THURSDAY, NOVEMBER 17, 1870

THE PRESENT EPIDEMIC OF SCARLET FEVER

THE prevalence of scarlet fever at this time throughout England and Wales is a question that not only affects medical men, but is one that demands the attention of the people of England. They ought to ask themselves the question as to how it is that a malady which is placed by medical writers under the class of "preventible" diseases is allowed to prevail to the extent of destroying several hundreds of persons weekly. Scarlet fever seems to enjoy that immunity which is accorded to what are called "necessary evils;" but when we come to inquire what right this disease has to carry off ten or twelve thousand persons annually, we find that it ought no more to exist amongst us than small-pox or cholera.

Scarlet fever is essentially a contagious disease, and exhibits all the phenomena of a malady which, being communicated from one individual to another, is more or less under the control of human action. Under these circumstances, it is impossible for the Government to stop such a disease by mere Acts of Parliament, or for medical men to superintend efficiently arrangements for the prevention of its spread. It is only by the intelligent apprehension on the part of the public who are infected, that any hope of the arrest of the disease can be expected. We therefore take this opportunity of addressing the public on the subject. Unless heads of families and the public generally are acquainted with the real nature of this disease, no external organisation of any kind is sufficient for its control.

We need not refer here specifically to the returns of the Registrar-General, to show how fearfully prevalent scarlet fever has been. In London the weekly mortality has been as high as one hundred and ninety in a week, giving a mortality for London alone of nearly ten thousand a year. Professor Huxley, in his late address at Liverpool as President of the British Association, says that in the years 1863, 1864, and 1869, 90,000 persons were killed in England and Wales by scarlet fever. These figures point to a much higher mortality for scarlet fever than we have ever had to record for cholera. The point most remarkable about this mortality is, that whilst death from cholera has agitated every British community, no public anxiety has been manifested about scarlet fever. Every one has submitted to it as a necessary evil, and no one has made any efforts to diminish its prevalence.

Yet, when we come to inquire into the nature of scarlet fever, and the laws of its distribution, there seems to be no more reason why it should prevail amongst us than plague, small-pox, or cholera, whose laws of distribution we now know, and on which we can exert the most obvious control. Scarlet fever is a contagious disease, and it is not too much to say that we have *all contagious diseases under our positive control.* Their nature, and the laws of their distribution, are so well known, that it is possible to teach the humblest individual interested in their destruction the means by which it may be effected.

We need not here enter into the discussion of the nature of "poison germs," of "microzymes," or other ultimate forms which the poisons of contagious diseases may assume, but we may affirm that in every body affected with scarlet fever there is produced poisonous matter, which, passing from the diseased body, is capable of generating anew the same disease as that which affects the body from which it is derived. The proofs of this are so abundant that we cannot for a moment admit that the question is open to discussion. The point of most importance here is to know how long the "poison germs" of scarlet fever retain their vitality—the terrible power of starting anew the changes of which they are the offspring. With regard to scarlet fever, we have more evidence of these "poison germs" retaining their vitality than with many other contagious diseases. Sir Thomas Watson, in his classical lectures on the "Practice of Physic," mentions a case in which a piece of flannel worn round the neck of a scarlet fever patient, being accidentally discovered two years after, and applied to the person of a servant in the family, produced an attack of scarlet fever. Were it necessary, I could mention several instances coming within my knowledge and reading, of the scarlet fever poison lying dormant in woollen clothes for years, and not having lost its vitality, or power of communicating the disease.

Another point of importance with regard to the scarlet fever "poison germs," is the length of time which a person once affected with scarlet fever is capable of communicating the disease to others. When a person has got well of scarlet fever as far as general health goes, it is by no means the case that he is no longer capable of communicating the disease, but many days after he is strong and apparently healthy, he is capable of disseminating "poison germs" from his body. A recent instance has been recorded in one of our medical journals, of the prevalence of scarlet fever in families supplied from the milk of a particular dairy. On searching inquiries being made, it was found that the persons connected with the farm from which the milk was supplied, had been affected with scarlet fever. Although they had not been allowed to milk the cows till they had recovered from the scarlet fever, it was, nevertheless, found that they had been engaged in this occupation whilst the effects of scarlet fever in the desquamation of the cuticle of the skin was still going on. There are abundant other examples on record to show that until the desquamation of the cuticle which always follows scarlet fever is complete, no person who has had scarlet fever is safe from giving it to others.

Such then being the nature of the poison of this disease, what ought to be known in families where it breaks out, and what to be done as the result of this knowledge to prevent its spread? It is no use saying that the doctors will give all necessary directions. In the first place, it may be said that the doctor in nine cases out of ten will not give any directions at all. It is not his interest to do so; and if it were, he gets no information in his books or lectures on the subject at all. The medical profession is not required by its governing or examining bodies to know anything about public health or preventive medicine. In the next place, however admirable may be the directions of medical men, persons utterly ignorant

of the nature of disease will fail to carry out the simples directions given to them. Nothing can be a substitute for a knowledge of the first principles involved in the destruction of "poison germs" in a family attacked with contagious disease.

What, then, ought to be done in a family when scarlet fever, or any other contagious disease, has broken out? In the first place, the entire isolation of the persons attacked should be secured. They should either be removed to a room in the house to which none but the nurse and doctor have access, or the family should be removed to some house of refuge or place where the disease does not exist. It may be urged that this cannot be done; but if it be a mere question of expense, it should be considered whether the cost of the deaths, the funerals, and the doctor's bills of a family of several children, and perhaps the father or mother, may not really, in a money point of view, be greater than any cost of isolation.

But whether isolation is attempted or not, there is another set of facts which must be borne in mind. The "poison germs" of which we have spoken can really be destroyed. If left alone they can lead a life of poisonous activity. We have the means of killing them—poisoning them in their birth, as it were—by certain substances whose properties we well know. We cannot here write a history of disinfectants, but they are well known, and the advertising sheets of every newspaper will afford information with respect to them. The most common and available are carbolic acid, permanganates of soda and potash (Condy's Fluid), chlorinated lime or soda, chloride of zinc (Burnett's Fluid), chloride of aluminium (chloralum), sulphate of iron, and others. These agents have the power of destroying the poisonous activity of scarlet fever germs. In the sick-room and around the patient they should be constantly employed. All the secretions of the person affected, whether they come from the nose, the mouth, or other excretory organs, should be immediately brought in contact with one of these agents. All linen and clothes worn by the patients should be placed in a solution of one of them. Nurses attending on the sick, and medical men touching them in any way, should not leave the room without washing their hands in one of these disinfecting fluids.

Woollen clothing that cannot well be washed should, by some agency or another, be exposed to heat. It is well known that a temperature of 212° F., the temperature of boiling water, will destroy poison germs. Woollen clothing of all kinds, such as shawls and mantles, men's clothes, as also curtains, bell-pulls, carpets, rugs, and beds, should be placed in ovens, or by some contrivance or another exposed to a heat above 212°. In St. James's, Westminster, a disinfecting apparatus has been erected in the Workhouse-yard, where the various articles mentioned can be disinfected.

Having thus indicated general principles, I must leave details. I am convinced that the holocaust of victims that we annually offer to this Moloch of scarlet fever arises from ignorance, and that a general knowledge alone of the facts above stated can suffice to drive from us this plague, so disgraceful alike to our intelligence and our humanity.

E. LANKESTER

SCHIMPER'S VEGETABLE PALÆONTOLOGY

Traité de Palæontologie Végétale. Par W. Ph. Schimper. Tome ii. Première Partie, pp. 522. (Paris, 1870.)

THE first part of the second volume of this very important work (the first volume of which was reviewed in the first number of NATURE) has just appeared, with a quarto atlas of twenty-five plates; and it maintains the high character with which it commenced. In this part the systematic description of the families, genera, and species, is carried down from *Lycopodiaceæ* to the end of Monocotyledons; and the same excellent plan is carried out, of giving under each principal group its most important botanical characters and geographical distribution, drawn from living types; thus supplementing the excessively meagre descriptions that the fossils afford, enabling the reader rightly to appreciate the strength or weakness of the evidence on which the alliances of the fossils are founded, and indicating the organs or structural points most desiderated, and to obtain which collectors should search for illustrative specimens.

The following extracts and remarks will give some insight into the general nature of this volume, and illustrate the author's views as to the relations between many of the most important existing types of vegetation and their fossil representatives or allies.

Dr. Schimper ranks the great recent group of club-mosses and their allies, *Lycopodineæ*, as a class, with four subordinate families; of which two, *Lycopodieæ* and *Isoeteæ*, are abundant at the present time; the other two, *Lepidodendreæ* and *Sigillarieæ*, disappeared before the Tertiary epoch.

Under the order *Lycopodiaceæ* he includes two families: of these (1), *Lycopodieæ*, which abounds at the present day, and inhabits all latitudes from the equator to the Arctic regions, Dr. Schimper recognises only seven fossil species, all congeneric with *Lycopodium* itself, and, strange to say, confined (with the exception of one doubtful species) to the coal measures—a most remarkable fact, if capable of confirmation; but, after making every allowance for the imperfection of the geological record, it appears impossible to admit that a group so well represented now-a-days should be absent from all intervening beds, including the most modern tertiaries; and it is a startling proof either of the vagueness of the characters by which we ally our fossils to existing types, or of the imperfection of the said record. Upon the whole, and under such suspicious circumstances, we should be rather disposed to doubt the fossils being Lycopodiums at all.

Dr. Dawson's four species of this order, from the Devonian of North America, Dr. Schimper considers not to be recognisable as such; so that, little as his own data can tell us of the fossil *Lycopodineæ*, there is a lower depth of obscurity still.

Of the second family, *Lepidodendreæ*, consisting mainly of arborescent plants, all are extinct: it includes the principal genus *Lepidodendron*, with fifty-six species, all carboniferous, and with a host of synonyms, generic and specific:—*Halonia*, *Lepidoploios*, *Knorria*, and *Ulodendron*, all well known to English palæontologists. *Lepidophylla* and *Lepidostrobi* he regards as respectively leaves and fruits of some one or other of the above.

To the third family, *Isoeteæ*, which includes some fifty or

sixty quill-worts and pill-worts of the temperate regions of the globe, he attributes three fossil genera; *Isoetes* itself (tertiary), and the remarkable *Psilophyton* of Dr. Dawson, which he regards as allied to *Pilularia* and not to *Psilotum*. But here again we are confronted by the suspicious fact, that nothing resembling *Psilophyton* has hitherto occurred between the Devonian and the present epoch!

The fourth family, *Sigillariæ*, is only known in a fossil state. *Sigillaria* is the principal genus, with eighty-three species. *Stigmaria* Schimper regards as roots, but not of *Sigillaria* only, for in a supplementary note to this genus he announces the discovery of a specimen of *Knorria longifolia* (one of the *Lepidodendreæ*), the roots of which are a *Stigmaria*, the base of the stem is his own genus *Ancistrophyllum*, and the upper part is *Didymophyllum Schottini* of Göppert; thus knocking four genera into one at a blow. Add to this the fact that the leaves and organs of fructification, now unknown or unrecognised, may represent two more genera, and that there is a suspicious look of *Ulodendron* in one point of the structure, and we have as instructive an example of the condition of our knowledge of the carboniferous flora as can be desired.

Passing from Acotyledones to Cotyledones, or flowering plants, Dr. Schimper's first class is of *Cycadeæ*. They date doubtfully from the middle of the carboniferous, allowing *Noeggerathia*, &c. to belong to this alliance; in the Permian they are pretty certainly present, and they abound in the Trias and Jura formations. Of the two tribes composing this group, the *Zamieæ*, which now extend from the Old to the New World, seem to have appeared first; both these and the *Cycadeæ* proper (which are now confined to the Old World) are found in greatest abundance in the Jurassic age, whence they decline and disappear in the cretaceous, except *Zamiæ*, of which one species persists to the middle of the Tertiary epoch.

Here again, if we hold that the geological record tells a fragment of truth, we must believe that the Cycads and Zamias, which occupy so very wide an area of the globe in the present day, and whose organs and tissues are so well suited for petrifaction, had all but disappeared from the globe during the lapse of countless ages, to reappear in numbers, and that over a most extensive area.

No fewer than thirteen genera of fossil *Zamieæ* are described, and about twenty of *Cycadeæ*, including a multitude of species; both genera and species are however very badly, if at all, defined, being most fragmentary; and Dr. Schimper was of course ignorant of Mr. Carruthers' paper on the Fossil *Cycadeæ* of the secondary rocks of Great Britain, in the twenty-sixth volume of the Linnean Transactions, which throws great light on the subject. The genus *Trigonocarpus* Dr. Schimper doubtfully regards as consisting of fruits of *Cycadeæ* (describing fifteen species), together with *Rhabdocarpus* (twenty-four species), *Cardiocarpus* (twenty-one), and *Carpolithes* (nine).

Ascending in the scale of vegetable life, we come to the great group of Conifers, which seem to have presented such remarkable facilities for petrifaction in all ages from the carboniferous onwards, and which is divided into four great sections. 1. *Abietaceæ*, with four families; *Walchieæ* and *Voltzieæ*, of very doubtful affinity, if Conifers at all, and of which the one is usually placed in *Lycopodiaceæ*, and the other in *Cupressineæ*—neither have any recent representative; *Araucariæ*, comprising four extinct genera, together with *Araucaria* itself; and *Abietieæ* proper, including *Pinus* with 101 species. All appear tertiary; many are miocene, extending from Spitzbergen southwards; and, judging from the number found in single localities, as at Armissan, it would appear that either the pine-forests of those days were, unlike the present, formed of numerous species, or, what is more probable, that the supposed specific characters are worthless. Of larch, four miocene species are enumerated; of cedars, three cretaceous; of Abies, twenty-two species, all tertiary.

2. *Taxodiaceæ* form Dr. Schimper's second order of Conifers; it includes *Sequoia* with twelve species, one of which, the well-known miocene *S. Langsdorffii*, he regards, with Heer, as "almost identical with the Californian *Taxodium sempervirens*, now confined to Mexico." In its fossil state it extends from Arctic East and West America and Greenland to Bonn, Italy, and Greece. Another existing *Taxodium*, again, contains a miocene species, absolutely identical with the living *T. distichum* of the Southern States of North America, where it forms a large proportion of the arboreous vegetation of the Great Dismal Swamp. In a fossil state this also extends from Greenland and Spitzbergen to North Italy. Two other existing genera of this group, the North Chinese *Glyptostrobus*, and the South African *Widdringtonia*, are both supposed to be found in the European miocene formations.

3. Of *Cupressineæ*, to which the modern cypress, arbor-vitæ, juniper, and the extensive genus *Callitris* of Australia belong, there are fossil types supposed to belong to recent genera; viz. of *Libocedrus*, now confined to New Zealand and South Chili, there are two Spitzbergen and a European species, all miocene; of *Thuja*, five, all found in amber beds. To the American and Japanese White Cedars (*Chamæcyparis*) two European fossils are referred. The four junipers are a very doubtful lot.

Taxaceæ, represented throughout the northern hemisphere by one living yew, in Japan by the Maiden-hair tree (*Salisburia*), and in the tropics and south temperate regions of both the old and new worlds by various species of *Podocarpus*, have been supposed to be discovered in small numbers in a fossil state. Thus, of *Salisburia*, the existing Japanese species is believed to be found in the miocene of Greenland and near Verona, and two other American species are described—all miocene. The fossil yews are all doubtful, and the *Podocarpi*—eight in all—are probably equally so.

Gnetaceæ, to which the curious existing genus *Gnetum* (tropical), *Ephedra* (temperate), and *Welwitschia* (tropical) belong, are represented by two modern species of *Ephedra*.

The volume closes with 130 pages of Monocotyledonous fossils—a very miscellaneous and for the most part exceedingly obscure assemblage. The existence of this class in the carboniferous epoch is regarded by Dr. Schimper as not proven, and more than doubtful. In the Trias it is pretty well established by *Yuccites* and *Æthophyllum*, but there is a hiatus between that age and the Jurassic, when an obscure set of water-plants and perhaps *Pandanus* occur, to be followed by another hiatus till the cretaceous epoch, when *Gramineæ*, *Cyperaceæ*, and several of the principal orders of monocotyledons appear in some force. Whether, however, we ascend or descend in the scale of vegetable life, as represented by fossil monocoty-

ledons, we feel the generic and even the ordinal determinations of the fossils to be throughout very doubtful, and to be reducible in most cases to the category of bold guesses. To refer the majority of living and growing monocotyledons, and especially of such orders as *Gramineæ*, *Cyperaceæ*, *Naiadeæ*, *Palmæ*, and *Liliaceæ*, to their genera from their leaves alone, is impossible; much more then from the fragmentary fossil remains of these organs. Such genera and species, however, have been made, and Dr. Schimper must sweep them into his net; and the more ungrateful the task, the more obliged we should be to him or performing it so well and so fully.

