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THURSDAY, SEPTEMBER 3, 1874

FIFTH REPORT OF THE SCIENCE COMMISSION *

II.

SO much has been written recently here and elsewhere on the origin and growth of the admirable Owens College, Manchester, that we shall not repeat the details on these points furnished by the Report of the Commission. Since it was opened in 1851, it has held its way through many discouragements, and now, despite its comparatively narrow income, it is, at least from the point of view of scientific teaching and research, one of the most efficient institutions in the kingdom. Considering its comparatively recent origin and its provincial situation, the gifts bestowed upon it have been almost lavish; and yet the same complaint is made in the case of the Manchester College as is made by the two London institutions: the efficiency of the work of the College, and especially of its scientific side, is seriously crippled from want of adequate resources.

The whole endowments of the College, from its foundation till the present time, have amounted to 34,582*l.* In connection with the recent movement for the erection of new buildings, including various general and special endowments, an additional 168,300*l.* has been obtained; but even this is short by 60,200*l.* of the sum required to carry out the proposed extensions. With the prospect of this deficiency the Governors of the College cannot at present undertake the establishment of any new chairs. If, however, they had adequate resources, it has been stated that they would probably proceed to divide the professorship of English and its History, and to found new chairs of Mixed Mathematics, of Applied Geology and Mining, of Astronomy and Meteorology, and of Architecture.

The total number of students in Owens College in 1873-4 was 356, being an increase of 19 on the previous year, and excluding 140 students belonging to the Medical School.

The number of students entering the evening classes in 1872-3 was 557, which in 1874 rose to the very large number of 889.

With regard to the Owens College, the Commission makes the following recommendation :—

"Considering the strenuous and persevering efforts made by the great commercial community by which the Owens College is surrounded, and the cordial sympathy which these efforts have evoked, and which has manifested itself in the incorporation of other societies and schools with the College, and in the subscriptions and benefactions for special objects by which the exertions of the governing body have been seconded; we are of opinion that this institution has established a claim to aid from the national funds. We therefore recommend, in accordance with the views which we have expressed with regard to the two metropolitan colleges, that the Owens College should receive assistance from Government both in the form of a capital sum to be regarded as a contribution towards its building fund, and also in the form of an annual grant in aid of its working expenses, with the especial view of enabling it to complete the curriculum of studies by the establishment of new chairs."

The Newcastle College of Physical Science originated in a feeling on the part of the authorities of the University of Durham, that that University did not completely meet the educational wants of the North. To render the University more generally useful, it was thought that the best step that could be taken would be to establish a School of Physical Science in connection with it. Newcastle, as the site of this school, was preferred to Durham, in deference to the wishes of all the eminent local employers of labour.

The College was founded in 1871 for the teaching of physical science, particularly in its practical application to engineering, mining, manufactures, and agriculture. The funds necessary for its endowment were provided in part by the University of Durham, which gave, in the first instance, 1,000*l.* a year in perpetuity, which has since been increased; and, in part, by a subscription raised in the north of England.

From local sources, and by amalgamating with the College the other scientific institutions of Newcastle, 117,000*l.* may be obtained.

The amount originally subscribed was of course insufficient to provide buildings for the new institution, and the College has at present to pay rent for the premises which it occupies. It is the opinion of the witnesses that it is extremely desirable that the College should be provided with buildings of its own. Sir William Armstrong says: "We consider the present accommodation as a makeshift, but without Government assistance it would be scarcely possible to undertake" to provide separate buildings appropriated solely to the College.

It was proposed, in the first instance, to provide four professorships, viz., of Pure and Applied Mathematics, of Chemistry, of Experimental Physics, and of Geology. To these professorships, lecturers have been added in literary subjects, in Greek and Latin, in English History and Literature, in French, and in German, besides a lectureship in Mechanical Drawing. It is thought very desirable by the founders of the College that other professorships of Science should be added to those already founded; indeed, a professorship of Biology has been recently established.

The number of students in 1873-4 was 78. The course of study is one of two years, there being two examinations, one at the end of each year; the candidates who pass the formal examination in Physical Science at the end of the second year to receive the title of Associate in Science of the University of Durham.

"There appears," the Report states, "to be every reason to think that the Newcastle College of Science is serving a most useful purpose in its own neighbourhood. There can be no doubt that local colleges in the great centres of manufacturing industry are in a position to meet local requirements which central institutions in London or the national universities are unable to do.

"According to Sir Wm. Armstrong the character of the instruction should be mainly, or almost entirely, of a purely scientific character, because at present there is no difficulty as regards practical knowledge, while on the other hand there is no means of acquiring scientific knowledge.

"The claims which the promoters of the College consider themselves to have upon the Government for assistance are founded upon the national usefulness of the institution, and on the amount of local support which it

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* Continued from p. 332.

has received. Sir William Armstrong's view is that the promoters 'have a very sound claim upon the Government, considering how liberally the scheme has been supported locally. I think it would be a very fair thing if the Government, considering how much the nation benefits from the establishment of such colleges, in every case were to contribute a sum proportional to what has been raised in the locality towards the attainment of the object.'

"We concur to a considerable extent in the opinions expressed by these witnesses. The degree of success which has attended the College of Physical Science at Newcastle-upon-Tyne, both in the collection of local subscriptions and in the organisation of its system of instruction, leads us to express with confidence the hope that by further efforts of the same kind it will before long be placed in a position to establish its claim to assistance from the State."

With regard to the Catholic University of Ireland, while the Commission believes that it is calculated to do much good to the cause of scientific education, it cannot recommend Government to grant it any endowment.

"On a review of the evidence," the Report states, "we are satisfied that the establishment of the Scientific Faculty of the Catholic University has not been without advantage to the instruction of the Irish people, an advantage which might be considerably increased if this faculty could be more completely organised, and its professors increased in number and supplied with adequate means for practical teaching. And we have not failed to observe that at the present time fresh efforts are being made by the persons interested in this institution, to improve and to render more widely available the instruction afforded by it.

"It is also indisputable that the Catholic University has received, and still continues to receive, a large amount of pecuniary support. The permanency, however, of this support, which proceeds, to a large extent at all events, from annnal subscriptions levied by clerical agency, cannot be predicted with any certainty.

"The peculiar organisation of this institution," the Report concludes, "the religious restrictions imposed upon the selection of scientific professors and lecturers—restrictions the removal of which it would be idle to anticipate; the incompleteness of a large portion of its arrangements for the teaching of science, and the uncertainty of its income, preclude us from recommending that it should receive a grant from public funds."

The general outcome, then, of the Fifth Report of the Science Commission is, that University and King's Colleges, London, and Owens College, Manchester, ought certainly to receive assistance from Government, that the Newcastle College is in a fair way to prove that it deserves such assistance, and that it would not be advisable to subsidise the Catholic University of Ireland, as it is at present constituted.

J. S. K.

THE APPLICATION OF THE LAWS OF SELECTION TO AGRICULTURE

IN every phase of life the law of selection comes into play. At one time it is natural, at another time it is more or less artificial. At every time, and in every place, we see evidence of the plastic character of the materials on which the vital principle operates.

In devoting my holidays to an agricultural tour in England this season, I have visited several seed-growers who are conferring great advantages on the public by careful selection of parent plants. I can speak on this point with the experience which a wide range of observation gives. I have myself, by selection, doubled the quantity of solid matter in turnips, and nearly doubled the number of seeds in ears of wheat.

If the principle of selection were universally applied with skill and care in the raising of our seed corn, what an enormous increase would thereby be made to the wealth of the agricultural classes of Great Britain and Ireland!

In our agricultural live stock a series of results, which are truly marvellous, have been accomplished by selection. And yet the principle is understood or practised only by a very small percentage of our farmers.

If any reader wishes to understand in a general way the change that has been made within the last quarter of a century, which is the measure of the life-time of the Royal Agricultural Society of England, let him take the Society's prize lists of 1839 and 1874. In the interval, several new breeds of sheep and cattle have come to be recognised as having distinct types. Nature has had her share in the work. The soil and climate of every district impress certain characters and qualities on the animal; and, in his artificial selection, the farmer preserves these in whole or part. In studying, some years ago, the origin of the older breeds, I was much struck with the extent to which their distinctive characters were due to the natural conditions under which they rose. And in a recent inquiry into the history of the newly-established breeds, the same leading truth has become still plainer.

To give point to this short paper I derive an illustration from the influence exercised on the art of sheep-breeding by the remarkable change which, common observation tells us, has taken place in the material of garments in common use. I refer to the well-known fact that tweeds and coarse cloths are now much more commonly used than in the last generation. To meet the demand thus created the farmer has produced sheep which carry wool of longer staple than the old breeds.

My argument is well illustrated in the great plains in the west of Ireland, where the flock-owners have established a splendid new breed, called the Roscommon Sheep. In the production of this variety the breeder has of course exercised his skill in selection. He crossed Leicester tups of the very best English strains of blood with the native ewe; and he repeated this over and over again until he obtained an animal of the type which suited him. Nature aided him in his art. It may be safely asserted that some of the peculiarities of the wool, as well as some of the peculiar conformations of the body, have been the work of Nature. And it is in retaining what was so well done by Nature that the highest skill is manifested. In England the best example of the argument is possibly afforded by the Lincoln breed of sheep, which stands so deservedly high in public estimation, affording as it does great weight of carcase with a remarkably heavy fleece of lustrous wool. Then, again, let us take the dark-shaded breeds—South Down, Shropshire Down, Oxford Down, and Hampshire Down. The South Down used to be more popular than it is now. It has been giving way in many places to an animal with a larger frame and with a fleece longer in the staple. The first that arose to displace it was the Shropshire, which has been followed by the Oxford Down. Each of these breeds pays best under a given set of circumstances; and this only shows the wide field open to British farmers for profiting by the laws of selection.

I look to the development of this great principle as one of the soundest and surest means of promoting the interests of the agricultural classes.

THOMAS BALDWIN

DARWIN'S "CORAL REEFS"

The Structure and Distribution of Coral Reefs. By Charles Darwin, M.A., F.R.S., &c. Second edition, revised. 1874; pp. 268. (Smith, Elder, and Co.)

THE rising generation of naturalists and geologists has not had, and most probably will never have, such feelings of intellectual pleasure as fell to the lot of the first readers of Charles Darwin's book on Coral Reefs, which was offered to science more than thirty years since. The recent researches into the nature of the deposits of the deep sea, and the discoveries of the bathymetrical zones of water of very different temperatures, are certainly full of vast interest, and will afford the data for the development of many a theory; but the clear exposition of facts, and the bold theory which characterised the book on Coral Reefs, came unexpectedly and with overpowering force of conviction. The natural history of a zoophyte was brought into connection with the grandest phenomena of the globe—with the progressive subsidence of more or less submerged mountains, and with the distribution of volcanic foci. The forces of the organic and inorganic kingdoms were shown to unite in the production of those circular growths of coral which appeared to rise from profound oceanic depths; and it was made evident that the existence and persistent growth of fragile Porites and *Madreporæ* were dependent upon movements of the crust of the globe, the result of forces acting almost from the beginning—upon movements so vast, equable and slow, that over thousands of square miles the coral grew upwards, whilst the supporting rock, its base, and the mother crust subsided in a wonderful unison. The pristine condition of the globe was in fact brought in relation with the formation of those beautiful islands, the theme of romance and poesy, the delight of the missionary, the dread of the navigator, and which should be, according to Dana, the luxurious home of enervated and used-up investigators.

The theory of the formation of barrier reefs and atolls is stated with Darwin's usual lucidity:—"From the limited depths at which reef-building polypifers can flourish, taken into consideration with certain other circumstances, we are compelled to conclude that both in atolls and barrier reefs the foundation to which the coral was primarily attached has subsided; and that during this downward movement the reefs have grown upwards." "There is not one point of essential difference between encircling barrier reefs and atolls; the latter enclose a simple sheet of water; the former encircle an expanse with one or more islands rising from it. Remove the central land, and an annular reef like that of an atoll in an early stage of formation is left." It was necessary, in order that this theory should be valid, that the depth at which reef-building corals can exist below the surface should be ascertained, and also that direct or indirect proofs of subsidence over a vast area should be offered. The nature of the bottom of the sea immediately surrounding Keeling atoll was carefully examined, and moreover soundings with the wide bell-shaped lead, with tallow armings, were carefully taken, off the fringing reefs of Mauritius. In Keeling atoll outside the reef it was found, "to the depth of ten or twelve fathoms the bottom is exceedingly rugged and seems formed of great masses of living coral, similar to those on the margin. The arming of the lead here invariably came up quite clean, but deeply indented, and chains and anchors which were lowered in the hopes of tearing up the coral were broken." "Between 12 and 20 fathoms the arming came up an equal number of times smoothed with sand and indented with coral; an anchor and lead were lost at the respective depths of 13 and 16 fathoms. Out of twenty-five soundings taken at a greater depth than 20 fathoms, every one showed that the bottom was covered with sand." Off the reef at Mauritius, "from 15 to 20 fathoms, the bottom was with few exceptions either formed of sand or thickly coated with Seriatopora (one of the Tabulata). At 20 fathoms one sounding brought up a fragment of Madrepora which I believe to be the same species as that which mainly forms the upper margin of the reef; if so, it grows in depths varying from 0 to 20 fathoms. Between 20 and 23 fathoms I obtained several soundings, and they all showed a sandy bottom with one exception at 30 fathoms, when the arming came up scooped out as if by the margin of a large Caryophyllia." "The circumstance of the arming having invariably come up quite clean when sounding within a certain number of fathoms off the reef of Mauritius and Keeling atoll (8 fathoms in the former case and 12 in the latter), and of its having always come up (with one exception) smoothed and covered with sand when the depth exceeded 20 fathoms, probably indicate a criterion by which the limiting of the vigorous growth of coral might in all cases be ascertained.'" Darwin admits that this limit might be exceptionally transgressed, but insists upon the importance of the gradual change, as depth progresses, from living clean coral to a smooth sandy bottom, in endeavouring to fix the depth at which the reef-builders can grow.

Even at this period of Darwin's life, the importance of the struggle for existence had been recognised by him, and had influenced his thoughts. He remarks that "we can understand the gradation only as a prolonged struggle against unfavourable conditions." All subsequent investigations by many independent observers have proved the correctness of this bathymetrical limit of the flourishing of reef-builders, and of late years the general characters of the coral which can exist at a greater depth and even on oceanic floors have been shown to differ essentially from those of the forms which live and flourish amidst the rush of the wave and surf. Darwin notices that where the sea is very shallow, as in the Persian Gulf and in parts of the East Indian Archipelago, the reefs lose their fringing character and appear as separate and irregularly scattered patches, often of considerable area. Around the Philippines the bottom of the sea is "entirely coated by irregular masses of coral, which, although often of large size, do not reach the surface and form reefs." There are huge clumps of Porites and many sponges on the floor of the sea off Cuba, but although these corals belong to reef-building genera, still as species they are not those which grow on flourishing reefs. The reef-builders evidently grow with great rapidity, and their struggle against the tide and currents and waves necessitates a constant process of reparation or of growth to replace fractured branches. They flourish in the warm, highly aërated, rushing water, which is full of living things —their proper food. Beyond the reach and influence of these conditions other species and genera exist, which add to the bulk of the coral mass, but which of themselves would never build up a reef, and it is some of these which have been dredged up from considerable depths. The simple corals and the branching forms without a cellular exotheca to hold them together have an enormous bathymetrical range, and can live in water of 76° F close to the surface, and also at a depth of more than 1,000 fathoms in a temperature of less than 32°. But the true reef-builder requires a high temperature, and it therefore becomes very important to discover, as has been suggested by Dr. Carpenter, whether the vast sub-zone of cold water which underlies the superficial and heated water has not much to do with this restriction of certain species to definite depths. We must wait for the results of systematic dredging at great depths in the Pacific before this question can be for ever settled, but at present all our knowledge tends to prove that this deep stratum of cold water would prevent reef-builders from living at any considerable depth, and therefore that they never could have risen by growth from the ocean floor itself. Growing, therefore, on submerged rocks, the reef-builders must have their foundation slowly subsiding, if they are to attain a great thickness and to assume the bulk and the characters of atolls. The direct proofs of subsidence advanced by Mr. Darwin were noticed especially in Keeling atoll. "Appearances indicating a slight encroachment of the water on the level are plainer within the lagoon: I noticed in several places, both on its windward and leeward shores, old cocoa-nut trees falling with their roots undermined and the rotten stumps of others on the beach, where the inhabitants assured us the cocoa-nut would not grow. Capt. Fitzroy pointed out to me near the settlement the foundation-posts of a shed, now washed by every tide, but which the inhabitants stated had seven years ago stood above high-water mark." "From these considerations I inferred that probably the atoll had subsided to a small amount: and this inference was strengthened by the circumstance that in 1834, two years before our visit, the island had been shaken by a severe earthquake, and by two slighter ones during the ten previous years." The observations of such authorities as Williams, Kotzebue, and Stutchbury, respecting the encroachment of the sea on, and the destruction of parts or the whole of islands, were noticed by Darwin in his early edition, and comparisons were made, as in the case of Whitsunday Island, between old and new charts, in support of the evidence of subsidence. The existence of submerged or dead reefs is very properly advanced as an indirect proof of subsidence, and the condition of the Great Chagos bank was considered to explain the effects of a rapid subsidence which killed the corals. But the principal and most interesting evidence is afforded by the relative positions of active volcanic vents and barrier reefs and atolls. Darwin noticed the absence of active volcanoes in the presumed areas of subsidence, and their frequent presence in areas of elevation, the exceptions being very few. In acknowledging Dana's suggestive criticism that he had not laid sufficient weight on the mean temperature of the sea in determining the distribution of coral reefs, Darwin very properly urges that some other cause must account for the absence of coral growth in localities where the surface temperature of the sea is sufficient, and he refers especially to the islands which rise up from the abyssal sea in the Atlantic; but he indicates that temperature evidently has much to do with the absence of reefs on the west coast of Tropical America, the cold current reducing the mean temperature of the sea there below 68°.

Although investigations made subsequently to those of Darwin add almost invariably to the proofs of his theory of atoll formation, and it is received as correct by every teacher, still there have been one or two able criticisms of its general applicability. For instance, Semper, in his description of the Pelew Islands, doubted the evidence of subsidence. His opponent, with his usual justice and candour, gives Semper's objections the most careful consideration, and indeed they deserved this treatment. "He (Semper) states that the southern islands consist of coral rock upraised to the height of from 400 to 500 feet; and some of them before their upheaval appear to have existed as atolls. They are now merely fringed by living reefs. The northern islands are volcanic, deeply indented by bays, and are fronted by barrier reefs. To the north there are three true atolls. Prof. Semper doubts whether the whole group has subsided, partly from the fact of the southern islands being formed of upraised coral rock; but there seems to me no improbability in their having originally subsided, then having been upraised (probably at the time when the volcanic rocks to the north were emptied), and again having subsided. The existence of atolls and barrier reefs in close proximity is manifestly not opposed to my views. On the other hand, the presence of reefs fringing the southern islands is opposed to my views, as such reefs generally indicate that the land has either remained stationary or has been upraised. It must, however, be borne in mind that when the land is prolonged beneath the sea in an extremely steep slope, reefs formed there during subsidence will remain closely attached to the shore and will be undistinguishable from fringing reefs. Now, the submarine flanks of most atolls are very steep; and if an atoll after upheaval and before the sea had eaten deeply into the land and had formed a broad flat surface, were again to subside, the reefs which grew to the surface during the subsiding movement would still closely skirt the coast." The appendix, which contains a detailed description of the reefs and islands in the well-known coloured map, is of the greatest value to the physical geographer, and it includes notices of nearly every known coral tract.

