THURSDAY, FEBRUARY 22, 1894.

BOLTZMANN ON MAXWELL.

Lectures on Maxwell's Theory of Electricity and Light. Part ii. By Dr. Ludwig Boltzmann. 8vo. pp. 166. (Leipzig: Barth, 1893.)

THIS second part of Dr. Boltzmann's account of Maxwell's electromagnetic theory is written from a somewhat different point of view from the first part. The first part presents the theory from the mathematical point of view of a dynamical system whose generalised coordinates are known. This one presents the theory from the physical point of view of a continuous medium whose intimate structure is indeed not fully understood, but whose changes of structure can be fully represented by certain vectors. Although Maxwell has presented the subject from both points of view, the one which really determines the form of his work, and that appears to have led him in his investigations, is the physical view of this second part. A purely analytical view is hardly ever as suggestive as a physical and geometrical one. This latter one suggests extensions, suggests advances in a way that purely analytical investigations seldom do. Compare, for instance, Ampère's investigation of the action of elements of currents on one another with Faraday's treatment of the same subject. The latter has suggested the whole of the recent advances, the working of the ether, the identity of light and electromagnetic waves. The former was magnificent, brilliant no doubt, but it was cold and dead.

In the preface to this second part, Dr. Boltzmann explains the absence of diagrams, marginal notes, &c. which appeared in the former part. He says a friend conveyed to him the valuable criticism that "Your book is dear." He has consequently left out the embellishments that Englishmen love, as being too expensive for the poor German student, and has only left the motto, which costs nothing, to wit:—

> "War es ein Gott, der diese Zeichen schrieb
> Die mit geheimnissvoll verborg'nem Trieb
> Die Kräfte der Natur um mich enthüllen
> Und mir das Herz mit stiller Freude füllen."

And even this he has not taken from that classic lore that Englishmen delight in, but from a German poet.

Although he regrets the dark and inconsequent character of much of Maxwell's work, he congratulates scientific men that this has left them the more to do. For himself, he only claims to be an exponent of Maxwell's views, and hopes that he may succeed in helping students to understand them.

Electromagnetic equations lend themselves to a great variety of interpretations by analogy with displacements of a medium. They are a system of vectors related to one another by a very simple method of derivation each from the last, by the process of what Dr. Boltzmann says the English call "curling." Starting with the vector potential, the magnetic force is its curl, and the curl of the magnetic vector is the time rate of variation of the electric vector. Any one of this system of vectors may be likened to the displacement or velocity of an incompressible medium, and hence we have one system in which the vector potential is so likened, one in which the electric displacement is so likened, and one in which the magnetic displacement is so likened. These latter two analogies have been the favourite ones. Maxwell frequently speaks in terms of the system in which the electric vector is likened to a displacement of an incompressible medium, and the likening of the magnetic vector to a flow is quite common. In these systems electric energy is generally considered as potential, and magnetic energy as kinetic. Dr. Boltzmann, however, likens the vector potential, which he calls the tonus, and which Mr. Oliver Heaviside relegates to the realms of merely convenient suppositions, to a displacement of the medium; and in accordance with this analogy the magnetic energy becomes potential, and the electric vector, which is proportional to the rate of variation of the vector potential, being thus a velocity, requires the assumption that electric energy is kinetic. An obvious difficulty arises here from the necessity of making an electric current an acceleration which cannot of course be constant for ever. In this connection it may be worth while observing that the possible existence of one closed surface inside another with static lines of the electric vector between them makes it necessary to assume either (1) that the vector potential represents a twist round its line, and not a displacement along it; or (2) that it is a displacement up some lines and down others; or (3) that there are sources and sinks of the ether where there is electrification, because without sources and sinks we cannot have a continuous flow going on out from a closed inner surface to a closed outer one. If the tonic vector be a twist, the magnetic vector will be of the nature of a Δ^2, and it would be this structure which should be elastically resisted, and not a twist, as Dr. Boltzmann's and Mr. Larmor's assumptions give. This would return somewhat to Mr. Glazebrook's proposal of years ago. Of course, a complex change of structure, such as a combination of 1 and 2, or any other change, such as crystallisation in a hemihedral crystal, would be a possible solution. In a hemihedral crystalline form, because its two ends must differ in sign. Gravity is probably due to a change of structure produced by the presence of matter, which is analogous to a non-hemihedral crystallisation because it is always attractive; there seems no reason to suppose that any bodies exist possessing negative gravitation, the supposed levity of the old philosophers.

To the vector potential, Boltzmann gives Faraday's name of electrotonic state at the point, or, shortly, the tonus of the element of volume. The rate of change of this tonus is the electric vector E, and the kinetic energy due to it is the electric energy per unit vol.

$$T = K/8\pi \, . \, E^2$$

This tonic strain is accompanied in general by a tonic stress depending on the curl of the tonic vector which is the magnetic vector, H = curl E, and a corresponding potential energy

$$V = \frac{\nu}{2} H^2.$$

All this is most interesting in connection with Mr. Larmor's recent papers. He uses Maxwell's analysis in which the magnetic energy is kinetic, and consequently assumes the magnetic vector to be a flow, which he has

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pointed out can exist as an irrotational one without reaction in MacCullagh's medium. This same observation of course applies to Dr. Boltzmann's analysis, the difference being that vortex rings would be rings of magnetic current instead of electric current, and atoms would act like electric diads instead of elementary magnets. The existence of unclosed lines of electric vector, however, seems to make this simple interpretation of Dr. Boltzmann's analysis impossible, and as we cannot in general substitute whirl for flow in fluid motion, the vortex ring analysis could not be applied if Dr. Boltzmann's electric vector were interpreted as a whirl; and hence Mr. Larmor's investigation seems confined to the interpretation he has given.

Having assumed that the medium is such as to react elastically against curl of the tonus (τ), and his fundamental equations thus being—

$$(1),\ E = \dot{\tau}\ ;\ (3),\ T = K/8\pi \,.\, E^2$$
$$(2),\ H = \text{curl}\ \tau\ ;\ (4),\ V = \nu/2 \,.\, H^2$$

he proceeds to deduce the equation corresponding to (2), namely, (5) $K\dot{E} = \text{curl } H$ by applying Hamilton's principle to $T - V$ in a way which is well known. He now remarks that in accordance with his dynamical principles $K\dot{E}$ is rate of change of momentum, and he adds to curl H any external impressed forces, which he divides into two classes: (1) those due to reversible causes, such as electromotive of contact, chemical action, &c. (F); and (2) those due to irreversible causes, such as ohmic resistance, &c., which are proportional to the electric vector, and thus obtains this equation in the form

$$KE = \text{curl } H - 4\pi C\ (E + F)$$

It would thus seem as if the electric conduction current were a different thing from a changing electric displacement, though both depending on curl H. This arises from the difficulty noticed above, and seems to require careful consideration. By judicious theories as to the function of the matter in stopping the continual acceleration without uncurling the H, the difficulty can be surmounted. In order to get over all the difficulties of discontinuities at the surfaces of bodies, Dr. Boltzmann assumes that the properties of the ether vary rapidly but continuously in passing across a surface, so that he can assume that these equations apply everywhere.

Depending on his dynamical basis, Dr. Boltzmann has obtained the following dimensions for electromagnetic quantities, which, of course, differ entirely from both the electric and magnetic systems of units—

$$[E] = [LT^{-1}],\ [K] = [ML^{-3}]$$
$$[H] = [ML^{-1}T^{-2}]^{*}\ [\mu] = [M^{-1}LT^{-2}]$$

After a short discussion as to the possibility of founding the science upon a purely analytical basis, by assuming equations and showing that they lead to true results, which is the basis of Hertz's method, and a short criticism of this method as applied by Hertz, Dr. Boltzmann proceeds to show how the old equations of action at a distance and von Helmholtz's work are connected with Maxwell's view of the subject. His treatment of superficial effects by means of a rapid variation of structure of the ether at the surface of solids, seems essentially the same as Mr. O. Heaviside has advocated in opposition to von Helmholtz's double electric layers. All this analytical method is, of course, necessary and interesting in what may be called a transition work, one that concerns those who have been brought up under one school of thought and are entering another; a sort of epistle to the Hebrews, a college between youth and manhood. It concerns the past rather than the future, towards which we should press, forgetting those things that are behind.

Although Dr. Boltzmann has left out all embellishments, he has had pity on his readers. There are necessarily included in a transition work of this kind innumerable formulæ, of which 168 are frequently referred to, and these he has collected into two folding sheets, each of five folds, at the end of his work. This is most considerate. Books dealing with many formulæ might well follow suit, although it certainly is a little terrifying to have 168 formulæ presented as the outcome of the book in a way that necessarily attracts the attention of anyone who thinks of reading it. Those who are frightened by this should, however, recollect that the whole subject of electromagnetism depends on only four very simple equations. Dr. Boltzmann would have much simplified his work if he had adopted any vector symbolism.

It is to be hoped that this part of Dr. Boltzmann's work, as well as the former part, will soon be translated, and so made easily accessible to English students. The work of a great master, the product of a great mind, helps all men who can understand it.

* This is misprinted in the text, but right in the table of formulæ at the end of the book.

THE STORY OF THE SUN.