We shall look for the Dicotyledonous volume of this great and most useful work with much interest, and lay an account of it before our readers as soon as it appears.

J. D. HOOKER

THE YOSEMITE VALLEY AND THE SIERRA NEVADA OF CALIFORNIA

The Yosemite Guide-book. By J. D. Whitney. Published by authority of the Legislature of California. 1869.

MR. J. D. WHITNEY, State Geologist of California, has prepared a guide-book to the Yosemite Valley and the adjacent country, which is a model of its kind. It is well written, and is eminently lucid in its descriptions; it is amply illustrated, and has two clear maps on a large scale half a mile and two miles to the inch); and it is admirably printed (at the University Press, Cambridge, U.S.). The unpretending title of the work gives but a poor idea of its contents; botanists, geologists, and geographers will find pleasure in reading it, although it is ostensibly put forth or the sake of the ordinary tourist. It is a valuable contribution to our knowledge of the Pacific coasts of America, and throws light upon a large district which remains very imperfectly known.

The British public has been already tolerably familiarised with the most striking characteristics of the Yosemite* Valley, Mariposa County, California, by the fine picture of Mr. Bierstadt; and some excellent photographs (by Watkins, of San Francisco), which are now lying before us, show that that artist has not drawn upon his imagination. Some of our readers, however, may perhaps need to be told that it is not only a very remarkable valley, but boasts the possession of the highest waterfalls (with an equal volume of water) in the world. It is trough-shaped; a cross section of it is like the letter U, and in this respect, as well as in its length, breadth, and the steepness of its cliffs, it is comparable to the Valley of Lauterbrunnen in Switzerland, but the dimensions of its cliffs exceed those of that valley, as much as its chief waterfall (the great Yosemite Fall) surpasses the Staubbach. This magnificent cascade makes a descent of more than half a mile in sheer height, and 500,000 cubic feet of water pass over it per hour during the month of June. It is made up of two falls—an upper one, which has a vertical descent of 1,500 feet, and a lower one of 400 feet. The remainder of the total height is consumed by a series of cascades between the two; but seen from the opposite side of the valley, the effect is increased rather than diminished by the subdivision, and it well deserves all the praise which has been lavished upon it.

* The word Yosemite means a full-grown grisly bear.

Mr. Whitney does not believe that the peculiar trough form of the valley has been even modified by glacier action. There are no proofs, he says, "that glaciers have ever occupied the valley, or any part of it," and he scouts the notion that it was *produced* by glacial agency. "In short," he says, "we are led irresistibly to the adoption of a theory of the origin of the Yosemite in a way which has hardly yet been recognised as one of those in which valleys may be formed, probably for the reason that there are so few cases in which such an event can be absolutely proved to have occurred. We conceive that during the process of upheaval of the Sierra, or possibly at some time after that had taken place, there was at the Yosemite a subsidence of a limited area, marked by lines of 'fault' or fissures crossing each other somewhat nearly at right angles. In other and more simple language, the bottom of the valley sank down to an unknown depth, owing to its support being withdrawn from underneath, during some of those convulsive movements which must have attended the upheaval of so extensive and elevated a chain, no matter how slow we may imagine the process to have been." It should be mentioned that the Yosemite Valley is exclusively granite, no remains of sedimentary rocks having been found within it.

Although the Yosemite Valley itself is not ice-ground, in its neighbourhood there are very emphatic traces of glacial action, and it is said that in some places the polish is so perfect upon the rocks, that "the surface is often seen from a distance to glitter with the light reflected from it, as from a mirror." It is remarkable that no glaciers are known to exist at the present time throughout the entire length of the Sierra Nevada in California. Throughout the entire region, from 35° to 42° N. lat., Mr. Whitney asserts that there is not a single glacier. Yet Mount Shasta, at the northern extreme, with a height of pretty well 15,000 feet, is more or less permanently covered with snow upon its upper 6,000 feet, and at the southern end of the district there are numerous peaks (now being surveyed) which range from 14,000 to 15,000 feet, and even higher. We confess that we do not understand how "masses of snow, several miles long, and hundreds of feet in thickness, remain all summer without showing any indication of becoming glacier ice," if they remain permanently of these dimensions; but as such seems to be the fact, there is no use in disputing it.

The most valuable portions of Mr. Whitney's book are those which he has devoted to elucidating the topography of the mountain ranges bordering upon, or in proximity to, the Pacific Coast. We learn from him in a clearer way than we have seen it put before, that there are two great ranges of mountains running throughout the length of California, which are orographically distinct. These he terms the Coast Ranges, and the Sierra Nevada. They are roughly parallel to each other, and to the coast line; and they are divided by the valleys of the Sacramento and San Joaquin rivers. The coast ranges are, geologically speaking, younger than the Sierra Nevada, and they are chiefly made up of cretaceous and tertiary strata. They are also of comparatively small elevation, and their highest summits attain only 8,000 feet. The core of the Sierra Nevada, on the other hand, is mainly granitic, flanked on both sides by metamorphic slates, and has a much greater elevation.

Both of these ranges are totally distinct from the Rocky Mountains, which are hundreds of miles further inland. The only drawback to travel among them is the "poor Indian," who watches travellers from a distance, and signals by smokes. We can well understand the enthusiasm of Mr. Whitney for the mountains of California, and we heartily hope with him, that neither the Yosemite valley nor the grove of Sequoias which—by a unique act of Congress—were ceded to the State for public use, resort, and recreation, for all time, will be suffered to fall into the hands of the acute but unpatriotic speculators who are endeavouring to use them for their own purposes; and who will, if they are not "sat upon," undoubtedly turn them into "gigantic institutions for fleecing the public." W.

OUR BOOK SHELF

The Science and Art of Arithmetic: Part II. Vulgar Fractions; Part III. Approximate Calculations. By A. Sonnenschein and H. A. Nesbitt, M.A. 260 pp. (London: Whittaker & Co. 1870.)

THE authors of this excellent school arithmetic are to be congratulated on having brought their work to a successful termination. In our notice of Part I., which appeared in a former number of NATURE,* we pointed out the principles by which the authors had been guided, and as we believed those principles to be sound, and the authors to have carried them out successfully, we had no hesitation in commending the work as we did. The same good arrangement, ample store of illustration, and copious examples for practice, are to be found in this volume as had place in the first. The fulness with which the elementary portions were treated appeared to us to be a merit rather than a defect. From such wealth of illustration each teacher could select what was most suited to his purpose. In this volume we have more advanced subjects treated in like manner. But an analysis of the contents will give a good idea of the work. Under Part II. we have the subject of Vulgar Fractions clearly treated, with applications to Practice, and a chapter which treats of Proportion, the Chain Rule, Compound Proportion, and Proportional Parts. In Part III. are chapters on Converging Fractions, Decimals with their properties, and several applications to Money, Weights, Measures, &c., the Metric System, Progressions, Interest, Discount, Stocks, Evolution, and a good chapter on Arithmetical Complements. There is also a chapter in which we have Continued Product to a given limit, Compound Interest, Equation of Payments, Complex Decimals, Duodecimals, and International Calculations. At the end of the work are given 250 Miscellaneous Exercises. There is enough here to satisfy any youthful arithmetician, and the methods employed are the "latest out." The complete work gives ample evidence that it is the composition of men who have given much time and thought to the subject, and have had much tuitional experience. R. T.

Die Schmetterlinge Deutschlands und der Schweiz, systematisch bearbeitet. Von H. von Heinemann. Zweite Abtheilung, Kleinschmetterlinge. Band II. Die Motten und Federmotten, Heft 1. 8vo. (Brunswick: C. A. Schwetschke and Son, 1870.)

M. HEINEMANN'S work on the Lepidoptera of Germany and Switzerland is well known to entomologists, by whom it is highly valued. It contains an admirable systematic description of the species of butterflies and moths inhabiting the above-mentioned countries, and has been carried out by the author in so conscientious a manner that the students of European Lepidoptera can hardly wish for a better handbook. The author is now approaching the conclusion of his labours. The first section, including the larger forms of Lepidoptera, was completed some years ago; of the second section, the first volume, published in 1863 and 1864, contained the descriptions of the Tortrices and Pyralides, and the part now before us commences the true Microlepidoptera, the Tineæ and Pterophori. The former are exceedingly numerous, and the present portion contains descriptions of the species of only five out of the thirteen families into which the author divides the group. W. S. D.

* See NATURE, Vol. II., p. 186.

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 Teachings of Tribulation—"The Captain"

THE technical questions suggested by the loss of Her Majesty's ship *Captain* have received their full share of attention—indeed, more than their share, for able leaders and letters in the newspapers will not teach us how to build war ships. I do not intend to trouble you with freeboards, turets, and metacentres. Nor do I propose to dive into all the published evidence and statements, in order to discover some delinquent on whom to saddle the blame of the terrible disaster.

The case of the *Captain* is an example, on a scale sufficiently startling to attract the notice of the whole nation, of a want in our administrative arrangements which has hitherto escaped the notice of the many. The few who have long deplored this want did not require that a quarter of a million of money and 500 gallant lives should be sacrificed in order to prove its existence. Employing, then, the loss of the *Captain* as merely one very striking instance out of thousands of others that pass unheeded every day, permit me to suggest what is the administrative lesson taught by it. The facts must first be briefly stated. This cannot be better done than by quoting a portion of the judgment of the court-martial, who tell us that they "find it their duty to record the conviction they entertain that the *Captain* was built in deference to public opinion as expressed in Parliament and through other channels, in opposition to the views and opinions of the Controller of the Navy and his department, and that the evidence all tends [*sic* in *Times'* report] that the Controller of the Navy and his department generally disapproved her construction."

Now let us analyse the system the working of which justified the above reiterated condemnation. Beginning at the top, we have a Minister of State, a gentleman usually of high character and great general attainments, but not necessarily conversant with naval architecture. Being a party politician, he may at any moment vacate his post on some question totally unconnected with his department. In order to provide him with the knowledge which he does not pretend to possess, and to supply the element of permanence in which he is also deficient, he has under him secretaries, superintendents of works, and scientific advisers. These persons are in the strictest sense of the terms also subordinates of the Minister, bound to obey his orders. Outside this department are two bodies, independent of the Minister, and capable of bringing enormous pressure to bear upon him, namely, inventors and the public—the first interested, the second ignorant of science and owing no responsibility to any one. I disclaim any personal allusions in this analysis. Well, here we have two forces pulling the Minister different ways. The whole question is, whether the departmental, or, as we may call it, the home, force is strong enough to enable the Minister to resist, when necessary, the foreign invader; whether reiterated adjurations, noisy clamour, and threatened loss of popularity will silence and overbear official counsels. The question is already answered. The official counsels were overborne. It may be argued, however, that the official counsels may be wrong and the public right. But the official counsels in the case before us were right and the public wrong. Here we are landed in a difficulty. How is the Minister, who by our hypothesis has no knowledge of the question at issue, to tell when his advisers are right and when the public? He cannot tell. But he exercises his discretion. Now, it must be remembered that his advisers are his subordinates, and therefore, taking men as we find them on an average, not in a

position calculated to prompt them to oppose very strenuously a policy on which they see that their superior is set. Moreover, the advisers, who in the case before us are but two or three individuals, may be, as it was believed they were, prejudiced against the contrivance under consideration. It would be natural that the Minister should make a considerable deduction from the weight of their remonstrances on account of the departmental jealousy by which he might imagine them to be more or less tinctured. Thus he is at sea, as deficient in the elements of stability as the *Captain* herself.

An obvious remedy for such a state of things might be to appoint as permanent heads to our great technical departments men thoroughly acquainted with their duties who could act on their own independent judgment. But this would subvert that perfect and inviolable edifice, the British Constitution. Far be it from a humble unit like myself to attempt such sacrilege!

What remains, then, as we cannot repress inventors and silence public clamour if we would, than to give the Minister stronger and more independent scientific support than that which was found in the case of the *Captain* too weak to prevent the most humiliating and disastrous blunder of modern times?

The suggestion I now venture to make is not new, nor do I make it now, on the pinch of the moment, for the first time. I brought it more than a year ago before a committee of the British Association, of which I was chairman. My proposal was, and is, that a powerful body of the most eminent men in every branch of science should be constituted a permanent paid Council for consultative, as distinguished from executive, purposes. Space does not admit of my detailing the constitution, mode of electing, and functions of this body. But, having long had the matter in my mind, I may say that I see no difficulty in securing the main conditions of varied and profound acquirements, and of due official relation to, yet thorough independence of, the Ministry and politics of the day. I need hardly say that such a consultative Council should comprise not only men distinguished in abstract science, but also men representing all branches of the sea and land forces, all technical departments, the public works, and the principal arts and manufactures of the country.

No mistake can be greater than to consider this proposal revolutionary, as some at first sight have done. It is in fact only a consolidation and systematisation of agencies actually in existence. The principle of supplying the country gentlemen who become Ministers of State with scientific advice through permanent secretaries and other subordinates, and through temporary committees entrusted with specific inquiries, has long been in force. It is certain that these individuals and bodies are often selected capriciously, and it is not saying too much to assert that the results of their labours would have been more valuable if their functions had been less narrow and their existence less precarious. The great domain of physical science cannot be parcelled out in neat little squares like a chess-board; its varied districts, as Nature has planned them, run into and mix with each other so intimately that in order to trace the boundaries of one, some knowledge at least of the adjoining tracts is necessary. Special committees, however well chosen, are seldom even numerically strong enough to comply with these conditions.

The Council now advocated purposes to substitute for innumerable, scattered, temporary, incomplete, hand-to-mouth expedients a permanent, properly selected organisation. In one case the work is done somehow—we see to our cost how; in the other it will be done as well as human intelligence can do it; but in both cases the very same work will be done—namely, that of bearing really the burden of responsibility which Ministers only bear nominally. The principle will be the same under the existing and the proposed régime, but whereas it is now only recognised, it would then be realised. The details of the proposed reform, which are present to my own mind, would occupy more space than you could spare on one occasion from other important subjects. Nor is it possible in the brief limits of one letter to meet all those objections, now so well known to me, which start up directly this subject is mooted. Should, however, the remarks I have ventured to offer prove of sufficient interest to provoke discussion, I will on a future occasion solicit your permission to extend them.—I am, Sir, obediently yours,

Oct. 22 ALEX. STRANGE, Lieut.-Colonel

The Earliest Mention of the Aurora Borealis

THE first appearance of the Aurora Borealis noticed in Mr. E. J. Lowe's "Natural Phenomena and Chronology of the Seasons" is that on Jan. 30, 1560. Other appearances are mentioned under the years 1564, 1574, and 1575. No further record of it appears until Nov. 10, 1707, when it was seen in Ireland. Five more displays are noticed between this and the memorable one of Feb. 23, 1716, which, happening to take place on the day of Lord Derwentwater's execution, obtained for the phenomenon in the north of England the appellation of "Lord Derwentwater's Lights." On March 6 of the same year occurred another grand display, which is referred to in the chronologies of remarkable occurrences published in the almanacks of last century as "The Great Amazing Light in the North," continuing to be seen (more or less) at several times since, yearly. Previous displays in this century had probably not been visible in London. The phenomenon is thus described, with an attempt at explanation, in the *Flying Post* of March 8:—

"Last Tuesday night, as soon as it was dark, a pale sort of a light broke out in the north-west part of our horizon, which looked like the dawn of day, or rather like the moon breaking through the clouds. It darted many streams towards all parts of the sky, which looked like smoke. It proceeded towards the S.E., and continued by several intervals till midnight, when it totally disappeared. Some ignorant people, whose ideas are on such occasions stronger than their senses, fancied they saw armies engaged, giants with flaming swords, fiery comets, dragons, and the like dreadful figures; and others fancied they heard the report of fire-arms, and smelt powder; whereas there was nothing but what may easily be accounted for from natural causes, the sun having been hot for two days past, and particularly that afternoon, by which vapours were exhaled both from the earth and water, and the sulphurous particles mixed with them taking fire might occasion that light, and some coruscations, as is very common over marshy and fenny places in spring and summer nights."

The writer goes on to observe that "the disaffected party have worked this up to a prodigy, and interpret it to favour their cause," which accounts for a very obvious design to write the phenomenon down. Another display, not in Mr. Lowe's list, was witnessed at Leominster, on Feb. 21, 1718, as appears by a letter in the *Weekly Journal* of March 1. The streamers are there compared to the tail of the great comet of 1681.

London, Nov. 7 R. G.

THE fallacy of trusting for scientific information to any other than a recognised scientific source, cannot be better illustrated than by Mr. Pocklington's letter in your issue of Nov. 3. He there seems to thinks that the statements of the editor of a volume of popular poems on a matter of science are worthy of notice, and therefore thinks it worth while to inquire whether or not it is true that no aurora borealis ever appeared before 1715. The absurdity of such a rash statement is so apparent that it seems almost superfluous to show it. In 1754 a book was published by M. de Mairan, entitled, "Traité Physique et Historique de l'Aurore Boréale," in which he collects from all the writers, ancient and modern up to that date, accounts of all the Aurorae Borealis which had been seen. Their total number amounts to 1,441 between the years A.D. 583 and 1751.

