Nature. Vol. II, No. 41 August 11, 1870

London: Macmillan Journals, August 11, 1870

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THURSDAY, AUGUST 11, 1870

SCIENCE SCHOOLS AND MUSEUMS IN AMERICA

AT the present time when we are, as it were, taking stock of our Scientific Institutions, an account of the various schools and colleges in the United States, in which Science is made a chief, if not *the* chief subject, may be welcome to our readers. A paper in the *Canadian Naturalist*, by Prof. Dawson—the result of the Professor's travels through the States, in order to determine by personal visits the practical working of the American Science Schools, and to use the experience so obtained, in the founding of a Canadian School of Science at Montreal—has been largely used.

It was for similar reasons that Prof. Agassiz visited the various museums of the Old World, in order to determine what errors he should avoid, and what precedents he should follow, in founding the magnificent Museum at Cambridge, U.S.

Referring to the various institutions in the various States, we will follow the footsteps of Prof. Dawson, adding whatever has been changed or improved since his visit.

In New York, Science has for some time past been at a very low ebb. Unlike London or Manchester, that busy mercantile community has no time to spend on such apparently trifling matters as the propagation of scientific knowledge, and the acquisition of materials for scientific investigation. The only School of Science in New York is Columbia College; an old-fashioned brick building, in a quaint, old-fashioned square, formerly outside the town, now, by the rapid increase of building, quite enclosed and surrounded. The College-buildings form three sides of a quadrangle, and are long, narrow rooms lighted by windows in the sides. Three rooms are used as laboratories for practical analysis, qualitative and quantitative. Another room is the furnace-room, for assaying purposes; another is used for purposes of drawing, and there are numerous class-rooms and lecture-rooms, but all sadly out of proportion to the size of the town. Two rooms are set apart, one for the mineralogical, the other for the geological and palæontological collections. Among the latter the private cabinet of Prof. Newberry, especially rich in remains from the Carboniferous strata, is the most prominent feature.

The staff of professors, lecturers, and assistants, numbers eighteen in all; and the lectures, practical and theoretical, are purely scientific, embracing mineralogy, metallurgy, chemistry, botany, mechanics, physics, geology and palæontology, assaying, and drawing.

The full course for students is three years, at the end of which they are duly qualified for practical mining work, mineral surveying, and practical chemistry. They have to pass an entrance examination in algebra, geometry, and trigonometry, and at the end of their course most of them attain the degree of "Engineer of Mines," or "Bachelor of Philosophy." The number of students in the last account was from 112 to 120. An important feature of the course is that students are expected in the vacation to visit mines and metallurgical and chemical establishments, and to report on these, and also to make illustrative collections; while, during the session, short excursions are made to the machine shops and the metallurgical and chemical establishments in or near the city.

The practical value of such a course of training cannot be too highly appreciated; it is an example which many of our science schools and colleges would do well to follow. In justice, however, to one of the chief of these in England, it should be mentioned that the engineering and chemical classes in Owens College, Manchester, are in the habit of being taken by the respective professors to the leading scientific and chemical works near Manchester.

Besides Columbia College, New York is now in a fair way soon to possess a very fine public museum. In the beginning of 1869 a bill was carried in the House of Congress to establish in New York a museum, under Government control, similar to the British Museum. This museum is called "The American Museum of Natural History," and it published its first report a little time ago. The fine collection of the Prince of Neuwied, formed chiefly in the Brazils and South America, has been purchased, and communications have been sent to all the United States consuls throughout the world to aid the museum by the collecting and purchasing of valuable natural history specimens in their several localities. We cannot leave the State of New York without noticing how the liberal founder of the Cornell University has made provision for practical and theoretical instruction in natural science. Laboratories, museums, herbariums, libraries of scientific works, have been either presented or bought; and by the scheme by which the students can work out the expenses of their education by their skilled labour, it is now possible for the very poorest artisan or mechanic in America to obtain as valuable a scientific education as any given anywhere in the world.

Proceeding from New York to New Haven, we find here a very important and very fine school of science. The Sheffield Scientific School is a modern outgrowth of the University of Yale College, and was first established in 1847 as a small undertaking, conducted by the elder Silliman, chiefly as a school for applied chemistry and scientific agriculture. In 1860 Mr. Sheffield, a wealthy citizen of New Haven, gave it a building and apparatus valued at 50,000 dollars, supplementing this by a grant of 50,000 dollars to found chairs of engineering, metallurgy, and chemistry. In 1863 it was further enlarged by grants of land from the State of Connecticut in aid of scientific education. It also participates in the benefit which Yale College derives from Mr. Peabody's Museum and his magnificent endowment of 150,000 dollars. The Sheffield School contains some fine natural history collections, one of the most valuable of which is one of Economic Geology, admirably arranged, and which shows the immense mineral wealth of America to great advantage.

The buildings of the school are finer and better arranged than those of Columbia College, and its educational scope is wider. There are six distinct courses of scientific instruction open to the students:—

1. Chemistry and mineralogy.
2. Engineering and mechanics.
3. Mining and metallurgy.
4. Agriculture.
5. Natural history and geology.
6. A select scientific and literary course.

The class-rooms and laboratories are extremely well adapted and arranged; there are small cabinets of specimens of great assistance to the professors in delivering their lectures. It also possesses a fine equatorial telescope, made by Clark, having an object-glass with an aperture of nine inches. For this there is a properly constructed tower, furnished with all the necessary instruments for astronomical observations.

The staff of professors, lecturers, &c., numbers twenty-three, and the number of students is now about 150. Amongst the distinguished men holding appointments here, the names of Dana, Silliman, and Marsh are of European renown.

In Philadelphia we find valuable scientific collections belonging to the Academy of Science, an old and still very vigorous scientific establishment, though sadly wanting more room for its collections. Amongst the treasures of these collections there are several that merit particular notice.

Professor Hawkins has just set up in the museum the skeleton of the Hadrosaurus of the New Jersey Greensand, one of the most gigantic of the immense mesozoic reptiles. Besides this extremely valuable skeleton and other remains, there are portions of the skeleton of a gigantic carnivorous reptile of the same age, with formidable cutting teeth, similar to those of the megalosaurus, and, like it, possessing hooked claws, some of which must have been ten inches long.

Belonging to the Philadelphia Academy of Sciences is a fine scientific library, but both it and the collections suffer from want of space. The museum also contains a fine and very complete collection of American skulls, which Messrs. Morton, Wilson, and Meigs have so elaborately worked out. At Philadelphia two valuable works on palæontology have been undertaken. One of these just published was reviewed in NATURE for June 16, and is an extremely valuable monograph on "The Fossil Mammals of America," by Prof. Leidy. The other work is an equally valuable one, being a monograph on the "Fossil Reptiles of America," by Prof. Cope.

Proceeding from Philadelphia to "the queenly city," Baltimore, we find there an Academy and a band of zealous naturalists, as Tyson, Morris, and Dalrymple. The Peabody Institute here was founded by Mr. Peabody, who was a resident in this city for some time, and who presented to the town the sum of 100,000 dollars for these objects, viz.: (1) to found an extensive library; (2) to provide for the delivery of lectures in science and art; to found (3) an academy of music, and (4) a gallery of art.

The building of the institute was in 1868 being rapidly proceeded with, and is probably now finished. There was then no museum, but the library had already become extremely valuable.

In Washington is located the splendid Smithsonian Institution, of world-wide fame, founded for "the increase and the diffusion of knowledge amongst men," now under the direction of Prof. Henry, the able president of the American Academy of Sciences, and who is now in this country. The chief work hitherto done by the Smithsonian Institution has been in the subjects of geology and natural history, and it already possesses extremely valuable collections, carefully and systematically arranged under the able supervision of Prof. Baird. This institution sets a glorious example to other and much older museums in their treatment of those anxious and willing to make use of the accumulated treasures. Their collections are open to the inspection of any naturalist from any part of the world, who, in some cases, are accommodated with rooms for their work, as well as access to the specimens. Under Prof. Henry's personal superintendence a fine collection of American antiquities is in process of formation, and the number and importance of these objects of early warfare and art make the museum extremely valuable and instructive.

It was in Washington that President Lincoln was so basely assassinated. The building where the event took place, formerly Ford's theatre, has been converted into a museum of a character it is believed perfectly unique. This, the Army Medical Museum, contains a series of excellently mounted preparations of great professional interest, remarkable chiefly for its profuse exhibition of the effects of shot and shell, and other implements of war on the human frame. The materials for this museum were chiefly collected during the American Civil War. It may well be said that the Americans are a wonderful people; there are few other nations which would have been capable of so utilising the results of a protracted internecine war as to make them available in after years towards the advancement of medical science and the alleviation of human pain.

In addition to the purely medical collection, this museum also contains a fine and well-arranged collection of skulls of the various aboriginal American tribes, with a few Mexican and Peruvian. There is also a fine collection of skulls in the Smithsonian Institution.

Before concluding this paper it is necessary to mention two separate munificent gifts, by which the late Mr. Peabody did so much to promote the cause of science in America. The Peabody donation to Yale College has been previously alluded to. One result of this is that Yale College, which was formerly devoted to other subjects, has recently made great progress in science, and bids fair to become one of the leading scientific institutions in America. It still lacks, however, funds towards founding more scientific professorships. Besides this donation, Mr. Peabody in 1867 left 140,000 dollars "for the promotion among the inhabitants of my native county (Essex County, Massachusetts) of the study and knowledge of the natural and physical sciences and their application to the useful arts."

From this gift started the Peabody Academy of Sciences in Salem, Massachusetts, which was inaugurated August 18, 1869. The objects of the foundation are still kept in view by the formation of a museum, which, besides a general collection, shall embrace a complete collection of local specimens from the whole county, and shall keep up and augment the museum of East Indian antiquities collected by the East Indian marine societies, and by the formation of a series of lectures on science, to be yearly delivered.

And, lastly, the nature of the valuable museum which Professor Agassiz is collecting at Harvard College, Cambridge, aided by Government grants and private subscriptions, will have been sufficiently learnt from the article on the Harvard Museum, which appeared in NATURE for June 23.

THE VERTEBRATE SKELETON

SKELETAL archetypes, and "theories of the skull," have of late years gone much out of fashion. The view which made each man a potential Briareus as to limbs, seems itself to be considered as no longer having a leg to stand upon. The fortress of the "Petrosal" has long been carried by assault, and is peaceably and securely occupied; and although we have had lately a brilliant passage of arms apropos of the "auditory ossicles" from which the unlucky Sauropsida retired with broken "hammers" and diminished "anvils;" yet the once widespread interest in skeletal controversies seems to have long subsided. The old war-cries are no longer heard, the question "Is the post-frontal a parapophysis?" falls on indifferent or averted ears, and we fear that even not a few of our anatomists call into daily functional activity a mandible, to the true nature and homologies of which they are comparatively indifferent.

What was the surprise of some, then, who last year witnessed, in the theatre of the Royal College of Surgeons, an unlooked-for resurrection. Some rubbed their eyes—could they have had a long sleep, and was it still the year 1849 instead of 1869? A quasi-vertebrate theory of the skull once more! Again an exposition of cranial hœmal arches!

"Jam redit et Virgo, redeunt Saturnia regna."

But yet in justice it must be said that it was by no means the reproduction of an old or familiar system. The views propounded were in some respects as novel as striking; while in spite ot this a careful re-statement of assertions made in the first year of the last Hunterian professorship showed how subordinate after all were the changes made, and how trifling the modifications required as to the statements of that first year. Nor in fact was any new archetypical idea of the whole vertebrate skeleton distinctly proposed for acceptance, though the serial relationship of certain inferior arches was clearly demonstrated, and a striking suggestion made concerning the most anterior of them.

But *some* ideal conception of the vertebrate skeleton as a whole is a necessity for anyone who proposes to extend his osteological labours over several classes, and provided such a conception be a simple "generalised expression of observed facts," no one has a right to complain of its introduction. What best conception then of this kind can be now supplied from the accumulated labours of successive osteologists?

As the points of exit from the skull of the cranial nerves supply the best fixed points for determining special cranial homologies, so probably the arrangement of the nervous system as a whole will supply the handiest key to the explanation of skeletal difficulties.

In the Hunterian Lectures of 1869, the nervous system was treated in a new way, and one by which the sympathetic system lost its isolation, and was called in to take its morphologically important part in the general system of spinal nerves. The embryonic condition being referred to (with ascending dorsal plates and descending ventral-plates—the latter bifurcating to enclose the pleuro-peritoneal space between their outer and inner laminæ), each spinal nerve of the trunk was represented as sending one branch upwards into the dorsal plate, another downwards into the outer lamina of the ventral plate (abdominal and intercostal nerves), and another, also downwards, but into the inner lamina of the ventral plate, the collection of these latter internal nerves with their serial homologues forming the sympathetic system. In addition to these, a branch was represented as running directly outward towards the skin, above the external descending branch. Now, such being the condition of the nervous system, what might we *a priori* expect to find in the skeleton? Surely we might expect to find—1st, Parts related to the dorsal laminæ (epaxial); 2nd, Parts related to the external ventral laminæ (paraxial), and 3rd, Parts related to the internal ventral laminæ (hypaxial). To the first category would belong the neural arches, &c.; to the second, the transverse processes, ribs, and steruum; to the third would belong those skeletal structures, if such there are, within the pleuro-peritoneal cavity or medianly situated beneath the vertebral column.

But as to the nerves passing directly outwards above the external descending ones, are there any skeletal structures to answer to them?

Now, fishes present us sometimes with a double series of ribs, whereof the upper strike out towards the skin, while the lower tend to enclose the abdominal cavity. In tailed Batrachians we have two superimposed transverse processes to which a bifurcating Y-shaped rib articulates, and this rib sometimes bifurcates distally also. In mammals we have a rib essentially similar as to its proximal end, but one branch of the Y is diminished into a tubercle which, however, meets a transverse process. Can it be, then, that our own ribs are morphologically double, and that their upper proximal parts together with the fascia ascending from them to bound externally the *erector spinæ*, are homologous with the upper series of the ribs of fishes?

But what are the hypaxial structures, and first, what parts of the skeleton are within the pleuro-peritoneal cavity or are serially homologous with parts so situated? Here an important modification seems necessary in the views given out by Professor Huxley in 1869. He demonstrated unanswerably that the branchial arches are, as Professor Goodsir considered them, thoroughly homologous with the hyoidean and mandibular arches, and not only this, but he also suggested—what was as novel as important—that the *trabeculæ cranii* may be the foremost members of the same group of parts. He considered, however, that all these parts were *costal* in their nature. Now, accepting this view as far as regards the serial homology of the branchial arches with parts more anterior, it is nevertheless here submitted that the branchial arches should be considered parts *within* the pleuro-peritoneal cavity, and this because the heart lies *outside* them, and the great vessels (which even in man have reflected on them a continuation of the pericardium) extend along their *outer* sides. It is contended, then, that these arches are hypaxial parts, and, if this is so, then the hyoidean, mandibular, palato-quadrate, and trabecular structures, as they are serially homologous with the branchial arches, must be hypaxial also. If so, the nerves which accompany them (the vagus, &c.) must be serially homologous with the sympathetic nerves of the trunk, and, indeed, this view was put forward by Professor Huxley in the lectures referred to. Are there, then, no true representatives of costal arches in this part of the frame? I think that the external branchial cartilages of sharks and the branchial basket of the Lamprey will be found to be such, and therefore to belong to a quite different category from that to which the branchial arches of osseous fishes pertain.

