THURSDAY, JUNE 9, 1870

THE NATURAL HISTORY COLLECTIONS

WE have been favoured with a copy of a memorial, drawn up as long ago as 1858, by some of the most distinguished geologists and botanists in England, on the subject of the organisation of the British Museum Natural History Collections, the removal of which was then thought to be imminent. As our readers are aware, no steps towards this removal have yet been taken, but as the subject has been brought again before Parliament, it would be well that so carefully considered a document as this appears to be should be weighed in reference to any contemplated change in the governance and disposition of our national Natural History Collections.

It is further most desirable that gentlemen interested in this subject should communicate their views upon the proposals embodied in this memorial to the public, and such will be thankfully received by the Editor of NATURE. Of the nine memorialists, four are zoologists, all happily alive. Of the five botanists, on the other hand, Mr. Bentham alone survives; it is especially desirable that the opinions of botanists on so important a question should be heard.

Copy of a Memorial addressed to the Right Honourable the Chancellor of the Exchequer.

SIR,—The necessity of the removal of the Natural History departments from the British Museum having been recently brought prominently before the public, and it being understood that the question of their reorganisation in another locality is under consideration, the undersigned zoologists and botanists, professionally or otherwise engaged in the pursuit of natural science, feel it their duty to lay before Her Majesty's Government the views they entertain as to the arrangements by which national collections in Natural History can be best adapted to the twofold object of the advancement of science, and its general diffusion among the public,—to show how far the Scientific Museums of the metropolis and its vicinity, in their present condition, answer these purposes, and to suggest such modifications or additional arrangements as appear requisite to render them more thoroughly efficient.

The Scientific Collections or Museums, whether Zoological or Botanical, required for the objects above stated, may be arranged under the following heads :—

1. A general and comprehensive Typical or Popular Museum, in which all prominent forms or types of Animals and Plants, recent or fossil, should be so displayed as to give the public an idea of the vast extent and variety of natural objects, to diffuse a general knowledge of the results obtained by science in their investigation and classification, and to serve as a general introduction to the student of Natural History.

2. A complete Scientific Museum, in which collections of all obtainable Animals and Plants and their parts, whether recent or fossil, and of a sufficient number of specimens, should be disposed conveniently for study; and to which should be exclusively attached an appropriate *library*, or collection of books and illustrations relating to science, wholly independent of any general library.

3. A comprehensive Economic Museum, in which Economic Products, whether Zoological or Botanical, with Illustrations of the processes by which they are obtained and applied to use, should be so disposed as best to assist the progress of Commerce and the Arts.

4. Collections of Living Animals and Plants, or Zoological and Botanical Gardens.

The Typical or Popular Museum, for the daily use of the general public, which might be advantageously annexed to the Scientific Museum, would require a large building, in a light, airy, and accessible situation. The collections should be displayed in spacious galleries, in glass cases, so closed as to protect them from the dirt and dust raised by the thousands who would visit them ; and sufficient room should be allowed within the cases to admit of affixing to the specimens, without confusion, their names, and such illustrations as are necessary to render them intelligible and instructive to the student and the general public.

The Economic Museums and living Collections in Botany might be quite independent of the Zoological ones.

The Scientific Museum, in Zoology as in Botany, is the most important of all. It is indispensable for the study of natural science, although not suited for public exhibition. Without it the naturalist cannot even name or arrange the materials for the Typical, Economic or Living Collections, so as to convey any useful information to the public. The specimens, though in need of the same conditions of light, airiness, &c., as, and far more numerous than, those exposed in the Typical or Popular Museum, would occupy less space; and they would require a different arrangement, in order that the specimens might, without injury, be frequently taken from their receptacles for examination. This Scientific Museum, moreover, would be useless unless an appropriate library were included in the same building.

The union of the Zoological and Botanical Society Museums in one locality is of no importance. The juxtaposition of each with its corresponding Living Collection is desirable, but not necessary; although, in the case of Botany, an extensive Herbarium and Library are indispensable appendages to the Garden and Economic Museum.

The existing Natural History Collections accessible to men of science and to the public, in or near the metropolis, are the following :—

In Botany.—The Kew Herbarium, as a scientific collection, is the finest in the world, and its importance is universally acknowledged by botanists. It has an excellent scientific library attached to it; it is admirably situated; and being in proximity with, and under the immediate control of the head of, the botanic garden, it supersedes the necessity of a separate herbarium for the use of that garden and museum. But a great part of it is not the property of the State; there is no building permanently appropriated for its accommodation, and it does not include any collection of fossil plants.

The Botanical Collection of the British Museum, consisting chiefly of the Banksian Herbarium, is important, but very imperfect. It is badly situated, on account of the dust and dirt of Great Russell Street; and the want of space in the existing buildings of the British Museum would prevent its extension, even were there an adequate advantage in maintaining, at the cost of the State, two Herbaria or Scientific Botanic Museums so near together as those of London and Kew. The British Museum also contains a valuable collection of fossil plants, but not more readily available for science than its zoological collections.

There exists no typical or popular Botanical Museum for public inspection.

The efficiency of the Botanical Gardens and Museum of Economic Botany at Kew, as now organised, and the consequent advantages to science and the public, are too generally recognised to need any comment on the part of your memorialists.

In Zoology.—The British Museum contains a magnificent collection of recent and fossil animals, the property of the State, and intended both for public exhibition and for scientific use. But there is no room for its proper display, nor for the provision of the necessary accommodation for its study—still less for the separation of a *popular typical* series for public inspection, apart from the great mass of specimens whose importance is appreciated only by professed naturalists. And, in the attempt to combine the two, the public are only dazzled and confused by the multiplicity of unexplained objects, densely crowded together on the shelves and cases; the man of science is, for three days in the week, deprived of the opportunity of real study; and the specimens themselves suffer severely from the dust and dirt of the locality, increased manifold by the tread of the crowds who pass through the galleries on public days,—the necessity of access to the specimens on other days preventing their being arranged in hermetically closed cases.

A Museum of Economic Zoology has been commenced at South Kensington.

There is an unrivalled Zoological Garden or living collection, well situated in the Regent's Park, but not the property of the State, nor receiving any other than indirect assistance, in the terms on which its site is granted.

The measures which your memorialists would respectfully urge upon the consideration of Her Majesty's Government, with a view to rendering the collections really available for the purposes for which they are intended, are the following :—

That the Zoological Collections at present existing in the British Museum be separated into two distinct collections,—the one to form a Typical or Popular Museum, the other to constitute the basis of a complete Scientific Museum.

These Museums might be lodged in one and the same building, and be under one direction, provided they were arranged in such a manner as to be separately accessible; so that the one would always be open to the public, the other to the man of science, or any person seeking for special information. This arrangement would involve no more trouble, and would be as little expensive as any other which could answer its double purpose, as the Typical or Popular Museum might at once be made almost complete, and would require but very slight, if any, additions.

In fact, the plan proposed is only a further development of the system according to which the Entomological, Conchological, and Osteological collections in the British Museum are already worked.

That an appropriate Zoological Library be attached to its Scientific Museum, totally independent of the zoological portion of the Library of the British Museum, which, in the opinion of your memorialists, is inseparable from the general library.

That the Scientific Zoological Museum and Library be placed under one head, directly responsible to one of Her Majesty's Ministers, or under an organisation similar to that which is practically found so efficient in regard to Botany.

That the Museum of Economic Zoology at South Kensington be further developed.

Your memorialists recommend that the whole of the Kew Herbarium become the property of, and be maintained by, the State, as is now the case with a portion of it—that the Banksian Herbarium and the fossil plants be transferred to it from the British Museum—and that a permanent building be provided for the accommodation at Kew of the Scientific Museum of Botany so formed.*

This consolidation of the Herbaria of Kew with those of the British Museum would afford the means of including in the Botanical Scientific Museum a geographical botanical collection for the illustration of the colonial vegetation of the British Empire, which, considering the extreme importance of vegetable products to the commerce of this country, your memorialists are convinced would be felt to be a great advantage.

Your memorialists recommend further, that in place of the Banksian Herbarium and other miscellaneous botanical collections now in the British Museum and closed to the public, a Typical or Popular Museum of Botany be formed in the same building as that proposed for the Typical or Popular Museum of Zoology, and, like it, be open daily to the public.

Such a collection would require no great space; it would be inexpensive, besides being in the highest degree instructive; and, like the Typical or Popular Zoological Collection, it would be of the greatest value to the public, and to the teachers and students of the Metropolitan Colleges.

That the Botanical Scientific Museum and its Library, the Museum of Economic Botany, and the Botanic Garden, remain, as at present, under one head, directly responsible to one of Her Majesty's Ministers.

The undersigned memorialists, consisting wholly of Zoologists and Botanists, have offered no suggestions respecting the very valuable Mineralogical Collection in the British Museum, although aware that, in case it should be resolved that the Natural History Collections generally should be removed to another locality, the disposal of the minerals also will probably come under consideration.

GEORGE BENTHAM, V.P.L.S.

W. H. HARVEY, M.D., F.R.S. & Z.S., &c., Professor of Botany, University of Dublin.

ARTHUR HENFREY, F.R.S., & L.S., &c., Professor of Botany, King's College, London.

J. S. HENSLOW, F.L.S. & G.S., Professor of Botany in the University of Cambridge.

JOHN LINDLEY, F.R.S. & L.S., Professor of Botany in University College, London.

GEORGE BUSK, F.R.S. & Z.S., Professor of Comparative Anatomy and Physiology to the Royal College of Surgeons of England.

WILLIAM B. CARPENTER, M.D., F.R.S. & Z.S., Registrar of the University of London.

CHAS. DARWIN, F.R.S., L.S. & G.S.

THOMAS HUXLEY, F.R.S., Professor of Natural History, Government School of Mines, Jermyn Street.

Nov. 18, 1858

* Since this Memorial was written great changes have taken place in the extent and position of the Botanical collections both at Kew and the British Museum, and the above recommendations would require some modification. This applies especially to the fossil plants, which it seems highly desirable to retain within an easy distance of the principal geological collections, and which might be fully illustrated by including the geographical botanical collection in the typical museum in London.—[G. B., June 1870.]

LONGEVITY IN MAN AND ANIMALS

On Comparative Longevity in Man and the Lower Animals. By E. Ray Lankester, B.A., Junior Student of Christ Church, Oxford. (London: Macmillan and Co. 1870.)

IN this interesting little essay Mr. Lankester appears to have accumulated most of the facts with which we are at present acquainted, in respect to the duration of life. He defines longevity to be the length of time during which life is exhibited in an individual; but does not, of course, apply the term individual to entire masses proceeding, as in the case of *alsinastrum* and many polypes, from a process of asexual generation; and he proceeds to point out that there is a longevity belonging to the species, and a longevity characteristic of the individual, and further, that the average longevity of a species never equals its potential longevity, since a thousand accidents happen to destroy individuals at an early period of their lives; he then distinguishes between normal and absolute potential activity, and shows that in man alone do these two nearly coincide.

The two chief circumstances which favour longevity are high individualism, for this in itself requires time; and small expenditure, the latter embracing the wear and tear put forth in the procurement of food and in the reproduction of the species. In support of these statements Mr. Lankester then adduces a considerable number of the more trustworthy observations that have been made in reference to the longevity of individuals belonging to different classes of the animal kingdom, some of which we here append. Our knowledge, it appears from these tables, of the duration of life in the lower classes, is very imperfect. Amongst the Protozoa, *Spongilla fluviatilis* dies yearly, leaving gemmules. Amongst the Cœlenterata, *Hydra viridis* reproduces sexually in autumn and then dies. An *Actinia mesembryanthemum* has been living forty-two years in an aquarium and is still alive. Amongst the Crustacea, some of the larger crabs and lobsters must have attained a great age; but Mr. Lankester has observed one species, *Cheirocephalus diaphanus*, which developes from the egg, reproduces and dies in from two to three months. In the Insecta, the imago, as a rule, lives part of a year, from six months to a few hours, dying on reproduction. The length of life of the larva varies greatly in closely allied forms, from four years or more to a week. Fleas may live as long as nine months. Scarcely any observations have been made on the length of life in the molluscoidea and mollusca. Fish appear to have great tenacity of life. Thus, the carp is believed to have attained the age of 150 years, and the pike 267 years, if a ring with the following inscription is genuine—"I am the fish which was first of all put into this lake by the hands of the Governor of the Universe, Frederick II., the 5th of October, 1230." It weighed 350lbs., and was 19 feet long. Its skeleton was exhibited at Mannheim, and it was taken at Halibrun in Suabia in 1497. Of the Amphibia, the toad lives 36 years, the frog 12 to 16, and various tortoises are inferred to be of great age from their size. Amongst birds, the parrot, goose, falcon, and raven are long-lived, the two former reaching 100 to 120 years, and the two latter exceeding 150 years. Wrens only live two or three years. Amongst mammals, the whale and elephant have the longest term of life, both probably exceeding 100 years, and possibly reaching 200; horse 25, but occasionally reaching 40 years; ox 15 to 20 years; sheep and goat 12 years; lion 20 to 50 years; cat 9 to 18 years.

Mr. Lankester comments on these tolerably well ascertained facts, and shows how they support the theory that longevity depends on the influence of generative and personal expenditure.

The last part of the work is devoted to the longevity of man, satisfactory conclusions respecting whom are almost limited to the very highly civilised nations. Mr. Lankester appears to entertain no doubt that cases have occurred where the age of one hundred has been exceeded. We have limited ourselves to a brief epitome of the contents of Mr. Lankester's work, and cordially recommend it to our readers.

H. POWER

OUR BOOK SHELF

The Fuel of the Sun. By W. Matthieu Williams, F.C.S. (London: Simpkin, Marshall, and Co.)

WE have in the work before us a proof that a very interesting and readable volume may be produced, although the hypothesis which has called it into being may be one with which we do not agree. Mr. Williams discusses at great length the very perplexing question of the sun's fuel, nevertheless we do not think that his hypothesis is an improvement upon that of Helmholtz and Thomson. But let us hear the writer speak for himself. After having come to the conclusion that an atmosphere very similar to our own, but only more attenuated, pervades all space, he supposes that the sun, in its progress through space, encounters new portions of this atmosphere, and then asks the following question—"Does there exist in the actual arrangements of the solar system any machinery for stirring in an important quantity of the new atmospheric matter and ejecting the old? If so, the maintenance of the sun's heat may be fully accounted for." The question is answered in the affirmative; the atmosphere is supposed to be the sun's fuel, and the planetary attendants of the sun are supposed to perform the duty of stokers with untiring vigilance and efficiency. The mode of action of this atmospheric fuel in furnishing heat is supposed to be as follows:—"It is evident then that the first result of the great evolution of heat from mechanical condensation of the mixed atmosphere of aqueous vapour, carbonic acid, and free oxygen and nitrogen, will be the dissociation of the water and the carbonic acid. But there must somewhere be a height at which the temperature capable of effecting dissociation terminates; where the atmosphere of elementary gases fringes upon that of combined aqueous vapour, and where these separated gases must revert into reunion with a furious chemical energy which will be manifested by violent combustion. Thus we shall have a sphere of dissociated gases and a sphere of compound vapours separated by an interlying stratum of combining gases, a spherical shell of flame, constituting exactly what solar observers have described as the 'photosphere.'" In fine, Mr. Williams' hypothesis is "a perpetual bombardment of 165 millions of millions of tons of matter per second without in any degree altering the density, the bulk, or any other element of the solar constitution."

B. STEWART

Einleitung in die Physik. Bearbeitet von Professoren G. Karsten, F. Harms, und G. Weyer.

THE volume before us is introductory to the "Allgemeine Encyclopadie der Physik," which is in course of publication, under the general editorship of Professor Karsten. The authors endeavour to supply whatever would not naturally be found or expected in the separate treatises of which the Encyclopædia is made up, which have been written independently by specialist authors from their individual points of view. They add a systematic treatment of everything that may be considered auxiliary to the entire group of the physical sciences.

Professor Harms is the author of the most important part of the work—a philosophical and historical introduction to the whole subject. The discussion ranges over three principal heads—

1. What are the proper limits, and the true relations of physical science, and what distinctions can be drawn between it and the other sciences of matter?

2. What are the methods of physical inquiry, with a critical estimate of induction, of speculation or deduction, and of the theory of cognition (Erkenntniss-theorie) which has arisen in Germany since the days of Kant. This discussion is naturally conducted both historically and metaphysically. The rapid but exhaustive reviews of Bacon,

Locke, Hume, Sir J. Herschel, Mill, and Whewell—the only authors quoted on the subject of induction, will be specially interesting to English readers.

3. The philosophical basis of the conceptions at the root of the natural sciences, with a full treatment of Idealism and Materialism, and a discussion of the differences between matter passive and without force, and matter active, bound up, that is to say, with capacities of change of state.