After reading and pondering over this long-prized work, there comes the feeling that Mr. Darwin should at some future time enlarge its scope and deal with the distribution of coral species, and trace back in time the reefs of old. Who would not be glad to be taught from the vigorous pen of the man whose theory has lasted more than thirty years, and will last as long as science, what was the condition of the vast Pacific area prior to the age of reefs and atolls? Mountains of different heights are now more or less submerged, and either capped with vast thicknesses of coral, or their tops are girt with barrier and fringing reefs. Take away the sea and the coral growth, and imagine the conditions which prevailed during the slow piling up of these volcanic rocks, their denudation and final overwhelming by the inrush of the ocean incident to the first phase of subsidence. Little is known concerning the age of the raised reefs of the Pacific, and therefore of the duration of the existing state of things; but in the Caribbean there have been reefs in consecutive ages since the early Cretaceous period, and in that area there have been during past ages subsidences and upheavals with contemporaneous volcanic action, following the same laws as those so elaborately described by Darwin as influencing coral growth in the Pacific.

P. M. D.

LETTERS TO THE EDITOR

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

The Long Peruvian Skull

IT was not my intention to have replied to Dr. J. B. Davis's letter on "The Long Peruvian Skull" in NATURE, vol. x. p. 123, as I shall have an opportunity before long of presenting the subject in detail before scientific readers. I find, however, by letters from England that an answer is expected from me. To me, it seemed little more than a reiteration of his disbelief in the existence of such a type; while it leaves unnoticed what I specified as the main point in the discussion.

Dr. Davis demands the production of "half a score of ancient Peruvian dolichocephalic skulls, the appearance of which totally precludes the possibility of interference by art, or other deforming process." Had an anonymous correspondent so stated the issue, I should have supposed that the writer had never seen half a score of Peruvian skulls in his life. The collection presented by Mr. Hutchinson to Prof. Agassiz numbered 368; and out of this Prof. Wyman reports only *eleven* not flattened or distorted. Is Dr. Davis prepared to rule the remaining 357 out of court as of no value in relation to his brachycephalic type? This question of Peruvian long and short heads must be settled in connection with a deforming element affecting both types, or it cannot be settled at all. Hence my specification of the real issue. Keeping this in view, I must beg leave meanwhile to refer, for the sake of brevity, to my statements in NATURE, vol. x. p. 48, in reference to examples previously adduced; while I now point out others easily accessible to Dr. Davis.

The large collection furnished to Prof. Agassiz was obtained, apparently at one time, from a single locality, "Ancona and its neighbourhood." Hence no doubt the uniformity of type. Doubling this number of skulls from the same locality would add nothing to the evidence. It is otherwise with the London Anthropological Institute. Its collection was obtained at different times, partly from the same accessible locality; but also from Santos, Ica, Passamayo, and Cerro del Oro. These include places hundreds of miles apart; and Prof. Busk, after minute study, reports that the evidence of the existence of a dolichocephalic type afforded by the collection, though "not very abundant, *is nevertheless decisive.*"

It is a case precisely analogous to the remarkable dolichocephalic British type recognised by the acute sagacity of the late lamented Dr. Thurnam, in the Uley, Kennet, Littleton Drew, Rodmarten, and other long barrows in Wiltshire, &c., as illustrated in the *Crania Britannica*, for which so great a debt of gratitude is due to Dr. Davis and his gifted colleague. Those dolichocephalic skulls are exceedingly rare; they are found along with brachycephalic skulls; but, as Dr. Thurnam showed, accompanying elements suggestive of the latter as an inferior or servile class. Long ago, in a paper in the *Canadian Journal* of September 1862, I referred to the analogy this presents to the long Peruvian skull mingling in the ancient Inca cemeteries with crania of a markedly diverse type.

No multiplication of specimens of the less rare brachycephalic skull of the British cist or round barrow will invalidate this exceedingly rare but valuable dolichocephalic British type produced by Dr. Thurnam; and the exhibition of a whole ship's cargo of brachycephalic skulls from the accessible coast cemetery of Ancona is equally ineffective in disproof of the rare Peruvian dolichocephalic skull of Titicaca and other ancient burial-grounds.

Dr. Davis refers to an error in one of the woodcuts of my first edition of "Prehistoric Man." To anyone conversant with the difficulties of a Canadian author correcting proof-sheets for the London press, the chances of error, with proofs passing while the woodcut swere in the engraver's hands, and their mere titles or blank spaces in lieu of them, must be obvious enough. Dr. Davis will find the error pointed out in the preface to the second edition.

University College, Toronto, Aug. 6 DANIEL WILSON

Pollen-grains in the Air

I AM very sorry to find that, owing to my absence from home at the time, a question addressed to me by Mr. A. W. Bennett, in NATURE, vol. ix. p. 485, has escaped my notice hitherto and remained unanswered. Mr. Bennett, alluding to my letter on "Microscopic Examination of Air" (NATURE, vol. ix. p. 439), asks on what ground I refer the "triangular pollen" captured on my slide to the birch and hazel. The identification resulted from comparison under the microscope. The pollen-grains which I obtained from catkins of birch and hazel exhibited three conspicuous equidistant prominences (pores) giving each grain a triangular appearance. I cannot now remember if this appearance was equally distinct before and after immersion in glycerine: probably there was a change of shape due to osmosis. I confess that I used the word "triangular" not in its strict geometrical meaning, but in order to mark a feature which distinguished the pollen-grains of birch and hazel from those of poplar. Referring to my notes, I must admit that the shape of the grains which I identified with birch pollen would have been more accurately described as "spherical with three large protuberances."

Blackheath, S.E., Aug. 31 HUBERT AIRY

Chrysomela Banksii

I SHOULD be much obliged if you would allow me to ask the following question of Coleopterists in the columns of NATURE:—

Does *Chrysomela Banksii* possess any quality, such as that of exuding an acrid liquid or the like, which would be likely to make it distasteful to spiders or other animals? I have seen it first taken and then rejected unharmed by a Trap-door Spider, and as these spiders feed largely on beetles, I am led to suppose that this particular beetle has some special protection.

J. TRAHERNE MOGGRIDGE

2, Foxton Villas, Richmond, Surrey, Aug. 27

The Aurora Borealis

MAY I ask the readers of NATURE for information on the following points:—

1. Where can I find references to any observations on the polarisation or otherwise of auroral light?

2. Are there any published lists of auroræ arranged with a view to determine the periodicity of its recurrence: or, if not so arranged, sufficiently extended for such an investigation?

3. Has any observer besides Mr. Backhouse noted the relative proportion between eastward and westward movement of auroral rays?

North Shields, Aug. 29 HENRY R. PROCTER

ROBERT EDMOND GRANT, M.D., F.R.S.

ON Sunday, August 23, after an illness of about a fortnight, died Dr. R. E. Grant, for many years Professor of Zoology and Comparative Anatomy at University College, London. The family from which Dr. Grant was descended had its head-quarters in the county of Elgin, whence his father removed to Edinburgh, settling as an accountant and a writer to the signet in Argyll Square. He was one of fourteen children, twelve brothers and two sisters, being the seventh son, and the longest surviving of them all. Neither he nor any of his brothers were married; one sister was, but she left no children. He was born in 1793. Between 1803 and 1808 he was a pupil at the High School, Edinburgh, after leaving which he entered the University of that city as a medical student, attending the lectures of Drs. Monro, Hope, Gregory, Duncan, and others. He took his doctor's degree in 1814, for five years after which he devoted his time to travelling on the Continent, visiting Paris, Rome, Florence, as well as Germany, Bohemia, Hungary, and Austria. In 1822 he settled in Edinburgh, and from then till 1828 contributed several zoological papers to different Scotch scientific societies and journals, including one to the Wernerian Natural History Society, in 1827, on the circulation of fluids through the structure of sponges, in which attention was first drawn to the function of the ossicula and pores of those animals, and which led Mr. Fleming to give the generic name *Grantia* to one member of the family.

In June 1827, whilst still in Edinburgh, Dr. Grant was elected Professor of Zoology and Comparative Anatomy in the new University of London, then being formed; his first lecture was not however delivered until October 1828. For the first few years after he settled in London he communicated several papers on zoological subjects to the Scientific Committee of the Zoological Society, some of which, on points in the anatomy of *Sepiola*, *Loligopsis*, and *Beroë*, read in 1833, are to be found in the first volume of their Transactions. From that time Dr. Grant published no papers of importance.

In 1836 Dr. Grant was elected a Fellow of the Royal Society, and in 1837 he was appointed to the triennial Fullerian Professorship of Physiology at the Royal Institution in Albemarle Street.

At his classes, during one session, it is said that Dr. Grant had only two attendants, these being Mr. Hallam, the illustrious historian, and a young boy; it was always a matter of surprise to the other students of the college how he managed to adapt his lectures to the mental capacity of this trying audience.

During the forty-six years that he held his professorship, he never missed a single lecture. It was his determination, if he had lived, to resign his appointment during the present year.

In disposition Dr. Grant was very retiring and seclusive, and a great reader. He travelled much and was an excellent linguist; so fond of languages was he, that only two years ago he attended lectures on Anglo-Saxon in University College. By his will Dr. Grant leaves his extensive library and all his private collection to University College, together with a sum of money to be employed in maintaining and extending the zoological and zootomical department of the library of the college.

CONFERENCE FOR MARITIME METEOROLOGY

A GENERAL wish having of late been expressed that the measures for the prosecution of Maritime Meteorology, proposed at the International Conference at Brussels in 1853, should be reconsidered, now that the experience of more than twenty years of the operation of these measures has enabled meteorologists to form opinions as to their utility, a conference is now being held at the Meteorological Office, 116, Victoria-street, consisting of the following gentlemen —Austria—R. Müller, K. K. Hydrographic Office, Pola. *Belgium—Van Rysselberghe, Navigation School, Ostend. Bengal—H. F Blanford, Meteorological Office, Calcutta. China—J. D. Campbell, Secretary Commissioners of Maritime Customs. Denmark Capt. N. Hoffmeyer, Meteorological Institute Copenhagen. France C Sainte-Claire Deville, Inspector of Meteorological Stations; A. Delamarche, Ministry of Marine, Paris. Germany W H. von Freeden, Deutsche Seewarte, Hamburg; G. Neumayer, Hydrographer, Berlin; Capt. Stempel, Imperial Navy; H. A. Meyer, Commissioner for Investigating German Seas, Kiel. Great Britain -(Board of Trade), Capt. Toynbee; R. H. Scott, Director Meteorological Office, Hon. Sec.; *(Admiralty), Rear-Admiral Nolloth; R. J. Mann, M.D., President Meteorological Society. Holland—Buys Ballot, Royal Meteorological Institute, Utrecht, President; Lieut. J. E. Cornelissen, R.N. Italy—Capt. N. Canevaro, R.N. Norway—H. Mohn, Meteorological Institute, Christiana. Portugal—J. C. de Brito Capello, Observatory, Lisbon. Russia—Capt. M. Rikatcheff, I.R.N., Central Physical Observatory, St. Petersburg; *A. Movitz, Observatory, Tiflis. Spain—C. Pujazon, Marine Observatory, San Fernando; Captain Montijo, S.N. *Turkey—Admiral Hobart Pacha. The basis of discussion will be the Report of the Brussels Conference above referred to, with some other heads relating to instructions, instruments, &c. The Conference will be divided into two sub-committees:—1. Instruments; 2. Observations. A Report of the proceedings will be published by the Meteorological Committee. A programme has already appeared in NATURE, vol. x. p. 152.

DEEP-SEA SOUNDINGS IN THE PACIFIC OCEAN

WE take the following extracts on this subject from a report made to the United States Secretary of the Navy by Commander George E. Belknap, dated United States Steamer *Tuscarora*, Hakodadi, Japan, June 26:—

"I left Yokohama on the 8th inst., and at dawn the next morning began the work of sounding homeward on a great circle passing through the island of Tawaga, of the Aleutian group, and towards Puget's Sound. When about 100 miles east by south from Kinghasan or Sendai Bay, on the east coast of Japan, the lead sank to a depth of 3,427 fathoms, showing a descent of 1,594 fathoms in a run of 30 miles. The result seems extraordinary at so short a distance from the land, but the next coast revealed a depth still more astonishing, the sinker carrying the wire down 4,643 fathoms without reaching the bottom.

"On this occasion, when some 500 fathoms of wire had run out, the sinker was suddenly swept under the ship's bottom by the strong undercurrent, and all efforts to get the wire clear and keep it from tending underneath were unavailing, the difficulty being increased by a fresh breeze and a moderately heavy sea. Finally, when 4,643 fathoms of wire had run out, and only 150 fathoms of wire were left on the reel, it broke close to the surface, and about five miles were lost.

"The strain on the reel was very great, and notwithstanding a weight of 130 lb. on the pulley line, it took three men to check and hold the drum, and the wonder was that the wire had not parted sooner. This great strain must have been due to the action of the strong undercurrent upon the sinker, sweeping it with great force from the ship, as since that cast we have sounded repeatedly in depths of more than 4,000 fathoms, and had no trouble in reaching the bottom.

"The position of the cast, as shown by observation was about 45 miles distant from the previous one, the strong current having carried the ship beyond the position where it was intended to sound. .

"I determined to run back inshore and skirt the stream, beginning a new great circle off Point Komoto, in latitude 40° north. I also concluded to increase the weight of the sinker some 20 lb.

"It will be seen, by an inspection of the track chart of sounding, that the moment the second line diverges from the coast of Niphon and enters the edge of the Japan

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Not present at the meeting on Aug. 31.

stream, but yet runs parallel to the island of Yesso, the water begins to deepen rapidly, and at the cast No. 24, or the third cast from the initial point of curve, a depth of 3,493 fathoms is found. Forty and eighty miles further on depths of 3,587 fathoms and 3,307 fathoms are reached; then the ocean bed or trough of the stream drops nearly a statute mile in the run to the next position, where the sinker is not detached until it has descended to the extraordinary depth of 4,340 fathoms.

"A good specimen of bottom soil was brought up from that great depth, and the Miller's Casella thermometer, No. 18,136, came up a perfect wreck.

"The next six casts were made in over 4,000 fathoms water, the last two revealing depths of 4,411 fathoms and 4,655 fathoms respectively, and on both occasions the wire was lost.

"Sometimes the wire comes in much easier than at others, and cast No. 31, made in 4,120 fathoms, occupied only 1h. 47m. 42s.

"The difference must be due to the varying action of the undercurrents upon the rod, specimen cup, and small lead, increasing or diminishing the resistance in hauling in, according to the extent of curve from the perpendicular.

"The conditions under which all these deep casts were made were eminently favourable. Believing that such deep water would be impracticable for cable purposes, I resolved to run inshore and sound back along the coast of the Kurile Islands to the position of cast No. 22, then to return and skirt those islands and the coast of Kamschatka as far as Cape Chipounsky, then passing over to the Alutian group.

"If the time on the great circle route for the proposed cable has failed, at least for the present, the results of these soundings will be of interest and value to hydrographic science, as establishing the fact of depths in the sea hardly to be expected, in view of the numerous soundings made by her Majesty's steamship *Challenger* and this ship, over wide expanses of the Atlantic, Pacific, and Indian Oceans, and confirming the existence of a very deep trough under the Japan stream, similar to that cut by the Gulf Stream on our own coast.

"As we passed by Sturup, of the Kurile group, dense volumes of smoke were seen rising out of a crater on the east end of the island."

PROCEEDINGS OF THE FRENCH ASSOCIATION

ON Sunday the 23rd there was an excursion to Boulogne, to visit the steel-pen factory established by the Blanzy Company, and the Laboratory of Zoology, which Prof. Giard of Lille has organised by the seaside. On Monday many members paid a visit to Turcoing and Roubaix, two large manufacturing places in the vicinity of Lille, where the visitors were received with much courtesy; every workshop was eagerly thrown open for inspection.

At a general session held in the evening, M. Ménier, the large chocolate manufacturer who has realised an immense fortune in his trade, delivered a very appropriate lecture on the creation of wealth by science. No one has had so much practical experience on that subject in the society. M. Alglave, formerly a professor in the Academy of Douai, gave an impressive address on coal-mining in Northern France. It was the first time that M. Alglave, who is very popular in Northern France, was allowed to deliver an address since he got into difficulties with the Government. His address created quite a sensation in the city.

On Tuesday there was a general excursion to Anzin coal-mines. A splendid luncheon was given to the visitors by the Anzin Company, in a large storehouse tastefully ornamented for the occasion with national flags and a trophy of all implements used by miners in their underground industry. M. Marsilly, the general director, proposed "The Visitors," in the name of the Council of Administration. M. Wurtz, in return, proposed "The Council and the illustrious President," whom he did not name, but who is no less a person than M. Thiers, at the mention of whose name enthusiastic cheers broke forth, interrupting M. Wurtz for more than five minutes. M. de Marsilly delivered a very long and able address, summarising all that the mining industry owed to science, and giving a few curious figures relating to his Company. It is 137 years old, and was the first French firm to import steam-engines from England. The number of hands is 15,000, and persons depending upon them 60,000. They are now constructing steam-engines, of 500 horse-power, for underground work. The society visited the Haveley pit, one of the forty belonging to the Company, whose concession covers about 100,000 acres, and is said to be worth more than 8,000,000 sterling. On the same evening M. Gaston Tissander delivered an address on aërostation specially considered as to its meteorological uses. The lecture was illustrated by diagrams showing forms of clouds, optical phenomena connected with aërostation, &c.

On Wednesday all the Sections were busy discussing the several communications, and held two sessions. M. Bergeron gave a most interesting address in the Engineering Department, on the boring of the tunnel between France and England. He said, upon authority, that the French Government had sent to Lord Derby a note asking him if he objected to the granting of the exclusive right for a number of years to a private Company. If the English Government does not raise any objection, the bill will be laid before the Versailles Assembly at the end of the present parliamentary holidays. Special provisions will be made for inundating the tunnel in case of war breaking out between the two countries. The holders of the concession can renounce their rights after spending 80,000*l.* in boring a gallery of exploration at least 1,100 ft. under the sea from low-water mark. The works are to begin on the French side as soon as the concession will have been granted. MM. Léon Say, Rothschild, André, &c. are amongst the petitioners.

There was a very sharp discussion in the Anthropological Section on some theological points which had been raised.

In the evening Col. Laussedat delivered a lecture on optical military telegraphy. Almost all the officers of the garrison were present at the lecture, which was practically illustrated by various experiments.

In the morning of Thursday the business of the Sections was transacted as on the previous day, and at one o'clock a general meeting was held in the Hôtel de Ville under the presidency of M. Wurtz. Some modifications of the by-laws and regulations of the society were unanimously adopted, and the committee was instructed to ask from the Government a decree declaring the society of public utility. This is a step necessary, according to the French laws, to give societies the right of holding properties, accepting legacies, and obtaining parliamentary grants.

M. Wurtz had directed a message to the British Association asking them to send a delegation to take part in the Lille meeting. This could not be accomplished, owing to the distance, but it ended in an exchange of telegraphic courtesies between the two societies.

The British Association being our model, it is necessary for us to study its workings, in order to adapt them as far as we can to our French circumstances and scientific peculiarities. Consequently the committee was instructed not to name the opening day for the 1875 meeting before ascertaining whether it shall not coincide with the opening of the next session of the British Association.

Two cities were in competition for the 1875 meeting—Clermont Ferrand, where the Puy de Dome Observatory will be inaugurated next year; and Nantes. It was generally supposed that Clermont Ferrand would be selected, but Nantes had sent a special delegate with the power of offering the grant of a large sum of money. Clermont Ferrand is poor and has drained its exchequer in helping M. Alluard in his admirable work; consequently Nantes was all but unanimously selected. The president for the Nantes meeting (1875) will be M. d'Eichtal, a gentleman of great fortune and influence, largely connected with the railway interest, and possessed of high scientific qualifications, having been educated at the Polytechnic School. The assembly appointed M. Faye, the astronomer, to be president of the 1876 meeting, but the town where it is to be held has not been decided on. The meeting was brought to a close by a banquet given at the Hôtel de Ville, by the Mayor of the city.

The number of the members of the Association is 800; it is an excess of 200 on the number of the Lyons meeting. The ladies are very few. Madame Thureau de Villeneuve, the wife of the secretary of the Société de Navigation Ardenne, was the only lady who delivered an address. This was in the section of Geography.

The Paris papers have published very short articles on the proceedings of the Association; none have shown so much interest as the *Times*, who sent a special reporter and published long telegrams on the work of the Sections.