Again those azygos processes which descend from beneath the vertebral column in the region of the trunk, must be in the line of origin and suspension of the internal lamellæ of the ventral plates of the embryo, and being related to them may be deemed to be hypaxial parts also. Their serial homologues often bifurcate, and are repeated serially in the caudal region by processes or distinct ossicles (chevron bones) protecting the caudal vessels, and which I deem to be hypaxial also. Professor Goodsir has demonstrated that in the crocodile such parts, at the root of the tail, lie within the backward prolongation of the abdominal cavity, and the chevron bones or processes beyond that cavity in the same individual, are clearly the serial homologues of those within it.

According to this view then, the vertebrate axial skeleton in its most generalised expression consists of an antero-posteriorly extended axis, bearing above it (1) a cylinder of *epaxial parts*, for the protection of the cerebro-spinal centres. This cylinder expands anteriorly, and has intercalated three sets of sense capsules, olfactory, optic, and auditory. Everything, whatsoever it be, outside the anterior end of this cylinder (the cranial capsule) is morphologically *outside* the skull, and therefore in such an essentially *external* position is the sella turcica, the anterior communicating artery, &c.

2. From the axis of the skeleton diverge on each side more or less bifid *paraxial parts*, tending to protect or surround the visceral cavity, or homologous with parts which do so tend.

3. From the same axis descend *hypaxial parts*, which parts attain their maximum of size and importance towards the two ends of the skeleton. At the anterior end they by their varied degree of development and coalescence, build up the frame-work of the face, the jaws, and the hyoidean structures.

To this axial skeleton is added, in completely developed forms, two limb-girdles, each consisting of one upwardly and two inwardly and downwardly directed parts on each side. Two limbs, bilaterally symmetrical, are attached to each girdle, and a serial symmetry, bone answering for bone, exists between the anterior and posterior limbs of each side.

Can the skeleton structure of these limbs be expressed in yet simpler terms? Professor Gegenbaur has attempted very ingeniously so to express it, considering the limb bones as differentiations of primitive similar offshoots from a chain of marginal fin bones or cartilages. But much as one would naturally wish to accept so tempting a theory, two obstacles at present oppose themselves. One is the presence of a radial ossicle answering to the pisiforme of the ulnar side. The other is the occasional presence, in fossil forms, of at least one whole chain of such ossicles. So that at present we can hardly in this respect venture upon a more generalised view of the skeleton than the one here adopted.

This conception of the vertebrate skeleton takes little account of the mode of origin of skeletal parts—whether exogenous or autogenous, or of their segmented or unsegmented condition. But such considertaions have been neglected deliberately from a conviction of the completely subordinate importance of such conditions. The views here stated suggested themselves during the study of the skeleton as it exists in tailed batrachians; they have elsewhere been given at length, and their defence attempted, but it has been thought that a brief statement of them here might not be altogether unacceptable to some who are engaged in osteological inquiries.

ST. GEORGE MIVART

HOOKER'S BRITISH FLORA

The Student's Flora of the British Islands. By J. D. Hooker, C.B., M.D., F.R.S., Director of the Royal Gardens, Kew. (London: Macmillan and Co., 1870.)

NOTWITHSTANDING the number of British Floras already in existence, field-botanists have long lamented the want of a text-book combining all the requisites for out-of-door work, unquestionable accuracy, clearly-expressed definitions, a good arrangement, and a portable form. Although the hand-books we have hitherto used have possessed one or other of these features in an eminent degree, no one has yet succeeded in uniting them. For accomplishing this difficult task the best thanks of every British botanist are due to Dr. Hooker. The publication in quick succession of several works with a similar scope, may be taken as an indication of a reviving interest in British botany. Thirty years since, when the Linnean system of classification was still in use, a sufficient acquaintance with plants to enable anyone to give the Latin names of the species of their own districts was a fashionable acquirement, especially with ladies. The knowledge, however, was extremely superficial; it consisted mainly in counting the number of stamens and of pistils, so as to determine the class and order, and of observing the trivial specific characters of the foliage, colour and size of the flowers, &c., and was unaccompanied with the least real acquaintance with structural or physiological botany. An artificial classification like that of Linnæus, must always conduce to this result, and the ease with which plants can be named by such a method, is in itself an evil rather than an advantage. When we advance from an empirical to a natural system, in which the diagnoses of the orders depend on a variety of characters, some of them connected with minute details of structure, the gain, both to the learner and teacher, is immense. The learner is compelled to begin at the root of the matter, and to acquaint himself with the structure and physiological function of every separate organ, and with the different forms it may assume, before he attempts to name a plant; and the teacher can no longer cram his class with that showy surface knowledge which is the bane of popular science teaching. The general adoption of the Natural system of classification was followed by a great falling-off in the ranks of amateurs. The number of real students of botany is now however, we hope, increasing day by day, and the substantial interest and instruction derived from the science are in proportion enormously augmented.

The difficulties of the Natural system must be familiar to all teachers; probably every lecturer has more than once been perplexed by finding that the specimens which have been provided for illustration belong to a species differing in some point of structure from the characters which he has given as belonging to the order under consideration. Teachers will also differ as to the relative importance of certain points of structure, and consequently as to the position of some Natural orders. The uncertainty which still hangs over our classification will be illustrated by the following list of points in which the book before us differs from the fourth edition of Professor Babington's "Manual of British Botany," published as recently as 1856: *Droseraceæ* has been moved from Thalamifloræ to Calycifloræ, the genus *Parnassia* being incorporated with *Saxifrageæ; Acerineæ* undergoes the same change of position; *Balsamineæ* and *Oxalideæ* are abrogated as separate orders, their genera being united to *Geraniaceæ; Portulaceæ, Tamariscineæ,* and *Paronychieæ,* on the other hand, are transferred from Calycifloræ to Thalamifloræ, the tribe *Sperguleæ,* however, of the latter order being relegated to *Caryophylleæ; Grossulariaceæ* is no longer found as a separate order, but is united to *Saxifrageæ; Ilicineæ* or *Aquifoliaceæ* changes its quarters from Corollifloræ to Thalamifloræ; *Loranthaceæ* from Monopetalæ to Apetalæ; *Empetraceæ* from Apetalæ to Thalamifloræ; while the apetalous order of *Callitrichaceæ* disappears, its species being found under Calycifloræ united to *Halorageæ;* the hop, on the other hand, is eliminated from *Urticaceæ,* and appears as a separate order, *Cannabineæ;* and Babington's miscellaneous collection of *Amentiferæ* is divided into the four distinct orders, *Salicineæ, Cupuliferæ, Betulaceæ,* and *Myricaceæ; Trilliaceæ, Colchicaceæ,* and *Asparageæ* are combined with *Liliaceæ.* Although these alterations concern chiefly comparatively small and unimportant orders, there is sufficient change to perplex the student, independently of minor re-arrangements of genera, &c. On the whole it will be seen that the tendency is towards the English practice of "lumping," as contrasted with the Continental practice of "splitting," the number of orders of Flowering Plants being reduced from ninety-seven to ninety-two. We are glad, however, to find that this tendency is not carried to the extent we meet with it in Hooker and Bentham's "Genera Plantarum," where *Papaveraceæ* and *Fumariaceæ* are united; we doubt, indeed, whether the interests of students would not have been better served by keeping apart orders with such clear distinctions of outward structure, as far as British species are concerned, as *Saxifrageæ* and *Grossulariaceæ; Geraniaceæ, Oxalideæ,* and *Balsamineæ; Liliaceæ* and *Colchicaceæ.* The re-arrangements of position are no doubt, in nearly all cases, based on correct botanical principles. The difficulty, however, often experienced in drawing up satisfactory diagnoses of the natural orders may be illustrated by comparing those given by such authorities as Hooker and Oliver. In Hooker's "Student's British Flora," for instance, we find the pistil of *Nymphæaceæ* described as "syncarpous;" in Oliver's "Lessons on Elementary Botany," as "apocarpous." Hooker speaks of the stamens of *Oleineæ* as "epipetalous," Oliver as "hypogynous." In the synopsis of natural orders given at p. xiv. of the book under review, mention of the hypogynous stamens in some genera of *Oleineæ* is omitted.

In his analysis of certain difficult and intricate genera, Dr. Hooker has followed the lead of botanists who have made them their special study, as in the case of *Rubus, Rosa,* and *Hieracium,* where Mr. J. G. Baker's descriptions are adopted. This, no doubt, was a wise course in these instances; we regret, however, to find in the whole work so little of the author's own observations; we are sure that in many cases he could have improved greatly on the method of the "London Catalogue," which has been too implicitly followed. This is especially the case with regard to the plants admitted as "colonists" or "denizens." Why should a place be given, for instance, to *Galinsoga parviflora,* found nowhere, we believe, except within a radius of a few miles from Kew Gardens, from which it has escaped? while, on the ballast hills of the north-east coast and some other localities, many plants have apparently become permanently established, of which no mention is made, or their name is merely given in the Appendix. The time of flowering of plants is also one on which little exact observation appears to have been made; one would judge from our hand-books that the only wild flowers to be gathered in December and January are the groundsel and the daisy; while at least a dozen others could be named that are equally, if not more, perennial. We shall look with eagerness for a work embodying a record of recent physiological and morphological observations on British plants, which Dr. Hooker states, in his preface, it was his original intention to have incorporated with the present volume, and which will possess so great a value from his pen.

The specific descriptions in Dr. Hooker's "Student's Flora" are so admirable, terse, and yet sufficient, the arrangement so excellent, and the size so convenient, that it must rapidly become the work in general use, the companion of every botanist during his summer rambles.

ALFRED W. BENNETT

WATER ANALYSIS

Water Analysis: a Practical Treatise on the Examination of Potable Water. By J. Alfred Wanklyn, M.R.C.S., and E. T. Chapman. Second Edition, edited by E. T. Chapman, Member of the Council of the Chemical Society. Pp. 108. (London, 1870.)

IN the preface to this edition we are told that the whole of the last edition has been transferred almost without alteration, but with slightly different arrangement; and that some new matter has been added, consisting of the tetration of waters; a modification of the process for estimating nitrates; a chapter on volatile organic matter; a method of estimating minute traces of lead; and a chapter on the purification of waters. The tetration of waters is the estimation by standard solutions of the amount of acid present in waters contaminated by the refuse of certain factories or in rain water which has fallen near alkali works. We had thought that the word *tetration,* in the preface, was a misprint for *titration* until we found it so spelt in the text; the latter word (or modification), however, appears on p. 38. The modification of the process for the estimation of nitrates consists in treating the water to which caustic soda has been added with a large excess of aluminium scraps, and pouring off the liquid before distillation, instead of permitting all the aluminium to be dissolved, as in Mr. Chapman's original modification of Schulze's process. In the chapter on volatile organic matter, it is pointed out that on distillation of a water with potassic hydrate, there passes over, together with the ammonia, some combined nitrogen, probably in the form of organic bases, and a process is described for its estimation. The method of estimating minute traces of lead by comparison of the coloration produced by sulphuretted hydrogen water in dilute standard solutions of lead with that obtained by the same reagent in the water under examination is not new, for we remember having seen the experiment illustrated in Dr. Hofmann's lectures at least fifteen years ago. The previous removal of oxidising agents by sulphurous acid is, nevertheless, a useful addition. In the chapter on the purification of water, an ingenious experiment is described to show that separation of suspended matter by filtration through sand and similar substances is really due to subsidence within the interstices of the filter.

As in the first edition, we find no mode of estimating sulphides or the gases dissolved; nor does the treatise contain any indication of the success of the application of the ammonia process in the examination of sea water; it may be answered that the title of the book only refers to potable waters, but then we are at a loss to explain the appearance of some experiments on sewage.

It is much to be regretted that the present editor has thought fit to reprint the preface to the first edition, and also the appendix, which consists of nothing but an attack on Frankland and Armstrong's process. These entail on us the necessity of noticing this treatise at much greater length than the positive information contained in it would justify; for if the statements set forth were passed over without remark, it might seem as if the untrustworthiness of the process were acknowledged by all chemists, and doubt thrown on the accuracy and value of the reports issued by the Registrar General. The authors state in the preface to the first edition that it requires great length of time and great skill to execute it. This may be true, but the vast advantages which it possesses over the old processes amply compensate for the additional trouble. The manipulation, though delicate, is not, we are given to understand, beyond the powers of an average first year's student, at least when he is not influenced by a preconceived distrust in the efficiency of the process, or by a desire to do things with as little trouble as possible. It was not quite fair for the authors to state, two months after the publication of the process, that chemists in general agreed with them as to its invalidity. They should have remembered that "chemists in general" are not such rapid workers as Messrs. Wanklyn and Chapman, and would wish for more time to give a definite decision on the capabilities of a process so entirely new. The appendix consists of a note read by the authors before the Chemical Society. They commence by stating that they do not consider the complete conversion of organic nitrogen into ammonia by their method as being essential to its applicability for determining the relative quality of a water, and that they rely simply on the constancy of the ratio between the amount of albumenoid substance in the water and the quantity of ammonia produced. This is a retractation, though not a very straightforward one; for in their first published account of the process it was stated that all the nitrogen was evolved as ammonia. But it would be well to ascertain if this ratio is really constant, for although this may be the case with white of egg, is it not almost too much to assume that the nitrogenous organic matter present in natural water acts in the same manner as albumen, when we are quite ignorant of its proximate constituents? If the quantity of ammonia always bears a certain relation to the organic matter, and if this ratio is known, the determination of the nitrogenous impurity is merely a matter of calculation; but is it not a fact that some bodies, when treated according to the author's process, evolve more ammonia, in proportion to the amount of nitrogen they contain, than do others? The experiments on strychnine, narcotine, and quinine sulphate, published by Frankland and Armstrong (but not commented on by our authors), show this to be the case. But in their published papers, Messrs. Wanklyn and Chapman admit that the quantities of "albumenoid ammonia" from the following compounds are far from uniformly proportional to the amount they contain, thus:—

Urea and Picric acid gave no albumenoid ammonia

Creatine	gave	1/3	of its nitrogen as	albumenoid ammonia
Caffeine	,,	1/4	,,	,,
Uric Acid	,, about	1/6	,,	,,
Albumen	,,	2/3	,,	,,
Morphine & 11 other organic substances		1/2	,,	,,
Hippuric acid and 7 other organic substances		all	,,	,,

Next follows a list of Frankland and Armstrong's results, in which the differences of the quantities of carbon and nitrogen obtained by experiment and calculations are pointed out. But it should be remembered that in these experiments the substances were weighed instead of being measured in standard solution, and that these solids were first dissolved and the solutions evaporated in order to perform the combustion of the residue. It is afterwards deduced from this same list that the error is inherent in the process, as the results are not better when the amount of organic matter is reduced; but with this reduction the liabilities of error in weighing increase. If this discrepancy were really caused by the imperfection of the process, we should expect to find great variation in the quantities of nitrogen obtained in actual analyses of waters, but in the list of forty nitrogen determinations given in Frankland and Armstrong's paper, the numbers vary from 0·000 to 0·068 per 100,000, or, as our authors would express it, from 0 to 0·68 milligrammes per litre, whereas they accuse the process as being liable to produce an error of no less than 1·29 milligrammes per litre. The following extraordinarily opportune accident has happened during the month of May, and from the results we shall be able to obtain an indication of the concordance of the numbers arrived at by Dr. Frankland's process. In the Registrar General's report it will be seen that the water supplied by the Grand Junction Company was drawn at the cabstand in Woodstock-street, whilst a sample of water was taken at 14, Lancaster Gate, under the impression that it came from the works of the West Middlesex Company. Now, to our certain knowledge, 14, Lancaster Gate is supplied by the Grand Junction Company; thus Dr. Frankland has been analysing two samples of the same water, supposing them to have come from different sources. This mistake could not have been found out till after the report was printed, and the results obtained by the two experiments are as follow :—

	Total solid impurity.	Organic carbon.	Organic nitrogen.	Ammonia.	Nitrogen as nitrates and nitrites.	Total combined nitrogen.	Previous sewage contamination.	Chlorine.	Total hardness.
Grand Junction, collected at the cab stand in Woodstock Street, May 17.	24·7	·135	·020	·000	·184	·204	1520	1·62	19·36
Water collected at 14, Lancaster Gate, supposed to be from the West Middlesex Company; but actually from the Grand Junction, May 15.	24·6	·129	·023	·000	·188	·211	1560	1·60	19·36

It may be objected that even these numbers do not approximate so closely as those of Messrs. Wanklyn and Chapman, but they represent actual quantities obtained from water, and not a theoretical "albumenoid ammonia," which may not be an indication of the quantity of organic impurities.