Professor Karsten's contributions involve an enormous amount of statistical and bibliographical labour. Fifty pages are occupied with a complete catalogue of the literature of general physics. All the encyclopædias, all the scientific periodicals and collections, all the books on the history of science, and all the handbooks and general treatises of all modern nations, are gathered together in one most useful and naturally bewildering list. Germany, Switzerland, England, the United States, France, Belgium, Holland, Denmark, Sweden and Norway, Russia, Italy, Spain and Portugal, are the countries which contribute. The order in which we have given them exhibits the civilised world from the German professor's point of view.

A second treatise by the same author deals with all of what are called the universal properties of matter, and discusses in full the problems of chemical affinity and the newest theories of atoms. Little is really carried lower than the year 1860, but references are given to all books of importance published as late as 1867.

His third treatise gives us the methods of measurement, with full descriptions of the instruments and copious tables of comparison between the units of different countries. Professor G. Weyer finally supplies a separate work on the determinations of space and time. All questions of latitude and longitude, of apparent and real magnitude, are fully discussed, and the astronomical data which affect our estimates of time are exhibited in full.

We have preferred to give our readers a simple statement of what is contained in the closely-printed volume of 900 royal octavo pages before us. Detailed criticism of five separate treatises, in the space at our disposal, is a mere impossibility. It is sufficient to say here that every subject discussed is worked out in all the painstaking and exhaustive detail to which the separate volumes of Karsten's Encyclopädie previously published, have accustomed us. Such works are of the greatest possible service to literature. They are not produced in England. Our scientific men are too busy conquering new worlds, and lecturing on the exciting incidents of every fresh conquest. There is not a man too many thus engaged, but we confess that we sometimes turn with desire to our two great mediæval universities, where the liberality of our forefathers has established hundreds of fellowships, expressly that men might have leisure to devote themselves to lifelong studies. How is it that Oxford and Cambridge leave us to sigh for impossible translations of laborious books like this, which has been sent us principally by the University of Kiel?

W. J.

De l'abus des boissons alcooliques. Par L. F. E. Bergeret. (Paris: Baillière et fils.)

THE author, who is the senior physician of the Hospital D'Arbois (Jura), has for the last thirty years devoted special attention to the effects produced by the excessive use of alcoholic liquors. Though denying that alcohol is in any form a necessary of life, he fully admits that the moderate use of alcoholic liquors has its advantages, and the work has been written chiefly with the object of affording a popular illustration of their physiological action, and of exciting a wholesome fear of their abuse. The volume contains a large amount of very interesting information, and the results of much personal observation relating to the consequences of habitual intoxication.

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 Cretaceous Epoch

THE President of the Linnean Society having been good enough to credit me, in the interesting address which has just appeared in NATURE, with the doctrine that the formation of chalk has been going on continuously over some part of the North Atlantic sea-bed from the Cretaceous epoch to the present time, I feel it due to my friend and colleague, Prof. Wyville Thomson, to disclaim most explicitly the merit of having originated this doctrine, which entirely belongs to him. I regret that the form in which it was promulgated in my report of the *Lightning* expedition should have led to this misapprehension; but that form was adopted at my friend's express desire; and I have on every occasion (as in my recent lecture at the Royal Institution) spoken of the idea as exclusively *his*. Whilst myself fully accepting and advocating it, I am the more anxious that there should be no mistake in this matter, as it seems to me that the idea is one which must exert so important an influence on the future course of geological inquiry that its introduction will be one of the landmarks in the history of the science.

The *similarity* of the globigerina-mud, at present in process of formation, to the mesozoic chalk, had been recognised by various microscopists who had studied both—as Ehrenberg, Bailey, Williamson, Huxley, Wallich, and Sorby. But no one, so far as I am aware, had ventured to advocate the *unbroken continuity* of the chalk formation throughout the Tertiary and Quaternary periods, until it was pointed out by Prof. Wyville Thomson that there is no adequate evidence of its ever having ceased, though its locality has changed.

This doctrine has received most striking confirmation from the discovery of the persistence of numerous cretaceous types, more or less modified, not merely in our own explorations, but in those carried on by the United States Coast Survey in the Gulf of Mexico. That we could not expect to find the cretaceous fauna *as a whole* in our modern chalk is evident from the considerations so admirably set forth in Mr. Bentham's address; and if the cretaceous epoch is to be limited by the duration of that particular *ensemble*, it may, of course, be affirmed to have closed long since. But if there has been a continuous production of globigerina-mud from the time when the cretaceous area of Europe was a deep-sea-bed, the elevation of that sea-bed, so as to bring a large part of it above the surface, being probably coincident with the depression of what is now the North Atlantic Basin, to which the globigerinæ then migrated, and if there be found in the newly-formed chalk so large a number of representatives of the types most characteristic of the old, as indicates the continued prevalence of the same general, physical, and biological conditions, there is, I submit, a fair justification of the assertion (the pregnant words of which are Professor Wyville Thomson's) that "we may be said to be still living in the cretaceous epoch." And, as I ventured to put forth in the lecture referred to, the *onus probandi* now seems to me to rest on those who assert that this continuity has been interrupted, and that the chalk formation now in progress is anything else than a continuation of that of which Dover Cliff is composed.

Whilst "on my legs," I would venture to call the attention of geologists to the question which has been much considered by Professor Wyville Thomson and myself, whether we are not justified, by the probabilities of the case, in carrying *backwards* the continuity of the accumulation of Foraminiferal mud on the deep-sea bed, into geological epochs far more remote; since there must have been deep seas in all periods, and the changes which modified the climate and depth of the sea-bottom must have been for the most part sufficiently gradual to admit of the migration of animals to whose continued existence in the same locality those conditions were no longer favourable. It is a most interesting confirmation of the view we are disposed to entertain on this point, that, as I have recently learned from Sir William Logan, coccoliths and coccospheres have been discovered in some of the most ancient Palæozoic limestones of North America.

WILLIAM B. CARPENTER

[See also Prof. Gümbel's letter to Prof. Huxley on this subject in NATURE, vol. I. p. 657.—*Ed.*]

The Aye Aye

I FIRST saw the quotation from the *Pall Mall Gazette* in the columns of NATURE concerning the long finger nail of the Aye Aye, and the "exquisite argument" founded by Professor Owen upon it.

As a simple matter of fact, allow me to state that I kept a living Aye Aye (now preserved in the British Museum) in a large cage in the Mauritius, and as its food I gave it the maggot that infested branches of a species of Acacia. The animal used to spend its evenings in feeding, as follows. It listened attentively at the branches, tapping occasionally the most perforated parts; it then tore off pieces of the wood around the maggot hole, inserting the peculiar long finger as a probe from time to time, and ended by extracting the maggot by means of this long finger and its strong rodent teeth.

I have seen the operation scores of times.

Athenæum, May 28 HUMPHRY SANDWITH

Carp and Toads

IN the last number of NATURE you give an abstract of a paper by M. Duchemin on the destruction of carp by toads. The fact that carp are so destroyed is, or was, well known. Walton, in his "Complete Angler," says:—"And I have known of one (person) that has almost watched the pond, and at the fishing of the pond, found of seventy or eighty large carps not above five or six; and that he had foreborne longer to fish the said pond, but that he saw on a hot day in summer, a large carp swim near the top of the water with a frog upon his head; and that he, upon that occasion, caused his pond to be let dry; and I say, of seventy or eighty carps, only found five or six in the said pond, and those very sick and lean, and with every one a frog sticking so fast on the head of the said carps, that the frog would not be got oft without extreme force or killing." Walton also mentions that pike are attacked and destroyed in the same manner. Walton wrote his "Complete Angler, or Contemplative Man's Recreation" in 1653. The confusion between frogs and toads was one likely to be made at a time when natural history was so little studied. In all other respects Walton's account agrees with M. Duchemin's. C. H. G.

Anticipated Destruction of the Cheesewring

I AM sure your readers will pardon me for drawing their attention to the very perilous situation of that remarkable pile of rocks, six or seven miles north of the town of Liskeard, in Cornwall, and known as the Wring-cheese or Cheesewring. Wilkie Collins, in his "Rambles beyond Railways," thus describes the general appearance of this natural curiosity:—"If a man dreamt of a great pile of stones in a nightmare, he would dream of such a pile as the Cheesewring. All the heaviest and largest of the seven thick slabs of which it is composed are at the top; all the lightest and smallest at the bottom. It rises perpendicularly to a height of thirty-two feet, without lateral support of any kind. The fifth and six rocks are of immense size and thickness, and overhang fearfully all round the four lower rocks which support them. All are perfectly irregular; the projections of one do not fit into the interstices of another; they are heaped up loosely in their extraordinary top-heavy form on slanting ground, half way down a steep hill." Of late years this hill has been so extensively quarried for granite that the workmen are now within a few paces of the Cheesewring itself. When a lease of the ground was first granted, it was stipulated that no stone should be removed within a certain distance of this well-known landmark, so as to prevent any possibility of its being destroyed. Now, however, the boundaries of the quarry have been so extended that powerful blasting operations are continually being carried on within a short distance of it, not without very great risk to the whole structure. In fact, it is on the eve of being destroyed, unless a vigorous and influential attempt is made to save it. Six months ago the Royal Institution of Cornwall sent a deputation of its members to report on the exact state of the Cheesewring; and although a memorial was addressed to the authorities of the Duchy of Cornwall, the owners of the property, praying that some means be adopted for the preservation of this extraordinary geological formation, no satisfactory reply has been hitherto received. Since, therefore, *local* influence appears to be of little or no avail, it is to be hoped that the matter will be taken up by those who are especially interested in the preservation of remarkable objects of this kind. The untimely fate of the great Tolmên last year should be remembered; and measures immediately adopted to avoid a repetition of a similar catastrophe within so short a period. Whatever is done should be begun speedily, or in the meantime the impending calamity may actually take place. E. H. W. DUNKIN

Greenwich

Left-handedness

I HAD been intending, if no one had anticipated me, to suggest what I have little doubt is the true explanation of the destruction of carp by toads. I see in p 59 of NATURE for May 19, that Mr. Wade hints at this solution of the difficulty.

Let me take this opportunity of saying a word on Left-handedness. The late eminent anatomist, Professor Gratiolet, maintained that in the early stages of fœtal development, the anterior and middle lobes of the brain on the left side were in a more advanced condition than those on the right side, the balance being maintained by an opposite condition of the posterior lobes. Hence, in consequence of the well-known decussation of the nerve-roots, the right side of the body—so far as it is influenced by brain force—would, in early fœtal life, be better supplied with nervous power than the left side; and movements of the right arm would precede and be more perfect than those of the left. If Gratiolet's view is regarded as established (it has, I believe, been disputed), we have a physiological clue to the explanation of Right-handedness. In rare cases the development of the interior and middle lobes of the right side may precede that of the left; we shall then have Left-handedness.

May 21 M.A., CANT.

IN answer to Mr. Meyer's letter on this subject, I can only say that the question of the dependence of Left-handedness on abnormality of the subclavian artery cannot be settled by the authority of even so eminent an anatomist as Professor Hyrtl, when it is adduced against the facts I mentioned in my last note. That there have been cases like that quoted from Dr. Buchanan of transposition of viscera and left-handedness occurring in the same individual, and that they will be observed again, I do not doubt. Otherwise the conclusion would be that such abnormalities prevent left-handedness, which no one pretends. What I venture to think the cases already on record prove is, that the one condition has no relation to the other in either causing or preventing it.

With regard to the origin of the right subclavian direct from the end of the transverse aorta, though I have met with several cases of this variation, they were in subjects whose history was unknown; while, unlike complete transposition of the viscera, the condition cannot of course be recognised during life. But at the last meeting of the Pathological Society, a case of aneurism with this abnormality was brought forward; and, by the kindness of Dr. Peacock, who exhibited it, I am informed that the patient was undoubtedly right-handed during life.

Even apart from facts like this, one would scarcely expect to find the explanation of left-handedness in abnormalities affecting only the upper extremity. The condition is one of the eye, the leg, and the whole body. This one discovers in using the microscope, shooting, and batting; and the facts of aphasia appear to show that we are all, to use M. Broca's phrase, *gauchers de cerveau.* In fact, we must first explain normal right-handedness, and that not only as seen in man, but also in the paces of the horse, and in many other alternate or otherwise unsymmetrical movements of animals.

The only anatomical explanation of right-handedness I have met with is the unconfirmed statement of Gratiolet that the left half of the brain is developed more rapidly than the right. But even if true, this would only throw back the question to the origin of such a condition of the brain.

That the primitive habit of bilaterally symmetrical animals is to use right and left organs equally, seems probable, and also that when more complicated movements have to be performed, one limb alone must be chosen and so in time become more skilful. This preference may be transmitted hereditarily. But, granting in addition that the convenience of the community requires that all its members should select the same limb, it yet is not clear by what process of natural selection the right one has come to be universally preferred to the left—how, in fact, except from practice, right-handedness has come to be dexterity, and left-handedness *gaucherie.*

This question of the reason of symmetrical organs having different functions might perhaps be elucidated by a study of the conditions under which deviation from bilateral symmetry occurs in the *structures* of animal life, even among the highest arthropods and vertebrates. P. S.

May 23

The "Chromatic Octave"

RENEWED attention to the "chromatic octave" tempts me to suggest an experiment. There used to be a gentleman, Smith, I believe, by name, who refuted the undulatory theory by means of a disc, divided into black and white sections, which he whirled with very high velocities, producing colours (so the *Times* positively stated) varying according to the velocities. It is plain that such a result might on the contrary confirm the theory, if, for instance, the disc were divided into 400 black and 400 white sectors, and whirled at the rate of one or two million million times a second. It is also plain that Mr. Smith, in the words of an authority who has been quoted in your columns for weightier judgments, would have blown his disc into smoke first. But *once* a second is only 40 octaves below a million million times; and it is just possible that something practicable between the two might throw light on the "chromatic octave," among other things. There are some obvious objections: the question is, whether they make it worth while to repeat Mr. Smith's experiments.

I have just received the number of *Poggendorff* (Oct. 1867, CXXXI.) containing Professor Listing's paper (p. 564), referred to by Mr. Barrett in NATURE for Jan. 13 and March 31. I did not expect it would support Mr. Barrett, but I was surprised to find how directly it contradicts him. "Listing," he says, "concludes that, although physiologically and psychologically there may be differences, yet there is an indisputable *physical* basis for the analogy between tones and colours." I had myself, at the end of my remarks in NATURE for Feb. 3, admitted the "physical basis" in that sense "of the word *physical* which excludes biological relations," and the remark is too trivial to have formed Listing's *conclusion*. It forms part of a sentence in the first page of the paper. "The analogy between tones and colours, which has often been pursued with excessive predilection, and which certainly has an indisputable physical basis, has against it numerous points of disagreement (*Discongruenzen*), even now not in general sufficiently attended to, which depend rather on the physiological and psychological aspect of the phenomena." In the same page of the number it clearly appears how much is meant by the *physical basis*. "Physically," he says, "it is the period of vibration that determines both tone and colour; but the physiological effects stand in very different relations to the common element in the two cases." He proceeds to show, as correctly explained by Mr. Barrett, that the several colours divide the spectrum in an arithmetical progression of their rapidities of vibration; and at the end of the paper, contrasting this phenomenon with the geometrical progression of a uniform series of tones, he says: "This point of disagreement, a very vital one in my opinion, between the scales of tone and colour, may be briefly stated thus: *In the musical scale* (chromatic and with equal temperament*) *the logarithms of the tones are in arithmetical progression, in the scale of colour the colours themselves.*" That this should mean what Mr. Barrett understands it to mean, you must read *agreement* for *disagreement*.

Of the reality of Listing's result, I suppose there can hardly be any doubt; and I am glad that Mr. Barrett has corrected my suggestion that it probably represented a conventional demarcation. There does seem something arbitrary in the number of divisions made, but their positions represent a mean among the impressions of different observers as to the boundaries between colours answering to the names assigned; and the accuracy of these determinations may be fairly estimated by likening them to the case of a person who, having to divide a space of nine inches into nine equal parts, should be correct *as often as not* within one 24th of an inch. But the most important point is this, that the observers would not be aided, but must rather have been distracted, by the spaces actually occupied by the colours in the spectrum. For the observations were made on two different spectra, the irregular one obtained from the prism, and the diffraction spectrum in which the colours proceed uniformly by wave-lengths; and the result was a division into equal spaces, not on either of these visible spectra, but on the ideal spectrum, which should proceed uniformly by rapidities of vibration. It would have been in the spirit of good German precedents if we had been given some measure of the variation between different observers.