Lille, August 29 W. DE FONVIELLE

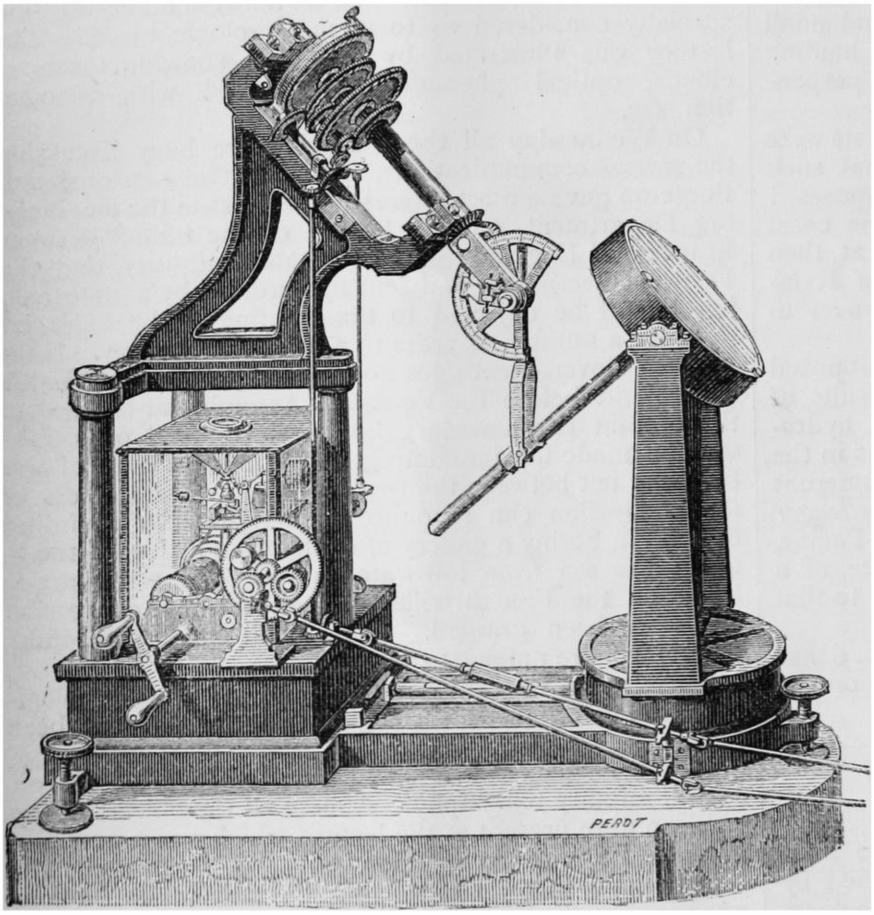

FIG. 1.—The Siderostat.

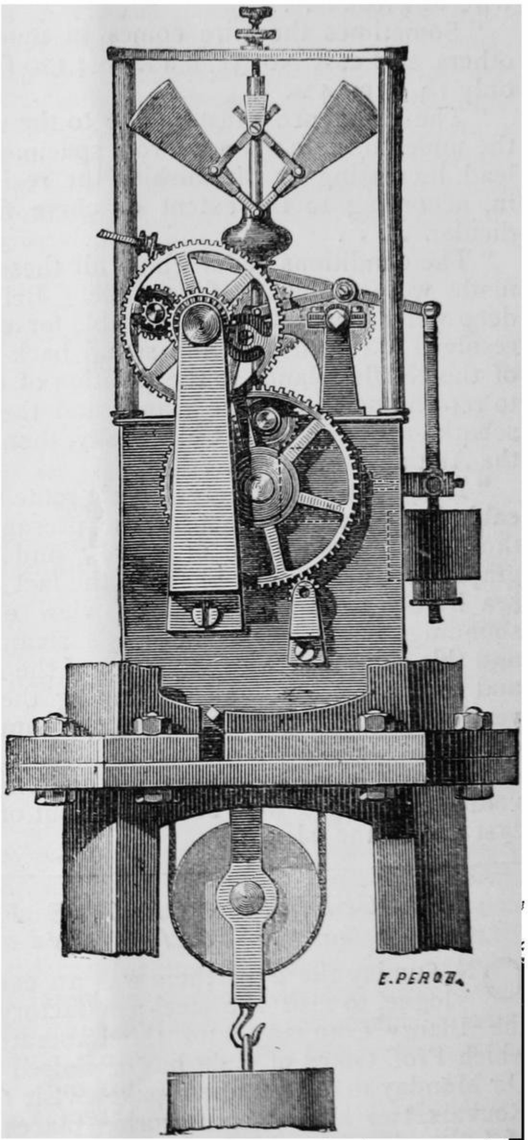

FIG. 2.—Clockwork movement, with isochronous regulator

*THE SIDEROSTAT**

THERE is in use at the present moment in the Paris Observatory an instrument of a new construction, which is destined to play a large part in the Astronomy of the future. It is not too much to say that the new instrument will play as important a part in, and will be as essential to the new Astronomy, as the transit instrument plays in the Astronomy of position.

For this instrument in its present form we are indebted to the genius of Foucault, who also gave it its name, the Siderostat.

The use of the present instruments obliges the astronomer to change his position to follow the eye-piece, and consequently to observe frequently in uncomfortable positions. To escape this inconvenience the Germans have long employed the bent telescope, meridian circles and theodolites. But the use of this arrangement is limited to small instruments, while it is precisely in the case of the largest instruments that it would be most useful.

Foucault, who died in the midst of his most important labours, wished in the latter years of his life to give to the equatorial the power of making the entire heavens pass before the observer without his having to disturb himself or to displace the instrument. A telescope fixed horizontally in an invariable position, before which a plane mirror brings successively the various points of the sky—such was the Siderostat in his mind, the idea in all probability having occurred to him from a singular employment of the heliostat by M. Laussedat in observations of the eclipse of 1860. (See Fig. 1.)

The instrument was constructed after the death of its inventor, by M. Eichens, under the direction of the Commission charged with the carrying out and the publica-

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In part translated from an article by M. A. Fraissenet, in *La Nature*. For the woodcuts we are indebted to the kindness of M. Gauthier-Villars.

tion of the works of Foucault, and at the expense of the Imperial treasury. It was presented to the Academy of Sciences on December 13, 1869, then given by Napoleon III. to the Observatory, where it has been installed since 1872.

The instrument, as designed by Foucault, of which M. Wolf has published a complete and detailed account, rests on a brass stand supported by three screws, with two levels placed crossways, and a regulating azimuth movement. There are three distinct parts—the mirror and its mounting, the polar axis and the mechanism which connects this axis with the mirror, and lastly the regulator.

The plane mirror, 30 centimetres in diameter, was constructed by M. Ad. Martin, according to the method devised by Foucault; it is carried by a horizontal axis on the top of two vertical supports, which turn round a centre. This movement is perfectly effected by means of a circle of small wheels placed at the foot of the supports. The mirror is kept in its mounting by means of cleats and spiral springs, in order to avoid all irregularity of surface. In the centre of the mounting is fixed perpendicularly a directing handle, which slides through a ring carried by a fork jointed to the lower extremity of the horary axis. The direction of the incident ray being that of the axis of the fork, and the length of this fork being equal to the distance of its point of articulation from the horizontal axis of the mirror, the line which measures that distance gives the constant direction of the reflected ray.

Finally, a clockwork movement, the isochronous regulator of Foucault (Fig. 2), placed at the foot of the instrument, communicates to the mirror a motion sensibly equal to the diurnal motion, so that the celestial bodies maintain invariable positions in the field of a horizontal telescope, in front of the apparatus directed towards the mirror.

The entire apparatus, the principle of which is the same as that of the heliostat, rests on a triangular support; a hole on the north side receives the weight which drives the clock. A wooden cabin, moving on wheels from north to south, forms a shelter for the instrument. For the purpose of observation the siderostat is completely exposed by rolling the hut towards the north. The telescope, supported on two pillars, is placed in a brick hut, some little distance from the siderostat; this hut is very slightly elevated for the purpose of intercepting the least possible portion of the southern sky. A telescope with a mirror of silvered glass, pierced in the centre to receive the eye-glass, is the one at present employed.

If it is desired to bring into the telescope the light proceeding from a star whose polar distance and right ascension are known, this is done by two circles, which correspond, the one to the polar distance and the other to the horary angle for the moment of observation in the usual way. Then, the circles being fixed, the clockwork is put in motion and the mirror throws continuously into the telescope the rays proceeding from the star under observation. The clock movement, already applied to some great equatorials, is perfectly regular, and obtained for its clever maker, M. Eichens, the grand prize in the mechanical arts at the Universal Exhibition of 1867.

It was necessary to possess, for the siderostat, some means of adjustment so as to be able to vary in very small quantities the horary angle or the polar distance without stopping the movement. The former variation is obtained by means of a subsidiary wheelwork which has already been long in use. But the variation of the polar distance was more difficult to accomplish; M. Eichens, however, has solved the difficulty after a very ingenious fashion.

The siderostat, since its construction, has been almost exclusively employed for photographic experiments in connection with the approaching transit of Venus. Consequently we do not yet know what results we have a right to look for. But in the ideal of Foucault, the instrument ought to be an indispensable auxiliary of physical astronomy; this is its proper purpose. Experiments which demand perfect steadiness will be advantageously made, such as the measure of the positions of spectrum lines and of the displacement of these lines by means of fixed spectroscopes of large dimensions. It is easy to conceive, besides, the numerous advantages resulting from the fixed direction of reflected rays. We may henceforth adapt, with the greatest ease, to the observing telescope, the apparatus necessary for the work of celestial photography for photometric researches.

The complete instrument, telescope and siderostat, placed in the plane of the meridian, may be regarded as a meridian instrument; and the determination of the right ascensions and polar distances of known stars will enable us to rectify the adjustment already made of the relation between the telescope and the siderostat. The purpose is evidently thus not to obtain a transit instrument, but only to get an approximation equal to that of equatorial observations. It is, besides, always in our power to increase the precision by comparing the star under observation with a well-known neighbouring star.

Observations by means of the siderostat may be made in two ways—with the mirror fixed, or turning under the action of the clockwork. In the former case, the instrument becomes to some extent an equatorial, but with the advantage to the observer that he has not to change his position. An inconvenience appears here; each time that the mirror is moved the direction of the apparent movement changes, and consequently it becomes necessary to make a new adjustment of the micrometer threads.

This inconvenience is more serious if, when the mirror is in motion, it is desired to effect measurements of double stars. In this case the direction of the diurnal motion changes the angles of position. It is then necessary to measure the angles of position by starting with the vertical and the horizontal, and, by means of the hour of observation, reducing them to the ordinary form.

The real defect of the siderostat, which, however, it has in common with all other instruments of observation, is that it does not enable us to examine the entire heavens. But the most interesting region for research is comprised between the pole and the southern horizon, and the siderostat which we have described permits observations between these limits. Should it be desired to investigate the rest of the sky, a second siderostat would be necessary, reflecting the rays towards the north.

Let us not, in conclusion, forget that the reflection from the mirror of the instrument causes a slight loss of light: the proportion of light reflected is constant and equal to 93·100 of the incident light for new silver.

From this description it is clear that it is only from the standpoint of physical astronomy that the employment of the instrument will be most useful; and no doubt, in this direction, it will give numerous and important results. The problems of the universe offer, indeed, a productive and inexhaustible mine, and the new astronomy, with its powerful means of investigation, gives us reason to hope that future researches will bring to light some brilliant discoveries.

NOTES

THE *Western Morning News* has received from its correspondent on board the *Challenger* an account of the voyage to New Zealand, which has been stormy and protracted. The result of the soundings has been most satisfactory, and it is confidently expected that New Zealand will be telegraphically connected with Europe next summer. The bottom was sand and mud, gradually shelving to a depth of 2,600 fathoms, at which it remained very evenly for a long distance, the temperature at this depth being 33 degrees and at the surface 64 degrees. At this point the soundings commenced getting less, and the next was found to be 1,075 fathoms (temperature 36 degrees). Two days after this 1,100 fathoms was recorded, the temperature rising to 36 degrees. These indications of shallow water were not without cause, for on the second day they came unexpectedly into 400, 350, and at last only 275 fathoms. This was about 200 miles from land. The future movements of the *Challenger* have now been arranged, and are thus stated :—At Wellington we remain till July 6, then proceed along the east coast, probably calling at Auckland for a few days, after which a course will be shaped to Tongataboo (Friendly Islands), and from thence to Kandsvan (Fiji Islands), where a supply of coal will be taken on board prior to leaving for New Guinea. Here a complete series of explorations and soundings will be made, and it is expected that the dredge and trawl will bring even greater wonders of marine life to the surface than have yet been secured, while the question of coral reefs and their history will have special attention. After cruising about Polynesia generally for some time, we expect to reach Hong Kong early in November, where probably a month will be spent in coaling, provisioning, refitting, &c.

THE last number of Petermann's *Mittheilungen* contains a summary of the recent work done by the *Challenger* Expedition, which is accompanied by an excellent and ingeniously constructed series of coloured diagrams, showing the distribution of temperature in the North and South Atlantic, as well as the configuration of the bottom over which the *Challenger* has sailed. The number also contains the continuation of Prof. Hans Höfer's paper on the structure of Novaya Zemlya.

THE growth of tea and sugar in European soil are perhaps branches of culture which we can scarcely expect to be remunerative in a commercial point of view. Be this as it may, the sugar-cane is now grown and sugar manufactured to some extent in the neighbourhood of Malaga, Spain. Tea has also been introduced into the southern districts of Sicily, and though the first attempt made last year to raise the plants on a large scale was not successful, owing, it is said, to the injury caused to the plants and seeds by immersion in sea-water on their transit from Japan, it is confidently hoped and believed by the promoters that another attempt with healthy seeds and plants will prove quite successful. Meanwhile tea is being grown at the Cinchona plantations in Jamaica, and a sample has recently been received at the Kew Museum which was grown and manufactured as above from Assam tea plants received through Kew in 1868. So far as the appearance of the sample is concerned, it is roughly manipulated, not being sufficiently twisted or curled, and apparently not sufficiently roasted. Nevertheless, its manufacture is little inferior to that of the earliest samples of Assam tea that appeared in the English market. Its quality, however, is another thing, for it produces a very watery infusion of a very herby flavour, and devoid of the aroma for which tea is noted. Care, however, in the cultivation of the plant, as well as in the selection and manipulation of the leaves, may in time produce a more marketable article.

THE Ochro (*Abelmoschus esculentus*), a Malvaceous plant, is well known in all tropical countries, being cultivated for the sake of its fruits, which are gathered in a green state, and either boiled and eaten as a vegetable, pickled in vinegar like capers, or used for thickening soups on account of the mucilage they contain a common property of the Malvaceæ. In India the seeds are sometimes boiled for making a mucilaginous drink. But we now learn that a fine oil has recently been discovered in them of a quality equal to olive oil, and that it is intended to introduce this oil to commerce. Supposing the oil to be all that is said about it, the question arises as to the supply of seeds. Though the plant is easily cultivated, can it compete with other oleaginous plants?

WE some time since noticed the formation, in connection with the French Geographical Society, of a Commission of Commercial Geography. Under the patronage of this Commission a joint stock company has been formed for the publication of a weekly journal to assist in carrying out the objects aimed at by the Commission. The title of the journal will be *L'Explorateur, Journal Géographique et Commercial.*

EXPERIMENTAL verifications are becoming daily more numerous in favour of the view that the phenomena attending the electrical stimulation of the brain are, in reality, dependent on the indirect excitation of the cerebral basal ganglionic centres by the currents employed. Besides the observations of Dr. Sanderson on this point, already published in this journal (NATURE, vol. x. p. 245), Dr. J. J. Putnam has recorded the results attending electrical stimulation of the so-called surface-centres after their almost complete separation from the rest of the hemisphere in the form of flaps. He finds that under these circumstances no movements follow the excitation; but that if the flap is raised and the surface below it irritated, a current slightly more powerful than the minimal required in the uninjured condition produces exactly similar results. The details of these experiments, taken from the *Boston Medical and Surgical Journal*, will be found in the *London Medical Record* for last week.

THERE has been issued from the Standards Department, by Mr. H. W. Chisholm, an account of the comparisons at that department between two Russian pendulums and Repsold's scale of 21 old French inches, and between Repsold's scale and the standard subdivided imperial yard.

THE French Geological Society has decided upon holding its next meeting at Mons, in Belgium, a most interesting place for excursions. It is very seldom that French Scientific Societies meet in a foreign land.

ON Friday evening M. Flammarion, the French astronomer, started from La Villette gas-works, Paris, in a balloon called *Lumen*, at half-past seven, with a brisk breeze from the north-west. The balloon was under the guidance of M. Jules Godard, and M. Flammarion, who was married in the beginning of August, was on board with his young wife; he wishes to spend his *lune de miel* in Italy. Such a trip was proposed in the beginning of the century to the celebrated Mdme. de Stael by the great philosopher, Saint-Simon; but the lady declined. The moon was full and bright.

THE use of carrier pigeons for press purposes is on the increase, and the breed is rapidly improving. By careful "selection" and allowing only the "survival of the fittest," powers have been developed which a few years ago would have been thought impossible. They can be specially trained to fly over 500 miles, and it is no uncommon thing for despatches to be brought to London from Paris, Lisbon, or Brussels. *Land and Water* records a case of interest. An ocean homing bird, of great docility, intelligence, and spirit, has been found in Iceland which flies at the meteor-like speed of 150 miles an hour. A pair of these birds whose present home is in Kent, within ten miles of London, recently carried despatches from Paris to their home in one hour and a quarter. Press pigeons carried on the despatches to London, and the whole journey of the despatches from Paris to London occupied only one hour and a half. The press pigeons now commonly used are not the ordinary carrier pigeons, but are bred by Messrs. Hartley, of Woolwich, from prize birds selected from the best lofts of Antwerp, Brussels, and Liege.

AN alarming shock of earthquake was felt in the island of Porto Rico on the morning of Aug. 26, at 6.15 A.M. The vibration lasted two minutes. No report of the extent of damage done has yet been received.

An eruption broke out in Mount Etna on Sunday evening last. The lava issued from the crater by three mouths, all of which, however, are happily some distance from human habitations.

The *Times of India* states that the report which M. Victor de Lesseps and Mr. C. Stuart will have to make on their return to Europe on the feasibility of the great Central Asian Railway scheme will be of a character to render it likely that preliminary funds will be subscribed to enable the first surveys to be effected with a view to definitely settle the route which it would be desirable to follow.

We have received from Mr. Stanford the Alpine Club Map of Switzerland, edited by Mr. R. C. Nichols, the preparation of which we noticed in vol. vi. p. 205. It is a very fine specimen of map making, and a credit to English cartography. We hope soon to notice it in detail.

If the observations recorded by Mr. F. M. Balfour at the recent meeting of the British Association, on the development of the notocord from the hypoblastic, instead of the mesoblastic layer of the embryo in the shark, are confirmed, they will shake to the foundation the importance of the elaborate arguments which have been, of late, so frequently based upon the origin of the different morphological elements of the living frame.

We are sure many of the recent visitors to Belfast must have found an invaluable aid in their wanderings about the town and district, which so abounds in varied interest, in the very excellent "Guide to Belfast and the Adjacent Counties" (Belfast, Ward and Co.), which has been brought out under the care of the members of the Belfast Naturalists' Field Club. Great prominence is of course given to the scientific aspects of the districts embraced in the Guide, but a fair portion is also devoted to the ordinary objects of interest, to trade, commerce, manufactures, &c. The Guide is well arranged under the various headings of Physical Geography, Geology, Botany, Zoology, Topography, &c., and is amply illustrated with forty-six roughly executed but very useful plates, mostly of objects of antiquarian interest. We heartily recommend the book to any visitor who wants an intelligent guide to the counties of Down and Antrim, a good map of which is appended.