Since Frankland and Armstrong's paper was published an immense number of analyses have been made with the process, and in his annual report to the Registrar General, Dr. Frankland states that he has seen no reason to be dissatisfied with the results. Probably no one, unconnected with Dr. Frankland's laboratory, knows better than Mr. Chapman that improvements in the details of the manipulations have been made during the last two years; and it is, therefore, with very questionable taste that he has reprinted the appendix to the first edition of the book without a single word of qualification.

It is much to be deplored that two young chemists, with such undoubted abilities as Messrs. Wanklyn and Chapman possess, should have rendered themselves notorious by attacking older workers in scientific investigation. It is, no doubt, very laudable in a young and ardent investigator, when he points out that high authorities may err and frequently have erred, but the manner in which these gentlemen have carried out their corrections has made their matter more distasteful. It would almost seem as if they found an incentive to work in the hope of being able to overthrow the "huge superstructures" which have been raised by men who have been longer in the field of scientific research.

OUR BOOK-SHELF

A Sketch of a Philosophy. Part III. *The Chemistry of Natural Substances.* Illustrated by two folding plates and 150 figurate diagrams of molecules in the text. By John G. Macvicar, LL.D., D.D. (Williams and Norgate, 1870.)

IT is a hard matter to give a just account of this pamphlet. The views propounded by the author are so entirely different from those usually held by chemists, and according to the author's own statement they have been so little studied by others, that it is difficult to know exactly how to treat the subject. We should scarcely be justified in saying that the whole system is mere imagination, though some hold this opinion; but the book, though evidently written with the intensest earnestness, is the work of an enthusiast, which will explain the bitter complaints he makes against modern chemists for not taking more notice of molecular morphology. The author endeavours to explain the formation of all matter by the aggregation of the ethereal element, supposing that all bodies tend to assume a symmetrical, and more or less spherical form. The simplest form of aggregation Dr. Macvicar considers to be the *tetrad*, consisting of four specks of the material element so arranged in space that they form the angles of a tetrahedron, the lines joining them indicating the attracting and repelling forces operating between the units. Two tetrads are also assumed to join base to base producing the *bitetrad*, and from these two forms the tetrad and the bitetrad, all the atoms and molecules of our planet are supposed to be produced. This tetrad by attracting another unit opposite to one of its faces constitutes a group of five units, considered to be the atoms of hydrogen, and with the atomic weight of five. The author proceeds to show the mode of genesis of many other elements and compounds by the juxtaposition of these elemental forms. By calculation he can determine by his system the specific gravities of solids and liquids referred to water as unity, in a manner similar to that by which the densities of gases and vapours may be deduced by the old system. This alone would seem to show that the method deserves more attention from chemists than it has yet received. The non-reception of this molecular morphology may be ascribed to several causes: the diction of the author is peculiar, and he writes in a dogmatic manner, which might be expected in a theological work, but is not usually found in a treatise on natural science; then he pushes his inferences to such an extent (or as some would say, rides his hobby so hard) that his conclusions appear somewhat ludicrous, unsupported as they are by experiment: thus he traces the coincidence between the assumed hexagonal form of the molecule of aqueous vapour and the shape of the minimum snow-flake and ice-flower, and "the inflorescence of plants of the monocotyledonous order, in which an aqueous tissue predominates"; he thinks that one of the forms of aqueous vapour which occupies half the volume of the other, may possibly be converted into the second variety at a high temperature, and thus explain the explosion of steam boilers. Again the dimorphism of water may be the cause of the production of animal heat; for water in the body may be transformed from one of its varieties into the other with evolution of heat; but on escaping from the body as perspiration, the inverse action takes place and cold is produced. But underneath all these extravagances there may be a stratum of truth, and we hope that either the author or some one who understands and accepts his views thoroughly, will so develop them as to ensure their reception by chemists.

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 Source of Solar Energy

I HAVE not Mr. Proctor's "Other Worlds" by me to refer to, but my impression on reading that book some little time ago, certainly was, that if it did not directly support the *meteoric* theory of solar energy, it at least favoured the idea of innumerable meteors falling into the sun. The principal portion of my letter in last week's NATURE, was not, however, so much addressed against any special views of Mr. Proctor's relative to this *meteoric* theory, as it was against the probability of meteors falling on to or into the sun at all. And whatever be the real value of the arguments I used, I certainly had not before seen them anywhere clearly stated. The question as to meteors being consumed in producing heat by friction in the solar atmosphere, or of their striking the sun's proper surface in a solid form, is of very secondary importance, and would of course be determined chiefly by the size and nature of the meteoric bodies themselves.* The potential or heat result theoretically would be the same either way; though in the former case it might hardly so well explain how whilst heated flames are shot up 60,000 or 70,000 miles from the sun itself, that body is also probably far hotter than the upper regions of this very atmosphere, in which the meteors themselves would be giving out heat.

Prestwich, Manchester, Aug. 5 R. P. GREG

* Suppose the case of a large mass of meteoric iron falling through the solar atmosphere, the first result would of course be a development of heat and light; but if the original or cosmical velocity of the meteoric body was lost through the resistance of the atmosphere (as in similar cases terrestrially happens) *before* the entire mass was consumed, there might possibly result an actual loss of solar energy, caused by the subsequent and necessary melting of the residuum.

Kant's Transcendental Distinction between Affection and Function

AT page 80 of my "Revival of Philosophy at Cambridge," I ventured to describe a question set by Mr. Mahaffy, at Trinity College, Dublin, as "very oddly worded." I might have said *very improperly worded*, and have justified the sentence; but for the fact that it was the Cambridge examination-papers only that were the subject of my criticism in that work. My boldness in this censure on Mr. Mahaffy has occasioned remark. That gentleman is confessedly a capital metaphysician, perhaps of greater power than the learned professor whose work he translated and annotated. I therefore ask for space in NATURE to assign the reasons on which I asserted that his question was "very oddly worded." Here is the question: "Explain the statement that his [Kant's] doctrine of Space and Time is based on a *transcendental distinction*." I think I cannot be in error in taking this as a reference to the *Kritik der reinen Vernunft*, Transac. Æsth. § 8, *Allgemeine Anmerkungen*, &c., and in particular to the paragraph beginning "Die Leibnitz-Wolfische Philosophie," &c., and that which immediately follows. In the former, and indeed in its immediate precursor, Kant is impugning the view that affection and function (Sense and Intellect) have only a logical difference, as if Sense were only differenced from Understanding by the inferiority of its representations, in precision and clearness. The latter paragraph (the third of those I have referred to) beginning "Wir unterschieden sonst wohl unter Erscheinungen das," &c., may be thus rendered:—

"We otherwise draw a proper distinction, in phenomena, between that which is an essential property of the intuition of it, and is generally valid for every one's sense, on the one hand; and on the other, that which belongs to it accidentally, inasmuch as it is not valid for the faculty of general sensibility, but only for a particular state or organisation of this or that sense."

This, Kant names an *empirical distinction*. To it he opposes what he calls a *transcendental distinction*, viz., that between phenomena and things in themselves, or what is involved therein, that between affection and function. As Kant points out, if we do not make the distinction between affection and function, we cannot explain the transcendental constituents of a phenomenon, and thereby we take it for a thing in itself, *i.e.*, a reality existing independently of our perception of it; and we so lose the distinction between the phenomenon and the thing in itself, of which latter we know nothing whatever.

Now, Mr. Mahaffy's question concerns a certain statement. Whose? Well, probably, it is his own, viz., that which occurs in a footnote to p. 57 of his translation of Fischer's Commentary on Kant's C. p. R. Mr. Mahaffy here says, "We must not confuse the *empirical* distinction between real object and merely subjective appearance with the *transcendental* distinction upon which his (Kant's) doctrine of space and time is based." This I believe to be the topic referred to in the question. It is manifestly open to two objections. Each of the terms "real object," and "merely subjective appearance" is equivocal. "Real object" may be the phenomenal object or the noumenal object. "Merely subjective appearance" may be what Kant calls "blosz Erscheinung" or the impression which the object makes on the particular sense of this or that subject. The actual terms used by Mr. Mahaffy more properly import the distinction between the thing in itself and its phenomenon; yet it is plain he means to follow Kant, and to speak of the distinction between phenomenal object, and particular subjective impression. It is the other matter of the extract with which the question deals; and it is this which I have no hesitation in pronouncing a very improper and misleading statement.

Kant's doctrine of Space and Time is *not* based on this distinction. On the contrary, the distinction is an outcome of that doctrine, and does not, cannot, emerge till that doctrine is established. The fruit of the doctrine cannot be its root; nor can that which is the basis of the distinction be itself based on that distinction. The truth is, that Kant's doctrine of Space and Time is one that concerns Sense only; it is Æsthetic; and as discovering the *a priori* sense-elements of experience, it is called Transcendental Æsthetic. Accordingly it touches but one pole of the distinction, and only so far helps it out! How can such a doctrine be properly said to be based on a distinction between the two poles? The very notion is preposterous, and derives, of course, no countenance whatever from the *Critic of pure Reason*.

The extremely curt and concise manner in which I have dealt with the actual subjects of examination in my book was essential to its brevity and corresponding lowness of price. The above remarks will show how insecure will be any inference of the poverty of my reasons from the paucity of my words.

Ilford, E., Aug. 1 C. M. INGLEBY

Spontaneous Generation

IF there is one thing more curious than another in the "Spontaneous Generation" theory, it is the way in which so-called matters of fact, as proved by careful experiment, are brought forward by the one side to be disproved by the other; one need only instance Pasteur's famous flask experiments, which were thought to be so overwhelming at the time, but which were afterwards refuted, I think by Frémy and others.

I notice with surprise the letters of Prof. Wanklyn and Dr. Lionel Beale in NATURE, with regard to the presence of germs in the air; there is an experiment of Pasteur's, given in his "Memoirs upon the organised Corpuscles which exist in the Atmosphere, 1862," and which I have never seen disproved, and if not disproved it must surely settle at least this part of the question. He passed a quantity of air, taking various precautions to eliminate error, which I need not here detail, by means of an aspirator through a plug of gun-cotton; he then dissolved the gun-cotton in ether, and on examining the sediment which subsided in the course of an hour or two, he found abundant evidence of the presence of organised corpuscles.

Bath CHARLES EKIN

Mirage

I HAVE just returned to England from H.M.S. *Porcupine*, having accompanied the dredging expedition as far as Lisbon. In reading the back numbers of NATURE, I notice in that for July 28 an account of an extraordinary mirage in the Firth of Forth on July 22. A reference to my journal shows me that on the same day we were dredging on the Portuguese coast, within sight of the Ferilhoe and Berlinga Islands, about forty miles north of Lisbon. The bearings of these islands and their exact distance, calculated by the aid of the known height of the lighthouse, gave us, of course, an exact position, which our "dead reckoning" also confirmed. Several solar observations, both for latitude and longitude, were taken by two of the officers during the day, both of whom always arrived at the same result, but this was so widely different from our position as previously determined by two other methods, that we were forced to the conclusion that there was a very false horizon. It was the only instance of the kind during the month I was at sea.

Clifton, Aug. 8 WM. LANT CARPENTER

The Sun's Corona

IF "my mathematical result was based upon data among which the principal point at issue was accepted as proved," it will be easy for you to state what that point is,* and to quote one passage at least in which Mr. Lockyer has associated it with his theory. In this way alone can you justify the assertion in your last editorial note.

So much you are bound in common justice to do. But further, it would be satisfactory if a distinct statement of Mr. Lockyer's

* A possible action at the moon's limb suggested by Faye, Gould and others.—ED.

opinion respecting the corona could be made public. At present all that is generally known is, that he regards the corona as "an effect due to the passage of sunlight through our own atmosphere near the moon's place." Those are the words he used (see NATURE, vol. i., p. 14). I imagined that I had understood them. It seems I had not. Will he explain them, and perhaps indicate how the sunlight gets there? I only need to learn how one ray of sunlight can reach the atmosphere near the moon's place, during central totality in any considerable eclipse, and why the atmosphere actually *at* the moon's place (that is, all that cone of air which lies between the eye and the moon) is left free of this sunlight. This being satisfactorily explained, I should waive all other objections and accept the atmospheric glare theory without further question.

RICHARD A. PROCTOR

[Mr. Proctor should have quoted the context, in which Mr. Lockyer carefully refers to Dr. Frankland's and his own conclusion (or theory) "against any such extensive atmosphere as the corona had been imagined to indicate." He then states that the "conviction" that the corona is probably an atmospheric effect, "is growing stronger and stronger," if the negative evidence of the new method of observation were alone taken into account; but this is not to elaborate a theory.—ED.]

The Horse-Chestnut

REFERRING to NATURE, No. 37, your correspondent, Eugene A. Connell, has fallen into a mare's nest in the matter of the horse-chestnut.

Country people are well aware of the impression of the horse's foot he has discovered, but the coincidence is quite accidental, and has nothing to do with the name.

"Horse" is a very common prefix, denoting largeness or coarseness, in the same way that the prefix "dog" indicates inferiority and contempt. Thus we have horse-chestnut, horse-bean, horse-radish, horse-mint, horse-parsley, horse-leech, dog-rose, dog-violet, dog-berry (the berry of *Solanum nigrum*), &c.

These prefixes are common to nearly all languages; we have *ἱππό-κρημνος*, a horse-laugh, "fièvre de cheval," a violent fever, and a host of like terms.

Bath, July 27 CHARLES EKIN

The "English Cyclopædia"

IF the Editor of the "English Cyclopædia," in his letter contained in your issue of July 7, had restricted himself to defending his own handiwork, and had abstained from denying the correctness of my statements, I should not have ventured to ask for space in your columns to reply to him.