It must be confessed that all this is damaging to the theory of a "chromatic octave," essentially a theory of geometrical progression. Still more obviously damaging is the fact that "lavender" would be the octave above something so unlike it as "brown," or "brown" and "red."

Mr. Murphy's argument (NATURE, April 28) seems to assume that complementary wave-lengths must be in *some* constant ratio. His theory is, at any rate, inconsistent with his author's; for primary red and blue would be nearly complementary, so that "true white" could not be produced by any mere preponderance of blue, and would be white only to the green-blind.

C. J. MONRO

* If this is what is meant by "chromatischen gleichschwebenden."

IN Mr. Murphy's interesting letter in No. 26 of NATURE, April 28, 1870, he assumes that the number expressing the frequency of vibration producing a colour complementary to another, is the geometrical mean between the frequency of vibrations corresponding to that other, and its double. By this means he does not get colours complementary from sunlight. Thus red and bluish green (whose numbers are respectively 36·4, 48·3) are not complementary on his hypothesis; which would require the number for bluish green to be 51·47. So for yellow and indigo, the numbers are 41·4, 54·7, but should be 41·4, 58·4. This he attributes to the impurity of the solar spectrum. There seems as much reason, however, for taking the *harmonic mean* instead of the geometric; and, on this supposition (the harmonic mean between two quantities being twice their product divided by their sum), the numbers would be red, 36·4; bluish-green, 48·5; yellow, 41·4; indigo, 55·2. The second and fourth, 48·5, 55·2, are not very different from 48·3, 54·7. Taking then a colour twice over in the spectrum and its intermediate complementary, the relation between the three would be that of a musical note, its fifth and its octave.

Little Wratting, Suffolk, May 16 M. A.

The Colour of the Moon by Day and by Night

CAN any of your readers give me a full explanation of the reason why the moon looks white by day and yellow by night? The light that proceeds from it is of course the same at both periods; whence does the change in appearance arise? Two reasons occur at first thought, but they do not completely satisfy the many requirements of the problem. The one is, that the light, being really somewhat yellow, though less so than it often appears to be, passes in daytime through an atmosphere made blue by the solar rays, and the blue and yellow neutralising each other, the moon looks white. The other reason is, that as the evening closes in, the twilight becomes purple, and the moon being but moderately yellow in itself, looks more intensely yellow by contrast. All this is correct so far as it goes; but I do not see why the moon should often look extremely yellow in the middle of the night after twilight has quite disappeared. Does it show that the light, one knows not exactly whence it comes, which is found even on clouded and moonless nights, is purple? There are some grounds for this hypothesis, because the moon almost always, as I have been assured by a practical astronomer, looks comparatively white through a telescope, which of course isolates the field of vision. Also, it seems to me that the street gaslights are just as yellow at midnight as in twilight; the stars, also, commonly look yellow all the night through. It is strange that the very frequent and beautiful phenomenon of the white moon of the day suddenly turning yellow as the evening closes in, should not have long since attracted scientific comment.

F. G.

What is a Boulder?

A CORRESPONDENT in your journal of the 26th of May inquires about the size of boulders, and states that he cannot find any definition of the word which gives a notion of its size accurate enough for scientific purposes.

There are several definitions of boulder-stones given by geologists and others, which determine their size within tolerably narrow limits.

Dr. Page defines boulders as being "any rounded or water-worn *blocks* of stone, which would not, from their size, be regarded as pebbles or gravel;" or generally, "any *large* water-worn and smoothed blocks, found embedded in the clays and gravels of the drift formation." The maximum size he assigns to gravel is the size of a hen's egg, and pebbles may be ten times as large as this, so that the smallest boulder may be regarded as being of more than a foot diameter. Again, the same author describes boulders as being "often of great size, and weighing many tons."

Dr. Nuttall, in his Pronouncing Dictonary, defines boulders as being "large round pebbles;" and it would not often be possible to put even a small pebble in one's waistcoat pocket, and it must be a high wind that could blow a large pebble for any distance.

Chambers gives, as the etymology of boulder, or bowlder, as it is sometimes spelt, the verb "to bowl," and so states that it means a rolling stone, and, afterwards, one rounded by water. Now boulders are very often found separate, and at some distance from each other, and how could they have been rounded separately, without friction with each other, if they had been otherwise than large?

These arguments may be enough to show that boulders cannot be very small, and, in fact, may vary from stones 18 inches diameter, or thereabouts, to immense rocks of 10 feet diameter, "weighing many tons."

Christ's Hospital, May 27 J. W. CRAWLEY

Scandinavian Skulls

MR. G. STRACHEY, commenting in your columns on a recent lecture of Professor Huxley's, makes the following statements:—

"According to the highest Copenhagen authorities, there is no ground whatever for the assertion that modern Scandinavian skulls are of the long type. It is equally incorrect to say that Scandinavians are fair-haired and blue-eyed."

Without at all wishing to endorse Prof. Huxley's views on British ethnology, which I regard as rather bold than sound, I may be allowed to express my astonishment at the statements just quoted.

The skull measurements of Retzius, Van der Hoeven, and Barnard Davis all exhibit the Swedes as a more or less dolichokephalous people. My own measurements of the living head (Anthrop. Memoirs, ii. 351, and iii. 378) tend to controvert both Mr. Strachey's facts and Prof. Huxley's theory. Lastly, I should be glad to know where in the world fair hair and blue (or light) eyes are to be found, if not in Scandinavia. They are not universal there, nor any where else, but I do not think they are anywhere more common, except possibly in Lithuania or Esthonia.

Clifton, June 7 JOHN BEDDOE

Formation of Caverns

IN NATURE of 21st April, Mr. W. Boyd Dawkins, writing of caves in Yorkshire, tells us:—"All have been at one time or other subterranean water-courses." In the *Popular Science Review* for October 1869, the same gentleman writes:—"The ceiling, at the time of its deposition, must have been supported by a layer of cave earth." With your permission I will explain the character of a phenomenon, which I have published in my "New Pages of Natural History" (Newby, 1868), which may suggest to Mr. Dawkins the manner in which the caves at Streddle and Kent's Hole were formed. In the province of Poona, Bombay, the Ghar Nuddee (white river) has, in the mountains near its source, several lime formations spanning its level. In the dry season the river runs below as a subterranean stream; in the Monsoon it runs over this sheet of lime, which varies in thickness from a few inches to two feet along the centre or crown of the cavity, increasing in thickness towards the sides; there are several fissures on the surface; the hole at the upper end is smaller than that at the lower; the whole formation is in layers, and is due to water containing vast quantities of lime in solution. Originally there was a dip or hollow place on the spot, which gradually filled with all sorts of materials, till they grew nearly to the level of the dry season stream. In this condition the first thin sheet of lime was deposited on them, till by successive seasons the formation grew into a substantial covering composed of yearly layers. As there were perishable materials down below, they subsided, so that the lime covering, having in places no support, gave way to the force of the water, or to the weight of boulders hurled upon it, and water found admission between the lime sheet and the buried materials; as these were more moveable than the covering, they gradually washed out, and left the river to resume its ancient course beneath a covering of its own formation. Of course subterranean streams may excavate caverns; but if these streams owe their origin to percolation only, no large organic remains will be found in the caverns. If these are formed after the fashion of the Ghara caves, some organic remains may still be found, though waters have washed through them for years. When these lime formations withstand all the forces to which they are subjected, and have grown into large hills, the materials which formed the mould of the cave are still *in situ*, the perishable portions have changed into an oily, loamy soil; but bones, pebbles, and other materials are there mixed up with the stalagmite, which originally forming upon the surface of these materials, sunk and broke up as the supports failed, and remain, as Mr. Dawkins found them in Kent's Hole. H. P. MALET

The Anglo-Saxon Conquest

I SAW, with perfect clearness, the grounds upon which Prof. Rolleston now rests his defence. It is assumed that we can identify, to a nicety, the precise period of an interment, having no recording date, that took place fourteen or fifteen hundred years ago. I need not contest or discuss this point, but I am at liberty to doubt it.

The Britons petitioned Rome for assistance in A.D. 446. Cerdic landed in 494; the *groans of the Britons* were a piteous wail of "old men and children." The interval, taking it at three generations, passed in constant warfare, would not restore the balance of youth removed by conscription prior to A.D. 418, which circumstance therefore remains in part as an element to affect the general average of assumed longevity in the time of Cerdic. I urge this, with all respect, not as affecting, in any way, the real facts discovered or expounded by Prof. Rolleston, with whose address I was very much gratified; but only as affecting certain conclusions sought to be founded thereon. A. HALL

Curious Effect of the Words "Carmine" and "Germinal Matter"

DURING the twelve or thirteen years that Mr. Huxley has performed the duties of Examiner at the University of London, it appears that he has been much disturbed by the frequent use the candidates have made of the words, "cell," "germinal matter," and "carmine." He says: "*I declare to you I believe it will take me two years at least of absolute rest from the business of an examiner to hear either of the words without a sort of inward shudder.*" * This is surely a very remarkable declaration on the part of the examiner, since it is extremely doubtful if hearing these words ever made anyone else shudder, and if any other words written or spoken ever produced an effect so exceptional upon Mr. Huxley. LIONEL BEALE

Holly Berries

WILL some learned botanist, or Darwinian theorist, kindly inform me through the columns of NATURE why some holly berries appear obnoxious to birds? This is a great holly neighbourhood, and there are at present several trees actually loaded with ripe berries; the ground is also thickly strewed with berries beneath the trees, and yet not a single species of bird appears to eat them. Last winter the holly trees bore an abundance of berries, but the majority of the trees were stripped by the migratory *Turdi*, &c., as early as the beginning of February. I presume, in the "struggle for existence" these berries, obnoxious to birds, will stand a better chance of propagating and increasing that peculiar variety, and in course of time raise a distinct and well marked species.

Thruxton, May 23 HENRY REEKS

Origin of Languages

IT seems to me that your correspondents, Mr. Taylor and S. J., have discovered merely imaginary differences between the origin of species and the origin of languages. Mr. Taylor sees an essential difference in the fact that in the one case the process "is carried on by the countless efforts of *rational* beings," whilst in the other case there is "*reasonless* variation and selection."

* Address on Medical Education," *Lancet*, June 4th, 1870.

But I think the analogy still holds good, inasmuch as the "gradual variation, &c., of a few primary sounds," is not the result of an *intention* to originate a new language, any more than the origination of a new species of animal by natural selection is *intentional* on the part of the animals engaged in the struggle for life.

S. J.'s "impression is that the dialects which run wild are much more variable than those under man's care, which is the reverse of the case with wild and domestic animals and plants." But it must be remembered that it is the object of the "host of schoolmasters, lexicons, and grammars," who "watch over the Queen's English" to make that language uniform, to check its variations. If breeders of cattle made an effort to obtain perfect uniformity in a certain species of animal, I have no doubt that the wild herds of that animal, if allowed to exist under different conditions, would show much more variation "than those under man's care." But I question whether, in spite of the conservative influences of "schoolmasters, lexicons, and grammars" languages do not undergo as much variation through the *artificial selection* of writers, whose aim it is to make language more and more expressive, as animals and plants undergo through the artificial selection of breeders and agriculturists.

King's Lynn, May 28 ARTHUR RANSOM

STONE IMPLEMENTS FROM BURMA

SOME notes on the stone implements of Burma, by W. Theobald, jun., of the Geological Survey of India, contained in the number of the Proceedings of the Asiatic Society of Bengal for July, 1869, seem worthy of notice in these pages. "The implements are curious as differing in form and type, not only from anything found in India, but from anything hitherto described from any part of Europe, though any implement yet found in India has its precise analogue in Europe." According to Mr. Theobald, not only is the form but the material remarkable, as these Burmese implements are fashioned either of basalt or some schistose rock, quite unlike anything to be met with in the district where the implements themselves occur; a fact which he thinks points to their having been brought down from Upper Burma (where such implements are common) by the original settlers of the country.

That curious superstition which prevails over almost the whole of the globe, and connects the origin of these stone implements with the "thunderbolt," is found also in Burma. They are there called *mo-gio*, or thunderbolts, and are believed to accompany the lightning. If a flash of lightning is seen to strike the ground, and an earthen vessel is inverted over the spot, in the course of a year or so the *mo-gio* will be found in it, having worked its way back again to the surface by its own recoil.

The classical, or rather Plinian, view of this subject has been well given by Bishop Marbodæus, who wrote his Book "De Gemmis" early in the 12th century, and who thus speaks of the Ceraunius:—

> Ventorum rabie cum turbidus æstuat aër,
> Cum tonat horrendum, cum fulgurat igneus æther,
> Nubibus elisus cælo cadit ille lapillus,
> Cujus apud Græcos extat de fulmine nomen.

Its virtues were great in Europe as preserving from injury by lightning or shipwreck, and they had even aggressive as well as prophylactic powers, enabling the possessors to take beleaguered cities and to destroy hostile fleets.

In Burma they are also highly valued, but are put to crucial tests to prove them to be the genuine article, before a purchaser pins his faith to them. One test is that if wrapped in cloth and fired at with a gun, no effect will be produced either on the cloth or its contents, however near the aim may be taken, and it is from its presence producing invulnerability in its wearer that the *mo-gio* is mainly valued. It may be observed that it is not stated whether it is the seller who is entrusted to take aim with the gun. Another test of its celestial origin is placing it on a mat with a quantity of rice. If genuine, no fowl or other creature will venture near it. Again, a plaintain tree cut down with it ought to die, and not, as is usual, to send up a new shoot. If genuine, it preserves from fire, but it has also great medicinal virtues, and a small chip administered internally is considered a cure for inflammation of the liver or other internal organs, and is also a specific for ophthalmia. The virtues of stone axes in Germany, as summarised by Preusker in his "Blicke in die vaterländische Vorzeit," are curiously similar to those of the Burmese *mo-gios*. They preserve the house in which they are from lightning, they perspire when a storm is approaching, they are good for diseases of man and beast, they increase the milk of cows, they assist the birth of children, and powder scraped from them may be taken with advantage internally as a remedy for certain diseases.

The types of these Burmese instruments described by Mr. Theobald seem susceptible of arrangement under four heads.

1. "A rough, stout, wedge-shaped instrument," which, to judge from the figure, resembles closely the better finished specimens of flint hatchets, of the type which occurs in the Danish Kjökkenmöddings.

This form is very rare.

2. A hatchet with flat sides converging towards the base, which is square, and with a segmental edge, much like a common German form.

This type is common.

3. A long adze, with square, slightly converging sides, and a bevelled segmental edge, in character much resembling some of the implements discovered in Java, Borneo, and Sumatra, and also a New Zealand form; and—

4. Implements of the same character so far as the edge and sides are concerned, but having the butt end reduced in width so as to produce a square shoulder on each side of the blade. In some this reduction in width extends more than half the length of the blade, so as to produce a T-shaped form. These shorter specimens are the most common. This form appears to be peculiar to Burma. One of them has been figured by the Society of Antiquaries (Proc. N.S. vol. ii. 96).

In some cases the lashings used to fasten them to their hafts have left traces on the stone. The implements are usually picked up on the surface of the hills, in the fields, or clearings made for cultivation, and not in the plains. Mr. Theobald seems inclined to doubt whether, without the use of iron also, those who made these implements could have effected clearances in the gigantic forests of Pegu; but it may be urged against this view that by calling in the aid of fire the efficiency of such tools is almost as great as if they had been formed of metal, and it is difficult to conceive a people in possession either of bronze or iron bestowing the necessary time and trouble on the fashioning of stone tools, when those of metal were at their command, which, whether fire were employed in the clearance or no, were for general purposes so much more effective. If the makers of these stone tools had been in possession of other means for clearing the hill sides, then Mr. Theobald would be inclined to regard the stone relics as agricultural implements used in hand agriculture, at the end of sticks, as a kind of spade, to form the shallow holes for the cultivation of "hill rice." If not explained in this manner, he argues, we must regard them as weapons of the chase and war, though this use is, he thinks, negatived by their thoroughly inefficient character for such purposes. To this may be objected, first, that the material of which they are usually formed is basalt, a stone constantly used as a material for cutting tools; secondly, that the presence of the square shoulders, so like those on the horn sockets for hatchets of the Swiss Lake-dwellers, seems to testify to the tools having been used as adzes or axes, or possibly chisels; and thirdly, that if they had been required merely for hoeing or digging, the trouble of grinding and polishing might and would have been saved. We will only add that the paper is a valuable contribution to our knowledge of Eastern Neolithic implements, and that our present remarks are, like those of Mr. Theobald, "merely tentative, and designed to elicit additional information."