The additions to the Zoological Society's Gardens during the past week include a Cassowary (*Casuarius ?*) from N.E. New Guinea, presented by Capt. Maisby; a Javan Chevrotain (*Tragulus javanicus*) from Java, presented by Mr. G. Mannings; a Formosan Deer (*Cervus pseudaxis*) from the Island of Formosa, presented by Mr. Abel A. J. Gower; two Black Swans (*Cygnus atratus*) from Australia, presented by Mr. R. H. Bower; an Indian Python (*Python molurus*); a Vervet Monkey (*Cercopithecus lalandii*) from South Africa, presented by Mr. C. Hassam; two Black-eared Marmosets (*Hapale penicillata*) from Brazil, presented by Mr. J. P. Harrison.

THE BRITISH ASSOCIATION

The Belfast Session of the British Association was brought to a conclusion on Wednesday, the 26th ult., with mutual congratulations between all concerned. In our animadversions on the high charges for sleeping accommodation charged from some of the members of the Association, we of course meant in no way to reflect on the local authorities or local committee, who exerted themselves to the utmost to render the meeting in every way a success. The vote of thanks to the Mayor was thoroughly deserved, as was also the tribute of praise awarded by the Rev. Dr. Henry to the "unflagging zeal" of Dr. Andrews in behalf of this meeting of the Association. One very pleasing result of the meeting, and of a discussion in the Economical Section, was the sudden termination of the extensive strike which had existed in Belfast for a considerable time. The various excursions organised on Thursday were a decided success.

The next meeting opens at Belfast on August 25, 1875.

The Committee, among other things, have recommended, and their recommendation has been adopted, that the Council of the Association be requested to take such steps as they may think expedient to urge upon the Government of India the desirableness of continuing solar observations; that the Council of the Association be requested to take such steps as they may think desirable with the view of appointing naturalists to vessels engaged on coasts of little-known parts of the world; that they be requested to take such steps as they may think desirable to promote any application that may be made to her Majesty's Government by the Royal Society to promote physiological and biological explorations in the seas round the British Isles; that they be requested to take such steps as they think desirable for supporting a request to her Majesty's Government to undertake an Arctic expedition on the basis proposed by the Council of the Royal Geographical Society at the beginning of the present year, and which will be made again by that body.

The following is a synopsis of grants of money appropriated to scientific purposes by the General Committee at the Belfast Meeting:—

MATHEMATICS AND PHYSICS.	
*Cayley, Prof.—Printing Mathematical Tables	£100
*Balfour Stewart, Prof.—Magnetisation of Iron	20
*Brooke, Mr.—British Rainfall	120
*Glaisher, Mr. J.—Luminous Meteors	30
Maxwell, Prof. C.—Testing the Exactness of Ohm's Law	50
Stokes, Prof.—Reflective Power of Silver and other Substances	20
*Herschel, Prof.—Thermal Conducting Powers of Rocks	10
*Tait, Prof.—Thermo-Electricity (renewed)	50
CHEMISTRY.	
*Williamson, Prof. A. W.—Records of the Progress of Chemistry	100
Roscoe, Prof.—Specific Volumes of Liquids	25
Allen, Mr.—Estimation of Potash and Phosphoric Acid	10
*Armstrong, Dr.—Isomeric Cresols and their Derivatives (renewed)	20
GEOLOGY.	
*Willett, Mr. H.—The Sub-Wealden Exploration	100
*Lyell, Sir C., Bart.—Kent's Cavern Exploration	100
*Lubbock, Sir J.—Exploration of Victoria Cave, Settle	50
*Bryce, Dr.—Earthquakes in Scotland (renewed)	20
Hull, Prof.—Underground Waters in New Red Sandstone and Permian	10
BIOLOGY.	
Dresser, Mr.—Report on Ornithology	10
Rolleston, Prof.—Development of Myxinoid Fishes	20
*Stainton, Mr.—Record of the Progress of Zoology	100
*Fox, Col. Lane.—Forms of Instruction for Travellers	20
*Brunton, Dr.—The Nature of Intestinal Secretion	20
GEOGRAPHY.	
Wilson, Major.—Palestine Exploration Fund	100
STATISTICS AND ECONOMIC SCIENCE	
*Houghton, Lord.—Economic Effects of Trades' Unions	25
MECHANICS.	
Froude, Mr.—Instruments for Measuring the Speed of Ships (renewed)	50
Total	£1,180

Reappointed.

ON THE HYPOTHESIS THAT ANIMALS ARE AUTOMATA, AND ITS HISTORY*

AT this period of the meeting of the British Association I am quite sure it is hardly necessary for me to call to your minds the nature of the business which takes place at our sectional meetings. We there register the progress which science has made during the past year, and we do our best to advance that progress by original communications and free discussion. But when the honourable task of delivering this evening's lecture was imposed upon me, or rather as my friend the President has just said, when I undertook to deliver it, it occurred to me that the occasion of an evening lecture might be turned to a different purpose, that we might with much propriety and advantage turn our minds back to the past to consider what had been done by the great men of old, who "had gone down into the grave with their weapons of war," but who had fought bravely for the cause of truth while they yet lived—to recognise their merits, and to show ourselves duly grateful for their services. I propose, therefore, to take a retrospect of the condition of that branch of science with which it is my business to be more or less familiar—not to a very remote period, for I shall go no further back than the seventeenth century, and the observations which I shall have to offer you will be confined almost entirely to the biological science of the time between the middle of the seventeenth and the middle of the eighteenth centuries. I propose to show what great ideas in biological science took their origin at that time, in what manner the speculations then originated have been developed, and in what relation they stand to what is now understood to be the body of scientific biological truth. The middle of the sixteenth century, or rather the early part of it, is one of the great epochs of biological science. It was at that time that an idea, which had been dimly advocated previously, took the solid form which can only be given to scientific ideas by the definite observation of fact—I mean the idea that vital phenomena, like all other phenomena of the physical world, are capable of mechanical explanation, that they are reducible to law and order, and that the study of biology, in the long run, is an application of the great sciences of physics and chemistry. The man to whom we are indebted for first bringing that idea into a plain and tangible shape, I am proud to say, was an Englishman, William Harvey. Harvey was the first clearly to explain the mechanism of the circulation of the blood, and by that remarkable discovery of his, and by the clearness and precision with which he reduced that process to its mechanical elements, he laid the foundation of a scientific theory of the larger part of the processes of living beings—those processes, in fact, which we now call processes of sustentation—and by his studies of development he, further, first laid the foundation of a scientific knowledge of reproduction. But besides these great powers of living beings, there remains another class of functions—those of the nervous system—with which Harvey did not grapple. It was, indeed, left for a contemporary of his, a man who, as he himself tells us, was mainly stimulated in these inquiries by the brilliant researches of Harvey—René Descartes—to play a part in relation to the phenomena of the nervous system, which, in my judgment, is equal in value to that which Harvey played in regard to the circulation. And when we consider who Descartes was, how brief the span of his life, I think it is a truly wonderful circumstance that this man, who died at fifty-four, should be one of the recognised leaders of philosophy—that, as I am informed by competent authority, he was one of the first and most original mathematicians who has ever lived, and that, at the same time, the fertility of his intellect and the grasp of his genius should have been so great that he could take rank, as I believe he must, beside the immortal Harvey as a physiologist. And you must recollect that Descartes was not merely, as some had been, a happy speculator. He was a working anatomist and physiologist, conversant with all the anatomical and physiological lore of his time, and practised in all methods by which anatomical and physiological discoveries were then made; and it is related of him—and a most characteristic anecdote it is, and one which should ever put to silence those shallow talkers who speak of Descartes as a merely hypothetical and speculative philosopher—that a friend once calling upon him in Holland begged to be shown his library. Descartes led him into a sort of shed, and, drawing aside a curtain, displayed a dissecting-room full of bodies of animals in course of dissection, and said, "There is my library." It would take us a very long time if I were to attempt to pursue the method which would be requisite for the full establishment of all that I am about to say; that is to say, if I were to quote the several passages of Descartess works which bear out my ascription to him of the several propositions which I am going to bring before you. And I must beg you, therefore, to be so good as to take it on my authority for the present, although for the present only, that there are to be found clearly expressed in Descartes' works the propositions which I shall proceed to lay before you, and each of which I shall compare as we go on, as briefly as may be, with the existing state of physiological science, in order that you may see in what position with respect to physiology—ay, even to the advanced physiology of the present time—this man stood. And, happily, the matters with which we shall treat are such as to require no extensive knowledge of anatomy—no more, in fact, than such as, I presume, must be familiar to almost every person.

I think I need only premise that what we call the nervous system in one of the higher animals consists of a central apparatus, composed of the brain, which is lodged in the skull, and of a cord proceeding from it, which is termed the spinal marrow, and which is lodged in the vertebral column or spine, and that from these soft white masses—for such they are—there proceed cords which are termed nerves, some of which nerves end in the muscles, while others end in the organs of sensation. That bare and bald statement of the fundamental composition of the nervous system will be enough for our present purpose.

The first proposition culled from the works of Descartes which I have to lay before you, is one which will sound very familiar. It is the view, which he was the first, so far as I know, to state, not only definitely, but upon sufficient grounds, that the brain is the organ of sensation, of thought, and of emotion—using the word "organ" in this sense, that certain changes which take place in the matter of the brain are the essential antecedents of those states of consciousness which we term sensation, thought, and emotion. Nowadays that is part of popular and familiar knowledge. If your friend disagrees with your opinion, runs amuck against any of your pet prejudices, you say, "Ah! poor fellow, he is a little touched here;" by which you mean that his brain is not doing its business properly, and, therefore, that he is not thinking properly. But in Descartes' time, and I may say for 150 years afterwards, the best physiologists had not reached that point. It remained down to the time of Bichat a question whether the passions were or were not located in the abdominal viscera. This, therefore, was a very great step. It is a statement which Descartes makes from the beginning, and from which he never swerves. In the second place, Descartes lays down the proposition that all the movements of animal bodies are effected by the change of form of a certain part of the matter of their bodies, to which he applies the general term of muscle. You must be aware of this in reading Descartes; you must use the terms in the sense in which he used them, or you will not understand him. That is a proposition which is now placed beyond all doubt whatever. If I move my arm, that movement is due to the change of this mass of flesh in front called the biceps muscle: it is shortened and it becomes thicker. If I move any of my limbs the reason is the same. As I now speak to you, the different tones of my voice are due to the exquisitely accurate adjustment of the contractions of a multitude of such portions of flesh; and there is no considerable and visible movement of the animal body which is not, as Descartes says, resolvable into these changes in the form of matter termed muscle. But Descartes went further, and he stated that in the normal and ordinary condition of things, these changes in the form of muscle in the living body only occur under certain conditions; and the essential condition of the change is, says Descartes, the motion of the matter contained within the nerves, which go from the central apparatus to the muscle. Descartes gave this moving material a particular name—the animal spirits. Nowadays we should not talk of the existence of animal spirits, but we should say that a molecular change takes place in the nerve, and that that molecular change is propagated with a certain velocity, from the central apparatus to the muscle. Nevertheless, the modification of the idea is not greater than that which has taken place in our view of electricity, in our change of conception of it as a fluid to our conception of it as a condition of propagated molecular change. Modern physiology has measured the rate of the change to which I have referred; it has thrown marvellous light upon its nature; it has increased our knowledge of its characters, but the fundamental conception

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* Address by Prof. Huxley, F.R.S., at the British Association, Belfast, Aug. [illegible]

remains exactly what it was in the time of Descartes. Next, Descartes says that, under ordinary circumstances, this change in the contents of a nerve, which gives rise to the contraction of a muscle, is produced by a change in the central nervous apparatus, as, for example, in the brain. We say at the present time exactly the same thing. Descartes said that the animal spirits were stored up in the brain, and flowed out along the motor nerves. We say that a molecular change takes place in the brain that is propagated along the motor nerve. The evidence of that is abundantly supplied by experimental research. Further, Descartes stated that the sensory organs, or those apparatuses which give rise to our feelings when acted upon by the influences which produce sensation, caused a change in the sensory nerves, which he described as a flow of animal spirits along those nerves, which flow was propagated to the brain. If I look at this candle which I hold before me, the light falling on the retina of my eye gives rise to an affection of the optic nerve, which affection Descartes described as a flow of the animal spirits to the brain. We should now speak of it as a molecular change propagated along the optic nerve to the brain; but the fundamental idea is the same. In all our notions of the operations of nerve we are building upon Descartes' foundation. Not only so, but Descartes lays down over and over again, in the most distinct manner, a proposition which is of paramount importance not only for physiology but for psychology. He says that when a body which is competent to produce a sensation touches the sensory organs, what happens is the production of a mode of motion of the sensory nerves. That mode of motion is propagated to the brain. That which takes place in the brain is still nothing but a mode of motion. But, in addition to this mode of motion, there is, as everybody can find by experiment for himself, something else which can in no way be compared to motion, which is utterly unlike it, and which is that state of consciousness which we call a sensation. Descartes insists over and over again upon this total disparity between the agent which excites the state of consciousness and the state of consciousness itself. He tells us that our sensations are not pictures of external things, but that they are symbols or signs of them; and in doing that he made one of the greatest possible revolutions, not only in physiology but in philosophy. Till his time it was conceived that visible bodies, for example, gave from themselves a kind of film which entered the eye and so went to the brain, *species intentionales* as they were called, and thus the mind received an actual copy or picture of things which were given off from it. It is to Descartes we owe that complete revolution in our ideas, which has led us to see that we have really no knowledge whatever of the causes of those phenomena which we term external things, and that the only certainty we possess is that they cannot be like those phenomena. In laying down that proposition upon what I imagine to be a perfectly irrefragable basis, Descartes laid the foundation of that form of philosophy which is termed idealism, which was subsequently expanded to its uttermost by Berkeley, and has since taken very various shapes.

But Descartes noticed not only that under certain conditions an impulse made by the sensory organ may give rise to a sensation, but that under certain other conditions it may give rise to motion, and that this motion may be effected without sensation, and not only without volition, but even contrary to it. I trouble you with as little reading as I can, because it occupies so much time; but I must ask your patience for one very remarkable passage which is contained in the answer that Descartes gave to the objections raised by the famous Port Royalist Arnauld to his Fourth Meditation. Descartes says: "It appears to me to be a very remarkable circumstance that no movement can take place either in the bodies of beasts or even in our own, if these bodies have not in themselves all the organs and instruments by means of which the very same movement would be accomplished in a machine, so that, even in us, the spirit or the soul does not directly move the limb, but only determines the course of that very subtle liquid which is called the animal spirits, which, running continually from the heart by the brain into the muscles, is the cause of all the movements of our limbs, and often may cause many different motions, one as easily as the other. And it does not even always exert this determination, for, among the movements which take place in us, there are many which do not depend upon the mind at all, such as the beating of the heart, the digestion of food, the nutrition, the respiration of those who sleep, and, even in those who are awake, walking, singing, and other similar actions when they are performed without the mind thinking about them. And when one who falls from a height throws his hands forward to save his head, it is in virtue of no ratiocination that he performs this action; it does not depend upon his mind, but takes place merely because his senses, being affected by the present danger, cause some change in his brain, which determines the animal spirits to pass thence into the nerves in such a manner as is required to produce this motion, in the same way as in a machine, and without the mind being able to hinder it." I know in no modern treatise of a more clear and precise statement, of a more perfect illustration than this of what we understand by the automatic action of the brain. And what is very remarkable, in speaking of these movements which arise by a sensation being as it were reflected from the central apparatus into a limb—as, for example, when one's finger is pricked and the arm is suddenly drawn up, the motion of the sensory nerve travels to the spine and is again reflected down to the muscles of the arm—Descartes uses the very phrase that we at this present time employ; he speaks of the "*esprits réfléchis*," the reflected spirits; and that this was no mere happy phrase lost upon his contemporaries will be obvious if you consult the famous work of Willis, the Oxford professor, "De Anima Brutorum," which was published about 1672. In giving an account of Descartes' views he borrows this very phrase from him, and speaks of this reflection of the motion of a sensory nerve into the motion of a motor nerve, "sicut undulatione reflexa," as if it were a wave thrown back; so that we have not only the thing reflex action described, but we have the phrase "reflex" recognised in its full significance.

And the last great service to the physiology of the nervous system which I have to mention as rendered by Descartes was this, that he first, so far as I know, sketched out a physical theory of memory. What he tells you in substance is this, that when a sensation takes place, the animal spirits travel up the sensory nerve, pass to the appropriate part of the brain, and there, as it were, find their way through the pores of the substance of the brain. And he says that when this has once taken place, when the particles of the brain have themselves been, as it were, shoved aside a little by a single passage of the animal spirits, the passage is made easier in the same direction for any subsequent flow of animal spirits; and that the repetition of this action makes it easier still, until, at length, it becomes very easy for the animal spirits to move these particular particles of the brain, the motion of which gives rise to the appropriate sensation; and, finally, the passage is so easy that almost any impulse which stirs the animal spirits causes them to flow into these already open pores more easily than they would flow in any other direction; and the flow of the animal spirits recalls the image, the state of consciousness called into existence by a former sensory impression. This view is essentially at one with all our present physical theories of memory. That memory is dependent upon a physical process stands beyond question. The results of the study of disease, the results of the action of poisonous substances, all conclusively point to the fact that memory is inseparably connected with the integrity of certain material parts of the brain and dependent upon them, and I know of no hypothesis by which this fact can be accounted for except by one which is essentially similar to the notion of Descartes, a notion that the impression once made makes subsequent impressions easier and therefore allows almost any indirect disturbance of the brain to call up this particular image.

So far, the ideas started by Descartes have simply been expanded, enlarged, and defined by modern research; they are the keystones of the modern physiology of the nervous system. But in one respect Descartes proceeded further than any of his contemporaries, and has been followed by very few of his successors in later days, although his views were for the best part of a century largely dominant over the intellectual mind of Europe. Descartes reasoned thus: "I can account for many of the actions of living beings mechanically, since reflex actions take place without the intervention of consciousness, and even in opposition to the will." As, for example, when a man in falling mechanically puts out his hand to save himself, or when a person, to use another of Descartes' illustrations, strikes at his friend's eye, and although the friend knows he does not mean to hit him, he nevertheless cannot prevent the muscles of his eye from winking. "In these cases," Descartes said, "I have clear evidence that the nervous system acts mechanically without the intervention of consciousness and without the intervention of the will, or, it may be, in opposition to it. Why, then, may I not extend this idea further? As actions of a certain amount of complexity are brought about in this way, why may not actions of still greater complexity be so produced? Why, in fact, may it not be that the whole of man's physical actions are mechanical, his mind living apart, as it were, and only occasionally interfering by means of volition?" And it so happened that Descartes was led by some of his speculations to believe that beasts had no souls, and consequently could have no consciousness; and thus, his two ideas harmonising together, he developed that famous hypothesis of the automatism of brutes, which is the main subject of my present discourse. What Descartes meant by this was that animals are absolute machines, as if they were mills or barrel organs; that they have no feelings; that a dog does not see, and does not hear, and does not smell, but that the impressions which would produce those states of consciousness in ourselves, give rise in the dog, by a mechanical reflex process, to actions which correspond to those which we perform when we do smell, and do taste, and do see. On the face of it this appears to be a most surprising hypothesis, and I do not wonder that it proved to be a stumbling-block even to such acute and subtle men as Henry More, who was one of Descartes' correspondents; and yet it is a very singular thing that this, the boldest and most paradoxical notion which Descartes broached, has received as much and as strong support from modern physiological research as any other of his hypotheses. I will endeavour to explain to you in as few words as possible what is the nature of that support, and why it is that Descartes' hypothesis, although I am bound to say I do not agree with it, nevertheless, remains at this present time not only quite as defensible as it was in his own time, but I should say, upon the whole, a little more defensible.

