THURSDAY, NOVEMBER 28, 1872

*FERMENTATION AND PUTREFACTION**

IT is one of the great attractions of the science of Botany, an attraction common to all the other branches of the study of Nature, that wherever we may happen to be, and under whatever circumstances, something interesting and suggestive is continually brought before the eye and mind educated to understand its teachings, and no true naturalist ought long to be in a difficulty seeking for a suitable subject for illustration. At this season of dearth of flowers I hold in my hand a basket of "Duchesse" pears. These have, after their kind, been plucked in France before they were ripe, and some few of them are hard, green, and flavourless; others are soft, full, and mellow, with a rich, delicate aroma—morsels fit for the gods—while others have gone too far, and show the

—"little pitted speck on garner'd fruit,
That rotting inward slowly moulders all."

If you will allow me, I will, during the few minutes still at my disposal, give you a brief sketch of what has been done of late towards the explanation of the two phenomena which are for the moment the most prominent in connection with these pears, their ripening, and their decay.

These changes depend upon *fermentation* and *putrefaction*, two processes which are very familiar, and which have of late engaged the attention of some of the most able and skilful men of science, both on account of their vast importance in the economy of nature and of art, and of the singular phenomena which accompany them. These phenomena are very complex and difficult; but chiefly through the patient researches of botanists such as De Bary on the one hand, and of chemists and physiologists who may be represented by Pasteur, Lister, Burdon Sanderson, and Hartley on the other—steady progress is undoubtedly being made towards their solution, although much still remains obscure.

The character which most broadly distinguishes the vegetable from the animal kingdom is certainly the power which the former possesses when taken in mass of winning over from the inorganic kingdom binary compounds which cannot contribute directly to the nutrition of animals, decomposing them, and re-combining their elements into organic compounds suitable for the support of animal life. This process—the decomposition of water into oxygen and hydrogen, of carbon dioxide into carbon and oxygen, and of ammonia into hydrogen and nitrogen, and the re-combination of these four elements while in a nascent condition into starch, sugar, gum, protoplasm, &c.—is, so far as we know, carried on in plant-cells containing endochrome under the influence of light, and in such cells and under such circumstances alone. We thus find that this truly vegetable process is performed by a very small portion of an ordinary plant. The cells of the internal organs of plants and of large pulpy masses, such as these pears, connected with the function of reproduction, are perfectly colourless; simple sacs of cellulose, containing in their early condition protoplasm, and increasing and multiplying by its agency; and afterwards containing other substances in addition, such as starch and sugar, the products of its assimilation and excretion. These masses of protoplasm with their investing membranes composing the so-called "cells" of the pear, feed, indeed, upon the ternary and more complex compounds produced by the leaves of the pear tree, and are aërated by the fluids which are passing through the tissues of the pear tree; but, secluded from the light, and developing no special colouring matter, their reactions are not in the strict sense "vegetable;" they absorb the organic compounds and breathe the distributed air in the true animal sense, just as *Amœbæ* would do. To take the function of respiration as a test, they absorb oxygen and exhale carbon dioxide, while in the green parts of plants, which alone perform the great function of the vegetable kingdom in keeping up the "balance of organic nature," the exhalation of carbonic acid is in the sunshine entirely masked by the exhalation of oxygen—the product of its decomposition. A green tree may be likened to that wonderful animated tree, one of the oceanic Siphonophora, where a certain set only of the polyps are set aside to feed and to supply nutrition for the whole, while others, identical with these in essential structure, feel, or sting, or reproduce the species, or palpitate through the water as locomotive swimming-bells.

It is, perhaps, not easy at once to realise this difference in the vital relations of the different parts of the same plant, but it becomes clear enough in the case of pale parasites, for example *Cuscuta*. The dodder possesses no endochrome cells of its own; it feeds like an animal upon the organic compounds elaborated by its host. It contributes in no way as a vegetable to the balance of organic nature, and yet it is evidently a plant nearly allied to the ordinary bird-weeds, with all the characters of their well-known natural order.

These "Duchesse" pears are separated from the tree. They were probably separated physiologically before they were taken off, for before we would consider them fully ripe a certain shrivelling takes place in the cells and vessels of the fruit-stalk at a kind of joint, and the communication between the pears and the tree is at first partially and then entirely interrupted. But the pear does not die; it hangs out in the sunshine, and certain chemical changes take place within it, still under the guidance of vital action, sweetening it and developing its flavour. We learn from the beautiful researches of M. Bérard that if fruit be placed to ripen in air or in oxygen gas, a considerable quantity of oxygen is absorbed and an equivalent proportion of carbon dioxide is given off; that, in fact, a notable quantity of oxygen is burned in a true process of respiration. It is calculated by De Bary that the number of plants in which chlorophyll is absent—that is to say, which have no power of decomposing and re-combining the elements of water, carbon dioxide, and ammonia, and which consequently require to have their food presented to them in the form of organic matter—is fully equal to that of green plants, say 150,000. These plants are chiefly fungi. The part they play in the economy of the organic world is wonderful. The moment a plant gets worsted in the battle of life, becomes delicate from uncongenial soil or other circumstances, or gets smothered by a more vigorous rival, they set upon it and burn it.

* From the Opening Address for the Session 1872-73 to the Botanical Society of Edinburgh, delivered on Nov. 14, by Prof. Wyville Thomson, F.R.S., President of the Society.

If we look just now in the Botanic Garden at any of the old summer beds of half-hardy plants, we shall see them shrouded in a maze and network of white fleecy mould. That mould is a fungus finishing the work of extermination which the frost has begun, and then burning the bodies. In all the odd corners there are heaps of rotting vegetation. These stems and leaves are not rotting of themselves; heat them to 212° F., so as to kill the seeds of the fungi, and seal them up in closed cases, and although they will slowly decompose, they will never rot. They are being burned by the process of respiration of fungi just as effectually as they would be if they were collected into a heap, dried, and set fire to. Most of these fungi are very minute, but each of them, when it is found in anything like a well-developed condition, is thoroughly characteristic. Still they are so small and so simple that it is difficult to distinguish parts of those organs whose form is not strongly marked.

I will give a brief sketch of the life-histories of one or two of these fungi, and the first I will choose is a well-known mildew, *Mucor stolonifer*. This species is often found on juicy fruits, covering them with white woolly patches scattered over with small black heads, and producing a very rapid putrefaction beneath the surface of the fruit. A number of delicate branching filaments form a rich network in the substance of the fruit, filaments which are easily distinguished from those of some nearly-allied forms by their long simple tubes without partitions. These delicate filamentous tubes, which are the parts first to appear, and form the basis, as it were, of the fungus, are called the *mycelium*, and are found in almost all fungi. From the mycelium, at certain points, long rather wide tubes start from the surface on which the fungus is growing obliquely into the air, and, after running along for a time, again dip down and give origin to other tufts of mycelium tube-roots. At the point where these roots come off, as at the bud of a strawberry-runner, a little tuft of tubular stems rises up vertically, and ends in round vesicles which at first are entirely filled with transparent protoplasm. These are cut off from the stem by a partition which is at first flat, but afterwards assumes an arched form, giving the space between it and the outer wall the shape of a very deep meniscus. The protoplasm in the space ultimately breaks up into a mass of black polygonal spores, which escape by the giving way of the outer wall of the sporangium. These spores are thus produced by no process of true reproduction, but are simply separated particles of the protoplasm of the parent plant. In hot summer weather, chiefly on the surface of sour fruit, *Mucor stolonifer* forms thick patches, with broad stolons, and from these, twigs spread over the surface of the fruit. When two of these twigs meet one another they form large vesicular expansions, and then apply themselves to one another. A diaphragm is formed across each of the vesicles, thus cutting off the distal end of the vesicle, which is filled with protoplasm. The double wall between the two cells gives way, and the protoplasm in the two unites, as in the union of the cell-contents in the conjugation of *Zygnema*. These "coupling cells" have thus become fused into a single cell called a *zygospore*, which goes on enlarging, and is covered with a thick skin.

The simple spores, when scattered on moist ground, send out filamentous shoots of mycelium, which in their turn originate stolons as before; but the zygospores do not produce mycelial filaments when they germinate, but form one or two sporangium-bearers directly at the expense of the substance of the zygospore, and the ordinary course of growth is resumed from their spores. There are thus two modes of multiplication in *Mucor*—one by sporangia and spores, non-sexual, a simple method of propagation by buds—the other a true reproductive process, by the conjugation of male and female elements. It seems to be only occasionally and under specially favourable circumstances that the latter process occurs, and this mildew often goes on reproducing itself by spores alone for many generations.

The life history of *Mucor mucedo*, one of the commonest of the mildews, is not yet thoroughly known. Here the cells are again simple and undivided, but each sporangium-bearer usually ends in several large sporangia. Under certain circumstances this sporangium-bearer sends out tufts of finely dividing twigs, each of which ends in a small sporangium, which, to distinguish it from the larger form, has been called a sporangiolum. At other times processes are produced from the main cells which rise into delicate tubular branches, and give off globular cells which are called *conidia*—simple external spores, differing entirely in their character from the spores produced in sporangia; and if this mould be grown in a solution in which it is fairly nourished without a full supply of oxygen gas, long fibres are produced which break up into a multitude of separate bead-like cells filled with protoplasm, and capable of reproducing the organism. WYVILLE THOMSON

(*To be continued.*)

EXPLORATION OF THE SOUTH POLAR REGIONS

II.

WHILE Balleny was making the discoveries to which we alluded at the close of the previous article, two other expeditions were actively pursuing their researches and extending our knowledge of the Antarctic regions,—a French expedition under Dumont d'Urville and an American under Lieutenant Wilkes. Neither expedition was originally intended for South Polar exploration, and to this among other reasons is it to be ascribed that the results, with respect to the exploration of the South Polar regions, are of but little value compared with those obtained by the almost contemporaneous English expedition.

From the South Shetlands D'Urville directed his course to the south, and discovered on February 27, 1838, in 63° 10′ S. lat. and 57° 5′ W. long., a coast which bears the name of Louis Philippe Land, and rises to a height of between 2,000 and 3,000 feet above the sea. The general outlines of this coast were already indicated on Weddell's chart.

Two years later we find the same explorer again active, and with a better result. In January 1840 he left Tasmania for the south, steering for the region between 120° and 160° E. long. On January 19, in 66° S. lat., he found land from 2,000 to 3,000 ft. high, entirely covered with snow and ice. On the 21st some of the sailors landed on a little island consisting of gneiss, which D'Urville named Adelie Land. On the 30th and 31st D'Urville sailed round a promontory in 64° 40′ S. lat. and 132° 20′ W. long., naming this part of the coast Clairie Land. Shortly after this the expedition turned northwards, a number of the men having been lost through illness. Dr. Neumayer seems to think that the French constitution is not at all well adapted for expeditions of this kind.

On February 25, 1839, four ships, under the command of Lieutenant Wilkes, set out from Orange Harbour in Terra del Fuego, for the purpose of exploring these southern seas. The season was, however, too far advanced to admit of much being accomplished, though one of the ships, the *Flying Fish*, under Lieutenant Walker, penetrated as far south as the 70th degree of latitude in 100° 16′ W. long., and that at the end of March. This in itself is a fact of some interest and value, that so late in the season a point was reached as far south as Cook and Bellinghausen attained to, two months earlier.

On December 27 of the same year the squadron left Sydney, again for the south. Two of the ships, the *Flying Fish* and the *Peacock*, were soon compelled to return on account of injuries, so that there were only the two vessels, the *Vincennes* and the *Porpoise*, left to pursue their discoveries. On January 30, 1840, in 140° 2′ 30″ E. long., and 66° 45′ S. lat., Wilkes saw for the first time clearly and distinctly the land standing out of the mist; to this he gave the name of "Antarctic Continent." Five days previously the *Vincennes* reached its farthest south point, 67° in 147° 30′ E. long., where it was hard bestead by the ice. Indeed both vessels during their course along the coast had constantly to fight with the ice, and were frequently in the greatest danger of being crushed. Wilkes found the coast girt by a wall of ice, 150 to 200 ft. high, behind which rose the mountains to a height of 3,000 ft. He advanced thus to 98° E. long., and hoped on February 17 to be in a position to reach the point in this quarter to which Cook had come in 1773; but the ice-wall compelled him to turn to the north-east, quite away from the desired point. After he had followed the wall of ice to 62° S. lat. and 100° E. long., he had to give up all hope of being able to penetrate farther west, and returned to Sydney.