J. EVANS

M. FIZEAU'S EXPERIMENTS ON "NEWTON'S RINGS"

A COMPARISON of the values given by Professor Ångström (in his magnificent *Recherches sur le spectre solaire*) for the wave-lengths corresponding to the two principal components of Fraunhofer's line D, with some observations made eight or nine years ago by M. Fizeau, not only reveals a remarkable agreement between the results of these two distinguished investigators, but yields one of the most striking confirmations of the truth of the undulatory theory of light that recent optical research has afforded.

The experiments of M. Fizeau to which we refer were, essentially, the following. He produced the phenomenon of "Newton's rings," by laying a convex lens of very long focus upon a piece of glass with plane parallel surfaces, and illuminating the combination by the yellow flame of spirit of wine containing a little common salt. The lens was so arranged that it could either be made to touch the glass plate or be separated a short distance from it, its position being regulated by a micrometer screw. On gradually separating the lens from the glass plate, the rings were seen to contract and move in towards the centre of the lens, where they successively disappeared, while their place was supplied by fresh rings which made their appearance at the circumference of the lens. So far, all was in accordance with what was well-known before. But M. Fizeau found that when the phenomenon was observed with sufficient care, nearly 500 rings could be counted, flowing inwards one after another, but that after about this number the rings ceased to be visible, the surface of the glass showing a nearly uniform illumination all over instead of a sharply defined alternation of light and dark bands. When, however, the distance between the lens and the glass plate was further increased the rings re-appeared, getting gradually more and more distinct, until when nearly another 500 had passed they had become as sharp as at first; but a still further increase of distance caused them again to become confused, and they ceased a second time to be discernible at about the 1,500th. With a still greater separation of the glasses, however, they reappeared again, and became quite sharp at about the 2,000th, after which they for a third time got gradually confused and became indistinguishable at about the 2,500th.

So the phenomenon went on, the stream of rings inwards towards the centre of the lens, followed by fresh ones from the circumference, continuing as the lens was moved further and further away from the glass plate; but the succession of rings was not uniform, but broken up into batches of about 1,000 each, separated by short intervals of confusion in the way that has been described. The rings did not finally cease to be distinguishable until *fifty-two* such batches had been counted, and the two glasses were at a distance of about fifteen millimetres (more than half an inch) from each other.

This remarkable phenomenon of the alternate periods of distinctness and confusion of the rings is easily explained, as M. Fizeau points out, when we remember that the light employed was not strictly homogeneous, but consisted of two portions of nearly, but not quite, equal degrees of refrangibility. If either of these two constituent parts of the light had been used by itself, it would have produced a set of rings, but the rings of one set would have been a very little larger than the corresponding rings of the other. Hence if the two sets of rings are put together (as they were in Fizeau's experiment), they will nearly, but not quite, fit each other. If we examine a few rings at the centre, when the two glasses are in contact, they will appear to coincide precisely; but if they are traced to a sufficient distance from the centre, the coincidence is seen not to be exact. For although the *twentieth* ring (say) of one set is not perceptibly bigger than the twentieth ring of the other set, the *five-hundredth* of one set is perceptibly bigger than the five-hundredth of the other, and, when put upon it, falls almost exactly halfway between the five-hundredth and five-hundred-and-first of this set. Consequently, at about this part of the phenomenon, the bright spaces of one set of rings will occupy the same position as the dark spaces of the other set, and they will mutually obliterate each other. But since the *thousandth* ring of one set is nearly the same size as the thousand-and-first of the other, the two sets of rings will appear to fit each other again about this point; the *fifteen-hundredth* of the first set, however, is larger than the fifteen-hundred-and-first of the second set, but not so large as the fifteen-hundred-and-second; and hence, at about the position of this ring, the rings of the two sets will overlap each other, and mutually efface each other's outlines. And, carrying such considerations further, it is evident that the apparent coincidence and overlapping of the two systems of rings would recur alternately at regular intervals.

In order to simplify this explanation, we have tacitly assumed the lens to be so large that several thousand rings could be seen between its centre and its circumference. Practically, this would be impossible; but, by gradually separating the lens from the plane glass, we can, as it were, draw in towards the middle the rings which, with a larger lens, would be formed at a great distance from the centre.

Now, according to the explanation which the undulatory theory gives of the formation of "Newton's rings," the distance by which the interval between the glasses must be increased, in order that a given ring may come into the position previously occupied by the next smaller ring, must be equal to half the wave-length of the kind of light used for the experiment; and the distance of 0·28945 millimetres, through which, as M. Fizeau found by actual measurement, it was necessary to vary the space between the glasses, in order to make the rings go through one of the recurrent periods above described, that is to say, pass from sharpness to confusion and become sharp again, must contain just one more half wave-length of one portion of the light by which the rings were formed than it does of the other.

This brings us to the point of contact between M. Fizeau's observations and those of Prof. Ångström, to which we referred at the beginning. According to the latter, the wave-lengths of the two principal constituents of the light emitted by a flame containing the vapour of sodium (such as the flame employed by M. Fizeau) are respectively—

Millimetres
0·000589513
and 0·000588912.

Now, if we divide 0·28945 by half the former of these numbers, we get as the quotient 982; and if we divide it by half the second, we get as the quotient 983. That is to say, we find, precisely as the undulatory theory requires, that the distance measured by M. Fizeau contains exactly one more half wave-length of the more refrangible constituent of the light of a sodium-flame than it does of the less refrangible part. And, moreover, if we calculate, from Ångström's determination of the wave-lengths, the number of rings which must intervene between the positions of greatest confusion and greatest distinctness, we find 491 of the one set and 491½ of the other, which agrees entirely with M. Fizeau's estimated round number 500.

G. C. FOSTER

THE NEW AUSTRALIAN MUD-FISH

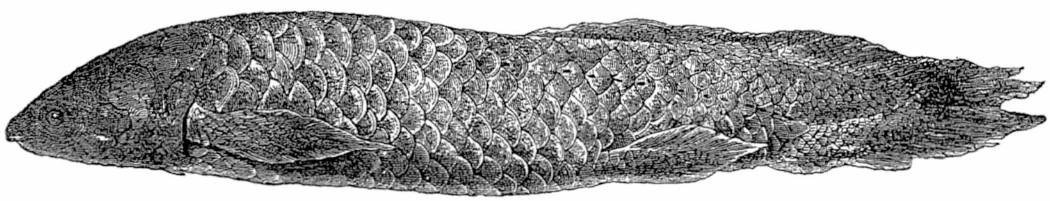
IN his well-known essay upon the arrangement of the extinct fishes of the Devonian epoch, published in the Memoirs of the Geological Survey* Professor Huxley, after showing that the *Polypterus* of the African rivers is probably the descendant of the Crossopterygian Ganoids with rhomboidal scales, continues as follows :—

"It is singular that while the line of the rhombiferous *Crossopterygidæ* has so distinct a modern representative, the cycliferous *Crossopterygidæ* seem to have died and left no issue at the end of the Tertiary epoch. But, without wishing to lay too much stress upon the fact, I may draw attention to the many and singular relations which obtain between that wonderful and apparently isolated fish, *Lepidosiren*, sole member of its order, and the cycloid glyptodipterine, ctenodipterine, phaneropleurine, and cœlacanth *Crossopterygidæ*. *Lepidosiren* is in fact the only existing fish whose pectoral and ventral members have a structure analogous to that of the acutely lobate paired fins of *Holoptychius*, of *Dipterus*, or of *Phaneropleuron*, though the fin-rays and surface-scales are still less developed in the modern than in the ancient fish. The endoskeleton of *Lepidosiren*, again, is as nearly as possible in the same condition as that of *Phaneropleuron*, and is more nearly similar to the skeleton of the Cœlacanths than that of any other recent fish; while perhaps it is not stretching the search for analogies too far to discover in the stiff-walled lungs of *Lepidosiren*, a structure more nearly representing the ossified air bladder of the Cœlacanths than any with which we are at present acquainted, among recent or fossil fishes. Furthermore, *Lepidosiren* is the only fish whose teeth are comparable in form and arrangement to those of *Dipterus*. Though *Lepidosiren* may not be included among the Crossopterygidæ, nor even in the order of the Ganoids, the relations just pointed out are not the less distinct; and perhaps they gain in interest when we reflect that while *Polypterus*, the modern representative of the rhombiferous Crossopterygidæ, is that fish which has the most completely lung-like of all air-bladders; *Lepidosiren*, which has been just shown to be, if not the modern representative of the cycliferous Crossopterygidæ yet their 'next of kin,' is the only fish which is provided with true lungs. These are unquestionable facts. I leave their bearing upon the great problems of zoological theory to be developed by every one for himself."

In this remarkable passage, written some ten years ago, we may almost say that Prof. Huxley prophesied the discovery, announced in these columns some weeks since,† of the new Australian Mud-Fish, which appears to unite the dentition and other peculiarities of some of the extinct Crossopterygians with the external form of *Lepidosiren*, and thus to connect what, in the opinion of some naturalists, are two very widely separated forms of the order Pisces. Since the announcement of this discovery appeared, a paper has been read before the Zoological Society of London, containing a preliminary account of this wonderful fish, by Mr. Gerard Krefft, the curator and secretary of the Australian Museum, Sydney. Mr. Krefft proposes to call it *Ceratodus forsteri*, "after the Hon. William Forster, M.C.A., its discoverer."

The general form of *Ceratodus forsteri*, and its striking resemblance to *Lepidosiren*, will be at once seen by the accompanying figure, which has been reduced from one of the photographs forwarded to England by Mr. Krefft. The length of the specimen described (which, at the time the paper was written, was the only individual yet obtained) was about three feet; it has a broad flat head, small eyes, and four limbs in the shape of flappers. The body is stated to be covered with large cycloid scales, ten rows on each side. A large gill-opening in front of the pectoral limb contains well developed branchiæ, but their accurate examination was not possible, on account of the bad condition of the specimen. A rather large pair of nostrils, situated just below the upper lip, communicates by a short tube with the roof of the mouth.

The skeleton of this fish is partly ossified and partly cartilaginous, the vertebræ being pure cartilage, and the ribs hollow tubes, filled with cartilaginous substance. The palate and upper part of the skull are bony, and the head is covered by two large scales. The tongue is very small, and is attached to what appears to be a large hyoid bone, ossified externally. The rays which support the dorsal and caudal fin consist of hollow tubes filled by cartilage. In the upper jaw are two large teeth, which Mr. Krefft terms incisors, and which are obviously the representatives of the peculiar teeth in the corresponding position in *Lepidosiren*.* Behind these are dental plates, divided on each side into six tooth-like projections. The lower jaw is provided with similar dental plates, but has no teeth in front; the rami are joined only by a tough skin.



CERATODUS FORSTERI, THE NEW AUSTRALIAN MUD-FISH

Such is an abstract of Mr. Krefft's preliminary notice of this strange animal, which is certainly one of the most remarkable zoological novelties that Australia (that land of wonders) has yet produced. It is singular, indeed, that a creature like this, which appears to have been long well known to the squatters of Queensland, should have hitherto altogether escaped the observation of naturalists. It is said to have flesh of the colour of salmon, and to be excellent eating, so that the settlers have named it the "Burnett," or "Dawson salmon," from the two Queensland rivers in which it is principally found. The native name is given by Mr. Krefft as *Baramoonda* or *Baramoondi*. The fish is stated to attain sometimes a length of six feet and upwards.

* Decade X., 1861.

† See NATURE, No. 28, Vol. ii. p. 38.

* See Dr. Cobbold's discussion of these teeth, in his article on the cranium of *Lepidosiren*: Proc. Z.S., 1862, p. 129.

As regards the correct position of the *Ceratodus forsteri* in the "Systema Naturæ," we must, of course, wait until more specimens are procured for the examination of competent naturalists, the single example in the Australian museum being in an incomplete condition, from the internal organs having been removed. Mr. Krefft calls it a "gigantic amphibian," not being aware probably that all the best authorities now follow Johann Muller in classing *Lepidosiren* as a fish. It is, however, certain that, as Dr. Gunther pointed out at the meeting of the Zoological Society when Mr. Krefft's paper was read, this fish must either be placed, along with *Lepidosiren* and its African representative *Protopterus*, in the order Dipnoi, or that it must form of itself a new division of the Ganoid fishes. We must know more of its internal structure, and in particular of the organs of circulation, before this question can be decided. Mr. Krefft has referred the fish to the genus *Ceratodus*, a name established by Agassiz in his "Poissons Fossiles" for the indication of certain teeth which were then supposed to be those of some kind of shark. Dr. Günther, our best living authority on the class of fishes, is, I believe, of opinion that, so far as the structure of *Ceratodus* is known, there is nothing to show that Mr. Krefft's decision is wrong, though it would appear to me to have been better to have proposed a new generic name for this animal.

In conclusion, I may express a hope that this short notice may have the effect of calling the attention of some of the colonists of Queensland to the wonderful nature of this relic of the Devonian epoch that is now swimming about beneath their noses, and that they will cease, for the present at least, to kill it and eat it as "salmon." Any specimens that may "rise to their fly" should be carefully kept out of the way of the cook, preserved in alcohol and transmitted to the British Museum or some other scientific institution. When the existence of *Ceratodus forsteri* becomes more widely known, there will be no lack of applicants for examples of it. P. L. SCLATER

NOTES

THE Syndicate appointed by the University of Cambridge to consider the question of the manner in which provision may be made for the establishment of a Professorship of Physical Science, and increasing the facilities for instruction in it, have again reported to the Senate. It may be remembered that the former report, after having been discussed in the Schools, was referred back to the Syndicate by the Council. The present report, however, only differs in one important point from the former, in recommending that the increase in the tax levied upon the M.A. members of the University should be temporary instead of permanent. It had been hoped by many that the Syndicate would have taken courage from the general tone of the debate, have cancelled their former report, and proposed some bolder and more thorough measure. It is, however, not very probable that the University will accept the report. Several influential members of the Syndicate have not signed it; and there appears to be a growing feeling in Cambridge that, notwithstanding the obvious evil of delay, to postpone for a few months the appointment of a Professor would be better than to carry at once a temporary and unsatisfactory scheme.

THE Council of the Society of Arts at their meeting on Monday last, awarded the Albert gold medal for the present year, to M. F. de Lesseps, "for services rendered to arts, manufactures, and commerce, by the realisation of the Suez Canal."

WE regret to learn that Baron Liebig continues dangerously ill. He has recently submitted to two operations for a very painful abscess in the shoulder, and continues very weak, but perfectly clear and cheerful, although believing that death is near.

THE new lectures on physiology at Trinity College, Cambridge, by Prof. Foster, will, we understand, be open to all members of the University, so that this liberal act on the part of the college is practically equivalent to the foundation of a professorship of physiology in the University.

THERE will be an examination at St. Peter's College, Cambridge, on Tuesday, June 14, for a Natural Science Scholarship, of the value of 60*l.* per annum. It will be open to all persons who may desire to enter at the University. The subjects are chemistry, comparative anatomy, physiology, and botany. Names of candidates must be sent a week previously to Rev. J. Porter, tutor of the college, from whom further information may be obtained.

AT the meeting of the French Academy of Sciences, held on the 30th ult., Mr. Joule was elected a correspondent in the Physical section by 32 votes out of 43. Of the remainder, Professor Lloyd obtained eight, and Professors Angstrom, Dove, and Volpicelli one each.

THE Senate of the Queen's University in Ireland has unanimously passed a resolution conferring the honorary degree of Doctor in Science on Prof. William King, in recognition of his scientific attainments.

WE learn from the *Moniteur Scientifique*, that M. R. Radau has been charged with a commission from the French Minister of Instruction to visit Germany, for the purpose of studying the organisation for instruction in the higher mathematics in the principal universities of that country, and prepare a report.

A MONUMENT to John Kepler, at Weil der Stadt, the birthplace of the great astronomer, will be uncovered on the 24th inst. Subscriptions have been received from all parts of Germany, France, Russia, and even North America.

ON Friday, May 26, Dr. Carpenter delivered a lecture on the Physical and Biological Conditions of the Deep Sea, in the Senate House, Cambridge. His remarkably lucid and interesting account of the results of the late expeditions, illustrated by some beautiful diagrams, was listened to with great attention by a very large audience, and at the conclusion, the Master of Christ's College (who occupied the chair in the unavoidable absence of the Vice-Chancellor) proposed, and Prof. Sedgwick seconded, a vote of thanks to the lecturer, which was carried with enthusiasm. The latter made some remarks upon the bearing of Dr. Carpenter's discoveries upon Geology, which showed that notwithstanding the burden of eighty-five years, the old fire was still burning brightly.

DR. HASSKARL, formerly superintendent of the Botanic Gardens, and introducer of the Cinchona plant into Java, publishes a monograph of the Indian *Commelinaceæ*, especially those of the Indian Archipelago, with a few other species.