These are divided as follows: From A.D. 583 to 1354, 26 were recorded; 1354 to 1560, 34; 1560 to 1592, 69; 1592 to 1633, 70; 1633 to 1684, 34; 1684 to 1721, 219; 1721 to 1745, 961; 1745 to 1751, 28. Of these, 972 occurred in the winter half year, and 469 in the summer half year, the greatest numbers occurring in March and October. Since that date the two most remarkable displays have been those of the 23rd of October, 1804, and the 24th of October, 1847. An account of the latter aurora was published at Cambridge in the same year, giving twelve large coloured lithographic views of the brilliant display which are, without doubt, the best views ever given of any Aurora.

J. P. EARWAKER

Merton College, Oxford, Nov. 5

THE quotation given by C. Pocklington in your last issue as the words of the Editor of Routledge's edition of Collins's Poems, is the very note given by Dr. Langhorne in the "Poetical Works of William Collins," published in the year 1808, in a small book entitled "The Laurel," and as it has not been reprinted word for word its sense is somewhat obscured. In the original it runs thus:—

"By 'Young Aurora' Collins undoubtedly *meant* the first appearance of the Northern Lights, which happened about the

year 1715, at least it is *most* highly probable, from *this* peculiar circumstance, that no ancient writer *whatever has* taken any notice of them, nor even any *one* modern, previous to the above *period*."

The words restored are those italicised. The passage, as it came from Dr. Langhorne, is excusable for the knowledge displayed, but cut up as it is by the *modern* editor, shows great lack of it; and more than this, the honesty is not what every one would admire. Both, however, are incorrect. Dr. Halley, in the Phil. Trans., No. 347, page 406, gives a history of auroral observations, and for the information of "C. P." I have extracted a few particulars showing that the "northern lights have been observed and recorded long before 1715.

The first account, says Dr. Halley, recorded in English annals is that of the appearance which was noticed January 30, 1560, and called "Burning Spears," by the author of a book entitled "A Description of Meteors," by W. F., D.D. London: 1654. The next of a like kind was the appearance recorded by Stow, which occurred on October 7, 1564.

In 1574, Camden and Stow inform us, an Aurora Borealis was seen for *two* successive nights, viz., the 14th and 15th of November, with appearances similar to those observed in 1716, and which are not commonly noticed. The same phenomenon was twice seen in Brabant in 1575, viz., on the 13th of February and the 28th of September, and the circumstances attending it were described by Cornelius Gemma, who compares them to spears, fortified cities, and armies fighting in the air. In the year 1580, M. Mastline observed these *phasmata*, as he calls them, at Baknang, in the county of Wurtemburg, in Germany, no less than seven times in the space of twelve months; and again at several different times in 1581. On September 2, 1621, the same phenomenon was seen all over France; and it was particularly described by Gassendus in his "Physics," who gave it the name of "Aurora Borealis." Another was seen all over Germany in November 1623, and was described by Kepler. Since that time, for more than eighty years we have no account of any such phenomenon either at home or abroad. In 1707 Mr. Neve observed one of small continuance in Ireland. In the years 1707 and 1708 this sort of light had been seen no less than *five* times.

There is not the least doubt in my mind that the commentator of Collins must have been wholly ignorant of the literature of scientific records, else he would never have said what he did on the lines in question.

In the Orkneys the Northern Lights are known by the name of the "merry dancers." And it is not at all surprising among an unphilosophical people, that this, one of the grandest phenomena in nature, should be the subject on which the imagination fondly dwells.

The various conflicts of Odin may probably have been suggested by the dancing and flickerings of these Lights.

In the "Prosa Edda" there is a direct allusion to the Aurora Borealis; at least, the translation given in Mallett's "Northern Antiquities," edited by J. A. Blackwell (1847), page 404. It says:—"From his skull," continued Thridi, "they formed the Heavens, which they placed over the earth, and set a dwarf at the corner of each of the four quarters. These dwarfs are called East, West, North, and South. They afterwards took the wandering sparks and red hot flakes that had been cast out of Muspellheim, and placed them in the Heavens, both above and below, to give light unto the world, and assigned to every *other errant coruscation* a prescribed locality and motion."

If by "errant coruscation" he meant all the meteoric phenomena including the Northern Lights, then we have in this "Edda" the most ancient record of this observation.

JOHN JEREMIAH

43, Red Lion Street, Clerkenwell, Nov. 4

Hereditary Deformities

THE following instance of hereditary deformity is taken from Mr. L. W. Dillwyn's "Materials for a Fauna and Flora of Swansea and the Neighbourhood" (Swansea, 1848), a privately printed and therefore little known book. It will be seen that the evidence respecting the origin of the malformation is not conclusive: "In 1804 there was in the Neath Valley a remarkable breed of a sort of sheep-dog, with nothing more than a flat depression, about half an inch broad, between the nostrils, and was said to have originated in a bitch which had her nose longitudinally cloven by some accident. The breed retained this deformity forseveral years, but I believe it is now extinct."

The same book contains the following very remarkable illustration of the dispersion of species by means of oceanic currents: "On the sandy sea-shore, opposite the race-course on Crumlyn Burrows, and more than a mile from any sort of house or garden, Mr. L. L. Dillwyn, in 1839, found a thriving young plant of *Yucca gloriosa*, and it had all the appearance of having risen from a seed which the tide had cast there. Notwithstanding its exposed situation, and the looseness of the soil, this native of Carolina was not materially injured by the unusually severe winter of 1840-1, and Mr. Moggridge informs me that for two or three years it continued to thrive, till it was destroyed by a heap of shingle, which a violent storm and high tide threw over it."

R. G.

London, Nov. 7

Fertilisation of Plants

WITH candle in hand I have pored through all that you have printed of the speeches delivered by the members of the British Association at Liverpool, till I made a full stop at page 482, where I found "Observations on Protandry and Protogyny in British Plants, by A. W. Bennett, F.L.S.—The arrangement of the reproductive organs in hermaphrodite plants, the presence in the same flower of both pistil and stamens, suggested to the minds of the older botanists no other idea than that of self-fertilisation. It is, however, now generally admitted that even in hermaphrodite flowers, cross-fertilisation is the rule, self-fertilisation the exception. Two sets of facts have been especially observed—in particular by Darwin in this country, Hildebrand in Germany, and Delpino in Italy—to favour cross-fertilisation in hermaphrodite flowers, the phenomena of dimorphism and trimorphism, and the special arrangements which render it easier for the pollen to be brushed off by insects visiting the flowers than to fall on its own stigma." I cannot understand what this special arrangement which renders it easier for the pollen to be brushed off by insects visiting the flowers, can mean, when applied to the flowers of our forced peaches, French beans, &c., in blossom during the dark months of December, January, February, and March, when there is no insect on the wing.

As I do not keep bees in my garden, and there are none kept within a mile of it, and if there should a swarm of bees take up an abode near us, I destroy it, my peaches blossom and set their fruit without the assistance of the bee or any other insect. It is the sun that sets the blossoms, so to speak, and consequently we make the best use of every gleam of sunshine that by chance may visit the earth during the blooming time of our forced "things."

From this fact it must be evident that the Creator did not leave the all-important functions of the fertilisation of flowers to the insects which are simply in search of food, though it amuses our great thinking and closely observing philosophers to try to assure us plodding practicals of our sheer ignorance in the *modus operandi* of the fertilisation of hermaphrodite flowers.

That insects disperse the pollen of flowers there cannot be a doubt. Neither is there a doubt that hermaphrodite flowers fertilise their own pistils during the dark months of winter, without the presence of plundering insects. An industrious morphologist might find much employment in an early forced peach-house, in flower, say in January, on which the sun may not have gleamed since the first blossom opened.

A PEACH-GROWER

[Our correspondent should read the article in NATURE, No. 1, by Mr. Bennett, "On the Fertilisation of Winter Flowering Plants," which he will find to be in accordance with his own views.—*Ed.*]

Chip Hats

IN the last number of NATURE it is stated that palm-leaf was formerly imported to St. Alban's for the purpose of making *chip* hats. Allow me to state that the trade has never ceased. Large quantities of palm-leaf are imported under the name of "Brazilian grass," and many persons are constantly employed in St. Alban's and the villages around in plaiting these hats, principally for exportation. They are called *grass* hats.

Kew, Nov. 16

F. H. HOOKER

The Electric Telegraph and Earthquakes

THE use of the electric telegraph for recording earthquakes is not so new as the *Echo* (quoted in your last number, p. 35) supposes. Dr. Hector, Director of the Geological Survey, has, ever since the last great convulsion, systematically

used the telegraph for this purpose in New Zealand, where indeed it forms an important element in the admirable system he has inaugurated for recording shocks throughout the islands. In his last letter he writes as follows:—"Not long ago, one operator asked another 200 miles distant 'Did you feel that?' and got the answer, 'No. What? Yes; there it is,' all in a breath, so to speak!"

Dr. Hector, writing in August last, goes on to say, "I have called attention to the coincidence of the aurora in both hemispheres on April 5, and can't help thinking that when our observations are sufficiently extended, we shall find many phenomena that are looked upon as local to be general. Could not NATURE give us a column recording such phenomena as auroras, earthquakes, tornados, &c., experienced in all parts of the world, somewhat in the form of an almanac? At the present one has to rummage over all sorts of periodicals, and after all find the matter most imperfectly recorded. We shall soon have a capital earthquake register here, since I induced the Government to cause the observers to report every shock they felt in the colony. Their number and coincidence is very remarkable, and I shall publish the results as soon as I have collected a sufficient number. I feel a great want of a good table eusmometer which should be cheap enough to distribute to all telegraph stations." Perhaps some of your readers can inform Dr. Hector where he can procure this great desideratum. JOS. D. HOOKER

Ocean Currents

AMONG the "Notes" in last week's number it is mentioned, on the authority of the *Inverness Courier*, that a number of glass globes had lately been washed ashore on the western coast of the Isle of Lewis. The question is asked, "Have these been used for some experiments made for the purpose of ascertaining the course of some ocean current?" I have seen precisely similar globes which had been cast ashore after rough weather on the western coast of Shetland; and I ascertained that they were floats used by Norwegian fishermen for buoying their long lines. North-easterly winds had drifted them to that part of the coast where they were found; and the same cause may be assigned for the occurrence of the glass globes or floats in the outer Hebrides. J. GWYN JEFFREYS

Nov. 12

IN reference to the statement (NATURE, Nov. 10) that glass globes have been washed ashore on the west side of the Isle of Lewis, may not these be floats which are used by the fishermen of Newfoundland? These are, I believe, occasionally found as far to the north-east as Nova Zembla, and this fact is, I think, not indicative of "some ocean current," but of the *aerial* current from the south-west, which is so prevalent in the north temperate region, and which may be called the *return* current of the north-east trade wind of the north tropical region.

This south-west wind from the sea modifies the summer heat and the winter cold of west coasts in the north hemisphere, and produces their so-called "insular climate."

GEORGE GREENWOOD, Col.

Brookwood Park, Alresford, Nov. 12

The Milky Way

THERE appears to be in Wales a remnant of a tradition connected with the Milky Way. During a short stay in Caermarthenshire, an old man, well read in local history, and who is apparently the oracle of the neighbourhood of Llangadock, directed my attention one evening to the Milky Way, remarking at the time "we shall have fair weather to-morrow, as you see it is in the south," meaning that the wind will blow from that quarter. My friend supported this extraordinary statement by appealing to the Welsh word "Heol y Gwynt," the *road or way of the wind*. Can any of your readers inform me whether this belt of stars is the subject of a fable in Britain, or how it came to be connected with foretelling the weather? The Scandinavians call it the "Road of Winter;" possibly "Heol y Gwynt" may be traced to northern influence, but, in the absence of facts, I will not commit myself to this explanation.

JOHN JEREMIAH

43, Red Lion Street, Clerkenwell, Nov. 12

P.S.—"Heol y Gwynt" is the only proper Welsh name for the Milky Way, and is not a mere local one.

The Colour of Butterflies' Wings

IN making some experiments a short time ago, I came across a fact of which I was hitherto ignorant. I wished to test the effect of acid on the colours of the wings of a butterfly or moth, and with this view applied muriatic acid to a dried and set specimen of the Six-spotted Burnet (*Zygæna filipendulæ*). The red parts immediately became yellow, while the dark parts were unaltered. In subsequent experiments the red was the only colour in any moth which underwent any change. When there was no red there was no change, and the only change was from red to yellow. Next I applied the acid to the red parts of the Red Admiral Butterfly (*Vanessa atalanta*), when, to my surprise, no change took place. Comparative examination under the microscope in no way cleared up the matter. I now seek for an explanation of this phenomenon, which appears to point out a clear difference in the nature of the moth and butterfly's wing. A remarkable fact, perhaps connected with this, is that a yellow variety is known of almost every moth containing red in the wings. Perhaps some of your scientific readers may be able to throw some light upon this subject. E. V. F.

Winchester, Oct. 18

A New Mode of Evolving Light

A SINGULAR phenomenon of the evolution of light has been recently observed by me. By tearing sharply a piece of twilled calico into strips in a room well guarded from light, a perceptible luminosity was clearly distinguishable, which appeared at its maximum at the final parting of the fabric. This phenomenon is exceedingly well marked in dry, new calico, and appears to me due to the dressing, as after being washed no light is evolved. Whether attributable to electricity, phosphorescence, or fluorescence, I leave for further investigation. The light appears similar to that produced on breaking a lump of sugar in the dark. So far as I can ascertain, the phenomenon of light being evolved on tearing a fabric is new, hence I hope worthy of notice in your valuable journal. ANDREW PRITCHARD

Canonbury, N.

Philology and Darwinism

IN NATURE, No. 30, I attempted to show that the analogy between Mr. Darwin's teachings as regards plants and animals, and the conclusions of comparative philologists, broke down, when we compared man's conscious influence on plants, &c., to his more and more enlightened control of language. Man's influence on organic forms tends to produce variety, while, with increase of knowledge, language is becoming more uniform. Mr. Ransom (No. 32) replied that the difference I insisted on seemed imaginary; and if man's object was to produce uniformity in plants or animals, that then the domesticated species would be likely to become less varied than the wild species of the family. Now it seems to me that if man had any such intention, no care on his part could produce permanent types yielding so little divergence in the individuals during enormous time as those produced by nature; a permanence so marked that geology only throws light on the law of evolution, in anything like a direct way, through the study of the mammalia (see Prof. Huxley's recent address on the progress of Palæontology), and even with regard to the mammalia naturalists of high standing refuse to see anything but permanent and all but uniform types, necessitating the hypothesis of special creation. How man could obtain by any possible efforts (and with some breeds his aim is uniformity) to maintain species as invariable as nature has done, is what is hard to conceive.

On the other hand, as man's mental faculties become blunter, his consciousness less vivid, and his material conditions harder, his language is more and more in a state of flux, branching forth continually into dialects which mark one group of men from another. Thus in some Polynesian islands we have a number of languages as distinct as those of great part of Europe; and missionaries complain that the Bible requires to be translated anew to the same tribe more than once in a century; in some cases the very numerals in a few generations becoming changed, partly from whimsical customs, and partly from want of frequent intercourse, and of any literature being at the disposal of the tribe. In the progress of time, we may look forward to a period when the language of Shakespeare, Milton, and Addison, which is considered as good English as that of our daily papers—may be the language of the world, but we can scarcely expect that the planet will ever hold only one species of animal or plant. S. J.

NATURAL SELECTION—MR. WALLACE'S REPLY TO MR. BENNETT

MR. A. W. BENNETT'S article entitled "The Theory of Natural Selection from a Mathematical Point of View," contains several criticisms on my own writings, and touches on some points which have not yet been fully discussed. I propose, therefore, to reply to such of these as appear to be of sufficient importance.

