaching of Physical Science, was given by Prof. W. G. Adams, of which the following is the substance:—

Mechanics treats of the laws of equilibrium and of motion of bodies, and in its widest sense, as the science of energy, must include all branches of Physics, for the solid, liquid, and gaseous states of bodies are determined by the more or less free motion of their molecules, and heat, light, electricity and magnetism are all different forms of motion. The study of the laws of equilibrium and of visible motion is important, both for their practical applications and because on them are founded the principles of thermo- and electro-dynamics. Before entering on a study of mechanics, students should have a knowledge of algebra and geometry, and on account of the importance of accurate measurement, the elements of trigonometry should also be studied. By a proper method of teaching geometry, boys can be taught to think, and the exact definitions and proofs of Euclid's Elements are better fitted to train the judgment and the reasoning powers than any less exact system of geometry. The way to teach geometry (and the same remark applies to mechanics) is not to expect boys to get up their Euclid from a book, and to say it off by the aid of a book of figures (a system which has been practised in many schools), but to explain the meaning of and illustrate every proposition, so that boys may understand it. The true method of teaching mechanics is illustrated by the way in which Galileo established the first principles of dynamics, and placed them before his pupils. Due weight should be given both to experimental and to rational mechanics, and the best way of bringing the subject before students is to have parallel but distinct courses of experimental and theoretical lectures attended by students at the same time. The practical applications of the subject are important, and some of them of great simplicity. The "Triangle of Forces" may be employed to build up diagrams to represent the thrusts on a jointed framework; so that by "Diagrams of Forces" the conditions of stability of loaded structures, and the form and tensions of suspension bridges, could at once be determined, by measurement of these diagrams or by calculation from them. Of the variety of text-books on the subject of mechanics, the teacher should reject books that profess to be adapted for examinations, as well as those which contain gross errors on the laws of friction, or on the inertia of matter and the laws of motion, or on the subject of dynamical units and should select from those which are not liable to such objections.

Competitive examinations may be useful if they are made tests of thorough knowledge; but too often they injure the student who is preparing for them by narrowing his mind, and create a class of dabblers in science, and are worthless for the purpose for which they are intended. Test examinations given to a class on the subject of their lectures are the best tests of the knowledge and progress of the student.

In teaching the laws of equilibrium of liquids and of gases, the same method must be followed as in teaching the laws of equilibrium and of motion of solids; and in addition to lectures and ordinary teaching students should have the opportunity of making experiments and measurements in these subjects in a physical laboratory. Some knowledge of other kindred sciences is necessary before a student can be said to have an intelligent knowledge of the principles of mechanical science. Accurate investigation and experiment show that near the melting and the boiling points the special properties and laws of solids or of liquids are no longer true, and Dr. Andrews has pointed out the existence of a border-land between the liquid and the gaseous states, and has shown that there is no breach of continuity between them. Taking a model, of which three rectangular edges shall represent the pressure, volume, and temperature, the upper surface will represent the state of the substance, and will explain in what way it is possible to pass from the liquid to the gas without change of state or any sudden change of volume. The ease with which we can conceive of the state of a gas under different circumstances, when we have such a model before us, shows the importance of employing figures and models to give a boy clear ideas of the propositions of mechanics.

Regarding Mechanics in its wider sense as the Science of Energy, there are three great principles—the Conservation, the Transformation, and the Dissipation of Energy, which have been established, and these principles are illustrated in the conversion of water into steam, in winding up a watch, in the diffusion of gases, in the conduction of heat, in the friction of the tides on the earth, and in the rushing of water down a mountain side. This latter source of energy has been employed in piercing the Mont Cenis tunnel.

The accuracy of the calculations by which the axes of the two tunnels on opposite sides agreed so completely with one another shows the importance of accurate measurement, and of the correct application of theoretical principles to practice.

These principles of energy tell us that in raising the waters of the ocean to the mountain tops, as much energy must be expended as can be expended by those waters in their return to the ocean, and the atmosphere, acted upon by the solar heat, is the vast air-engine by which these changes are accomplished.

NOTES

At the last meeting of the Royal Society the names of the candidates for election into the Society were read, in accordance with the statutes, as follows:—Andrew Leith Adams, Surgeon-Major; William Grylls Adams, M.A.; William Aitken, M.D.; Sir Alexander Armstrong, K.C.B., M.D.; Edward Middleton Barry, R.A.; John Beddoe, B.A., M.D.; Henry Bowman Brady, F.L.S.; Frederick Joseph Bramwell, C.E.; James Brunlees, C.E.; Edwin Kilwick Calver, Capt. R.N.; Alexander Carte, M.A., M.D.; William Chimmo, Commander R.N.; Prof. Arthur Herbert Church, M.A.; Fredk. Le Gros Clark, M.R.C.S.; Prof. John Cleland, M.D.; Herbert Davies, M.D.; Henry Dircks, F.C.S.; August Dupré, Ph.D.; Michael Foster, jun., M.A., M.D.; Peter Le Neve Foster, M.A.; Wilson Fox, M.D.; Arthur Gamgee, M.D.; Prof. Thomas Minchin Goodeve, M.A.; Townshend M. Hall, F.G.S.; Edmund Thomas Higgins, M.R.C.S.; Rev. Thomas Hincks, B.A.; Rev. A. Hume, LL.D.; Henry Hyde, Lieut.-Col. R.E.; Prof. William Stanley Jevons, M.A.; Edmund Charles Johnson, F.R.G.S.; George Johnson, M.D.; Prof. Thomas Rupert Jones; John Leckenby, F.G.S.; Clements R. Markham, Sec. Geog. Soc.; William Mayes, Staff-Comm. R.N.; Edmund James Mills, D.Sc.; Thomas George Montgomerie, Major R.E.; Robert Stirling Newall, F.R.A.S.; Edward Latham Ormerod, M.D.; Francis Polkinghorne Pascoe, F.L.S.; Prof. Oliver Pemberton; Rev. Stephen Joseph Perry; John Arthur Phillips, F.C.S.; Bedford Clapperton T. Pim, Captain R.N.; William Overend Priestley, M.D.; Charles Bland Radcliffe, M.D.; Edward John Routh, M.A.; George West Royston-Pigott, M.D.; William Westcott Rundell; William James Russell, Ph.D.; Osbert Salvin, M.A.; Harry Govier Seeley, F.L.S.; Alfred R. C. Selwyn (Geol. Survey, Canada); Peter Squire, F.L.S.; George James Symons, V.P. Met. Soc.; Edwin T. Truman, M.R.C.S.; Wildman Whitehouse, C.E.; Henry Woodward, F.G.S.; Archibald Henry Plantagenet Stuart Wortley, Lieut-Col.

The Earl of Lonsdale, whose death is recorded this week, was the father of the Royal Society, his election having taken place sixty-two years ago, in 1810. This honour now devolves on Sir Henry Holland, elected in 1815.