WE have received papers of Port Louis, Mauritius, for April 9th, containing a report of the meeting of the Meteorological Society, held March 24, when a valuable paper was read "On the Origin of Storms in the Bay of Bengal," by the secretary, Mr. C. Meldrum, with special reference to the monsoons and cyclones of the Indian Ocean.

THE last volume of the "Transactions of the Linnean Society," just published, contains Mr. Carruthers's long-expected paper "On Fossil Cycadean Stems from the Secondary Rocks of Britain."

MR. CHARLES T. BROWN, of the Geological Survey of Demerara, has lately returned from a journey of three months' duration in the interior. He has examined the Potaro, Siparunie, and Burroburro rivers, and the country beyond the heads of the latter two, which he finds to be table-land, composed of slightly-inclined beds of sandstone and conglomerate. On the Potaro river he met with a magnificent fall, hitherto unknown, It is formed by the river falling from a table-land 1,375 feet above the sea, perpendicularly, in an unbroken fall of about 900 feet. The river is 100 yards wide, and from 10 to 15 feet deep in its deepest parts. It is with much regret we hear that the Combined Court of this colony have determined that this survey shall be discontinued.

IN the *Artisan* for June is an article by Mr. John Scott Russell, on "International Communication by Railway Steamships," a mode of performing the Channel transit which he has long advocated. The real point of difficulty he states to be this: Dover Harbour is the property of the Government; Calais Harbour is also the property of the French Government. He believes that if the two Governments lay their heads together, and simply make their harbours accessible, which can be done at a moderate expense, a great international communication can be easily established.

IN his annual address as President of the Canadian Institute, the Rev. Wm. Hincks makes use of the following argument in opposition to the Darwinian theory of Natural Selection:—"Nothing is to me more evident than that both seemingly permanent specific and higher differences, and varieties which have no pretensions to permanence, depend on the comparative development of different elements of a common plan; from which it seems to follow both that the non-existence from the commencement of living nature, of all the distinct plans of structure is in the highest degree improbable, and that the tendency of development, sometimes in one direction, sometimes in another, among the same primitive elements, must produce a harmonious system; whilst the preservation of the forms best adapted to a situation amongst a great number of variations arising without order must produce a confused mass of objects having no regular relations and incapable of being reduced to a common system. Which of these prevails in nature I cannot for a moment hesitate in deciding, and consequently I must maintain that, if there is variation, it must be within definite limits, and according to a fixed plan, so as to maintain a uniform order and harmony in the whole system."

A REMARKABLE *mirage* was seen at Ostend on May 19. Ships riding in the roads in the horizon were reflected in the sky as by a gigantic mirror; a brig, a steamer, and several fishing-boats appeared to have other similar vessels attached to the summits of their masts in a reversed position. The phenomenon lasted through the whole of the afternoon and evening, the wind being light from the N.N.E. From 5 to 6.30 P.M. the French coast was visible as far as Dunkirk, the houses being quite distinct, and the *dunes* appearing suspended in the air at a height of several metres, and somewhat moved in position towards the N.W. The port of Niewport seemed so near that the bridge was distinctly visible.

IN the last number of the *Revista das Obras Publicas* (Review of Public Works), Senhor C. A. Beirao mentions an interesting phenomenon of terrestrial refraction, observed by him on the evening (or afternoon, *vespera*) preceding the storm which caused such damages on the Tagus on Easter Day. Here is what Senhor Beirao says about the phenomenon:—"From the house in which I live, in the Rua (street) of S. Joao da Praça,* only about a third of the upper part of the Bugio Lighthouse is to be seen, all the rest is interrupted by the roofs of the buildings in the Praça do Commercio (called by the English 'Black Horse Square,' where all the public departments stand). On that occasion (half-past four o'clock P.M. on Saturday of Alleluia) as I by chance directed my spy-glass towards the tower (*i.e.* of the Bugio), I remarked that not only the latter was completely in view, but also the surface of the water to a distance of many metres on this side the Tower, which indicated, with respect to the latter, an elevation of fifty to sixty metres, at least, over the line of normal vision."

* This street is about four-and-a-half miles from the said Bugio lighthouse.

THE second part of the Proceedings of the Bristol Naturalists' Society contains a notice, by Dr. H. E. Fripp, of recent observations of Amœbæ and Monads by Greef and Cienowski, in which he describes the species of Amœbæ and Rhizopods recently discovered living in the earth; a paper by S. H. Swayne on the Scales and other tegumentary organs of Fish; an analysis by Mr. Sanders, of the Report on Theories of Elevation and Earthquakes, presented to the British Association by the late Professor Hopkins; a note by a lady associate on a novel application of tea leaves for promoting the growth of plants; and a paper on the Descent of Glaciers, by the Rev. Canon Moseley, in which he refers the motion of glaciers to a succession of small alternations of temperature, causing expansion and contraction with a cumulative effect. There is also a paper, by Mr. Stoddart, on Rain-water collected in Bristol, showing the presence of considerable amounts of saline material in the water.

AN icono-photograph album, containing numerous figures of the appearances presented by sections of the nervous centres, has just been presented by Dr. Duchcune, of Boulogne, to the French Academy of Medicine. He states he has obtained excellent results from sections of the great sympathetic nerve, the spinal ganglia, the spinal cord, and of the medulla oblongata when magnified from 8 to 500 times. The plan was suggested some years ago by Dr. Ducheune himself; but it was found that the photographs obtained in the ordinary method were not persistent. He therefore fixed them on stone by a process he terms photo-autography, the details of which, however, he does not communicate. It is satisfactory to find him stating that the results of his experiment and photographs only confirm the substantial accuracy of the beautiful drawings made by Dr. Lockhart Clarke on the central parts of the nervous system, and especially upon the medulla oblongata. In his later experiments Dr. Ducheune has adopted Dr. Clarke's mode of preparation with chromic acid and carmine. He states that certain micrographic details come out with wonderful clearness in the photographs, and that by this means some important additions may be made to our knowledge. Thus he has ascertained that in the white substance of the medulla oblongata there are a large number of very small nerve tubules (0^{mm}, 0033) diameter mingled with others of average and of large diameter 0^{mm}, 0 1 to 0^{mm}, 0 2 and ·0·3.

FROM the proceedings of the Institute of Lombardy, reported in the *Imparziale* of the 16th May, we extract the following results of the important experimental researches of Prof. Mantegazza on the action of the essences of flowers on the production of atmospheric ozone. 1. The essences of mint, turpentine, cloves, lavender, bergamot, anise, juniper, lemon, fennel, nutmegs, cajeput, thyme, cherry laurel, in contact with atmospheric oxygen in light, develop a very large quantity of ozone, equal if not superior in amount to that produced by phosphorus, by electricity, and by the decomposition of permanganate of potash. 2. The oxidation of these essences is one of the most convenient means of producing ozone, since even when in very minute quantity they can ozonise a large quantity of oxygen, whilst their action is very persistent. 3. In the greater number of cases the essences, in order to develop ozone, require the direct rays of the sun; in a small number of cases they effect the change with diffused light; in few or none, in darkness. 4. In some cases, however, the action just commenced in solar light was found to persist to some extent when the essence was placed in darkness. 5. In some cases a vessel perfumed with an essence and afterwards thoroughly washed with alcohol and perfectly dried, could still develop a proportionate quantity of ozone, provided that it retained a slight odour of the essence. 6. The essences that developed the largest quantity of ozone were those of cherry laurel, palmarosa, cloves, lavender, mint, juniper, lemons, fennel, and bergamot; those that gave it in less quantity were anise, nutmeg, cajeput, and thyme. 7. Camphor, as an ozonogenic agent, is inferior to all the above-named essences. 8. Eau de Cologne, honey water, and other perfumes, or aromatic tinctures, develop a proportionate quantity of ozone when they are exposed to the direct rays of the sun. 9. The flowers of the narcissus, hyacinth, mignonette, heliotrope, lily of the valley, &c., develop ozone in closed vessels. Flowers destitute of perfume do not develop it, and those which have but slight perfume develop it only in small quantities. As a corollary from these facts the professor recommended the use of flowers in marshy districts and in places infected with animal emanations, as the powerful oxidising influence of ozone may destroy them. The inhabitants of such regions should surround their houses with beds of the most odorous flowers.

THE fourth part of the "Researches undertaken in the Physiological Laboratory of Wurzburg," edited by Dr. Richard Gscheidlen, has been published. It contains a kindly notice of the life of Albert von Bezold, so untimely snatched away by death; a most profound investigation on nerve and muscle electricity, an abstract of which, we fear, would be unintelligible to our readers, by Worm-Müller, of Christiania; an essay on the Calabar bean, by the editor, and some remarks on the movements of the Iris, by Dr. Engelhardt. In this last it is shown that atropine paralyses the extremities of the third nerve, whilst Calabar bean acts as an irritant upon them, and he holds that there is a ganglionic nervous centre or organ imbedded in the iris, and intercalated between the fibres of the third and the sphincter which acts as an inhibitory centre on the sympathetic.

PRIZE MEDALS OF THE ROYAL GEOGRAPHICAL SOCIETY

THE Royal Geographical Society of London having taken into consideration the fact that a knowledge of geographical facts, and still more a knowledge of the science of geography, is not so common among educated persons as it ought to be, determined, about two years ago, to offer prizes in the shape of gold and bronze medals, to be competed for by boys from certain selected public schools. The principle which governed the choice of the schools, was the number of boys who were receiving education therein; no school having less than 200 boys being chosen for the competition.

An effort was made, at the time that the matter was under discussion, to get girls admitted as candidates. It was, however, objected that there were no public schools for girls which could be invited to compete, and it was also said that it might be well to see how the experiment would work with boys before inviting girls to the competition.

These questions being settled, the next thing to arrange was the manner in which the examinations should be conducted. It was obvious that to bring a number of boys from the various schools scattered all over the country to London, would be practically impossible; and it was equally obvious that some process of selection would have to take place in the schools themselves, so that the best boys only should have their papers sent to the examiners appointed by the Council of the Royal Geographical Society. It was therefore determined that the subjects of examination should be divided into Political Geography and Physical Geography, and that no candidate should be permitted to be examined in both subjects at the same time. Moreover, not more than four candidates in each subject were allowed to take the papers; these four, of course, having been selected by the masters of the various schools from among those most likely to pass. The Council of the Geographical Society could not undertake to direct any preliminary examination—that had to rest entirely with the authorities of the various schools.

It was ultimately determined that the examination should take place by means of papers of questions, which should be sent in sealed packets to the Head Masters of the selected schools; and that these papers should be given out and worked simultaneously at all the schools. The superintendence of the examinations was to be done by a master of each school, who was to be present during the whole time the paper was worked. In order to ensure, as far as possible, the strict fulfilment of the conditions under which the examination was to take place, a declaration was required to be signed by all the masters who were present while the work was going on. This declaration set forth that the candidates worked the papers without assistance, and that there were no globes or maps in the room where they were written. It also declared that the specified masters were present during the whole of the time.

Thirty-seven schools, containing in the aggregate about 12,700 boys, were invited to compete at the first examination. These schools included the nine schools of the Royal Commission of 1864, and all others in England, Scotland, and Ireland which, according to the latest edition of the "Public Schools Calendar," contained not less than 200 boys.

Twenty-one of the invited schools sent candidates; no school being permitted to send more than eight boys, four candidates in the subject of Physical, and four in that of Political Geography. Many schools did not send the maximum number. In fact, four schools had only one candidate each; and out of the twenty-one schools, only four presented the highest number of candidates allowed. Last year forty-two schools were invited to take part in the examinations, but only nineteen sent candidates.

The papers set, both in Physical and Political Geography, bore a remarkable resemblance to each other, and in looking over the syllabus published by the Geological Society, it is impossible not to feel that the line between the two is very feeble, and, in some instances, indefinitely indicated. The special subject appointed for next year is North America.

The examiners appointed for the first year were the Rev. W. G. Clark, F.R.G.S., Public Orator of the University of Cambridge, for Political, and Mr. A. R. Wallace, F.R.G.S., for Physical Geography; and for the second year the Dean of Chester for Physical, and Mr. Wallace for Political Geography.

The successful candidates received their awards last year and this at the anniversary meetings of the Royal Geographical Society.

Rossall School and the Liverpool College have been very distinguished in the examinations. In the first year Rossall carried off both the gold and the bronze medals for Physical Geography; and in the second year the bronze medal for Physical, and the gold medal for Political Geography, both fell to pupils of the same school. The Liverpool College gained the gold medal for Political Geography in the first year, and the gold for Physical, and bronze for Political, in the second year. Honourable mentions have also been gained by the pupils of various other schools.

A very remarkable fact, to be noticed as the result of these examinations, is that the schools which obtained the prizes are not those which the general public is accustomed to look upon as the leading schools of England; while Eton, Harrow, and Rugby are among those who have, as yet, sent no candidates at all. This is the more noticeable as, to Englishmen, the travellers of the world, and the subjects of a monarch on whose dominions the sun never sets, it does appear as if geography ought to be a subject of vast importance instead of being one in many cases almost neglected. Sir R. Murchison, in presenting the medals at the recent meeting of the Royal Geographical Society, hoped that Eton, Harrow, Rugby and other great schools might, in future years, send candidates, "for, without geography, a man cannot be said to be educated at all." Westminster School, it may be stated, to the disgrace of the rest, is the only public school represented in the competition, and has twice had honourable mention.

The Royal Geographical Society has made a wise step in inaugurating this movement, which will give, it is to be hoped, a powerful stimulus to the popular study of geography. The learned societies are at present too much dissociated from the general education of the country. Science has so few votaries among the bulk of the population, that a knowledge of scientific facts, with anything like accuracy, or an acquaintance with scientific methods of working, is almost totally absent from the education of the majority. The scientific man is still to too many persons a species of magician, arriving at his information in occult ways, not to be penetrated by the ordinary observer.

Government is called upon from all sides to do this and that in the matter of education. But Government is slow to move, and is quite sure not to please everybody when it does. In the meantime let scientific societies, each anxious for the spread of knowledge on its own subject, take example by the Royal Geographical Society. Let prizes and honourable mentions be offered, let them be somewhat difficult of attainment, and let the distinction be matter of public award; and it will soon be seen that the scientific education of the country will have received a healthy and vigorous impulse, which will do much to spread the desired instruction through all classes of the nation.

J. A. CHESSAR

ON THE PROGRESS OF BOTANY IN 1869

II.

WITH regard to the succession of races which have undergone a complete specific change through successive geological periods, we have not in plants, in as far as I am aware, any such cases of "true linear types or forms which are intermediate between others because they stand in a direct genetic relation to them," as Professor Huxley appears to have made out in favour of the pedigree of the horse in his last anniversary address to the Geological Society. And I may, in regard to plants, repeat with still greater emphasis his dictum, that "it is no easy matter to find clear and unmistakable evidence of filiation among fossil animals; for in order that such evidence should be quite satisfactory, it is necessary that we should be acquainted with all the most important features of the organisation of the animals which are supposed to be thus related, and not merely with the fragments upon which the genera and species of the palæontologist are so often based." The difficulty is much greater in the case of fossil plants; for instead of bones, teeth, or shells, portions of internal or external skeletons, the parts preserved to us from the Tertiary period are generally those least indicative of structural organisation. Mr. Carruthers has recently (*Geological Magazine*, April and July 1869, and *Journal of the Geological Society*, August 1869) adduced satisfactory evidence of the close affinity of *Sigillaria* and the allied genera of the coal-period with the living Lycopodiaceæ, formerly suggested by Dr. Hooker, but, as he informs me, no connecting links, no specimens indeed of the whole order, have as yet been found in any of the intermediate Cretaceous or Tertiary deposits. Among the latter the presence of numerous types, to which we may plausibly refer as to the ancestors of living races, is established upon unimpeachable data; but I have been unable to find that a single case of authentic pedigree, as successively altered from the Cretaceous through the abundant deposits of the Eocene and Miocene period to the living races, has been as yet as satisfactorily made out as that of the absolute identity of *Taxodium* and others above mentioned, although I feel very little doubt that such a one will yet be traced when our palæontologists will have ceased to confound and reason alike upon the best proved facts and the wildest guesses. Our late distinguished foreign member, Professor Unger, whose loss we have had so recently to deplore, had indeed, shortly before his death, published, under the name of "Geologie der Europäischen Waldbäume, part 1. Laubhölzer," no less than twelve tabular pedigrees of European forest races; but it seems to me that in this, as in another of the same eminent palæontologist's papers to which I shall presently have to refer, his speculations have been deduced more freely from conjectures than from facts. There is no doubt that the presence of closely allied representatives of our Beeches, Birches, Alders, Oaks, Limes, &c., in the Tertiary deposits of central and southern Europe is fully proved by inflorescences and fruits as well as leaves; but how can we establish the successive changes of character in a race when we have only the inflorescence of one period, the fruit of another, and the leaf of a third? I do not find a single case in which all three have been found in more than one stage, and by far the great majority of these fossil species are established on the authority of detached leaves or fragments of leaves alone.