The Story of the Sun. By Sir Robert Ball, LL.D. (London: Cassell and Co., 1893.

THERE is no more interesting chapter in science than that which deals with our great central luminary. Its story has been gradually gaining in interest since the first application of the telescope to its study by Galileo, and since the advent of the spectroscope our knowledge of solar phenomena has advanced by leaps and bounds. At the present time the scrutiny of the sun is more minute and continuous than ever, and the constant acquisition of fresh information sufficiently explains the need for additional works on the subject, or for new editions of old ones.

The author of the book before us does not approach the subject as a practical investigator in this branch of astronomy, and his efforts are therefore chiefly intended for the delectation of that class of readers for which he chiefly caters. The first thing that strikes one on glancing through the pages of the book is the great variety of the matter which it contains, and one begins to wonder if he has mistaken the title of the volume. It is not too much to say that nearly every department of astronomical inquiry is touched upon more or less; from the determination of the polar flattening of the earth to the photography of minor planets and the appearances of nebulæ. Though the author never seems at a loss to give reasons for the introduction of matter apparently not at first sight connected with the subject in hand, his reasons frequently appear to be nothing more than excuses for filling so many pages. For example, we fail to see the necessity of devoting a whole chapter to the members of the solar system, or a large part of a chapter and a full-page plate to eclipses of the moon; again, the discussion of the Glacial Period surely belongs more to the story of our own planet than to that of the sun, and might very well have been omitted.

This method of treatment is the more objectionable as it has evidently involved the omission of reference to many observations of great interest, and must inevitably tend to give the impression that our knowledge is very much less than it is in reality. At the same time it does an injustice alike to the reader, and to the army of workers who devote their energies to the pursuit of this branch of knowledge.

Again, the story of the sun would certainly lose none of its charm by historical treatment, but we look in vain for even the barest mention of the names of Ångström, Thalén, Faye, Cornu, Perry, Balfour Stewart, and a host of other workers who have taken so great a part in solar inquiries.

So far as it goes, however, the story of the sun is told in that fascinating way which has deservedly brought the author fame, and our greatest cause of complaint is that it does not go far enough. The first five chapters, occupying nearly one-third of the book, deal with the solar system, the sun's distance, and the sun's mass. In these well-worn subjects there is nothing new to tell and little scope for novelty, but occasionally we come across some of the bright illustrations at which the author is so expert; as, for instance, the endeavour to impress the reader with the magnitude of the velocity of light.

In chapter vi. a fair account is given of the total amount and spectroscopic analysis of the "light of the sun." A coloured plate of the solar spectrum is of unusual excellence, but many of the finer details of Mr. Higgs's photographic spectrum are lost in the reproductions, and scarcely do justice to the originals.

After a chapter on the causes of eclipses, we come to one on sun-spots, and here we first find evidence of the incompleteness to which reference has been made. The appearances presented by spots are fully described and illustrated by a most liberal allowance of diagrams, but no mention is made of "veiled spots." The rate of solar rotation is discussed in considerable detail, but if the spectroscopic results are to be mentioned at all, Dunér's observations might have found a place alongside those of Mr. Crew. The author seems to favour the idea that the varying rotation of the photosphere in different latitudes is produced by the friction of concentric shells of the matter of which it is formed; other views, not less probable, are utterly ignored; as, for instance, one which follows from the theory that spots are formed by downrushes of cool vapours—an explanation which Sir Robert Ball has adopted as the most probable. The reader is also left in blissful ignorance of the fact that astronomers have taken the trouble to make a minute study of the spectra of sunspots, although this work has been going on continuously for the last fifteen years, chiefly at Kensington and Stonyhurst. Of the existence of the Committee on Solar Physics, and of the continuous photographic record of the spots which it has organised, the author seems to have no knowledge. Even the importance of the eleven-yearly period does not appear to be clearly grasped, and only the very briefest references are made to this fundamental solar unit.

The chapter on solar prominences contains most of the ordinary information on the subject, and has the merit of including some of the most recent observations by Trouvelot and Fenyi. In addition, Prof. Hale's remarkable work in photographing these objects is considered in some detail. Very little attempt is made, however, to distinguish between quiet and eruptive prominences.

The solar corona is dismissed in very few words, and the illustrations have not been well chosen. In giving a somewhat detailed account of the American expedition to French Guiana to observe the eclipse of December 1889, it would have been gracious to record the fact that it was during an expedition to this place at the same time that the late Father Perry met with his fate. The author's estimate of our spectroscopic knowledge of the corona is very low, and it is disposed of in twenty lines; but this meagre description is due to the fact that the most recent observations referred to are those made by Janssen in 1871!

It has been well remarked that "hypothesis is the soul of investigation," but our author makes no attempt to give a full or complete account of any theory; apparently on the ground that we are still so "very ignorant concerning the actual physical nature of the great luminary." The principal point of theory touched upon is that which concerns the materials of which the photosphere is composed, the ordinary view that it consists of glowing clouds being accepted. Working on the lines of a suggestion made by Dr. Johnstone Stoney in 1867, the author argues in favour of the view that to carbon "belongs the distinction of being the main source whence sunlight is dispensed." (p. 289.) This certainly seems as probable as the generally accepted idea that the photosphere consists of liquid metals, but it does not give us any further insight into the causes of the various phenomena which are observed. The study of the circulation of the sun's atmosphere will no doubt eventually furnish the key to most of the problems of solar physics; but here our author leaves us, with nothing more than an unexplained diagram illustrating a theory of the solar currents.

Of the chapter dealing with solar and magnetic phenomena, we have only to note that the author repeats the mistake with reference to the Carrington-Hodgson outburst—a subject which has already been discussed in these columns. He makes a suggestion, however, which may be well worth consideration, namely, that the solar and magnetic disturbances may not stand in the relation of cause and effect at all, but are each of them "manifestations of some other influence of electromagnetic waves on a vast scale sweeping through our system, and influencing the magnetic phenomena in the various bodies of which our system is composed." (p. 234).

We see the author at his best in the next three chapters, discussing step by step the probable cause of the maintenance of solar radiation. The somewhat difficult subject of molecular physics no longer remains obscure under the influence of his luminous exposition, and their application to the sun will be clear to any intelligent reader. This part of the subject is only marred by the false analogy with Nova Aurigæ, which is pointed out as suggesting a possible original source of the sun's heat. We should have imagined that phenomena which last only for a few weeks, must be vastly different from those which continue for millions of years.

From one point of view the discourse on "the sun as a star" is excellent. It is certainly interesting to know that the sun is only one of many millions of stars, that it does not travel so quickly in space as 1830 Groombridge; or again, that it is a certain number of times less massive than Arcturus; but not less interesting is the study of its physical relation to the other stars—what stars it may have resembled in the past, and what it will probably resemble in the future. On this latter question much light has been thrown by recent work on stellar and nebular spectra, with which we cannot but suppose the author to be familiar. The whole of this great problem, however, is discussed in little more than a page of text and a page of *drawings* of stellar spectra, on various scales, to which no direct reference is made. The fact that carbon plays such a prominent part in the absorption spectra of one group of stars is not mentioned, and the author seems to have utterly failed to see the significance of it in relation to the presence of carbon in the sun, of which he makes so much in another chapter. From the evolutionary point of view this is obviously a fact of the first importance, indicating that as the sun goes on cooling the carbon absorption will increase until finally its spectrum will resemble that of such stars as 152 Schjellerup. The presence of a plate illustrating various nebulæ led us to suppose that we should be treated to the story of the sun's probable growth from the nebulous stage, but we were disappointed to find that they were only intended to indicate that our sun is but one of a myriad host of stars!

Sir Robert Ball's views of the cause of the Ice Age, which have already been discussed in NATURE, are very clearly set forth, and he maintains that "it is impossible to doubt the truth of the main factors in the astronomical theory of the cause of Ice Ages" (p. 319).

The final chapter, on "the movements of the solar system," is an excellent exposition of the method by which the direction of the sun's motion in space is ascertained.

We may perhaps repeat that the story of the sun is told admirably so far as it is told at all, but we regret to find that so many solar inquiries of the greatest interest have not had the great benefit of description by the author's graphic pen.

We have nothing but praise for the excellence of the majority of the plates and diagrams, and the printing is also bold and clear. A. FOWLER.

THE LEPIDOPTERA OF THE ATLANTIC ISLANDS.

The Butterflies and Moths of Teneriffe. By A. E. Holt White. Edited by Rashleigh Holt White, Vice-President of the Selborne Society. Illustrated from the Author's Drawings. (London: L. Reeve and Co., 1894.)

THE coleopterous fauna of the Atlantic Islands has been well worked by the late Mr. Wollaston, but as regards the *Lepidoptera*, the only obtainable information has been either from large books (which rarely supply complete or detailed information) or detached papers, some of them very valuable, but not always easily accessible.

Consequently, when Mrs. Holt White, the wife of one of the descendants of a brother of Gilbert White, spent the winter of 1892-93 in Teneriffe for the benefit of her health, and occupied herself with the collecting and rearing of butterflies and moths, she could find no available information on the subject, and bravely resolved to do her best to supply the want. The result is the little book before us, which, though making no pretensions to be otherwise than popular, will yet be most useful to scientific entomologists, by supplying them with detailed descriptions and fairly good figures (though the first plate of the four strikes us as being somewhat coarsely coloured) of nearly all the larger *Lepidoptera* of a very interesting, though very limited fauna. One moth is described as new, and others are now figured for the first time.