The expedition under Wilkes had travelled over a stretch of 1,500 miles along the margin of the ice, and frequently in sight of land. Even if through their labours the continuity of the land through its whole extent was by no means proved, yet the extension of Balleny's discovery in connection with that of D'Urville's considerably increased the probability of the existence of a great mass of land in these regions. Moreover, the observations made by Wilkes and his officers are of the highest value to science. It has been latterly disputed to whom belongs the merit of having first discovered the Antarctic Continent, both French and Americans claiming it each for themselves. But in the present state of our knowledge we must characterise such a dispute as perfectly objectless, for Balleny two years earlier had discovered his Sabrina Land, and had seen the coast at other points; therefore to him, if, indeed, an Antarctic Continent of the extent indicated by Wilkes exists, the honour of discovering it must be ascribed.

The researches initiated by Gauss and the Göttingen Society into the nature of the magnetism of the earth had given rise to a number of undertakings which had for their object to assist inquiry in this direction. From the southern hemisphere trustworthy data were altogether wanting, and on this account the British Government resolved to send an expedition to the magnetic South Pole, and that moreover, under the leadership of Captain James Ross, who had spent the greatest part of his earlier youth in the North Polar regions, and already in the year 1831 had discovered the magnetic North Pole. The results of his expedition, therefore, are incomparably rich and valuable.

After some preparatory cruising, the two ships, the well-known *Erebus* and *Terror*, well appointed for their work, set out from Hobarton on November 12, 1840, directing their course southwards, after a brief visit to Campbell Island. On December 27 the first ice was seen in 63° 20′ S. lat., and 176° 30′ E. long., and on January 1, 1841, the Polar circle was passed in 170° E. long., where the ships first encountered the pack-ice. Sir James, after careful consideration, determined to endeavour to penetrate the inner masses of pack-ice which, by the two previous voyagers, had only been skirted, and on the 9th, in 69° 15′ S. lat., and 176° 15′ E. long., came out into open sea. On the 11th land was discovered in 71° 15′ S. lat., the mountains of which, covered with perpetual snow and ice, reared themselves high into the air. The highest of these was named after Sir Edward Sabine, who for more than half a century, says Dr. Neumayer, has devoted his energies to researches in physical geography in all regions and in all parts of the earth, and who has largely added to our knowledge, especially of terrestrial magnetism.

The whole land, which Ross followed to nearly 79° S. lat., he named South Victoria Land, and an active volcano, 12,400 feet high, which he discovered on January 28, he named Mount Erebus. The name of the *Terror* was given to an extinct volcano, somewhat higher than the other, lying farther to the east. On the same day it was found that farther advance was impracticable, as the explorers found themselves suddenly face to face with an immense wall of ice, from 150 to 200 feet high, exactly similar to that which had been seen by D'Urville, Wilkes, and others. In the far distance over this wall they descried mountain peaks of great height and covered with ice: Ross named them after Parry.

In the vain attempt to reach the end of the ice-wall or find an opening in which the ship could pass the winter, they gained, on February 2, in 173° E. long., their greatest south latitude of 78° 4′. The rest of the month Ross spent in the further exploration partly of the southern sea and partly of the coast of the newly discovered Victoria Land from Franklin Island to the North Cape, when he turned his course to the west in 70° 40′ S. lat. In 68° S. lat. and 165° E. long. was seen a series of what seemed either islands or mountain peaks belonging to the continent, and farther on were seen the islands discovered by Balleny. Ross found that the land placed by Wilkes on his chart under 65° 40′ S. lat. and 165° E. long., in reality did not exist. As about the beginning of March the young ice began rapidly to form, Captain Ross determined to return northwards. On the return voyage, magnetic researches of the highest value were made. More especially was determined the position of the line of non-deflection of the compass.

On November 25, 1841, the expedition again shaped its course southwards. On December 16, in 57° S. lat., Ross reached the first ice, and on January 1, 1842, crossed the Polar Circle in 156° 28′ W. long., surrounded at times by pack-ice. On February 2, 1842, in 68° 23′ S. lat. and 159° 52′ W. long., the vessels reached open water, and on the 23rd of the same month, they approached, in 77° 49′ S. lat. and 162° 36′ W. long., a perpendicular wall of ice, only half the height of that in the neighbourhood of Mount Terror. On this day also they reached in 161° 27′ W. long. their highest latitude, 78° 9′ 30″ S., where they observed unmistakeable signs of neighbouring land. On the following day was commenced the return journey, and on April 6 the ships anchored at Port Louis, in the Falkland Islands.

On December 17, 1842, Ross set out a third time for the far south, this time to explore D'Urville's Louis Philippe Land, and to penetrate to the region which Weddell had reached in 74° 15′. There is little to record for our present purpose concerning this journey, but that, amid the greatest difficulties and dangers from the ice, the two plucky ships penetrated as far south as 71° 30′ in 14° 51′ W. long., on March 5, 1843. On September 2, the two ships *Erebus* and *Terror* reached England, and fifteen months later, under the guidance of Sir John Franklin, they set out again towards the north, from which they never returned.

Such is a brief review of the progress of geographical knowledge in reference to the South Polar regions. In what follows it is not intended to give a comprehensive analysis of the valuable material contained in the journals of the various voyages concerning the nature and physical conditions of these regions, but only to bring into prominence what is of greatest importance, and, in connection therewith, what is of importance to any expedition that may yet be organised, to draw some general conclusions as to the form of the South Polar regions. By this means a rational plan may be suggested for the further investigation of these regions.

In these observations the conditions as to ice claim our attention in the first place, as they enable us to draw important conclusions with respect to the extent of the land and the currents inside the Polar Circle. With reference to the latter, the drift-ice is of special value, as it enables us to recognise them in spite of the surface-currents caused by the winds and obliterating the main phenomena. In the southern hemisphere drift icebergs of 200, 300, and even more feet in height above the water are common; and if we take into consideration that their depth under the water must be six or seven times greater, we shall be able to understand that the movement of these ice-masses, which, moreover, are of colossal extent horizontally, must be the result of various forces working upon them. Under-currents, surface-currents, and prevailing winds, have to be considered in interpreting this phenomenon, according to the size and proportion of the mass presented; though the comparatively feeble surface-currents are of subordinate importance.

It would lead us too far here were we to attempt a thorough discussion of all the modifying causes and phenomena; it need only be mentioned that the water-masses of the Polar zone move, so to speak, to lower latitudes, as is sufficiently expressed in the so-called "Antarctic Drift." And by the south-east winds, which prevail in the southern summer, colossal masses of ice during that season push their way northwards, where they partly break up, but partly also are driven back by the north-west winds of autumn to the south. Although nothing certain can be said concerning their course towards the Pole, and only in particular circumstances is a south-easterly return movement established, yet it may in general be stated with regard to the masses which outlast the summer, that their northward and southward course may be explained by a periodical prevalence of the impulse from the surface or under-current. In winter, when the Antarctic drift-current, on account of the smaller differences of temperature and evaporation in lower latitudes, is less powerful, will the impulse, strengthened by the north-west winds, be directed towards the south-east or the south? Also, the movements of the ice during the other seasons can only be explained in this way; and on this account is the study of the various ice-charts of considerable advantage, especially the chart of the British Admiralty, on which are registered the results obtained from the commerce of the world.

The glacier ice-walls, of which we have accounts in several narratives of voyages, are, through the almost unbroken, and in winter specially rapid formation of ice, pushed continually farther and farther on into the sea, until at last, being insufficiently supported at the bottom, and subject to the buffetings of the waves, they break into pieces. Towards the end of the cold season this disintegration is still further assisted by the great differences of temperature betwixt air and water in high south latitudes, and with the setting in of September commences the journey of the masses of drift-ice from the south. It is in consequence of this that during the southern winter, scarcely any drift-ice is found on the great highways of the world's commerce. The result of a strict comparison is that the frequency of the occurrence of ice in June and December is in the proportion of 1 to 13. Further, that the prevalence of the drift-ice is greater in March and April than in September and October, is only a further proof of what has been stated, seeing that in the last-mentioned months the ice is still in the neighbourhood of the spot where it was formed, while in the former months it has either not yet been broken up, or has not got beyond the bounds of traffic on its return journey.

The masses of drift-ice do not everywhere move equally far to the north: in some places they are met with much nearer the equator than in others, as will be seen if we examine the region around the poles on the charts on which the position of the ice in the various months is laid down. The causes of this inequality are to be seen partly in the conditions of the current, but also in the difference of the distances from the pole of the places of formation of the various masses of ice: indeed, the greater this distance is, so much farther to the north, *cæteris paribus*, will the ice be driven. We might now attempt to construct an average boundary line for the equatorial drift-ice, but from the variety of the boundaries and the lack of material for this purpose, its course would be very indefinite. We shall best arrive at useful results if we draw these boundaries in accordance with trustworthy notes, leaving meanwhile peculiar, and evidently altogether abnormal circumstances out of account. The ice is found in some pathways of commerce in much greater force than in others. According to the excellent map appended to Dr. Neumayer's pamphlet, the drift-ice extends farthest into the South Atlantic Ocean, where it makes its way to the 40th parallel of latitude, while in the Pacific Ocean its boundary coincides nearly with the 52nd parallel; in the Indian Ocean the limit lies between the 40th and 50th degree of latitude. Clearly the cause of the ice pushing its way so far into the Atlantic Ocean is to be found in the fact that most of the icebergs there met with have their origin about Graham's Land and the South Shetlands lying far to the north, and, according to Towson's observations, they sail northward at a daily rate of ten miles in an easterly direction, when once they are exposed to the combined influence of the current and the westerly winds.

It is easy to see that these attempts to fix an average boundary are fraught with difficulty, and therefore, also, with uncertainty, especially when it is taken into consideration that all parts of the ocean have not been equally explored—many, indeed, not at all, or only by single vessels. By an uncritical examination of all the collected facts relating to the subject, one might be led to very false conclusions. Therefore we ought to examine most narrowly any conclusions which may be drawn at first from the frequency of observation; but for so complicated an inquiry, the material within reach is by no means sufficient, and therefore we must seek for help from some other quarter. Those parts of the ocean are best compared which have been explored by a nearly equal number of ships. But this at present is the case only along the regular routes in the South Atlantic, the Indian, and the South Pacific Oceans, and therefore in these cases an immediate comparison is admissible. By such a comparison it is immediately perceived that in each of the oceans places are found which are almost free from drift-ice, and that throughout the whole year. A specially valuable means of determining such places as are free from ice, is the fact that, towards the equator, a gap in the ice-boundary is shown, through which a ship coming from lower latitudes may sail to the 60th degree south without having to pass through ice. These ice-free areas may be set down as follow:—

1. To the south-west of Cape Horn the boundary of the drift-ice reaches down to about 57° S. lat.

2. Between the 60th and 80th degree of E. long. the boundary is found in 61° S. lat.

3. Between 160° E. and 175° W. long. it has the same limit as in No. 2.

Of course, icebergs, which are influenced more by surface currents and the prevailing winds than by undercurrents, are occasionally found in these clear areas; especially is this the case in years which are exceptionally abundant in ice, as was the case in 1854-5.