Now let us consider for a moment what place a leaf really holds in systematic botany. Would any experienced systematic botanist, however acute, on the sole examination of an unknown leaf, presume to determine, not only its natural order and genus, but its precise characters as an unpublished species? It is true that monographists have sometimes published new species founded on specimens without flower or fruit, which from collateral circumstances of habitat, collector's notes, general resemblance, &c., they had good reason to believe really belonged to the genus they were occupied with; but then they had the advantage of ascertaining the general *facies* derived from insertion, relative position, presence or absence of stipular appendages, &c., besides the data supplied by the branch itself. And with all these aids even the elder De Candolle, than whom no botanist was more sagacious in judging of a genus from general aspect, was proved to have been in several instances far wrong in the genus, and even order, to which he had attributed species described from leaf specimens only. Palæontologists, on the other hand, have, in the majority of these Tertiary deposits, had nothing to work upon but detached leaves or fragments of leaves, exhibiting only outward form, venation, and, to a certain degree, epidermal structure, all of which characters may be referred to that class which Professor Flower, in his introductory lecture at the Royal College of Surgeons in February last, has so aptly designated as *adaptive*, in contradistinction to essential and fundamental characters. They may, when taken in conjunction with relative individual abundance, assist in forming a general idea of the aspect of vegetation, and thus give some clue to certain physical conditions of the country; but they alone can afford no indication of genetic affinity, or consequently of origin or successive geographical distribution.

Lesquereux, in speaking of Cretaceous "species, or rather forms of leaves," observes in a note to his paper on Fossil Plants from Nebraska (*Silliman's Journal*, vol. xlvi. July 1868, p. 103), that "it is well understood that when the word *species* is used in an examination of fossil plants, it is not taken in its precise sense, for indeed no *species* can be established from leaves or mere fragments of leaves. But as palæontologists have to recognise these forms described and figured, to compare them and use them for references, it is necessary to affix to them specific names, and therefore to consider them as species." But the investigators of the Tertiary floras of Central and Southern Europe have acquired the habit, not only of neglecting this distinction, and naming and treating these forms of leaves as species equivalent to those established on living plants, but of founding upon them theories which must fall to the ground if such specific determination proves inaccurate. Nothing can be more satisfactory than such determinations as that of *Podogonium* for instance, which Professor Heer has succeeded in proving, by numerous specimens of leaves, fruits, and even flowers, some of them still attached to the branches, which I had myself the pleasure of inspecting last summer under the friendly guidance of the distinguished Professor himself. This genus of Cæsalpineæ, from its evident affinity with *Peltogyne*, *Tamarindus* and others now scattered over the warmer regions of America and Africa, and more sparingly in Asia, tells a tale of much significance as to the physico-geographical relations of the Swiss Tertiary vegetation, confirmed as it is by some other equally, or almost equally, convincing examples. But the case appears to me to be far different from the theory so vividly expounded by Professor Unger in 1861 in his Address entitled "Neu Holland in Europa;" this generally admitted theory seems to me to be established on some such reasoning as this:—There are in the Tertiary deposits in Europe, and especially in the earlier ones, a number of leaves that look like those of Proteaceæ; Proteaceæ are a distinguishing feature in Australian vegetation; *ergo*, European vegetation had in those times much of an Australian type derived from a direct land communication with that distant region.

This conviction that Proteaceæ, belonging to Australian genera, were numerous in Europe in Eocene times, is indeed regarded by palæontologists as one of the best proved of their facts. They enumerate nearly 100 Tertiary species, and most of them with such absolute confidence that it would seem the height of presumption for so inexperienced a palæontologist as myself to express any doubt on the subject. And yet, although the remains of the Tertiary vegetation are far too scanty to assert that Proteaceæ did not form part of it, I have no hesitation in stating that I do not believe that a single specimen has been found that a modern systematic botanist would admit to be Proteaceous, unless it had been received from a country where Proteaceæ were otherwise known to exist. And, on other grounds, I should be most unwilling to believe that any of the great Australian branches of the order ever reached Europe. As this is a statement requiring much more than mere assertion on my part, I shall beg to enter into some detail, commencing with a short summary of my grounds of disbelief in European Tertiary Proteaceæ, and then examining into the supposed evidences of their existence.

The analysis and detailed descriptions I have had to make within the last few months of between five and six hundred Proteaceæ, and consequent investigation of their affinities and distribution have shown that the order as a whole is one of the most distinct and most clearly defined amongst Phanerogams. I do not know of a single plant intermediate in structure between that and the nearest allied orders, which I cannot say of any other of the large orders I have worked upon. There is, moreover, especially amongst the Nucamentaceæ, a remarkable definiteness in the majority of genera without intermediate species, whilst the whole order exhibits the greatest uniformity in some of its most essential characters, derived from the arrangement of the floral organs and the structure of the ovary and embryo, accompanied by a truly Protean foliage. All this points, in my mind, to unity of origin, very great antiquity, and long isolation in early times. And the species themselves appear to be for the most part constitutionally endowed with what I designated in my last year's address as individual durability rather than with rapidity of propagation. The order may be divided into about five principal groups, more or less definite in character, but very different in geographical distribution. First, the Nucamentaceæ (from which I would exclude *Andripelatum* and *Guevina*), which we may suppose to be the most ancient, and perhaps the only one in existence where Proteaceæ inhabited some land in direct communication, either simultaneously or consecutively, with extra-tropical Africa and Australia; for it is the only group now represented in the former. It is pre-eminently endowed with the characteristic definiteness and durability of the order. It is very natural as a whole; it has about 250 species in eleven distinct African genera, and nearly 200 species in twelve equally distinct Australian genera, no single genus common to the two countries, and the species mostly abundant in individuals in very restricted localities. In both countries it is chiefly confined to the south. Africa sends only one or two species northward, as far as Abyssinia. The Australian portion has extended to New Zealand, where it has left a single species, now quite differentiated from the Australian ones; very few species (not half-a-dozen) have reached tropical Australia; and, if ever it extended farther, no representatives have yet been discovered in America, Asia, or even in New Caledonia. The four remaining groups, constituting the Folliculares, must have all been formed since the isolation from Africa. 1. Banksieæ, two genera, with above 100 species, have the type of distribution of the Australian Nucamentaceæ, chiefly southern, local, and abundant in individuals, with three or four species penetrating into the tropics, but none beyond Australia 2. Grevilleeæ, in which the genera are somewhat less definite and the distribution more extended, have above 300 species in about eight genera, of which the greater portion are still southern and local; but yet a considerable number are tropical, and a few extend to New Caledonia, although none beyond that. 3. Embothrieæ, with about twenty-five species in half a dozen genera, form part of that southern, chiefly mountain, flora which extends from Tasmania and Victoria to New Zealand, Antarctic and Chilian America, a flora which comprises many species which we might imagine to have spread from the northern hemisphere down the Andes to Antarctic America, and thence to New Zealand and Australia, whilst others may have extended in a contrary direction; and amongst these we may conjecturally include the Embothrieæ, which in America are not found farther north than Chile; whilst in Australia, although chiefly from the southern and eastern mountains, two or three species are northern, and one or two more are found in New Caledonia, but none in the Indian Archipelago, nor in Continental Asia. 4. We have lastly the tropical form of Proteaceæ, the Helicieæ, which are but a slight modification in two different directions (modifications either of the flower or of the fruit) of the Grevillea type, probably of a comparatively recent date; and although now widely spread over South America and Asia, have, nevertheless, left representatives in the original Grevillea regions of Australia. There are nearly 100 species in about eight genera, almost all tropical or subtropical; three small genera are exclusively Australian; *Helicia* itself is Asiatic, chiefly from the Archipelago, extending, in four species, to tropical Australia; in one or two species to New Caledonia; in two or three northward to the mountains of Bengal and Sikkim; and in one species even to Japan. Two American genera, with about forty species, are represented in New Caledonia by one genuine species of each, and one of an allied genus or section; and in tropical Australia by one species showing still the Australian connection; and two small genera are, as far as hitherto known, exclusively American; and may have been there differentiated. No Helicieæ, nor indeed, as already observed, any Folliculares, have hitherto been discovered in Africa. If, therefore, Proteaceæ have really ever extended to Europe, it would naturally be in this Helicioid group that we should seek for them. As far, however, as I can learn, among the supposed century of European Proteaceæ, there is only one which palæontologists refer to it, the *Helicia sotzkiana* of Ettingshausen, founded on a single leaf, which Ettingshausen himself admits to bear much resemblance to the leaves of about twenty genera in thirteen different families; and, upon much consideration, he thinks it rather more like a *Helicia* than anything else, and therefore definitively names it as such, a decision in which it is difficult to concur.

In answer to the above negative considerations, which, after all, lead to presumption only, we are told that we have positive evidence of the existence of Proteaceæ in the Miocene, and still more in the Eocene formations of Europe, in leaves, fruits, and seeds. As none of these have been found attached to the branches nor even in sufficiently abundant proximity to be matched with anything like certainty, we must take the three separately. First, as to seeds, those referred by palæontologists to Proteaceæ are winged and samaroid, some of them probably real seeds, shaped, without doubt, like those of some *Hakeæ* and *Embothria*, but quite as much like those of several Coniferæ, or of certain genera of Meliaceæ, Sapindaceæ, and various other Dicotyledonous orders, there being no evidence of internal structure, conformation of the embryo, &c., by which alone these several samaroid seeds can be distinguished. Moreover, those figured by Ettingshausen in his paper entitled "Die Proteaceæ der Vorwelt" (Proc. Imp. Acad. Sc. Vienna, vii. 711, t. xxxi. f. 11, 12, 14, 15, and δ), have a venation of the wing very different from that of any Proteaceæ I have seen, and much more like that of a real samara of an ash. Next, as to fruits, the hard follicles or nuts of Proteaceæ are as remarkable for their durability as the capsules of so many Australian Myrtaceæ; and we should be led to expect that, where Proteaceous remains are abundant, they should include a fair proportion of fruits, as is the case with the Conifers, Leguminosæ, &c., which have been undoubtedly identified. These supposed Proteaceous fruits in the Tertiary deposits are, however, exceedingly rare. The only ones I have seen figured are: (1) a supposed Embothrium fruit figured by Heer in his Tertiary Flora of Switzerland (t. xcvii. f. 30), an outline impression, with a deficiency in the upper portion, and without indication of internal structure; if this deficiency were filled up, and the seeds inserted, as in the imaginary restoration, f. 31 (for which I see no warrant, and in which the seeds are in the wrong position), it would be something like, but to my eyes not much like, the follicle of an *Embothrium*, and quite as much like what Ettingshausen figures (t. xxxi. f. 5) as the veinless leaf of a *Lambertia*; and (2) the supposed *Persooniæ* and *Cenarrhenes* drupes figured by Ettingshausen (t. xxx.) The former, in the absence of all indication of structure, are quite as good, if not better, representations of young fruits of *Ilex*, *Myoporum*, and many others, as of *Persoonia*; and where, in figures *c* and *d* of the same plate, recent (unripe) Persoonia fruits are inserted, for comparison, with the fossil figures β, γ, and δ, it appears to me that in the latter the long point is the pedicel, and the short point the style, whilst in the former, on the contrary, the short point is the pedicel, and the long one the style. To suppose that fig. 5 of the same plant represents the fruit of a *Cenarrhenes*, which, as far as known, has always an obliquely globular drupe, requires indeed a great strain upon the imagination. I can find no other fossil Proteaceous fruit figured or described.

Lastly, with regard to leaves, necessarily the mainstay of palæontologists, I must admit that there is a certain general *facies* in the foliage of this order that enables us in most, but not in all cases, to refer to it with tolerable accuracy leafy specimens known to have come from a Proteaceous country, even without flowers or fruit; but as to detached leaves, I do not know of a single one which, in outline or venation, is exclusively characteristic of the order, or of any one of its genera. If we know the genus and section of a specimen, we may determine its species by the venation; and we may sometimes fairly guess at its genus if we know it to be Proteaceous; but that is all. Outline is remarkably variable in many species of Grevillea and others, and venation is not always constant even on the same individual. But then we are told, with the greatest confidence, that the structure of the stomata in these fossil leaves, as revealed by the microscope, proves them beyond all doubt to be Proteaceous. In reply to that, I can only refer to the highest authority on these curious organs, Hugo Mohl, who, in a very careful and elaborate memoir specially devoted to the somata of Proteaceæ, has the following passage ("Vermischte Schriften," p. 248):—"Striking as is the above-described structure of the stomata in Proteaceæ, we should, nevertheless, not be justified in regarding this as a peculiarity of this family; for all the variations which we meet with in the structure of the stomata in Proteaceæ are also to be found in plants belonging to widely distant orders."

From the above considerations, I cannot resist the opinion that all presumptive evidence is against European Proteaceæ, and that all direct evidence adduced in their favour has broken down upon cross-examination. And however much these Eocene leaves may assume a general character, which may be more frequent in Australia (in Proteaceæ and other orders) than elsewhere, all that this would prove would be, not any genetic affinity with Australian races, but some similarity of causes producing similarity of adaptive characters.

Another series of conclusions drawn by palæontologists from their recent discoveries, which appears to me to have been carried too far, relates to the region where a given species originated. The theory that every race (whether species or group of species derived from a single one) originated in a single individual, and consequently in one spot, from which it has gradually spread, is a necessary consequence of the adoption of Darwinian views; and when Mr. R. Brown ("On the Geographical Distribution of Conifers," Trans. Bot. Soc. Edin. x. p. 195) sneers at my having qualified it as a perfect delusion, he must have totally misunderstood, or rather misread, the passage he refers to in my last year's address. The expression is there specially applied to the idea of general centres of creation, whence the whole flora of a region has gradually spread, in contradistinction to the presumed origin of individual races in a single spot, which is there as distinctly admitted. The determination of where that spot is for any individual race is a far more complicated question than either geographical botanists or palæontologists seem to suppose. "Every vegetable species," as well observed by Prof. Heer, "has its separate history," and requires a very careful comparison of all the conclusions deducible as well from present distribution as from ancient remains. The very important fact that *Taxodium distichum*, *Sequoiæ*, *Magnoliæ*, *Salisburia*, &c., existed in Spitzberg in Miocene times, so satisfactorily proved by Heer, shows that the vegetation of that country then comprised species and genera now characteristic of North America; but it appears to me that the only conclusion to be drawn (independently of climate and geology) is, that the area of these species and genera had extended continuously from the one country to the other, either at some one time or during successive periods. The proposition that "Spitsberg appears to have been the focus of distribution of *Taxodium distichum*," because an accidental preservation of its remains shows that it existed there in the Lower Miocene period, would require at least to be in some measure confirmed by a knowledge of the flora of the same and preceding periods over the remainder of its present area, the greater part of which flora is however totally annihilated and for ever concealed from us. The fact that *Pinus abies* existed in Spitsberg in Miocene times, and that no trace of it has been found in the abundant Tertiary remains of Central Europe, is very instructive. It might show that that tree was of more recent introduction into the latter than the former country; but it cannot prove that it was not still earlier in some other region, whence it may have spread successively into both territories, still less that its course of dissemination was directly from Spitsberg over Northern and Central Europe. Moreover, the determination of *Pinus abies* is not so convincing as that of the *Taxodium*, resting as it does, if I correctly understand Prof. Heer's expression, on detached seeds and leaves, with a few scales of one cone, and may require further confirmation.

In the above observations it is very far from my wish to detract from the great value of Professor Heer's researches. Interested as I have been in the investigation of the history of races of plants, I have deeply felt my general ignorance of palæontology, and consequent want of means of checking any conclusions I may have drawn from present vegetation by any knowledge of that which preceded it, and the impossibility at my time of life of entering into any detailed course of study of fossils. Like many other recent botanists, I am obliged to avail myself of the general results of the labours of palæontologists, and if I have here ventured on a few criticisms, it is only as a justification of the hope that they may in some measure distinguish proved facts from vague guesses, in order that we may know how far reliance is to be placed on their conclusions.