If it should happen to a man that by accident his spinal cord is divided, he would become paralysed below the point of injury. In such case his limbs would be absolutely paralysed; he would have no control over them, and they would be devoid of sensation. You might prick his feet, or burn them, or do anything else you like with them, and they would be absolutely insensible. Consciousness, therefore, so far as we can have any knowledge of it, would be entirely abolished in that part of the central nervous apparatus which lies below the injury. But although the man under these circumstances is paralysed in the sense of not being able to move his own limbs, he is not paralysed in the sense of their being deprived of motion, for if you tickle the soles of his feet with a feather the limbs will be drawn up just as vigorously, perhaps a little more vigorously, than when he was in full possession of the consciousness of what happened to him. Now, that is a reflex action. The impression is transmitted from the skin to the spinal cord, it is reflected from the spinal cord, and passes down into the muscles of the limbs, and they are dragged up in this manner—dragged away from the sources of irritation, though the action, you will observe, is a purely automatic or mechanical action. Suppose we deal with a frog in the same way, and cut across the spinal cord. The frog falls into precisely the same condition. So far as the frog is concerned, his limbs are useless; but you have merely to apply the slightest irritation to the skin of the foot, and the limb is instantly drawn away. Now, if we have any ground for argument at all, we have a right to assume that, under these circumstances, the lower half of the frog's body is as devoid of consciousness as is the lower half of the man's body; and that the body of the frog below the injury is in this case absolutely devoid of consciousness, is a mere machine like a musical box or a barrel-organ, or a watch. You will remark, moreover, that the movement of the limbs is purposive—that is to say, that when you irritate the skin of the foot, the foot is drawn away from the danger, just as it would be if the frog were conscious and rational, and could act in accordance with rational consciousness. But you may say it is easy enough to understand how so simple an action might take place mechanically.

Let us consider another experiment. Take this creature, which certainly cannot feel, and touch the skin of the side of the body with a little acetic acid, a little vinegar, which in a frog that could feel would give rise to great pain. In this case there can be no pain, because the application is made below the point of section; nevertheless, the frog lifts up the limb of the same side, and applies the foot to rubbing off the acetic acid; and, what is still more remarkable, if you hold down the limb so that the frog cannot use it, he will, by and by, take the limb of the other side and turn it across the body, and use it for the same rubbing process. It is impossible that the frog, if it were in its entirety and were reasoning, could perform actions more purposive than these, and yet we have most complete assurance that in this case the frog is not acting from purpose, has no consciousness, is a mere automatic machine. But now suppose that instead of making your section of the cord in the middle of the body, you had made it in such a manner as to divide the hindermost part of the brain from the foremost part of the brain, and suppose the foremost two-thirds of the brain entirely taken away, the frog is then absolutely devoid of any spontaneity; it will remain for ever where you leave it; it will not stir unless it is touched; it sits upright in the condition in which a frog habitually does sit; but it differs from the frog which I have just described in this, that if you throw it into the water it begins to swim—swims just as well as the perfect frog does. Now, swimming, you know, requires the combination, and indeed the very careful and delicate combination, of a great number of muscular actions, and the only way we can account for this is, that the impression made upon the sensory nerves of the skin of the frog by the contact of the water, conveys to the central nervous apparatus a stimulus which sets going a certain machinery by which all the muscles of swimming are brought into play in due order and succession. Moreover, if the frog be stimulated, be touched by some irritating body, although we are quite certain it cannot feel, it jumps or walks as well as the complete frog can do. But it cannot do more than this.

Suppose yet one other experiment. Suppose that all that is taken away of the brain is what we call the cerebral hemispheres, the most anterior part of the brain. If that operation is properly performed, the frog may be kept in a state of full bodily vigour for months, or it may be for years; but it will sit for ever in the same spot. It sees nothing; it hears nothing. It will starve sooner than feed itself, although if food is put into its mouth it swallows it. On irritation it jumps or walks; if thrown into the water it swims. But the most remarkable thing that it does is this—you put it in the flat of your hand; it sits there, crouched, perfectly quiet, and would sit there for ever. Then if you incline your hand, doing it very gently and slowly, so that the frog would naturally tend to slip off, you feel the creature's fore-paws getting a little slowly on to the edge of your hand until he can just hold himself there, so that he does not fall; then, if you turn your hand, he mounts up with great care and deliberation, putting one leg in front and then another, until he balances himself with perfect precision upon the edge of your hand; then if you turn your hand over, he goes through the opposite set of operations until he comes to sit in perfect security upon the back of your hand. The doing of all this requires a delicacy of co-ordination, and an adjustment of the muscular apparatus of the body which is only comparable to that of a rope-dancer among ourselves; though in truth a frog is an animal very poorly constructed for rope-dancing, and on the whole we may give him rather more credit than we should to a human dancer. These movements are performed with the utmost steadiness and precision, and you may vary the position of your hand, and the frog, so long as you are reasonably slow in your movements, will work backwards and forwards like a clock. And what is still more wonderful is, that if you put the frog on a table, and put a book between him and the light, and give him a little jog behind, he will jump—take a long jump, very possibly—but he won't jump against the book; he will jump to the right or to the left, but he will get out of the way, showing that although he is absolutely insensible to ordinary impressions of light, there is still a something which passes through the sensory nerve, acts upon the machinery of his nervous system, and causes it to adapt itself to the proper action.

Can we go further than this? I need not say that since those days of commencing anatomical science when criminals were handed over to the doctors, we cannot make experiments on human beings, but sometimes they are made for us, and made in a very remarkable manner. That operation called war is a great series of physiological experiments, and sometimes it happens that these physiological experiments bear very remarkable fruit. I am indebted to my friend General Strachey for bringing to my notice an account of a case which appeared within the last four or five days in the scientific article of the *Journal des Débats*. A French soldier, a sergeant, was wounded at the battle of Bazeilles, one, as you recollect, of the most fiercely contested battles of the late war. The man was shot in the head, in the region of what we call the left parietal bone. The bullet fractured the bone. The sergeant had enough vigour left to send his bayonet through the Prussian who shot him. Then he wandered a few hundred yards out of the village, fell senseless, but, after the action, was picked up and taken to the hospital, where he remained some time. When he came to himself, as usual in such cases of injury, he was paralysed on the opposite side of the body, that is to say, the right arm and the right leg were completely paralysed. That state of things lasted, I think, the better part of two years, but sooner or later he recovered from it, and now he is able to walk about with activity, and only by careful measurement can any difference between the two sides and his body be ascertained. The inquiry, the main results of which I shall give you, has been conducted by exceedingly competent persons, and they report that at present this man lives two lives, a normal life and an abnormal life. In his normal life he is perfectly well, cheerful, does his work as a hospital attendant, and is a respectable, well-conducted man. This normal life lasts for about seven-and-twenty days, or thereabouts, out of every month; but for a day or two in each month he passes suddenly and without any obvious change into his abnormal condition. In this state of abnormal life he is still active, goes about as usual, and is to all appearance just the same man as before, goes to bed and undresses himself, gets up, makes his cigarette and smokes it, and eats and drinks. But he neither sees, nor hears, nor tastes, nor smells, nor is he conscious of anything whatever, and he has only one sense organ in a state of activity, namely, that of touch, which is exceedingly delicate. If you put an obstacle in his way, he knocks against it, feels it and goes to the one side; if you push him in any direction, he goes straight on until something stops him. I have said that he makes his cigarettes, but you may supply him with shavings or of anything else instead of tobacco, and still he will go on making his cigarettes as usual. His actions are purely mechanical. He feeds voraciously, but whether you give him aloes or assafœtida, or the nicest thing possible, it is all the same to him. The man is in a condition absolutely parallel to that of the frog I have just described, and no doubt when he is in this condition the functions of his cerebral hemispheres are, at any rate, largely annihilated. He is very nearly—I don't say wholly, but very nearly—in the condition of an animal in which the cerebral hemispheres are extirpated. And his state is wonderfully interesting to me, for it bears on the phenomena of mesmerism, of which I saw a good deal when I was a young man. In this state he is capable of performing all sorts of actions on mere suggestion. For example, he dropped his cane, and a person near him putting it into his hand, the feeling of the end of the cane evidently produced in him those molecular changes of the brain which, had he possessed consciousness, would have given rise to the idea of his rifle; for he threw himself on his face, began feeling for his cartridges, went through the motions of touching his gun, and shouted out to an imaginary comrade, "Here they are, a score of them; but we will give a good account of them." But the most remarkable fact of all is the modification which this injury has made in the man's moral nature. In his normal life he is an upright and honest man. In his abnormal state he is an inveterate thief. He will steal everything he can lay his hands upon, and if he cannot steal anything else, he will steal his own things and hide them away.

Now, if Descartes had had this fact before him, need I tell you that his theory of animal automatism would have been enormously strengthened? He would have said: "Here is a case of a man performing actions more complicated, and to all appearance more dependent on reason, than any of the ordinary operations of animals, and yet you have positive proof that these actions are purely mechanical. What, then, have you to urge against my doctrine that all animals are mere machines?" In the words of Malebranche, who adopted Descartes' view, "In dogs, cats, and other animals, there is neither intelligence nor spiritual soul as we understand the matter commonly; they eat without pleasure, they cry out without pain, they grow without knowing it, they desire nothing, they know nothing, and if they act with dexterity and in a manner which indicates intelligence, it is because God having made them with the intention of preserving them, He has constructed their bodies in such a manner that they escape organically, without knowing it, everything which could injure them and which they seem to fear." Descartes put forward this hypothesis, and I do not know that it can be positively refuted. We can have no direct observation of consciousness in any creature but ourselves. But I must say for myself—looking at the matter on the ground of analogy—taking into account that great doctrine of continuity which forbids one to suppose that any natural phenomena can come into existence suddenly and without some precedent, gradual modification tending towards it, and taking into account the incontrovertible fact that the lower vertebrated animals possess, in a less developed condition, that part of the brain which we have every reason to believe is the organ of consciousness in ourselves, it seems vastly more probable that the lower animals, although they may not possess that sort of consciousness which we have ourselves, yet have it in a form proportional to the comparative development of the organ of that consciousness, and foreshadow more or less dimly those feelings which we possess ourselves. I think that is the most rational conclusion that can be come to. It has this advantage, though this is a consideration which could not be urged in dealing with questions that are susceptible of demonstration, but which is well worthy of consideration in a case like the present, that it relieves us of the very terrible consequences of making any mistake on this subject. I must confess that, looking at the terrible struggle for existence which is everywhere going on in the animal world, and considering the frightful quantity of pain with which that process must be accompanied, if animals are sensitive, I should be glad if the probabilities were in favour of the view of Descartes. But, on the other hand, considering that if we were to regard animals as mere machines, we might indulge in unnecessary cruelties and in careless treatment of them, I must confess I think it much better to err on the right side, and not to concur with Descartes on this point.

But let me point out to you that although we may come to the conclusion that Descartes was wrong in supposing that animals are insensible machines, it does not in the slightest degree follow that they are not sensitive and conscious automata; in fact, that is the view which is more or less clearly in the minds of every one of us. When we talk of the lower animals being provided with instinct, and not with reason, what we really mean is, that although they are sensitive and although they are conscious, yet they act mechanically, and that their different states of consciousness, their sensations, their thoughts (if they have any), their volitions (if they have any), are the products and consequences of their mechanical arrangements. I must confess that this popular view is to my mind the only one which can be scientifically adopted. We are bound by everything we know of the operations of the nervous system to believe that when a certain molecular change is brought about in the central part of the nervous system, that change, in some way utterly unknown to us, causes that state of consciousness that we term a sensation. It is not to be doubted that those motions which give rise to sensation leave in the brain changes of its substance which answer to what Haller called "*vestigia rerum*," and to what that great thinker, David Hartley, termed "Vibratiuncles." The sensation which has passed away leaves behind molecules of the brain competent to its reproduction—"sensigenous molecules," so to speak—which constitute the physical foundation of memory. Other molecular changes give rise to conditions of pleasure and pain, and to the emotion which in ourselves we call volition. I have no doubt that is the relation between the physical processes of the animal and his mental processes. In this case it follows inevitably that these states of consciousness can have no sort of relation of causation to the motions of the muscles of the body. The volitions of animals will be simply states of emotion which precede their actions. To make clear what I mean, suppose I had a frog placed in my hand, and that I could make it, by turning my hand, perform this balancing movement. If the frog were a philosopher, he might reason thus:—"I feel myself uncomfortable and slipping, and, feeling myself uncomfortable, I put my legs out to save myself. Knowing that I shall tumble if I do not put them further, I put them further still, and my volition brings about all these beautiful adjustments which result in my sitting safely." But if the frog so reasoned, he would be entirely mistaken; for the frog does the thing just as well when he has no reason, no sensation, no possibility of thought of any kind. The only conclusion, then, at which there seems any good ground for arriving is that animals are machines, but that they are conscious machines.

I might with propriety consider what I have now said as the conclusion of the observations which I have to offer concerning animal automatism. So far as I know, the problem which we have hitherto been discussing is an entirely open one. I do not know that there is any reason why any person, whatever his opinions may be, should be prevented, if he be so inclined, from accepting the doctrine which I have just now put before you. So far as we know, animals are conscious automata. That doctrine is perfectly consistent with any view that we may choose to take on the very curious speculation—Whether animals possess souls or not, and if they possess souls, whether those souls are immortal or not. The doctrine to which I have referred is not inconsistent with the perfectly strict and literal adherence to the Scripture text concerning "the beast that perisheth," nor, on the other hand, does it prevent anyone from entertaining the amiable convictions ascribed by Pope to his untutored savage, that when he passed to the realms of the blessed "his faithful dog should bear him company." In fact, all these accessory questions to which I have referred involve problems which cannot be discussed by physical science, inasmuch as they do not lie within the scope of physical science, but come into the province of that great mother of all science, Philosophy. Before any direct answer can be given upon any of these questions we must hear what Philosophy has to say for or against the views that may be held. I need hardly say—especially having detained you so long as I find I have done—that I do not propose to enter into that region of discussion, and I might, properly enough, finish what I have to say upon the subject—especially as I have reached its natural limits—if it were not that an experience, now, I am sorry to say, extending over a good many years, leads me to anticipate that what I have brought before you to-night is not likely to escape the fate which, upon many occasions within my recollection, has attended statements of scientific doctrine and of the conclusions towards which science is tending, which have been made in a spirit intended at any rate to be as calm and as judicial as that in which I have now laid these facts before you. I do not doubt that the fate which has befallen better men will befall me, and that I shall have to bear in patience the reiterated assertion that doctrines such as I have put before you have very evil tendencies. I should not wonder if you were to be told by persons speaking with authority—not, perhaps, with that authority which is based upon knowledge and wisdom, but still with authority—that my intention in bringing this subject before you is to lead you to apply the doctrine I have stated, to man as well as brutes, and it will then certainly be further asserted that the logical tendency of such a doctrine is Fatalism, Materialism, and Atheism. Now, let me ask you to listen to another product of that long experience to which I referred. Logical consequences are very important; but in the course of my experience I have found that they are the scarecrows of fools and the beacons of wise men. Logical consequences can take care of themselves. The only question for any man to ask is—"Is this doctrine true, or is it false?" No other question can possibly be taken into consideration until that one is settled. And, as I have said, the logical consequences of doctrines can only serve as a warning to wise men to ponder well whether the doctrine submitted for their consideration be true or not, and to test it in every possible direction. Undoubtedly I do hold that the view I have taken of the relations between the physical and mental faculties of brutes applies in its fulness and entirety to man; and if it were true that the logical consequences of that belief must land me in all these terrible consequences, I should not hesitate in allowing myself to be so landed. I should conceive that if I refused I should have done the greatest and most abominable violence to everything which is deepest in my moral nature. But now I beg leave to say that, in my conviction, there is no such logical connection as is pretended between the doctrine I accept and the consequences which people profess to draw from it. Some years ago I had occasion, in dealing with the philosophy of Descartes, and some other matters, to state my conviction pretty fully on those subjects, and, although I know from experience how futile it is to endeavour to escape from those nicknames which many people mistake for argument, yet, if those who care to investigate these questions in a spirit of candour and justice will look into those writings of mine, they will see my reasons for not imagining that such conclusions can be drawn from such premises. To those who do not look into these matters with candour and with a desire to know the truth, I have nothing whatever to say, except to warn them on their own behalf what they do; for assuredly if, for preaching such doctrine as I have preached to you to-night, I am cited before the bar of public opinion, I shall not stand there alone. On my one hand I shall have, among theologians, St. Augustine, John Calvin, and a man whose name should be well known to the Presbyterians of Ulster—Jonathan Edwards—unless, indeed, it be the fashion to neglect the study of the great masters of divinity, as many other great studies are neglected nowadays; and I should have upon my other hand, among philosophers, Leibnitz; I should have Pere Malebranche, who saw all things in God; I should have David Hartley, the theologian as well as philosopher; I should have Charles Bonnet, the eminent naturalist, and one of the most zealous defenders Christianity has ever had. I think I should have, within easy reach, at any rate, John Locke. Certainly the school of Descartes would be there, if not their master; and I am inclined to think that, in due justice, a citation would have to be served upon Immanuel Kant himself. In such society it may be better to be a prisoner than a judge; but I would ask those who are likely to be influenced by the din and clamour which are raised about these questions, whether they are more likely to be right in assuming that those great men I have mentioned—the fathers of the Church and the fathers of Philosophy—knew what they were about; or that the pigmies who raise the din know better than they did what they meant. It is not necessary for any man to occupy himself with problems of this kind unless he so choose. Life is full enough, filled to the brim, by the performance of its ordinary duties; but let me warn you, let me beg you to believe, that if a man elect to give a judgment upon these great questions; still more, if he assume to himself the responsibility of attaching praise or blame to his fellow-men for the judgments which they may venture to express—then, unless he would commit a sin more grievous than most of the breaches of the Decalogue, he must avoid a lazy reliance upon the information that is gathered by prejudice and filtered through passion. Let him go to those great sources that are open to him as to every one, and to no man more open than to an Englishman; let him go back to the facts of nature, and to the thoughts of those wise men who for generations past have been the interpreters of nature.

THE CARNIVOROUS HABITS OF PLANTS*

I HAVE chosen for the subject of my address to you from the chair in which the Council of the British Association has done me the honour of placing me, the carnivorous habits of some of our brother-organisms—Plants.

Various observers have described with more or less accuracy the habits of such vegetable sportsmen as the Sundew, the Venus's Fly-trap, and the Pitcher-plants, but few have inquired into their motives; and the views of those who have most accurately appreciated these have not met with that general acceptance which they deserved.

Quite recently the subject has acquired a new interest, from the researches of Mr. Darwin into the phenomena which accompany the placing albuminous substances on the leaves of Drosera and Pinguicula, and which, in the opinion of a very eminent physiologist, prove, in the case of Dionæa, that this plant digests exactly the same substances and in exactly the same way that the human stomach does. With these researches Mr. Darwin is still actively engaged, and it has been with the view of rendering him such aid as my position and opportunities at Kew afforded me, that I have, under his instructions, examined some other carnivorous plants.

In the course of my inquiries I have been led to look into the early history of the whole subject, which I find to be so little known and so interesting that I have thought that a sketch of it, up to the date of Mr. Darwin's investigations, might prove acceptable to the members of this Association. In drawing it up, I have been obliged to limit myself to the most important plants; and with regard to such of these as Mr. Darwin has studied, I leave it to him to announce the discoveries which, with his usual frankness, he has communicated to me and to other friends; whilst with regard to those which I have myself studied, Sarracenia and Nepenthes, I shall briefly detail such of my observations and experiments as seem to be the most suggestive.

Dionæa.—About 1768 Ellis, a well-known English naturalist, sent to Linnæus a drawing of a plant, to which he gave the poetical name of Dionæa. "In the year 1765," he writes, "our late worthy friend, Mr. Peter Collinson, sent me a dried specimen of this curious plant, which he had received from Mr. John Bartram, of Philadelphia, botanist to the late King." Ellis flowered the plant in his chambers, having obtained living specimens from America. I will read the account which he gave of it to Linnæus, and which moved the great naturalist to declare that, though he had seen and examined no small number of plants, he had never met with so wonderful a phenomenon:—

"The plant, Ellis says, shows that Nature may have some views towards its nourishment, in forming the upper joint of its leaf like a machine to catch food; upon the middle of this lies the bait for the unhappy insect that becomes its prey. Many minute red glands that cover its surface, and which perhaps discharge sweet liquor, tempts the animal to taste them; and the instant these tender parts are irritated by its feet, the two lobes rise up, grasp it fast, lock the rows of spines together, and squeeze it to death. And further, lest the strong efforts for life in the creature just taken should serve to disengage it, three

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* Address in the Department of Zoology and Botany, British Association, Belfast, August 21, by Dr. Hooker, C.B., D.C.L., Pres. R.S.

small erect spines are fixed near the middle of each lobe, among the glands, that effectually put an end to all its struggles. Nor do the lobes ever open again, while the dead animal continues there. But it is nevertheless certain that the plant cannot distinguish an animal from a vegetable or mineral substance; for if we introduce a straw or pin between the lobes, it will grasp it fully as fast as if it was an insect."