Let us inquire, now, what kind of explanation can be given of these phenomena. These will naturally result, in some measure, from the considerations previously advanced: either the place of their origin must be placed far to the south of the places indicated, so that the icebergs have a long journey to make before reaching the highways of commerce; or the conditions of temperature within the region indicated are favourable to the melting and disintegration of the ice; or finally the direction of the current is such that the icebergs cannot reach the north. The winds, so far as the present question is concerned, can exercise no influence, for all round the Pole they are nearly the same for the same parallels. With regard to the currents which are open to direct observation, it is difficult to come to any fixed conclusions, for these observations refer only to the surface-currents, which are almost universally caused by the prevailing winds. Besides, the observations as to currents of the usual kind, for special purposes, are not to be relied on. That an interchange of water takes place, besides that in the way of the general drift, through powerful currents, we know through many instances which need scarcely be mentioned. The question is concerning the position of these currents, their exact course in the various oceans. But for this purpose the drift-ice offers a capital means of solution, while the warm currents born of the water from the equator, in virtue of their southerly direction against the advance of the ice, and in virtue of their temperature, must be highly unfavourable to the continuance of this. Further, it is evident that the interchange of water occurs along such routes as are determined by the rotation of the earth and the configuration of the lands and the bottom of the seas. These routes will lead to places lying as far as possible from the Pole. All three would agree in furnishing strong proofs that the area is free from ice, if only we assume that the warm waters of the equator proceed southwards towards the places referred to.

In reference to that principal current of the ocean which is always referred to in a theoretical view, it may be said that the fact has been established that the equatorial waters, in their powerful currents from east to west, are made to diverge by the east coast of the Continent, and turned towards the Poles in both hemispheres. We find the direction taken by the south in a current along the east coast of America, of Africa, and Australia, from which it then makes its way towards the south-east, and when not to be recognised as a surface-current, it will be found to have assumed the form of an under-current of much greater force. The warm South American current will probably split on the point of Graham Land; one part makes for Alexander Land, the other towards that part where Weddel, after he penetrated the pack-ice, reached water free of ice. The Australian branch tends towards Victoria in the south of New Zealand, and made it possible for Sir James Ross to reach the 78th degree of S. latitude, while the Mozambique or African current flows towards the Pole, beyond Kerguelen Land, between Termination and Kemp Lands. In the first two cases we have positive proof; while with respect to the last, the evidence for a correct judgment can only be obtained by researches in the high south. The direction of the axis of the ice-free area is, in agreement with the general direction of the current, according to the present condition of our knowledge, from north-west to south-east; at least this is true for the Indian and Pacific Oceans, while the wedge-shaped form of South America must tend to modify the general rule.

We know from several voyagers that a girdle of pack-ice lies around the Poles, whose position and strength are liable to change, though in a less degree than is the case with the drift-ice. Naturally in this case, the evidence necessary to a determination of the mean position is still more scarce, and still more data must be obtained to form a mean value, although the pack-ice girdle thus obtained can only have an ideal importance. If we follow the boundaries of this pack-ice girdle, we notice to the south of New Zealand a bend towards the south analogous to that of the drift-ice, only shifted in accordance with the general direction of the current; also on the west side of Graham Land this tendency of the ice to bend towards the south is noticed, for it is always influenced by the direction of the land. To the south of Kerguelen Land the pack will only be to the west of Termination Land, and, indeed, stretching in a direction which makes us suspect another bend towards the south.

We must yet mention a sort of product of the drift, of considerable importance in ascertaining the condition of the drift and the ocean-currents, viz., the seaweed. This is loosened from its position especially by drifting icebergs, and turned adrift to the play of the waves. This conglomeration of plants, which reaches a little above the surface, though the long fibres stretch deep down into the water, shows the currents very correctly, for it is as little influenced by the winds as a drifting bottle. The curve that limits the place of growth is confined, in the Atlantic and Indian Oceans, to about the 60th meridian of E. long., near to that of the drift-ice. In some places this also holds good for regions to the south of Australia and the south-east of New Zealand. At all events, it can thus be understood how those places which are free from drift-ice are also free from drift-weed, a point of some importance when we consider the fact that drift-weed is the attendant of icebergs. According to the Dutch voyagers, on the west of the meridian of Paul and Amsterdam are found weeds different from those on the east thereof, which points to a different current.

(*To be continued.*)

TYNDALL'S RESEARCHES ON RADIANT HEAT

Contributions to Molecular Physics in the Domain of Radiant Heat. By John Tyndall, LL.D., F.R.S. (Longmans, 1872.)

FORTY years ago our knowledge concerning the radiation and absorption of heat was very meagre. We believe that the earliest experiment in this direction is to be found in that wonderful book containing the "Essayes" of the Florentine Academicians. There we meet with the fact that the heat of the sun, converged by a mirror, can ignite a pastile placed within a Torricellian vacuum. A little later, in 1682, Mariotte communicated to the Paris Academy of Sciences that the heat of a common fire, made very sensible in the focus of a burning mirror, was entirely cut off by the interposition of a sheet of glass. The mirror in this case must have been of polished metal. These experiments were subsequently repeated and extended by Lambert, who, assigning the true cause, pointed out the necessity of employing metal, and not glass mirrors, in the reflection of heat from terrestrial sources. Lambert further showed that if the radiation from a clear fire were converged by a large glass lens, no heat was felt where the brilliant focus was seen, whilst Hoffmann first collected the obscure heat of a stove to a focus by a metal mirror.

About a century ago Franklin made his experiments with bits of coloured cloth on snow, and found that, whereas colour strongly influenced the amount of solar heat absorbed, it made little or no difference on the emission from a lamp or candle. Beyond the foregoing information, we believe little more was known in this subject till, in 1777, Scheele published his famous treatise on "Air and Fire." But a great deal of ignorant talk, clothing itself beneath a barbarous jargon, was prevalent at that time. Putting much of this aside, Scheele asks himself the question, which he was unaware Lambert had already answered, whether it was really the heat, or only the light of a fire, that was reflected from a metal mirror; but first he seeks to know whether there are different kinds of heat. "There will be many," he remarks, "who will not hesitate to give an answer to my queries; for I am well acquainted with the vague phrases according to which everything is called Fire that hath a distant similarity to it. But I am of opinion," he naively adds, "it is best not to begin to read before one knows the elements of the alphabet, and to withhold an answer till one has reflected on the following experiments." Then comes a series of admirably simple experiments, pregnant with important results. As they deserve to be more widely known than they seem to be, the reader will pardon our quoting Scheele's own words at length. They are to be found in section 57 of his work already alluded to:—"From these experiments"—made in winter, before an open stove, and with only a candle, a concave mirror, and sheet of glass and metal, for apparatus—"it appears," says Scheele, "that the heat, mounting with the air in the stove, and passing through the chimney, is materially different from the heat passing through the door of the stove into the room. That the latter heat departs from the centre, where it is generated in straight lines, and is reflected from polished metals under an angle which is equal to that of incidence. That it unites not with air, nor can be diverted by a current of air, into a direction different from that which it originally had received. For that very reason, the vapours of the breath are visible in this great heat; for since air and heat are really united during summer, and warm air can dissolve more water than cold air, it likewise hence appears that this kind of heat is not united with air, nor has this kind of heat probably rarefied the air; and of consequence it becomes evident why it causes no tremulous motion in sunshine. You may, by means of a glass mirror, separate the light from this heat, when the heat remains in the glass, and the radiant light yields no heat. Hence it follows that the heat, passing through the door of the stove, coincides, in some points, with the light, but is not yet quite become light, since it is not reflected in the same manner from a glass surface as from a metallic one—a remarkable circumstance. This heat is soon changed into the usual heat, whenever it unites with a body, which may be observed in the glass, and in the metallic concave mirror blackened by soot, and in more instances. Represent to yourself," he concludes, "a little hillock of burning coals; in this case the heat darting from this hillock all around is that which may be reflected by a metallic polished plate; that, on the contrary, which rises upwards, and may be driven by winds to and fro, unites with air. I will call the first kind, for distinction sake, *Radiant Heat*." Thus arose the term we still employ. The whole passage reads as if it were written almost at the present day; and the lucid style of the last sentence cannot fail to strike the reader. This is the more remarkable if we contrast it with the current ideas of the time, or even with Scheele's own description of the heat of contact; for a little further on he states, "This heat is a peculiar acid, which has admitted a certain quantity of phlogiston in its mixture."

Soon after this Pictet made his well-known experiments on the reflection of heat. In these he confirms the fact of the reflection and convergence of obscure rays, and discovers that the velocity of radiant heat is beyond the reach of experiment. To him is also due our first knowledge of the apparent reflection of cold, a fact explained some ten years later (1792) by Prevost, according to his famous theory of exchanges.

The experiments on radiation published in England at the close of the last and the early part of the present century, will be familiar to most of our readers. It will be remembered that Sir William Herschel established the refraction of heat, and the difference in the quality of solar and terrestrial heat; that he confirmed Leslie's experiments on the heating power of different parts of the solar spectrum, and first discovered that the maximum heat was beyond the visible red (experiments subsequently verified by Sir H. Englefield); that he also determined the transcalency of various kinds and colours of glass, both to white light and to the light of the spectrum.

Both Rumford's and Leslie's inquiries into the Nature and Propagation of Heat quickly added to this knowledge; to Leslie belonging the capital discovery of the reciprocity of radiation and absorption. The accounts in our modern text-books render a further allusion to these experiments unnecessary.

A quarter of a century now lapsed; the attention of the scientific world being diverted by the electro-chemical discoveries of that period. One of the products of the new activity thus aroused was the discovery of thermo-electricity by Prof. Seebeck in 1822. Some ten years afterwards, Nobili constructed the well-known thermo-electric pile. Associating this instrument with a galvanometer, Melloni at once turned Seebeck's discovery into a thermoscope of surpassing delicacy. The fruit of one man's work thus soon became the seed of new and more vigorous investigation. And so prolific was this seed in Melloni's hands, that the blackened face of a thermo-pile is at present considered the indispensable pre-requisite in every exploration in "the domain of radiant heat." For six years Melloni pushed on with his researches; determining the amount of heat transmitted through innumerable solids and liquids—their relative *diathermancy*, as he expressed it—and using these determinations to investigate the quality of heat emitted from various sources. But the discovery with which his name will always be associated is that each material possesses a selective absorption, a veritable heat-tint, to which he gave the name of *thermochrosis*; thus confirming and explaining a similar fact previously noticed by De La Roche. Hence it was that Melloni called the volume "La Thermochrose," in which he grouped together the investigations that he had published, in the "Annales de Chimie," and the *Comptes Rendus*, between the years 1833 and 1839. The appearance of these researches was characterised by M. Biot as "un nouveau champ de découvertes, que M. Melloni a exploité avec un sagacité une addresse et une patience inimaginables;" the subsequent verdict of physicists has not lessened this high opinion.

The interest awakened by Melloni's inquiries was no doubt the main cause of the rapid additions to our knowledge of the phenomena of radiation and absorption, that followed. Among others, Forbes, Dulong and Petit, De la Provostaye and Desains, Knoblauch, Jamin, Masson and Courtépée, Müller and Balfour Stewart, signalised themselves by the value of their investigations in this department of natural knowledge. But the whole of these inquiries were directed to the behaviour of solids or liquids, or the analysis of radiation itself. The influence of gases and vapours on radiant heat was not entertained. Melloni, indeed, thought such attenuated bodies could not come within the reach of experiment; for he had ascertained that a column of air some 20 ft. long exerted practically no absorption on the radiation from his source. Pouillet and Forbes, however, showed that the heat of the solar rays are largely absorbed by our atmosphere; and Franz believed (though erroneously) that he discovered a considerable absorption of heat by the air contained in a tube only 3 ft. long.

Briefly speaking, this was the state of our knowledge in this branch of Physics when Dr. Tyndall approached the subject in 1859. After having wrought for twelve years, Prof. Tyndall has now gathered into the volume before us the important results his unremitting labour has won. A summary of these results must be left to another article.

W. F. BARRETT

LETTERS TO THE EDITOR

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

On the supposed new Marine Animal from Barraud's Inlet

As some interest seems to be excited as to the nature of the animal that forms the long calcareous axis that has been received from Barraud's Inlet, I forward you a copy of a short notice I read on the subject before the California Academy of Sciences, July 17, 1871. I also enclose a piece of the stem with some of the soft parts still adhering to it, as it is possible its examination by competent observers will determine if I correctly referred it to the sponges.