The first objection brought forward (and which had been already advanced by the Duke of Argyll) is, that the very title of Mr. Darwin's celebrated work is a misnomer, and that the real "origin of species" is that spontaneous tendency to variation which has not yet been accounted for. Mr. Bennett further remarks, that throughout my volume of "Essays" I appear to be unconscious that the theory I advocate does not go to the root of the matter; and this unconsciousness is not apparent only, for I maintain, and am prepared to prove, that the theory, if true, does go to the root of the question of the origin of species. The objection, which, from its being so often quoted and now again brought forward, is evidently thought to be an important one, is founded on a misapprehension of the right meaning of words. It ignores the fact that the word "species" denotes something more than "variety" or "individual." A species is an organic form which, for periods of great and indefinite length as compared with the duration of human life, fluctuates only within narrow limits. But the "spontaneous tendency to variation" is altogether antagonistic to such comparative stability, and would, if unchecked, entirely destroy all "species." Abolish, if possible, selection and survival of the fittest, so that every spontaneous variation should survive in equal proportion with all others, and the result must inevitably be an endless variety of *unstable forms*, no one of which would answer to what we mean by the word "species." No other cause but selection, has yet been discovered capable of perpetuating and giving stability to some forms and causing the disappearance of hosts of others, and therefore Mr. Darwin's book, if there is any truth in it at all, has a logical claim to its title. It shows how "species," or stable forms, are produced out of unstable spontaneous variations; which is certainly to trace their "origin." The distinction of "species" and "individual" is equally important. A horse or a number of horses, as such, do not constitute a species. It is the comparative *permanence* of the form as distinguished from the ass, quagga, zebra, tapir, camel, &c., that makes them one. Were there a mass of intermediate forms connecting all these animals by fine gradations, and hardly a dozen individuals alike—as would probably be the case had selection not acted—there might be a few horses, but there would be no such thing as a species of horse. That could only be produced by some power capable of eliminating intermediate forms as they arise, and preserving all of the true horse type, and such a power was first shown to exist by Mr. Darwin. The origin of varieties and of individuals is one thing, the origin of species another.

Mr. Bennett next discusses the phenomena of "mimicry," and proposes to show, by mathematical calculations, that the effects could not be produced by natural selection. But, at the very outset, he makes an important error, which seriously affects his subsequent reasonings; for he leads his readers to understand that there is only one completely mimicking species of *Leptalis*, while the majority are of the normal white-butterfly type. The fact is, however, that but few species of *Leptalis* retain the simple colouring of their allies the Pieridæ, while the great majority are either coloured like the Heliconidæ, or show a considerable amount of colour or marking in that direction. He is also apparently unaware that some Heliconidæ (*Ithomia eurimediæ*, for example) approximate in colour to the normal white and yellow species of *Leptalis*, and thus renders it much less difficult to understand how a sufficient amount of variation in colour might occur at a first step, to produce a resemblance which, viewed at some considerable distance, would be deceptive, and therefore useful.

We next come to the demonstration by means of figures, and we here find still more serious errors. Mr. Bennett says, that supposing a *Leptalis* may vary in twenty different ways, one only being the direction required,—"the chance of any individual producing a descendant which will take its place in the succeeding generation varying in the required direction, is $\frac{1}{20}$; the chance of this operation being repeated in the second generation is $\frac{1}{20^2} = \frac{1}{400}$; the chance of this occurring for ten successive generations is $\frac{1}{20^{10}}$, or about one in ten billions;" whence it is concluded that there are overwhelming chances against any progressive variation in the right direction ever taking place. But first, I do not admit the assumption that only one variation out of twenty would be in the right direction; when it is remembered how great is the variety of the Heliconidæ, both in colour and marking. It seems more likely that one-fourth or one-third at least would help to approximate to some of them, and thus be useful. Taking, however, Mr. Bennett's own figures, there are three great oversights in this one short sentence. The first is, that each *Leptalis* produces, not one only, but perhaps twenty or fifty offspring; the second is, that the right variation has, by the hypothesis, a greater chance of surviving than the rest; and the third, that at each succeeding generation the influence of heredity becomes more and more powerful, causing the chance of the right variation being reproduced to become greater and greater.

Now with these three modifications the weight of the argument is entirely destroyed; for, allowing the *Leptalis* to produce only twenty offspring (a small number for a butterfly), the chances become even that one out of the twenty varies in the right direction. But nineteen out of the twenty, on the average, are soon killed off by the various causes that keep down the population of the species, and the chances are very much in favour of that one surviving which, by the hypothesis, has varied in the right direction. It is not pretended that this one would survive always, or even on the average, but in a large number of cases it would certainly do so; and taking Mr. Bennett's own estimate of a million individuals as the population of a rare species, we may fairly estimate that in a quarter, or say even in a tenth part of these, the surviving offspring would possess the favourable variation. But now a new factor enters into the problem, of which Mr. Bennett takes no account. Those that have already varied tend to leave offspring varying in the same direction as themselves; and as these will all have an advantage, the offspring of the one-tenth will increase at the expense of those of the nine-tenths; and this tendency being still more powerful in the third generation, with the additional advantage as the numbers increase of the chance of both parents being favourable varieties, we may fairly expect the favourable to have completely exterminated the unfavourable variations, and to have firmly established themselves as a well-marked race. The enormous possible rapidity of multiplication, enabling a pair of individuals to produce millions in a few generations; the survival of the fittest, giving to favourable variations—not their bare numerical chance, as Mr. Bennett supposes, but—a certainty in the long run of living at the expense of the rest; and the powerful influence of heredity, which actually increases the *tendency* to produce the favourable variations with each succeeding generation,—are three of the main foundation-stones of the theory of natural selection, yet all three are ignored in this attempted mathematical demonstration of its insufficiency.

There is one other point in the theory of the origin of "mimicry" that deserves notice. It is, that the modifications leading to it are much more easy to explain than

those leading to new genera and families, because the changes effected are wholly superficial and are almost entirely confined to colour. Now colour is both more variable than any other character, and is less intimately correlated with structure, so that great changes of colour may rapidly occur without in any other way affecting the individual, as we see in almost all our domestic animals. Experiments in breeding show that very large spontaneous variations of colour are frequent in insects; and thus the number of steps to produce a required amount of change may be much fewer than in cases of structural modification, in which every other part of the organism has to be co-ordinated to work harmoniously with the modified organ.

I may here take the opportunity of denying that I have argued, as Mr. Bennett says I have, that "an infinitesimal and inappreciable distinction may make the difference of a slightly longer span of life being allowed to the butterfly to lay its eggs in safety;" and I cannot imagine how he could have imputed to me anything so absurd. What I have maintained is, that for natural selection to act, either in producing "mimicry," or structural changes, no large or special variations are required, because the usual amount of *variability* which occurs in every part of every organism is sufficient. ("Contributions," pp. 287—291.) But so far from supposing this to be "infinitesimal" or "inappreciable," I show that it is so palpable and so readily appreciated by horticulturists and breeders as to have enabled them to produce all the wonderful variety in our domestic animals and cultivated plants. And every entomologist knows that similar variability exists in insects, and that the constantly occurring variations of colour are especially great.

Mr. Bennett next returns to the laws of variation, and, because Mr. Darwin says that we are profoundly ignorant of these (although he himself has done so much to elucidate them), maintains that we cannot really know anything of the origin of species. As well might it be said that, because we are ignorant of the laws by which metals are produced and trees developed, we cannot know anything of the origin of steamships and railways. Spontaneous "variations" are but the materials out of which "species" are formed, and we do not require to know how the former are produced in order to learn the origin of the latter. But though we may not know the laws which determine each variation in detail, the general causes which lead to variation are not difficult to perceive. We do not know all the laws and causes that have given their peculiar form to each mountain or each valley, but we know a good deal of the general causes which have produced them, and we can perceive that the reason no two are exactly alike is, the number and complexity of the causes and the endless variety of conditions under which these causes have acted. In the far more complex operations of the development and growth of organisms, affected as we know they are by almost infinitely numerous and ever varying external and internal causes, it would be a much greater mystery if there were no variations, and if absolutely identical forms were produced by constant diversity of conditions. Even the successive offspring of the same parents are developed under very different conditions. At each succeeding year, and at every different period of each year, the parents have changed in age, in size, in vigour, health, and constitution; they may be living in a different locality, have different food, and be subjected to very different physical and mental influences. Add to this the effect of cross unions of distinct individuals, each with its own characteristic peculiarities, which are in varying degrees transmitted to the offspring; and further, that these modified offspring are submitted to a somewhat different set of conditions from the parents, and intercross perhaps with a distinct set of individuals; and then add the effects of atavism in bringing up long lost ancestral characters, and it can hardly be said that the almost universal fact of "spontaneous variation" is quite unaccounted for. But, as I have already remarked, this variability could never by itself produce *species*, but must absolutely prevent their production without the eliminating, accumulating, and fixing powers of selection, multiplication, and heredity.

In Mr. Bennett's concluding passages he advances a theory of his own on the subject of "mimicry," to the effect that it is connected with intelligence or instinct, "and runs almost *pari passu* with the development of the nervous system." In support of this view he asserts that it is "strongly developed in birds." This is erroneous. In birds it is very rare, only two or three cases being known, and these not nearly so remarkable as hundreds that occur in insects; and in mammalia, with the exception of one doubtful case, it is absolutely unknown. This view, therefore, is directly opposed by the facts.

I have only one more point to notice, a charge of inconsistency against myself. Mr. Bennett quotes me to the effect that man's chief peculiarities of form and structure were developed before his intellect had raised him above the condition of the brutes, and also *imputes* to me the belief that certain peculiarities in his structure (the absence of hair on his body, for example) "must have been in some way connected with *his* reasoning powers." But this is Dr. Laycock's view, which I have expressly repudiated, and I have never used a word to show that I believed that man has modified his own structure in any important degree, by the conscious or unconscious exercise of his reasoning powers. I have, it is true, declared my belief that "some intelligence" has acted on him, but I have also, I think, made it quite clear that I did not believe it to be his own intelligence. The inconsistency, therefore, is of Mr. Bennett's making.

I think I have now noticed the chief points in this last assault on the theory of Natural Selection, which has failed, like all preceding ones. Its author also exhibits the usual inability to keep steadily before him the great fundamental principles of the theory he is discussing, so that his arguments continually break down owing to his taking a partial and wholly inadequate view of its mode of operation. In the case of "mimicry" he is not sufficiently careful in his statement of the facts, and this, combined with his imperfect grasp of the theory, entirely neutralises the elaborate numerical proofs which at first sight appear so overwhelming. ALFRED R. WALLACE

SCIENCE IN PARIS

IN the course of an article on the present condition of Paris, the *Engineer* gives the following account of the effects of the war. The use of the electric light is common to both sides in the present struggle, but the French have used it largely. The apparatus set up on Montmartre is arranged by M. Bazin, and is electro-magnetic. The central cylinder supports four series of double coils covered with copper wire enveloped in silk; the cylinder is rotated by a small steam-engine of 3-horse power, making 400 revolutions per minute. The lamp used is of the ordinary form, with the Foucault-Dubosc regulator. The reflector is parabolic in form, and the whole is surrounded by a shield to hide it from the enemy. This light, from its elevated position, commands the whole of Paris and the plains around. A spectator on Montmartre sees distinctly the details of the *façade* of a building which stands 2,600 metres off; at 2,900 metres a man may be seen standing at a window, at 3,000 metres a mass of cavalry or infantry is distinguishable, and at 4,000 metres the dome of the Invalides, with its bands of gold, is brilliant. A man cannot be seen on the dome at that distance, but on walking towards the building all soon becomes clear. On the ramparts, at 3,800 metres from Montmartre, the light is sufficient to read an ordinary newspaper.

Thus, though the practical effect of the lamp only extends about 300 metres from its position, the field is illuminated to the extent of 700 metres, for the benefit of all placed between the light and the object. Thus a sentinel on the ramparts can see about 3,000 metres from the *enceinte*, and by this means strict watch is kept upon the plains around the city at night, as far, in one direction, as 1,000 metres beyond St. Denis. M. Bazin is now occupied in applying his apparatus to the purposes of night telegraphs, by the adoption of the system of flashes—mentioned some time since in the *Engineer*—and with the aid of coloured lenses. A corvette—the *Coligny*—already possesses such a signal apparatus, and the signals are distinctly visible at more than eight miles' distance. The action of the lamp, and also the movements of the apparatus, are remarkably steady, and M. Bazin has received high testimonials from the authorities of the good working of his instrument.

M. Viollet-le-Duc, who is M. Alphand's second in command of the corps of civil engineers and architects aiding the military authorities, has made an interesting report to General Trochu respecting the works executed during the past month by the auxiliary engineers around Paris. It appears, according to this document, that the expense of these works has been only 105,000f., while under the military system they would have cost 230,000f. We have not seen the report, but we presume that M. Viollet-le-Duc, and the other architects and engineers, gave their services and advice gratis, and this would, of course, save the country a considerable sum.

The Government has voted the sum of 40,000 francs to enable M. Dupuy de Lôme to carry out his proposed plan of navigable balloons. The subject has been twice discussed in the Academy of Sciences, and although some members have advocated the use of a small steam-engine or other motive power, M. Dupuy de Lôme is no doubt wise in adhering to manual power, which presents all the force necessary with none of the inconveniences of machinery, and an adaptability to circumstances which no machinery can possibly possess. The men will form a crew to aid the landing of the balloon, or in extricating it from any difficulty, while any engine would be, when not in use, a dead weight and awkward encumbrance. Moreover an aërostat, with steam or other power, is now in construction by another inventor or adapter.

During the discussion M. Dupuy de Lôme showed by calculations that his balloon would have a constant ascensional force up to 870 metres altitude, but beyond that to 1,110 metres a little gas must be lost. At all events, between 250 metres and 870 metres the altitude could oscillate (by means of the extra pocket or swimming bladder) without loss of gas; and, of course, there would be the usual expedient of ballast to be depended on also. Dr. Monra advocated the use of heated air in place of gas. Fifty deg. Centigrade would be sufficient, and the heat might be retained by making the balloon double—that is to say, one balloon within another. The Aigle Mongolfière, said the Doctor, used to be inflated in twenty minutes, while it takes a whole day to fill a balloon (upwards of 1000 cubic metres) with gas. Mongolfier balloons sent up within twenty leagues of Paris would certainly fall in the city, and it is a pity they have not been tried before this. Should the siege not soon be raised, will not the English aëronauts or others try and send a few letters into Paris?

PROFESSOR HELMHOLTZ ON FARADAY

WE have been favoured by Prof. Tyndall with the following translation of a portion of the preface to the German Edition of "Faraday as a Discoverer," recently superintended by Professor Helmholtz:—

"The name of Faraday is one to be held in reverence by all natural philosophers. Many times in London, in connection with lectures which I delivered at the Royal Institution, I had myself the privilege of his obliging help and the pleasure of his amiable society. The perfect simplicity, modesty, and undimmed purity of his character gave to him a fascination which I have never experienced in any other man. I had therefore a duty of gratitude to fulfil towards him.

"But apart from this, and apart from that friendship for Faraday's younger associate and successor, the author of this book, which induced me to undertake the task, I believed that I should render a service to German readers by facilitating, as far as in me lay, an insight into the action and character of a mind so richly and peculiarly endowed, and so entirely the product of natural growth.

"It is, moreover, by no means for the philosopher only that such an insight possesses interest. His interest, certainly, is the most immediate, for it has hardly been the lot of any single man to make a series of discoveries so great and so pregnant with the weightiest consequences as those of Faraday. Most of them burst upon the world as surprises, the products, apparently, of an inconceivable instinct; and Faraday himself, even subsequently, was hardly able to describe in clear terms the intellectual combinations which led to them. These discoveries, moreover, were all of a kind calculated to influence in the profoundest manner our notions of the nature of Force. In the presence of Faraday's magneto-electric and diamagnetic discoveries more particularly, it was impossible for the old notions of forces acting at a distance to maintain themselves, without submitting to essential expansions and alterations. The clearer expression of these changes is at the present hour the object of physical science.

"In what way such extraordinary results were achieved is naturally a question of the first interest to the investigator who strives after similar though more modest ends. But Faraday's development appears to me to possess no small human interest in relation to many theoretic questions of psychology, and to the art of education. The external conditions under which he cultivated those striking capacities which excite our wonder were the simplest that can be imagined. He was completely self-taught; brought up in humble circumstances, having received no more than the commonest instruction, and having been only favoured by fortune in the circumstance, that when he was a poor apprentice to a bookbinder, he found, at the right time, a helper in Humphry Davy, who recognised his peculiar gifts, and procured for him the possibility, though in a subordinate position, of working in the direction towards which his genius impelled him.

"And throughout his whole life and labours, the advantages and disadvantages of such a mode of development reveal themselves in simpler and larger traits than in the case of most other similar celebrated names. The principal advantage rose undoubtedly from the fact that his intellect was not too soon subjected to theoretic fetters, but enjoyed its freedom in the presence of natural phenomena; and that instead of book-learning he permitted the fulness of Nature herself to operate upon his open mind. The disadvantages are, perhaps, of a subordinate kind; but they reveal themselves in quite as unmistakeable a manner when he strives to give expression to his ideas; and to supply, by all kinds of sensuous imagery, the want of mathematical culture. This is manifestly the way in which he alighted upon his Lines of Force, his Ray Vibrations, and other notions, which bewildered the investigators of his time, and the truer and clearer meaning of which has been in part made out by mathematical theory since Faraday himself ceased from his labours.