A striking feature of the Atlantic Islands is the extreme poverty of their lepidopterous fauna. Our British *Lepidoptera* are considered few; but we can at least point to upwards of 2000 species; and even Iceland, though possessing no indigenous butterflies, boasts of nearly as many moths as Madeira or Teneriffe. Several causes combine to produce the scarcity of *Lepidoptera* in the Atlantic Islands. They are islands, far from the mainland, and on the extreme limits of the faunas to which they respectively belong. The native flora has in some places almost disappeared, and with it, of course, the insects dependent on it. How far the present insects of the islands are endemic, it is difficult to say. Some are certainly peculiar to the islands; the bulk of the species of the northern islands are European, or representative of European species; one or two are American, but whether introduced, or whether remnants of an outlying American fauna, it is at present impossible to say; and stranger still, one or two are East Indian in their affinities, and are not species likely to have been introduced by accident. The best representatives of the last two classes are *Pyrameis huntera* and *P. callirhoë*.

The six principal groups of Atlantic islands from north to south are the following: the Azores, Madeiras, Canaries, Cape Verdes, Ascension, and St. Helena. Of the *Lepidoptera* of the Cape Verdes and Ascension very little is recorded, and we need say no more of them in this place.

The Azores lie further to the north and west than any of the other groups. Mr. Godman's "Natural History of the Azores" (1880) is our latest authority on the *Lepidoptera*. He enumerates nine butterflies and twenty-eight moths, all British, except the North American *Danais archippus*, and the South European *Hypena obsitalis*. It is worthy of remark that the typical *Pieris brassicas* occurs in the Azores, instead of the allied *P. Wollastoni*, which occurs both in the Madeiras and Canaries, or *cheiranthi*, which is confined to the Canaries. *P. Wollastoni*, we may here note, much resembles the North Indian *P. nipalensis*.

In the *Transactions* of the Entomological Society for 1891, Mr. Bethune-Baker published "Notes on the *Lepidopter* collected in Madeira by the late T. Vernon

Wollaston," with one plate, enumerating eleven butterflies and fifty-six moths. The *Micro-lepidoptera*, not here included, and which have partially been worked out by Messrs. Wollaston and Stainton, were reserved for a future paper. In addition to *Picris Wollastoni*, already mentioned, the remarkable form *maderensis* of *Gonepteryx cleopatra* (intermediate between the type and the Canarian *G. cleobule*), and the dark forms of *Satyrus semele* and *Polymmatus phlœas* are remarkable; but much more so is the occurrence of a *Deilephila* apparently identical with the Indian *D. lathyrus*.

Previous to Mrs. Holt White's book, the principal sources of our information regarding the Canaries were Webb and Berthelot's "Histoire Naturelle des Iles Canariennes," in which twenty butterflies and thirty-three moths were enumerated, and a paper by Alpheraky in the fifth volume of Romanoff's "Mémoires sur les Lépidoptères," noticing fifty-seven species, of which seventeen were butterflies, several of which are figured. We may mention that the white form of *Danais chrysippus*, found in Teneriffe, more resembles the Indian var. *alcippoides* than the common African var. *alcippus*. An interesting species figured by Mrs. Holt White is *Euchloë charlonia*, a species previously known from North Africa and Western Asia (not North and West Africa); and among the moths we notice a figure of *Rhyperioides rufescens*, described, but not figured, by Brullé, in Webb and Berthelot's work, and several other species peculiar to the islands. Mrs. Holt White describes twenty-nine butterflies and thirty-five moths, and adds a list of twenty-seven others, chiefly *Micro-lepidoptera*, which she considered too small or obscure to be included in a popular work. However, if a new edition of her useful little book should be required, we hope she will complete it at least as regards the *Macro-lepidoptera*, and that she may also be induced to extend it to include the *Macro-lepidoptera* of Madeira.

We may add that Dr. H. Rebel has lately published a paper on the *Micro-Lepidoptera* of the Canaries, in which sixty-three species are enumerated (*Annalen d. k k. Naturhist. Hofmuseums*, vii.; Vienna, 1893), with one plate.

The last list of the *Lepidoptera* of St. Helena was published by Mrs. T. Vernon Wollaston in *Ann. and Mag. Nat. Hist.*, ser. v. vol. iii. (1879). A large proportion of the species are endemic; the others are chiefly wide-ranging African species, several of which are common to the Northern Atlantic Islands, and even to Europe.

W. F. KIRBY.

THE ACTIVE PRINCIPLES OF PLANTS.

Dictionary of the Active Principles of Plants. By C. E. Sohn. (London: Baillière, Tindall and Cox, 1894.)

PROBABLY no section of organic chemistry has been more prolific of results, or has added more to the literature of recent years, than that which has dealt with the vegetable kingdom. So many investigators have been occupied with the so-called active principles of plants, that the task of keeping up acquaintance with current researches is a very laborious one, and there is little cause for surprise if much work in this field is in danger of being overlooked or undervalued. On this account the publication of a work which undertakes to gather together so many scattered papers, and to summarise in a convenient form their most important matter, is likely to be hailed with gratitude by many workers both in organic chemistry and in vegetable physiology. The author has wisely limited himself to some definite sections of the work, and the present volume deals especially with the alkaloids, the glucosides, and the bitter principles. In dealing with the literature of these, he has first set forth the members of these groups which have been chemically examined, taking them in the order of the botanical name of the plant which yields them. In the case of each he gives an account of its botanical source, the workers who have investigated it, and the chief chemical and physical peculiarities it presents. Where, as in so many cases, one plant yields more than one of such principles, all that have been prepared from it are described successively. A summary of the more striking features of each, put in tabular form to admit of ready reference and comparison, forms the second part of the work, while a rearrangement of them, grouped according to their behaviour with various chemical reagents, constitutes Part iii. An idea of the completeness and care with which the book has been compiled may be obtained from the fact that nearly 600 of these vegetable bodies have been described, while the references to contemporary literature embrace the work of the first half of 1893.

The author deals with the various bodies described chiefly, if not entirely, from the point of view of the chemist or the analyst. The therapeutical action of the drugs is but slightly touched upon, though the chief physiological actions of each have been briefly stated in many cases. Their importance to the plants in which they occur is apparently beyond the limits that the author has set himself.

As a work of reference the new dictionary will be much appreciated. It would have been more convenient for use if each page in Part i. had been headed by the name of the plant which is being treated. This has been done in Part ii., where it seems scarcely so necessary.

It is hardly to be expected in a work of this character that the proofs should pass without some slight inaccuracy. A list of errata would no doubt rectify the statement that the name of the darnel grass is *Loliam telumentum*, as stated on p. 62.

The botanist will regret that the author did not include in the scope of the work the vegetable enzymes or ferments which play such an important part in vegetable physiology. They are not very numerous, and would well have repaid inclusion. The only exception made is Papaïn, to which a few lines on p. 76 are allotted.

OUR BOOK SHELF.

Forschungsberichte aus der Biologischen Station zu Plön. By Dr. O. Zacharias. Theil 2, pp. 1 152. Two plates and a map. (Berlin: R. Friedländer and Sohn, 1894.)

THE second annual report from this station contains the additions made during last year to a knowledge of the fauna, flora, and physical conditions of the Plöner See, prefaced by a geological and hydrographical paper by Dr. Ule. Lists of the Diatoms are furnished by Count Castracane and Prof. Brun. A case of "the breaking of the meres," caused by great swarms of *Rivularia* (*Gloio* *tricha) echinulata*, is recorded, and the species is described in detail. Amongst the additions to the fauna, a fresh-water Nemertine (*Tetrastemma lacustre*) and a northern leech (*Placobdella raboti*, recorded by Prof. Blanchard), several Protozoa and Rotifers are noteworthy. It is, however, to the Plankton that the Director has devoted special attention since the founding of the station in 1891, and accordingly the influence of temperature on the constituents, their unequal distribution through the lake, and their appearance, maximum abundance, and gradual disappearance are carefully noted, together with the bearing of these facts on the present position of the Plankton question.

The occurrence of certain Protozoa (*Carchesium polypinum* and *Epistylis lacustris*) freely floating in the Plöner See during June and July in great numbers, and under conditions that do not warrant the supposition that they had been torn away from their supports, is recorded by Dr. Zacharias, who suggests that this may be a periodic change from the fixed to the free-floating habit, and that, further, the pelagic species of *Dinobryon* and *Floscularia* may have a similar origin. The researches conducted at Plön are, however, not the first to direct attention to this point, as Dr. Zacharias asserts (p. 123). Lang ("Ueber den Einfluss der festsitzenden Lebensweise," p. 152: Jena, 1888) has already made the same suggestion, based on the presence of *Zoothamnium* noticed by himself, and more frequently by Brandt and others in plankton collected at Naples.

Another interesting point about which we at present know very little, is the changes of form assumed by the same species at different times of the year. In reference to this matter, Dr. Zacharias describes the seasonal changes in three species of *Hyalodaphnia*, *Bipalpus vesiculosus* (a rotifer), and *Ceratium hirundinella*.