The death is announced, on the 3rd inst., of Dr. A. B. Granville, F.R.S., at the age of 88. He was one of the oldest Fellows of the Royal Society, having been elected in 1817, and was member of a large number of foreign learned societies.

We are very glad to be able to state that intelligence has just been received from Prof. Huxley that his health has already been greatly renovated by the pure air of Upper Egypt. He wrote from Thebes, and was then contemplating a visit to Assouan, from which he would probably have returned to Thebes before this.

Sir William Thomson has accepted the office of President of the Geological Society of Glasgow.

The Radcliffe Travelling Fellowship at Oxford has been awarded to Mr. F. H. Champneys, B.A. of Brasenose College. This Fellowship is of the annual value of 200*l.*, and tenable for three years, provided the Fellow does not spend more than eighteen months within the United Kingdom.

The President of the Quekett Microscopical Club will hold a *soirée*, on Friday evening, March 15, at University College.

Dr. Liebreich, the eminent ophthalmist, of St. Thomas's Hospital, delivered a lecture at the Royal Institution on Friday evening last, on certain faults of vision, with special reference to Turner and Mulready. The later "aberrations" of Turner's style he attributed to a physical change in the refractive power of the eye, by which illuminated points were converted into illuminated lines. The change of manner in Mulready's later pictures he accounted for, in like manner, by increasing yellow degeneration of the crystalline lens. We hope in a future number to give a report of the lecture.

THE Royal Academy of Sciences of Belgium offers prizes on the following subjects for Essays to be sent in during the year 1873 :—(1) A simplification of the theory of the integration of equations of the two first orders; (2) On the disturbing causes which influence the determination of the electro-motor force and of the interior resistance of an element of the electric pile; (3) On the relations of heat to the development of flowering plants, especially with regard to the periodic phenomena of vegetation, and on the dynamical influence of solar heat on the evolution of plants; (4) On the mode of reproduction of serpents; (5) On the composition and mutual relations of albuminoid substances; (6) On the coal fields of the basin of Liége. A gold medal of the value of 1000 fr. will be given for the first, fifth, and sixth questions, and of 600 fr. for the second, third, and fourth. The essays must be written in Latin, French, or Flemish. For 1874 the subjects are :—(1) On uric acid and its derivatives, especially in relation to their chemical structure and synthesis; (2) On the polymorphism of the Mucedineæ, their real nature, and the conditions of their development; (3) On the question whether the fungi of fermentation can, under certain circumstances, become changed into the higher fungi, with positive proof of the fact or the contrary.

Harper's Weekly states that Uriah F. Boyden, of Boston, U.S.A., has deposited with the Franklin Institute, of Philadelphia, the sum of one thousand dollars, to be awarded as a premium to any resident of North America who shall determine by experiment whether all rays of light and other physical rays are or are not transmitted with the same velocity. The conditions of the premium limit the applicants to those living north of the southern boundary of Mexico, and including the West India Islands. Applications must be made before the 1st of January, 1873, at which time the judges, appointed by the Franklin Institute, shall examine the memoirs and decide whether any one is entitled to the premium.

WE are desired by Colonel Grant to say that the botanica collection from Tropical Africa, referred to at p. 339, was not made in conjunction with Captain Burton, but during the journey of Captain Speke and himself in 1860-3, from Zanzibar to the great central Lake Region. The results will shortly be published in the Transactions of the Linnean Society; it will be illustrated by 100 (not 600) 4to plates, and the descriptions will be in great part drawn up by Prof. Oliver. We are glad to hear that Mr. W. O. Livingstone, who is accompanying the Livingstone Search Expedition, has considerable botanical knowledge, and is intending to bring home a collection.

IN reference to the hairy tapir of the South American Andes (*Tapirus Roulini*), the acquisition of skeletons of which by the Smithsonian Institution was spoken of in our last number (p. 370), we are informed that a fine series of skins and skeletons of the animal has recently been obtained by Mr. Buckley in Ecuador. Some of these are now in the British Museum; the others have been purchased by Mr. Edwd. Gerrard, jun., of Camden Town. At the last meeting of the Zoological Society a paper was read by Dr. Gray, describing the specimens acquired by the British Museum, and referring them to a new species, *Tapirus leucogenys*. But we are informed that there are no valid grounds for separating them from Roulin's Tapir of the U.S. of Colombia.

WE desire to call attention to the Annual General Meeting of the Iron and Steel Institute, which will be held in Willis's Rooms, King Street, St. James's, London, commencing on Tuesday, March 19, under the presidency of Mr. Henry Bessemer. The programme of proceedings will be found in our advertising columns. It is expected that on Tuesday evening, March 19, a paper, by Mr. I. Lowthian Bell, "On the Conditions which Favour and those which Limit the Economy of Fuel in the Blast Furnace for Smelting Iron," will be read and discussed at the meeting of the Institute of Civil Engineers, Great George Street, Westminster. The Council have kindly promised to issue invitations to members of the Iron and Steel Institute, to attend on this occasion.

OUR readers will have noticed in our advertising columns the list of subscriptions at present received to the "Priestley Memorial Fund." The object is worthy of the attention of all who are able and disposed to assist in so meritorious an object.

AN important letter, by M. Berthelot, appears in the *Moniteur Scientifique* for February, in which this eminent *savant* insists on the reconciliation of the scientific worlds of France and Germany, pointing out that the united action of France, Germany, and England, in the advancement of civilisation and science, is necessary for the progress of the world.

IT is stated that shocks of earthquake were felt at Dresden, Pirna, Schandau, Chemnitz, Rodenbach, Weimar, and Rudolstadt, between three and four o'clock on the afternoon of the 6th inst. They continued to recur during an hour, and in some cases several hours.

THE return of Professor C. F. Hartt, of Ithaca, from his late expedition to Brazil, has been already announced in the papers; and we are glad to learn, from *Harper's Weekly*, that he succeeded in making many important discoveries in natural history and the geography of the country, and especially the languages of the native tribes. By his researches in this latter direction he has already become quite an authority, and, we presume, will before long begin to publish his linguistic results. In the course of his expedition Professor Hartt took occasion to examine the great Kjoekkenmoedding, near Santarem, referred to by various travellers, which, however, yielded him only a few fragments of coarse pottery and a few bones. He was very fortunate in the opportunity of excavating the sites of a number of Indian villages on the edge of the bluffs bordering the Amazon and the Tapajos, in the angle made by the two rivers. Here he found an immense quantity of broken pottery, often highly ornamented, idols, stone implements, &c., probably derived from the Tapajos, now extinct as a tribe, or merged into the mixed Indian population of the Amazon. In an ancient burial-place on the Tapajos he dug up a number of burial-pots; none, however, containing complete skeletons. An examination of the mounds of the island of Marajo was to be made by some of his associates who remained behind.