"And still, in this unlearned son of a smith, who held fast throughout his life the pious creed of his fathers, ran a vein of philosophy which gave him the right to be ranked among the foremost of those engaged in the general intellectual travail of our age. That

as Tyndall informs us, he retained the term "Natural Philosophy," usual in England to express physical science, and the name "Philosopher" for the cultivator of that science—lies essentially in the nature of his work. After the science of our age, in its laudable efforts to make human knowledge a true image of the actual universe, had shattered many an old metaphysical idol, it halted amid the transmitted forms of physical ideas regarding Matter, Force, Atoms, and Imponderables. These names were even converted into new metaphysical shibboleths by those who thought themselves most advanced in the way of enlightenment. It was these ideas that Faraday sought in his riper labours to purify from everything theoretical, which was not the true and immediate expression of the facts. More especially he opposed the action of forces at a distance, the assumption of two electric fluids and of two magnetic fluids, and, in like manner, all hypotheses which contradicted the law of the conservation of force, of which he had an early presage, though he singularly misapprehended its mathematical expression. And in these precise directions he exercised, in the first place, the most unmistakeable influence on the physicists of England. The mathematicians among them, especially, labour to render theories of phenomena the pure and true expression of the laws of fact, to the exclusion of all arbitrary theoretic devices. In this way Faraday's ideas, though in a modified form, often reveal themselves with their true significance assigned to them."

EARTHQUAKE OF OCTOBER 20, 1870

WE have been favoured with the following particulars of this earthquake byProf. Newton, of Cambridge, U.S.A. :—

On Thursday morning, Oct. 20, an earthquake vibration was felt throughout Canada, and the northern part of the United States from Maine to Iowa. It seems to have been more severe in Canada and in New England. In many places the shock was sufficient to throw down chimneys, crack the walls of buildings, and do other damage. It was remarkably severe for the region of country visited. At New Haven, as well as in many other places, there were two distinct series of vibrations. Prof. Twining has carefully collected information from several persons as to the time of the occurrence and duration of the vibrations. The beginning of the first shock was at $11^h\ 19^m\ 45^s$ A.M. New Haven mean time. It lasted ten seconds, and its individual vibrations were about two-thirds of a second in duration, or one and one-third second for a complete double vibration. After an interval of five seconds there was a second series like the first lasting eleven seconds. The motion was not a simple oscillation, but there was a rocking motion, indicating a vertical component in the movement of the earth. The vibrations were not severe enough to arrest universal attention, though multitudes felt a peculiar sensation without recognising the cause. The direction of the vibration was N.N.E. to S.S.E. At Cambridge, Mass., according to Prof. Winlock, the direction was about 10° north of east, as determined by the appearance of the sides of a vessel containing milk. Mr. Farmer, at Boston, gives $11^h\ 25^m\ 37^s$ for the time of the ending of the vibrations, Cambridge mean time. This would imply that the shock reached Boston a minute and three-fourths earlier than New Haven. At Cleveland, Ohio, several clocks were stopped by the earthquake, each indicating very nearly $10^h\ 45^m$ A.M. This is approximately the instant at which the shock reached New Haven. It is reported that the shock reached Quebec 30 seconds before it did Montreal, the telegraph operator of the former city being in the act of inquiring of the operator in the latter one respecting the earthquake, when it arrived at Montreal. These data seem to show that the general progress of the wave was from North to South. Slight vibrations were felt as far south as Richmond, Va., and as far west as Dubuque, Iowa.

NOVEMBER METEORS OBSERVED AT THE RADCLIFFE OBSERVATORY

THE following is a list of Meteors observed at this Observatory by Mr. Lucas, on the nights of November 12, 13, and 14, 1870.

On Nov. 12 a watch was kept from $7^h\ 15^m$ to $8^h\ 30^m$, and from 11^h to $13^h\ 30^m$.

At $8^h\ 20^m$ a meteor was seen of the 4th magnitude, from Capella, a little to northward.

At $12^h\ 29^m$, one of the 1st magnitude, white, flashed from α Cephei to α Cygni.

At $12^h\ 48^m$, one of the 1st magnitude, from α Ursæ Majoris to Polaris, below the stratus cloud which overspread the sky, and nearly hid all the stars, the two mentioned being just visible. Duration, $1^s{\cdot}5$.

At $12^h\ 57^m$, one of the 1st magnitude, white, seen for an instant about 7° or 8° east of Polaris, appearing to burst over the point of appearance; motion southwards.

Nov. 13.—At $8^h\ 5^m$, one of the 3rd magnitude, visible for 3^s, downwards towards the south.

At $8^h\ 20^m$, one of the 1st magnitude, visible for 2^s, with a long train, from ϵ Cygni to α Aquilæ.

At $9^h\ 38^m$, brighter than Jupiter, of a blue colour, visible for 4^s, from near Capella to o Ursæ Majoris.

The sky was overcast at $11^h\ 30^m$, and continued so nearly all the night, raining from 14^m to 16^m.

Nov. 14.—At $10^h\ 54^m$, one of the 1st magnitude, white, visible for $1^s{\cdot}5$. From β Aurigæ to a cloud near 25 Lynxis; direction, N.E.

At $11^h\ 1^m$, one of the 1st magnitude, visible for $1^s{\cdot}5$. From a cloud a little to the west of Rigal; downwards.

At $11^h\ 19^m$, one of the 1st magnitude, visible for 2^s; near ι and χ Andromedæ to α Cygni.

At $12^h\ 33^m$, one of the 4th magnitude. From α Cephei downwards; a short path.

Cloudy from 10^h to $11^h\ 30^m$; tolerably fine afterwards till 13, when clouds covered the sky again.

ROBERT MAIN

NOTES

WE are glad to announce that the Government has expressed its intention to aid in the most ample manner the proposed Eclipse Expeditions. In making this announcement we feel that what has recently appeared in the daily Press renders certain explanations desirable, which otherwise might have been omitted. In our last number we stated that deputations had been appointed both by the Joint Committee and the Council of the British Association to wait upon Mr. Gladstone. To this we must now add that the letter of the Secretary of the Joint Committee, which was to ask Mr. Gladstone to appoint a time to receive a deputation, was not sent to Mr. Gladstone, and did not ask that a time should be named; in fact it was a letter sent to the *Treasury*, apparently only for their information, and was nothing more than a copy of the resolution passed at the meeting of the Joint Committee. After a week had elapsed without any answer being received by the Joint Committee to the letter which it was supposed had been sent to Mr. Gladstone, asking him to name a time for a deputation; a member of the Joint Committee, Mr. Lockyer, called at the Treasury to inquire the reason of the delay in the answer, and of course he was informed that no letter had been received requiring any answer. Upon it being represented that a delay in the announcement of the Government intentions until a proper letter could be received from the Secretary of the Royal Society would be fatal to the Expedition, Mr. Lockyer was requested to state the actual requirements of the Scientific Bodies to Mr. Lowe, and upon his having done so, the Chancellor of the Exchequer, who, so far as we know, heard then of the expedition for the first time, at once expressed his opinion that such an expedition was one eminently worthy of Government aid, and that the Govern-

ment would do all that was requisite to further the objects sought. This decision of the Government was announced to the Royal Astronomical Society by the Astronomer Royal on Friday last, and since then, the arrangements for the Expedition have moved apace. There will be a ship furnished by the Government to carry observers to Spain. There will be funds to convey observers overland to Naples, and a ship to carry them on to Messina. The various European governments have been requested to aid the various parties, and, generally, the influence of the Government is being brought to bear in every way. This taken in connection with what has appeared in these columns before, and a letter which the Astronomer Royal has addressed to the *Daily News*, shows that now all the facts are out, the whole blame of the long delay must fall on the officers of the Joint Committee, who, apparently unable to perform the duties entrusted to them, still did not call the Committee together to receive instructions We should not write in so decided a tone on such a painful subject did we not feel that it is simply an act of justice to the Government to state, as plainly as our information enables us to do, exactly where the real blame rests.

IT will have been observed that Dr. Carpenter's letter in our number for Oct. 27, called in question, on the authority of Sir R. I. Murchison, the accuracy of our report of the conversation which followed the reading of Mr. Gwyn Jeffreys' paper in Section C of the British Association. We have communicated, in consequence, with Mr. Jeffreys, by whom we were favoured with the report in question, and who assures us that "to the best of his recollection and belief the report was quite accurate as conveying the purport of Sir Roderick Murchison's remark." While, therefore, we are unable to decide the relative accuracy of two different recollections of the same circumstance, we cannot but congratulate ourselves that the misunderstanding, if it were so, has drawn out such a full exposition of Dr. Carpenter's views of the bearings of the *Porcupine* discoveries on modern geological and physico-geographical theories.

IN addition to the notices of papers to be read at the meetings of the scientific societies in London, we shall be glad to insert in our "Diary" the titles of those to be read at the leading provincial societies, when of sufficient interest, and if forwarded to us in good time.

BRASENOSE College, Oxford, offers an open fellowship for Natural Science or Mathematics in February next. Candidates for Natural Science must communicate with the Principal by letter not later than the 20th December. As this is one of the few colleges which have hitherto held entirely aloof from Natural Science, this fellowship is clear evidence that the value of such studies is yearly becoming more appreciated at Oxford.

THERE will be offered for competition, at St. John's College, Cambridge, in 1871, an Exhibition of 50*l.* per annum for proficiency in Natural Science, the Exhibition to be tenable for three years in case the Exhibitioner have passed within two years the previous examination as required for candidates for honours: otherwise the Exhibition to cease at the end of two years. The candidates will be examined in (1) Chemistry, including practical work in the laboratory: (2) Physics, viz., electricity, heat, light; (3) Physiology. They will also have the opportunity of being examined in one or more of the following subjects—(4) Geology, (5) Anatomy, (6) Botany, provided that they give notice of the subjects in which they wish to be examined four weeks prior to the examination. No candidate will be examined in more than three of these six subjects, whereof one at least must be chosen from the former group. It is the wish of the masters and seniors that excellence in some single department should be specially regarded by the candidates. They may also, if they think fit, offer themselves for examination in any of the classical or mathematical subjects. The Exhibitions are not limited in respect to the age of candidates. The examination will take place on April 21st and 22nd, and the names of candidates must be sent in at least ten days beforehand.

THE series of science lectures addressed to working men at Manchester are a great success. Three weeks ago Prof. Huxley opened the series by a very interesting lecture on Coral Islands. On Wednesday se'nnight, Prof. Roscoe lectured on Spectrum Analysis, and this week Mr. Huggins lectured on the Spectrum Analysis of the Stars. These lectures, which are to be continued through the winter, have been literally crowded by most attentive audiences, and since full reports are published in the Manchester daily papers, there is every hope that much permanent benefit will be derived from them.

THE following are the lecture arrangements at the Royal Institution of Great Britain for 1870-71:—Christmas lectures (adapted to a juvenile auditory), Prof. Odling—Six lectures "On Burning and Unburning," on Dec. 27, 29, 31, 1870; Jan. 3, 5, 7, 1871. Before Easter, 1871, Prof. Michael Foster—Eleven lectures "On the Nutrition of Animals," on Tuesdays, Jan. 17 to March 28; Prof. Odling—Eleven lectures "On Davy's Discoveries in Chemistry," on Thursdays, Jan. 19 to March 30: Mr. W. H. Channing—Four lectures "On the Progress of Civilisation," on Saturdays, Jan. 21 to Feb. 11; Prof. Jowett—Three lectures "On Socrates," on Saturdays, Feb. 18, 25, and March 4; Mr. H. O'Neil—Four lectures "On the Spirit of the Age," on Saturdays, March 11 to April 1. The Friday evening meetings will commence on Jan. 20. The Friday evening discourses before Easter will probably be given by Profs. Tyndall and Odling, the Dean of Westminster, Mr. E. J. Reed, Mr. James W. Douglas, Dr. Carpenter, Capt. Noble, Prof. Clerk Maxwell, Mr. Norman Lockyer, and Mr. W. Mattieu Williams. After Easter:—Prof. J. J. Sylvester—Three lectures "On Emanuel Kant," on Tuesdays, April 18, 25, and May 2. Mr. Charles Brooke—Two lectures "On Force and Energy," on Tuesdays, May 9 and 16. Prof. Tyndall—Eight lectures, on Thursdays, April 20 to June 8. Mr. Norman Lockyer—Eight lectures "On Astronomy," on Saturdays, April 22 to June 10.

THE *British Medical Journal* reports that at a meeting of a committee held in Edinburgh on November 7, it was stated that the form of the national memorial to the late Sir James Y. Simpson had been agreed upon as follows: 1. A monument and statue in Edinburgh; 2. A marble bust in Westminster Abbey; 3. A hospital in Edinburgh for the diseases of women, constructed on those principles which Sir James so often and so clearly enforced; 4. Similar hospitals in London and Dublin, should sufficient funds be obtained. It was also stated that a sum of 1,950*l.* had already been subscribed.

AFTER the conclusion of the ordinary business of the meeting of the Royal Medical and Chirurgical Society on Tuesday the 8th inst., the meeting was made special for the purpose of confirming, or rejecting, the following resolution, proposed by Mr. Paget, seconded by Dr. Quain, and passed at a special meeting of the Society on October 25th:—"That the Council be requested to consider whether, while maintaining the charter and constitution of the Royal Medical and Chirurgical Society, it may be possible to obtain a more complete co-operation with the Pathological, Obstetrical, Clinical, and Epidemiological, or other societies for the promotion of Medical Science." After a lengthened debate, Dr. Paget's resolution was confirmed by a small majority.

THE *Chemical News* gives a short account of a convenient form of spectroscope for use in a laboratory, by Mr. John Browning. It is so constructed that it may be kept in close proximity to a chemical laboratory without injury. The prism is provided with

a cover, which should be put on with a little bees'-wax; or, better still, bees'-wax and tallow. This prism, with cover complete, can be removed, and replaced without deranging the adjustment of the instrument, to allow of a bottle prism being substituted for the purpose of taking the refractive index or dispersive power of any liquid. The stand of the instrument is of wood, and the whole is enclosed in a circular cover which fits tightly round the base of the instrument, and has no other joint or opening.

THE Leeds Naturalists' Field Club will hold the following conversational open meetings, at the Rooms, South Parade, every alternate Monday evening, at eight o'clock:—(1870), November 7, "A November Day at Boston Spa," Mr. J. W. Taylor; November 21, "Geology as a Study," Mr. L. Acomb; December 5, "Life History of the Painted Lady Butterfly (*Vanessa Cardui*)," Mr. W. Turner; December 19, "Animalculæ," Mr. W. Coates; (1871), January 2, "The History of a Mushroom," Mr. J. Abbot; January 16, "Wasps," Mr. W. D. Roebuck; January 30, "Our Trees and their Uses," Mr. Jas. Brodie; February 13, "Protozoa," Mr. T. Hick, B.A.

THE Annual Exhibition of the Photographic Society of London was opened at the Architects' Gallery, No. 9, Conduit Street, by a private reception of their friends by the members of the Society, and the exhibition will remain open to the public until the last day of the current month of November. The exhibition is quite as large as was the eminently successful exhibition of last year, and certainly leaves the impression that there is manifest and unmistakeable evidence of progress in the art during the year. The work of many of the leading and best known exhibitors is in advance of their exhibited specimens of the last year, and there are some individual photographs now to be seen upon the walls of the gallery which it can hardly be too much to say are of higher excellence, both as pictures and as photographs, than any that have before been produced. We may mention in particular ten large portraits by Col. Stuart Wortley, taken direct from life, and marvellous specimens of tasteful and exquisite portraiture; two large portraits, also from life, by Mr. Warwick Brookes, of scarcely inferior power and excellence; Vandyke and Brown's solar camera enlargements of the poet Longfellow; and Mr. Blanchard's large Rembrandtesque life portraits. Of large landscape work, Mr. Care, of Worcester, deserves special notice; and Mr. Robinson's sea pieces, "Turn of the Tide" and "Outward Bound," are admirable bits of true nature. Mr. Robert Faulkner has some very pretty applications of the instantaneous branch of the art in catching the fitful aspects and characteristics of children; and Mr. Vernon Heath has two exquisite bits of true scenery and foliage; nor ought we to pass by "Hop-picking," by Mr. Stephen Thompson, and Captain Lyons' Indian pictures; and we may add to this list Mr. Manners Gordon's small pictures from dry plates.