The enlarged size of this report gives evidence of the increasing interest in fresh-water biology, also shown by the fact that a new station is in process of erection on the border of the Müggel See, near Berlin. Two plates, illustrating the new species obtained, and a map of the neighbourhood of Plön, are given with this part.

F. W. G.

Biology as it is applied against Dogma and Freewill, and for Weismannism. By H. Croft Hiller. Second edition. (London: Williams and Norgate, 1893.)

ON a first glance through this unusual book, there rises in one's mind the delightful remark that the mother of David Hume is reputed to have made to him—"Man, Davie, you'ed believe anything if it's no in the Bible." For Mr. Croft Hiller accepts in the most trusting spirit the newest conclusions and theories of modern biology, and thrusts them with a fierceness that makes the index as combative as the text, against freewill and dogma—by dogma apparently meaning ecclesiastical Christianity. But it is only fair to say that although his acceptance of scientific authorities is from the point of view of science absolutely uncritical, he states the views he has selected with an acumen that his discursive and flamboyant style cannot disguise completely. A considerable part of the book is given to accounts of controversies in which the author has been engaged, and hell-fire, plenary inspiration, and the immorality of the clergy reappear like King Charles' head. He endeavours to show that recent investigations have established the dependence of man's physical qualities on physical structure, and he accepts Weismann's view that acquired characters are not inherited. From these premises he draws sociological conclusions that made a writer in the *National Reformer* (to the pages of which Mr. Hiller was an esteemed contributor) accuse him of Toryism. But his conclusions do not always justify such a use of that appellation. They are such as the following:—That however society may attempt to equalise men, nature will insist on producing great inequalities. That education, as its effects are not transmitted, will not directly ameliorate society by raising the general standard. That criminals are no more worthy of punishment than geniuses of reward. That while for the benefit of individuals training of individual qualities is necessary, for the benefit of the race selection of the naturally better endowed is necessary. That the mainspring of all action is selfishness, but in practice the selfishness of the individual is restrained by the selfishness of the community.

P. C. M.

Heat: an Elementary Text-Book, Theoretical and Practical, for Colleges and Schools. By R. T. Glazebrook, M.A., F.R.S. (Cambridge: University Press, 1894.)

A FEW months ago it was announced that the Cambridge University Press intended to publish a series of science manuals, and since that time we have looked forward with pleasurable anticipation to the appearance of the works in the series. But expectations are rarely realised. The book before us is the first of the volumes devoted to physical science, and we are not strikingly impressed with it. Some books favourably force themselves upon one's notice by their originality of treatment or lucidity of expression, but Mr. Glazebrook's volume possesses neither of these characteristics to a noticeable degree. This is said at the risk of being considered hypercritical; but there are so very many ordinary books in existence, that we almost expect a new work to be different from its predecessors in order to justify its publication at all. However, though the book before us is not the best elementary class-book on heat, it is very good. The author has not confined himself to the experimental or to the theoretical side of his subject, but has happily combined the two, so that the book suits both the lecture-room and the physical laboratory. Another commendable feature is the statements of "sources of error" after the descriptions of some of the experiments. The illustrations are line-drawings, and though somewhat coarse, they possess the merit of being clear, and that is, perhaps, the chief desideratum of a book designed for use in our schools and colleges. These institutions will certainly benefit by adopting the book for their students.

Electrical Experiments. By G. E. Bonney. (London: Whittaker and Co.)

"THIS book," the author states, "is written in response to suggestions received from correspondents," and is intended to show how "induction coils and other electrical apparatus" may be used for instructive amusement.

In the two hundred and fifty pages to which the book extends, the writer describes in some detail a number of well known electrical experiments. The experiments described appear to be well chosen, and the instructions given for performing them are fairly accurate, but the theoretical explanations are, in most cases, entirely wrong. The claims of the book to scientific accuracy may be judged of from the following typical extracts, which convey the full meaning of the context. On p. 68 it is stated that "an electric current passing through a wire conductor develops therein a magnetic condition which exerts an influence on the air surrounding the wire, converting it into a magnetic shell," and on p. 203 we find the statement that "the quantity of electricity passing through a resistance of one ohm in one second will liberate ·000158 grain of hydrogen." Inaccuracies of this kind are far too serious to pass unnoticed, even in a book intended to provide instructive amusement, and we cannot recommend the seeker after electrical knowledge to trust to the guidance of a work in which they occur. From the publisher's point of view, however, the book is well got up, and will no doubt answer the purpose for which it was written and published.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

On M. Mercadier's Test of the Relative Validity of the Electrostatic and Electromagnetic Systems of Dimensions.

A SERIES of papers, by M. Mercadier, on the dimensions of physical quantities, has recently been appearing in the *Comptes Rendus*. They are summarised and extended in the *Journal de Physique* (July, 1893, p. 289).

In a note (p. 296) the author states that—"En 1883 nous avons montré, M. Vaschy et moi, séparément d'abord, puis en collaboration, que les deux systèmes [of the dimensions of electrical and magnetic quantities] imaginés par Maxwell étaient contradictoires et que l'un d'eux était inadmissible. Depuis, en particulier MM. Hertz en 1885 (*Wied. Ann.* t. xxiv. 1885) et Rücker (*Phil. Mag.* 5ᵉ série, t. xxvii, 1889) sans mentionner notre travail, sont arrivés aux mêmes conclusions."

In general it is, I think, wisest to leave such claims to priority alone, but as M. Mercadier's paper has appeared in two important French journals, I should like to make a few remarks on the history of the dimensional formulæ of electrostatic and electromagnetic quantities, which I do the more readily because I have no claims to priority to establish on my own behalf.

Maxwell's theory leads to the conclusion that between electrical, magnetic, and ordinary dynamical quantities there exist relations which are one less in number than the electrical and magnetic unknowns. Hence the dimensions of all the latter can be expressed in terms of length, mass, time, and of the unknown dimensions of any one of them. Maxwell gave two examples of such expressions in which electrical quantity and strength of magnetic pole are the unknowns selected (vol. ii. first ed. p. 241). It is absurd to suppose that he did not know that similar tables could be drawn up in terms of specific inductive capacity (K) and magnetic permeability (μ).

After discussing the general problem, Maxwell expressed the opinion that the "only systems of any scientific value are the electrostatic and electromagnetic systems" (*loc. cit.* p. 241), and proceeded to explain how they are obtained. Unfortunately, in order to emphasise the fact that the fundamental assumption in the electrostatic system is that the specific inductive capacity of the standard medium (air) is taken as unity, he used a notation in which K is represented as without dimensions instead as of unknown dimensions.

It must also, I think, be admitted that this notation conduced to the use of phrases which might very easily mislead. Thus (*loc. cit.* pp. 368–9) he concludes that a quantity n, which he defines as "the number of electrostatic units in one electromagnetic unit," "is a velocity." Had the symbols K and μ been retained, his argument would have led to the conclusion that $n\,[\mu^{-\frac{1}{2}}\,K^{-\frac{1}{2}}]$ is a velocity. The matter is not of the first importance, but a notation which requires the statement that the ratio of two like things "is a velocity," makes the subject unnecessarily difficult.

Any doubt to which such expressions might have given rise has, however, been completely set at rest since 1882.

A discussion, initiated by Prof. Clausius, then took place in the *Philosophical Magazine* (5th series, vols. xiii. and xiv.), in which Profs. Everett, J. J. Thomson, Lodge, and Larmor took part. It is only necessary for my present purpose to cite the fact that Prof. Lodge explicitly stated that "the number of fundamental relations must be limited by the number of fundamental experiments, viz. three—Coulomb, Coulomb, and Oersted; and the shortest way of writing the independent relations is this:—

$$[\mu e^2] \equiv [Km^2] = [ML]$$

and

$$[\mu K v^2] \equiv 1.$$

The electrostatic convention makes $[K] = 1$; the electromagnetic convention makes $[\mu] = 1$."

This paper was published in September 1882.

In January 1883 a paper by MM. Mercadier and Vaschy appeared in the *Comptes Rendus*. No reference was made by them to the discussion between English and German physicists, but up to a certain point they adopted precisely the same line of argument as that with which we were familiar in England.

Taking the formulæ

$$f = k\frac{qq'}{r^2}$$

and

$$f \propto k'\frac{ii'dsds'}{r^2}$$

they had no difficulty in showing that

$$\left[\frac{k}{k'}\right] = \left[\frac{L^2}{T^2}\right]$$

It was well known in 1883 that Maxwell's theory requires only two constants, K and μ, to define the constitution of the medium, and that

$$[k] = \left[\frac{1}{K}\right],\ [k'] = [\mu]$$

(Maxwell, vol. ii. p. 289, equation 24).

Hence from this point of view the equation

$$\left[\frac{k}{k'}\right] = \left[\frac{L^2}{T^2}\right]$$

is the same as

$$[\mu K v^2] = 1.$$

Up to this point, therefore, there was nothing in the paper of MM. Mercadier and Vaschy which could not be directly deduced by Maxwell's theory from the explicit statements of Lodge.

After this they proceeded to develop the subject further in an argument which may be summarised as follows:—

The constant k is inversely proportional to the specific inductive capacity.

Specific inductive capacity is proportional to the square of the index of refraction.

The index of refraction is inversely proportional to the velocity of light in the medium.