THE Royal Horticultural Society has just issued an exceedingly comprehensive and valuable series of meteorological observations made at their gardens at Chiswick from 1826 to 1864, and analysed by Mr. James Glaisher. The number of tables is nearly sixty, including the mean temperature of every day, and the extremes of mean temperature for every day in each month during the year specified, the excess or deficiency above or below the average of the mean temperature of every day, month, and year; the daily ranges of temperature on every day of the year, and the daily falls of rain in each month. Comparisons are made with the series of observations taken at Greenwich; general conclusions are deduced, and the introductory observations are of value and interest to all meteorologists.

WE understand that the Meteorological Committee have resolved to issue lithographed illustrative charts of the Daily Weather Report, which will be delivered in London, within a reasonable distance from the office of the printer in Lincoln's Inn Fields, between 1 and 2 P.M., or posted in time for the evening mails. Up to the 31st of March these charts will be supplied gratuitously.

AURORA AUSTRALIS

ON Sunday the 4th instant, at 9h. 28m. P.M., my attention was suddenly called to a "fire." Looking in the direction indicated, I saw at S.S.E., about 15° above the horizon, a glare of reddish light. Curious to know whereabout the supposed fire was, I kept my eyes upon that part of the heavens. Presently, similar patches of light broke out on either side of the first, and in a few seconds I could see, on the assumption made, that there must be several fires blazing away over a wide range, for the sky was here and there lit up with a peculiar dark red light over an extent of at least 70° of the horizon. My attention being now aroused, I had recourse to various conjectures, which were speedily abandoned. The idea of an aurora had occurred almost at the outset; but as I had never, with certainty, seen one in Mauritius, and never heard or read of any having been observed there by others, I felt some reluctance to admit the fact that I was actually witnessing one. My doubts, however, were soon dispelled. I noticed that patches of cloud floating across the illuminated portions of the sky reflected no light, and on one or two occasions, a faint flickering, like lightning, was seen among the upper cirrus clouds. These and other facts, coupled with the knowledge that the magnets had been occasionally disturbed to a considerable extent on Friday and Saturday, and on the morning of Sunday, left no doubt on my mind.

Hastening to the house, I immediately mounted a portable inclinometer and declinometer, and took all the measures I could to observe what might take place, dividing my time and attention between the instruments, which I put up in a verandah facing the south, and the aurora right in front of me.

The needle of the inclinometer did not give the slightest indication of a disturbance, but the declinometer magnet was affected to the extent at times of 9′.

It was 9h. 48m., or 20m. after I saw the luminosity supposed to have been caused by a fire, that I began to observe the aurora systematically, and I append a copy of the notes which I took from that time up to 1h. 20m. A.M.

What struck me particularly was the apparent quietness of the whole scene. Unlike the "merry dancers," which I have often seen and admired in Scotland, rapidly changing shape and colour, and rushing in variegated columns and bands in different directions with great velocity, thereby conveying an impression of energy and violence, the display of Sunday night was calm and serene, giving one an idea of peace and repose. Except shortly after I first saw the phenomenon, I could not make out any motion of the arches, segments, or luminous bands. They appeared and disappeared without change of locality, the intensity of the light increasing or decreasing without any flickering. I could see no shooting, darting, or rushing of the bands or beams. Each made its appearance and disappearance simultaneously along its whole length, as if the action was vertical.

The spectacle presented by the beams from 11 P.M to 11.20 P.M. was at once grand and lovely beyond description. Almost from the extreme left to the extreme right, and from as low down as I could see up to a meridional altitude of about 72°, the sky was furrowed with alternate white and dark bands, all of which, so far as I could judge, were parallel to each other and to the magnetic meridian. They were generally at unequal intervals, sometimes crowded together, and sometimes considerably apart; but in this respect I could only judge of those near the meridian. At times they presented the appearance of graceful folds and convolutions, but the action seems to have been performed so gently and imperceptibly as to convey no idea of motion. They presented the same colour during the whole time, namely, a steel grey to a silver white.

The arches and segments were of a blood, cherry, or Indian red, and every now and then, when the intensity of the light increased, the stars twinkled like gems seen through a delicate pink curtain or veil placed before them. Occasionally one could fancy that he was looking at the Southern Cross through very transparent glass or crystal of an exquisite ruby tint into an inner chamber lit up with light of a similar colour.

The light was never very strong. I saw no part of the landscape lighted up by reflection. It is to be borne in mind, however, that I was occupied with the instruments, and that much may have escaped my attention.

During some parts of the night black clouds passed over the field of view, and I believe, although I could not see them, except on one or two occasions, that they were light cirrus and cirro-stratus clouds in the upper regions, as had been the case throughout the day.

The wind was light from E. by S. throughout, and the barometer was ·100 inch below the mean for the season.

After 1 A.M. the aurora speedily died away. At 3 A.M. I could see nothing; but looking out at 4.30 A.M. I saw a red glow in the southward, which at first I took for aurora, but which turned out to be cirrus clouds lit up in the early dawn.

Throughout Monday the magnets were quiet. A great many cirri appeared, which, in the evening, assumed at eastward and westward a dark red colour, very much resembling that of the aurora.

The Magnetic Observatory, which had barely commenced operations, may be said to have been inaugurated on Sunday night, and it is possible that its future records will show, amongst other things, that aurora is not so unfrequent in Mauritius as is supposed, although such a display as that which has just occurred may not be seen for many years to come. In the end of August and beginning of September, 1859, aurora was observed over a considerable portion of both hemispheres, and on one night during that period I saw a reddish glow in our southern sky, which may have been the Aurora Australis. Probably the present display has been seen over a great part of the globe. Has any unusual solar activity been observed? On Friday a chain of spots stretched over nearly the whole of the sun's disc, and a large group occupied another part of it. On Monday the chain had disappeared. Any one who may have made observations in the colony or at sea on Sunday night would oblige me by sending them to the Observatory. It would be interesting to know the height of the aurora.

Aurora Australis seen at Mauritius on the 4th to 5th February, 1872.

9.48 P.M —An irregular convex arch of dark red light extending over about 60° of the horizon, and having its vertex in the line of the magnetic meridian. Brightest below the Southern Cross.

9.58 P.M.—An arch of a dark red colour having a cord of about 70°. Vertex in or near the magnetic meridian. Patches of black cloud passing over the coloured segment from E. by S., but they reflect no light.

10.1 P.M.—The segment is of a more intense dark red colour. Its eastern limit is about 3° east of the Cross, and its brightest portion from 1° to 2° above the Cross. It is broken off towards the west, and extends in that direction to about only S. by W.

10 4 P.M —No segment now seen, but patches of Indian or cherry red on either side of the magnetic meridian at a distance of 30° to 40° from it.