HERR A. PETERMANN has published a brief paper, in which he recapitulates the main results of the various North Pole Expeditions of the present year. Herr von Heuglin and Count Zeil, of the German Expedition, remained from July 15 till Sept. 15 in and near East Spitzbergen, which they explored, mostly in boats, from 77° to 79° N. lat. They claim to have discovered extensive land to the east of Spitzbergen. This land, Herr Petermann maintains, it is a mistake to identify with the land discovered by Gillis in 1707, which lay 80° N. lat. The land which was seen from the White Mountain of Spitzbergen by the Swedish Expedition in 1864, 80 nautical miles to the east, was put down on the map as a neck of land lying 79° N. lat. "Herr von Heuglin and Count Zeil," says Herr Petermann, "have now discovered, 36 nautical miles to the east of Spitzbergen, a continent, extending from 79° to 78° N. lat.—therefore, from north to south, at least 60 (German) miles—which contains numerous sharply pointed peaks, and which, in case it is really connected with Gillis Land, might at least equal Spitzbergen in size." This is claimed as the most important polar discovery that has been made for some years. Herr Heuglin has brought home with him from East Spitzbergen fourteen chests of geological, zoological, and botanical specimens. The news of the war reached the explorers in September. Count Zeil, a lieutenant in the Second Royal Würtemberg Jäger Battalion, at once hastened home to Stuttgart, and, having had an audience of the King on October 20, proceeded forthwith to his regiment in France. Herr Petermann announces that the Russian Expedition, which has been accompanied by the famous academician, Von Middendorff, has prosecuted interesting scientific researches between Novaia Zemlia and Iceland. Among other things, it is said, he has identified the Gulf-Stream as far as Novaia Zemlia at the very considerable temperature of + 10° Réaumur. With reference to an article by Herr Petermann on the subject of the Gulf-Stream, Herr von Middendorff writes to him:—"I am extremely glad that your theory respecting the extension of the Gulf-Stream is not only confirmed, but has even been greatly surpassed; you were bold, but Mother Nature is still bolder."

AT a time when so much is being said about the value of fungi in general as profitable and wholesome articles of food, and also when France is being so largely overrun by foreign troops, the following notes on the Truffle cultivation in the Department of the Dordogne, written a short time before the outbreak of the present war, may be of some interest. It shows the money value of these delicacies, and how profitable a business is their cultivation. The method adopted for propagating them is to sow acorns, and the best truffles are found under the resulting oak-trees; but the evergreen oak, and juniper trees are also grown for the same purpose. An instance is cited of a person who inherited a piece of land worth 8*l.*, and who thus sowed it with acorns; the truffles thereby obtained realise now as much as 160*l.* a year. There are many varieties easily distinguishable to those accustomed to the trade. The truffle is dearer in Périgord than in Paris, where it is mixed with an inferior quality, and therefore can be sold at a lower price. It comes to perfection about the middle of November, but large quantities are collected and sent to market in September and October. These are called "fleurs," and are without smell. It is pretended that they have not come to maturity, and that a large portion of the produce is thus spoilt. The total revenue derived from the truffle commerce amounts to 20,000*l.* a year in the Arrondissement of Tarlat, and to about the same amount in Périgueux.

IT is interesting to note the progress the Japanese are making in the art of printing, &c. Hitherto they have only been acquainted with the Chinese mode of printing, from engraved wooden blocks. Lately, however, they have engaged the services of an English gentleman, to set up for them an establishment for type-founding, electrotyping, and printing on the Western method, and to give them such instructions in these arts as will enable them afterwards to carry on the business. Type-founding and electrotyping have now for the first time been introduced into Japan.

AT the last ordinary meeting of the Hackney Scientific Association, held on Nov. 8th, Mr. Henry T. Vivian announced that he had discovered the variability of ϵ Herculis, a small star to the N.E. of π. From observations extending back to the autumn of last year, he had deduced a period of about 21 days, with probably a second longer period. The amount of variability in the star's lustre is from a large 5th mag. to a small 6th. The star is numbered 60 in Map 10 of Mr. Proctor's New Star Atlas, although in Map 8 it is numbered 69; evidently an error on the part of the engraver.

THE MICROSCOPE

THE VALUE OF NITRATE OF SILVER AS A REAGENT FOR DEMONSTRATING MINUTE STRUCTURE.—Since Von Recklinghausen drew attention to the beautiful results to be obtained by the use of a weak solution of nitrate of silver, in the study of the finer distribution of vessels, and especially of the lymphatics, much discussion has taken place as to the trustworthiness of results obtained by this method, and there are some histologists of merit who maintain that the whole thing is false and delusive in its effect. The method consists in immersing perfectly fresh (warm from the animal) tissues in a ½ per cent. aqueous solution of nitrate of silver, leaving them there for from five to ten minutes, and then, after thorough washing, mounting in glycerine, and exposing to sunlight for half an hour, or two or three hours as the case may be. If desired, the tissue may be plunged while fresh into serum (preserved with just a trace of iodine), and after two hours' maceration therein may be smoothed with a small paint-brush, to remove superficial epithelium (as, for instance, in the centrum tendineum of the guinea-pig), and then placed in the silver solution. It is, however, preferred by Dr. Klein, of Vienna, to brush away the superficial epithelium of the thoracic surface, if the lymphatics of the centrum tendineum are to be examined, before removing the diaphragm from the body, whilst perfectly fresh and warm, warm water being used for this purpose, and a camel's-hair paint-brush: immediately after this "pencilling" the centrum tendineum is cut out and placed in the nitrate of silver solution. In the same way, in researches on the cornea the conjunctival epithelium may be removed in the living animal and the silver solution then applied. Dr. Klein, Professor Stricker's assistant, has found that the best results were to be obtained with the cornea, by removing the conjunctival epithelium, and rubbing the corneal surface well with lunar caustic, and removing the cornea in half an hour's time. This method proves admirable with frogs, and is probably applicable to other corneæ. The results of the nitrate-of-silver-staining as seen in the centrum tendineum, are that both thoracic and peritoneal epithelium (where not pencilled away) are stained, the outlines of the cells being brought out very distinctly. In addition to this the epithelium lining the vessels which run in the centrum is also brought out, and especially the curiously crenated epithelium of the fine lymphatics is demonstrated. Moreover, Recklinghausen pointed out that small branched cavities become delineated connected by their branches to one another, and opening into the lymphatics with the crenate epithelium. These lacunar channels, "the juice canals," are brought into view by the action of the silver in staining the substance of their walls but leaving the cavity free. A *successful* silver preparation of the centrum tendineum of a guinea-pig, for instance, shows these structures clearly, but it is not always possible to succeed. Besides the remarkable epithelium of the lymphatics and the juice-canals, a hexagonal pavement of enormous cells is demonstrated on the nerve sheaths, and an exceedingly fine cell pavement on the adventitia of the small veins in some beautiful silver preparations of the "centrum" made by Dr. Burdon Sanderson. The results in the cornea are very remarkable, for the whole substance of that tissue in the frog is demonstrated to be filled with oblong radiating cavities, communicating with one another by their branches, and of so angular and regular a character as to give the appearance of a mosaic-work or some such elaborate device. These cavities correspond to the juice-canals in the centrum tendineum, it is by their means that the cornea is nourished, it is they which contain the large star-like cells or protoplasm-masses brought out by gold chloride, and it is along their branches that the divided portions of these cells wander so remarkably in the process of inflammation. And now we are told by some persons that these are artificial productions, that the juice-canals and the radiate lacunæ, and the crenate lymphatic epithelium too, are due to fortuitous coagulation caused by the silver, that they do not represent structures existing in life. There are two sets of objectionists, some who deny the whole thing, others who go so far as to deny the juice-canals and stellate lacunæ, but admit the epithelium. An exponent of the first order is a certain Dr. Robinski, who in a paper published in the excellent *Archives de Physiologie* of Brown-Séquard, pretends that the crenate epithelium which so many have seen in the finer lymph vessels, as well as the juice-canals, are due to imperfect retention of portions of the superficial epithelium, and gives a drawing intended to show this. The author of the paper has simply failed utterly and entirely in using the method, and the outrageous view which he advances does not require refutation. Everyone knows who has made silver preparations successfully that the thoracic, the lymph-canal, and the peritoneal epithelium may all be seen lying one over the other in parts of a preparation, and that the form of the second prevents its being mistaken for the first or third. Others maintain that the crenate lymph-epithelium is simply a form due to the silver, and not to cells at all. This is impossible, because the nucleus and cell-contents are sometimes clearly demonstrated, and moreover the same form of epithelium may be readily obtained from large lymph-sacs, of some of which it is characteristic. The juice-canals, if they appear doubtful at all when taken by themselves (which indeed they do not if a *good* preparation is examined), when seen in the light of the stellate lacunæ of the frog's cornea, are clearly understood, and must be fully admitted as normal living structures. There are, however, persons who object to these, among whom is so eminent a histologist as Professor Schweigger-Seidel, and also that accomplished micrographer, Dr. Ranvier, of Paris. They actually hold that these forms are *post-mortem* products, the effect of the action of nitrate of silver on the albumen and gelatine of the tissue. It is almost as strong a position to take up as that of certain schoolmen who maintained that we know nothing of the anatomy of living animals, since the air admitted on cutting them open, and the scissors, produced all the various viscera seen in a dissection. There are some strong facts which we have witnessed which render such a view untenable. In the first place, the stellate lacunæ of the frog's cornea are shown in a preparation in our possession, with the intermediate substance stained by *gold* and the lacunæ and cells unstained, in fact, exactly the same appearance as is produced with silver. Hence the stellate form of the lacunæ cannot be due to a specific action of the silver. Again, by silvering the living cornea whilst it is still part of the frog, the structure is brought out, and may be obtained of all degrees of intensity, the *form*, however, always remaining the same, and not varying with the amount of silver allowed to act on it, as might be expected had we to do with a precipitation-form, which should appear less completely developed when less silver is used. Moreover, the living cells were thus demonstrated lying in these stellate lacunæ, creeping up the radiating branches, drawing back again, and creeping along others, thus obtaining that curious direction to their movements which one always observes in studying them in the fresh, unstained, inflamed cornea. The necessity of admitting that the normal structure of the frog's cornea is brought out by the silver method, covers also the question as to the similar structures in the centrum tendineum. The "Saft-canälchen" must equally be admitted as having a living existence, and are not due to the imaginary precipitating action of nitrate of silver on some albuminoid, as Schweigger-Seidel would have us believe, but a parallel to which he cannot find. The chief difficulty with regard to the crenate epithelium of the lymph vessels in the centrum tendineum is to explain why we only see one layer of these cells; they should appear all round the wall of the vessel so as to produce a double layer as we look through it, but we only see one layer, the other not being stained. The action of light, the absorption of the silver, and other causes, may be hazarded as explanations of this; but it would be satisfactory to get a double layer clearly shown.

So far, then, from agreeing with Schweigger-Seidel and other objectors, that the silver method of Von Recklinghausen "gives no true indication of the structure of the cornea," nor of similar structures, and "should be abandoned,"* we believe that when carefully applied it furnishes most admirable results, difficult to attain in any other way at present in our hands, and is worthy of all confidence, and should be used as a means of investigation in other structures besides those to which it has been already applied.

E. R. L.

REPORT OF THE KEW COMMITTEE OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE FOR 1869-70

THE Committee of the Kew Observatory submit to the Council of the British Association the following statement of their proceedings during the past year:—

At the meeting of the General Committee at Exeter it was re-

* Berichte der Math. Phys. Classe der Kön. Sachs. Gesellsch. 1869. p. 352.

solved that the existing relations between the Kew Committee and the British Association be referred to the Council to report thereon. In consequence of this resolution, the Kew Committee, on the 23rd November, 1869, prepared for the information of the Council a statement on the past and present condition of the Observatory, which was presented to the Council on the 11th December. In this statement it was shown that while the establishment at Kew Observatory received its main support from the British Association, and was under the control of that body, yet much of the apparatus in use at Kew was furnished from other sources. Thus, the Royal Society had from the Government-Grant Fund supplied the establishment with the apparatus for testing barometers, with that for testing sextants, with the dividing-machine for constructing standard thermometers, and also with the set of self-recording magnetographs at present in use, while from the Donation Fund they had furnished the photoheliograph and the Whitworth lathe and planing-machine. The Royal Society had likewise defrayed from the Donation Fund the expense of introducing gas into the Observatory, and of building a house for the verification of magnetic instruments, besides which they had borne from the Government-Grant Fund since 1863 the whole expense of working the Photoheliograph (including the purchase of a chronometer) and of reducing its results. The instruments used at Kew for determining the absolute magnetic elements are the property of Her Majesty's Government, and have been lent to the Kew Observatory by the Magnetic Office at Woolwich, under the direction of Sir E. Sabine, and many of those magnetic instruments with which Kew has been the means of furnishing scientific travellers have been derived from the same source. Of late Kew has become the central observatory of the Meteorological Committee, and a commodious workshop has been erected near the Observatory by that committee, since otherwise the main building would have been too small for the access of work consequent upon the arrangement entered into. The statement prepared by the Kew Committee contained likewise a summary of the scientific work done at the Observatory, as well as some interesting historical remarks connected with the origin of the establishment, drawn up by Sir C. Wheatstone, and in this shape it was submitted to the Council of the British Association. The Council then recommended "That the present existing relations between the Kew Observatory and the British Association be continued unaltered until the completion, in 1872, of the magnetical and solar decennial period, and that after that date all connection between them should cease." In consequence of this recommendation, the Kew Committee were led to contemplate the dissolution of the Kew establishment in 1872, and they became anxious to make such arrangements as might enable them to complete their scientific labours in a creditable manner before the time of the anticipated dissolution. The magnetic work in particular caused them anxiety; for the annual income of the establishment is insufficient to permit of that work being fully completed by the time of the annual meeting of the Association in 1872. Under these circumstances the chairman offered to supplement the deficiency. It will be seen by this report that the magnetical tabulations and reductions are now proceeding very fast. The recommendation of the Council was also a matter of anxiety to the superintendent, Mr. Stewart; and as the Professorship of Natural Philosophy at Owens College, Manchester, became vacant about this time, he applied for the appointment and was successful in obtaining it. This will render it necessary for Mr. Stewart to reside in Manchester, but the staff at the Observatory are such that Mr. Stewart will undertake by their aid to assist the committee in the superintendence of the work of the Observatory until 1872.

(A) Work done by Kew Observatory under the direction of the British Association.

1. *Magnetic Work.*—In the present state of magnetical science it would appear to be desirable to preserve as completely as possible the details of observations, so that future theorists may have a large and valuable source of information by which to test their speculations. The Committee are therefore desirous that by the autumn of 1872 a manuscript record should be completed, containing all the hourly tabulated values from the Kew Magnetographs arranged in monthly tables. This record should be carefully preserved, along with the original photographic traces, in the archives of the Association. Pursuing the method indicated by Sir E. Sabine, and adopting the separating values finally determined by him, the Committee further propose to obtain monthly results indicating the following points for each of the three magnetic elements, distributed according to the hour of the day :—

1. Aggregate of disturbance tending to increase the numerical values.
2. Aggregate of disturbance tending to decrease the same.
3. Solar-diurnal range of the undisturbed observations.

They suggest that the monthly results embodying these facts should be published in detail. Finally, they propose to continue the discussion of the lunar-diurnal variations commenced by Sir E. Sabine, and carried on by him up to the end of the year 1864. In order to work this scheme with sufficient rapidity to complete it before the autumn of 1872, additional assistance has been procured, the expense of which has been defrayed by the chairman. Mr. Whipple, magnetical assistant, has displayed much zeal and ability in organising the work and in superintending its immediate execution. Already the hourly numerical values of the three magnetic elements have been obtained and tabulated in monthly forms from the commencement of the series in 1858 to the present date; and considerable progress has also been made in the next step of the reduction. A unifilar, formerly employed by Captain Haig, and of which the constants have been determined at the Observatory, has been lent to Lieut. Elagin, of the Russian navy, for use in the Japanese seas and elsewhere. A dip-circle, by Dover, has been verified and sent to Prof. Jelinek, of Vienna, and another, by the same maker, has been verified for Dr. A. B. Meyer, for use in the East Indies. This gentleman has likewise received magnetic instruction at the Observatory. A dip-circle by Adie, furnished with a deflecting cylinder apparatus, has been verified and despatched to Prof. Bolzani, of the University of Kasan. Three dipping-needles have likewise been constructed for Dr. Bergsma, of Batavia, and one for Mr. Chambers, of the Colaba Observatory, Bombay. A deflection-bar has been procured and verified for the Russian Central Observatory. A declinometer has been sent to the Lisbon Observatory, and a Fox's circle has been lent to Dr. Neumayer, after having been repaired by Adie. The instrument devised by Mr. Broun for the purpose of estimating the magnetic dip by means of soft iron, and constructed at the expense of the British Association in pursuance of a resolution of that body passed at the Oxford meeting, has been forwarded to that gentleman at his request. The usual monthly absolute determinations of the magnetic elements continue to be made by Mr. Whipple, magnetic assistant. A paper embodying the results of the absolute observations of dip and horizontal force, made at Kew from April 1863 to April 1869, has been communicated by the superintendent to the Royal Society, and published in the "Proceedings" of that body. The results obtained evidence the accuracy with which the monthly observations have been made by Mr. Whipple. The self-recording magnetographs are in constant operation as heretofore, also under his charge; and the photographic department connected with these instruments remains under the charge of Mr. Page.