Hence $k = \alpha V^2$, "α etait une constant *numerique*" (the italics are in the original).

Hence, since

$$\left[\frac{k}{k'}\right] = [V^2] \text{ and } [k] = [V^2],$$

k' is a number "et l'emploi du système électromagnetique d'unité électriques se trouverait justifié *théoriquement*."

The fallacy in this is obvious even if the experimental justification of the step $k' \propto$ (refractive index)^2 be admitted.

Because $k \propto V^2$ it does not follow that $[k] = [V^2]$ unless we are sure that all the physical conditions have been included in the equation. Yet Maxwell had given the strongest reason to believe that the magnetic permeability was also involved. He had distinctly pointed out that K would only vary as the square of the refractive index if μ were constant (vol. ii. p. 388). In other words, M. Mercadier, arguing from experiments on materials whose magnetic permeabilities differ but little from that of air, treats $[k] = [V^2]$ or $[K] = [V^{-2}]$ as an independent equation. He thus *assumes* that k' or μ is of no dimensions, and then proves the truth of the electromagnetic system which is avowedly based on that assumption. It would be difficult to find a more complete instance of arguing in a circle.

The point, however, is elaborated by experiment and further argument in another paper (*C. R.* t. xcvi. 1883, p. 250).

The conclusion that k' is a constant is supported by the fact that an induced current is *cæteris paribus* the same, whether the currents are or are not surrounded by non-magnetic materials such as alcohol and benzene.

Again the statement is made—ten years after the publication of Maxwell's book—"D'après les idées universellement admises, les coefficients des formules de magnétisme et d'électromagnétisme seront analogues à k' par consequent ils devraient être comme lui independants des milieux" (p. 252).

The fact is that, according to Maxwell's theory, one of these coefficients is independent of the medium, while the other two vary, the one inversely as the other, when the medium is changed.

Quite apart then from the question as to whether these factors represented pure numbers or concrete quantities, it was at that date almost universally believed that the values of two of them depended on the medium. Of course the same view is even more universally held to-day.

All this might, however, have been passed over as an "indiscrétion de jeunesse" if M. Mercadier had not in June last made the extraordinary claim to have proved on such a basis of argument and experiment that the electromagnetic system of units has a theoretical justification which the electrostatic system lacks.

In this recent paper the notation is changed, and k' is used for $1/\mu$. Here again the invariability of this quantity in non-magnetic materials is used as an argument to prove that it does not depend on the nature of the medium.

For the rest M. Mercadier develops certain mixed systems of dimensions, which I need not discuss.

In answer to his complaint that I omitted to notice his memoir in a paper which I wrote on the same subject in 1889, I wish to point out that I did not then enter upon the bibliography of the subject. I regarded myself as dealing with a theory well understood by experts, and as advocating a change in notation chiefly for the benefit of less advanced teachers and students. The considerations advanced were direct deductions from Maxwell's theory. That theory was more generally understood in 1889 than when the discussion in the *Philosophical Magazine* took place in 1882, and since the latter date the practice of retaining K and μ in dimensional formulæ is spreading.

As far, however, as M. Mercadier's papers of 1883 were correct, the ideas they embodied had been explicitly stated in the *Philosophical Magazine* some months before. As far as they went beyond that point, by the attempt to discriminate between the theoretical validity of the electrostatic and electromagnetic systems, the arguments adduced were quite unsound.

ARTHUR W. RÜCKER.

Royal College of Science, South Kensington, February 5.

The Cloudy Condensation of Steam.

MR. AITKEN'S letter (p. 340) shows that he has curiously misunderstood me. I never entertained the smallest "objection to" his "not countenancing the nucleus theory to explain" the action of electricity upon the steam jet. On the contrary I was rejoiced to find that so able and distinguished a physicist appeared to hold the same opinion on this point as myself. In labouring to abbreviate I must have become very obscure. Perhaps my meaning may be made clearer by an amplified and annotated paraphrase of the words in question (see *ante* p. 213).

After trying to show that dense condensation takes place only when there is an actual *discharge* of electricity, which, however, need not necessarily electrify the jet, I go on: "The inference clearly is that in some way or other the action is brought about by the *air* in which electrical discharge has taken place, and not directly by the electricity itself. Since so much has been said in the earlier part of the lecture about the influence of dust in promoting condensation the [erroneous] idea has, no doubt, occurred to many of you that in the present case also the air owes its condensing power to the fact that it has become charged with dust. [The great majority of the many scientifically educated people to whom I have at different times shown the experiment at once made this suggestion.] Minute particles are indeed torn off the electrodes by the discharge and [you may think] form nuclei upon which the steam condenses. This [mistaken] hypothesis seems at first sight to be favoured by the experiments of Liveing and Dewar, and by the well-known fact that burning touchpaper induces condensation; it also has the support of Prof. Barus, who appears inclined to think that such condensation is *in all cases* due to the action of small particles of matter. On the other hand, it is noteworthy that Mr. Aitken, who knows more about the condensing property of dust than any man living, gives no countenance to the nucleus theory as explaining the action of electrical discharge upon the steam jet. The possibility of such an explanation must necessarily have presented itself to the mind of one so familiar with the subject, and since he does not make the slightest allusion to it, I imagine that his experiments have led him to the conclusion that it is untenable. This affords me great satisfaction, inasmuch as my own experiments have led me to the same conclusion—not only as regards the action upon the steam jet of electrical discharge, but also of burning matter." [I did not intend to imply, though the words of the abstract apart from the context unfortunately seem to bear that meaning, that Mr. Aitken thought the action of *burning matter* was not due to nuclei, but that I myself thought it was not.] Then follows an account of experiments tending to show that the air does not derive its power of condensing the steam jet from dust but from dissociated atoms.

The above will, I hope, convince Mr. Aitken that, except perhaps as regards one slipshod sentence, which I regret having overlooked when correcting the proof, he has no cause to feel aggrieved. I am confident that my hearers never for a moment understood me to say that he had abandoned one iota of his conclusions regarding the action of dust, but merely that he did not consider the dust-nucleus theory applicable to the case of the electrified steam jet.

I believe that I am well acquainted with all Mr. Aitken's papers on the subject of condensation, but I do not remember the experiment with the polished ball referred to in his letter. Perhaps it is an unpublished one. The experiments which he mentions in his final paragraph, relating to the condensation caused by certain acids, were made upon water-laden air contained in closed vessels, and not upon the steam jet. The conditions in the two cases are very different, so much so that, for example, hydrochloric acid, which in the steam jet is the most active source of dense condensation that I have met with, was found by Mr. Aitken (he will pardon me for reminding him) to form no foggy condensation at all in a receiver of moist filtered air; while ordinary dusty air, which exerts such a powerful action in the closed vessel, fails to produce any sensible effect when introduced into the open steam jet.

SHELFORD BIDWELL.

Southfields, Wandsworth, February 11.

On the Cardinal Points of the Tusayan Villagers.

IN the second volume of the *Journal of American Ethnology and Archæology* I have pointed out, for the first time, that the four cardinal points among the Tusayan villagers are not the same as those of the astronomers, or that their north is approximately north-west. I also gave, in the same article, tables with the amount of the angular variations, showing that the sacred rooms, or kivas, where the mysteries of their ceremonial worship are performed, are oriented, roughly speaking, in accordance with their conception of the positions of north, west, south and east. It was shown that the amount of angular variation was constant, and later, in a description of the ruins of A-na-to-bi, the same orientation was made known.

In an article published in the December number of the *Journal of American Folk Lore*, it was stated by me that the cardinal points among these aborigines are determined by the solstitial risings and settings of the sun.

The publication of Prof. J. Norman Lockyer's work on "The Dawn of Astronomy," in which the orientation of certain of the sun-temples in the Nile valley and elsewhere in the old world is referred to solstitial points in the horizon, gives a new interest to these observations among the aboriginal housebuilders and their descendants in America.

Since the publication (1892) of my observations on the orientation of Tusayan (Moki) kivas and its relationship to solstitial points of sunrise and sunset, I have examined the scanty data which we have regarding the orientation of temples in Central American ruins, and have unearthed significant facts bearing on this question, as well as that of the kinship of the Pueblo people and those who once inhabited the "cities" of Mexico, including Yucatan. Evidences of relationship between the aboriginal housebuilders of Arizona and New Mexico, and those of Nahuatl and Maya stocks have elsewhere been presented. It seems to me that the above observations made in 1891, quite independently of the discoveries of Lockyer on the orientation of temples in the old world, in the light of his discussion, open a field of research in the archæology of the housebuilders of Central America which is sure to lead to interesting discoveries.

J. WALTER FEWKES.

Boston, Mass., U.S.A.

The Scandinavian Ice-sheet.