10.8 P.M.—The whole has almost disappeared.

10.19 P.M.—An intense blood-red patch at S.S.E. having its centre 2° to 3° below the Cross. The stars shining through it with subdued light.

10.20 P.M.—The red light all gone, but a broad conical space of an ash-grey colour, with a slight green tinge, low down on the horizon, and apparently bisected by the magnetic meridian. Resembling early dawn.

10.22 P.M.—A dimly defined arch of a smoky red colour extends from about E.S.E. to S.W. by W. The height of its vertex is about 40° above the horizon.

10.24 P.M.—All gone.

10.25 to 10.30 P.M.—Appearing and disappearing. Some faint streaks of whitish light seen low down.

10.34 P.M.—Six bands of faint whitish light near horizon at S. by E.

10.37 P.M.—A bright meteor of first magnitude travelled slowly from α Centauri towards N. by E. It had a train of light and left sparks behind it. Colour white with a yellow tinge. The auroral bands brighter and higher.

11 P.M.—Sixteen luminous bands of a steel grey to a silver white colour, extending from as low down as I can see to within 20° of the zenith. The extremity of one of them is close to Canopus. Light of the Great Magellan cloud enfeebled. No apparent convergence of the beams; they seem to be quite parallel.

11.6 P.M.—The parallel bands are still seen. They cover the greater part of the hemisphere, extending (at the meridian) to about 72° above the horizon. On their eastern and western extremes there are patches of blood-red light, but none in the intermediate space. Some of the bands appear to be folded in a direction from west to east.

11.7 P.M.—Dying away.

11.15 P.M.—A deep red glow from E. to W. by S. along the horizon. Fourteen parallel bands of a silvery colour, with dark bands between them. They lie south and north, occupying nearly the whole southern hemisphere as far as the eye can reach, and are flanked at east and west by patches of blood and cherry red.

11.24 P.M.—The bands have disappeared. There is a deep red glare at E.S.E. and a lighter one at W.S.W.

11.28 P.M.—A few faint bands on either side of Canopus. A red light on their western, but none on their eastern side.

11.31 P.M.—A dark red glow at W.S.W., about 12° above the horizon.

11.33 P.M.—Clouds gathering in the lower regions of the atmosphere.

11.37 P.M.—Two parallel faint beams of whitish light 2° to 3° east of Canopus. A faint red glow at W.S.W., about 10° above the horizon.

11.42 P.M.—Two broad bands of faint whitish light to westward and three to eastward of Canopus. A patch of red light still at W.S.W. near horizon.

11.46 P.M.—Clouds gone. Aurora gone.

11.49 P.M.—A faint red glow at W.S.W. about 10° above the horizon, and a band of faint greyish light about 2° west of Canopus.

11.51 P.M.—The glow at W.S.W. is brighter and higher.

11.58 P.M.—Much fainter.

0.34 A.M.—A segment of dark red light from S.E. by S. to W.S.W., and rising at its middle to about 45° above the horizon.

1.20 A.M.—A bright red glow from S.E. to S.W. Intensest below the Centaur. Soon died away. J. MELDRUM

Royal Alfred Observatory, Mauritius, February 6

GEOLOGY

Supposed Legs of Trilobites*

MR. HENRY WOODWARD, of the British Museum, in a reply to the paper by the writer in vol i., p. 320, of the present series of this Journal, supports the view that the supposed legs are real legs. He says that the remark that the calcified arches were plainly a calcified portion of the membrane or skin of the under surface is "an error, arising from the supposition that the matrix represented a part of the organism." But Prof. Verrill, Mr. Smith, and myself, are confident that there is on the specimen an impression of the skin of the under surface, and that this surface extended and connected with the arches, so that all belonged distinctly together.

Moreover the arches are exceedingly slender, far too much so for the free legs of so large an animal; *the diameter of the joints is hardly more than a sixteenth of an inch outside measure; and hence there is no room inside for the required muscles.* In fact, legs with such proportions do not belong to the class of Crustaceans. Moreover the shell (if it is the shell of a leg instead of a calcified arch) is relatively thick, and this makes the matter worse.

We still hold that the regular spacing of these arches along the under surface renders it very improbable that they were legs. Had they been closely crowded together, this argument would be of less weight; but while so very slender, they are a fourth of an inch apart. Mr. Woodward's comparison between the usual form of the arches in a Macrouran and that in the trilobite does not appear to us to prove anything. We therefore still believe that the specimen does not give us any knowledge of the actual legs of the trilobite. Mr. Woodward's paper is contained in vol. vii., No. 7, of the *Geological Magazine.*

J. D. DANA

* From the *American Journal of Science and Arts* for March 1872.

PHYSIOLOGY

Blood Crystals

AN interesting volume has just been published by M. W. Preyer on Blood Crystals. The literature of this subject, which dates no farther back than 1840, is already extensive, no less than 143 authors being quoted in the "Bibliography," some of whom, as Böttcher, Hoppe-Seyler, Kühne, Lehmann, Rollett, Valentin, and M. Preyer himself, have written many separate essays on points bearing more or less directly upon the crystallisation of the blood. Though blood crystals were first observed by Hünefeld, the merit of discovering them is due to Reichert, who first recognised their nature. The fact of the crystallisation of a complex organic substance like blood was first received with some amount of incredulity, but the corroborative testimony of many microscopists soon cleared away all doubt, and a variety of methods were suggested by which the crystals could be obtained. The best plan for obtaining them is thus given by M. Preyer. The blood is received into a cup, allowed to coagulate, and placed in a cool room for twenty-four hours. The serum is then poured off, and a gentle current of cold distilled water passed over the finely divided clot placed upon a filter until the filtrate gives scarcely any precipitate with bichloride of mercury. A current of warm water (30°—40° Cent.) is now poured on the clot, and the filtrate received in a large cylinder standing in ice. Of this a small quantity is taken, and alcohol added drop by drop till a precipitate falls from which an estimate may be made of the quantity required to be added to the whole *without* producing a precipitate. The mixture, still placed in ice, after the lapse of a few hours, furnishes a rich crop of crystals. The forms of the crystals obtained from the blood of different animals do not vary to any great extent, and are all reducible to the rhombic and hexagonal systems. The vast majority are rhombic prisms, more or less resembling that of man. The squirrel, however, with several of the Rodentia, as the mouse and rat, and the hamster, are hexagonal. The hæmoglobin of several corpuscles is required to form a single crystal. All blood crystals are double refracting. The animals whose blood has been hitherto examined and found to crystallise, are—man, monkey, bat, hedgehog, mole, cat, lion, puma, fox, dog, guinea pig, squirrel, mouse, rat, rabbit, hamster, marmot, ox, sheep, horse, pig, owl, raven, crow, lark, sparrow, pigeon, goose, lizard, tortoise, serpent, frog, dobule, carp, barbel, bream, rudd, perch, herring, flounder, pike, garpike, earthworm, and nephelis. The spectrum of blood-colouring matter when oxidised with its two absorption striæ between D and E of Fraunhofer's lines or in the yellow part of the ordinary spectrum, and the single band of deoxidised hæmoglobin, are now well known. M. Preyer states he has not been able to obtain a spectrum from a *single* blood corpuscle, but that the characteristic bands are visible where certainly only a very few are present. The specific gravity of dry hæmoglobin he gives at about 1·3—1·4. The solubility of the crystals obtained from different animals varies considerably. Those of the guinea-pig and squirrel dissolving in water with great difficulty. Hæmoglobin is insoluble in absolute alcohol, æther, the volatile and fixed oils, in benzole, turpentine, chloroform, and bichloride of carbon. It is easily soluble in alkalies; acids rapidly decompose it. He calculates out for it the fearful formula of $C_{600}\ H_{960}\ N_{154}\ Fe,\ S_3\ O_{179}$, as agreeing very accurately with the percentage results of its analysis. Its equivalent is 4444, 4. Many pages of M. Preyer's work are occupied with an account of the action of various reagents upon it. The plates contain the forms of the principal crystals, and thirty-two spectra lithographed in colours. He describes five crystallisable products of the decomposition of hæmoglobin, namely, hæmin, hæmatosin, hæmatoidin, hæmatochlorin, and hæmatolutein, and several uncrystallisable, such as methæmoglobin, hæmatin, and hæmathion.