This account, which in its way is scarcely less horrible than the descriptions of those mediæval statues which opened to embrace and stab their victims, is substantially correct, but erroneous in some particulars. I prefer to trace out our knowledge of the facts in historical order, because it is extremely important to realise in so doing how much our appreciation of tolerably simple matters may be influenced by the prepossessions that occupy our mind.

We have a striking illustration of this in the statement published by Linnæus a few years afterwards. All the facts which I have detailed to you were in his possession; yet he was evidently unable to bring himself to believe that Nature intended the plant—to use Ellis's words—"to receive some nourishment from the animals it seizes;" and he accordingly declared, that as soon as the insects ceased to struggle, the leaf opened and let them go. He only saw in these wonderful actions an extreme case of sensitiveness in the leaves, which caused them to fold up when irritated, just as the sensitive plant does; and he consequently regarded the capture of the disturbing insect as something merely accidental and of no importance to the plant. He was, however, too sagacious to accept Ellis's sensational account of the *coup de grace* which the insects received from the three stiff hairs in the centre of each lobe of the leaf.

Linnæus's authority overbore criticism, if any were offered; and his statements about the behaviour of the leaves were faithfully copied from book to book.

Broussonet (in 1784) attempted to explain the contraction of the leaves by supposing that the captured insect pricked them, and so let out the fluid which previously kept them turgid and expanded.

Dr. Darwin (1761) was contented to suppose that the Dionæa surrounded itself with insect traps to prevent depredations upon its flowers.

Sixty years after Linnæus wrote, however, an able botanist, the Rev. Dr. Curtis (dead but a few years since) resided at Wilmington, in North Carolina, the head-quarters of this very local plant. In 1834 he published an account of it in the *Boston Journal of Natural History*, which is a model of accurate scientific observation. This is what he said:—"Each half of the leaf is a little concave on the inner side, where are placed three delicate hair-like organs, in such an order that an insect can hardly traverse it without interfering with one of them, when the two sides suddenly collapse and enclose the prey, with a force surpassing an insect's efforts to escape. The fringe of hairs on the opposite sides of a leaf interlace, like the fingers of two hands clasped together. The sensitiveness resides only in these hair-like processes on the inside, as the leaf may be touched or pressed in any other part without sensible effects. The little prisoner is not crushed and suddenly destroyed, as is sometimes supposed, for I have often liberated captive flies and spiders, which sped away as fast as fear or joy could carry them. At other times I have found them enveloped in a fluid of a mucilaginous consistence, which seems to act as a solvent, the insects being more or less consumed in it."

To Ellis belongs the credit of divining the purpose of the capture of insects by the Dionæa. But Curtis made out the details of the mechanism, by ascertaining the seat of the sensitiveness in the leaves; and he also pointed out that the secretion was not a lure exuded before the capture, but a true digestive fluid poured out, like our own gastric juice after the ingestion of food.

For another generation the history of this wonderful plant stood still; but in 1868 an American botanist, Mr. Canby, who is happily still engaged in botanical research—while staying in the Dionæa district, studied the habits of the plant pretty carefully, especially the points which Dr Curtis had made out. His first idea was that "the leaf had the power of dissolving animal matter, which was then allowed to flow along the somewhat trough-like petiole to the root, thus furnishing the plant with highly nitrogenous food." By feeding the leaves with small pieces of beef, he found, however, that these were completely dissolved and absorbed; the leaf opening again with a dry surface, and ready for another meal, though with an appetite somewhat jaded. He found that cheese disagreed horribly with the leaves, turning them black, and finally killing them. Finally, he details the useless struggles of a Curculio to escape, as thoroughly establishing the fact that the fluid already mentioned is actually secreted, and is not the result of the decomposition of the substance which the leaf has seized. The Curculio being of a resolute nature, attempted to eat his way out,—"when discovered he was still alive, and had made a small hole through the side of the leaf, but was evidently becoming very weak. On opening the leaf, the fluid was found in considerable quantity around him, and was without doubt gradually overcoming him. The leaf being again allowed to close upon him, he soon died."

At the meeting of this Association last year, Dr. Burdon-Sanderson made a communication, which, from its remarkable character, was well worthy of the singular history of this plant; one by no means closed yet, but in which his observations will head a most interesting chapter.

It is a generalisation—now almost a household word—that all living things have a common bond of union in a substance—always present where life manifests itself—which underlies all their details of structure. This is called *protoplasm*. One of its most distinctive properties is its aptitude to contract; and when in any given organism the particles of protoplasm are so arranged that they act as it were in concert, they produce a cumulative effect which is very manifest in its results. Such a manifestation is found in the contraction of muscle; and such a manifestation we possibly have also in the contraction of the leaf of Dionæa.

The contraction of muscle is well known to be accompanied by certain electrical phenomena. When we place a fragment of muscle in connection with a delicate galvanometer, we find that between the outside surface and a cut surface there is a definite current, due to what is called the electromotive force of the muscle. Now, when the muscle is made to contract, this electromotive force momentarily disappears. The needle of the galvanometer, deflected before, swings back towards the point of rest; there is what is called a *negative variation*. All students of the vegetable side of organised nature were astonished to hear from Dr. Sanderson that certain experiments which, at the instigation of Mr. Darwin, he had made, proved to demonstration that when a leaf of Dionæa contracts, the effects produced are precisely similar to those which occur when muscle contracts.

Not merely, then, are the phenomena of digestion in this wonderful plant like those of animals, but the phenomena of contractility agree with those of animals also.

Drosera.—Not confined to a single district in the New World, but distributed over the temperate parts of both hemispheres, in sandy and marshy places, are the curious plants called Sundews—the species of the genus Drosera. They are now known to be near congeners of Dionæa, a fact which was little more than guessed at when the curious habits which I am about to describe were first discovered.

Within a year of each other, two persons—one an Englishman, the other a German—observed that the curious hairs which everyone notices on the leaf of Drosera were sensitive.

This is the account which Mr. Gardom, a Derbyshire botanist, gives of what his friend Mr. Whateley, "an eminent London surgeon," made out in 1780:—"On inspecting some of the contracted leaves we observed a small insect or fly very closely imprisoned therein, which occasioned some astonishment as to how it happened to get into so confined a situation. Afterwards, on Mr. Whateley's centrically pressing with a pin other leaves yet in their natural and expanded form, we observed a remarkably sudden and elastic spring of the leaves, so as to become inverted upwards, and, as it were, encircling the pin, which evidently showed the method by which the fly came into its embarrassing situation."

This must have been an account given from memory, and represents the movement of the hairs as much more rapid than it really is.

In July of the preceding year (though the account was not published till two years afterwards), Roth, in Germany, had remarked in *Drosera rotundifolia* and *longifolia*, "that many leaves were folded together from the point towards the base, and that all the hairs were bent like a bow, but that there was no apparent change on the leaf-stalk." Upon opening these leaves, he says, "I found in each a dead insect; hence I imagined that this plant, which has some resemblance to the *Dionæa muscipula*, might also have a similar moving power."

"With a pair of pliers I placed an ant upon the middle of the leaf of *D. rotundifolia*, but not so as to disturb the plant. The ant endeavoured to escape, but was held fast by the clammy juice at the points of the hairs, which was drawn out by its feet into fine threads. In some minutes the short hairs on the disc of the leaf began to bend, then the long hairs, and laid themselves upon the insect. After a while the leaf began to bend, and in some hours the end of the leaf was so bent inwards as to touch the base. The ant died in fifteen minutes, which was before all the hairs had bent themselves."

These facts, established nearly a century ago by the testimony of independent observers, have up to the present time been almost ignored; and Trecul, writing in 1855, boldly asserted that the facts were not true.

More recently, however, they have been repeatedly verified: in Germany by Nitschke, in 1860; in America by a lady, Mrs. Treat, of New Jersey, in 1871; in this country by Mr. Darwin, and also by Mr. A. W. Bennett.

To Mr. Darwin, who for some years past has had the subject under investigation, we are indebted, not merely for the complete confirmation of the facts attested by the earliest observers, but also for some additions to those facts which are extremely important. The whole investigation still awaits publication at his hands, but some of the points which were established have been announced by Professor Asa Gray in America, to whom Mr. Darwin had communicated them.

Mr. Darwin found that the hairs on the leaf of Drosera responded to a piece of muscle or other animal substance, while to any particle of inorganic matter they were nearly indifferent. To minute fragments of carbonate of ammonia they were more responsive.

I will now give the results of Mrs. Treat's experiments, in her own words:—

"Fifteen minutes past ten I placed bits of raw beef on some of the most vigorous leaves of *Drosera longifolia*. Ten minutes past twelve two of the leaves had folded around the beef, hiding it from sight. Half-past eleven on the same day, I placed living flies on the leaves of *D. longifolia*. At twelve o'clock and forty-eight minutes, one of the leaves had folded entirely round its victim, and the other leaves had partially folded, and the flies had ceased to struggle. By half-past two, four leaves had each folded around a fly. The leaf folds from the apex to the petiole, after the manner of its vernation. I tried mineral substances, bits of dried chalk, magnesia, and pebbles. In twenty-four hours neither the leaves nor the bristles had made any move in clasping these articles. I wetted a piece of chalk in water, and in less than an hour the bristles were curving about it, but soon unfolded again, leaving the chalk free on the blade of the leaf."

Time will not allow me to enter into further details with respect to Dionæa and Drosera. The repeated testimony of various observers spreads over a century, and though at no time warmly received, must, I think, satisfy you that in this small family of the Droseraceæ we have plants which in the first place capture animals for purposes of food, and in the second, digest and dissolve them by means of a fluid which is poured out for the purpose; and thirdly, absorb the solution of animal matter which is so produced.

Before the investigations of Mr. Darwin had led other persons to work at the subject, the meaning of these phenomena was very little appreciated. Only a few years ago, Duchartre, a French physiological botanist, after mentioning the views of Ellis and Curtis with respect to Dionæa, expressed his opinion that the idea that its leaves absorbed dissolved animal substances was too evidently in disagreement with our knowledge of the function of leaves and the whole course of vegetable nutrition to deserve being seriously discussed.

Perhaps if the Droseraceæ were an isolated case of a group of plants exhibiting propensities of this kind, there might be some reason for such a criticism. But I think I shall be able to show you that this is by no means the case. We have now reason to believe that there are many instances of these carnivorous habits in different parts of the vegetable kingdom, and among plants which have nothing else in common but this.

As another illustration I shall take the very curious group of Pitcher-plants which is peculiar to the New World. And here also I think we shall find it most convenient to follow the historical order in the facts.

Sarracenia.—The genus Sarracenia consists of eight species, all similar in habit, and all natives of the Eastern States of North America, where they are found more especially in bogs, and even in places covered with shallow water. Their leaves, which give them a character entirely their own, are pitcher-shaped or trumpet-like, and are collected in tufts springing immediately from the ground; and they send up at the flowering season one or more slender stems bearing each a solitary flower. This has a singular aspect, due to a great extent to the umbrella like expansion in which the style terminates; the shape of this, or perhaps of the whole flower, caused the first English settlers to give to the plant the name of Side-saddle Flower.

Sarracenia purpurea is the best known species. About ten years ago it enjoyed an evanescent notoriety from the fact that its rootstock was proposed as a remedy for small-pox. It is found from Newfoundland southward to Florida, and is fairly hardy under open-air cultivation in the British Isles. At the commencement of the seventeenth century, Clusius published a figure of it, from a sketch which found its way to Lisbon and thence to Paris. Thirty years later Johnson copied this in his edition of Gerard's Herbal, hoping "that some or other that travel into foreign parts may find this elegant plant, and know it by this small expression, and bring it home with them, so that we may come to a perfecter knowledge thereof." A few years afterwards this wish was gratified. John Tradescant the younger found the plant in Virginia, and succeeded in bringing it home alive to England. It was also sent to Paris from Quebec by Dr. Sarrazin, whose memory has been commemorated in the name of the genus, by Tournefort.

The first fact which was observed about the pitchers was, that when they grew they contained water. But the next fact which was recorded about them was curiously mythical. Perhaps Morrison, who is responsible for it, had no favourable opportunities of studying them, for he declares them to be, what is by no means really the case, intolerant of cultivation (*respuere culturam videntur*).

He speaks of the lid, which in all the species is tolerably rigidly fixed, as being furnished, by a special act of providence, with a hinge. This idea was adopted by Linnæus, and somewhat amplified by succeeding writers, who declared that in dry weather the lid closed over the mouth, and checked the loss of water by evaporation. Catesby, in his fine work on the Natural History of Carolina, supposed that these water-receptacles might "serve as an asylum or secure retreat for numerous insects, from frogs and other animals which feed on them;"—and others followed Linnæus in regarding the pitchers as reservoirs for birds and other animals, more especially in times of drought; "*præbet aquam sitientibus aviculis.*"

The superficial teleology of the last century was easily satisfied without looking far for explanations, but it is just worth while pausing for a moment to observe that, although Linnæus had no materials for making any real investigation as to the purpose of the pitchers of Sarracenias, he very sagaciously anticipated the modern views as to their affinities. They are now regarded as very near allies of water-lilies—precisely the position which Linnæus assigned to them in his fragmentary attempt at a true natural classification. And besides this, he also suggested the analogy, which, improbable as it may seem at first sight, has been worked out in detail by Baillon (in apparent ignorance of Linnæus' writings) between the leaves of Sarracenia and water-lilies.

Linnæus seems to have supposed that Sarracenia was originally aquatic in its habits, that it had Nymphæa-like leaves, and that when it took to a terrestrial life its leaves became hollowed out, to contain the water in which they could no longer float—in fact, he showed himself to be an evolutionist of the true Darwinian type.

Catesby's suggestion was a very infelicitous one. The insects which visit these plants may find in them a retreat, but it is one from which they never return. Linnæus' correspondent Collinson remarked in one of his letters, that "many poor insects lose their lives by being drowned in these cisterns of water;" but William Bartram, the son of the botanist, seems to have been the first to put on record, at the end of the last century, the fact that Sarracenias catch insects and put them to death in the wholesale way that they do.

Before stopping to consider how this is actually achieved, I will carry the history a little further.

In the two species in which the mouth is unprotected by the lid it could not be doubted that a part, at any rate, of the contained fluid was supplied by rain. But in *Sarracenia variolaris*, in which the lid closes over the mouth, so that rain cannot readily enter it, there is no doubt that a fluid is secreted at the bottom of the pitchers, which probably has a digestive function. William Bartram, in the preface to his travels in 1791, described this fluid, but he was mistaken in supposing that it acted as a lure. There is a sugary secretion which attracts insects, but this is only found at the upper part of the tube. Bartram must be credited with the suggestion, which he, however, only put forward doubtfully, that the insects were dissolved in the fluid, and then became available for the alimentation of the plants.

Sir J. E. Smith, who published a figure and description of *Sarracenia variolaris*, noticed that it secreted fluid, but was content to suppose that it was merely the gaseous products of the decomposition of insects that subserved the processes of vegetation. In 1829, however, thirty years after Bartram's book, Burnett wrote a paper containing a good many original ideas expressed in a somewhat quaint fashion, in which he very strongly insisted on the existence of a true digestive process in the case of Sarracenia, analogous to that which takes place in the stomach of an animal.

Our knowledge of the habits of *Sarracenia variolaris* is now pretty complete, owing to the observations of two South Carolina physicians. One, Dr. M'Bride, made his observations half a century ago, but they had, till quite recently, completely fallen into oblivion. He devoted himself to the task of ascertaining why it was that *Sarracenia variolaris* was visited by flies, and how it was that it captured them. This is what he ascertained:—

"The cause which attracts flies is evidently a viscid substance resembling honey, secreted by or exuding from the internal surface of the tube. From the margin, where it commences, it does not extend lower than one-fourth of an inch. The falling of the insect as soon as it enters the tube is wholly attributable to the downward or inverted position of the hairs of the internal surface of the leaf. At the bottom of a tube split open, the hairs are plainly discernible, pointing downwards; as the eye ranges upward they gradually become shorter and attenuated, till at or just below the surface covered by the bait they are no longer perceptible to the naked eye, nor to the most delicate touch. It is here that the fly cannot take a hold sufficiently strong to support itself, but falls."

Dr. Mellichamp, who is now resident in the district in which Dr. M'Bride made his observations, has added a good many particulars to our knowledge. He first investigated the fluid which is secreted at the bottom of the tubes. He satisfied himself that it was really secreted, and describes it as mucilaginous, but leaving in the mouth a peculiar astringency. He compared the action of this fluid with that of distilled water on pieces of fresh venison, and found that after fifteen hours the fluid had produced most change, and also most smell; he therefore concluded that as the leaves when stuffed with insects become most disgusting in odour, we have to do, not with a true digestion, but with an accelerated decomposition. Although he did not attribute any true digestive power to the fluid secreted by the pitchers, he found that it had a remarkable anæsthetic effect upon flies immersed in it. He remarked that "a fly when thrown into water is very apt to escape, as the fluid seems to *run* from its wings," but it never escaped from the Sarracenia secretion. About half a minute after being thrown in, the fly became to all appearance dead, though, if removed, it gradually recovered in from half an hour to an hour.

According to Dr. Mellichamp, the sugary lure discovered by Dr. M'Bride, at the mouth of the pitchers, is not found on either the young ones of one season or the older ones of the previous year. He found, however, that about May it could be detected without difficulty, and more wonderful still, that there is a honey-baited pathway leading directly from the ground to the mouth, along the broad wing of the pitcher, up which insects are led to their destruction. From these narratives it is evident that there are two very different types of pitcher in Sarracenia, and an examination of the species shows that there may probably be three. These may be primarily classified into those with the mouth open and lid erect, and which consequently receive the rain-water in more or less abundance; and those with the mouth closed by the lid, into which rain can hardly, if at all, find ingress.

To the first of these belongs the well-known *S. purpurea*, with inclined pitchers, and a lid so disposed as to direct all the rain that falls upon it also into the pitcher; also *S. flava*, *rubra*, and *Drummondii*, all with erect pitchers and vertical lids; of these three, the lid in a young state arches over the mouth, and in an old state stands nearly erect, and has the sides so reflected that the rain which falls on its upper surface is guided down the outside of the back of the pitcher, as if to prevent the flooding of the latter.

To the second group belong *S. psittacina* and *S. variolaris*. The tissues of the internal surfaces of the pitchers are singularly beautiful. They have been described in one species only, the *S. purpurea*, by August Vogl; but from this all the other species which I have examined differ materially. Beginning from the upper part of the pitcher, there are four surfaces, characterised by different tissues, which I shall name and define as follows:—

1. An *attractive* surface, occupying the inner surface of the lid, which is covered with an epidermis, stomata, and (in common with the mouth of the pitcher) with minute honey-secreting glands; it is further often more highly coloured than any other part of the pitcher, in order to attract insects to the honey.

2. A *conducting* surface, which is opaque, formed of glassy cells, which are produced into deflexed, short, conical, spinous processes. These processes, overlapping like the tiles of a house, form a surface down which an insect slips, and affords no foothold to an insect attempting to crawl up again.

3. A *glandular* surface (seen in *S. purpurea*), which occupies a considerable portion of the cavity of the pitcher below the conducting surface. It is formed of a layer of epidermis with sinuous cells, and is studded with glands; and being smooth and polished, this too affords no foothold for escaping insects.

4. A *detentive* surface, which occupies the lower part of the pitcher, in some cases for nearly its whole length. It possesses no cuticle, and is studded with deflexed, rigid, glass-like, needle-formed, striated hairs, which further converge towards the axis of the diminishing cavity; so that an insect, if once amongst them, is effectually detained, and its struggles have no other result than to wedge it lower and more firmly in the pitcher.