Spontaneous generation, or Heterogeny, is a question which continues to excite much interest. It has been the subject of detailed memoirs, of violent controversies, and of popular articles in this country, and still more on the Continent; but the solution of the problems still involved in doubt does not seem to me to have much advanced since I alluded to the opposing theories of Pasteur and Pouchet in my Address of 1863. The present state of the case appears to me to be this: in the higher orders of animals every individual is known to proceed from a similar parent after sexual pairing; in most plants, and some of the lower animals, besides the result of that sexual pairing which they all are endowed with, reproduction from the parent may take place by the separation of buds, by division, or sometimes by parthenogenesis; in some of the lower Cryptogams, the first stage in which the new beings are separated from the parent is that of spores termed agamic, from the belief that they never require previous sexual pairing, although the range of these agamic races is being gradually restricted, a remarkable advance having been recently made in this direction by Pringsheim in his paper on the pairing of the Zoospores in *Pandorina* and *Eudorina*. In all the above cases, in all organised beings which in their earlier stages are appreciable through our instruments, every individual has been proved to have proceeded in some stage or another from a similarly organised parent. But there are cases where living beings, Vibrios, Bacteria, &c., first appear under the microscope in a fully formed state, in decaying organic substances in which no presence of a parent could be detected or supposed: three different theories have been put forward to account for their presence: first, that they are suddenly created out of nothing, or out of purely inorganic elements, which is perhaps the true meaning disguised under the name of spontaneous generation, a theory not susceptible of argument, and therefore rejected by most naturalists as absurd; secondly, that they are the result of the transformation of the particles of the organic substances in which they are found, without any action of parent Vibrios or Bacteria; and this appears to be what is specially termed Heterogeny; thirdly, that there existed in these organic substances germs which had proceeded from parent Vibrios and Bacterias, but too minute for optical appreciation, and that their generation was therefore normal. The supporters of Heterogeny rely on the impossibility of accounting for the appearance of the Vibrios and Bacterias in any other manner; for they say that although you treat the medium by heat in a hermetically closed vessel in such a manner as to destroy all germs and intercept all access, still these beings appear. This their opponents deny, if the experiments are conducted with proper care. So it was seven years ago, and so it is still, although the experiments have been frequently repeated in this country, in France, and in North America, almost always with varying results. All reasoning by analogy is still in favour of reproduction from a parent; but Heterogeny has of late acquired partisans, especially in Germany, among those who are prepared to break down the barriers which separate living beings from inorganic bodies.

Brown's celebrated theory of the Gymnospermy of Conifers and allied orders has been of late the subject of keen controversy. Objected to by Baillon, Parlatore, and others, it had been strongly supported by Caspary, Eichler, and lastly, by Hooker in his important Memoir on *Welwitschia*, published in our Transactions in 1863. There the question seemed to rest till last year, when two detailed papers appeared, the one contesting, the other advocating the theory. The most elaborate is without doubt that of Gustav Sperk, in the "Memoirs of the Imperial Academy of Sciences at St. Petersburg." He gives a very fair *résumé* of all that had been published on the subject, and proceeds to record in detail his own observations on the structure and anatomy of the flower in a considerable number of Coniferæ, of *Ephedra alata*, *Gnetum latifolium*, and two species of *Cycas*, illustrated by well-executed analytical figures. He endeavours to prove, chiefly by their anatomy and development; that the coating which encloses the nucleus is carpellary, not ovular, of independent origin, always free, and often earlier developed than the nucleus—that what is wanting in gymnosperms is not the ovarium or carpellary envelope, but the ovular coating—that these plants are in fact gymnosperms in the sense of having naked nuclei and embryo-sacs, not naked ovules.

P. Van Tieghem, on the contrary, in the *Annales des Sciences Naturelles*, ser. 5, vol. x., considers the gymnospermy of the ovules of Conifers to be proved by the anatomical structures of the organs on which they rest. He says that, as in normal Dicotyledons, the ovules are developed from, and continuous with, the margins of carpellary leaves, but these carpellary leaves are open, variously or imperfectly developed, and constitute so itary leaves on a secondary branch in the axil of the subtending bract, this secondary branch being arrested in its development, and the carpellary leaf facing the bract; the paper is illustrated by a large number of diagrams. These two Memoirs, published simultaneously at St. Petersburg and at Paris, contain of course no reference to each other. How far each author may or may not have proved his case, I cannot now take upon myself to inquire into. Neither of them appears to have had any knowledge of the views of Professor Oliver, who in his review of Hooker's Memoir on *Welwitschia* (Nat. Hist. Review, 1863) suggests the analogy of the disputed organ with the axial developments known under the name of floral discs. Both writers, however, confirm the anomalous structure of the flower in this great class of plants, and the position of the plants themselves in many respects intermediate between the higher Cryptogams and Dicotyledons, their connection with the former being clearly shown by the researches of Carruthers and other palæontologists, and with Dicotyledons through *Welwitschia* by Hooker in his above-mentioned Memoir.

Teratology is a subject which has again risen into importance, as aiding in the history of the variations worked upon by natural selection in the formation of species. There had always been a tendency to attribute monsters and prodigies, whether in the organic or the inorganic world, to an infraction of the laws by which natural phenomena are regulated, by the intermediate interposition *ad hoc* of a supreme will for temporary motives inscrutable to man, in which all that the man of science was called upon to do was to establish their authenticity, and detail their abnormities. This, however, was considered by D'Alembert as sufficient to constitute Teratology as one of the great branches of Natural History taken in its most extended sense; for in his once celebrated "Système Figuré des Connaissance Humaines," *Histoire Naturelle* has three great branches—*Uniformité de la Nature*, or the study of the laws which govern the organic or inorganic world, terrestrial and celestial; *Ecarts de la Nature*, the science of prodigies and monsters; and *Usages de la Nature*, or arts and manufactures. Jeremy Bentham, in his "Essay on Nomenclature and Classification," of which I published a French edition now nearly half a century since, strongly criticised such a classification, "by which a middle-sized man is placed in one niche, a tall man and a short man together in another." * Mr. Galton however, in his recently published interesting researches on Hereditary Genius, shows us, after Quetelet, that even in this respect the laws which govern the deviations from the average height of man, both above and below that average, are uniform under similar conditions, and may well be studied together.

We may not, indeed, with D'Alembert, combine the history of animal and vegetable monstrosities with that of mineral monsters and celestial prodigies (whatever these may be); but the course which Biology has taken in the last few years has shown the necessity of accurately investigating in each branch all observed departures from what appears to be the ordinary course, before the real laws of that ordinary course can be ascertained. A work, therefore, in which these observed aberrations are carefully collected, tested, and methodised, cannot fail to be of great use to the physiologist, and such a work with regard to plants, the want of which, brought down to the present state of the science, I alluded to in my Address of 1864, has now been provided for us by Dr. Masters, in his "Vegetable Teratology,"—a work which we should especially like to see deposited in local libraries at home and abroad, to which observers resident in the country could have ready access. Monstrosities or deviations from the ordinary forms in plants are comparatively rare and evanescent; they can be best observed in their fresh state, and often require watching in the course of their development. Country residents have the best means of doing so, and to them it is very important to have a systematic work at hand by which they can ascertain whether the aberration they have met with is one well known or of frequent occurrence, or whether it presents any new feature, adding another item to our store of data, and therefore requiring closer observation and accurate record.

* Essay on Nomenclature and Classification, or *Chrestomathia*, part ii. 1817, p. 157, French Edition, 1823, p. 48.

In making use, however, of Teratology in explanation of structure and affinities, great care is required. It is not every one who can handle these phenomena with the tact of a Darwin. In the course of my systematic labours I have met with several instances where teratologists have been led into conclusions which have proved to be far wide of the truth, owing to their having confined themselves to teratology to the neglect of homology and organogeny. This importance of teratological facts to the physiologist who is able duly to appreciate their bearing, and the discredit cast on their study owing to their misuse in hasty and incautious speculations, are alluded to in Dr. Masters's Introduction. But beyond some explanations of causes suggested by the bringing together a series of facts showing a physiological connection with each other, and with more normal formations, he enters little into the various questions the solution of which has been more or less attempted by the aid of teratology. These questions, indeed, could not have been discussed without fully working out on each occasion normal organogeny, development, and homology, and thus leading him far beyond the object of the present work, which was to present to the future physiologist such a digested record of facts as should best show their relative bearing to each other, to normal conditions, and to any observed causes of disturbance. This object appears to have been well fulfilled, and the method adopted by the author probably the best suited to the purpose. A classification, founded upon the nature of the causes inducing the several changes, might, indeed, as he observes, have been theoretically the best, but is wholly impracticable until these causes shall have been satisfactorily ascertained. For the inquiry into these causes this teratological digest supplies a record of one class of facts, a necessary one, but only one of many classes on which it must be founded.

G. BENTHAM

SCIENTIFIC SERIALS

Journal of the Chemical Society, April 1870. This number contains a "Note on some Reactions of Alcohols," by Mr. E. T. Chapman. The author finds that on distilling with caustic soda a mixture of the rotating and non-rotating amylic alcohols to dryness, the distillate contains a larger proportion of the rotating alcohol than the original liquid; and, on adding water to the residue of sodic amylate and distilling the alcohol which passes over with the water is almost free from the rotating variety. A repetition of the process renders it quite pure. He also finds that repeated treatment of the rotating alcohol by caustic soda converts it into the non-rotating. On treating amylic alcohol to which about 2¼ per cent. of water was added with a quantity of sodium just sufficient to decompose the water, and distilling, water first passed over, followed by amylic alcohol; sodic amylate almost free from caustic soda remaining in the retort; showing that the sodium replaces the hydrogen of the alcohol in preference to that of the water. Again, on distilling a solution of caustic soda in amylic alcohol, water passed over with the alcohol, the residue being sodic amylate.—"Note on the Organic Matter contained in Air," by Mr. E. T. Chapman. Several methods were tried for collecting the organic matter from the air before estimating its quantity. Passing the air through water in a Liebig's potash apparatus, or even in a tube with twenty-five bulbs, did not fix the whole of the organic matters. Cotton wool and gun-cotton failed on account of their invariably containing nitrogenous bodies, which vitiated the results; the condensation of steam in the air and washing with fine spray were better, but not satisfactory. Filtering the air through asbestos paper succeeded very well, but the asbestos was difficult to manage. The process finally adopted was to pass 100 litres of air through a quantity of finely powdered and moistened pumice stone, placed on a piece of wire gauze, fixed on the wide end of a funnel; distilling the pumice with dilute potassic hydrate and potassic permanganate, and determining the quantity of ammonia in the distillate by Nessler's test. In crowded rooms and near an untrapped sink, the air was found to contain organic bases as well as ammonia. 100 litres of air from crowded rooms contained quantities of nitrogenous substances, producing from 0·02 to 0·35 milligrammes of ammonia.—Then follows a lecture by Dr. Gladstone on "Refraction equivalents," which has already been noticed in these columns. The number concludes with a long paper by Dr. Thudichum on "Kryptophanic acid, the normal free acid of Human Urine." From the analysis of the salts it appears to be a dibasic acid of the formula $C_5\ H_9\ N\ O_5$ or a tetrabasic acid containing $C_{10}\ H_{18}\ N_2\ O_{10}$.

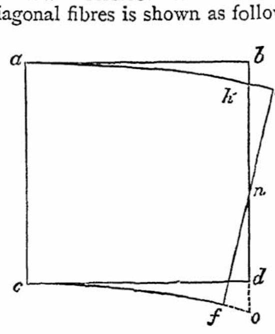
The May number of the *Journal of the Chemical Society* opens with an important paper by Mr. W. H. Perkin on "Artificial Alizarin." He first gives a sketch of the notions entertained by chemists as to the composition and relations of this important colouring matter prior to the investigations of Messrs. Graebe and Liebermann. These chemists showed that by treating alizarin obtained from madder with powdered zinc, a hydrocarbon was produced of the composition $C_{14} H_{10}$ and possessing all the properties of the anthracene of coal tar. By oxidising anthracene it is converted into anthraquinone $C_{14} H_8 O_2$; by the action of bromine dibromanthraquinone $C_{14} H_6 Br_2 O_2$ is produced, and by subsequent treatment with potassic hydrate at a high temperature, an alkaline solution of alizarin $C_{14} H_8 O_4$ is obtained—being the first instance of the artificial formation of a natural colouring matter. Mr. Perkin, in England, and Messrs. Caro, Graebe, and Liebermann, in Germany, have succeeded in obtaining the alizarin without the use of bromine. This may be effected by treating the anthraquinone with strong sulphuric acid, when disulphanthraquinonic acid $C_{14} H_6 O_2 (HSO_3)_2$ is formed, and this, when digested with potassic hydrate at 180°, gives rise to potassic sulphate and the potash compound of alizarin. The alizarin prepared from anthracene is identical with that extracted from madder. It has the same appearance, behaves in the same manner with reagents, produces the same effects in the dye bath, and exhibits the same absorption bands when examined spectroscopically. Two patterns dyed with artificial alizarin accompany the paper.—The second paper is by Mr. John Hunter, and contains some "Analyses of Deep Sea Water, and of some Ooze from the bottom of the Atlantic," collected during the expedition of H.M.S. *Porcupine*.—We then have a paper by Dr. Gladstone, on the "Refraction Equivalents of Aromatic Hydrocarbons and their Derivatives," being a continuation of the subject of the lecture reported in the previous number. Messrs. T. Bolas and C. E. Groves have experimented on the preparation of bromopicrin. They give exact directions for the preparation of this substance by acting on picric acid with bromide of lime, a compound analogous to bleaching powder. Bromopicrin has a very high specific gravity, 2·811 at 12·5°C. The authors announce that they have obtained the carbonic bromide CBr_4 by acting on bromopicrin with powerful brominating agents.—The concluding paper is by Prof. How, on an "Acidified Water from the Coal-field of Stellarton, Nova Scotia." This water was found to be distinctly acid, in consequence of its containing a considerable quantity of free sulphuric acid, probably produced from the iron pyrites present in the coal strata.

SOCIETIES AND ACADEMIES

London

Royal Society, May 19.—"On the Cause and Theoretic Value of the Resistance of Flexure in Beams." By W. H. Barlow, F.R.S.

The author refers to his previous papers, read in 1855 and 1857, wherein he described experiments showing the existence of an element of strength in beams, which varied with the degree of flexure, and acts, in addition to the resistance of tension and compression of the longitudinal fibres. It was pointed out that the ratio of the actual strength of solid rectangular beams to the strength, as computed by the theory of Leibnitz is, in cast iron, as about $2\frac{1}{4}$ to 1; in wrought iron as $1\frac{3}{5}$ and $1\frac{3}{4}$ to 1; and in steel, as $1\frac{3}{5}$ and $1\frac{3}{4}$ to 1. The theory of Leibnitz assumes a beam to be composed of longitudinal fibres only, contiguous, but unconnected, and exercising no mutual lateral action. But it is remarked that a beam so constituted would possess no power to resist transverse stress, and would only have the properties of a rope. Cast iron and steel contain no actual fibre, and wrought iron (although some qualities are fibrous) is able to resist strain nearly equally in any direction. The idea of fibre is convenient as facilitating investigation; but the word fibre, as applied to a homogeneous elastic solid, must not be understood as meaning filaments of the material. In effect it represents lines of direction, in which the action of forces can be ascertained and measured, for in torsion-shearing and "*angular deformation*" the fibres are treated by former writers as being at the angle of 45°, because it has been shown that the diagonal resistances have their greatest manifestation at that angle. Elastic solids being admitted to possess powers of resistance in the direction of the diagonals, attention is called to omission of the effect of resistance in the theory of beams. The author then states, as the result of his investigation, that compression and extension of the diagonal fibres constitute an element of strength equal to that of the longitudinal fibres, and that *flexure* is the consequence of the relative extensions and compressions in the direct and diagonal fibres, arising out of the amount, position, and direction of applied forces. Pursuing the subject, it is shown that certain normal relations subsist between the strains of direct fibres and their relative diagonals, evenly distributed strain being that in which the strain in the direct fibres is accompanied by half the amount of strain in the relative diagonal fibres. Any disturbance of this relation indicates the presence of another force. Thus tensile forces applied at *right angles* to compressive forces of equal amount, produce no strain in the diagonals. But if forces applied at right angles to each other are both tensile, or both compressive, the strain in the diagonal is as great as that in the direct fibres. It is also pointed out that in a given fibre *a, b, c*, the point may be moved with regard to *a* and *c*, thus producing plus and minus strains in the same fibre. Treating a solid as being made up of a series of laminæ, and showing that every change of figure can be represented by the variation in length of the diagonals, taken in connection with those of the direct fibres, the author proceeds to trace the effects of the application of tensile and compressive forces acting longitudinally on either side of the neutral plane, and shows that curvature is the result of the relation between the strains in direct fibres and those in the diagonals. The operation of a single tensile force applied along one side of the plate and a transverse stress are likewise traced out, and the conditions of "*elastic equilibrium*" referred to. The amount of resistance offered by the diagonal fibres is shown as follows:—



a, b, c, d represents a portion of a beam strained by transverse forces into the circular curve *a, e*. Two resistances arise. 1. That due to the extension and compression of the longitudinal fibres produced by the rotation of *b, d* about the neutral axis, which is the resistance considered in the theory of Leibnitz. 2. That due to the extension and compression of the diagonal fibres, caused by the deformation of the square *a, b, c, d* into the figure *a, h, o, c*, which is the resistance of flexure. It is then shown that in a solid rectangular beam, the second resistance is equal to the first, and that both resistances act independently, and consequently that the true theoretic resistance of a solid rectangular beam is exactly twice that arrived at by the theory of Leibnitz. The strength so computed is in general accordance with the results of experiments in cast iron, wrought iron, steel, and other materials, the maximum strength being found in cast iron, which is one-eighth above, and the minimum in glass, which is one-fourth below the calculated strength. The author considers this treatment of the subject as arising necessarily out of Dr. Hook's law "ut tensio sic vis," and that it is in effect completing the application of those principles which are only partially applied by Leibnitz. The paper concludes with some practical illustrations (accompanied by photographs) of the effect of diagonal action. The appendix contains the results of experiments on the tensile, compressive, and transverse resistances of steel.