"An examination of the specimens received from Barraud's Inlet enables me to refer them to the Protozoa class, Spongidæ or sponges. Although apparently nothing but the calcareous stem has reached us, yet on some specimens I found one end of the stem covered with a horny substance, which, when moistened and examined under the microscope, presents the character of a true sponge, being formed of a tough sarcode arranged in the form of irregularly reticulated tubes, the sides of which are studded with minute pores. The arrangement of the sarcode round the axis is not circular, but has somewhat the appearance of a Maltese cross. The central axis is formed by calcified layers of a tough chitinous substance. In the specimens we have received the greatest thickness is about $\frac{3}{8}$ of an inch, but the longitudinal fissures found in many of the stems would indicate that they had shrunk. They contain about 80 per cent. of carbonate of lime. At each end the stem is tapered off. The top terminates in a fine hair-like prolongation of uncalcified chitinous substance. The lower part of the stem, which in our specimens is the only part covered with sarcode, also tapers down to about the thickness of a straw, and here there is no calcified axis. A thin section of the stem in its thickest part showed that it had been formed in concentric layers which were perfectly circular and presented nothing corresponding to the stellate arrangement of the sarcode. These rings undoubtedly represent different phases in the life of the animal. I have counted as many as thirteen in one section, and should they indicate animal deposits, this would give us thirteen years as the time required for their formation, a period not too long when we consider the length (upwards of seven feet) which some of these stems have attained. Whether this specimen is new I am not prepared to state, and shall not therefore name it, although I believe it has not been before observed. Its generic relations will, I think, be with Hyalonema and Euplectella, both sponges of the Pacific." JAMES BLAKE

San Francisco, Oct. 27

Misleading Cyclopædias

CAN any of your readers inform me if there is such a thing as a good and honestly constructed cyclopædia—one that does not send you hunting for information from one volume to another, and refer you backwards and forwards to articles that do not exist?

I have been repeatedly annoyed by this kind of will-o'-the-wisp, but have to-day met with such an outrageous example of it, that, although it involves some trouble, I feel it to be a duty to make a public exposure of it in your columns.

Requiring some facts on unusual atmospheric refraction, I turned to "Refraction" in the "English Encyclopædia." This article referred me to "Mirage, Fata Morgana," &c., for information on this branch of the subject. Turning to "Mirage," I found not a word, but another reference to "Reflection and Refraction, Atmospheric, Extraordinary." Next I tried "Fata Morgana," again the same reference. Coming back to letter R, I found the article "Reflection and Refraction," but was here referred to "Light, Optics, Refraction, Refrangibility;" then to letter A, "Atmosphere, Atmospheric"—nothing on the subject. Letter E, "Extraordinary Refraction"—nothing but a reference back again to "Mirage!" "Light, Optics, and Refrangibility" contain nothing on the subject.

I was thus sent on a search through five volumes of the work, and made to hunt out nine distinct headings for what does not exist; and what makes the matter worse is, that the writer of the article "Refraction," at the end of the work, must have *known* that it did not exist when he referred back to "Mirage, Fata Morgana," &c., which words have not a word of information appended to them.

An alphabetical cyclopædia is so much the most convenient for reference, and might be such an invaluable addition to a library, that it is the more to be regretted that it should be brought into disrepute by the absence of all efficient editorial supervision.

A. R. WALLACE

Rainbows on Blue Sky

IN NATURE for Nov. 21 a correspondent asks for examples of bows seen on a cloudless background.

I have seen this phenomenon twice at least. In one instance I remember that the extremities of the bow were seen against cloud, while the central portion bridged a space of clear blue sky.

A more perfect example occurred on the 19th of February in the present year. The following is a *verbatim* extract from my notes of that day:—

"Peculiar rainbow at 11.50 A.M.; perfect (except quite near the extremities), fairly bright, but projected throughout its entire length against clear blue sky. No rain was falling at the time, nor was there any appearance of falling rain on the sky, but the character of the clouds and of the weather was consistent with the supposition of slight and partial showers."

The phenomenon, although rare, does not seem to call for any special explanation. In showery weather, especially with a low barometer, one may sometimes see rain falling from a mere shred of cloud, the sky round about being clear. In such a case it is evident that there may be places whence an observer would see a rainbow against blue sky. Even should there be no visible cloud from which the rain seems likely to have fallen, the same explanation will still serve, for the cloud may be too attenuated to be visible, or may indeed be actually exhausted, the rainbow being formed on its last drops.

It scarcely needs to be pointed out, that when a rainbow is seen, as it usually is, against a cloud, the presence of the cloud is accidental rather than essential, the bow being formed not on the cloud, but on the drops of falling rain, and those being generally much nearer to the observer than the cloud.

Clifton, Nov. 25 GEORGE F. BURDER

The Greenwich Date

I AM anxious to obtain the solution of a question which has for some time perplexed me, and which is rendered more pressing than formerly, now that telegraphic communication is established between England and Australia.

It appears that a telegram sent on October 21, 3h. 5m. mean astronomical time at Adelaide, was received on October 21, 21h. 40m. mean astronomical time at Greenwich. Now, to obtain the Greenwich date of its despatch, we apply the longitude in time, adding when the place is west of Greenwich, and subtracting when it is east. Adelaide is 9h. 35m. east of Greenwich, the date sought is October 20, 18h. 10m. But suppose a place 9h. 35m. west of Greenwich, then the date sought comes out October 21, 13h. 10m., that is to say, the result of the operations gives a difference in the day of the month at places where, in fact, the day of the month must actually be the same. The query then is—in what part of the globe, and in what meridian, does October 20 end, and October 21 begin?

Fleetwood Vicarage JAMES PEARSON

Ocean Meteorological Observations

I PRESUME that anyone looking at the chart on page 43 of this week's NATURE, or reflecting on the circumstances under which barometric observations at sea are ordinarily taken, will agree with me that it would be wiser to give only two places of decimals, and not indicate a degree of refinement which the observations do not warrant. This point being granted (and even if it is not I shall maintain the same line of argument), I submit that the writer of the article is in error in saying on page 44: "Range corrections for pressure and temperature over the region under discussion are not yet accurately enough known to justify the committee in 'correcting' the results on the large chart by hypothetical corrections."

The daily range of the barometer in the very square under notice was investigated under Admiral Fitzroy's direction, and the results were published so long ago as 1861, as the seventh number of Meteorological Papers, under the title of "Intertropical diurnal range tables of the barometer."

It is very strange if this publication is unknown both to the author of the work reviewed and to the reviewer, and yet it is so cognate to the subject in hand that there would surely have been some reference to it, had they been aware of what had already been done. G. J. SYMONS

Nov. 25

Earthquake

AN earthquake was felt at the Cavendish Bridge Brewery, near Derby, on November 13th, at 4h. 10m. P.M.

Mr. G. T. Eaton, who was in his greenhouse, says "the glass was very much shaken." Mrs. Sandford was considerably shaken by a vibratory motion of her chair. Mrs. Eaton's children, who were upstairs, were alarmed. The windows rattled, and the glasses danced on the tables. The sky was dark and threatening, with a slight fall of sleet and snow.

I have delayed sending a report until further evidence could be obtained. It is now certain that the shock extended through Shardlow; and the earthquake was also felt in the neighbourhood at Aston, Castle Donington, and more particularly at Chellaston.

E. J. LOWE

Highfield House, Nottingham, Nov. 24

The Birth of Chemistry

MR. RODWELL writes:—"The Greek name for tin, 'kassiteros' (κασσιτέρος), was perhaps derived from the Insulæ Cassiterides or Scilly Islands;" but he does not state how these islands came by such a high-sounding name.

I have heard that the root word is Sanscrit, and was known in India before the Phœnicians discovered Britain. A. H.

PHYSOSTIGMA AND ATROPIA*

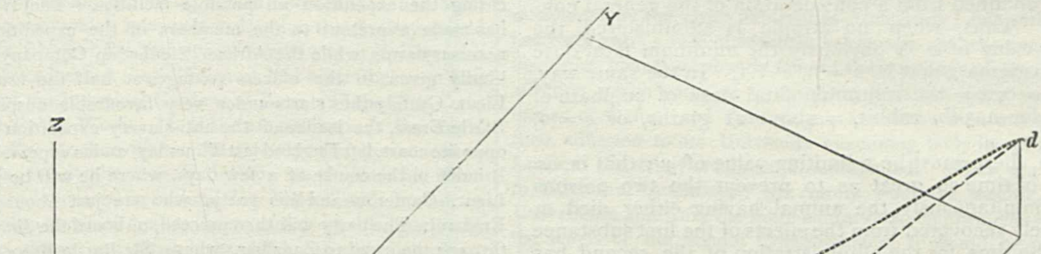
In this remarkable memoir, Dr. Fraser has shown how problems in experimental therapeutics may be treated with a kind of exactness which has hitherto been confined to purely physical inquiries.

That an antagonism exists between the physiological action of atropia and that of the Calabar bean had been strongly indicated by Kleinwächter and by Bourneville.† Dr. Fraser has not only confirmed this, but has, by means of a series of nearly 500 experiments, traced the character and extent of this antagonism. As the object of this communication is not to give an account of the matter of Dr. Fraser's paper, but to explain shortly its method, we refer the reader interested in therapeutics to the paper itself for details.

The experiments were performed on rabbits, of as nearly as possible uniform weight (about 3 lbs.) and condition. The doses of the two poisons were administered in the form of aqueous solutions, by subcutaneous injection.

It will be at once seen that, if we assume all the rabbits to have been of the same size, age, and general condition,* there are three quantities by change of which the conditions of experiment may be varied. These are—(1) the dose of physostigma; (2) the dose of atropia; and (3) the interval of time between the administration of the two doses. In the tabular summary of experiments, Dr. Fraser has noted in each experiment the effect on the pupils, on the heart's action, on the respiration, on secretion and excretion, and on the action of the voluntary muscles. We shall, however, in this notice confine ourselves to the general result, viz., recovery or death.

After proving, by means of upwards of 50 experiments, that the administration of small quantities of sulphate of atropia enables an animal to recover after a dose of extract of Calabar bean that would otherwise have caused death, Dr. Fraser proceeds to trace, by means of three series of experiments, the nature and extent of this antagonising action.



It is scarcely necessary to state that this action is of a purely physiological character, the two drugs having no chemical action upon one another.

In describing these three series of experiments, we shall, for the sake of shortness, refer to the three variable quantities mentioned above as follows:—

x = The dose of sulphate of atropia measured in grains per 3 lbs. of rabbit.

z = The dose of extract of Calabar bean (or of sulphate of physostigma) in units of 1·2 grain of extract, or 0·12 grain of sulphate of physostigma, per 3 lbs. of rabbit.†

y = Interval of time in minutes between the administration of the two substances, taken positive when the atropia, negative when the physostigma is administered first.

In the first series, $y = 5$; in the second, $y = -5$; and in

* "An Experimental Research on the Antagonism between the Actions of Physostigma and Atropia." By Thomas R. Fraser, M.D., Lecturer on Materia Medica and Therapeutics. Trans. R. S. E. xxvi 129—713.

† Since the publication of Dr. Fraser's preliminary note, Bourneville has published a series of experiments satisfactorily demonstrating the existence of this antagonism.

* As a given dose of poison affects a small animal more than it does a larger one, the doses were, for the purposes of comparison, multiplied by the factor $\frac{3}{\text{weight of rabbit in pounds}}$; thus reducing them to the doses which would have produced the same effect on a rabbit weighing 3 lbs. This is almost certainly not a perfect mode of correcting for difference of weight; but as the correction is always small—the animals being selected of as near 3 lbs. weight as possible—it may be assumed to be practically sufficient.

† This unit was chosen by Dr. Fraser as being the minimum dose producing death when no atropia was administered.

both x and z are varied, so as to obtain the limit which separates conditions leading to recovery from conditions leading to death.

In the third series, z is constant $= 1\cdot5$ (that is, a dose one-half greater than that which would produce death if no atropia were administered); and x and y are varied, so as to obtain sets of limiting conditions.

If the three variables, x, y, z, be expressed by means of a system of three rectangular co-ordinates, the conditions of each experiment will be represented by a point; and the points representing experiments resulting in recovery will be separated from those representing experiments resulting in death, by a surface passing through the points representing sets of limiting conditions.