Now, it is a very curious thing that in *S. purpurea*, which has an open pitcher, so formed as to receive and retain a maximum of rain, no honey-secretion has hitherto been found, nor has any water been seen to be secreted in the pitcher; it is, further, the only species in which (as stated above) I have found a special glandular surface, and in which no glands occur on the detentive surface. This concurrence of circumstances suggests the possibility of this plant either having no proper secretion of its own, or only giving it off after the pitcher has been filled with rain-water.

In *S. flava*, which has open-mouthed pitchers and no special glandular surface, I find glands in the upper portion of the detentive surface, among the hairs, but not in the middle or lower part of the same surface. It is proved that *S. flava* secretes fluid, but under what precise conditions I am not aware. I have found none but what may have been accidentally introduced in the few cultivated specimens which I have examined, either in the full-grown state, or in the half-grown when the lid arches over the pitcher. I find the honey in these as described by the American observers, and honey-secreting glands on the edge of the wing of the pitcher, together with similar glands on the outer surface of the pitcher, as seen by Vogl in *S. purpurea*.

Of the pitchers with closed mouths, I have examined those of *S. variolaris* only, whose tissues closely resemble those of *S. flava*. That it secretes a fluid noxious to insects there is no doubt, though in the specimens I examined I found none.

There is thus obviously much still to be learned with regard to Sarracenia, and I hope that American botanists will apply themselves to this task. It is not probable that three pitchers, so differently constructed as those of *S. flava*, *purpurea*, and *variolaris*, and presenting such differences in their tissues, should act similarly. The fact that insects normally decompose in the fluid of all, would suggest the probability that they all feed on the products of decomposition; but as yet we are absolutely ignorant whether the glands within the pitchers are secretive, or absorptive, or both; if secretive, whether they secrete water or a solvent; and if absorptive, whether they absorb animal matter or the products of decomposition.

It is quite likely, that just as the saccharine exudation only makes its appearance during one particular period in the life of the pitcher, so the digestive functions may also be only of short duration. We should be prepared for this from the case of the Dionæa, the leaves of which cease after a time to be fit for absorption, and become less sensitive. It is quite certain that the insects which go on accumulating in the pitchers of Sarracenias must be far in excess of its needs for any legitimate process of digestion. They decompose; and various insects, too wary to be entrapped themselves, seem habitually to drop their eggs into the open mouth of the pitchers, to take advantage of the accumulation of food. The old pitchers are consequently found to contain living larvæ and maggots, a sufficient proof that the original properties of the fluid which they secreted must have become exhausted; and Barton tells us that various insectivorous birds slit open the pitchers with their beaks to get at the contents. This was probably the origin of Linnæus' statement that the pitchers supplied birds with water.

The pitchers finally decay, and part, at any rate, of their contents must supply some nutriment to the plant by fertilising the ground in which it grows.

Darlingtonia.—I cannot take leave of Sarracenia without a short notice of its near ally, Darlingtonia, a still more wonderful plant, an outlier of Sarracenia in geographical distribution, being found at an elevation of 5,000 ft. on the Sierra Nevada of California, far west of any locality inhabited by Sarracenia. It has pitchers of two forms; one, peculiar to the infant state of the plant, consists of narrow, somewhat twisted, trumpet-shaped tubes, with very oblique open mouths, the dorsal lip of which is drawn out into a long, slender, arching, scarlet hood, that hardly closes the mouth. The slight twist in the tube causes these mouths to point in various directions, and they entrap very small insects only. Before arriving at a state of maturity the plant bears much larger, suberect pitchers, also twisted, with the lip produced into a large inflated hood, that completely arches over a very small entrance to the cavity of the pitcher. A singular orange-red, flabby, two-lobed organ hangs from the end of the hood, right in front of the entrance, which, as I was informed last week by letter from Prof. Asa Gray, is smeared with honey on its inner surface. These pitchers are crammed with large insects, especially moths, which decompose in them, and result in a putrid mass. I have no information of water being found in its pitchers in its native country, but have myself found a slight acid secretion in the young states of both forms of pitcher.

The tissues of the inner surfaces of the pitchers of both the young and the old plant I find to be very similar to those of *Sarracenia variolaris* and *flava*.

Looking at a flowering specimen of Darlingtonia, I was struck with a remarkable analogy between the arrangement and colouring of the parts of the leaf and of the flower. The petals are of the same colour as the flap of the pitcher, and between each pair of petals is a hole (formed by a notch in the opposed margins of each) leading to the stamens and stigma. Turning to the pitcher, the relation of its flap to its entrance is somewhat similar. Now, we know that coloured petals are specially attractive organs, and that the object of their colour is to bring insects to feed on the pollen or nectar, and in this case by means of the hole to fertilise the flower; and that the object of the flap and its sugar is also to attract insects, but with a very different result, cannot be doubted. It is hence conceivable that this marvellous plant lures insects to its flowers for one object, and feeds them while it uses them to fertilise itself, and that, this accomplished, some of its benefactors are thereafter lured to its pitchers for the sake of feeding itself!

But to return from mere conjecture to scientific earnest, I cannot dismiss Darlingtonia without pointing out to you what appears to me a most curious point in its history; which is, that the change from the slender, tubular, open-mouthed to the inflated closed-mouthed pitchers is, in all the specimens which I have examined, absolutely sudden in the individual plant. I find no pitchers in an intermediate stage of development. This, a matter of no little significance in itself, derives additional interest from the fact that the young pitchers to a certain degree represent those of the Sarracenias with open mouths and erect lids; and the old pitchers those of the Sarracenias with closed mouths and globose lids. The combination of representative characters in an outlying species of a small order cannot but be regarded as a marvellously significant fact in the view of those morphologists who hold the doctrine of evolution.

Nepenthes.—The genus Nepenthes consists of upwards of thirty species of climbing, half shrubby plants, natives of the hotter parts of the Asiatic Archipelago from Borneo to Ceylon, with a few outlying species in New Caledonia, in Tropical Australia, and in the Seychelle Islands on the African coast. Its pitchers are abundantly produced, especially during the younger state of the plants. They present very considerable modifications of form and external structure, and vary greatly in size, from little more than an inch to almost a foot in length; one species, indeed, which I have here from the mountains of Borneo, has pitchers which, including the lid, measure a foot and a half, and its capacious bowl is large enough to drown a small animal or bird.

The structure of the pitcher of Nepenthes is less complicated on the whole than that of Sarracenia, though some of its tissues are much more highly specialised. The pitcher itself is here not a transformed leaf, as in Sarracenia, nor is it a transformed leaf-blade, like that of Dionæa, but an appendage of the leaf developed at its tip, and answers to a water-secreting gland that may be seen terminating the mid-rib of the leaf of certain plants. It is furnished with a stalk, often a very long one, which in the case of pitchers formed on leaves high up the stem has (before the full development of the pitcher) the power of twisting like a tendril round neighbouring objects, and thus aiding the plant in climbing, often to a great height in the forest.

In most species the pitchers are of two forms, one appertaining to the young, the other to the old state of the plant, the transition from one form to the other being gradual. Those of the young state are shorter and more inflated; they have broad fringed longitudinal wings on the outside, which are probably guides to lead insects to the mouth; the lid is smaller and more open, and the whole interior surface is covered with secreting glands. Being formed near the root of the plant, these pitchers often rest on the ground, and in species which do not form leaves near the root they are sometimes suspended from stalks which may be fully a yard long, and which bring them to the ground. In the older state of the plant the pitchers are usually much longer, narrower, and less inflated, and are trumpet-shaped, or even conical; the wings also are narrower, less fringed, or almost absent. The lid is larger and slants over the mouth, and only the lower part of the pitcher is covered with secreting glands, the upper part presenting a tissue analogous to the conducting tissue of Sarracenia, but very different anatomically. The difference in structure of these two forms of pitcher, if considered in reference to their different positions on the plant, forces the conclusion on the mind that the one form is intended for ground game, the other for winged game. In all cases the mouth of the pitcher is furnished with a thickened corrugated rim, which serves three purposes: it strengthens the mouth and keeps it distended; it secretes honey (at least in all the species I have examined under cultivation, for I do not find that any other observer has noticed the secretion of honey by Nepenthes), and it is in various species developed into a funnel-shaped tube that descends into the pitcher and prevents the escape of insects, or into a row of incurved hooks that are in some cases strong enough to retain a small bird, should it, when in search of water or insects, thrust its body beyond a certain length into the pitcher.

In the interior of the pitcher of Nepenthes there are three principal surfaces: an *attractive*, *conductive*, and a *secretive* surface; the *detentive* surface of Sarracenia being represented by the fluid secretion, which is here invariably present at all stages of growth of the pitcher.

The attractive surfaces of Nepenthes are two: those, namely, of the rim of the pitcher, and of the under surface of the lid, which is provided in almost every species with honey-secreting glands, often in great abundance. These glands consist of spherical masses of cells, each embedded in a cavity of the tissue of the lid, and encircled by a guard-ring of glass-like cellular tissue. As in Sarracenia, the lid and mouth of the pitcher are more highly coloured than any other part, with the view of attracting insects to their honey. It is a singular fact that the only species known to me that wants these honey-glands on the lid is the *N. ampullaria*, whose lid, unlike that of the other species, is thrown back horizontally. The secretion of honey on a lid so placed would tend to lure insects away from the pitcher instead of into it.

From the mouth to a variable distance down the pitcher is an opaque glaucous surface, precisely resembling in colour and appearance the conductive surface of the Sarracenia, and, like it, affording no foothold to insects, but otherwise wholly different; it is formed of a fine network of cells, covered with a glass-like cuticle, and studded with minute reniform transverse excrescences.

The rest of the pitcher is entirely occupied with the secretive surface, which consist of a cellular floor crowded with spherical glands in inconceivable numbers. Each gland precisely resembles a honey-gland of the lid, and is contained in a pocket of the same nature, but semicircular, with the mouth downwards, so that the secretive fluid all falls to the bottom of the pitcher. In the *Nepenthes Rafflesiana* 3,000 of the glands occur on a square inch of the inner surface of the pitcher, and upwards of 1,000,000 in an ordinary sized pitcher. I have ascertained that, as was indeed to be expected, they secrete the fluid which is contained in the bottom of the pitcher before this opens, and that the fluid is always acid.

The fluid, though invariably present, occupies a comparatively small portion of the glandular surface of the pitcher, and is collected before the lid opens. When the fluid is emptied out of a fully formed pitcher that has not received animal matter, it forms again, but in comparatively very small quantities; and the formation goes on for many days, and to some extent even after the pitcher has been removed from the plant. I do not find that placing inorganic substances in the fluid causes an increased secretion, but I have twice observed a considerable increase of fluid in pitchers after putting animal matter in the fluid.

To test the digestive powers of Nepenthes I have closely followed Mr. Darwin's treatment of Dionæa and Drosera, employing white of egg, raw meat, fibrine, and cartilage. In all cases the action is most evident, in some surprising. After twenty-four hours' immersion the edges of the cubes of white of egg are eaten away and the surfaces gelatinised. Fragments of meat are rapidly reduced; and pieces of fibrine weighing several grains dissolve and totally disappear in two or three days. With cartilage the action is most remarkable of all; lumps of this weighing 8 or 10 grains are half gelatinised in twenty-four hours, and in three days the whole mass is greatly diminished, and reduced to a clear transparent jelly. After drying some cartilage in the open air for a week, and placing it in an unopened but fully formed pitcher of *N. Rafflesiana*, it was acted upon similarly and very little slower.

That this process, which is comparable to digestion, is not wholly due to the fluid first secreted by the glands, appears to me most probable; for I find that very little action takes place in any of the substances placed in the fluid drawn from pitchers, and put in glass tubes; nor has any followed after six days' immersion of cartilage or fibrine in pitchers of *N. ampullaria* placed in a cold room; whilst on transferring the cartilage from the pitcher of *N. ampullaria* in the cold room to one of *Rafflesiana* in the stove, it was immediately acted upon. Comparing the action of fibrine, meat, and cartilage placed in tubes of Nepenthes fluid, with others in tubes of distilled water, I observed that their disintegration is three times more rapid in the fluid; but this disintegration is wholly different from that effected by immersion in the fluid of the pitcher of a living plant.

In the case of small portions of meat, ½ to 2 grains, all seem to be absorbed; but with 8 to 10 grains of cartilage it is not so—a certain portion disappears, the rest remains as a transparent jelly, and finally becomes putrid, but not till after many days. Insects appear to be acted upon somewhat differently, for after several days' immersion of a large piece of cartilage I found that a good-sized cockroach, which had followed the cartilage and was drowned for his temerity, in two days became putrid. In removing the cockroach the cartilage remained inodorous for many days. In this case no doubt the antiseptic fluid had permeated the tissue of the cartilage, whilst enough did not remain to penetrate the chitinous hard covering of the insect, which consequently decomposed.

In the case of cartilage placed in fluid taken from the pitcher —it becomes putrid, but not so soon as if placed in distilled water.

From the above observations it would appear probable that a substance acting as pepsine is given off from the inner wall of the pitcher, but chiefly after placing animal matter in the acid fluid; but whether this active agent flows from the glands or from the cellular tissue in which they are imbedded, I have no evidence to show.

I have here not alluded to the action of these animal matters in the cells of the glands, which is, as has been observed by Mr. Darwin in Drosera, to bring about remarkable changes in their protoplasm, ending in their discoloration. Not only is there aggregation of the protoplasm in the gland-cells, but the walls of the cells themselves become discoloured, and the glandular surface of the pitcher that at first was of a uniform green, becomes covered with innumerable brown specks (which are the discoloured glands). After the function of the glands is exhausted, the fluid evaporates, and the pitcher slowly withers.

At this stage I am obliged to leave this interesting investigation. That Nepenthes possesses a true digestive process such as has been proved in the case of Drosera, Dionæa, and Pinguicula, cannot be doubted. This process, however, takes place in a fluid which deprives us of the power of following it further by direct observation. We cannot here witness the pouring out of the digestive fluid; we must assume its presence and nature from the behaviour of the animal matter placed in the fluid in the pitcher. From certain characters of the cellular tissues of the interior walls of the pitcher, I am disposed to think that it takes little part in the processes of either digestion or assimilation, and that these, as well as the pouring out of the acid fluid, are all functions of the glands.

In what I have said I have described the most striking instances of plants which seem to invert the order of nature, and to draw their nutriment—in part, at least—from the animal kingdom, which it is often held to be the function of the vegetable kingdom to sustain.

I might have added some additional cases to those I have already dwelt upon. Probably, too, there are others still unknown to science, or whose habits have not yet been detected. Delpino, for example, has suggested that a plant, first described by myself in the Botany of the Antarctic Voyage, *Caltha dionæafolia*, is so analogous in the structure of its leaves to Dionæa, that it is difficult to resist the conviction that its structure also is adapted for the capture of small insects.

But the problem that forces itself upon our attention is, How does it come to pass that these singular aberrations from the otherwise uniform order of vegetable nutrition make their appearance in remote parts of the vegetable kingdom? why are they not more frequent, and how were such extraordinary habits brought about or contracted? At first sight the perplexity is not diminished by considering—as we may do for a moment—the nature of ordinary vegetable nutrition. Vegetation, as we see it everywhere, is distinguished by its green colour, which we know depends on a peculiar substance called chlorophyll, a substance which has the singular property of attracting to itself the carbonic acid gas which is present in minute quantities in the atmosphere, of partly decomposing it, so far as to set free a portion of its oxygen, and of recombining it with the elements of water, to form those substances, such as starch, cellulose, and sugar, out of which the framework of the plant is constructed.

But, besides these processes, the roots take up certain matters from the soil. Nitrogen forms nearly four-fifths of the air we breathe, yet plants can possess themselves of none of it in the free uncombined state. They withdraw nitrates and salts of ammonia in minute quantities from the ground, and from these they build up with starch, or some analogous material, albuminoids or protein compounds, necessary for the sustentation and growth of protoplasm.

At first sight nothing can be more unlike this than a Dionæa or a Nepenthes capturing insects, pouring out a digestive fluid upon them, and absorbing the albuminoids of the animal, in a form probably directly capable of appropriation for their own nutrition. Yet there is something not altogether wanting in analogy in the case of the most regularly constituted plants. The seed of the castor-oil plant contains, besides the embryo seedling, a mass of cellular tissue or endosperm filled with highly nutritive substances. The seedling lies between masses of this, and is in contact with it; and as the warmth and moisture of germination set up changes which bring about the liquefaction of the contents of the endosperm and the embryo absorbs them, it grows in so doing, and at last, having taken up all it can from the exhausted endosperm, develops chlorophyll in its cotyledons under the influence of light, and relies on its own resources.

A large number of plants, then, in their young condition, borrow their nutritive compounds ready prepared; and this is in effect what carnivorous plants do later in life.

That this is not a merely fanciful way of regarding the relation of the embryo to the endosperm, is proved by the ingenious experiments of Van Tieghem, who has succeeded in substituting for the real, an artificial endosperm, consisting of appropriate nutritive matters. Except that the embryo has its food given to it in a manner which needs no digestion—a proper concession to its infantine state—the analogy here with the mature plants which feed on organic food seems to be complete.

But we are beginning also to recognise the fact that there are a large number of flowering plants that pass through their lives without ever doing a stroke of the work that green plants do. These have been called Saprophytes. Monotropra, the curious bird's nest orchis (*Neottia nidus-avis*), Epipogium, and Corallorhiza are instances of British plants which nourish themselves by absorbing the partially decomposed materials of other plants, in the shady or marshy places which they inhabit. They reconstitute these products of organic decomposition, and build them up once more into an organism. It is curious to notice, however, that the tissues of Neottia still contain chlorophyll in a nascent though useless state, and that if a plant of it be immersed in boiling water, the characteristic green colour reveals itself.

Epipogium and Corallorhiza have lost their proper absorbent organs; they are destitute of roots, and take in their food by the surfaces of their underground stem structures.

The absolute difference between plants which absorb and nourish themselves by the products of the decomposition of plant-structures, and those which make a similar use of animal structures, is not very great. We may imagine that plants accidentally permitted the accumulation of insects in some parts of their structure, and the practice became developed because it was found to be useful. It was long ago suggested that the receptacle formed by the connate leaves of Dipsacus might be an incipient organ of this kind; and though no insectivorous habit has ever been brought home to that plant, the theory is not improbable.

Linnæus, and more lately Baillon, have shown how a pitcher of Sarracenia may be regarded as a modification of a leaf of the Nymphæa type. We may imagine such a leaf first becoming hollow, and allowing *débris* of different kinds to accumulate; these would decompose, and a solution would be produced, some of the constituents of which would diffuse themselves into the subjacent plant tissues. This is in point of fact absorption, and we may suppose that in the first instance—as perhaps still in *Sarracenia purpurea*—the matter absorbed was merely the saline nutritive products of decomposition, such as ammoniacal salts. The act of digestion—that process by which soluble food is reduced without decomposition to a soluble form fitted for absorption—was doubtless subsequently acquired.

The secretion, however, of fluids by plants is not an unusual phenomenon. In many Aroids a small gland at the apex of the leaves secretes fluid, often in considerable quantities, and the pitcher of Nepenthes is, as I have shown elsewhere, only a gland of this kind, enormously developed. May not, therefore, the wonderful pitchers and carnivorous habit of Nepenthes have both originated by natural selection out of one such honey-secreting gland as we still find developed near that part of the pitcher which represents the tip of the leaf? We may suppose insects to have been entangled in the viscid secretion of such a gland, and to have perished there, being acted upon by those acid secretions that abound in these and most other plants. The subsequent differentiation of the secreting organs of the pitcher into aqueous, saccharine, and acid, would follow *pari passu* with the evolution of the pitcher itself, according to those mysterious laws which result in the correlation of organs and functions throughout the kingdom of Nature; and which, in my apprehension, transcend in wonder and interest those of evolution and the origin of species.