H. P.

SCIENTIFIC SERIALS

Annalen der Chemie und Pharmacie, September 1871.—Kochlin has continued his researches on "compounds of the camphor group." By the action of nitric acid on camphor the author has obtained a new body, $C_9H_{12}O_5$, which he calls camphoronic acid, and which has the property of forming salts in which H_2 and H_3 are replaced by metals. By distillation with potassic hydrate, butyric acid is produced; with bromine in presence of water camphoronic acid is oxidised, yielding oxy-camphoronic acid; this acid forms salts, in which H_1, H_2, and H_3 are replaced by metals.—An important physiologico-chemical paper follows by Hlasiwetz and Habermann on "Proteids," and a paper by Naumann on the length of time for the evaporation and condensation of solid bodies," which, however, do not possess much general interest.—Bender contributes a paper on the "hydrate of magnesic oxychloride." This substance, however, does not appear to be very stable, or to have very marked properties.—Mulder has experimented on allantoin and bodies derived therefrom; by the action of nitric acid two substances are obtained, allanic and allanturic acid.—An interesting paper on a new series of aromatic hydrocarbons, by Zincke, follows; by heating together benzol, benzyl-chloride and zinc powder, or finely divided copper, a reaction sets in with the evolution of hydrochloric acid gas, and the partial formation of a metallic chloride; the principal reaction seems to be, however, $C_7H_7Cl + C_6H_6 = C_{13}H_{12} + HCl$. Benzyl-benzol is a solid crystalline body, melting at 26°—27°, and boiling at 261°—262°; by oxidation it is transformed into $C_{13}H_{10}O$, a crystalline body belonging to the monoclinic system, which fuses at 26°—26·5°. Benzophenon, however, has the same composition, but crystallises in the rhombic system, and fuses at 48°—49°; the body obtained above is therefore an isomeric benzophenon, it, however, easily passes into the modification fusing at 48°—49°. The composition of benzyl-benzol will therefore probably be $C_6H_5—CH_2—C_6H_5$.—This number concludes with the translations of two papers by Messrs. Friswell and Armstrong respectively, which have already appeared in the English journals.

The *Geological Magazine* for January (No. 91) opens with a paper on a subject connected with an important branch of geology which is too much neglected in this country, and, indeed, has but few cultivators anywhere, namely, the microscopic structure of the so-called igneous rocks. This is Mr. S. Allport's notice of the microscopic structure of the pitchstones of Arran, the appearance of the sections of which under the microscope is, as described by Mr. Allport, exceedingly beautiful; and it is to be hoped that this paper and the illustrations accompanying it may induce others to enter upon this most interesting and important line of research.—The Rev. O. Fisher contributes a note on "Cirques and Taluses," with reference to Mr. Bonney's paper in the December number of the magazine. Mr. Fisher ascribes an essential part in the excavation of cirques to glacial action.—Mr. D. Forbes communicates a severe criticism of some remarks made by Mr. A. H. Green in his account of the geology of part of the county of Donegal.—"The Age of Floating Ice in North Wales" is the subject of a paper by Mr. D. Mackintosh; and Mr. James Geikie publishes the second part of his "Memoir on Changes of Climate during the Glacial Epoch."—The number includes the usual notices and reviews.

Memoires de la Société des Sciences Naturelles de Cherbourg. Tome xv. (Deuxième Série, Tome v.) 1870. "On the Swell and Roll of the Sea," by W. Bertin.—"Notes on the Comora and Seychelles Archipelagos," by M. Jouan. These islands were visited in 1850; a very brief list of the flora and fauna is appended. The list of birds has been apparently overlooked in the Zoological Record for January 1870.—"Notes on the Tubercles met with in *Callitriche autumnalis*," by MM. Karelschtikoff and Rosanoff, with a plate.—"On the *Lophobranchs*," by M. Dumeril.—"Notes of a Visit to Aden, Point de Galle, Singapore, and Tché-fou," by M. Jouan.—"On the Influence of Climate on the Growth of some Resinous Trees," by M. Békétoff.—"Geological Essay on the Department of La Manche," by M. Bonissent. "Supplementary notes to a paper on the Swell and Roll of the Sea," by M. Bertin.—Works received by the Society from July 1869 to August 1870.

Proceedings of the Natural History Society of Dublin, for the Session 1869-70, 1870-71, vol. vi., part i. (Dublin 1871) contains the following papers by Dr. A. W. Foot:—1, Notes on Irish Lepidoptera; 2, On Goitre in Animals; 3, On the Breeding of some Birds from the Southern Hemisphere in the Dublin Zoological Gardens; 4, Notes on Animal Luminosity; 5, Notes on Entomology; 6, Notes on Irish Diptera; 7, On some Irish Hymenoptera; and the following by Mr. William Andrews:—1, On the Inhabitants of the Rock-pools and caves of Dingle Bay, records, as new to the fauna of Ireland, *Aiptasia couchii*, *Stomphia churchiæ*, *Balanophyllia regia*, *Capnea sanguinea*, and "a deep-water species of stony coral, formed by hydroid animals, and related to the Tabulate Madrepores, which is nearly allied to, and indeed considered identical with, *Millepora alcicornis* of Linnæus;" 2, Ichthyological Notes; 3, On *Orthagoriscus oblongus*, with two plates; 4, On some rare Crustacea from the south-west of Ireland; 5, On the Ichthyology of the south-west of Ireland; 6, Notes on Hymenophylla, especially with reference to New Zealand species; 7, On some Irish Saxifrages; also papers by Prof. Macalister, on the mode of growth of Discoid and Turbinated shells; by G. H. Kinahan, on the Ferns of West Connaught and the south-west of Mayo.