In opposition to my statement that I looked in vain for "*Arvicolæ, Crociduræ, Crossopi, Hypudæi, Sorices*, shrews, and voles," the Editor asserts that "all the species mentioned in the Close Time Report are described or noticed in the Cyclopædia." This may be and probably is quite true. I merely asserted that they were not to be found under their respective names. I have stated that I found *Hypudæus* and the voles under the heading *Muridæ*. In return for my information he now tells me that if I wished to become acquainted with *Crocidura* and *Crossopus* I ought to have turned to the article *Sorecidæ*. But how is an unlearned reader like myself to know where to turn? The Editor only confirms the accuracy of my statement as to the great want of cross references. If the Index and the Supplement had contained such references as *Hypudæus* [Muridæ, E.C.], *Crossopus* [Sorecidæ, E.C.], &c., I should probably never have given public utterance to my troubles. In reply to my assertion that I looked in vain for an article on *Rhizocrinus*, I am told, much to my astonishment, that the proper place to find it is under *London Clay*. In my ignorance I had sought for it under its own name, *Apiocrinites*, and *Encrinites*. According to this mysterious system of arrangement, if I had complained that there was no article on *Sparrows*, I should probably have been told that I ought to have looked for a notice of them under the heading *London*.

In my letter I gave a list of twenty-three important subjects on which there were no articles. In defence of these real or apparent omissions the Editor, after making the strange assertion that two of these, *Acclimatisation* and *Deep Sea Dredging*, belong rather to the "Arts and Sciences" than to the "Natural History" division, goes on to say that most of the subjects stated by me to have been omitted "do occur." He must be well aware that I never asserted that they "do not occur." I simply said that there were no special articles on them. He might have had the candour to notice that I unearthed from their hidden recesses the subjects to which he expressly refers in his letter, viz., *Eophyton, Eozoon, Hyalonema*, and *Protoplasm*.

As I must not trespass further on your space, I will conclude by observing that I fully concur with the Editor in the opinion that "what a Cyclopædia ought or ought not to contain is an open question;" but when an Editor has the moral courage to assert, in illustration of the mode in which he discharged his functions, that "*Meloe* was inserted and *Sphegidæ* rejected, because there was not room for both," and gives no less than twelve columns to the former instead of dividing the space between the two; and when he tells us that *London Clay* is the proper place to seek for information regarding *Rhizocrinus*, the readers of NATURE may draw their own inferences as to what a Cyclopædia, under his superintendence, is likely to be.

I must add that I have not the slightest idea who the Editor of the Supplement is, and that until his letter appeared, I did not believe in his existence.

South Devon, July 8 NEMO

Entomological Inquiries, etc.

I WAS much interested, two nights ago, at finding on the wall of my drawing-room a flattish, dark-grey winged insect, six or seven tenths of an inch in length, which, on being placed in the hand, exhibited two small but brilliant sparks of light towards the extremity of the tail. In the imperfect light in which it was examined, the wings seemed to have elytra and the body to be somewhat like a small caterpillar, with a tapering tail. In size and general aspect it resembled the Italian fire-fly, with which I made acquaintance last summer on the Lake of Como, without, however, a sufficient examination to justify more than the most superficial comparison. My knowledge of entomology is so defective, that I feel unable to form an opinion whether it might be that insect or the male of the common glow-worm (which, however, is not common in my neighbourhood). If so meagre a description may enable any of your readers to give me satisfactory information as to this point, I shall feel much obliged to them.

There is adequate evidence that some kind of fire-fly has been seen during hot seasons in England. Kirby and Spence give a reference, which I have no opportunity of verifying, to Phil. Trans. 1684, as to their appearance in Hertfordshire, and their having been considered the genuine *Lampyris italica*. The following unpublished account may be interesting as having come to me from a perfectly reliable source: "In 1822—the year is pretty certain—during the month of June or July, the weather being very hot, on at least two evenings a number of fire-flies were seen at a village near the Thames, between Reading and Henley; they were flying about the fields and the lawn before a gentleman's house, and some of them came into the house; three or four or more might be seen at a time, like little flying lamps. The insect was brown [reference is then made to a sketch from memory thirty-two years afterwards, from which it must have greatly resembled my specimen], and seems to have had opaque elytra and network wings; the light was in the tail, like that of a glowworm, as bright, but probably not as large. A very intelligent gentleman who was upon the spot, an acquaintance of Dr. Wollaston's, who had been in America and the West Indies, was greatly astonished; he caught some of them, and considered them identical with the West Indian firefly. He said he had heard of their being in England, but never seen them."

A lady, whose experience must be referred to a later date than the foregoing account, has informed me that she once observed them for a single day in Wiltshire.

The newspapers of 1868 or 1869—I am not certain which—spoke of them as abundant in some places; particularly, I think, at Caversham in Kent, where they were even considered "nuisances!" if I recollect right. Some of the readers of NATURE may perhaps be able to furnish information as to this alleged fact.

There is something very remarkable in the occasional appearance of these beautiful insects in our climate. They can hardly be thought to reach us by direct migration. Can it be supposed —as it has been ingeniously suggested to me—that their ova are frequently being imported from warmer countries, but are only fully developed in the temperature of our hottest summers?

While speaking of entomological matters, allow me to mention that in the month of August last year the small blue lobelias in my garden were the favourite resort not only of my hive-bees, but of a species of wild bee so singularly resembling them in every respect (excepting, perhaps, a barely perceptible amount of greater firmness and roundness of form), that they could only be distinguished by the presence of a tuft of lemon-yellow hairs in the front of the head between the eyes. I thought at first that this might have resulted from a lodgment of pollen, but it soon became evident that it was a specific distinction. These pretty insects were very numerous, but I never found any nest. Will some apiarian reader oblige me with an identification?

To turn to a somewhat different subject. In a little book called "Flowers of the Year," published by the Religious Tract Society, is the following passage: "An interesting phenomenon is sometimes exhibited by red and orange-coloured flowers, and also, in a less degree, by yellow-tinted blossoms. It is that of a light of their own colour playing about the plant. This is not the result of an inflammable vapour igniting on the approach of a candle, but seems rather, as Sharon Turner has remarked, 'an actual secretion of light additional to their usual show.' The cause of this phenomenon has not been discovered, but it seems dependent on an electrical state of the atmosphere. It has not been seen during the bright sunshine, but has been observed after sunset in several flowers, as the marigold, the different species of poppy, the scarlet geranium, and even in the heartsease." I have several times met with a similar statement, and much wish to know what trustworthy foundation there may be for it. I have repeatedly tried to verify it by observation, but in vain.

Since penning the above remarks, the yellow-visored bee has again appeared on the blossoms of a Linaria Alpina, the descendant of a plant brought by my wife from Switzerland seven years ago, and now blooming profusely with us. I have seen several specimens of my old friend, but cannot as yet satisfy myself that the yellow tint is due to hair of that colour, as I supposed last year.

Hardwick Vicarage, July 12 T. W. WEBB

The Solar Spots

I HAVE been much interested of late in observing the solar spots, and especially in the greatly-increased numbers which have lately made their appearance. I have counted from 100 to 200 spots, through a six-feet telescope, with a power of 100, quite frequently in the past few months. On the 22nd ult., with a power of 200, I counted 675 sun spots, and on the 27th saw 470 with the 100 eyepiece. In general, I think the number of spots visible is about in proportion to the power used when the atmosphere is favourable for high powers. During the month several spots were visible to the naked eye.

I feel quite desirous to learn what our spectroscopists find about the sun's margin, also if observations indicate a terrestrial magnetic force corresponding with the sun's activity.

Would not many readers of NATURE be much interested with results from Huggins and Lockyer and the Kew observers?

Spiceland, Indiana, July 6 W. DAWSON

DARWIN BEFORE THE FRENCH ACADEMY

THE discussion on the claims of Mr. Darwin for election into the Zoological Section of the Paris Academy was continued at the meeting on August 1 in *comité secret*, and the *Revue des Cours Scientifiques* gives a report, of which the following is an abstract, of M. de Quatrefages' brilliant and able reply to M. Blanchard:—There are two men included in Mr. Darwin, a naturalist observer and a theoretical thinker: the naturalist is exact, sagacious, and patient; the thinker is original and penetrating, often just, sometimes too rash. That the theory with which his name is connected, that of Natural Selection, has in it at least something seductive and plausible, is shown by its having been worked out by such men as Darwin, Wallace, and Naudin, labouring independently and in different paths. If the ideas and the works of Darwin are such as some of his opponents represent, how can they have obtained the support in less than ten years of such men as Lyell, Hooker, Huxley, Karl Vogt, Lubbock, Haeckel, Filippi, and Brandt himself, who has just been elected correspondent in opposition to Mr. Darwin? In Darwin's great work there are certainly some things which are found in Lamarck, the laws of heredity, and the transmission and progressive development of characters. The point of departure of Lamarck is an incessant spontaneous generation, that of Darwin is a unique archetype which he supposes to pre-exist, and the origin of which he does not seek. That which belongs to Darwin alone is the laws of variation which he has established, and the law of correlation of growth. His error has been the confusion between the laws which regulate the foundation and propagation of races and of species; substitute the former for the latter and his theory is incontrovertible. Without defending Mr. Darwin's theories, some of which he has indeed publicly combated, M. de Quatrefages then proceeded to enumerate the various branches of scientific inquiry in which Mr. Darwin has made original observations, and concerning which he has contributed works of great importance to science. In geology we find seven great memoirs—1. On coral islands; 2. Geological observations on volcanic islands; 3. Geological observations in South America; 4. On the connection of the volcanic phenomena in South America; 5. On the distribution of erratic blocks in South America; 6. On the geology of the Falkland Islands; 7. Origin of the saliferous deposits of Patagonia. In botany the speaker invoked the testimony of Dr. Hooker that the most beautiful discoveries made during the last ten years in vegetable physiology belong to Mr. Darwin. Finally, in zoological literature we have the report of the voyage of the *Beagle*; and the monograph of the Cirrhipedes, one of the most important monographs ever published. After speaking of his more popular works on the origin of species and the variation of animals and plants under domestication, M. de Quatrefages referred to his important and laborious investigations of the strange variations in fowls, pigeons, and rabbits; and summed up his eloquent address as follows:—"En résumé, M. Darwin est un naturaliste éminent qui veut écarter de la science l'invocation de la cause première, et chercher l'explication des faits naturels du monde organisé dans les causes secondes, comme on le fait depuis longtemps en géologie, en chimie, en physique. Mais il ne va pas au delà, et il ne faudrait pas juger Darwin sur la parole de quelques disciples qui semblent parfois ne pas avoir lu ses ouvrages. Il y aurait injustice à le rendre responsable des exagérations et des aberrations de ceux qui s'abritent sous son nom."

M. de Quatrefages was followed by M. Ad. Brongniart, who attacked the Darwinian system, denying the existence of variation in plants. The appearance of species is a fact which can only be explained by a supernatural cause, and Darwinism is nothing but a fairy tale. M. Ch. Robin considered that in respect of proved facts which he had introduced into science, there would be a hundred zoologists who should have precedence over Darwin. M. H. Milne-Edwards replied to M. Brongniart, that the sea sometimes discloses fairy tales, and spoke of the very great value of the monograph of the Cirrhipedes. Although himself opposed to Darwinism, he strongly supported his nomination. M. de Quatrefages, in reply, denied the charge against Darwin made by M. Blanchard, that he had declared that man was descended from the apes. In deciding Mr. Darwin's claims, we ought not to be influenced by those points in which we have to combat his views, any more than Lamarck was judged in this manner. In spite of his errors, he will be none the less one of the glories of science and of the Academy. His nomination will not make the Academy Darwinian. Men of science know that the Institute appreciates work independently of doctrine, and men of the world know that the supporters of Darwin in the zoological section, MM. Milne-Edwards, and de Quatrefages, have always professed themselves opposed to his ideas. The discussion was then adjourned to last Monday evening.

TRANSMISSION OF POLARISED LIGHT THROUGH UNIAXAL CRYSTALS

THE appearances presented by the transmission of polarised light through crystals, have long been known as the most magnificent in Optics. It is our intention in this paper to give an account of the more recent observations which have been made respecting the phenomena exhibited by uniaxal crystals, accompanied by such an explanation as will, we hope, render them intelligible to persons very slightly acquainted with science. We shall therefore avoid as much as we can the use of technical terms, and assume as little as possible to be previously known to the reader.

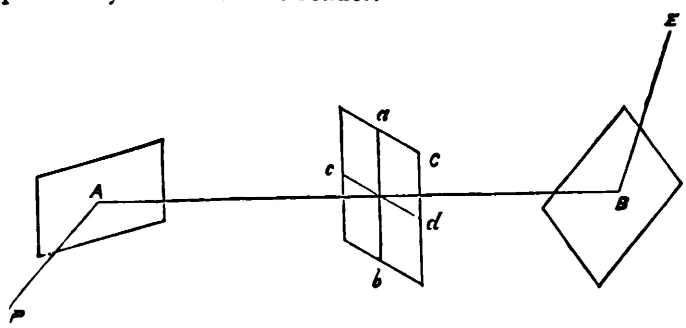

FIG. 1.

Common light consists of undulations of the supposed ætherial medium, in which the vibrations of each particle are perpendicular to the direction of the wave's motion, and in every conceivable direction which this condition admits.

Suppose now a ray of sunlight reflected from a plate of unsilvered glass. The principle of the composition and resolution of forces plainly enables us to regard it as made up of two sets of vibrations, one in the plane of reflection, and the other perpendicular to it. Now for reasons which cannot be explained to a reader unacquainted with mathematics,* it is found that the vibrations of the reflected ray in the plane of reflection, when the angle of

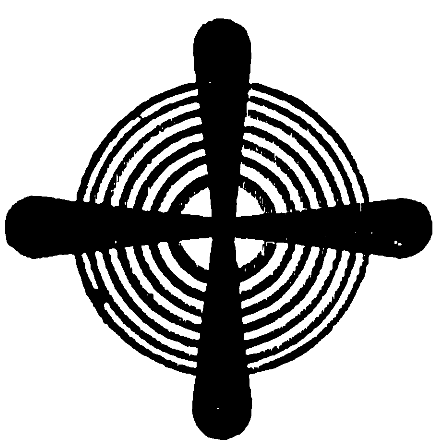

FIG. 2.

reflection is equal to 56° 30′, wholly disappear, so that the reflected ray consists entirely of vibrations perpendicular to the plane of reflection, and is said to be polarised. The reader will carefully remember the distinction between common light and polarised light. Common light is light in which the vibrations occur in every conceivable plane passing through the ray; polarised light is light in which the vibrations occur in only one plane of fixed inclination passing through the ray.

Now let a ray of sunlight fall upon a plate of unsilvered glass at an angle of about 56°, let the reflected ray be received by another glass plate at the same angle, and be thence reflected to the eye. Suppose the position of the mirror to be such that the planes of first and second incidence and reflection are perpendicular to each other. Then remembering what we have just stated, we see that the vibrations of the ray reflected from the first mirror lie altogether in the plane of second incidence. These vibrations, therefore, will all be destroyed by the second reflection, therefore nothing but a dark spot will be perceptible to the eye.

If now the position of the second mirror be shifted, so that the planes of first and second incidence coincide with each other, while the angle of second incidence remains unaltered, the vibrations of the incident ray will all lie perpendicular to the plane of second incidence, and therefore a bright spot will be visible to the eye.