Delpino has recorded the fact that the spathe of Alocasia secretes an acid fluid which destroys the slugs that visit it, and which he believes subserves its fertilisation. Here any process of nutrition can only be purely secondary. But the fluids of plants are in the great majority of cases acid, and, when exuded, would be almost certain to bring about some solution in substances with which they came in contact. Thus the acid secretions of roots were found by Sachs to corrode polished marble surfaces with which they came in contact, and thus to favour the absorption of mineral matter.

The solution of albuminoid substances requires, however, besides a suitable acid, the presence of some other albuminoid substance analogous to pepsine. Such substances, however, are frequent in plants. Besides the well-known diastase, which converts the starch of malt into sugar, there are other instances in the synaptase which determines the formation of hydrocyanic acid from emulsine, and the myrosin which similarly induces the formation of oil of mustard. We need not wonder, then, if the fluid secreted by a plant should prove to possess the ingredients necessary for the digestion of insoluble animal matters.

These remarks will, I hope, lead you to see, that though the processes of plant nutrition are in general extremely different from those of animal nutrition, and involve very simple compounds, yet that the protoplasm of plants is not absolutely prohibited from availing itself of food, such as that by which the protoplasm of animals is nourished; under which point of view these phenomena of carnivorous plants will find their place, as one more link in the continuity of nature.

BRITISH ASSOCIATION REPORTS

Report of the Committee on Mathematical Tables.

The objects for which the Committee were appointed at Edinburgh were twofold, viz., the preparation of a list of tables scattered about in books and mathematical journals and transactions, and the calculation of new tables. With regard to the first object, the tables were roughly divided into three classes, viz. (1) ordinary tables (such as trigonometrical and logarithmic) usually published in books; (2) tables of continuously varying quantities, generally definite integrals; and (3) theory of number of tables. On the first class Mr. J. W. L. Glaisher had already written a report, to which it was intended, after the lapse of several years, to add a supplement; with the second some progress had been made; while Prof. Cayley proposed to undertake the third. The Committee had to acknowledge the assistance of several foreigners, and chiefly of Prof. Bierens de Haan, who had forwarded to them an account of 128 logarithmic and 105 non-logarithmic tables; to Dr. Carl Ohrtmann, of Berlin; and Profs. W. W. Johnson and J. M. Rice, of Annapolis, Maryland. The principal achievement, however, which the Committee had to report related to the second object, and was the completion of the tables of the Elliptic Functions, the commencement of which was noticed in NATURE nearly two years ago, and on which six or seven computers, under the superintendence of Mr. J. Glaisher, F.R.S., and Mr. J. W. L. Glaisher, have since been constantly engaged. These tables (which are of double entry) give the four theta functions, which form the numerators and denominators of the three elliptic functions, and their logarithms for 8,100 arguments; so that they contain nearly 65,000 tabular results. The calculation has been carried to ten figures, but only eight will be printed, the tabular portion of the work occupying 360 pages. Parts of the introduction will be written by Prof. Cayley, Sir William Thomson, and Prof. H. J. S. Smith, and it is hoped that before the next meeting of the Association the whole work, which will form one of the largest tables that have appeared as the result of an original calculation, will be in print. It is perhaps desirable to state that the elliptic functions which have thus been tabulated are, as it were, generalised sines and cosines. Sines and cosines may be combined so as to represent any singly periodic function, as is well known; and in the same way elliptic functions represent every possible doubly periodic function; and no quantities can be of a higher degree of periodicity. The elliptic functions (which are in a sense inverse to Legendre's Elliptic Integrals) are thus quantities of the highest importance and generality in mathematics, and they are daily becoming of more importance in physics. They appear conspicuously in the investigation of the motion of a rigid body and in electrostatics, and have also numerous applications in the theory of numbers. The calculations were just completed before the meeting, and the printing will commence immediately: it is intended that the tables shall be stereotyped to ensure freedom from typographical errors.

Report of the Committee on the Nomenclature of Dynamical and Electrical Units.

They have circulated numerous copies of their last year's report among scientific men both at home and abroad. They believe, however, that in order to render their recommendations fully available for science teaching and scientific work, a full and popular exposition of the whole subject of physical units is necessary, together with a collection of examples (tabular and otherwise) illustrating the application of systematic units to a variety of physical measurements. Students usually find peculiar difficulty in questions relating to units; and even the experienced scientific calculator is glad to have before him concrete examples with which to compare his own results as a security against misapprehension or mistake.

Some members of the Committee have been preparing a small volume of Illustrations of the C. G. S. System (centimetre-gramme-second system) intended to meet this want. The Committee do not desire to be re-appointed; at all events at present.

On Siemens' Pyrometer, by Prof. G. C. Foster.

The committee appointed to report upon Siemens' pyrometer has sought to determine whether or no the resistance is altered after exposure to high temperatures. The resistance was measured by means of Wheatstone's Bridge. An arrangement was adopted whereby the heat of the connecting wires was prevented from affecting the measurements. As a long thick iron tube surrounded the platinum coil of the pyrometer, it was impossible, in order to secure a standard temperature, to plunge the instrument into ice-cold water, because, owing to the conductivity of the iron, there was no certainty that the pyrometer wire was actually at the same temperature as the water. The temperature of 10° which was near the usual atmospheric temperature, was adopted as the standard.

Four instruments were examined: in one of them (1) the coil was surrounded by an iron sheath, in (2) and (3) a piece of stout platinum foil surrounded the cylinder between the iron sheath and the coil. In (4) there was no iron sheath, but a platinum tube instead. Nos. (1) (2) and (3) were found to be considerably altered after having been exposed to a high temperature. The instruments were placed in an ordinary fire and repeatedly heated to a red heat, at which they were maintained for several hours. The original resistance was ten units. The following numbers show the increase of resistance:—

(1) 0·834 (2) 1·608 (3) 1·169

These numbers expressed as fractions of the original resistance become (1) ·0834 (2) ·1608 (3) ·1169.

Equivalent change of temperature = (1) 30°, (2) 58°, (3) 43°. These measurements show that the change in resistance produced by exposure to high temperatures is so great as to invalidate the usefulness of these instruments.

No. (4). Resistance increased ·046, which expressed as a portion of the original resistance = ·0046. Equivalent change of temperature = 1°·5. The last instrument therefore gives results which are sufficiently constant for industrial application if not for strictly scientific purposes.

Prof. Williamson suggested that the change in the resistance might be due to a change in the platinum, as it has been found that platinum in contact with silica, in a reducing atmosphere, is altered at high temperatures.

Report of the Committee appointed to prepare and print tables of Wave Numbers.

Mr. G. J. Stoney stated that the work of this Committee was in progress, and that the Committee hoped to be in a position to make a full report at the next meeting of the Association. Under these circumstances they merely asked to be reappointed.

Second Report on the Sub-Wealden Exploration. By H. Willett and W. Topley.

This Report gave an account of the progress of the work since the last meeting of the Association. Most of the results attained have been already made public through the Quarterly Reports, and they were recently summarised in these columns. At the time of the Bradford meeting only 300 feet had been reached, and the age of the beds then being traversed was unknown. Mr. Peyton and Prof. Phillips discovered Kimmeridge Clay fossils immediately after the Report was read; since that time a large collection of fossils has been made, including most of the characteristic English Kimmeridge species, and some which are new. An undescribed species of *Modiola* is very abundant, and so is a small *Astarte*—the *A. Mysis* of D'Orbigny. A new species of this genus has been found, and a small *Trigonia* which Dr. Lycett believes to be also new.

The Kimmeridge Clay appears to be nearly 700 feet thick; generally it is a rather sandy clay, but towards the base there are some thick bands of cement stone. The Coral Rag is apparently absent. Amongst the fossils from the Oxford Clay the following were noticed:—*Ammonites Jason*, *Am. Lamberti*, *Am. Sedgwicki*, *Pollicipes concinnus*, *Gervillia*, and *Macrodon*. The total depth now reached is 1,030 feet, and 3,000*l.* has been spent. The Association has voted an increased grant of 100*l.*, and the Government has promised aid to the extent of 100*l.* for each 100 feet completed below 1,000 feet; but as each 100 feet will cost from 300*l.* to 400*l.* (including the cost of lining the hole), the Committee trust that subscriptions will still be forthcoming to enable them to continue the work.

Report of the Committee on the Influence of Forests on Rain.—It appeared from the very lengthened report that the operations of the committee during the past year had been restricted to the meteorological observations made at Carnwath, Lanarkshire. In order to carry on the operations at Carnwath, and extend them, a grant from the Association of not less than 25*l.* would be required for next year. They did not propose to commence observations at any new station.

SCIENTIFIC SERIALS

The Journal of Mental Science, July 1874—Dr. Nicolson proceeds with his Morbid Physiology of Criminals, discussing, on this occasion, prison discipline as a test of mind; and he finds a large number of prisoners who, tried by this test, he must class together as "weak-minded." In spite of his strong common sense, Dr. Nicolson at times betrays amiable leanings towards the hopeful rather than towards a perhaps unpalatable truth. We must confess ourselves among the "sceptics" from whom "the sight of a class of adult and veteran criminals plodding away at their books in the halls of a prison" "would but draw an ominous shake of the head." Granting that the book education of criminals could be carried further than there is any reason to believe possible, the assumption remains that this would tend more than any other form of discipline to make them less criminal than before—the only thing in which society has any special interest concerning them. The "weak-minded" criminal, being on the border line of sanity, is naturally a perplexing subject to the prison authorities. In dealing with him practically Dr. Nicolson's sagacity might be fully relied on, though in such expressions as "we can *punish* badness, but we must *treat* madness," there is implied a sharp line of distinction which exists only in our phraseology. Madness ought to be punished when that is the best treatment; and badness ought to be treated when treatment is the best remedy. —In an interesting paper On children fostered by wild beasts, W. W. Ireland, M.D., favours the opinion that there is not a single authentic instance of the kind.—J. H. Balfour Browne, barrister, makes a psychological and medico-legal problem of the character of Léonce Miranda, the hero of Mr. Browning's Red Cotton Night-Capt Country; and by intensely commonplace standards of measurement concludes that Léonce was mad. We sincerely hope his principles of judgment will never find place in the deliberations of actual legal tribunals. It would be a terrible prospect to think that our wills might be set aside at the instance of greedy relatives on the ground that we were somewhat "anomalous," not exactly like the herd "in our mental constitution;" "to say which," says Mr. Balfour Browne, "is only to say that a man is insane." Perhaps "all the doctrines of Rome will not make a *practical* man who *professes* its creed believe in a nowadays miracle;" but what is the worth of the statement? Strike out the word *practical*, which here means stupid, and give the sentence definite meaning by substituting *believes* for *professes*, and the proposition becomes a contradiction in terms. But to be logical may be to be insane, according to the wisdom of our practical men who profess instead of believing.—The Morisonian Lectures; The treatment of insanity, abstracted from Drs. Bucknill and Tuke's chapter on that subject; Clinical notes and cases; Notes of the quarter, and reviews, make up the number. Dr. Carpenter's "Mental Physiology" is the most important review. His defence of the old free-will doctrine is severely handled; and an attempt, not quite so successful, is made to set aside the theory of unconscious cerebration.

Journal of the Franklin Institute, July.—Among the matter contained in this number is the first instalment of an elaborate paper by Mr. J. A. Henderson, M.E., On the theory of aëro-steam engines, which, an editorial note informs us, is the first theoretical treatise on the subject that has appeared to complement the work of the late Prof. Rankine on other heat-engines. The "Principles of Shop Manipulation" is continued by Mr. J. Richards.—Chief Engineer W. H. Shock, U.S. Navy, under the head of "Strength of Materials," gives an account of a series of very carefully conducted experiments on bolts of various dimensions, under the two possible conditions—double cut and single cut—in which they might be used in connection with the bracing of boilers, and for other purposes.—There is a translation of M. Baudrimont's paper, On the tenacity of malleable metals at various temperatures.—Mr. C. J. Wister, in a paper On the moon's figure as obtained in the spectroscope, objects to Gussew's deductions from De la Rue's photographs of the moon at the extremes of her librations.—Prof. Thurston's paper On the mechanical properties of materials of construction, is continued.

The American Naturalist, August.—On the Flora of Southern Florida, by Frederick Brandel. The question considered is whether the flora of Southern Florida and the Keys is really North American or South Indian; and the conclusion reached is that it is not North American, but a link between it and that of the West Indies, and that a portion of those species which are peculiar to the northern portions of the State and the immediately adjacent region may have been derived from the south.—The Classification of the Rhynchophorous Coleoptera, by Dr. John L. Leconte.—Herbarium Cases, by Dr. C. C. Parry. A case is described, with a woodcut, specially designed for being readily moved.—A Key to the higher Algæ of the Atlantic Coast between Newfoundland and Florida, by Prof. D. S. Jordan. Part II. Rhodospermeæ. Part III. Chlorospermeæ. An etymology of names of genera is appended.—Under the section Zoology a new species of North American bird is described, named *Tringa ptilocnemis.*—In the Mammoth Cave Mr. A. S. Packard met with a new Japyx, to which he has given the specific name "*subterraneus*."

Astronomische Nachrichten, No. 2,003.—This number contains a paper by W. A. Rogers, of Harvard, On the orbit of the minor planet Felicitas (109). The elements and perturbations are given. Tacchini gives a number of observations of Coggia's comet, made with the meridian circle at Palermo. Schmidt also gives a list of observations of the position of the same comet for almost every night from May 3 to July 15. Schulhof gives several sets of elliptic elements for Coggia's comet, and it appears that it may be the same body as was seen in 1734, and so having a period of 137·1 years; or it may have a period of 12184·3 years, as shown by another set of elements. The author also adds an ephemeris from Aug. 31 to Oct. 6. D'Arrest also gives observations on this comet.—Dr. Zenker contributes a note On the light of the comet being polarised in a plane passing through the sun and comet, showing the presence of reflected sunlight.—Konkoly adds a note On the spectrum of the comet.

No. 2,004 contains a catalogue by Engelmann of the positions of fixed stars.—Pogson gives his observations on Biela's comet, made in November and December 1872. At Madras, on Nov. 2, at 17h. 31m. 1·3s. Madras mean time, its R.A. was 14h. 7m. 12·66s., and P.D. 124° 45′ 21·1″; and on Dec. 3, at 17h. 13m. 11·3s. its R.A. was 14h. 21m. 55·11s., and P.D. 124° 4′ 37·5′—Prof. Watson gives the elements and an ephemeris of Aethra (132).—Winnecke and Bruhns contribute notes on the positions of Borrelly's comet, and Dr. Holetschek has calculated the following element and ephemeris:—

T = Aug. 26.7199 Berlin time.

$$\begin{aligned} & \quad\ {}^{\circ}\quad\ {}'\quad\ {}'' \\ \pi &= 343\ 57\ 50 \\ \Omega &= 251\ 44\ 18 \\ i &= \ \ 41\ 55\ 32 \\ \text{Log. } q &= 9{\cdot}99292 \end{aligned}$$

	R.A.			D.		
	h.	m.	s.		°	′
Aug. 26	12	33	48	+	74	4·0
,, 30	11	58	19	+	74	20·7
Sept. 15	9	50	27	+	72	0·6
Oct. 1	8	22	21	+	66	17·0

Zeitschrift der Oesterreichischen Gesellschaft für Meteorologie, Aug. 1.—The first article in this number is a statement by Capt. Hoffmeyer, director of the Royal Meteorological Institute at Copenhagen, of his plan, already noticed in NATURE, vol. x. p. 146, by Mr. R. H. Scott, for publishing daily weather charts for Europe and part of the Atlantic. It is here illustrated by a specimen chart. Next follows an examination by M. Raulin of the distribution of rain in Turkey in Europe and neighbouring parts. Observations were made at Pirano and Trieste between 1787 and 1807, and since 1841; in Corfu since 1845; at Ragusa since 1851; and at other stations, of which five are outside the peninsula, in later years. All the stations are near the margin of this large region, so that the weather of the interior is not yet well known. M. Raulin divides the year into two periods, a cold one from October to March, and a warm one from April to September. The practical significance of this division is that the rainfall of the warm period satisfies the immediate wants of vegetation, while that of the cold season goes mainly to the supply of wells and rivers. The rainfall at Fiume is very large, also at Ragusa, Janina, and Corfu, but very small at Athens and Smyrna. France has been divided into districts, each having its peculiar distribution of rain through the year, and the same method is adopted here. The first district, like the plain of Northern Europe, has more rain in summer than in winter, and includes Austria, Carinthia, Styria, Hungary, Southern Russia, and the Lower Danube, to Bucharest. Laibach belongs to the second district, having a rainfall steadily increasing from winter to autumn. To the third, with a very dry winter and summer and very wet autumn, belong St. Magdalena, Trieste, and Semlin. To the fourth, with a dry summer and rainy autumn, Dalmatia, Albania, Athens, Pera, and Scutari. Among the "Kleinere Mittheilungen" we have an interesting account of the climate of the Isthmus of Tehuantepec, from a report of the United States Government Survey Expedition; a notice of Herr Mohn's results derived from observations at Novaya Zemlya and Spitzbergen, made by Tobiesen, who died while wintering at the former place; and of Mr. Draper's paper, in which he shows the fears of a supposed change of climate in the Eastern States of North America to be groundless.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, Aug. 24.—M. Bertrand in the chair. The following papers were read:—Ninth note on guano, by M. E. Chevreul.—Study of the fossil grain found in a silicified state in the coal formation of Saint-Etienne. Second part: Description of genera, by Ad. Brongniart. The author describes *Polylophospermum*, *Codonospermum*, *Stephanospermum*, and *Ætheotesta*.—Note on the Central Sea of Algeria, by M. E. Roudaire. This is a reply to objections raised by MM. Fuchs and E. Cosson.—Researches on the effects of powder in firearms, by M. E. Sarrau.—On the passivity of iron; second note, by M. A. Renard.—Memoir on vegetable protoplasm, by M. Ganeau.—On some phenomena of localisation of mineral substances in the Articulata; physiological consequences of these facts, by M. E. Heckel. The author has been feeding insects with arsenic. The metallic powder was mixed with flour, and after repeated small doses the insects (*Mantis religiosa*, *Blatta occidentalis*, and *Cerambyx heros*) were killed and various parts of the intestinal tube examined. The Malpighian tubes only gave decided indications of arsenic.—Various communications on *Phylloxera vastatrix* were received from MM. Ador, Boutin, Rommier, Morlot, Barnier, and others.—On a new formula for obtaining by successive approximations the roots of an equation of which all the roots are real, by M. Laguerre.—On the direct combination of chromic acid with wool and silk; applications to the colouring and analysis of wines, by M. C. Jacquemin. M. C. Chevreul made some remarks on the foregoing paper.—On the ureides of pyruvic acid and its brominated derivatives, by M. E. Grimaux. Pyruvic acid heated with urea gives a substance of the formula $C_{12}H_{14}N_8O_4$. When excess of urea is employed the compound $C_{10}N_{16}N_8O_7$ is produced. With excess of acid another body is obtained, of which the composition has not yet been established. A nitro-body of the formula $C_{12}H_{10}N_8O_{11}$ has been prepared from these compounds, and likewise a ureide of tribrompyruvic acid of the formula $C_{10}H_8Br_6N_8O_6$.—Analyses of various pieces of calf flesh, mutton, and pork sold in the Paris market in 1873 and 1874, by M. Ch. Mène.—Anæsthesia produced by the injection of chloral into the veins for the removal of a cancerous tumour, by M. Oré.—Application of the graphical method to the determination of the mechanism of rejection in rumination, by M. J. A. Toussaint.—Note on the physiological action of apomorphine, by M. C. David. The author has experimented on dogs, cats, pigeons, rabbits, and guinea-pigs. The influence of various reagents on the alkaloid has also been studied.—Action of the sulphydric acid of the sources of the Luchon on granitic galleries, by M. F. Garrigou.—Observations of the Perseides made at the Observatory of Toulouse on August 5, 7, 8, and 9, 1874, by M. Gruey.—Observations made at Paris of the shooting stars of the month of August 1874; progress of the phemonenon from 1837 to 1874, by M. Chapelas.

CONTENTS