SOCIETIES AND ACADEMIES

London

Royal Society, March 7.—"On the organisation of the Fossil Plants of the Coal-measures.—Part III. *Lycopodiaceæ*." By Prof. W. C. Williamson, F.R.S. An outline of the subject of this memoir has already been published in the Proceedings in a letter to Dr. Sharpey. In a former memoir the author described the structure of a series of Lepidodendroid stems, apparently belonging to different genera and species. He now describes a very similar series, but all of which, there is strong reason for believing, belong to the same plant, of which the structure has varied at different stages of its growth. The specimens were obtained from some thin fossiliferous deposits discovered by Mr. G. Grieve, of Burntisland, in Fifeshire, where they occur imbedded in igneous rocks. The examples vary from the very youngest half-developed twigs, not more than $\frac{1}{12}$th of an inch in diameter, to arborescent stems having a circumference of from two to three feet. The youngest twigs are composed of ordinary parenchyma, and the imperfectly developed leaves which clothe them externally have the same structure. In the interior of the twig there is a single bundle, consisting of a limited number of barred vessels. In the centre of the bundle there can always be detected a small amount of primitive cellular tissue, which is a rudimentary pith. As the twig expanded into a branch, this central pith enlarged by multiplication of its cells, and the vascular bundle in like manner increased in size through a corresponding increase in the number of its vessels. The latter structure thus became converted into the vascular cylinder so common amongst Lepidodendroid plants, in transverse sections of which the vessels do not appear arranged in radiating series. Simultaneously with these changes the thick parenchymatous outer layer becomes differentiated. At first but two layers can be distinguished—a thin inner one, in which the cells have square ends, and are disposed in irregular vertical columns, and a thick outer one consisting of parenchyma, the same as the epidermal layer of the author's preceding memoir. In a short time a third layer was developed between these two.

When the vascular cylinder had undergone a considerable increase in its size and in the number of its vessels, a new element made its appearance. An exogenous growth of vessels took place in a cambium layer, which invested the pre-existing vascular cylinder. The author distinguishes the latter as the vascular medullary cylinder, and the former as the ligneous zone. The newly-added vessels were arranged in radiating laminæ, separated from each other by small but very distinct medullary rays. At an earlier stage of growth traces of vascular bundles proceeding from the central cylinder to the leaves had been detected. These are now very clearly seen to leave the surface of the medullary vascular cylinder where it and the ligneous zone are in mutual contact; hence tangential sections of the former exhibit no traces of these bundles, but similar sections of the ligneous zone present them at regular intervals and in quincuncial order. Each bundle passes outwards through the ligneous zone, imbedded in a cellular mass, which corresponds, alike in its origin and in its direction, with the ordinary medullary rays, differing from them only in its larger dimensions. At this stage of growth the plant is obviously identical with the *Diploxylon* of Corda, with the *Anabathra* of Witham, and, so far as this internal axis is concerned, with the *Sigillaria elegans* of Brongniart. The peculiar medullary vascular cylinder existing in all these plants is now shown to be merely the developed vascular bundle of ordinary Lycopods, whilst the exogenous radiating ligneous zone enclosing that cylinder is an additional element which has no counterpart amongst the living forms of this group.

Though the central compound cellulo-vascular axis continued to increase in size with the general growth of the plant, it was always small in proportion to the size of the stem. The chief enlargement of the latter was due to the growth of the bark, which exhibited three very distinct layers,—an inner one of cells with square ends, and slightly elongated vertically and arranged in irregular vertical rows, an intermediate one of prosenchyma, and an outer one of parenchyma. These conditions became yet further modified in old stems. The exogenous ligneous zone became very thick in proportion to the medullary vascular cylinder, and the differences between the layers of the bark became yet more distinct. These differences became the most marked in the prosenchymatous layer; at its inner surface the cells are prosenchymatous, but towards its exterior they become yet more elongated vertically, their ends being almost square, whilst numbers of them of exactly equal length are arranged in lines radiating from within outwards. These oblong cells often pass into a yet more elongated series with somewhat thickened walls, which become almost vascular, constituting a series of bast-fibres. In the transverse sections these prosenchymatous cells are always arranged, like the vessels of the ligneous zone, in radiating lines. Yet more external is the sub-epidermal parenchyma passing into leaves composed of the same kind of tissue. The petioles of the leaves have been long, if not permanently, retained in connection with the stem, a character of Corda's genus *Lomatophloios*.

Where young twigs branch, the vascular medullary cylinder divides longitudinally into two parts; the transverse section of this cylinder now resembles two horse-shoes pointing in opposite directions. The break in the continuity of each half of the cylinder occasioned by the division is never closed by new vessels belonging to the cylinder; but when the stem develops exogenously, the cambium-layer, from which the new growths originated, has endeavoured to surround these openings in the cylinder, and, by closing them, once more to separate the medullary from the cortical tissues. Some beautiful specimens have been obtained, which exhibit these new exogenous layers in process of formation. The vessels of the young layers are not half developed. At first they meander vertically through masses of delicate cellular tissue; but they soon arrange themselves in regular radiating vessels and cells, becoming mere outward prolongations of the woody wedges and medullary rays of the older part of the stem. At this stage of their growth, the walls of the vessels are deeply indented by the contiguous cells, as if the plastic issues of the former had been moulded upon the latter structures. As the new vessels enlarge, the superfluous intervening cells disappear, until each medullary ray finally consists of a single vertical pile of from one to a small number of cells, arranged in as many Conifera. The exceptional cases are those where vascular bundles pass outwards to the leaves; these bundles have protected the contiguous cells above and below them from the pressure of the enlarging ligneous vessels and limited their absorption. Both these and the smaller ordinary rays pass outwards in horizontal and parallel lines. The evidences of an exogenous mode of growth afforded by these young, half-developed layers of wood is clear and decisive.

The Burntisland deposits are full of fragments of strobili, especially of torn sporangia and of macrospores. Several fine *Lepidostrobi* have been obtained, like those to which the fragments have belonged, and which the author believes to have been the fruits of the stems described. The structure of these strobili is very clear and of interest; the primary branches from the central axis subdivide, so that each sporangium rests upon a separate bract, from the upper surface of which a vertical lamina arises, and, extending the entire length of the sporangium, ascends far into its interior, where it bifurcates. The cellular walls of the sporangium blend with the bract along each side of this sporangiophore. The microspores occupy the upper part of the *Lepidostrobus*, and are usually triplospores, sometimes tetraspores. The macrospores occupy the lowermost sporangia, are of large size, and are very remarkable from having their external surfaces clothed with numerous projecting caudate appendages, each one of which is slightly capitate at its extremity. So far as the author is aware, this is an undescribed form of macrospore.