The three series of experiments make us acquainted with three lines on this surface, viz., the intersections of the surface and the three planes, the equations of which are, $y = 5$, $y = -5$, and $z = 1\cdot5$.

Some further knowledge of the character of the surface may be obtained from a consideration of the general conditions. Thus, when no atropia is administered, the limiting value of z is obviously the minimum fatal dose of physostigma; that is, $x = 0$, $z = 1$. In the same way, when $z = 0$, $x =$ the minimum fatal dose of sulphate of atropia for a 3 lb. rabbit, = about 21 grains, or $z = 0$, $x = 21$.

Again, there must be a limiting value of y; that is, an interval of time so great as to prevent the two poisons acting simultaneously, the animal having either died or completely recovered from the effects of the first substance before the time for the administration of the second has arrived. When y exceeds this value, the surface must consist of two planes, the equations of which are, $x = 21$ and $z = 1$.

This limiting value is of course not necessarily the same on the positive and on the negative side; and, in fact, Dr. Fraser's experiments show that it lies very much nearer to the plane $y = 0$ on the negative than on the positive side.

Beyond these values of y, the surface consists of two plane sheets meeting in a rectangular edge. Each of these sheets separates points representing conditions under which recovery takes place from conditions leading to death; but in the case of the sheet $z = 1$ the space on the one side represents recovery and on the other side death caused by *physostigma*; while in the case of the sheet $x = 21$ the space on the one side represents recovery and on the other side death caused by *atropia*.

If we look closely at the portion of the surface lying between these limiting values of y, we shall see that here, also, the surface consists of two sheets; in the one $\frac{dz}{dx}$ is positive, in the other negative; in the one a small increase of the dose of atropia tends to recovery, in the other to death; the one runs continuously into the plane $z = 1$, the other into the plane $x = 21$. These two sheets meet in an edge, which is particularly well seen in the sections by the planes $y = 5$ and $y = -5$. (The various lines above mentioned are represented, in orthogonal projections, in the accompanying woodcut.) There can be no doubt that this edge is continuous with the rectangular edges between the plane sheets before mentioned. The conditions represented by points situated on this edge are such that increase of the dose of either substance will produce a fatal result, and that either increase or diminution of the dose of atropia will produce a fatal result.

This paper appears to us specially worthy of attentive consideration, as the first systematic investigation of the combined action of two poisons, and also on account of the method employed in arranging the results of the investigation; a method of which we have given a sketch in this notice, and which seems certain to lead to increased accuracy of observation, by giving the means of greater definiteness in the statement and classification of results.

NOTES

THE Royal Commission on Scientific Instruction and the Advancement of Science have this week examined the Marquis of Salisbury and Sir Stafford Northcote.

THE Anniversary Meeting of the Royal Society will be held on Saturday next.

AT the meeting of the Royal Geographical Society held on Monday night, the President stated that Mr. Young, the firm friend of Dr. Livingstone, to whom we recently referred, had sent him a cheque for 2,000*l*. to help to defray the expenses of the Livingstone Congo Expedition, which, under the command of Lieut. Grundy, who is well acquainted with the West Coast of Africa, is expected to start for Africa during the course of the present week. Government, we are glad to say, has given this expedition all the assistance in its power, furnishing letters to its officers on the West Coast of Africa, for the purpose of procuring the expedition all possible facilities. The War Office has made a present to the members of the expedition of the necessary arms, while the African Steamship Company has very kindly given to the officers passages at half the usual price. Lieut. Grundy thus starts under very favourable auspices. Sir Bartle Frere, the leader of the anti-slavery expedition from the opposite coast, left England last Thursday, and is expected to reach Brindisi in the course of a few days, where he will be joined by Lieut. Cameron and his party, who are just about to leave England. The party will then proceed, onboard the *Enchantress*, through the canal to Zanzibar, where Sir Bartle Frere will give his final instructions to those gentlemen who are to form the expedition. The president, Sir Henry Rawlinson, places every confidence in Lieut. Cameron, and in the zeal of the officers by whom he is accompanied; he is determined to avail himself of every possible opening to penetrate into the interior of Africa, for his own credit, for the advantage of science, and for the purpose of aiding and relieving Livingstone.

THE Khedive of Egypt is also about to send a force comprising 5,000 men, under Purdy Bey (one of his American officers) to Zanzibar in transports. The ostensible object of the expedition is to go into the country which it is supposed that Livingstone may be exploring, and to co-operate with him, if it be agreeable to him; but if he declines assistance, the expedition would undertake on its own account a search for the sources of the Nile, where, if discovered, the Egyptian flagwould be planted.

THE scientific news which comes to us every other week from France is refreshing and hopeful. There has been founded at Bordeaux a scientific association embracing the whole of the south-west of France, especially the Gironde and neighbouring departments. It is connected with the French Scientific Association, preserving, however, its autonomy and special organisation, its title being "Groupe Girondin" of the French Association for the Advancement of Science. Its seat is at Bordeaux, and, for scientific purposes, it is divided into four sections, each section corresponding to one or more sections of the French Association. They are—1. Section of the Mathematical Sciences; 2. Physical and Natural Sciences; 3. Medical Sciences; 4. Moral and Social Sciences. Each section meets monthly at Bordeaux, the first in the first week of the month, the second in the second week, and so on. The work of the sections consists of lectures, exhibitions, and scientific discussions on the subjects proper to each section. This provincial association intends to publish at intervals such papers as are likely to be of general interest; to encourage scientific researches by pecuniary help; and to give prizes for the best memoirs on subjects to be proposed by it. Most heartily do we wish the society success.

SIR JOHN BOWRING, whose death took place on Saturday last, at the age of eighty, was better known to the public in the world of politics than of science. In the latter, however, he filled a by no means unimportant position, as one of the most strenuous advocates of an international decimal system of weights and measures, and as an old and and very regular attender of the meetings of the British Association, where he devoted himself chiefly to the Section of Economical Science.

IT is expected that Sir William Jenner will be the President of the Pathological Society for the ensuing year.

THE Master and Fellows of Gonville and Caius College, Cambridge, have recently determined to appoint a prælector in chemistry to superintend the laboratory, and to have charge of the chemical studies of the students at the college. The stipend will be 200*l*. a year, and the prælector will have the status of a Fellow of the college. The election will take place about the middle of next month.

AT a meeting of the Royal Society of Arts and Sciences of Mauritius, held on September 25, it was resolved that, like other scientific societies which have met in London the Society should convey to Dr. Hooker the feelings of regret and sympathy with which they have learned that differences had arisen between him as Director of the Botanical Gardens at Kew, and the First Commissioner of Public Works. It is the earnest hope of the Royal Society that Dr. Hooker, whom the Society has the honour to include among its members, as it did for many years his illustrious father, will succeed in maintaining himself with honour in the Directorship to which he has been raised by his merit and extensive knowledge, and which he has held with such distinction to himself and advantage to the public.

THE Civil Service Commissioners have announced that on December 31 they will hold an open competitive examination for the appointment of clerk to the Curator of Kew Gardens. Candidates must be between the ages of twenty and thirty, and must be familiar with the routine duties of the garden, and competent to direct the foremen in matters relating to their accounts. On the same day the Civil Service Commissioners will hold an examination for the appointment of second assistant in the Herbarium at Kew, for which persons between eighteen and thirty who are skilled in practical botany will be eligible to compete. In each case the Commissioners will apply to Dr. Hooker for a report on the technical qualifications of the candidates.

THE *Times of India* speaks of a rumour that the Government intends to abolish the Deccan College, or rather to amalgamate it with Elphinstone College. By this plan, Government pretends to think, higher education would be advanced in India. But the *Mora Prakash*, an Indian paper quoted by the *Times of India*, says the end would be much more effectually accomplished by appointing to both colleges a greater number and more efficient teachers than has generally been the case hitherto. For the two colleges there are thirteen professors; but Elphinstone College is allowed two Professors of Mathematics, while the Deccan College has none, and no Professors of History and Political Economy. We hope the threat is a mere bait to ascertain public opinion. Intelligent public opinion, we believe, would certainly condemn the step, and urge Government to make the teaching staff more numerous and effective.

A CORRESPONDENT writes to the *Athenæum* :—"The question of admission of women to medical degrees in Edinburgh University has been rather unexpectedly solved, at least for the present. Miss Jex Blake, a foremost champion of the movement, has actually been plucked in her examinations, and sent back to complete her scientific studies." Many people will be quite unable to see that this by no means surprising accident affects in any way the great question of the unrestricted admission of women to the privileges of university teaching.

WE are delighted to notice that the Liverpool *Daily Post* has for some time past been devoting about a column to science, giving, besides notifications of the meetings of the numerous learned societies in and around Liverpool, a selection of scientific notes from this and other journals. We cannot give too great praise for the step taken by this paper in the right direction, and we only wish that all other provincial, as well as metropolitan, papers would follow the example, and give the latest news of a power which a distinguished Frenchman recently declared woul d soon become the ruler of the world.

IN a recent speech by the Rev. Mr. Tuckwell, he made some pertinent remarks on the Future of University Local Examinations. After referring with all due praise to the "Regulations of Oxford and Cambridge," he was yet compelled to say that without most serious modifications, the machinery of these examinations will be insufficient to meet the demand of the time which is surely coming, when compulsory universal public examinations will be imposed upon all the English schools. They show deficiency in four vital points. "They are administered by the older universities exclusively; but within the last forty years a race of teachers has grown up, who owe to an institution young yet already famous those feelings of loyalty and affection which some of us associate with the more venerable names of Oxford and Cambridge; and these men will give in their adhesion to no University examining body in which the London University remains unrepresented. They are costly to individual candidates: yet surely, from the wealth of the Universities and from the large educational endowments now in the hands of the School Commissioners, it would be possible to find funds for the extinction or the diminution of this tax. They unwisely limit subjects. Five optional subjects are permitted to junior candidates, of which Scripture must be one. They take up Scripture then because they must; Greek, Latin, and Mathematics, because these are supposed to gain higher marks than anything else, and are the leading subjects in their school work; there remains the choice between modern languages and science; nine boys out of ten, under the pressure of parents or teachers, take up French, and thus a severe though unintended blow is dealt at physical science. Lastly, they are in no sense compulsory; and the temptation to an unscrupulous master to pick out a clever boy, and work him exclusively for high distinction, while he starves the rank and file, is too obvious to need further notice. When these four blots are wiped away; when the three Universities combine to hold one great examination once a year; when the fees are lessened or abolished; when free trade in subjects is set up; and when all boys in every school above a certain age are compelled to undergo the ordeal; then, indeed, and not till then, we shall see such a system of examinations, so perfect in theory, so priceless in its effect upon school-teaching, as for the present we must be contented to behold only in our dreams."

AN astronomical and meteorological observatory is about to be erected by the Russian Government at Tashkend, in Central Asia, about 100 miles north-west of Khokan.

THE fossil man discovered at Mentone is at present being exhibited at the Jardin des Plantes, Paris.

MR. W. F. DENNING, of Bristol, noticed on Saturday evening last a meteor of considerable brilliancy. It radiated from a place at the extreme north-west part of Andromeda, passing through the sword-hand of Perseus, and onwards through Camelopardalus, becoming extinct, as if burnt out, on reaching the head of Ursa Major. In its flight the meteor faded several times and revived again with great rapidity. It did not leave any train of light marking the path it had traversed, though it emitted a spark in its course. In reference to its brightness Mr. Denning says that it excelled Venus when at her maximum degree of brilliancy.

PROF. PIAZZI SMYTH, writing to the *Athenæum*, says that the finest specimen of one of the "casing stones" of the Great

Pyramid known at present to exist either in Europe, or even in Egypt, was received last week in Edinburgh from Mr. Waynman Dixon, a young engineer who has recently completed an iron bridge across the Nile between Cairo and Jeezeh. The specimen possesses, Prof. Smyth says, in a more or less injured condition, five of the anciently-worked sides of the block, including the upper and lower horizontal surfaces, together with the levelled surface between. It was the exact angle of this levelled slope which led the late Mr. John Taylor to what Prof. Smyth calls "the immortal archæological truth, that the shape of the entire monument was carefully so adjusted and exactly fashioned in its own day to precisely such a figure that it does set forth the value of the mathematical term π, or does, vulgarly, demonstrate in the right way the true and practical squaring of the circle." Whether this be the case or no, Prof. Smyth declares that the length of the front foot of the stone, or that line or edge from which the angular π slope of the whole stone commences to rise, measures, "within the limit of mensuration error now unavoidable, the number of just twenty-five pyramid inches, neither less nor more. And twenty-five pyramid inches have been shown to be the ten-millionth part of the length of the earth's semi-axis of rotation." Prof. Smyth is very severe on the Egyptologists of the British Museum for the manner in which they conduct their department.