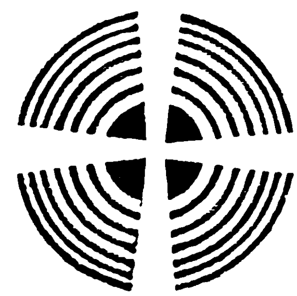

FIG. 3.

Let now the second mirror be gradually turned round an axis situated in the direction of the ray incident upon it from the first position into the second, then the angle of second incidence will remain unaltered during the revolution, while the plane of second incidence will revolve with the mirror, and, as might be expected, light will begin to be visible to the eye and continually increase, till it attains its maximum in the second position of the mirror. It is usual to call the first mirror the polariser, the second the analyser.

Let a plate cut from a transparent crystal be interposed between the polariser and analyser thus arranged, luminous

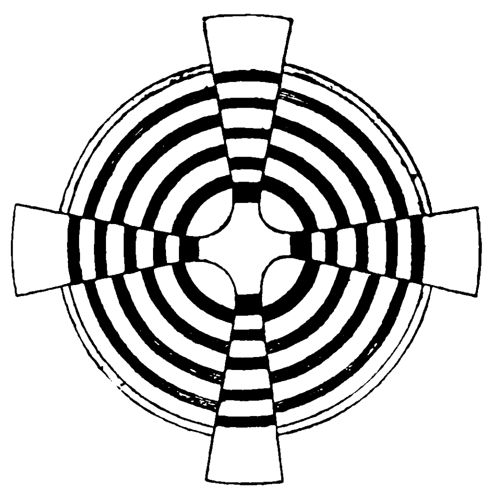

FIG. 4.

curves of varying colours intersected by dark bands will be visible to the eye. Before describing these gorgeous appearances, we must say a few words on the modification light undergoes in passing through a crystal.

When a ray of light passes through a crystal, it is in general divided into two, so that if a plate of crystal be interposed between the eye and a luminous point, the point will appear doubled. But in all crystals there is one, and in some two fixed directions in which no bifurcation of the ray takes place. If we look along one of these lines, called the optic axes, we shall see only one image of the object of vision. Crystals with one axis are called uniaxal crystals; those with two axes are called biaxal

* The mathematical reader is referred to Green's "Memoir on the Reflection of Light," Cambridge Philosophical Transactions, vol. vii.

crystals, and in this paper I shall confine myself to the former.

We observe that a plane drawn through the direction of the ray or pencil and the axis of the crystal is called a principal plane. It is found that one of the two rays into which the incident ray is divided is refracted according to the ordinary law, and is therefore called the ordinary ray, the other according to a more complicated law, and is called the extraordinary ray. But we especially remark that the vibrations constituting the ordinary ray lie wholly perpendicular to the principal plane of the crystal, those of the extraordinary ray parallel to the principal plane. Consequently both rays will consist of polarised light, according to the definition we have just given of polarisation, and this polarised light will arise from the vibrations of the incident ray being resolved perpendicular and parallel to the principal plane.

Of all uniaxal crystals the best known is Iceland spar, which possesses the power of double refraction in an eminent degree. It occurs in the form of a rhombohedron, and the axis of the crystal corresponds with the shortest diagonal.

Now suppose a plate of this crystal, cut perpendicular to its axis, is interposed between the polarising and analysing plates, when the first and second planes of incidence are perpendicular to each other.

The colour of light depends on the length of the wave. The length of a wave of violet light is ·0000167, and that of a wave of red light ·0000266 of an inch. The wave-lengths of the other colours lie between these. White light is a compound of all colours, and therefore its vibrations are of all lengths lying between these extremes. It will therefore manifestly simplify the explanation we are going to submit to the reader, if we commence by assuming the light to be monochromatic, and its vibrations of the same length. Let a small pencil of monochromatic light, P (Fig. 1), be reflected from a plate of glass, A, at an angle of about 56°, pass through the crystal C, and then be reflected a second time from the plate B at an angle of about 56° in the direction B E. The plane P A B is supposed to be perpendicular to the plane of the paper, the plane A B E to coincide with it. The crystal is supposed to be bounded by parallel surfaces perpendicular to its axis, and in a position perpendicular to the line A B. Let *a b*, *c d*, be two straight lines drawn in the crystal, one in the plane of the paper and the other perpendicular to it. The eye is supposed to be placed at E.

The vibrations of light proceeding from A will all be parallel to the plane of the paper. In general, each ray as it enters the crystal will be resolved into two, and thus will arise two sets of waves which will interfere with each other. Hence a succession of dark and bright curves will be visible to an eye placed at E. Moreover, as the crystal is symmetrical round A B, it may be expected that these curves will be circular. It may also be expected that the diameters of the bright circles will depend on the length of the wave, and therefore on the colour of the light.

We have already defined the principal plane of the crystal for a given ray, to be the plane passing through the ray and through the axis of the crystal, which in this case coincides with A B. As therefore the vibrations of the rays incident on the crystal are all parallel to the plane of the paper, those which enter the crystal along *c d* are all perpendicular to a principal plane of the crystal, and those which enter it along *a b* are all parallel to a principal plane. Therefore every ray which enters the crystal along *a b* and *c d*, will be transmitted to the second plate unresolved, and unchanged, and will therefore be incapable of being reflected to an eye situate at E. Consequently a dark cross will appear to the eye at E, corresponding to the lines *a b*, *c d*, intersecting the system of rings we have just described. Now suppose white light to be substituted for monochromatic light. White light is composed of light of all colours, and we have seen that the diameters of the bright rings are different for each colour. Consequently, instead of dark and bright rings, we shall have a series of coloured rings intersected by a dark cross. This phenomenon is shown in Fig. 2.

If the second plate be turned round an axis in the direction A B, till the first and second planes of reflection coincide, the reader who has followed this investigation will perceive that the dark cross intersecting the rings will be changed into a white cross. This bright cross is shown in Fig. 3.

If the second plate is turned round the same axis into any position intermediate to those we have just described, the rings will be intersected by two crosses, inclined to each other at an angle equal to that between the planes of first and second reflection. There will be coloured arcs between the crosses, but those distances which give a maximum light for the arcs outside the crosses will give a minimum light for the arcs inside the crosses. This phenomenon is shown in Fig. 4. These are the appearances observed when the surfaces of the crystal plate are perpendicular to the axis of the crystal, and to the axis of the incident pencil. Dr. Ohm has investigated the phenomena which occur when the axis of the crystal is inclined at any angle to its surface, and the crystal itself is placed in any position between the polarising and analysing plates. His mathematical investigations will be found in the seventh volume of the "Munich Transactions," and the following are the principal results which he has obtained.

We shall suppose the light monochromatic. There will be a succession of dark and bright curves which are either parabolas, ellipses, or hyperbolas. For a given position of the crystal, the ellipses and hyperbolas are concentric; but the centres are never in the centre of vision, unless the axis of the crystal is either perpendicular or parallel to its surfaces. When the bright curves are parabolas, their vertices are equidistant from one another. When the bright curves are ellipses or hyperbolas, the difference of the squares of the semiaxes of two consecutive curves is a constant quantity. We have seen that when the axis of the crystal is perpendicular to its faces, the bright curves are circles. As we cut the plates so that the axis may be inclined at angles more and more acute to the surfaces, the bright circles become ellipses, which elongate continually till they become parabolas.

Suppose two plates of crystal, with parallel surfaces of equal thickness, and with their axes inclined at the same angle to the parallel surfaces, be placed in contact, so that their axes may lie in the same plane, but not in the same straight line, and then introduced between the polarising and analysing plates, a succession of dark and bright ellipses and hyperbolas, with their centres in the centre of vision, will be seen. It was this experiment which led Dr. Ohm to the results we have endeavoured to lay before the reader. W. H. L. RUSSELL

NOTES

TWELVE months ago the Erdington Orphanage, founded, built, and endowed solely by Mr. Josiah Mason, at a cost of nearly 250,000*l.*, was opened at Birmingham. Mr. Mason, exactly following the example of Mr. Peabody, has now in contemplation another public work of even greater ultimate importance, namely, a college and schools for scientific and technical instruction, open to all classes, and if the hopes of the founder should be realised, capable of expansion into one of the noblest institutions in the kingdom. As yet the plan is only broadly formed, and some time must elapse before it can be carried into effect; but the *Birmingham Daily Post* states that a beginning has been made, and that for the purpose above mentioned, Mr. Mason has agreed to buy a large block of land in Edmund Street, exactly facing Ratcliff Place, between the Town Hall and the Institute, and reaching back to Great Charles Street. The purchase money, we believe, is more than 20,000*l.*, a magnificent earnest of the ultimate scheme which Mr. Mason has in contemplation. We make this announcement with the greatest pleasure, and ask—Why are citizens who thus consecrate their wealth to such noble and enlightened purposes so rare when their reward is so great?

THE following are the arrangements for the approaching fortieth annual meeting of the British Association for the Advancement of Science, to be held in Liverpool, commencing on Wednesday, Sept. 14. President-elect—Professor Huxley, LL.D., F.R.S. Vice-Presidents-elect—The Right Hon. the Earl of Derby; Right Hon. W. E. Gladstone, M.P., D.C.L.; Sir Philip G. Egerton, Bart., M.P., F.R.S.; Sir Joseph Whitworth, Bart., LL.D., F.R.S.; S. R. Graves, Esq., M.P.; J. P. Joule, Esq., LL.D., F.R.S.; Joseph Mayer, Esq., F.S.A. Chairman of Local Executive Committee—The Mayor of Liverpool (Joseph Hubback, Esq.) Local Treasurer—Henry Duckworth, Esq., F.G.S. The sections are—(A) Mathematics and Physics; (B) Chemical Science; (C) Geology; (D) Biology; (E) Geography and Ethnology; (F) Economic Science and Statistics; (G) Mechanical Science. These will meet each day from eleven to three o'clock, in St. George's Hall, the Town Hall, Free Public Library, and other places to be duly announced. General and Evening Meetings—Wednesday, Sept. 14: The first general meeting will be held in the Philharmonic Hall, at 8 P.M., when Professor Stokes, M.A., D.C.L., will resign the chair, and Professor Huxley, LL.D., F.R.S., will assume the Presidency and deliver an address. Thursday, Sept. 15: The Mayor's First Reception at the Town Hall. Friday, Sept. 16: Lecture in Philharmonic Hall, at 8.30 P.M., by Professor Tyndall, LL.D.; the Mayor's Second Reception at the Town Hall. Saturday, Sept. 17: Address to Working Men, in Concert Hall, Lord Nelson-street, at 8 P.M., by Sir John Lubbock, Bart., M.P., F.R.S. Monday, Sept. 19: Lecture in Philharmonic Hall, at 8.30 P.M., by Professor Rankine, LL.D., F.R.S. Tuesday, Sept. 20: Soirée in St. George's Hall, at 8 P.M. Wednesday, Sept. 21: Concert in St. George's Hall, at 8 P.M. Thursday, Sept. 22: Excursions to several places. Members requiring further information should apply to any of the following Hon. Local Secretaries—Wm. Banister, B.A.; Reg. Harrison, F.R.C.S.; H. H. Higgins, M.A.; A. Hume, D.C.L., LL.D., Municipal-buildings, Dale-street, Liverpool.

PROFESSOR WINLOCK is now engaged in photographing the sun on a plan which, so far as we know, has not before been put into practice. He uses a single lens object-glass, 4½ inches diameter, 40 feet focal length, of crown glass, made by Clark, and corrected for spherical aberration by means of an artificial star of homogeneous (sodium) light in the focus of a 5-inch collimator. The image of the sun is 4¼ inches in diameter. The tube of the telescope points to the North, and the image of the sun is thrown in by means of a reflector of plate glass. This glass is *not* roughened or blackened on one side, because when that was done the heat of the sun distorted the plane surface. The slit is at the object-glass end of the telescope, and that position has the advantage that when it is thrown across no dust is shaken down on to the plate, as is apt to happen in the usual way of working. It is Mr. Winlock's intention to photograph the sun every fair day now. It seems also probable that this mode of photographing might be of advantage for the partial phases of an eclipse.

MR. H. POWER, M.B. (Lond.), and Mr. B. J. Vernon have been appointed ophthalmic surgeons to St. Bartholomew's Hospital.

DR. LAPEYRERE insists in the *France médicale*, on the desirability of disposing of the bodies of the slain during the Franco-German war by incremation rather than sepulture. Although there is a feeling against this mode of obsequy during present times, he points out that it was practised by the most civilised nations of antiquity. The burial of the dead after a battle is always a difficult task; it is probably never done so completely as to destroy the probability of the tainted air giving rise to all kinds of infectious diseases; and when we recollect the enormous masses of men concentrated in a small space in the present conflict, and the season of the year, the matter becomes one of very serious moment.

THE fearful destructiveness of so-called "natural" causes of death, as compared with even the most sanguinary battles, is shown by the fact that during the siege of Sebastopol, the French army lost 20,240 men by death in the field or as the result of their wounds, 75,000 from epidemic and other diseases. During the Italian campaign of two months, the French losses were 3,664 killed or mortally wounded, 5,000 from disease.

THERE was a shock of earthquake on the 12th July at Smyrna. It was not very strong, but lasted a considerable time. The previous shock was on the 24th June, and was felt at different parts of Asia Minor and in Cyprus, Crete, and Egypt. Of the July shock there were only particulars at the last advices of its having been felt at Aivali and some other inland points of Asia Minor. We learn further that the earthquake of the 24th June was felt at Damascus at 6^h 15^m P.M. It was felt also in the town of Zebedani in the Anti-Lebanon, N.W

THE Geological Survey of Italy will begin its regular definitive work next August. Commencing with Florence, it will first of all complete the study of a portion of that province. As the Ordnance map on the scale of 1 to 50,000 is not yet completed, the Geological Survey will make use of the map made by the Austrian Ordnance surveyors on the scale of 1 to 86,400, enlarged by photography to the scale of 1 to 50,000.

WHATEVER claims Sir Christopher Wren may possess to be considered the originator of the Thames Embankment, it is hardly fair to leave out of sight those which belong to Sir John Kiviet. The latter gentleman was a refugee from Rotterdam who came to England in 1666, and possessed some of the ingenuity of his brother-in-law Admiral Van Tromp. It does not appear how soon after the Fire of London it occurred to Sir John to propose a river embankment, but as early as 2nd December, 1666, we find him examining the soil of the foreshores with a view to discovering whether it was suitable for making clinker-bricks. On the 6th of March following Evelyn definitely proposed to the Lord Chancellor "Monsieur Kiviet's undertaking to warfe the whole river of Thames, or Key, from the Temple to the Tower, as far as the fire destroied, with brick, without piles, both lasting and ornamental." We may presume it was favourably received by Lord Clarendon, as upon the 22nd of the same month Evelyn had audience of the King with reference to building the Quay, and a few days later Sir John Kiviet and the Diarist "went in search for brick earth in order to a greate undertaking." No further mention is made of the scheme, and we may perhaps conclude that it was abandoned ei[illegible] on account of the unpopularity of the inventor (whose Dutch extraction would at that time have been a natural bar to success), or of the fall of Clarendon at the ignominious close of the war with Holland. At any rate, Kiviet has some right to divide the honours with Wren, though, in view of the work just completed, we cannot regret that its execution was reserved for our own times.

THE *Food Journal* for August, which contains some excellent articles, thus sums up the possible influence of War on our food supplies:—"*Free trade in live-stock* is by no means synonymous with *free trade in meat.* France will now get meat whence she cannot get live-stock. Her soldiers and sailors will eat the salt beef and pork of Cincinnati, Chicago, and St. Louis.