MANY geologists affirm that the Scandinavian ice-sheet became confluent with that of Scotland, and reached the East

Anglian coasts. Perhaps some of your readers could inform me whether the following difficulty, which has occurred to me, has been already raised, or has received a satisfactory answer. A submarine channel, some 400 fathoms deep, sweeps round the southern coast of Norway from the Cattegat to about the 62nd parallel of latitude, whence it gradually opens out into the deeper water further north. If the 100 fathom-line of soundings were to become the coast margin of north-western Europe, this channel would form a fjord, considerably broader than the straits of Dover, and for the most part 1800 feet deep. A further general upheaval, amounting in all to some 2500 feet, would convert this fjord into a wide valley, sloping gently towards the north, which was bounded on one side by the Scandinavian mountains (then commonly rising to a height of about 5000 to 9000 feet); on the other by a nearly level plateau (with a yet slighter slope, but in the main northward), elevated generally some 2000 feet above the bed of the valley. In such cases, if any trust can be placed on the evidence afforded by Greenland at the present day, the drainage of Scandinavia would obey the law of gravitation, even when in the form of ice, and would be diverted down the fjord or valley towards the northern Atlantic.

T. G. BONNEY.

The Nomenclature of Radiant Energy.

REFERRING to Prof. Simon Newcomb's letter in your issue of November 30 last (p. 100), suggesting a nomenclature for radiant energy—if no one else has already pointed it out, I would suggest that the word *irradiate* might be used in place of *illuminate*. It would be just as expressive, and would have the advantage of consistency; and its use would leave the word "illuminate" to its proper sphere.

A. N. PEARSON.

Melbourne, January 9.

THE FOUNDATIONS OF DYNAMICS.

IT is rather curious that at the present time, when applied dynamics embraces so wide a range, so much attention should be directed to its foundations. One would have thought that the basis of a department of science which is used and used successfully in the investigation of the motion of vortex rings in a fluid, and the propagation of waves of electromagnetic disturbance, had been fully understood, and that no doubt of the firmness of the logical structure on which so huge a weight is laid, was entertained by those who are most active in turning it to practical account. If, as some appear to believe, our dynamical methods are founded on a vicious circle, how is it that the same men have been so successful in applying them to the elucidation of physical phenomena? Surely the repeated attempt to do this ought only to have led, if not to confusion of contradictory results, to continual failure to obtain any explanation at all.

On the other hand the extended use of dynamics has led scientific men themselves to a more general familiarity with dynamical processes. The study of dynamics is now a recognised part of scientific education, and the exigencies of teaching the subject have rendered necessary a much more complete examination of its fundamental assumptions than was usual before, when a few gifted mathematicians, by the force of their own genius, were led, almost "by a way they knew not," to the glorious results of physical astronomy. Again the recognition, more or less clear, that the old action-at-a-distance theories are really mathematical shortcuts, each gathering up into a single formula the result of the physical actions on molar matter of a medium in which it is immersed, has directed attention to the ether, and raised many questions of extreme interest as to the localisation of energy, and the conditions of its transference from place to place. Though a whole race of subtleties has with the new views sprung into being to mock our attempts to find firm footing, we are forced to the conviction that in this action of a medium lies the best means of scientific progress at the present time. As a consequence we are led to the reconsideration of the theory of energy, and therefore also of the conceptions of force, &c., and discussions as to the foundations of dynamics have been revived and carried on with a keener interest.

No one has worked with more zeal at the task of restating the doctrine of energy on anti-action-at-a-distance principles than Dr. Oliver Lodge, and it happens that recently his views have again been brought to the front by an address on the Fundamental Hypotheses of Dynamics delivered in 1892 by Prof. J. G. MacGregor before the Royal Society of Canada, and an article by the same author in the *Philosophical Magazine* for February 1893. An instructive paper has been presented by Dr. Lodge to the Physical Society, in which he has re-stated and defended his position. The discussion which took place on that paper, and the divergence of opinion then manifested, showed how wide is the interest in this subject, and how far it is still from being completely settled.[1]

The chief points in Dr. Lodge's papers are his insistence upon contact action as the cause of all action between bodies, and his re-statement of the principle of the conservation of energy. Only incidentally and as a preliminary, in his last paper at least, are the laws of motion touched upon. On the other hand, the chief burden of Dr. MacGregor's address is the laws of motion, and an attempt so to formulate them so as to give a logical basis for the science of dynamics in its application to physics. In his *Phil. Mag.* paper, however, he deals with Dr. Lodge's views with respect to energy.

I do not propose to restate the positions of the parties to the present controversy, but to endeavour to say how the question appears to an outsider who has felt keenly the difficulty of teaching the elementary principles of dynamics without introducing confusion by unnecessarily obtruding the fundamental *cruces* of the subject; or, on the other hand, slurring over matters of really vital importance.

In the first place, it seems to me that there is in general no sufficiently clear recognition of the fact that abstract dynamics is really abstract, and depends upon certain ideal conceptions just as much as does geometry, and that its application to practical problems must be made on certain assumptions, axiomatic in the proper sense or not, which must be justified by the results of experience. Abstract dynamics is a purely ideal science, geometric in a somewhat extended sense, caused by the introduction of certain notions not ordinarily employed in purely geometrical processes. So long as we confine ourselves to the ideal as we do in geometry, there are about it only difficulties of the same kind as we have in geometrical conceptions, and these I do not here propose to discuss. It is only when we apply the science to the interpretation of nature that we meet with the difficulties that every one must admit do exist, and which there is no blinking if we want to be straightforward, as to absolute direction, uniform motion, &c.

In this application we take some standard for the measurement of time. In this we are guided by the idea derived from the first law of motion, that any body in relative motion, which there is reason to conclude is not changed by the action of other bodies, may be taken as timekeeper. In practice we have recourse to a joint result of this idea and the equality of action and reaction, and take as our standard the rotation of the earth on its axis. [Of course this standard may not agree with some other and preferable standard means of time reckoning, but this will not affect the argument.]

In abstract dynamics we can and do imagine a system of axes of reference of some kind or other, but quite ideal so far, and agree upon or assume the existence of some mode of measuring intervals of time. We then consider the velocities and accelerations of different particles rela-

1 A rejoinder to this paper appeared in the September number of the *Philosophical Magazine*.

tively to those axes. We suppose different particles to have any accelerations relative to those axes which may be assigned, or which are deducible from data given, and so from the configuration at any given epoch that at any other, that is, to speak shortly, the motion, can be found. If the particles do not change their configuration relatively to one another a limitation is imposed on the motion, the particles constitute a rigid body. Thus we may consider any conceivable cases, and the science which deals with them is one of pure kinematics.

Now we may suppose our reference system, which we may call A, to have a motion relatively to some other reference system B, and the motion of the particles considered if referred to that other system will be compounded, for any instant, of the motion which the particles would have with respect to B, if they were rigidly connected with A, *in the positions they have at that instant*, and of the motions which the particles then have with respect to A. There is no difficulty, if the motion of A with respect to B is specified, in determining the former part of the motion of each particle. It will vary, of course, with the changing positions of the particles in consequence of their motions with respect to A.

Similarly we can push the reference still further back, and so from reference system to reference system whenever we find it desirable to do so. Of course we should never by any such process as this reach axes absolutely fixed; but it is the process by which we introduce corrections suggested by experience, as explained below.

It is, then, a result of observation that we can stop at some reference system, it may be the first A, which is suggested to us by the circumstances of the case. To a certain extent we can consider the effect of referring our chosen reference system to other reference systems naturally suggested, and be sure that the additional motions necessary for the parts of our system are negligible.

In practice we generally make the supposition that we may refer to a naturally suggested system of reference and find in what manner the results deduced require correction. For example, we refer the motion of a projectile to axes fixed in the earth, say one vertically upwards, and two others, one north the other west, and consider the motion. We find that the results only approximately coincide with experience, and we have to correct them on account of the earth's rotation. It may be that there are other corrections which on account of their smallness relatively to unavoidable errors of observation we can take no account of.

So far we have made no mention of mass or inertia. This idea is derived from experience of physical phenomena.

If we wish to apply our ideal science to the investigation of physical relations from experimental or observational data, we can only do so on certain assumptions tacitly or explicitly made, and these are to be regarded as postulates to be justified by the consistency and accuracy of our results when tested in their turn by observation. The term axiom, it may be remarked, seems inapplicable to many of these unproved assumptions, inasmuch as though they are simple concise statements, neither their truth nor their falsehood commends itself at once to the mind.

Now, with reference to our naturally chosen system of axes, we find that different bodies have, *in the same circumstances*, different accelerations, and hence we get the idea of the masses of bodies. In estimating similarity of circumstances we assume the constancy of the physical properties of materials, such as constancy of the quantity of matter in a body, the elastic properties of a spring, and the like. Thus, if we take a given spiral spring and apply it repeatedly to the same body with the same stretch, we find the same acceleration given to the body each time. Of course this result might be produced by a *pari passu* variation of the mass of the body, and the properties of the spring, but since we find the results consistent with those obtained with different masses and springs, the possibility of such variations need not be discussed. To this ideal method of comparing masses, the ordinary method by weighing is shown to be equivalent by Galileo's experiment with the falling bodies, Newton's pendulum experiment, &c.

Thus applying *similar circumstances* (which we may typify by a spring with a given stretch) to different bodies, we find their accelerations different, and we are led to a comparison of their masses, and thence to a prediction of the accelerations which in different circumstances will be produced in the same mass or in different masses, that is to the comparison of rates of change of momentum or of force. For example, suppose a spring with a given stretch in it to be applied for a second to each of a number of masses, and let the accelerations produced be a_1, a_2, a_3, &c. Then if we take quantities inversely proportional to a_1, a_2, a_3, &c., say μ/a_1, μ/a_2, μ/a_3, &c., and multiply each of these by the accelerations produced, we obtain, of course, the same product μ in each case, and we take this as a measure of the stress in the spring regarded as the producer of motion in bodies. In the ordinary system of measuring forces we take μ as ma, where m is the mass of the body reckoned in terms of a chosen unit of mass. This gives the dynamical method of comparing the masses of bodies. The masses of the bodies here considered are μ/a_1, μ/a_2, &c.