2. *Meteorological work.*—The meteorological work of the Observatory continues in the charge of Mr. Baker. Since the Exeter meeting, 150 barometers have been verified, and thirty have been rejected; 1,160 thermometers and 103 hydrometers have likewise been verified. Nineteen standard thermometers have been constructed for Prof. Tait, and two for the Meteorological Office. The self-recording meteorological instruments now in work at Kew will be again mentioned in the second division of this report. These are in the charge of Mr. Baker, the photography being superintended by Mr. Page.

3. *Photoheliograph.*—The Kew heliograph, in charge of Mr. Warren De la Rue, continues to be worked in a satisfactory manner. During the past year 351 pictures have been taken on 237 days. It was considered desirable that six prints should be obtained from each of the negatives of the sun pictures taken at the Observatory during the whole time that the photoheliograph should remain at work, which will probably be from February 1862 to February 1872. In order to accomplish this, an outlay of 120*l.* spread over two years was found to be necessary, and this sum has been voted from the Donation Fund of the Royal Society. A large number of these prints has already been obtained, and it is proposed to present complete sets to the following institutions :—

The Royal Astronomical Society,
The Imperial Academy of Paris,
The Imperial Academy of St. Petersburg,
The Royal Society of Berlin,
The Smithsonian Institution, United States,

leaving one set for the Royal Society. A paper embodying the positions and areas of the sun-groups observed at Kew during the years 1864, 1865, and 1866, as well as fortnightly values of the spotted solar area from 1832 to 1868, has been communicated to the Royal Society by Messrs. Warren De La Rue, Stewart, and Loewy. This paper is in course of publication in the "Philosophical Transactions," and will shortly be distributed. A table exhibiting the number of sun-spots recorded at Kew during the year 1869, after the manner of Hofrath Schwabe, has been communicated to the Astronomical Society, and published in their monthly notices. M. Otto Struve, director of the Imperial Observatory at Pulkowa, visited England in the month of August last. He brought with him, for the Kew Observatory, some sun-pictures made at Wilna with the photoheliograph, which, it will be recollected was made some years ago, under the direction of Mr. De La Rue, by Mr. Dallmeyer. This instrument combines several important improvements on the original Kew model, the value of which is forcibly brought out in the superior definition of the Wilna sun-pictures. As, however, the series of the ten-yearly record at Kew was commenced with the instrument as originally constructed, it was not deemed desirable to alter it in any way until the series had been completed and reduced, and the corrections for optical distortion ascertained and applied. In the event of the sun-work being continued after 1872, it will be desirable to do so with a new and improved heliograph. M. O. Struve proposed to exchange the complete series of pictures obtained at Wilna for that made at Kew. He also stated that it is contemplated to erect a second heliograph at the Central Observatory at Pulkowa.

4. *Miscellaneous Work.*—A few experiments have been made on the rotation of a disc *in vacuo*. By an arrangement devised by Mr. Beckley, a very perfect carbonic-acid vacuum has been obtained, the residual pressure being 0·02 inch as indicated by a mercurial gauge with a contracted tube, but it was believed that the vacuum was even more perfect. A disc of paper and one of ebonite gave very sensible heat effects in such a vacuum, and it was hoped that the experiments might have been satisfactorily completed; but while they were in progress the pressure of the outer atmosphere shattered the receiver into a number of pieces, fortunately without any injury to the experimenters. Another receiver has now been made, and it is purposed in future to use it with a cover. A transit instrument has been lent to Mr. G. J. Symons, and one sextant has been verified.

(B) Work done at Kew as the Central Observatory of the Meteorological Committee.

It is stated in the report for 1867 that the Meteorological Committee had appointed Mr. Balfour Stewart as their secretary, on the understanding that he should, with the concurrence of the Kew Committee, retain his office of Superintendent of the Kew Observatory. On the 8th October, 1869, Mr. Stewart resigned his appointment as Secretary to the Meteorological Committee and Director of their Central Observatory—a step which took effect on the 31st of March, 1870, and which was followed by a modification of the relation between the two committees. The Meteorological Committee, at their meeting on 12th November, 1869, resolved that they were prepared to make the following proposals to the Council of the British Association:—

I. That Kew be continued as one of the ordinary self-recording observatories, in which case the committee would be prepared to allot to it annually 250*l.*; or,

II. In addition to the foregoing work, that Kew be maintained as the central observatory for examination of records and tabulations from all the other observatories, in which case the committee will be prepared to allot a further annual sum of 400*l.* The Kew Committee, having been furnished with this resolution of the Meteorological Committee, resolved that it be recommended to the Council of the British Association that Kew be continued for the next two years as one of the ordinary self-recording observatories of the Meteorological Committee, that body allowing it annually 250*l.*; and that, in addition, it be maintained as the central observatory for the examination of the records and tabulations from all the other observatories, for the further sum of 400*l.* per annum. This arrangement was approved by the council; and it was thereupon resolved by the Kew Committee, that out of the 650*l.* received from the Meteorological Committee, 200*l.* be given to Mr. Stewart for superintending the meteorological work of the Observatory, this resolution to take effect after 31st March, 1870.

1. *Work done at Kew as one of the Observatories of the Meteorological Committee.*—The barograph, thermograph, and anemograph furnished by the Meteorological Committee are kept in constant operation. Mr. Baker is in charge of these instruments. From the first two instruments traces in duplicate are obtained, one set being sent to the Meteorological Office and one retained at Kew; as regards the anemograph, the original records are sent, while a copy by hand of these on tracing paper is retained. The tabulations from the curves of the Kew instrument are made by Messrs. Baker, Page, and Foster.

2. *Verification of Records.*—The system of checks devised by the Kew Committee for testing the accuracy of the observations made at the different observatories continues to be followed, the only alteration being that the Kew staff, at the suggestion of the Meteorological Office, have undertaken to rule on the barograms and thermograms a set of zero lines, which are of great use in pantagraphic operations. Mr. Rigby continues to perform the main part of this work; Mr. Baker, Meteorological Assistant, having the general superintendence of the department.

3. *Occasional Assistance.*—The Meteorological Committee have availed themselves of the permission to have the occasional services of Mr. Beckley, Mechanical Assistant at Kew; and he has lately been visiting the various observatories of the Meteorological Committee. The self-recording rain-gauge mentioned in last report as having been devised by Mr. Beckley has been adopted by the Meteorological Committee, and instruments of this kind are at present being constructed for their various observatories. The staff at Kew continue to make occasional absolute hygrometrical observations by means of Regnault's instrument, with the view of testing the accuracy of the method of deducing the dew-point from the observations with the dry and wet bulb thermometers. Two erections have been made in the grounds adjoining the Observatory, and on one of these a large Robinson's anemometer is placed, while a small instrument of the same kind is placed on the other. By this means the indications of the large and those of the small-sized instrument may be compared with each other. The cost of this experiment has been defrayed by the Meteorological Committee.

J. P. Gassiot, Chairman

Kew Observatory, Sept. 9, 1870

SCIENTIFIC SERIALS

The November number of the *Geological Magazine* (No. 77) opens with an article by the editor, Mr. H. Woodward, on Fossil Crustacea from various formations. The species described and figured by the author are *Scyllaridia bellii* from the London clay of Sheppey, and *Palæga carteri* from the lower chalk. One of the most important papers in the number is on the coal-bearing rocks of Southern Chile by MM. Lebour and Mundle. The coal appears to be only a kind of lignite; it is found in beds of tertiary age. Mr. Lucy contributes a paper on the Post-pliocene Drift of Charnwood Forest; Mr. H. F. Hall, a note on the Glacial and Post-glacial deposits in the neighbourhood of Llandudno; Mr. L. C. Miall, a paper on the formation of swallow-holes in mountain limestone; and Mr. E. Wilson, a notice of some altered clay-beds and sections in Tideswell Dale, Derbyshire. The remainder of the number is occupied as usual by reviews, notices, and miscellaneous matter.

The October number of the *American Naturalist* (vol. iv., No. 8) is chiefly devoted to Geological and Archæological subjects. It opens with an address on recent advances in Geology, delivered by Mr. J. H. Foster to the American Association for the Advancement of Science, and contains also a paper on the Primitive Vegetation of the Earth by Dr. J. W. Dawson, and a note on Indian Stone Implements by Mr. J. H. Gregory. The only other paper is a short note on bud-variations in the colour of the flowers in *Trillium* and *Wisteria*. This number also contains an interesting illustrated report of the nineteenth meeting of the American Association, including a valuable paper by Dr. A. S. Packard, jun., on the embryology of *Limulus polyphemus;* and another, by Prof. E. D. Cope, on the Structural Characteristics of the Cranium in the Lower Vertebrata.

Proceedings and Communications of the Essex Institute.—The first part of volume vi. was published in the spring of this year. Of four papers given in it, three relate to entomological subjects; these are descriptions (with figures) of numerous species of ants from Mexico, by Mr. Edward Norton; an excellent monograph of the Phalangea (Harvest Spiders) of the United States, by Dr. Horatio C. Wood, also illustrated; and an important notice of insects inhabiting salt water, by Dr. A.

S. Packard. The fourth paper is a continuation of Mr. A. E. Verrill's Synopsis of the Polyps and Corals collected by the American North Pacific Exploring Expedition in the years 1853 to 1856; this includes descriptions of a great many new species, and is illustrated with two plates. This part also contains the "Proceedings" of the Essex Institute for the year 1868.

The third part of the *Zeitschrift* of the German Geological Society, including its proceedings for the months of May, June, and July, opens with a long and important memoir, by M. F. J. Wurtenberger, on the Tertiary Formation of the Klettgau, a district situated on the northern border of the Swiss Molasse. The tertiary deposits, which are referred by the author to the Miocene and Oligocene periods, are of both freshwater and marine origin, and appear to be very complicated; fossils are scarce in them, except in certain localities, which have furnished the remains of plants and animals in considerable abundance. Prof. Ferdinand Roemer describes and figures a new fossil *Python* from the Island of Eubœa, under the name of *P. euboicus*. The specimen is contained in a slab of tertiary calcareous marl slate, and the portion preserved indicates that the snake would have been about nine-and-a-half feet long when living. M. H. Laspeyres communicates a monographic revision of the genus *Leaia* (T. R. Jones) belonging to the Phyllopodous Crustacea. He discusses the systematic position of the genus, which he places among the Linmadiadæ, and of which he describes and figures five species, one of them (*L. wettinensis*) as new, and two others as having been previously described as varieties of *L. Leidyi*. All the other papers relate to mineralogical subjects; they include a continuation of Prof. G. vom Rath's valuable geognostico-mineralogical Italian fragments; an analytical notice of the Palatinite of Norheim, by Prof A. Kenngott; and a notice of the occurrence of zircon in the hypersthenite of the Radauthal near Harzburg, by Prof. Gustav Rose.

THE *Journal of Botany* for November contains a continuation of Dr. Seemann's Revision of the Bignoniaceæ and several articles of special interest to British botanists. There is also a reprint of an interesting paper by Dr. Parry, of Washington, read at the recent meeting of the British Association, on the North American Desert Flora, between 32° and 42° north latitude. He calls particular attention to the contrast between the annual and perennial vegetation of desert tracts, the former being very evanescent and rapid in its growth; the latter either storing up a large amount of surplus nourishment in their thick tuberous or tap roots, or, in the case of trees and shrubs, possessing exposed stems and foliage of the most scant and starved character; spine-clad branches and green-backed stems are, in many places, made to supply the office of leaves, or, where these latter are present, they are often chiefly coated with resinous varnish, or clothed with tomentose hairs or scales, serving to check evaporation.

SOCIETIES AND ACADEMIES

LONDON

Zoological Society, Nov. 1.—Prof. Newton, F.R.S., V.P., in the chair. The Secretary read a Report on the additions that had been made to the Society's Menagerie during the months of June, July, August, and September. Amongst the more valuable acquisitions, particular attention was called to a collection of animals from Chili, purchased in the month of July last. Of twenty-two species obtained in this collection, no less than twelve were new to the Society's series, and some of these, such as Burmeister's Cariama (*Chunga burmeisteri*) and the small Coscoroba Swan (*Cygnus coscoroba*) were of special interest. Another valuable addition was a male of the Ethiopian Ant-bear (*Orycteropus Æthiopicus*) from Upper Nubia, purchased July 29. This animal had been placed in company with the Cape Ant-bear (*Orycteropus capensis*) acquired in June 1869, and afforded an opportunity for the comparison of the two species together.—A communication was read from Prof. W. Peters, containing an elaborate memoir on the structure of *Pectinator spekei*, a peculiar Rodent of Eastern Africa. The specimens on which Dr. Peter's memoir was based, had been obtained by Mr. William Jesse, during his travels as Zoologist in company with the Abyssinian Expedition.—A seventh letter on the Ornithology of Buenos Ayres, addressed to the Society by Mr. W. H. Hudson, was read.—Prof. Newton exhibited a chick of *Anarhynchus frontalis*, a rare wader from New Zealand, remarkable for possessing an asymmetrical bill.—Mr. C. Darwin, F.R.S., communicated a note on the habits of the Pampas Woodpecker (*Chrysoptilus campestris*), in reply to some observations on this subject made by Mr. W. H. Hudson, in one of his previous letters.—Six communications were read from Dr. J. E. Gray, F.R.S., on various points connected with the Natural History of the Testudinata. The first of these contained notes on three Tortoises living in the Society's Gardens, one of which was believed to be new to science, and was proposed to be called *Testudo chilensis*. The second contained descriptions of two new species of Indian Tortoises in the collection of Mr. T. C. Jerdon. The third related to the family *Dermatemydæ*, and embraced the description of a species of this group living in the Society's Gardens. The fourth contained notes on a West African River-Tortoise (*Cyclanosteus senegalensis*), also living in the Society's Gardens. The fifth contained notes on *Bartlettia*, a proposed new genus of freshwater Tortoises, belonging to the family *Peltocephalidæ*, and the sixth notes on the species of *Rhinoclemmys*, in the British Museum. A communication was read from Mr. W. Theobald, containing critical observations on a paper by Dr. J. E. Gray, on the families and genera of Tortoises, which had been recently published in the Society's "Proceedings." Amongst other remarks, Mr. Theobald stated the skull upon which Dr. Gray had established his *Testudo* (*Scapia*) *falconeri*, appeared to have originally formed part of one of the typical specimens of *Testudo phayrei*, in the Indian Museum, Calcutta.—A paper was read by Mr. A. G. Butler, containing a list of Diurnal Lepidoptera, collected by Mr. Spaight in Northern India.—A communication was read from the Rev. O. P. Cambridge, containing descriptions of some new genera and species of *Araneidea*.—A communication was read from Mr. W. Vincent Legge, containing notes on a species of *Prinia* from Ceylon. A communication was read from Surgeon Francis Day, containing a memoir on the Fishes of the Andaman Islands. Mr. Day's list embraced no less than 255 species, chiefly marine, which had been collected during the short space of a three weeks' visit to those Islands.

Linnean Society, Nov. 3.—Mr. G. Bentham, President, in the chair. A paper was read by Dr. Mansel Weale, "On the Fertilisation of certain Orchids and Asclepiads." He described several species of *Habenaria*, some of them new, from South Africa, and explained the process of fecundation, which he believes to be self-fertilisation by the agency of insects. Some of the species, however, fructify comparatively seldom. At night some of the species emit a delicious fragrance, and these are chiefly visited by nocturnal moths. Dr. Weale also contributed a note on a solitary bee from South Africa.

Entomological Society, Nov. 7.—Mr. H. W. Bates, V.P., in the chair.—Mr. T. H. Briggs, of Lincoln's Inn, was elected a member.—Exhibitions of British *Lepidoptera* were made by Mr. Bond, Mr. Howard Vaughan, and Mr. McLachlan; of British *Coleoptera*, by Mr. F. Smith and Mr. Dunning. Mr. Edward Saunders exhibited *Xixuthrus heros*, a gigantic species of *Prionidæ*, from the Feejee Islands. Mr. Albert Müller exhibited galls of *Cynips agama*, and reniform spangles of *Cynips renum*, both on the underside of leaves of the oak.—Communications were read respecting swarms of *Chlorops lineata* which occurred in houses at Cambridge and elsewhere; and respecting the injury done to pear, plum, and cherry trees by the grubs of *Blennocampa Cerasi*.—The following papers were read: "On Butterflies from Basuto-land," by Mr. Roland Trimen; "On *Cerambycidæ* from the Amazons, by Mr. H. W. Bates: "On *Curculionidæ* from Australia," by Mr. F. P. Pascoe; "Notes on the *Eurytominæ*," by Mr. F. Walker.

Anthropological Society, Nov. 1.—Dr. Charnock, V.P., in the chair. Mr. Matthew Heslop, Mr. J. R. Mortimer, Mr. Ankithum Venkata Nursing Row, Mr. James Hope, and Mr. Walter M. Parker, were elected members. Mr. William Storey, M.D., was elected local secretary for Malta; and Mr. Frank Wilson was elected local secretary for St. Paul's de Loanda, W. Africa. A verbal communication was made by Mr. Charlesworth, giving some details of his discovery, and the exhumation, from a Tertiary formation in East Anglia, of a nearly perfect skeleton of one of the gigantic Bovine animals which the Roman legions met with when they first penetrated into the dense forests of Belgium and Gaul, and which are described by Cæsar under the names of *Uri* and *Bisontes*. Dr. R. Charnock, V.P., F.S.A., read a paper on "The People of the Isle of Marken."