Two new forms of *Lepidodendron* are described from the Oldham beds, in one of which the medullary axis attains to an unusually large size, even in the young shoots; whilst the other is remarkable for the magnitude of its leaves. It is obvious that the plant which is the chief subject of the memoir is a true example of Corda's genus *Diploxylon*, so far as its woody axis is concerned; whilst its bark and leaves are those of a *Lomatophloios*, and its slender twigs are *Lepidodendra*. The author also points out the probability that the plant had a true Stigmarian root.

The structure of these fossil types is compared with that of recent *Lycopodiaceæ*. The vascular medullary cylinder is shown to be an aggregation of the foliar vascular bundles, so that the vascular connection between the leaves and the stem is maintained exclusively by means of these vessels, which thus correspond most closely with the central vascular axes of living Lycopods. On the other hand, the exogenous layers do not communicate directly with the leaves in any way; but, on the other hand, they are homologous with the corresponding layers in the Stigmarian root, in which latter they receive the vascular bundles from the rootlets. The medullary cylinder does not enter the roots, but appears to terminate at the base of the stem, though the pith is prolonged through them. Hence it seems probable that the nutritive matters were taken up from the soil by the Stigmarian rootlets, that it ascended into the Diploxyloid stem through the exogenous layer, but that, in order to reach the leaves, if conveyed by the vessels, and not by the cellular tissues, it had to be transferred by endosmosis to those of the medullary cylinder. The bark of the fossil plants is compared with those of *Lycopodium chamæcyparissus*, and *Selaginella Martensii*, which two combined represent the former.

These discoveries necessitate some changes in generic nomenclature, since the several parts of the plant not only represent the three genera above mentioned, but also several others. Meanwhile some other errors require correction. Corda erroneously defined his genus *Diploxylon* as having no medullary rays, and Brongniart relied upon this distinction in separating *Diploxylon* from *Sigillaria*; but no difference exists between the ligneous structures of the two genera, so far as *Sigillaria* is illustrated by Brongniart's *S. elegans*. Corda, Brongniart, and King all agree in regarding *Diploxylon* (which is identical with Witham's *Anabathra*) as a Gymnospermous Exogen. The necessity for abandoning this separation of the plants in question from the *Lycopodiaceæ*, urged in the author's previous memoir, is now made more obvious than before, the distinctions upon which the great French botanist relied in his classification being now shown to be such as mere differences of age can produce. The author concludes from his own observations that the genera *Diploxylon*, *Anabathra*, *Lomatophloios*, and *Leptoxylon* must be united. Brongniart had already brought into one generic group Corda's genera *Lomatophloios*, *Leptoxylon*, and *Calamoxylon*, Göppert's genus *Pachyphyllum*, and Sternberg's genus *Lepidophloios*, giving the latter name to the whole. Hence no less than six obsolete generic names are disposed of. The author finally follows Brongniart in adopting the term *Lepidophloios*, and *temporarily* assigns to the plant described the trivial name of *L. brevifolium*. The further relations of this genus to more ordinary forms of *Lepidodendron* require further investigation.

Linnean Society, March 7.—Mr. G. Bentham, president, in the chair. "Revision of the genera and species of *Scilleæ*," by J. G. Baker. This paper contained technical details of the new groups and genera proposed of this difficult tribe of Liliaceæ in continuation of papers already presented to the society.—"On the Andræcium in *Cochliostema*," by Dr. M. T. Masters. In this singular genus of Commelynaceæ, from the Amazon region, the staminal arrangement is different to anything else observed in the vegetable kingdom. There are three petaloid stamens, all arranged on the posterior side of the pistil, within which are three spiral bodies constituting the anthers. Within these are three staminodes, one of which is not developed till a considerably later stage than the other two; they do not appear to have any physiological value. The mode of fertilisation is obscure; the stamens and styles are both so completely obscured that self-fertilisation seems impossible.—"On a supposed hybrid between *Vaccinium Myrtillus* and *V. Vitis-Idæa*," by Mr. Gardner. In the discussion which followed, the prevalent opinion was that the plant was but a variety of *V. Vitis-Idæa*.—"A list of the Marine Algæ of St. Helena," by Dr. Dickie. These are twenty-one in number, all dwarf, and, notwithstanding the remarkable peculiarity of the terrestrial vegetation, only one species is peculiar to the island.—"Catalogue of new *Leguminosæ* from Western India," by N. A. Dalzell.

Chemical Society, March 7.—Prof. Williamson, F.R.S., vice-president, in the chair.—In the course of the ordinary business of the society, the proposed changes in the officers and council of the society for the ensuing year were announced.—Dr. Debus, F.R.S., then read a paper "On the reduction of ethylic oxalate by sodium amalgam." In 1864 Dr. Friedlander described, as the result of this reaction, the production of the sodium salt of a new acid, which he named glycolinic acid. Although the author has carefully repeated Dr. Friedlander's experiments, and varied the details of the process in different ways, he has been unable to obtain glycolinic acid, the only acids formed being glycollic and tartaric. A comparison of the crystalline form of a specimen of sodium glycolinate, prepared by Friedlander, with that of sodium glycollate, would seem to indicate that it is identical with the latter.—Two other papers were read, one "On metastannic acid, and the detection and estimation of tin," by A. H. Allen; and the other, "Note on the quantity of cæsium contained in the water of the hot springs found in Wheal Clifford," by Colonel Philip Yorke, F.R.S., from which it appears that a gallon of this water contains 26 grs. of lithium chloride and one million parts 1·7 of cæsium chloride, or more than ten times as much of the latter as the Dürkheim water, in which, it will be remembered, that element was first detected by Kirchhoff and Bunsen in 1860.

Zoological Society, March 5.—Mr. John Gould, F.R.S., vice-president, in the chair. Mr. Howard Saunders exhibited and made remarks on specimens of *Falco barbarus* and *Cypselus pallidus*, obtained in Southern Spain, being the first recorded occurrences of these species on the continent of Europe.—A letter was read from Mr. Walter J. Scott, of Queensland, giving some further information respecting the supposed existence of an undescribed large carnivorous animal in that colony. This letter was accompanied by drawings of the impression of the foot of the animal.—Mr. A. H. Garrod read some notes taken on the dissection of an ostrich, recently living in the Society's menagerie. The examination of this bird proved that its death was due to copper poisoning, a number of copper coins and pieces of coin in a much worn state having been found in its stomach.—Mr. E. W. H. Holdsworth read a paper containing a catalogue of the birds found in Ceylon, with remarks on their localities and geographical distribution; and gave a description of two new species, which were proposed to be called *Zosterops ceylonensis* and *Arrenga blighi*. The total number of Ceylonese birds included in Mr. Holdworth's list was 323, of which 36 were stated to be peculiar to the island.—A communication was read from Dr. Hermann Burmeister, containing a list of the species of the Lamellirostral birds of the Argentine Republic, with remarks on their habits and times of occurrence.—A communication was read from Dr. W. Peters, containing a list of a collection of small mammalia recently made by Mr. J. J. Monteiro in Angola.—Dr. J. E. Gray communicated some notes on a new species of tapir (*Tapirus leucogenys*) from the snowy regions of the Cordilleras of Ecuador, recently obtained by Mr. Buckley; to which were added some observations on the young spotted tapirs of Tropical America.