On the other hand, when we have to compare the motion-producing powers of springs having different stretches, that is, the forces they exert, we may use the same system of bodies if we please (or any system of which the masses have been compared as just described), and suppose that accelerations a'_1, a'_2, a'_3, &c. are produced by different springs applied to the bodies. Thus applying the method of reckoning explained above, we are led to measure the forces exerted by the springs by the products $\mu a'_1/a_1$, $\mu a'_2/a_2$, &c.

Thus from the point of view here adopted, Newton's second law sets up this mode of comparing masses and forces, and thereby furnishes a perfectly simple and consistent method of writing in a form ready for solution the equations of motion of a body relatively to any system of axes which we know from experience we may regard as at rest.

Here I wish to remark that when we write such equations as

$$m\ddot{x} = X, \; m\ddot{y} = Y, \; m\ddot{z} = Z,$$

the quantities on the right, commonly called the applied forces on the particle of mass m, are, it seems to me, merely put provisionally for values of the quantities on the left, which from the given circumstances of the motion, that is from the relations and data given, we may be able to calculate, or to supply from the results of experiment or observation. There is not any necessity for considering them as the *causes* or the measures of the causes of the accelerations $\ddot{x}$, $\ddot{y}$, $\ddot{z}$, of the particle.

The idea of force as cause of acceleration is useful as enabling us to speak and write with brevity about dynamical problems, and so to arrive quickly at the necessary equations. For example, take the problem of the motion of a particle of mass m hung by a massless spiral spring which the weight of the particle stretches by a length s. Then we know (1) that the stretch of the spring if not counteracted by the weight mg of the particle would cause the particle to receive an upward acceleration g, and since experiment shows that different weights stretch the spring by amounts proportional to them, we infer (2) that when the spring is stretched by an amount $s + x$, the elastic reactiom would produce an acceleration $g(s + x)/s$. Hence an upward acceleration of amount

gx/s will be produced, and if $\ddot{x}$ represent downward acceleration, we get the equation of motion :—

$$m\ddot{x} = - mg\frac{x}{s}.$$

which is ready for solution, and gives the well-known result.

We greatly abbreviate the above statements by saying that the upward "force" exerted by the spring in the first case is mg, and in the second, from the experimental result, $mg(s + x)/s$. This gives at once $-mgx/s$ as the *downward* force on the particle, which being substituted for X in the formal equation of motion, $m\ddot{x} = X$, puts the latter into a form adapted for solution.

Thus, though we may use, and do use constantly, the language of cause and effect in this connection, it ought to be remembered that when matters have been reduced to the solution of a dynamical problem, we have a purely mathematical process to carry out, by which we render explicit only that which is already implicitly involved in our equations.

This does not exclude or do away with the consideration of stresses as physical realities, it only states what I believe is substantially involved in the application of dynamics to physical problems. The objectivity, in the metaphysical sense, of force does not concern us, and discussions regarding it are, so far at least as physical results are concerned, not likely to be profitable.

I have heard it said by more than one very competent judge, that there is a certain vicious circle at the foundation of dynamics which there is no avoiding. We define force by mass, and mass by force. Thus it is sometimes said in effect, "Equal forces are those which produce equal accelerations in equal masses—equal masses those in which equal accelerations are produced by equal forces." But, as shown above, if we can assume constancy of mass of a body, and of the physical properties—say of a spiral spring—there is no difficulty in getting out of this circle of definition. These are assumptions we are entitled to make as the result of experience.

It is to be observed that since the measure of force in Newton's second law, namely, $m\ddot{x}$, is relative, t[illegible] forces considered must be also relative. This is n[illegible] ced by Prof. MacGregor in his address (p. 4), but he states that as our idea of force is derived from sensation, force in this sense is not relative. "According to this conception a body either is, or is not, acted upon by force." It is possible that I have failed to follow Dr. MacGregor here, but it seems to me that he has confounded *real* with *absolute*. Our muscular sense certainly tells us that a force, that is a stress as distinguished from a mass-acceleration, exists, but in no case can it inform us as to what in any absolute sense are the forces acting on the body considered. The force we feel "does not depend upon our point of view," but the force we regard as acting on the body certainly does. An acceleration which we observe is also a perfectly real thing in itself, but the acceleration of the particle is altogether dependent for its value on the point of view from which we regard it.

The ordinary misunderstanding that continually crop up with respect to the equality of action and reaction is feelingly alluded to by Dr. Lodge in his paper, and perhaps as a sympathiser I may be pardoned for devoting a paragraph or two to its consideration. A recent discussion of precisely the same thing in another journal has made it clear that the difficulty felt by the beginner in this matter is not clearly appreciated by many who endeavour to remove it. Because action and reaction are equal and opposite in the case (to take Newton's illustration) of a horse pulling a stone, the student (and the would-be critic of dynamical processes!) imagines that neither the horse nor the stone can get into motion. Now the confusion arises from regarding the action which is a forward force on the stone as being cancelled by the (if for a moment we neglect the mass of the rope or chain between the two bodies) equal and opposite force which acts, and this is what is overlooked, *not upon the stone*, *but upon the horse*, and therefore cannot affect the motion of the stone.

There may be other forces acting on the stone, and others again acting on the horse, and the motion of each body is changed *by the forces acting on that body*, *and those forces alone*. Thus there are two groups of forces, one group acting on the stone, and the other on the horse, and all that is asserted in the law of equality of action and reaction, as applied in this illustration, is that that particular force of the first group, which is the force exerted on the stone by the horse, is equal to that force of the second group which is the force exerted on the horse by the stone.

Action and reaction, however, are, I believe, most properly regarded as applied at the same place, though not to the same thing. Across any cross-section of the rope in Newton's illustration a stress acts, one aspect of which is a forward force on the part of the cord immediately behind the cross-section, the other a backward force on the part of the cord just in front of the cross-section. An excellent example is the action and reaction between two links of a chain, which are exerted across the surface of contact between the links, the action being a force on one link, the reaction a force on the other link. Here, as in all other cases, the action and reaction do not cancel one another, simply because they are applied to what are here regarded as entirely different things. [Of course, if we are considering the motion which a system consisting of different parts may have *as a whole*, the actions and reactions between these parts do cancel one another.]

I agree with Dr. Lodge in believing that in a certain sense we have nothing but contact action, that is, that all radiation phenomena are propagated by contact between portions of matter (not necessarily ultimately discrete portions) filling space. Thus at every place where such propagation is going on, and consequently changes of the motions of bodies are taking place, stresses are set up, and just where we have one aspect of a stress we have its other aspect.

This view, if it is adopted, certainly seems to lead to the conclusion that a process of transformation accompanies transference of energy; but it is not, so far as I can see, inconsistent with, and does not render in any way untenable, the doctrine of conservation of energy as ordinarily stated.

The doctrine that all energy is kinetic in reality, and that transformation consists in a passage of the energy from being kinetic energy of the bodies whose velocities can be observed and measured to being kinetic energy of those parts of the system regarding which we cannot have such knowledge, or *vice versâ*, when it is more familiar, and more clearly understood in the light of further scientific progress, may possibly help to clear away some of the many difficulties which crowd round this subject.

This article is long enough, and we must defer to some other opportunity any further consideration of Dr. Lodge's theory of the transference of energy. But both he and Dr. MacGregor have done good service in discussing from their several points of view this very difficult but apparently for many minds exceedingly fascinating subject. Nothing but good can come of "a revision of the standards" in dynamics, provided it has no destructive object in view, but only the improvement and, if necessary, correction of the methods of presenting and teaching the science.

A. GRAY.

AN INCIDENT IN THE CHOLERA EPIDEMIC AT ALTONA.

THE third contribution by Dr. Koch last year to the subject of cholera appears in the *Zeitschrift für Hygiene*, vol. xv. part I. It covers no less than seventy-six pages, and is entitled "Die Cholera in Deutschland während des Winters 1892 bis 1893." As the title implies, it is an elaborate essay giving a most lucid and remarkably interesting exposition of the rise and course pursued by the several epidemics of cholera which visited Hamburg, Altona, and Nietleben near Halle, respectively. Several figures serve to illustrate the descriptions of sites, buildings, &c., referred to in the text.

From a bacteriological point of view, perhaps the most interesting part of the paper is that which relates to the disease in Altona, and in which an account is given of the successful elucidation of a remarkable outburst of cholera which occurred in a restricted area of that town, and which in many respects recalls the incidents of the now classical cholera explosion which took place in 1854 in connection with the Broad-street pump in London.