Prussia will probably suffer—she will certainly suffer if the war be prolonged—from her native animals being stricken by contagion; and England, that has hoped so long and so much from the resources of the East, will be left to seek for plenty in her own evergreen fields, and trust that some day her pre-eminence over the seas may render her independent of the transportation of a few hundred bullocks and sheep weekly from shores nearest her, by securing the produce—dead, not alive—of healthier, though more distant lands."

THE *Pharmaceutical Journal* (which now appears weekly) states that the plans for the erection of a Pharmaceutical Institute in connection with the University of Marburg have been approved, and the work was to have been commenced at once, but will now probably be delayed by reason of the war.

A COMMITTEE, appointed by the Royal Medical and Chirurgical Society to investigate the hypodermal method of administering injections, has just presented its report, on which the *Medical Times and Gazette* remarks:—"We may safely take, as a broad guide in practice, the rule that the physiological activity of nearly every substance which can thus be used, is three, if not four times greater when it is given by the skin than when it is swallowed." The proper commencing dose of strychnine is $\frac{1}{120}$ grain of the sulphate. The dose of atropine is also $\frac{1}{120}$ grain at first. The dose of morphia is $\frac{1}{10}$ grain to $\frac{1}{6}$ grain.

A TRANSLATION of Professor Huxley's Elementary Lessons in Physiology has just been brought out in Paris, edited by Dr. Dally, who introduces it as "*La Science sans Phrases.*"

THE following parliamentary papers have been recently issued:—Second Report of the Rivers Pollution Commission (discussing the A B C process of treating sewage); report of the Meteorological Committee of the Royal Society for the past year, an important document to which we shall return; and Volume VII. of the Irish Primary Education Commissioners' report.

SOME standard photographs and directions for measurement have been received by the Colonial Office from the Ethnological Society, through Prof. Huxley, the president. These are intended for the measurement and descriptions of aborigines in our Colonies.

THE Statistical Society has under consideration the means of promoting the study of political economy and of the numerical and statistical mode of investigation by granting prizes in our public schools. The first step will be to obtain the opinions of the head masters. The success of the Royal Geographical Society's examinations is an encouragement for this new effort.

OIL seeds having proved such a valuable branch of commerce in India, the Government is trying to promote the gum trade. The plants of the Australian gum tree, the *Eucalyptus*, introduced into the Punjaub, are taking well. It is hoped the gum will be of equal quality to the fossil *Damara Australis*, or New Zealand *Kauri pine*.

COAL containing 68 per cent. of carbon is reported to have been discovered near the town of Darjeeling, in the hill district of the Himalayas. Valuable copper and iron have also been discovered, so that important English settlement has the chance of other resources beyond tea and cinchona. The development of the minerals promises in time to give further employment for English settlers. At present in the hill regions the only metal working is in the iron mines of Kumaon. The hills can supply fuel for working charcoal iron. Our newly-annexed Bhootan territory is supposed to contain iron.

NOTWITHSTANDING the financial raid, the Lieutenant-Governor of Bengal has directed that nothing shall be done in the way of disbanding the establishment assigned for the survey of the Khasi, Naza, and Garrow hills, but, on the contrary, that the survey operations shall be taken up whenever the reduced strength of the staff will admit of it. The Government of India has confirmed this decision.

THE last scientific curiosity is a "Statement of a recently claimed discovery in Natural Science, incentive to mining enterprise," compiled by "Research," who endeavours to prove that geologists are entirely wrong in teaching that the changes in the ocean-line are due to continuous slow elevation or subsidence of the land. It appears that these changes are really due to shiftings of the mass of water in the ocean resulting from "a change of the centre of gravity of the earth's volume;" though how a knowledge that we are at any time liable to a sudden overflow of the waters of the ocean can be "an incentive to mining enterprise" is not clearly shown.

AMONG the novelties in the Exhibition at Paris in 1867 was the American Gatling gun, which, it was asserted, was to revolutionise the art of war. Like the mitrailleuse, the Gatling has as many locks as it has barrels. But whereas in the mitrailleuse the barrels are fixed and the locks have only a horizontal motion, in the Gatling both locks and barrels revolve. The locks have also, when the gun is at work, a reciprocating motion. The cartridges are kept in "feed-cases" side by side, and transferred to a "hopper," or shoot, when they are to be used. A handle is turned, and a process goes on analogous to that of the bullet-making machines, well known to visitors of the Royal Arsenal at Woolwich. When a ten-barrel Gatling is being fired, five of the cartridges are in process of loading and firing, five in process of extraction after discharging their bullets. The locks play backwards and forwards in the cavities where they work, and if one becomes disordered the action of the others is not stopped, provided that everything acts as it ought. Three sizes of Gatlings are generally made, the largest throwing a projectile of about half a pound weight. Case shot are made for the large-sized Gatling as well as solid bullets. It is, however, too heavy, and requires four horses. The chief defect of the Gatling, of all sizes, is that it cannot, like the mitrailleuse, fire volleys, but only single shots, always in the same line.

TWO experiments have recently been tried with a gunpowder invented by M. Pertuiset—the first by the Austrians against iron plates at Pola; the second recently in London. The former were made last September, in continuation of a series commenced some time before. The target was intended to represent the side of a first-class ironclad ship. There were two plates, one of 4·5 inches thick, the other 4·75 inches, English measure. The backing was 10 inches of hard wood, then an iron skin 1·5 inches thick, and behind it 12 inches of oak. The gun was of 8 inches calibre; the charge was 12 kilogrammes of common powder. The projectiles were of cast iron, made in Austria. They weighed 90 kilogrammes, and those intended to explode contained 1,500 grammes of Pertuiset powder. Two solid shot were first fired at the target, and only indented the face of it. The first of M. Pertuiset's shells not only broke the front plate and damaged the backing, but dislodged a mass of iron 22 inches by 15. The second round struck on a sound plate, and not only destroyed the iron, but so smashed the backing as to render the target unfit for further experiments. The opening in the front plates was this time far more considerable than after the first round. The experiments in London were made on horses. The skull was split, and on handling it large pieces of bone came away easily. The surface bones were removed, and the brain beneath was found to be utterly destroyed—a mass of gray and white matter devoid of consistency. When the loose material was lifted out there was a hole like the crater of a mine, 7 inches long by 6 broad. Part of the bullet had been driven up to the back of the head. And this work was done by a weapon that a man can carry in his pocket!

THE FLAGSTAFF OBSERVATORY AT MELBOURNE

ALL earnest workers in science must often have felt the extreme labour attaching to a complete reduction of an extended series of experiments. To derive some portion of truth from such a series may be a comparatively easy task, but to bring out the truth, the whole truth, and nothing but the truth, is a very difficult one. Yet how much more is this difficulty increased, when the experiments are made by Nature herself; so that while on the one hand the scale has become cosmical, we have, on the other hand, lost all control over the apparatus and the experimenter. Thus it happens that our progress in those sciences, which are strictly observational, is more slow than in those which are experimental, and truly we are yet in the infancy of Cosmical Physics.

In Dr. Neumayer, the late director of the Flagstaff Observatory, Melbourne, we have a most earnest observer of the highest type, and those who are aware of the difficulties with which he had to contend, must feel astonished at the very great and valuable work which he has successfully achieved. In the volumes of his results now before us, we have meteorological and magnetical observations of lasting value.* In meteorology we have a very complete set of elements extending throughout the years 1859 to 1863, and including, besides the usual phenomena, observations of the Zodiacal light, meteors, hailstones, atmospheric electricity, and the Aurora.

Not the least interesting of these are the observations of the Zodiacal light, and we may here note one or two remarks rather puzzling to those who assign an exclusively celestial origin to that phenomenon.

1859. Sept. 19.—The upper portion appeared to be much broader than the base.

Sept. 24.—It did not terminate in a point, but in a diffused edge of considerable breadth. The axis of the phenomenon was decidedly not in the ecliptic, but there was a particularly bright line about or in the ecliptic.

1860. March 20.—No regular column of light, but a broad patch of light towards W.; sky clear.

July 18.—A column of light much resembling the Zodiacal light was visible in S., 70° E. The column showed great change.

August 8.—The lower part of the phenomenon appeared of a rosy tint, which at times disappeared and returned.

1861. June 29.—A most extraordinary appearance of light in S.E. It descended in straight, well-defined lines. The light was white, and better defined than the Zodiacal light, and it appeared to be slightly bifurcated. Magnets perfectly quiet during the phenomenon.

The nautical observations form another important division of Dr. Neumayer's labours. These consist of the logs of various vessels which agreed to work in conjunction with the observatory, and from these logs results are derived of much practical benefit to the navigator, and of much interest to the ocean meteorologist. In them the usual method of dividing the ocean into five degree squares has been adopted.

A system of hourly observations in meteorology and terrestrial magnetism was carried on day and night, without interruption, for five years, after which a more simple system of observations was organised.

Another important task undertaken was the magnetic survey of the colony, in pursuing which Dr. Neumayer had frequently to be absent for three or four months at a time. The aggregate number of miles which he travelled over in the survey was 11,000, and the number of stations he examined, 230, situated at all elevations.

But this labour—great and Herculean as it is—represents only a portion of that which Dr. Neumayer has done, and merely denotes the work spent in obtaining raw materials. These have now to be tabulated, reduced, and discussed—a process not unlike that by which the sheaf of the reaper is made into good, wheaten bread. This reduction and discussion have been made by Dr. Neumayer in one of the volumes now before us, and, in obtaining his final results, the most approved scientific methods have been adopted. The magnetic observations are very completely discussed after the method of Sir E. Sabine, and many valuable results have been obtained. In particular, allusion may be made to a connection, traced by Dr. Neumayer, between the magnitude of the lunar-diurnal variation, and the moon's declination, forming a paper which has been published in the Transactions of the Royal Society.

These remarks would be imperfect without alluding to one point for which Dr. Neumayer deserves very great credit. While he has reduced his observations according to the most approved methods, he has, nevertheless, exhibited to his readers, as far as possible, the *actual observations* themselves, so that in future, if other methods of reduction should be followed and other objects sought, we can fall back upon actual facts, capable of being moulded anew into the form required. This is a point which ought surely to be borne in mind in all similar discussions.

B. STEWART

* "Discussion of the Meteorological and Magnetical Observations made at the Flagstaff Observatory, Melbourne."
"Results of the Magnetic Survey of the Colony of Victoria." (Trübner and Co.)

THE MANUFACTURE OF TAR PAVEMENT *

IN most provincial towns there are two important bodies of men, the paving commissioners and the gas directors. The one is pledged to keep the rates low, and the other to keep the price of gas as low as will enable them to provide the statutory dividend. As one means of ensuring a cheap supply of gas is to create a greater demand and obtain a better price for the residual products, it is of advantage to consider a subject the adoption of which would be advantageous to both of these bodies. It is not a new one, but has hitherto been a neglected source of revenue to gas companies, and will also be a great benefit to the public. That subject is tar pavement. In some counties, such as Yorkshire, where stone is as abundant as brain is said to be, tar pavement will receive but little attention; but in the eastern and some other counties where the same conditions do not exist, but where York flag costs 7*s.* per yard laid, tar pavement is a desideratum. In such districts there is a scramble for pavement; and, on account of the high price, unless a paving commissioner reside in the street, it remains unpaved.

Tar pavement may be made of the ordinary cinder-dirt produced in gas-works, of shingle, or of a mixture of both. The material is burnt in heaps like ballast, and when hot is mixed with hot tar. In practice a small fire of coke is made on the ground, and covered with cinder-dirt or shingle. When this layer is hot another is added, and so on in succession until a large enough heap has been provided. The tar is now boiled in an iron copper, and taken when hot and mixed with the hot material from the heap already described, in quantities of two bushels at a time, in about the proportion of one gallon to every bushel of cinder-dirt, and slightly less than a gallon for the gravel. It is turned over and over with the shovel until every part of the material has got a covering of tar. Then the whole is passed through a sieve with ⅝-in. mesh, and part of it through another with ¼-in. mesh, and put in heaps until required. Indeed, it may be kept for months before being laid down.

Before the pavement is laid, an edging should be provided about 2 in. thick, and projecting 2 in. above the surface of the ground to be covered, which should

* Paper lately read before the British Association of Gas Managers, by Mr. T. H. Methven.

be tolerably even. It is advisable to have the ground next the curb well trodden on and rammed before the pavement is laid, otherwise there will be an unseemly hollow next the curb. In laying, the rough stuff is put down first and rolled tolerably firm, then the second quality is put on, then the third, and when the whole has been raked level, a little of the finest material is sifted on through a sieve with ⅓-in. meshes, and a little fine white shingle or Derbyshire spar is sprinkled on the top. The whole must now be well rolled. The best roller is a water ballast roller, which at first is used without ballast, and well wetted to prevent adhesion of the material, and, when the pavement is slightly consolidated, the full weight should be applied.

For heavy cart traffic the material should be made of shingle only, heated and mixed as above, and well rolled. Both descriptions of pavement are laid best and most easily in warm weather, and should be rolled when the sun has warmed it well. Those parts in angles should be well rammed and trimmed off with a light shovel.

Though apparently a simple manufacture, there is a little difficulty in ascertaining the proportion of tar to gravel or cinder-dirt. A little experience will only be necessary in this, as well as in all other manufactures, to enable anyone to carry it out successfully.

This pavement cannot be spoken of too highly, as it is cheap, wears well, and can be easily repaired. The colour, which never can be made to equal York flag, and the smell for some time after it is laid, are the only objections to its use ; it can be laid with a good profit in any district at 1*s.* 4*d.* per square yard ; and besides being a boon to the public, who must otherwise walk on gravel, is a great advantage to gas companies. To them it provides a remunerative outlet for their tar, which often otherwise must be sold at a low price to distant distillers.

A late paragraph, which appeared in the daily press, states that it is proposed to pave the streets of London with stone laid in asphalte instead of lime grout. This is just a more systematic application of the above-described plan ; for the tar, by being boiled and thrown on hot stones, becomes an elastic asphalte.

INDIAN BARRACKS

RECENT discussions regarding the new Indian Barracks have shown the necessity for an adequate knowledge of physical science on the part of all engineers who have to cope with great natural forces.

These costly structures have been described as "sun traps," meaning thereby that the materials and details have been so selected, that instead of the interior of the rooms being as cool as, if not cooler, than the outside, it is cooler for the men to be in the open air under the sun. It is true that this objection has been raised at very few stations, but with adequate knowledge on the part of the architects, it ought never to have been raised at all. The barracks complained of are stated to be brick structures of the usual dark colour, with verandahs supported not on light easily heated and easily cooled columns, but on massive piers and arches like a bridge. The roofs have been constructed almost as they would have been at home, and no adequate means have been adopted for protecting the walls from sun-radiation. Of course the great mass of brickwork becomes heated during the day, and heats the air outside and inside the rooms at night, while the structure of the roof is such that the interior is heated both by direct conduction and radiation. Now surely such mistakes might have been avoided. There are such things as non-conducting materials to be had, and double walls with a ventilating air space between are not an uncommon expedient in this country for keeping out both heat and cold. Again, there is such a thing as white plaster or whitewash for outside walls, which reflects a large portion of the solar heat. Double roofs are not unknown, we believe, in India, and it is possible to construct such a roof as to interpose not only an air current between the outer and inner layer of the roof, but to prevent the radiation of the inner surface of the outer layer passing through the inner layer. These are very elementary applications of physical laws to Indian house construction, and how little they have been attended to may be inferred from a fact which has been stated—viz., that these barracks have been roofed with slate. The ordinary laws of conduction and radiation of heat appear to be at a discount in the Indian works department.