THE *Athenæum* informs us that the first volume of a Russian translation of Mr. E. B. Tylor's "Primitive Culture" has appeared at St. Petersburg. The German version of the same work is also announced as being just about to appear; and a French translation of Mr. Tylor's "Early History of Mankind," with notes by the translator, M. Emile Cartailhac, and by M. Quatrefages, is stated to be in preparation.

CONCLUSIVE proof has been obtained by a correspondent to *Notes and Queries*, that the treatise "On Probability" is not by De Morgan, but by the late Sir John William Lubbock. On inquiry at the Museum, the little slip of paper containing the original title was produced, and which gives the authorship to Sir J. W. Lubbock. On the back of the slip was inscribed the note—"Information from Prof. De Morgan, Dec. 62."

WE see from the *Times* of India, that Mr. Griffiths, of the Bombay School of Art, with a few of his students, intends, about the end of December, to go to the remarkable caves of Elephanta, to copy the very beautiful painted decorations which still cling to the walls, in spite of damp, neglect, bats, and the relentless teeth of time. These caves are on a small island in the harbour of Bombay, about seven miles S.W. of the city, and contain some very interesting Indian antiquities. They get their name from the gigantic figure of an elephant which formerly stood near the shore, but has now fallen to decay.

A CORRESPONDENT, Mr. W. B. Shorte, writes under date Nov. 4, from on board the steamship *Tanjore*, Bombay, giving us a few notes on the occultation of Venus, which he witnessed on the evening of Nov. 5. A small telescope with a magnifying power of about 100, and a pair of good binoculars, were the instruments employed. The planet shone with such lustre that it was clearly seen by the naked eye even before sunset, and after sunset appeared for some time as if resting on the upper part of the dark limb of the moon. In a few minutes a very gradual diminution of the planet's light was noticed, and as the occultation proceeded a singular phenomenon was observed, namely, the apparent position of Venus within the moon's circumference, the planet actually appearing for some time as if situated *upon* the disc of the satellite, though much diminished in size, and shining as a minute point of light. This continued until the moment of complete occultation, the Bombay time of which was 5h. 46m. 47s. The re-appearance on the illuminated edge occurred at 6h. 26m. 32s., so that the planet was invisible for 39m. 46s.

THE ORGANISATION OF ACADEMICAL STUDY IN ENGLAND

IN connection with the question of the best application of the endowments of Oxford and Cambridge, a public meeting was held at the Freemason's Tavern, on Saturday Nov. 16 by members of the Universities and others interested in the promotion of mature study and scientific research in England. The meeting was called in response to a preliminary resolution to the effect that "the chief end to be kept in view in any redistribution of the revenues of Oxford and Cambridge is the adequate maintenance of mature study and scientific research, as well for their own sakes as with the view of bringing the highest education within reach of all who are desirous to profit by it."

The Rev. Mark Pattison occupied the chair. He explained that gentlemen present were not the representatives of any political party or political movement, but were there simply for an academical purpose. Neither were they to be considered as having met to take an initiative: the initiative had already been taken by Mr. Gladstone in appointing a commission to inquire into the revenues of the colleges and universities. They were only there to discuss the direction which, in their opinion, ought to be taken by any reform, initiated, not by themselves, but by other people.

Professor Rolleston, who was the first speaker, commenced by remarking that until the end of the last century, it would be admitted that the Universities were neither seats of learning nor seats of teaching. The first thing that was done was to make them seats of examination; and, as far as that is concerned, they work tolerably well at this moment. The great danger is that they should be made simply into that utilitarian sort of machine —a machine for examining and a machine for teaching. The speaker by no means wished that their capabilities in the way of examining and that kind of work should be curtailed. Still he thought it of very vital consequence, in this somewhat utilitarian age, to make the Universities into places where original research, and where the production of fresh facts and means of knowledge, instead of the mere communication and reproduction of it, should be recognised. One result of our present examination system is that men who, as grown men and during the whole of their university career, are subjected to the ordeal of examination *in futuro*, do not look at what they have under study as so much truth, but look upon it as something to be reproduced on paper, and to further their designs on Fellowships and Scholarships, and other pecuniary rewards. Now when a man is kept for something like twenty-three or twenty-four years of his life under that sort of training, he gets apt to look at all work whatever of the intellectual kind, from the point of view of the examination merely. Men get demoralised by the process. They do not look at the truth for itself. They have no notion of shovelling forward the elements of knowledge into some area into which nothing has been before. That is entirely a new vein to them: and he thought one of the first things requisite was that examinations should be considered rather more the work of boys, and of people just emerging out of boyhood, than that they should be prolonged into a sort of struggle for men who have got to man's estate. We have then to consider:—how is it possible for us to encourage that which we feel is an advantage of a greater kind, although it is one which can only be shared by a larger number?—How is it possible to encourage original research without sacrificing soundness of learning in the many? How can we encourage the few to research without at the same time sacrificing the great advantages which we do get for the whole public, by passing a great number of mediocre men through the mill which does make them useful machines for doing work in this country of ours? There is a very serious objection which may be urged:—"But how do you propose to encourage original research? Original research is a work of genius—you cannot fetter genius by law—you cannot tie a man who has this gift of original research by rules and laws. You cannot give him definite duties to perform, within a definite time; and then you are in this dilemma:—a man has nothing given him to do—will he not then do nothing?" That is a very common saying among people who have got effectually case-hardened by looking at things in a schoolmaster's point of view. A man who has nothing to do, they tell you, will do nothing. Now he believed by using the system of examination judiciously, by rewarding people for what they do and show under that particular ordeal, and then by giving them something or another which does keep them, so to speak, from beggary for the time being, it is possible then, by a well-adjusted system, to keep their minds open to original research. But we know that funds must be found for it. A man cannot prosecute research unless he has got something to find him bread for the passing moment. Although he thought we should be entirely wanting in our duties if we laid aside the examination system, which has rescued the universities from the slough of idleness in which they were eighty or ninety years ago, yet, he said, we neglect our duty even more by neglecting the encouragement of men who have the capacity for original investigation and research. Again, a man who has not some notion of what original research means, is not fit to be a teacher at all. He would go even further, and say, if a man has the gift of original research, even if he entirely lack the power of communicating, and, what is another thing, the taste for communicating knowledge, he ought to have a place found for him. A man of that kind is like a light shining all around; setting by his example and his work a higher tone to society, a man who has the power of going into some new sphere, so that he may say to those whom he is teaching:—We are the first who ever burst into that silent sea.

Dr. Carpenter then spoke of the different system pursued by the German universities to that which prevails in our own.

Dr. Burdon Sanderson continued on the desirableness of fostering at the universities a class of what in Germany is called the *Gelehrter*, that is, said Dr. Sanderson, a man who not only possesses as adequate a knowledge as other men do of subjects in general, but has made a perfect study of some particular subject. The speaker then dwelt on the study of physical science, and of physiology in particular, as it should be conceived at an university.

The resolution "That to have a class of men whose lives are devoted to research is a national object" was then carried.

Sir Benjamin Brodie said that he had the strongest opinion that when the report of Mr. Gladstone's commission is published, and the true revenues of the colleges of Oxford are made known to the House of Commons and the world, the greatest surprise, and he might also say, the greatest indignation will prevail. He admitted fully that a great amount of good educational work is done by the Universities, but certainly thought that the work is totally disproportionate in every way to the machinery which exists for its performance, and it is idle and useless to say that we want an expensive collegiate system—a system of colleges manipulating actual revenues of thousands of pounds a year for the purpose of educating, however admirably, 2,000 students who, we may also say, absolutely pay for their education besides. When those statements are made, as they will be made, as to the property of the Universities and the Colleges, there will be the greatest danger that we may have a reform which perhaps none of us wish for—a reform which may be no improvement at all, but which may simply consist in the alienation from the purposes of knowledge of these great funds. Now with regard to the promotion of knowledge in various branches, this great object was entirely lost sight of by the Executive Commission in 1854. He believed that most persons in Oxford who are interested in real education, are not very well satisfied with the fruits of this Commission. The few things that they did in regard to the promotion of knowledge were done partly with that view, and partly under the pretext of reviving old foundations, such as the Linnæan professorship at Merton College, and four professorships at Magdalen College, and two or three other small institutions which the University had long ago buried under ground. The Commission dug these up, and therefore so far did something for the promotion of science. And indeed it is impossible, unless you absolutely destroy Oxford and Cambridge, to get rid of every record of the idea that those universities are founded for the promotion, and not solely for the diffusion, of knowledge; for that idea really runs through the whole university system. The great libraries of Oxford and Cambridge, and also the great collegiate foundations, bear witness to it. Now we wish to take up this thread where our predecessors dropped it, namely, this idea that the universities are institutions, not only for diffusing knowledge and education, but for absolutely promoting knowledge and investigation. However, a much more important object than that is the real welfare of the nation, as that welfare of the nation may be promoted by the growth of science and knowledge. With regard to scientific research, men are really hindered from investigation on all sides from the want of means of subsistence, and means of work. Certain aids are afforded to the investigators of science by existing institutions, by the Learned Societies of England and the Continent; and we have also two or three national institutions which certainly on such an occasion as the present ought by no means to be forgotten, because we shall be told that this is not an object for the nation to care for. One of those institutions is the British Museum, which really exists solely for the purpose of preserving knowledge. Another institution is the Royal Observatory at Greenwich. We have again private foundations: the Meteorological Observatory at Kew; the Radcliffe Observatory at Oxford, and the like. All those institutions are founded, not in the least with regard to education alone, but for the purpose of promoting the growth of knowledge. He thought it really very little use for us to be too indefinite; and that, if we wish to produce any result, we must have some definite plan and programme. His own idea was tha it would be very desirable to found in the universities of Oxfordt and Cambridge certain specific institutions for the promulgation of scientific research; using the term scientific research in its widest sense, and include in it all knowledge which is capable of being made the subject of research; but certainly specific institutions should be founded for this object. It will not do to trust these great institutions to the growth of mere ordinary professorships, but he would certainly like to see certain specific institutions devoted to this object, which should represent the various great departments of human knowledge. Those institutions to be connected with professors specially selected for the objects which they have to fulfil, and where the professors would be provided with assistance and apparatus, and every means and appliance which could really be valuable and useful to them for the purposes of research; and he did not think that much less, or anything less, than this, would fulfil the object which we desire.

The Chairman moved, as the next resolution, "That it is desirable, in the interest of national progress and education, that professorships and special institutions shall be founded in the universities for the promotion of scientific research."

Professor Seeley spoke on the question of prize fellowships. He said the speeches to which he had had the pleasure of listening had brought the question of University Reform to a focus. He anticipated that this meeting, particularly if the movement were followed out further, would convey to the English mind an idea which it had perhaps no very great natural capacity for conceiving. The preceding speakers, said the Professor, have introduced to the Englishman to-night a character for whom we have found it difficult to find a name, because there is no name for him in the English language, and we have been obliged to call him in the German *Gelehrter*, and in French we call him a *savant*, but there is no English name for him. He is a person who is engaged in mature study, and who lives by his study; and we have made it plain that our object in University Reform is one definite thing; and that is to find for this person at the same time as we find him a name, a career. But we shall be met by an assertion that he already has a career in England, and he has also a name—that he is, in fact, the Fellow of a College. He wished to say a word or two first about this Fellow of a College, and about certain popular reasons for which it is supposed to be desirable always to have such persons. If you were to ask most English people about the English universities, they would say that the most glorious feature about them is just this—that a young man may go up, from any part of the country, without a penny in his pocket, and may get 300*l.* a-year given him for life; and to take away that, is simply to take away the scholastic glory of England, and whatever makes its universities superior to the beggarly universities of the Continent. To give a young man 300*l.* a-year, they think, is a thing which explains itself; but if you come to examine their meaning, you will hardly question that they are looking at the matter as a question of charity; that they want the young man to receive so much to do him good, and to give him a start in life.