Ethnological Society, Nov. 8.—Dr. A. Campbell, Vice-president, in the chair. The following new members were announced :—The Earl of Antrim, Mr. W. Bragge, Mr. H. Rivett Carnac, and Mr. J. E. Lee.—A carved wooden implement found beneath 27 feet of guano in the Island of South Guanape, was exhibited and described by Mr. Josiah Harris. Col. A. Lane Fox exhibited a rudely worked stone implement from Borneo, being the first which has reached this country from that locality.—A note was read from Mr. C. R. Markham relative to the term *Aymara*. He maintained that the people inhabiting the basin of Lake Titicaca were known, at the time of the Spanish conquest, as Collas and not as Aymaras. An elaborate reply to this communication was made by Mr. David Forbes, F.R.S., who showed that the term Colla is merely a geographical expression.—A paper was then read "On the Kimmerian and Atlantean Races," by Mr. Hector McLean. The author described the Atlanteans as a dark race, forming a considerable ingredient in the population of Spain, southern and central France, South Wales, and the south and west of Ireland and Scotland; whilst the Kimmerians were a fair people, often of tall stature, with gaunt features, ruddy complexion, grey eyes, and red or yellow hair. These Kimmerians occupied the British Isles, and mixed with the Atlanteans previously to the arrival of the Scandinavians and Teutons. By means of the names of places, the author traced them in their migrations westwards from their original home between the Don and the Volga. They were known as the Galli or Galatæ, names signifying *white* people. The author concluded that the present population of England is more truly British or Kimmerian than the Welsh, and that our language is not the direct descendant of the language of the Saxon conquerors, but the descendant of both that of the conquerors and a kindred native language. The discussion was sustained by Mr. J. F. Campbell (of Islay), Dr. O'Callaghan, Mr. Hyde Clarke, Mr. Pusey, and Dr. Nicholas.

Chemical Society, Nov. 3.— Prof. Williamson, F.R.S., in the chair. The following gentlemen were elected as fellows :—D. Howard, T. Muter, C. W. Siemens, F.R.S. On opening this first meeting in the new session, the President alluded in a few earnest words to the loss the Society had recently sustained through the death of two of the most distinguished of its members. The following papers were read :—"On the Production of the Sulphates of the Alcohol Radicles from the Nitrites by the action of Sulphurous Acid," by E. T. Chapman. When sulphurous acid gas is passed into nitrite of amyl it is rapidly absorbed. The nitrite changes in colour from yellow to green, from green to blue; it then begins to effervesce, and at the same time becomes hot and boils violently. Nitric oxide is evolved in abundance, and a yellow liquid product remains. This liquid cannot be distilled without decomposition. It contains no nitrogen. When an attempt is made to distil it along with water, the oily liquid found in the distillate consists principally of amylic alcohol. When the liquid is distilled *per se* it blackens, gives off sulphurous acid, and yields a complex distillate which contains, amongst other things, valerianate of amyl. This circumstance stood in the way of any attempt to obtain an insight into the nature of this reaction. Mr. Chapman resolved, therefore to attack the question quantitatively. With this object he determined the nature and amount of the gas evolved by the action of excess of sulphurous acid on a known weight of the nitrite. The result proved that the gas evolved consisted of pure nitric oxide, and that the whole of the nitrogen present in the nitrate escaped in this form. The amount of sulphurous acid necessary to decompose a given quantity of nitrate was also determined by observing the quantity of sulphurous acid which disappeared when an excess of sulphurous acid acted on a known quantity of nitrite. It was then ascertained with sufficient accuracy that one atom of sulphurous acid acted upon two atoms of nitrite of amyl and liberated two atoms of nitric oxide. Lastly, the alteration of weight which nitrite of amyl undergoes by the reaction was determined. The result confirmed the supposition that the reaction consisted in the replacement of two atoms of nitric oxide by one of sulphurous acid.

$$\left.\begin{matrix}C_5H_{11}O.NO\\C_5H_{11}O.NO\end{matrix}\right\}SO_2 = \left.\begin{matrix}C_5H_{11}\\C_5H_{11}\end{matrix}\right\}SO_4 + 2NO$$

The resulting liquid compound had therefore the composition of neutral sulphate of amyl. It readily breaks up into amylic alcohol and sulphuric acid by boiling with water and by long standing even with cold water; treated with strong hydriodic acid it yields sulphuretted hydrogen, water, iodine, and amylic iodide; potassic bichromate and sulphuric acid cause it to yield valerianic acid. It is to be observed that it is necessary gently to warm the retort in which the nitrite is exposed to the action of a stream of dry sulphurous acid; if this is not done, the SO_2 is absorbed for some time without any reaction occurring, but when the reaction does start it is with almost explosive violence; whereas, if gentle heating has been applied from the beginning, the reaction starts at once and goes on regularly. It is also desirable to pass through the apparatus carbonic acid or hydrogen before the SO_2 has been passed into the nitrite, and to do this also afterwards, for the purpose of excluding air, the oxygen of which would unite with the NO, and the SO_2 would then be expelled. Sulphurous acid and butylic nitrite react upon one another in a manner analogous to that of SO_2 or amylic nitrite, but the resulting product is even more unstable. Sulphurous acid and nitrite of ethyl do not readily act upon each other, at least not at the common temperature. Mr. Chapman then proceeded to the theoretical considerations which are suggested by the above facts. Are these compounds, properly speaking, sulphates of alcohol radicles, or only bodies isomeric with them? The reaction of the amyl compound with water is very different from that of sulphate of ethyl under similar circumstances; it does not, when boiled with water, form an acid analogous to isethionic acid, but splits up into sulphuric acid and amylic alcohol. This would suggest a different linking of the molecules; most probably in the common amylic sulphate the two organic radicles are linked to oxygen directly, and by oxygen to sulphur; in the amyl compound obtained from the nitrite, one of the radicles is attached directly to sulphur and the other indirectly through the oxygen. Mr. Chapman then described the apparatus by which the nature of the reaction of sulphurous acid on amylic nitrite has been determined. The evolved nitric oxide was transformed into nitric acid, and this treated with barytic carbonate. In the discussion following this paper, Dr. Debus and Mr. Harcourt expressed their apprehension that along with the barytic nitrate also some barytic nitrite may have formed. Mr. Chapman replied that he had taken great care to ensure the complete transformation into nitric acid; that to this end he had passed great quantities of oxygen into the collecting cylinder, let the mixture stand for twenty-four hours over the water in the cylinder, and lastly applied gentle heat to it. As for the possibility of the water acting as a base and reducing the formed nitrous acid, there was the counteracting presence of SO_2, which had passed unabsorbed through the amylic nitrite into the collecting cylinder, and part of which must have been oxydised to sulphuric acid. Regarding Mr. Chapman's theoretical views of the union of SO_3 and $\left.\begin{matrix}C_2H_5\\C_2H_5\end{matrix}\right\}O$, the President thought that it is the same as that of SO_3 with H_2O, that the two C_2H_5 are linked to SO_3 by the oxygen, just like the two H are in hydric sulphate.—Mr. Vacher read a paper by Mr. Elliott "On the determination of Sulphur in cast-iron." This determination consists in the liberation of the sulphur in the form of sulphuretted hydrogen, absorption of the latter by solution of soda and estimation of the H_2S in the acidified soda solution by means of a standard solution of iodine. In the course of his experiments, Mr. Elliott noticed that a portion of the iron was not attacked by HCl, and that this insoluble residue yielded on fusing with pure caustic soda (prepared from sodium) sodic sulphate. From this Mr. Elliott draws the conclusion that cast-iron contains sulphuric acid as well as sulphur. In somewhat similar manner the presence of phosphoric acid in cast-iron was ascertained. Dr. Debus doubted the probability of the occurrence of sulphuric acid in iron which had once been in a molten state. Mr. Church remarked that he had noticed in dissolving natural pyrites in HCl, that very often an insoluble sulphide and phosphide remained. At the same time, he considered the fusion of those residues with caustic alkalis as a sure means of converting the sulphur and the phosphorus into the respective acids, the alkalis being under such circumstances very powerful oxydising agents. Mr. Harcourt expressed his concurrence with Mr. Church's remarks about the oxydising action in this case of caustic soda. Mr. Vacher did not wish to support Mr. Elliott's view about the presence of sulphuric acid in cast-iron, but there was the fact of one portion of the sulphur not being liberated by HCl, and this forces to the conclusion that cast-iron contains sulphur in another form than in that of the ordinary sulphide. —E. A. Letts, "On the Composition of Hyposulphites." Though the salts of the hyposulphurous acid have been comparatively well studied, much difference exists as to their chemical constitu-

tion. Rose states that an atom of water is retained by all the hyposulphites, and is essential to their composition. On the other hand, Pope and other chemists maintain that most, if not all the hyposulphites, can be obtained in the anhydrous condition. Mr. Letts undertook some experiments to ascertain whether hydrogen was to be regarded as an essential constituent of the hyposulphites. The salts submitted to investigation were those of sodium, barium, lead, strontium, magnesium, nickel, and cobalt. The sodium salt lost all its water by drying *in vacuo* over sulphuric acid. The barium salt gives off its water by drying at 100° C. The plumbic hyposulphite, prepared by adding a solution of lead acetate to one of sodic hyposulphite, crystallises out from the liquid without any water at all. This, then, is a case demonstrating without doubt that hydrogen is *not* needed to complete the chemical constitution of a hyposulphite. The strontium salt retained, even after drying at 200° C., half a molecule of water. The magnesium salt crystallising with six atoms of water, loses three of them at 100° C. ; but an attempt to expel more water causes the decomposition of this salt. The nickel hyposulphite, the crystals of which have also six H_2O, do not part with any of it without undergoing decomposition. The cobaltic hyposulphite is even more unstable than the former salt. The President observed that the contents of the last paper set at rest the doubts which hitherto existed as to the constitution of the hyposulphites, and showed that the water which they ordinarily contain is not essential to their chemical existence.

BERLIN

German Chemical Society, October 10.—President, Prof. Rammelsberg. The President reported on the means employed by the society to promote disinfection of the battle-fields and hospitals. A great number of German and foreign chemical manufacturers had kindly responded to a circular requesting gifts of chemical disinfectants. From England, Messrs. Berger, Spence, & Co., Ballman and Condy, F. C. Calvert & Co, Crowther and Graeson, L. Demuth & Co., C. Kurtz and Sons, C. Lowe and Co., G. Lunge, George Miller and Co., T. Storey and Co., had sent various disinfecting agents. A table detailing the right use of disinfectants had been published and sent to the proper medical authorities and members of the society who have undertaken the superintendence of disinfection in various towns. The seat of war has been visited for the same purpose by six members of the society. The following papers were then read :—A. W. Hofmann : "The history of Nitriles," a reply to M. Mendelejeff, who had published certain views, not knowing the same to have been promulgated before by the author.—T. Thomson : "On the double chloride of Beryllium and Platinum." This salt is isomorphous with the corresponding calcium salt, and not with the magnesium salts, as had been supposed. The same author on "The supposed Connection of the law of Avogadro with the Mechanical Theory of Heat." A mathematical deduction lately published by Naumann, contains, according to Thomson, an error invalidating its argument.—C. Rammelsberg, in a lecture on the relation of mineralogy and chemistry, urges upon mineralogists to apply the modern formulæ.—A. Bauer described an alloy of lead and platinum of the formula Pt Pb.

October 24.—H. Wichelhaus described β Nitronaphtol. This compound, which cannot be produced in the ordinary way, may be obtained by treating alcoholic solution of β naphthol with nitric acid, as was lately recommended by Bolley. The same chemist has obtained Triacetamide by applying a similar method to that employed by Kekulé in the preparation of diacetamide. The latter is obtained by acting on acetonitrile with acetic acid ; the former by replacing the acid by acetic anhydride : $CH_3 C N + (C_2 H_3 O)_2 O = (C H_3 C O)_3 N$. The three amides have nearly the same physical properties. The diamide, according to Kekulé, forms salts. The triamide is an anhydride converted by $P_2 O_5$ into acetonitrile and acetic anhydride.—Petersen on Nitrochlorophenoles. By continuing the researches of Baer-Predari, the author has produced five of the six possible isomeric bodies of the above constitution.—C. Rammelsberg, on Yttrocerite, determines the formula of the mineral $Ce F_3, 2Y F_3, 9 Ca F_2, 3$ aq. Berzelius had found 1 Y 2 Ce. Both Y and Ce represent rather groups of metals than well-defined single elements. In a discussion following this communication, G. Rose called attention to the isomorphism of yttrocerite and fluorspar, thinking that most likely the water found in the former (2½ per cent.) does not form a constituent part of the mineral.

BOOKS RECEIVED

ENGLISH.—The Science of Building : E. W. Tarn (Lockwoods).—Elementary Treatise of Natural Philosophy : A. P. Deschanel (Blackie and Sons).—Text-books of Science ; Metals : C. L. Bloxam (Longmans) —Virgil's Bucolis in English Verse : R. M. Millington (Longmans) —Osteology of the Mammalia : Prof. Flower (Macmillan and Co)—The Academy, vol. i.

AMERICAN —Kirklos on Experimental Investigations into the Relationship of certain Lines, pt. 1 : J. Harris (J. Lovell, Montreal).

FOREIGN.—(Through Williams and Norgate)—Biologische Studien : E. Haeckel.—Untersuchungen über den Bau des knöchernen Vogelkopfes : Dr. H. Magnus.—Der Schädel des Maskenschweines : Dr. C. J. C. Lucae.—Beiträge zur vergleichenden Neurologie der Wirbelthiere : N. von Miklucho-Maclay.

PAMPHLETS RECEIVED

Is a Ship-canal practicable? by S. T. Abert (Cincinnati).—Annual Report of the Director of the Cincinnati Observatory —Fossil Sponge Spicules in the Greensand of Haldon and Blackdown : E. Parfitt —Crustacea Podothalmata, and the Histology of their Shells : E. Parfitt —The Improvement of English Orthography : D. P Fry.—The Rainfall of the St Mary Church-road, Torquay : W. Pengelly —The Rainfall in Devonshire, 1869 : W. Pengelly —The supposed Influence of the Moon on the Rainfall : W. Pengelly.—Notes on Vessels made of Bovey Lignite and of Kimmeridge Coal : W Pengelly.—The Ash-hole and Bent-bone Caves at Brixham : W Pengelly —The Literature of the Caverns near Vealhampton : W. Pengelly.—Geography in relation to Physical Science : W. Hughes.

DIARY

THURSDAY, NOVEMBER 17.

LONDON INSTITUTION, at 7.30.—Acoustics of the Orchestra ; Wind Instruments : Dr. W. H. Stone.

CHEMICAL SOCIETY, at 8.—Mineralogical Notices : Prof. N. Story Maskelyne and Dr. Walter Flight.

LINNEAN SOCIETY, at 8.—On the *Passifloreæ* : Dr. M. T. Masters.—On the White-beaked Bottle-nose : Dr. James Murie.

SOCIETY OF ANTIQUARIES, at 8 30 —Egyptian Antiquities, with remarks by Dr. Birch : Mr W. R. Cooper.

SUNDAY, NOVEMBER 20.

SUNDAY LECTURE SOCIETY, at 3.30 —On the Antiquity of Man : Dr. Cobbold.

MONDAY, NOVEMBER 21.

LONDON INSTITUTION, at 4.—Chemical Action : Prof. Odling.

TUESDAY, NOVEMBER 22.

ETHNOLOGICAL SOCIETY, at 8.—On the Concord, the Origin of Pronouns, and the Formation of Classes or Genders of Nouns : Dr. W. H. J. Bleek.—On the Position of the Australian Languages : Dr. W. H. J. Bleek.

WEDNESDAY, NOVEMBER 23.

GEOLOGICAL SOCIETY, at 8.—On some Points in South-African Geology : Mr. G. W. Stow.—Noteon some Reptilian Fossils from Gozzo : Mr. J. W. Hulke.—On the Discovery of a Bone-bed in the Lowest of the Lynton Grey Beds, North Devon : Dr. F. Royston Fairbank.

SOUTH KENSINGTON MUSEUM, at 2 30 —On the Clavecin and the Pianoforte : Ernst Pauer.

ROYAL SOCIETY OF LITERATURE, at 8 30.—On the three Seals of Edward the Confessor : Walter De Gray Birch.

THURSDAY, NOVEMBER 24.

LONDON INSTITUTION, at 7.30.—On the Precious Metals and their Distribution : Prof. Morris

CONTENTS