THE PROVINCE OF MINERAL CHEMISTRY

PROFESSOR KOLBE has recently succeeded to the directorship of the *Journal für Praktische Chemie*, rendered vacant by the decease of Erdmann, its original founder ; and in his hands this periodical will doubtless become the recognised organ of the modern Leipsic School of Chemistry. Dr. Kolbe, in the first number o the new series of this work, has signalised his succession to the office of editor by an introductory essay, setting forth his opinions upon what he considers must be the future aim of the student of Inorganic Chemistry. Organic Chemistry, once the neglected sister of Inorganic Chemistry (to use the Professor's phraseology), has become so courted and honoured since Liebig introduced her as a young science into the chemical world, that little by little her relative has sunk into comparative obscurity. But the time has now arrived when, in Kolbe's opinion, it is evident that Inorganic Chemistry has not merited this neglect, but that she has it in her power to bestow rewards not less precious than those of Organic Chemistry, upon those who devote themselves to her service.

Much of the attractiveness of Organic Chemistry must, according to Kolbe, be traced to the zeal with which the origin of the almost numberless cases of Isomerism in organic compounds is being searched out ; indeed, this zeal has nearly displaced the lively interest in Inorganic Chemistry created by the discovery of Isomorphism. This cause can never actuate the investigator in the domain of Inorganic Chemistry—at least not to the same extent—and for the reason adduced by Kolbe, that Isomerism cannot possibly occur in inorganic compounds to anything like the same degree as among organic substances, owing to the greater simplicity of constitution possessed by the former class. That so few cases of Isomerism have been discovered in Inorganic Chemistry is undoubtedly due to the fact that hitherto we have neglected to investigate mineral substances with special regard to their chemical constitution.

In order to prove the truth of this latter assertion Kolbe asks how can we frame anything like a reply to any question respecting the chemical constitution of the naturally-occurring silicates—felspar, for example ? What are the proximate constituents of this compound, and what are their respective functions ?

In half a dozen brief sentences Kolbe disposes of all our knowledge upon this subject, beginning with the views of Berzelius, by whom felspar—the typical silicate—was regarded as possessing a constitution analogous to that of dehydrated alum—that is, as a double salt of neutral silicate of alumina and silicate of potash ; and ending with Gerhardt, who thought to explain the constitution of this and all bodies by his theory of types, or, in the expressive language of Professor Kolbe—"mit der mechanischen Handhabung eines todten Schematismus."

The greater portion of the paper is devoted to Kolbe's theoretical views upon the nature of felspar, and he suggests a number of structural formulæ to explain its constitution according to our present opinions upon the quantivalence of the component elements. Whether, however, any of these formulæ represent the actual constitution of felspar, or if, indeed, its true nature can be represented by such formulæ, is a question which Kolbe leaves untouched, for the simple reason that the necessary experimental foundation from which alone valid arguments can be drawn, is entirely wanting.

Professor Kolbe concludes this remarkable paper by defining what should be the future aim of the student in Mineralogical Chemistry. He must not now rest content with a mere quantitative analysis, or with the empirical deduction of rational formulæ from the results of such analysis. Such a process can never fully elucidate the chemical constitution of inorganic compounds. This can only be accomplished by a careful and systematic study of the decomposition, syntheses, and substitutions; in other words, by the application of methods of research similar to those which have yielded such splendid results in Organic Chemistry. T. E. THORPE

NEW OBSERVATORY IN THE SOUTHERN HEMISPHERE

THE following statement with regard to the Cordova Observatory, to the foundation of which we have before referred, is extracted from the last number of Silliman's *American Journal of Science and Arts.*

"The Argentine Congress voted to establish a national observatory at Cordova, at the instance of President Sarmiento, and through the exertions of the present Minister of Public Instruction, Dr. Avelleneda, who invited me to organise and take charge of it, knowing my desire to extend the catalogue of the southern heavens beyond the limit of 30° to which the zones of Argelander extend. Bessel went through the region from 45° N. to 15° S. with systematic zone observations at Königsberg, which have since been reduced and published in two catalogues by Weisse of Cracow. Argelander carried the same systematic scrutiny with the Meridian Circle, from Bessel's Northern limit to the pole, and afterwards from Bessel's Southern limit to 30° S.

"Since then Gilliss has observed a series of zones for 30° around the south pole; but the reduction of these, although very far advanced, was not completed at the time of his death, and the MS. is now stored somewhere in Washington. Let us hope that it may at some time be recovered, the work completed and given to the world.

"My hope and aim is to begin a few degrees north of Argelander's southern limit, say at 26° or 27°, and to carry southward a system of zone observations to some declination beyond Gilliss's northern limit, thus rendering comparisons easy with both these other labours, and permitting the easy determination of the corrections needful for reducing positions of any one of the three series to corresponding ones for the other. It is of course impossible to arrange in advance the details of such an undertaking, but my expectation is to go over the region in question in zones 2° wide (except in the vicinity of the Milky Way where the width would be but one-half as great), up to a declination of about 55°, after which the width would be gradually increased as the declinations became greater. Within these zones all stars seen as bright as the 9th magnitude would be observed, so far as possible, moving the telescope in altitude when no bright star is in the field until some one becomes visible, according to the well known method of zone-observations.

"For reducing the observations, differential methods will probably be employed, inasmuch as the time now assigned for my absence from home would be inadequate for proper discussion of the correction required for nice determinations of an absolute character. Still, it is my present purpose, so far as possible, to make such subsidiary determinations as might hereafter be needed in any attempt at computing the observations absolutely. But as I hardly venture to anticipate any opportunity of making a thorough determination of the constants of refraction, or of the errors of graduation, it seems best to arrange for a differential computation at least at first.

"It is improbable that a sufficient number of well-determined stars will be found available even for this differential reduction, and the necessity may thus be entailed of determining the comparison-stars myself, this determination, however, itself depending upon standard star places. So far as possible I propose employing those heretofore determined by me, and published by the Coast Survey, which form the basis of the star places of the American Nautical Almanac.

"With these observations of position it is my hope to combine others of a physical character to some extent; but in the presence of a plan implying so much labour and effort, it would be unwise to rely upon the possibility of accomplishing much more than the zone-work.

"The meteorological relations of the place are very peculiar, but I dare not undertake any connected series of observations bearing upon these, without self-registering apparatus, which is beyond my means.

"Cordova is one of the oldest cities, and contains the oldest university, of the Western hemisphere. It is situated in 31½° S. latitude, on the boundary of the Pampa, where the land begins to rise toward the group of mountains known as the Sierra de Cordova. It is connected with Rosario, on the Parana, by the Central Argentine Railway, which has probably been already opened to travel through its entire length of about 250 miles, although information to that effect has not yet reached this country.

"The two largest instruments will be a Repsold meridian-circle of 54 inches focal length and 4½ inches aperture, and an equatorial by Alvan Clark and Sons, provided with the 11-inch object-glass, by Fitz, lately in the possession of W. Rutherfurd, who has supplied its place by one of 13 inches. A photometer by Ausfeld of Gotha, according to Zöllner's latest form, has been constructed under the supervision of Prof. Zöllner himself; a spectroscope will be furnished by Merz of Munich, and a clock by Tiede of Berlin.

"The Scientific institutions of the U.S. have afforded the expedition every possible assistance. The Coast Survey lends a circuit-breaking clock, a chronograph, and a portable transit; the Smithsonian Institution lends a zenith telescope; the American Academy of Arts and Sciences of Boston (probably) a photometer and spectroscope; the Washington Observatory and the Nautical Almanac have greatly aided the undertaking by gifts of books and by a manuscript copy of Gilliss's catalogue of Standard Stars; and from the astronomers of England, Germany, and Russia important assistance has been freely and effectively contributed, in the order and supervision of instruments and apparatus, and by the gift of books, as well as by important and valuable suggestions.

"Four assistants will accompany me, Messrs. Miles Rock, John M. Thome, Clarence L. Hathaway, and William M. Davis, jun. We hope to reach Buenos Ayres not later than the middle of August.

"The building is now under construction in Boston. The means available proved inadequate for its construction according to the original plan, which was in the form of a cross, with four square rooms about its centre, and turrets at its four extremities. One half of it will be first erected, and it is hoped that the remaining portion will speedily be added." B. A. GOULD

SCIENTIFIC SERIALS

THE greater part of the *Revue des Cours Scientifiques* for July 23, is occupied by the commencement of a very able paper read before the Anthropological Society of Paris by Prof. Broca, on the Transformation of Species. Commencing with the pre-Darwinian theories of transformation of Blainville and Lamarck, he then proceeds to give a *résumé* of the theory of Darwin, and the arguments in favour of or against the permanence of species, drawn from the observation of living species and from palæontology. Following this we have, *à propos* of the war, an article on field ambulances and hospitals, by Prof. Champouillon. In the number for July 30, we have the rectorial address of von Littrow to the University of Vienna, on the backward state of science among the ancients, and the conclusion of M. Broca's paper on the transformation of species, in which the subject is treated from a philosophical point of view, and the professor sums up strongly against the idea of permanence. The hypothesis of Natural Selection is then discussed, but a much less certain conclusion arrived at. The number for August 6 opens with a report of the discussion on the nomination of Mr. Darwin as corresponding member of the Academy of Sciences, to which we have referred in another column. This is followed by a singularly able and exhaustive review by M. Claparède, of Geneva, of Mr. A. R. Wallace's Essays on Natural Selection, in which he points out that while Mr. Wallace demands the intervention of a superior force to explain the foundation of the human races, and to guide man in the path of civilisation, he altogether denies the existence of such a force as assisting to produce the inferior races of animals and plants, which he attributes entirely to the operation of Natural Selection. In the same remarkably interesting number of the *Revue*, we have also Mr. Marey's extremely important paper on the Flight of Birds.

THE current number (No. XXXIX.) of the *Quarterly Journal of Microscopic Science* is an unusually rich one, containing several papers of great importance. Amongst them we may draw attention to one of considerable length by Dr. Beale, entitled "Bioplasm, and its degradation," with Observations on the Origin of Contagious Disease. He introduces and defends the use of the term Bioplasm, which we think is admissible enough, and serves very well as a distinguishing appellative for actually living matter, as opposed to protoplasm, which has been rather vaguely used to designate organic matter whether dead or alive. The application, however, of the new term is sufficiently wide, since Dr. Beale appears to consider them in a special form for each separate structure in the body originating in the primary mass of bioplasm of the egg. From each subdivision of the latter "in pre-ordained order, and with perfect regularity more are produced, no doubt according to laws, but laws which we know nothing about, except that they are not physical." This last assertion indeed seems open to question, for if we know *nothing* about them, how can it be said with certainty that they are not physical, as it is certain we are not acquainted with all the physical laws of the world. Dr. Beale proceeds to describe the Bioplasm of the Amœba, the principal forms of that of man, and its relations to such morbid products as pus and infectious poisons. An interesting paper follows, by Dr. Macdonald, of H.M.S. *Fisgard*, on the minute anatomy of some of the parts concerned in the function of accommodation to distance. Dr. Caton describes the means he has found best adapted for studying transparent vascular tissues in living animals. To this succeeds a capital *résumé* of Prof. Stricker's "Studien aus der Institut für experimentelle Pathologie in Wien aus dem Yahre 1869," and the part devoted to original communications terminates with two essays, one by Mr. E. Ray Lankester, on the Migration of Cells, and one by A. M. Edwards, on *Diatomaceæ*.

The *Transactions of the Linnean Society*, vol. xxvi., pt. 4, is entirely occupied by two papers on Fossil Cycads. The first, by Prof. W. C. Williamson, is descriptive of the remarkable *Zamia gigas* Lindl. and Hutton, or *Williamsonia gigas* Carr., found in considerable abundance in the Lias at Whitby. He believes it to have borne a strong resemblance to existing Cycads with diœcious flowers. In Mr. Carruthers's valuable monograph of Fossil Cycadean stems from the Secondary rocks of Britain, he shows that these fossils are, as far as is at present known, entirely confined to Secondary strata, the so-called *Cycadeæ* of the coal-measures and other palæozoic strata being rather referable to cryptogamic *Lepidodendra*, and the few specimens reported from Miocene beds being very imperfect and uncertain. In his description which follows, Mr. Carruthers describes four new genera from British rocks, *Yatesia*, *Fittonia*, *Williamsonia*, and *Bennettites*. Both papers are illustrated by excellent plates.—Vol. xxvii., part 2, is of more varied interest. Mr. John Miers contributes two botanical papers; a description of three new genera of *Verbenaceæ*, *Rhaphithamnus*, *Phelloderma*, and *Diostea*, from Chile and the adjacent regions; and a paper on the anomalous genera *Gœtzia* and *Espadea*, the position of which is very unsettled, and which he proposes forming into a new order. Dr. Birdwood describes and figures three new species of *Boswellia*, natives of the Soumali country, all of which yield frankincense, and one of them, he believes, the bulk of the olibanum of commerce. Dr. J. B. Hicks points out a singular resemblance between the genus *Draparnaldia*, and the confervoid filaments of mosses. We have descriptions of new Agarics and Lichens from Ceylon, the former by the Rev. M. J. Berkeley, the latter collected by Mr. Thwaites, and named by the Rev. W. A. Leighton, who also contributes notes on the Lichens of St. Helena, and a description of a new British Fungus, *Sphæria tartaricola*, Nyl. The longest paper in this part is an important monograph by Messrs. Henry Brady, Parker, and T. Rupert Jones, of the genus *Polymorphina*, an attempt to rescue this difficult genus of Foraminifera from the almost inextricable confusion into which it has fallen. Dr. A. Rattray contributes a paper on the anatomy, physiology, and distribution of the *Firolidæ*, forming the section of Heteropoda with a straight and elongated form, and either wholly naked or furnished with a very small shell, and including the genera *Carinaria*, *Carinaroides*, *Firola*, and *Firoloides*. Sir John Lubbock proceeds with his Notes on *Thysanura*, part iv.; and from Dr. Edward Moss we have an account of the genus *Appendicularia*, with its remarkable appendage, or "haus," the object of which in the vital economy has not been ascertained.

SOCIETIES AND ACADEMIES

LONDON

Entomological Society, July 4.—Mr. Alfred R. Wallace, president, in the chair. The Rev. F. A. Walker and Mr. Edward Mackenzie Seaton were elected members. Numerous objects of interest were exhibited by or on behalf of Mr. Meek, the Hon. T. De Grey, Mr. F. Moore, Mr. Blackmoor, Mr. Albert Müller, Mr. Jenner Weir, Sir J. C. Jervoise, Bart., Mr. Tegetmeier, and others.—Prof. Westwood made some observations on a group of very minute four-legged *Acari*, and the President mentioned instances of protective mimicry in insects, recently observed by Mr. Everitt in Borneo.—The following papers were read:—"Further observations on the Relation between the Colour and the Edibility of *Lepidoptera* and their Larvæ," by Mr. J. Jenner Weir; "On a Collection of butterflies, sent by Mr. Ansell from South-Western Africa," by Mr. A. G. Butler; "Contributions to the Insect Fauna of the Amazons" (continuation, *Coleoptera longicornia*, Fam. *Cerambycidæ*), by Mr. H. W Bates; "List of the *Hymenoptera* captured by Mr. J. K. Lord in Egypt and Arabia, with Descriptions of New Species," by Mr. Francis Walker.