He would, however, remark that he thought the objects of charity should be those who stand in need of it, and are not likely to be able to help themselves. But we carefully select young men in the vigour of life; and, not only that, but young men who have shown themselves to be possessed of more than ordinary abilities, that is to say, just the very young men who can get on in life without any such help. He recommended, if these institutions are retained, simply on the ground of charity, that these fellowships should be given to men carefully selected, whose abilities are less than those of others. Again, it is said, how excellent a thing it is that a young man going to the bar, in his first year of brieflessness, should have his fellowship to fall back upon. That is partly the same object of charity; but mixed up with it is another notion, that it is a good thing for the bar that in this way men of high education are brought into it. That is a very important question indeed, but he could not say that it is a question which we of the universities are called upon to discuss. There are other institutions which have charge of the interests of the bar—let them consider it. We have in London several great Inns of Court; and it is often said that they have funds. If it be so, and if it be desirable, by means of fellowships, to procure men of high education to enter the profession of the law, let them establish fellowships themselves for that purpose. That is a very simple course. But now comes the question which this resolution deals with. Is this fellow of a college, of whom we have been speaking, a person of mature study, a person who devotes his life to advancing the bounds of knowledge? Of course it is quite possible to mention the names of distinguished men, who have risen to distinction in their particular branches as fellows. But the question for us is, are fellows of a college, as a rule, men who are preparing themselves for that career,—is their life devoted to study and to knowledge—are they persons who are either enlarging the bounds of knowledge, or are on the way to enlarging them? He answered, confidently, they are not the class of men. He did not charge them with being a class of men with whom any fault whatever can be found. They are not what we are told they used to be many years ago. It would not be possible, perhaps, to find instances of the torpid, vacant lives which used to be led under the protection of a fellowship. They occupy themselves now in some way. They supply the scholastic world, they supply the clerical world, sometimes they supply the bar, they conduct a great many examinations in the country, and they do a great deal of work which is very valuable; but mature study is a work which they do not, as a rule, engage in, only with some exceptions here and there. The Professor went on to say that fellows were neither chosen by the right kind of electing body, nor according to the right method, for the end of furthering mature research. He criticised the existing terms of the tenure of fellowships, as well as the existing system of examination at the universities.

The Chairman then put the resolution: "That the present mode of awarding fellowships as prizes has been found unsuccessful as a means of promoting mature study and original research, and that it is therefore desirable that it should be discontinued," which was carried.

The Chairman then said that the subject of the professoriate is of course a very wide subject, and it is impossible to do more than just indicate the position which that question holds in our scheme. It is desirable that we should make it clearly understood that we are not aiming a blow at what is called the educational efficiency of the place. The question of the professoriate is one which was first mooted twenty years ago as the question of the professoriate *v.* the tutoriate, and it was regarded as a revolution in the educational institutes of the University. The question which we are now raising of converting the University into a centre of mature study was not then raised. The question of University reform turned entirely upon the educational question of professors *v.* tutors. What the Executive Commission of 1854 did was not to substitute professors for tutors in any great measure in the educational system of the University. The storm that had been raised by the mere sound of the word "professor" was so great that they were daunted, and did not dare to propose any large creation of the professoriate. Things are entirely changed now, and even if we confine ourselves only to educational requirements, we have not that battle to fight. But we have the situation which the Commissioners of 1854 created for us, and that situation is this. They raised a certain number of the then existing professoriates, and added to them a few others; and so called into existence a body of professors, many of whom have been extremely valuable and influential members of the University. But the situation of a professor in the University at present, or at any rate of the philosophical professors, is that of persons who are entirely outside the working of the system. For instance a very eminent professor once advertised a course of lectures on accents simply. This course of lectures he had prepared not only with very great pains, but he had for years investigated the subject of the origin and growth of the accentuation of language, in a way in which it had never been done before. His work was an original work. He had collected all the special programmes that bore on the subject, and he had constructed a history of language accentuation. He advertised this course, and proceeded to give it. At the first lecture the room was full; but when they found that this was an original philological investigation, and not a lecture as to the rules for accenting the perfect participle of the Greek verb, in order that they might use it in Moderations, they immediately fell off, and left it. The consequence is that the professors are not at all working now as a portion of the system. Now if we say that we want to set up more of these professors, University men will say, "Professorships are doing no good as they are at present. We are doing the work. It is we, the tutors, who are doing the work of the place, and you professors are simply ornamental." This is the result of the way in which the Commission of 1854 set about its work. They were told that the great evil of the University at that time was that the colleges had absorbed the University, and the first thing that a reform of the University should aim at was the reconstituting the University as against the colleges. Now, it is very important for us to let our attitude be understood to be quite different. We do not want, as the phrase is, to rob the colleges to make the University rich. The antithesis between colleges and university is *nil*, for our purpose. We do not intend to perpetuate the mistake which the Commissioners of 1854 did, and to take away a few thousand pounds from the colleges, make it over to the universities, and leave the colleges as they are. The speaker then went on to specify the diversions of college revenues effected by the Commissioners of 1854 by the endowment of professorships, and said that was not the kind of precedent which the present meeting was anxious to see followed. We are agreed (he continued) in desiring the creation of a body of resident students and teachers—real students and real teachers—and the attitude we shall take will be to say, "We will leave the colleges exactly where they are. We do not intend to rob the colleges and give the proceeds over to the University, but we will gradually convert them into what we wish to see them." The supposed antithesis between professor and tutor should be sunk entirely, in our point of view, and the whole body of resident graduates should be brought into one homogeneous association of teachers all working together—these teachers naturally being of different ages, and consequently of different attainments. We would begin, as they do in Germany, with the *privat docent*. It has been very well said that the *privat docent* is the order upon which the principle of German universities principally rests. The eminent professors of whom we hear are not the actual working men of the place, but they are the men who have gone through the ordeal of working men as *privat docent*. They have been trained to that European celebrity under which we learn their names, but the *privat docent* are the working men of the place. Now, instead of putting the tutors into an attitude of hostility to the professors, as is the case at present, they might be reconciled to the professors by making them also professors, but making them of a lower grade in the teaching system. Of course there are various steps through which a successful tutor should have opportunities of working himself up until he may hope to attain the highest eminence that the University can afford him. Again (remarked the speaker) we must not endeavour directly to oppose the present examination system, however much we may be convinced of its effect, as actually carried out, in sacrificing literary and scientific ability. We must endeavour, as far as we can, to enfilter our system into the examination system; and for this reason we must not talk about professors who can be planted there to pursue their original research only, and make that our single object. We must take up the whole institution of teaching in the universities, and we must endeavour to impress upon the teaching the fact which has already been dwelt upon, namely, that there can be no healthy intellectual training unless the man who conducts it is a person who is himself capable of, and has the opportunity of engaging in, original research. That is the strong point; but we must not set ourselves to go and pull down the present system of examination directly. Another notion of university reform which we shall have to meet is that notion of transplanting a certain portion of the university revenue into the manufacturing and commercial centres of the population. That is an idea which, to those who attend to what one sees in the papers on the progress of opinion on the subject of the universities, has evidently taken deep root, and which more or less runs counter to our object—not altogether, but more or less. But that idea has taken such deep root, that it is doubtful whether, if we were to try, we could prevent something of that sort being done. If these persons who are sent over to Manchester and Liverpool are entirely under our direction, and are made not mere persons who go and deliver an evening lecture for the amusement of the fashionables in Manchester, then it would be very desirable if something like a connection between the universities and the centres of population could be opened. One great complaint is, that the manufacturing and commercial interests have outgrown us; that they no longer regard us; that they do not think we have got anything worth having; and of course it would be very desirable to reconquer that class of society, and bring them back; and this tendency in the public mind, to dispose of a portion of the University money, in sending it down to these places, might be directed in such a way as to regain the possessors of wealth for us.

The Chairman put the resolution--"That a sufficient and properly organised body of resident teachers of various grades should be provided from the Fellowship Fund," which was also carried.

After one or two more speeches, it was resolved to hold another meeting in January to continue the discussion. The persons present agreed to form themselves, together with others signing the resolution, into a Society for the Organisation of Academical Study. A provisional committee was elected, and the meeting adjourned.

SOCIETIES AND ACADEMIES

London

Royal Society, Nov. 21.—"On the Mechanical Condition of the Respiratory Movements in Man," by Arthur Ransome.—"Further Experiments on the more important Physiological Changes induced in the Human Economy by change of Climate," by Alexander Rattray, M.D.—"On Linear Differential Equations" (Nos. VI. and VII.), by W. H. L. Russell, F.R.S.

Zoological Society, Nov. 19.—The Viscount Walden, president, in the chair. Mr. Sclater called attention to the two Livingstone expeditions into the interior of Africa now in preparation, and urged the importance of endeavouring to have zoological collections made in the countries about to be traversed by them.—Mr. A. D. Bartlett read some notes on the birth of the hippopotamus which had been announced at the last meeting of the Society. Mr. Bartlett called particular attention to the fact that on one occasion the young one appeared to have remained under water, without coming to the surface to breathe, for nearly fifteen minutes, and also pointed out that this was the first instance of the hippopotamus suckling her young in captivity.—A communication was read from Mr. W. H. Hudson, of Buenos Ayres, containing notes on the habits of the Vizcacha (*Lagostomus trichodactylus*), and giving some interesting details of its manner of forming burrows and living in society with other animals.—A communication was read from Mr. George Gulliver, F.R.S., containing observations on the size of the red corpuscles of the blood of the Salmonidæ and of some other vertebrates.—Dr. A. Günther, F.R.S., gave a notice of a snake from Robben Island, South Africa, living in the Society's gardens, which appeared to belong to a new species proposed to be called *Coronella phocarum*.—A communication was read from Mr. J. Brazier, containing a list of the species of *Cassidæ*, found on the coast of New South Wales, with remarks on their habitats and distribution.—A communication was read from Mr. Andrew Garrett, of Tahiti, in which he gave a list of the species of *Mitridæ*, collected at Rarotonga, Cook's Islands.—A communication was read from Mr. W. H. Hudson, containing some further observations on the swallows of Buenos Ayres, being supplementary to a previous paper on the same subject.—A communication was read from Dr. J. E. Gray, F.R.S., containing notes on *Propithecus*, *Indris*, and other Lemurs (*Lemuriana*) in the British Museum.

Linnean Society, Nov. 21, Mr. G. Bentham, president, in the chair.—On the *Compositæ* of Bengal, by C. B. Clarke. The author corroborated Mr. Bentham's estimate of the very small proportion of *Compositæ* relatively to the whole flora of flowering plants in the Indian peninsula as compared with other countries. In Bengal they show only the proportion of about one in twenty-two, and in Malacca the still smaller proportion of one in about forty-five species. The number of Indian species of *Compositæ* in De Candolle's "Prodromus" will probably have to be considerably reduced.—On Diversity of Evolution under one set of external conditions, by Rev. J. T. Gulick.—The author recapitulated the facts connected with the distribution of the *Achatinellinæ* in the Sandwich Islands, familiar to readers of this journal, and drew some general conclusions.

Chemical Society, Nov. 21, Dr. Frankland, F.R.S., president, in the chair.—A paper on the "Standardising of Acids," by W. N. Hartley, was read by the secretary. The author finds it convenient to prepare the solution for rapidly standardising acids by dissolving a known weight of metallic sodium in alcohol and diluting the solution with water; it is then ready for use. A second communication on anthraflavic acid, by Mr. W. H. Perkin, F.R.S., included an account of two new derivatives, diacetyl-anthraflavic acid and dibenzoyl-anthraflavic acid.