Society of Biblical Archæology, March 5.—Dr. Birch, president, in the chair.—Mr. J. W. Bosanquet read a paper "Concerning Cyrus, son of Cambyses, grandson of Astyages, who took Babylon; as distinguished from Cyrus, father of Cambyses, who conquered Astyages." In this paper, the learned chronologist endeavoured to show that, contrary to the received opinion of historians, Cyrus, son of Cambyses, though leader of the Medes as early as the year B.C. 535, was contemporary with the early part of the reign of Darius Hystaspes; having taken the throne of the Persian Empire after the death of his father. This view he believed to be consonant with the results of recent discoveries, and afforded a satisfactory explanation of the confessedly difficult chronology of Ezra and the Chaldee writers. Mr. Bosanquet summed up his argument as having proved:—(1) that Cyrus, father of Cambyses, who conquered Astyages, neither conquered Babylon nor reigned in Babylon, as Ptolemy assumes in his Babylonian Canon; (2) that Cyrus, son of Cambyses, King of Persia, grandson of Astyages, twice conquered Babylon; but did not reign over Babylon till after his father's death in B.C. 518; (3) that Ptolemy's Canon rests upon no sound authority, either historical or astronomical, as regards placing the reign of Cyrus at Babylon before the reign of Cambyses; (3) that the alternative reckoning deduced from Demetrius is to be preferred to that of Ptolemy, as resting upon the dates of three solar eclipses.

Anthropological Institute, March 4.—Mr. G. Harris, vice-president, in the chair.—Mr. Charles F. Tyrrwhitt Drake was elected a member.—Captain Richard F. Burton read his third paper "On Anthropological Collections from the Holy Land." It contained accounts of the Hamath Inscriptions, facsimiles of which were exhibited, and of skulls from Siloam. An interesting discussion was raised on the high antiquity of the Hamath Inscriptions. Dr. Carter Blake described the human remains brought by Captain Burton from Siloam, and by M. Ganneau from the "Tomb of Jesus," near that place; the former were stated to be undoubtedly Jewish, and the latter of modern Turkish origin. Mr. J. Gould Avery read a paper "On Race-characteristics as related to Civilisation."

BOOKS RECEIVED

ENGLISH.—Dr. Pereira's Elements of Materia Medica: Edited by Bentley and Redwood (Longmans).—Sir John Herschel's Outlines of Astronomy, 11th edition (Longmans).—Science Primers: Chemistry, by Prof. H. E. Roscoe; Physics, by Prof. Balfour Stewart (Macmillan).—Astronomy and Geology compared: Lord Ormathwaite (J. Murray)—The Higher Ministry of Nature: J. R. Leifchild (Hodder and Stoughton).

FOREIGN.—Annuaire de l'Académie Royale de Belgique, 1871. (Through Williams and Norgate.)—Lehrbuch der Botanik: Dr. O. W. Thomé, 2te Auflage.

DIARY

THURSDAY, MARCH 14.

ROYAL SOCIETY, at 8 30.—Contributions to the History of the Opium Alkaloids.—IV.: Dr. C. R. A. Wright.—Further Investigations of Planetary Influence on Solar Activity: W. De La Rue, F.R.S., B. Stewart, F.R S., and B. Loewy.—The Decomposition of Water by Zinc in connection with a more Negative Metal: Dr. Gladstone, F.R S., and A. Tribe.

SOCIETY OF ANTIQUARIES, at 8.30.—Stone Altar and Thurible from Syria: Capt. Burton.—Further Facts in the History of the Discovery of Australia: R. H. Major, F.S.A.

MATHEMATICAL SOCIETY, at 8.—Shall the Society apply for a Charter?

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

FRIDAY, MARCH 15.

ROYAL COLLEGE OF SURGEONS, at 4.—On the Digestive Organs of the Vertebrata: Prof. Flower, F.R.S.

ROYAL INSTITUTION, at 9.—The Alphabet and its Origin: J. Evans, F.R.S.

SATURDAY, MARCH 16.

ROYAL INSTITUTION, at 3.—Demonology: M. D. Conway.

ASSOCIATION OF MEDICAL OFFICERS OF HEALTH, at 7.30.—Mr. Stansfeld's Public Health Bill: Dr. A. W. Barclay.—On the Criminal Deaths of Infants, as shown by the Records of the Coroner's Court of Liverpool: F. W. Lowndes.

MONDAY, MARCH 18.

ROYAL COLLEGE OF SURGEONS, at 4.—On the Digestive Organs of the Vertebrata: Prof. Flower, F.R.S.

ANTHROPOLOGICAL INSTITUTE, at 8.—Comparative Longevity of Man and Animals: George Harris.—Physical Condition of Centenarians: Sir Duncan Gibb, Bart., M.D.

TUESDAY, MARCH 19.

ROYAL INSTITUTION, at 3.—On the Circulatory and Nervous Systems: Dr. Rutherford.

ZOOLOGICAL SOCIETY, at 9.—Report on additions to the Society's Menagerie in February, 1872: The Secretary.—On a specimen of the Broad-fronted Wombat (*Phascolomys latifrons*): Prof. Macalister.

STATISTICAL SOCIETY, at 7.45.

WEDNESDAY, MARCH 20.

ROYAL COLLEGE OF SURGEONS, at 4.—On the Digestive Organs of the Vertebrata: Prof. Flower, F.R.S.

GEOLOGICAL SOCIETY, at 8.—On the Wealden as a fluvio-lacustrine Formation, and on the relation of the so-called "Punfield Formation" to the Wealden and Neocomian: C. J. A. Meyer, F.G.S.—Notes on Atolls or Lagoon Islands: S. J. Whitnell.—On the Glacial Phenomena of the Yorkshire Uplands: J. R. Dakyn.—Modern Glacial action in Canada: Rev. W. Bleasdell, M.A.

SOCIETY OF ARTS, at 8.—Notes from a Diamond Tour through South Africa; T. W. Tobin.

METEOROLOGICAL SOCIETY, at 7.

THURSDAY, MARCH 21.

ROYAL SOCIETY, at 8.30.

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

SOCIETY OF ANTIQUARIES, at 8.30.

LINNEAN SOCIETY, at 8.—On the Geographical Distribution of Compositæ: G. Bentham.

CHEMICAL SOCIETY, at 8.

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