EDINBURGH

Scottish Meteorological Society, July 21.—Half-yearly general meeting, Mr. Milne Home in the chair. The following report from the Council was read:—"The Council have to report that the number of the Society's stations is now ninety-one, there being an additiion of one since the last general meeting, in consequence of the services of an observer having been obtained for Leith. At the last half-yearly general meeting, reference was made to a renewed application by the Council to Government for pecuniary aid. The application was so far favourably received that the Board of Trade a second time recommended the Council to prefer to the committee of the Royal Society their claim, with a view to an allowance being made from the annual Parliamentary grant of 10,000*l*. for meteorological purposes, of which grant that committee have charge. The Council regret to say that the Royal Society committee have stated that they are unable to make or promise any allowance out of the grant, as the whole of it has been appropriated to other objects; for which objects the grant is, as the committee state, even too small in amount. The Council have in these circumstances been induced to renew their application to Government for a special grant to the Society. The Council have requested Mr. Buchan to prepare a report on the monthly temperature of the British islands, and to state to this meeting a few of the results obtained by him. The subject is one which it is believed has not been thoroughly investigated by any other society, or indeed by any meteorologist except Professor Dove; and Professor Dove's charts, which are now ten years old, were based on observations not only necessarily scanty, but in several cases unavoidably incorrect. The first chart which this Society prepared of the temperature of the British Isles was published in the Society's journal in January 1864, and it has been frequently referred to by meteorologists as the only one existing. That chart was constructed on returns obtained during a period of five years; the new charts which Mr. Buchan is now constructing are founded on observations for thirteen years, ending December 1869. These charts, besides giving the mean temperature of the year for the whole British Islands, give also the mean temperature of each month. The Council anticipate that, when these charts are published, with the tables explanatory of them, they will be found to afford valuable aid in the discussion of many important questions of a practical nature."—Mr. Mohn, Professor of Meteorology in the University of Christiania, and director of the Norwegian Meteorological Institute, who was at the meeting, presented a work entitled the "Storm Atlas," referring to various storms which had passed over the north of Europe. The Swedish returns were from twenty-five stations, where the observations were made three times a day, and were sent gratuitously by the Swedish Institute.

A paper "On the Temperature of the British Islands," by Mr. A. Buchan, was read; thirteen charts illustrating the temperature of the British Islands in each month of the year being exhibited. The author said that the investigation, the results of which were now exhibited on the walls, was one of the most important of all that the Society had undertaken. An early attempt was made to partially solve the problem about seven or eight years ago, and a chart was constructed showing the mean temperature of different parts of Britain in July and in January. These observations had two inherent defects. They were based only on five years, evidently too short a period to yield such results as were quite trustworthy. They were also defective in respect of number of stations, there being some parts of the country very badly represented. Now, however, an investigation had been completed for thirteen years, which must be a very close approximation, indeed, to the solution of the problem. The real temperature of the various months could differ very slightly from what was now exhibited. Further, the number of stations now brought under review was four times what the Society had at first. The observations were upon a mean of thirteen years of 68 Scotch stations, 54 English stations, and 11 Irish stations—Ireland being yet very badly represented. To enable him to draw the lines on the outskirts of the British Isles with greater accuracy than could otherwise have been attempted he had calculated the mean temperature for Faroe. Several Norwegian stations he had obtained from Professor Mohn's publications, and he had also ascertained the mean temperature of places in Belgium and France. The temperatures had been reduced to sea level by the ordinary method of allowing a degree for every 300 feet of elevation, and the lines were drawn upon the charts to show each difference of a degree of Fahrenheit. Among the results brought out, it appeared that in January there was as high a temperature in the north of Shetland as there was in London. As soon as the westerly winds crossed the high mountain ridge that was on the west of the British Isles, they deposited their vapour, radiation took place, and the temperature rapidly fell. In the same way, in Ireland, the lower temperatures were found inside, the higher temperatures outside. Another very marked result was the effect of Ireland upon Britain in increasing the summer temperature and lowering the winter temperature opposite to that island. In regard to the influence of the Irish Sea, Mr. Tennent, a member of Council of the Society, who had for a year or two given a good deal of attention to the direction of winds in different parts of the British Isles, is of opinion that the winds of the Irish Channel were not so much westerly as in Scotland and Ireland, but flowed to a great extent through the centre of the Channel. Now, on looking at the charts, it would be seen that the effect of this current in the winter months was to push up the isothermal lines over the Irish Sea. Through the whole of the months, the observations all showed the marked effect which that open space of water had upon the temperature of the British Islands, as clearly as the effect which Ireland had upon the part of Britain opposite. Questions of temperature had an important bearing upon agriculture. Many agriculturists believed that if the night temperature fell to 40°, there was no growth for twenty-four hours. If the night temperature fell to 40°, the mean temperature might be expected to be about 46°. Thus, then, by observing the charts for the various months, and taking note of those parts of Britain whose temperatures were less than 46°, one would ascertain the places where during certain months there was little growth, a very important question in discussing the crops of the British Islands. The next important temperature for agriculture was that required to ripen cereals. It had been proved by observations made by persons competing for prizes offered by the Marquis of Tweeddale, President of the Society, that for the purpose in question, with the ordinary range of temperature in Scotland, there must be an average of 56°. If it fell below that, there was a deficiency in the crop; if it rose, the crop was so much the better, provided there were rain and other necessary conditions. Accordingly, with the charts for the different months, one could point to those parts of the British Islands where there was some hazard in rearing cereal crops—the places, namely, where the necessary temperature was scarcely to be expected, or where it occurred so seldom that the risk was too great. It was generally supposed that the temperature fell one degree for every 300 feet of elevation, so that supposing at the sea level there was a temperature of 58°, at an elevation of 600 feet the temperature would be 56°, or a temperature sufficient to ripen cereals. In reference to this point, however, it was interesting to compare the station of Braemar on the Dee-side with that of Wanlockhead in the Leadhills, which were among the best equipped stations of the Society. It was well known by experience that on Deeside oats could ripen up to about 1,500 above the sea; but at Wanlockhead, which was only 1,300 feet above the sea, oats were sown only for the straw. Here, then, were very marked differences in the effects of temperature, as shown in the growing crops. Taking the month of June, he found that, adding a degree for every 300 feet of elevation, he got a mean temperature at Wanlockhead of 54·9°, while at Braemar, applying the same correction, he got a temperature of 56·8°. The cause of the difference between the two places he was not prepared to hazard an opinion upon, but it had an important bearing on the produce of the country. He thought that if this question were a little inquired into at some other stations, they might get some general law for guidance in reference to such matters.

DUBLIN

Royal Irish Academy, June 23.—The Rev. Professor Jellett, B.D., president, in the chair. Mr. Frith read a very interesting paper on arterial drainage in the west of Ireland. Mr. W. Andrews mentioned the second occurrence on the coast of Ireland of the rare Cetacean *Xiphis sowerbii*; it had been found in May last, in Brandon Bay, Co. Kerry. It was seventeen feet in length, the back of head and fins were of a velvety black colour with lines of white. Although sadly mutilated by the fishermen, yet several important parts had been obtained sufficient to enable him to supplement his (Mr. Andrews) provious paper on this very rare whale.—A paper was read by the secretary on the Book of Clonenagh.—The last part of the proceedings was laid on the table, and the receipt of a MS. index to the volume from the Rev. Dr. Reeves was acknowledged with many thanks and ordered to be printed.

PARIS

Academy of Sciences, August 1.—A note was read by M. G. Rayet, on the spectrum of the solar atmosphere, in which the author noticed the variability of the bright lines, in confirmation of Mr. Lockyer's observations. M. Berthelot communicated some thermo-chemical investigations upon the sulphurets, in which he described the action of the alkaline sulphurets upon metallic salts in solution, the action of acids upon the alkaline sulphurets, that of sulphuretted hydrogen upon various metallic salts, and of acids upon the metallic sulphurets. A note by M. L. Henry, on the action of pentachloride and pentabromide of phosphorus upon various æthers, was read. M. F. Pisani presented an analysis of nadorite, a new mineral from the province of Constantine. This mineral was supposed to consist of one equivalent of oxide of antimony, and two equivalents of oxide of lead; the author gave as its formula $(Sb^2 O^2, Pb O) + Pb Cl$. A note by Mr. F. C. Calvert, on the evolution of pure nitrogen from nitrogenous organic matters was communicated by M. Chevreul. The author described the evolution of nitrogen from animal matters, by the action of hypochlorite of lime, and gave a tabular view of the quantities produced from gelatine, albumen, calcine, wool, and silk; these amounted in each case, to rather more than one-third of the whole quantity contained in the substance operated upon. A note was read by M. Contejeau, on the maximum of temperature at Poitiers, on the 24th July, 1870. The maximum observed was at $1^h 10^m$ P.M. when the thermometer suspended under the shadow of a tree showed

$41^{\circ}\cdot2$ C. (= $106^{\circ}\cdot16$ F.).—A note by M. V. Raulin on the rain-fall of the French Alps was communicated by M. Leverrier. It included tables of mean, annual, seasonal, and monthly rainfalls for sixteen stations.—M. C. Saint-Claire Deville communicated an extract from a letter of M. Chassin on a severe earthquake felt in Mexico on the 11th May of the present year. This earthquake destroyed the town of Pochutta, in the State of Oaxaca, in twelve minutes; it continued until the 19th.—A note was read by M. Marès on the corpuscular disease of the silkworm, and Marshal Vaillant contributed some extracts, showing the advantages which have been obtained in various places by the adoption of the processes of selection of silkworm's eggs recommended by M. Pasteur.—M. Decaisne presented a note by MM. Planchon and Lichtenstein on the specific identity of the *Phylloseræ* of the leaves and roots of the vine.—A note by M. E. Roze on some mycological experiments, was communicated by M. Brongniart. The author confirmed the results obtained by M. Oersted as to the production of *Rœstelia penicillata* on the hawthorn by means of *Podisoma clavariæforme* from the juniper, and described some important experiments on the relation of *Claviceps purpurea* (Tul.) to the *ergot* of rye and other grasses.—A note by Mr. F. C. Calvert on the employment of phenic acid as a disinfectant, was presented by M. Chevreul. The author claimed the first application of phenic acid in this way for Dr. D. Davis, of Bristol, in 1867. The perpetual secretary states in a note that it was used on a large scale in Paris in 1865.

BERLIN

German Chemical Society, July 11.—Alexander Müller, having been engaged in analysing the waters of Berlin, stated, the principles which he thinks necessary for procuring good analyses, as follows:—The bad property of water depends upon the organic matter contained in it. Water should therefore be evaporated *in vacuo* and submitted to dialysis, the colloid portion to be examined with the microscope. The organic matter should be determined by elementary analysis. Another portion should be evaporated with a weighed quantity of carbonate of soda, to separate silicic and phosphoric acids. The residue should be heated to redness; the loss corresponds to the nitric and nitrous acids and organic matter. The remaining weights, minus the carbonate of soda added, is that of the salts contained in the water, with the exception of phosphates and silicates.—Dr. Schwarz related his experiments instituted to obtain the homologues of isothionic acid with methylic, amylic, and butylic alcohols. Working with large quantities of sulphuric anhydride, he was unable to remark the two modifications which have been lately observed by Schultz Sellac. The boiling point of his anhydride (prepared by distilling Nordhausen acid with phosphoric anhydride) was 26° C.—M. Jaffé in describing an experiment concerning the constitution of rufigallic acid established the mode of the formation of colouring matters in plants. Gallic acid $C_7 H_6 O_5$, a derivation of benzol, when heated with sulphuric acid, yields rufigallic acid, until now considered as $C_7 H_4 O_4$. By treating this compound with zinc powder M. Jaffé has obtained anthracene $C_{14} H_8$. He therefore doubles the formula of rufigallic acid, and thinks it probable that the complicated vegetable colouring matters are derived from the tannic acids which are a constituent ingredient of most plants.—A. Oppenheim described experiments on the action of sulphuric acid on oxygenated organic chlorides. This he found to be analagous to the action of sulphuric acid on the corresponding chlorinated hydrocarbons. The chlorhydrine of glycol yields glycolsulphuric acid, just as chloride of ethyl yields ethylsulphuric acid. Epichlorhydrine, C_3H_5ClO, yields the mixed ether of glycerine, $C_3\ H_5\ Cl\ OHSO_4\ H$, just as chloride of allyl yields a mixed ether of propylic glycol. Acid chlorides yield sulphuric acids in which the organic acid radical replaces one atom of hydrogen. These compounds are decomposed by water into the acids entering into their composition. Thus we have acetyl-sulphuric acid, phthalyl-sulphuric and benzoyl-sulphuric acid. The latter, however, is gradually transformed into sulphobenzoic acid. By heating, this molecular transposition takes place at once, and thus a good method exists for preparing this acid free from any secondary product. The same chemist, conjointly with M. Ador, established the identity of this acid with that discovered by Mitscherlich. It yields the same salts and is transformed into isophthalic and oxybenzoic acids by fusion with formiate and hydrate of potassium.—L. Carus communicated observations on the temperature necessary for his method of organic analysis in sealed tubes. P. Griess established the formula of benzo-creatine, $C_8 H_9 N_3 O_2$. obtained by the action of potash on the cyanide of amidobenzoic acid.—L. Henry reported on the action of $P Cl_5$ on the ethers of diatomic monobasic acids.—MM. Merz and Mülhauser described the properties of naphthoilic or naphthylcarbonic acid—T. Thomsen described experiments on specific heat. In order to arrive at a standard quantity of heat he heated liquids by burning a certain volume of hydrogen. He arrived at the conclusion that mixtures of sulphuric acid and water have the specific heat of the water entering into the mixture. C. Rammelsberg communicated experiments on the specific weights of the different modifications of tin.

BOOKS RECEIVED

ENGLISH.—A Manual of Zoology, Vol. I.: H. A. Nicholson (Blackwood and Sons).—Heat a Mode of Motion; new edition: Prof. Tyndall (Longmans).—Irregularities and Diseases of the Teeth: H. Sewell (Churchill).—Notes on Electricity: Prof. Tyndall (Longmans).—The Laboratory Guide: A. H. Church (Van Voorst).—Murby's Scripture Manuals, Genesis: Murby. An Elementary Course of Plane Geometry: R. Wormell (Murby).—Cassell's. Book of Birds, Part ix.—Co-operative Agriculture: W. Pare (Longmans).—Henfrey's Elementary Course of Botany, edited by Dr. Masters (Van Voorst) — Mushroom Culture: its Extension and Improvement: W. Robinson (Warnes).—Notes about Aldeburgh: N. F. Hele (J. R. Smith).

FOREIGN.—Archiv für Anthropologie, Vol. IV. (Trübner). —(Through Williams and Norgate) Berichte über die biologisch-geographischen Untersuchungen in den Kaukasus-ländern; with Atlas.

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ERRATUM.—Page 269, second column, last line of table, for 89,395, read 34,413.