Anthropological Institute, Nov. 19, Sir John Lubbock, Bart., M.P., in the chair.—Mr. Heath read a paper on the Moabite jars and inscriptions lately purchased by the Germans. The author entered first into the philological and other arguments in favour of their authenticity, which the English authorities had denied. Certain points in which the Moabite stone had been hitherto considered to throw light upon the earliest forms of Hebrew were shown to be decided differently by these jars, so that the question was still open. The following was given as the inscription on the first jar:—"Inscription on his jar dedicated by Jai, servant of Isaac in Mesha, such as is raised in devotion to Nataracu. This is a devotion to Dov, wife of Domiodu, the same who in the might of her knowledge has been incorporated with Mesho. She is united with Hachuasho in Mesha, raised to unity with Daocush. May he be gracious." In the discussion which ensued it was maintained that further evidence of the actual specimens or casts from them was necessary to the final determination of the authenticity of the jars.—A paper by Capt. Burton was also read on human remains from Thorsmörk, in Iceland, describing the conditions under which parts of a human skeleton were found under a cliff where much rocky matter, possibly moraine, has fallen. No date was given to the relics, which tradition assigned to the time of "Burnt Njah." Dr. Carter Blake gave a particular description of the bones and skulls found, which appeared to accord with those of the Norwegians. He was unable to detect Esquimaux, Irish, Lappish, or Russian affinities. The horse was larger than the present Icelandic horses.

Geologists' Association, November 1.—Mr. T. Wiltshire, president, in the chair.—"On the Influence of Geological Reasoning on other branches of Knowledge," by Mr. Hyde Clarke.

Entomological Society, Nov. 18.—Mr. H. W. Bates, F.L.S., in the chair.—Mr. S. Stevens exhibited an example of *Vanessa antiopa* captured by Mr. W. C. Hewitson in his garden near Weybridge, so lately as the 1st inst. Mr. H. Vaughan exhibited *Crambus verellus*, a moth new to Britain, captured at Folkestone by Mr. C. A. Briggs; also varieties of *Vanessa Atalanta* and *Pyrameis cardui*. Mr. Meek exhibited *Nephopteryx argyrella*, a species of *Phycidæ* new to Britain, from near Gravesend; also varieties of Lepidopterous insects. Mr. Meldola exhibited a beautiful drawing of the dark form of the larva of *Acherontia Atropos*. Mr. Wallace forwarded exuviæ of some insect, apparently of the family *Tineina*, which had committed ravages amongst the dried mosses and lichens collected by Dr. Spruce, in Brazil. Mr. Müller read notes on the entomological papers existing in the "Verhandlungen der Schweizerischen Naturforschenden Gesellschaft," from 1823 to 1864.

Celtic Society of London, Nov. 12.—Dr. Carter Blake read a paper on the Celtic and pre-Celtic populations of Western Europe. After pointing out the value to be attached to traditions of pre-Celtic races, the author commented at length on the extravagant statements of Schlotheim, Berghaus, and Jagel with regard to the alleged diminution of the Celtic race. He gave a description of the *races maudites* of France, especially of the Cagots, Burhins, and Chizerots, adopting the conclusions of M. Francisque Michel, and denying the affinity of the pre-Celtic tribes to the Basques or to the Laplanders, calling attention to the confusion which existed between the various definitions of the Celtic race, the "Celts" of history, of tradition, of philology, and of craniology not being in accordance with each other. The author defined the cranial characters as those which were most permanent and best defined, such characters assigned to the Celt features, which had been described by Beddoe, Pike, and Davis, and which the author amplified at length. In conclusion he partially adopted the opinions of Dr. Knox on the moral and mental characters of the Celts.

Cambridge

Philosophical Society, Nov. 11.—The following communications were made to the Society by Mr. W. Kingsley. 1. Certain advantages in E. B. Denison's Gravity Escapement Clock for recording time by electricity. 2. Description of a Remontoir Clock invented by M. Groux. 3. Observations on certain districts in North Wales with reference to the final wasting and disappearance of the glaciers. In the last of these papers the author called attention to the evidence that Wales had in the glacial epoch been occupied by a great ice sheet from which only the summits of the mountains had projected. Much of the so-called drift on them, he urged, was only moraine matter deposited and spread on slopes during the retreat of this ice-sheet, when it had shrunk up into true glaciers occupying the valleys. He described the distribution of this and the arrangement of some of the moraines; and in conclusion called attention to a very remarkable deposit consisting wholly or almost wholly of diatoms, which existed in many of the mountain lakes of North Wales. The diatoms in these were identical with species which came from Greenland.

NORWICH

Norfolk and Norwich Naturalists' Society, Oct. 29.—A list of West Norfolk fungi was contributed by Mr. C. B. Plowright. It appears that Mr. Plowright has collected and identified no less a number than 600 species of fungi within a radius of fifteen miles round Lynn; these have all been gathered by himself, but he hopes, through the assistance of several gentlemen in other parts of the country, to extend the area included in the list, and add largely to the number of species, the total number of British fungi being about 3000 species.

PHILADELPHIA

Academy of Natural Sciences, May 7.—Mr. Thomas G. Gentry called the attention of the Academy to what he regarded as a rare and remarkable case of hybridism, which occurred between *Macacus nemestrinus*, male, and *Macacus cynomolgus*, female. After exhibiting an alcoholic specimen of the young, and a stuffed specimen of the mother which was clearly identified as *Macacus cynomolgus*, he detailed the leading characters of the two parents. He stated that the male differed from the female in being more robust and of greater dimensions; in the almost perfect smoothness of the face, which is of a pale flesh colour, while in the female it is black and invested with a close growth of short black hairs; in the absence of a crest upon the head of the male, which is a prevailing characteristic of the species (*M. nemestrinus*), and its presence in the female, which is a prominent feature of the species to which she belongs; in colour; and, lastly, in the unequal development of the caudal appendage, which in the male is about seven inches in length, and densely clothed with long hairs, while in the female it is twice the length, and nearly naked for more than two-thirds of its extent.

May 14.—Mr. Thomas Meehan observed that on several occasions, he had offered some facts and suggestions tending to prove that what are popularly termed Pine needles are not properly leaves, but rather branchlets, which, through the real leaves becoming attached for nearly their whole length to the axis or stem, had of necessity taken on themselves the office of leaves. He now wished to offer two additional observations in favour of the axial origin of these so-called leaves. In plants in general the leaves unfolded contemporaneously with the branches or axis. He could not call to mind an instance where the axis first extended to its full length before the leaves ventured to push forth from the nodes. The axial buds usually remained dormant until this final length was approached. When this occurred, or if anything happened to destroy the apex of the growing shoot, then the axial buds pushed into growth, and never to any great extent before. In the Pine family we had the following axial experience:—The buds which bore the needle were axial buds, situated at the base of the scale—the adnated leaf as he maintained. These buds remained nearly at rest until the axis had reached its full length, and in this respect coincided with the axial buds of trees in general. A pine tree in the spring season presented the appearance of an immense chandelier, with its long axial shoots as the naked burners. In this respect it is apparent that, regarding the fascicles of pine needles as branchlets, the law of folial development coincidental with axial growth finds no exception in the pine family. The next striking consideration was one derived from the nature of the inflorescence. In vegetable morphology, the floral system of plants was made up of neither leaf nor axis separately, but conjointly of both. In the inflorescence of the pine, the male catkins each took the place of a fascicle. The axial bud at the base of the leaf scale, instead of a bunch of needles, developed as a spike of flowers. This spike or catkin is metamorphosed needles. If these needles were leaves merely, we could hardly expect inflorescence to be formed from them. It would be an exception to regular rule. But regarding the needles of the pine as rather axis than leaf, their development to flowers acords with general law; and he held that it was more philosophical to accept conclusions based on general law than to hunt for new laws to account for apparent exceptions to general rule.

BOOKS RECEIVED

ENGLISH.—Biblical Psychology: J. L. Forster (Longmans).—A Manual of Elementary Chemistry: G. Fownes; 11th edition, revised and corrected by H. Watts (Churchill).—Principles of Psychology, 2 vols.: H. Spencer; 2nd edition (Williams and Norgate).—The Electro-thermology of Chemistry: T. W. Hall (Edmonson and Douglas)—Figures Made Easy: L. Hensley (Macmillan).—Easy Lessons in Arithmetic, Part I: Rev. B. Smith (Macmillan)—Records of the Rocks: Rev. W. S. Symonds (Murray).—Anecdotal and Descriptive Natural History: A. Romer (Groombridge).—Fairy Mary's Dream: A. F. L. (Groombridge).—Ivy, its History and Characteristics: S. Hibbera (Groombridge).—Buds and Blossoms (Groombridge).—On Building and Ornamental Stones: E. Hull (Macmillan).

FOREIGN.—Lehrbuch der Physik: Dr. Paul Reis (Leipzig: Quandt and Händel).—Internationales Wörterbuch der Pflanzen-namen: Dr. W. Ulrich (Trübner)—Through Williams and Norgate:—Optisch-akustische Vorschule: G. Mach.—Die Robbe und die Otter: J. C. G. Lucae.—Die Anwendung der Spectral-apparates: K. Vierordt.

DIARY

THURSDAY, NOVEMBER 28.

SOCIETY OF ANTIQUARIES, at 8.30—The Milites Stationarii considered in Relation to the Hundred and Tithing of England: H. C. Coote, F.S.A

MONDAY, DECEMBER 2.

ENTOMOLOGICAL SOCIETY, at 7.

VICTORIA INSTITUTE, at 8.—Force and Energy: C. Brooke, F.R.S.

TUESDAY, DECEMBER 3.

ZOOLOGICAL SOCIETY, at 8.30.—On the Osteology of the Marsupialia IV. Phascolomys. Bones of the trunk and limbs: Prof. Owen, F.R.S.—Contributions to Ornithology of Madagascar, III.: R. B. Sharpe.

SOCIETY OF BIBLICAL ARCHÆOLOGY, at 8.30.—On a Cuneiform Inscription containing the Chaldean Account of the Deluge: G. Smith.—Address by Sir Henry Rawlinson.

LONDON INSTITUTION, at 4.—On Elementary Physiology: Prof. Rutherford.

ANTHROPOLOGICAL INSTITUTE, at 8.—Report on Anthropology at the Meeting of the British Association at Brighton: Col. A. Lane Fox.—On Some Implements bearing Marks Referable to Ownership Tallies and Gambling from the Caves of Dordogne: Prof. Rupert Jones, F.R.S.—Discovery of a Flint Implement Station in Wishmore Bottom, near Sandhurst: Lieut. Cooper King, R.N.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Discussion on the Aba Sugar Factory.

WEDNESDAY, DECEMBER 4.

GEOLOGICAL SOCIETY at 8.—On the Tremadoc Rocks in the Neighbourhood of St. David's, South Wales, and their Fossil Contents: H. Hicks, F.G.S.—On the Phosphatic Nodules of the Cretaceous Rock of Cambridgeshire: Rev. O. Fisher, F.G.S.—On the Ventriculitidæ of the Cambridge Upper Greensand: W. Johnson Sollas.—Observations on the more remarkable Boulders of the North-west of England and the Welsh Borders: D. Mackintosh, F.G.S.

SOCIETY OF ARTS, at 8.—On the Manufacture of Horse-nails by Machinery: J. A. Huggett.

MICROSCOPICAL SOCIETY, at 8.

LONDON INSTITUTION, at 7.—The Paraffin Industry: F. Field, F.R.S.

THURSDAY, DECEMBER 5.

LINNEAN SOCIETY, at 8.—On the Skeleton of the *Apteryx*: Thomas Allis.—On New and Rare British Spiders: Rev. O. P. Cambridge, M.A.

CHEMICAL SOCIETY, at 8.—On the Reducing Power of Phosphorous and Hypophosphorous Acids and their Salts: Prof. C. Rammelsberg.—On Hypophosphites: Prof. C. Rammelsberg.—On New Analyses of some Mineral Arseniates and Phosphates: Prof. A. H. Church.

CONTENTS

ERRATA.—No. 159, p. 30: The foot-line of Fig. 1 is erroneously given "Metamorphosis of Tortoise-shell Butterfly," instead of "Metamorphosis of *Sphinx Ligustri*."—No. 160, p. 55, 1st. col, line 36: For "Prof. Geikie" read "Mr. James Geikie."