"On some Elementary Principles in Animal Mechanics.—No. IV. On the difference between a Hand and a Foot, as shown by their Flexor Tendons." By the Rev. Samuel Haughton, M.D. Dubl., D.C.L. Oxon, Fellow of Trinity College, Dublin.

The fore feet of vertebrate animals are often used merely as organs of locomotion, like the hind feet; and in the higher mammals they are more or less "cephalised," or appropriated as hands to the use of the brain. The proper use of a hand when thus specialised in its action, is to grasp objects; while the proper use of a foot is to propel the animal forward by the intervention of the ground. In the case of the hand, the flexor muscles of the fore arm act upon the finger tendons, in a direction from the muscles towards the tendons, which latter undergo friction at the wrist and other joints of the hand, the force being applied by the muscles to the tendon above the wrist, and the resistance being applied at the extremities of the tendons below the wrist by the object grasped by the hand. From the principle of "Least Action in Nature" we are entitled to assume the strength of each portion of a tendon to be proportional to the force it is required to transmit; and since, in a proper hand, these forces are continually diminished by friction, as we proceed from the muscle to the fingers, we should expect the strength of the tendon above the wrist to be greater than the united strengths of all the finger-tendons. Conversely, in a proper foot, the force is applied by the ground to the extremities of the tendons of the toes, and transmitted to the flexor muscles of the leg, by means of the tendons of the inner ankle, which undergo friction in passing round that and the other joints of the foot. In this case, therefore, we should expect the united strengths of the flexor tendons of the toes to exceed the strength of the flexor tendons above the heel. In the case of the hand, friction acts against the muscles; in the case of the foot, friction aids the muscles. I have measured the relative strengths of the deep flexor tendons of the hand above and below the wrist in several animals, and also the relative strengths of the long flexor tendons of the foot above and below the ankle, in the following manner:—I weighed certain lengths of the tendons above the wrist and ankle, and compared these weights with the weights of equal lengths of the flexor tendons of the fingers or toes, assuming that the weights of equal lengths are proportional to their cross sections, and these again proportional to the strengths of the tendons at the place of section. The difference between the weights above and below the joint represents the sum of all the frictions experienced by the tendons between the two points of section. Tables are given showing the results of measurements, *e.g.*, in the case of the Pyrenean Mastiff the amount of friction is 65 4 per cent., while in the Boomer Kangaroo it is *nil*. The foregoing animals all realise the typical idea of a true foot, with a variable amount of friction at the ankle-joint; this friction disappearing altogether in the Boomer Kangaroo, whose method of progression realises absolute mechanical perfection, as no force whatever is consumed by the friction of the flexor tendons at the heel. The only animals whose feet deviated from the typical foot were three, viz., the Alligator, common Porcupine, and Phalanger. In these animals the foot has the mechanical action of a hand, or grasping organ; and the flexor tendons above the ankle exceeded those below the ankle by the following amounts:—Alligator, 11·5 per cent.; common Porcupine, 20·0; Phalanger, 29·2. In the case of the flexor tendons of the hand results were obtained varying from 71·0 in the case of the Common Porcupine, to *nil* in the case of the Goat. It will be observed that the fore foot of the goat, regarded simply as an organ of locomotion, attains a perfection comparable with that of the hind foot of the kangaroo, no force being lost by friction at the wrist-joint. The only animal in which I found a departure from the typical hand was the Llama, in which the flexor tendons of the fingers exceed the flexor tendon above the wrist by 14·4 per cent. The bearing of the foregoing results on the habits of locomotion of the several animals will suggest themselves at once to naturalists who have carefully studied those habits. I shall merely add that the subject admits of being carried into the details of the separate or combined actions of the several fingers and toes, and that the habits of various kinds of monkeys in the use of certain combinations of fingers or toes may be explained satisfactorily by the minute study of the arrangement and several strengths of the various flexor tendons distributed to the fingers or toes.

Paris

Academy of Sciences, May 23.—M. Jordan communicated a theorem on doubly periodical functions.—A note by M. L. Cailletet on the compressibility of gas at high pressures, was communicated by M. H. Sainte-Claire Deville.—M. Chapelas presented a note on the aurora borealis of the 20th May, which was observed to move from west to east, and was followed by great atmospheric disturbances coming from the south-west.—M. E. Lagout described a cheap equatorial sundial which he has invented for the purpose of regulating timepieces. It costs from 8 to 12 francs.—M. A. Trécul presented a note on the hailstones which fell at Paris during the storm of the 22nd May. These were pear-shaped, and very large, some measuring more than three-quarters of an inch in length. One of them showed at its larger end the form of an obtuse-angled rhombus.—A note on the clouds, fogs, and rains with sand, observed in Italy in 1869, by M. Zantedeschi, was read.—Several chemical papers were communicated, namely:—A memoir on the action of water upon iron, and of hydrogen upon oxide of iron, by M. H. Sainte-Claire Deville. A report by M. Chevreul on a memoir by M. Vétillart on the vegetable fibres employed in manufactures, describing the distinctive characters of the principal textile materials as evinced by microscopic observation and by treatment with iodine and sulphuric acid. A note by M. Schützenberger on the compounds of protochloride of platinum with oxide of carbon; one by M. M. Prudhomme on the action of acetylene upon acetohypochlorous anhydride (acetate of chlorine); and one by the same author, on the action of sulphuric anhydride upon protochloride and sesquichloride of carbon, all communicated by M. H. Sainte-Claire Deville.—Observations on the constitution of the flame of the fish-tail gas-burner, by M. A. Baudrimont. The author found that the obscure part of the flame possesses heat sufficient to fuse a platinum wire.—A note by M. A. Vezian, on the system of mineral veins of the Hundsrück, was read; and an extract from a letter by M. de Botella, noticing two recent cases of elevation of land in Spain. Upon the latter M. Elie de Beaumont made some remarks.—M. Cl. Bernard presented a contribution to the knowledge of the minute structure of the mammary gland, by MM. G. Giannuzzi and E. Falaschi. M. d'Abbadie suggested the desirableness of a decimal division of the circle and of time; and General Morin presented a note on some earthenware stoves, manufactured by Müller and Co., of Ivry, which, as he stated, utilise no less than 93 per cent. of the heat developed by the coke consumed in them. Several other papers and communications were read of which the titles only are given.

May 30.—A paper by M. E. Combescene on some differential formulæ was presented by M. Hermite; and a note on a formula of analysis, by M. F. Lucas, was communicated by M. Liouville. General Morin communicated a memoir by MM. C. Martins and G. Chancel on the physical phenomena which accompany the rupture of hollow projectiles of various calibres by the congelation of water contained in them. The numbers obtained by the authors are about one half those obtained by General Morin from the formulæ given by him in his lectures on practical mechanics. General Morin, M. Dumas, and M. Elie de Beaumont made some remarks upon this communication.—M. Jamin presented a note by MM. A. Cornu and E. Mercadier, on melodic and harmonic intervals, in reply to a paper by M. Guéroult, read on May 9.—M. Jamin also communicated two notes by M. Trève on electric currents. In one of these the author cited some further observations in support of his assertion that two currents cannot circulate in opposite directions in the same wire or in the same Geissler's tube; in the second he indicated a method of explaining the course of the currents in telegraphy when terrestrial communications are employed without a return wire. He maintained that the soil is to be regarded as a common reservoir rather than as a conductor.—A note by M. J. Mario on the phenomena of electrostatic induction was read. From his experiments he proposed a theory of terrestrial currents, according to which the sun would be a source of positive electricity acting by induction upon the earth.—A note by M. Neyreneuf, on the theory of electrical condensers, was also read.—The following papers on chemical subjects were read:—A note by M. Cloëz, claiming priority in the discovery of the cyanic and cyanuric æthers, presented by M. Cahours.—A note by MM. Gal and Gay-Lussac, also presented by M. Cahours, on some compounds homologous with tartaric and malic acids. These compounds were adipomalic, adipotartaric, suberomalic, and suberotartaric acids; they are obtained by the action of bromine upon adipic and suberic acids.—A note on the action of hydrochloric acid upon osseine, including researches upon determination of the quantity of osseine in fossil bones, by M. Scheurer Kestner, communicated by M. Balard. The author stated that the solubility of a portion of the osseine in bones is independent of the action of hydrochloric acid, which may be reduced to almost nothing by sufficient dilution. He replied to some remarks of M. Elie de Beaumont on his former paper on this subject.—A note, by M. Sacc, on the preparation of pyrotartaric acid. The process proposed by the author consists in disolving anhydrous tartaric acid in commercial acetic acid, and heating the mixture in a retort until it becomes syrupous; in a day or two the vessel is filled with acicular crystals.—A memoir was read on the organisation of silicified branches probably belonging to a *Sphenophyllum*, by M. B. Renault. The author described the structure of a stem, which he identifies with *Sphenophyllum* (naming the species *S. Charmassii*), and from the characters displayed by which he is led to remove *Sphenophyllum* from the Equisetaceæ.—M. Blanchard presented a memoir by M. Lacaze Duthiers containing his researches upon the evolution of *Molgula tubulosa*. The author stated that the larva of this species, instead of the tadpole-like form supposed to be common to the larvæ of the Ascidia, is an amœboid body which, as it were, flows out of the ruptured egg.—M. A. Duméril presented a note, by M. G. Pouchet, on some monstrous gold fish (*Cyprinus auratus*) from China. This note related to the well-known doubling of the tail in these fish, which, according to the author, always occurs below the extremity of the vertebral column.—M. de Quatrefages presented a note, by M. Bordone, on the organisms which are developed in the silkworms attacked by the disease called *Morts flats*. M. Dumas made some remarks upon this paper.—M. Decaisne communicated a note, by M. J. E. Planchon, on the Phthiriosis of the vine known to the ancients, and on the Coccidæ of the vine of modern times. The cause of the vine-disease known to the Greeks as Phthiriosis is said by M. Koressios to be the *Phylloxera;* the author stated that it was a coccid, but not, as supposed by Walckenaer, the *Coccus Vitis*. (Linn.) He identified it with *Dactylopius longispinus*, of Targioni Tozzetti.—M. A Duméril presented a note by M. E. Moreau, on the cranial region of *Amphioxus*, forming a continuation of his paper on the structure of the dorsal chord in that fish.—A note by M. N. Gréhant, on the rapidity of the absorption of oxide of carbon by the lungs, was communicated by M. Cl. Bernard. The author stated that the absorption of oxide of carbon into the blood commences immediately, and advances rapidly. The blood of a dog breathing air containing one-tenth of oxide of carbon, furnished a gaseous mixture containing 4·28 per cent. of oxide of carbon, in less than 25 seconds, and 18·41 per cent. in less than $1\frac{1}{2}$ minute.—M. C. Robin presented a note by M. J. Chéron, on the state of muscular contractibility, judged comparatively by means of continuous currents, and of currents of induction, in a certain number of paralyses.—M. C. Sainte-Claire Deville presented the first volume of the "Bulletin of the Meteorological Observatory of Montsouris," and made some remarks upon its contents.—Numerous other papers and memoirs were communicated, of which we have only the titles.

BERLIN

German Chemical Society, May 23.—M. Ador has obtained the radical of phthalic acid by treating its chloride with silver. Three molecules thus unite into one. The new body yields three different acids by oxidation, the last of which is phthalic acid.—A. Baeyer, in his own and in M. Emmerling's name, reported on the transformation of isatine into indigo. As isatine, when treated with nascent hydrogen, unites with it and forms indol, a substance not capable of uniting with the reduced substance was sought for, and discovered in phosphorus, the solvent employed being chloride of acetyle or of phosphorus. Real indigo-blue and indigo-red were thus produced. The latter stands in the same relation to the blue as purpurine does to alizarine. To complete the long hoped-for discovery of producing artificial indigo, all that remains to be done is now to transform indol into isatine.—Prof. Cannizzaro, of Palermo, sent in a paper on isomeric cyanuric ethers. Solid chloride of cyanogen acting on benzylic alcohol produces an ether isomeric with one formerly described, and belonging to the series lately investigated by Hofmann.—Messrs. Gomp-Besanez and Grinva have produced artificially the essential oil of rue by distilling together caprate of sodium $C_{10}H_{19}NaO_2$, and acetate of sodium. The acetonic nature and the formula $C_{11}H_{22}O$ (hitherto doubtful) of the oil are thereby fully established.—C. Rammelsberg, in a paper on meteorites, remarked on the absence of alkalis in these celestial rocks. The "Shalkah" stone found in India has lately been analysed, and consists of 22 per cent. of olivine and 88 per cent. of bronzit. He found that sulphuric acid only attacks the former mineral and leaves the latter untouched.—Messrs. Naumann and Vogt report on what Wurtz considered as a combination of chloride of cyanogen with hydrocyanic acid, but which according to them is a mixture.—A. Claus has studied the action of bromine on dichlorhydrine, and that of chloride of sulphur on aniline. The latter reaction has not as yet produced pure substances.—L. Henry by treating with nitric acid, glycol, lactic, and malic acids, has replaced the alcoholic group O H by the group NO_2.—A. W. Hofmann reported on the danger of preparing chloride of cyanogen by the action of chlorine on cyanide of mercury. Sometimes explosions take place during this preparation, particularly at the end of it, when the salt remaining behind and acted upon is a double salt of the chloride and the cyanide of mercury.—Messrs. Baeyer and Martins recommended the action of chlorine on hydrocyanic acid, and that of chloride of lime on cyanide of potassium for producing chloride of cyanogen.

DIARY

THURSDAY, JUNE 9.

ZOOLOGICAL SOCIETY, at 8.30.
MATHEMATICAL SOCIETY, at 8.
ROYAL INSTITUTION, at 3.—Electricity: Prof. Tyndall.

FRIDAY, JUNE 10.

ROYAL INSTITUTION, at 8.—The Ammonia Compounds of Platinum: Prof. Odling.
ROYAL ASTRONOMICAL SOCIETY, at 8.
QUEKETT MICROSCOPICAL SOCIETY, at 8.

SATURDAY, JUNE 11.

ROYAL INSTITUTION, at 3.—Comets: Prof. Grant.

MONDAY, JUNE 13.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

TUESDAY, JUNE 14.

ANTHROPOLOGICAL SOCIETY, at 8.—On the Kelts of Ireland: Dr. John Beddoe.—On the Irish Celts: Dr. H. Hudson.—Notes on the Race Elements of the Irish People: G. H. Kinahan.
PHOTOGRAPHIC SOCIETY, at 8.

WEDNESDAY, JUNE 15.

METEOROLOGICAL SOCIETY, at 7.—Anniversary Meeting.

THURSDAY, JUNE 16.

ROYAL INSTITUTION, at 8.30.
ROYAL SOCIETY OF ANTIQUARIES, at 8.30.
LINNEAN SOCIETY, at 8.
CHEMICAL SOCIETY, at 8.
NUMISMATIC SOCIETY, at 7.—Anniversary Meeting.

BOOKS RECEIVED

ENGLISH.—Atlas and Handbook of Physical Geography: Keith Johnston, jun. (Johnston and Co.).—The Story of Aristæus and his Bees: R. M. Millington (Longmans).—Selections for Latin Prose: R. M. Millington (Longmans).

FOREIGN.—Leçons d'Optique physique: E. Verdet. Masson, Paris.—(Through Williams and Norgate).—Traité de Paléontologie Végétale, et Atlas. Tom ii. pt. 1re.: W. P. Schimper.—Beiträge zur Morphologie und Physiologie der Pilze: A de Bary and Woromin.

CONTENTS

ERRATA.—In No. 31, p. 95, second column, line 52 from top, for "fires" read "fuses;" lines 58 and 61 (*bis*), for "carbonate" read "carbamate."