In a district of Altona, rejoicing in the suggestive name of "der lange Jammer," and inhabited by about 270 persons, cholera made its appearance on January 21, 1893, and in a week nine cases had occurred, of which seven ended fatally. Strange to say, in the neighbourhood and, indeed, for some distance around this centre, no other cases of cholera were recorded at all, thus pointing very clearly to some local cause as responsible for the outbreak. A searching investigation was at once instituted, resulting in the discovery that the infected houses were not connected with the Altona water-supply, but dependent for their water upon a well in their midst. The ordinary town water-supply was in fact regarded as an article of luxury and an extravagance which the humble inhabitants of "der lange Jammer" were too poor to indulge in. In May, 1892, a systematic investigation had, it appears, been made of all the wells in Altona, and ninety-two out of 366 had been condemned as unfit for use. This particular well was, however, amongst those which had been passed, as its construction appeared to be satisfactory, and its surroundings sufficiently protected to remove all fear of contamination. During the severe frost, however, there can be no doubt that surface water, unable to get away by the usual channels, gained access to the well, for when the courts of the surrounding houses were washed down with strong carbolic it was noticed that the well-water acquired a smell of this material. Thus the possibility of its contamination with choleraic matters was established, and on January 26 the well was closed. After this date only four more cases of cholera occurred, the last one recorded being on February 1, and all of these might have been contracted prior to the closing of the well, and are therefore still attributable to the use of this water.

The bacteriological examination of the water was taken in hand on January 31, and on this day large numbers of cholera bacilli were revealed by the usual special methods employed. A sample of the water collected on January 31 was preserved for further investigation, and was kept in a room having a temperature of 3—5° C.: in this sample cholera bacilli were found on the 2nd, 3rd, and 17th February respectively, showing that under the particular circumstances the bacilli were able to maintain their vitality for eighteen days in the water; *on the other hand, in samples of water collected later directly from the well itself no cholera bacteria could be detected.* It is to be presumed, therefore, that as no further cases of cholera occurred in the adjacent houses after February 1, no fresh bacilli found their way into the well, and those cholera bacilli which were proved to be present on January 31, must have either become altogether extinct or have been so much reduced in number as to defy detection.

The incident is instructive, if only in demonstrating the folly of presuming that a well with flagrantly unsanitary environment may be regarded as safe for drinking purposes, just because its past history happens to be untarnished by any observed connection with an outbreak of zymotic disease. But another point which I consider is very clearly brought out by the case in question, is the uncertainty which attaches to the actual discovery of the cholera or, indeed, of other pathogenic bacteria in water, even under such peculiarly favourable conditions as were present in the case of the Altona well. Had the examination of this water been delayed only for a few days, the search for cholera bacilli would have been absolutely fruitless, and the direct bacteriological evidence entirely wanting. Chance, in this particular instance, decided otherwise, and a very satisfactory confirmation of a most probable hypothesis was obtained.

Nevertheless, it is very apparent that however important bacteriological evidence may be in determining the hygienic value of water purification processes, and as I have so often pointed out, it is in this matter the only competent referee; on the other hand, in the matter of the actual detection of disease organisms in any given water, its usefulness is of a much more restricted character.

There is undoubtedly a tendency at the present time to regard the detection of pathogenic bacteria as the most important object of bacteriological water examination. It is, however, surely a matter of far greater moment to anticipate and be forearmed against evil by ascertaining whether the principal conditions, such as purity of source, efficiency of subsidence, filtration, &c. attaching to a given water-supply are such as to reduce to a minimum the danger of its disseminating zymotic disease, than to wait for the actual discovery of pathogenic bacteria, and only then to be led to see the necessity of, as it were, locking the stable-door after the horse has been stolen!

The failure to discover the typhoid bacillus in the Worthing water-supply is another instance in point, and in the majority of cases the task of tracing the connection between an outbreak of disease and an infected water-supply must obviously still be performed without the direct support of the bacteriological detection of the zymotic poison. PERCY FRANKLAND.

NOTES.

THE foundation of the Bakerian Lecture, to be delivered to-day at the Royal Society by Prof. Thorpe, F.R.S., and Mr. J. W. Rodger, although not so ancient as that of the Croonian, is yet of respectable antiquity. Established during the presidency of Sir John Pringle, the predecessor of Sir Joseph Banks, it has its origin in the bequest, in 1774, by Henry Baker, antiquary, naturalist, and Fellow of the Society, of the sum of one hundred pounds, the interest of which is directed to be applied for an oration, or discourse, to be spoken or read yearly by a Fellow on some subject in natural history or experimental philosophy. The forfeiture of the bequest is contingent on the lecture failing to be delivered in any one year. The founder of this lecture was himself a man of considerable parts, and, besides being the author of numerous memoirs in the *Philosophical Transactions* published two treatises on the microscope, and some poetical works. He was elected into the Royal Society in 1740, and in 1744 was awarded the Copley medal. He married the youngest daughter of Daniel De Foe. The first lecture under the bequest was given in 1775 by Mr. Peter Woulfe, the subject being "Experiments made in order to ascertain the nature of some mineral substances, and in particular to see how far the acids of sea-salt and of vitriol contribute to mineralise metallic and other substances."

IT is now arranged that the Croonian Lecture of the Royal Society will be delivered by Prof. Ramon y Cajal, on Thursday, March 8; not March 1, as announced in our issue of December 21.

WE understand that the U.S. Bureau of Weights and Measures has recently decided to use the metre and kilogram as fundamental standards, and, from the fifth day of next April, to consider the yard and pound as derivatives from the metrical standards. This decision practically means the adoption of the metrical system by the United States.

IT has been decided to hold the autumn meeting of the Iron and Steel Institute at Brussels, from September 2 to 7.

M. L. GUIGNARD has been elected president of the Botanical Society of France for the present year.

M. AIMÉ GIRARD has been elected a member of the Rural Economy section of the Paris Academy of Sciences, in succession to the late M. Chambrelent.

M. ALBOFF, who has been collecting for the past six months in the Caucasian Alps, for the Boissier Herbarium, has returned with large collections.

A BOTANICAL garden has been established in the mountains near Grenoble, at an altitude of 1875 m., under the direction of Prof. P. Lachmann.

DR. E. BARONI, of Florence, is preparing a monograph of the genus *Atriplex*, and would be obliged by specimens or memoirs from any botanists who have worked at the genus.

THE *Journal of St. Petersburg* states that the Russian Technical Society has decided on the organisation at St. Petersburg of an exhibition of gold ores and of precious metals and stones.

THE Council of the Sanitary Institute have accepted an invitation, received from the Lord Mayor and citizens of Liverpool, to hold their next congress and exhibition in that city in the autumn of this year.

MR. WILLIAM GARTON, of Woolston, Southampton, has presented a sum of five hundred pounds to the Council of the Hartley Institution towards the cost of the new engineering laboratory, which is about to be added to that institution.

THE fine engineering laboratory belonging to the Purdue University, Lafayette, Indiana, and which has cost some £35,000 to build and equip, has been completely destroyed by fire. The building was only completed on January 19 last, and was burnt four days afterwards.

WE learn from the *North British Agriculturist* that the Lancashire County Council have decided to take over a farm at Penwortham, at an annual rental of £400, on a lease terminable at five, ten, or fifteen years, for the purposes of agricultural experiment and instruction.

SIR H. TRUEMAN WOOD has been elected president of the Photographic Society of Great Britain.

THE 1894 Camera Club Photographic Conference will be held in the theatre of the Society of Arts, on Monday and Tuesday, April 23 and 24, under the presidency of Capt. W. de W. Abney. The members' annual exhibition of photographs will be commenced at the club on the first day of the conference.

ACCORDING to the *British Medical Journal*, the Hungarian Government has established a bacteriological institute at Buda-Pesth for the purpose of giving facilities for the study of infectious diseases from the scientific point of view; for the employment of bacteriological methods for the combating of such diseases; for general bacteriological researches; and for supplying information on bacteriological questions to public authorities and private inquirers.

AN interesting experiment, that of the cultivation of tea, is shortly to be tried in Russia (says the *Board of Trade Journal*). The Czar, under the guidance of experts, has given his consent to a proposal for the cultivation of this plant in the western limits of the Caucasus, where the temperature is much the same as that under which the plant grows in China.

THE death is announced of Prof. E. Weyr, at the age of forty-six. He was known especially for his contributions to modern geometry.

THE *Athenæum* announces the death of Prof. J. von Dümichen, the Egyptologist, at Strasburg, on February 7. He was born in 1833 at Weissholz, in Silesia, and pursued his Egyptological studies under Lepsius and Brugsch. In 1862 he made his first journey into Egypt, Nubia, and the Soudan, returning in 1865. At the foundation of the German University in Alsace, Dümichen was nominated to the chair of Egyptology. In 1875–76 he spent a great time in Egypt in order to complete the researches begun during his earlier journeys. He was the author of numerous works on the geography, inscriptions, architecture, and history of ancient Egypt.

THE anniversary meeting of the Geological Society was held at Burlington House, on Friday, February 16, when the medals and funds were awarded as follows:—The Wollaston Medal to Geheimrath Professor K. A. von Zittel; the Murchison Medal to Mr. W. T. Aveline; the Lyell Medal to Prof. J. Milne, F.R.S.; the balance of the proceeds of the Wollaston Fund to Mr. A. Strahan; that of the Murchison Fund to Mr. G. Barrow; that of the Lyell Fund to Mr. W. Hill; and a portion of the proceeds of the Barlow-Jameson Fund to Mr. C. Davison. The following is a list of the officers and council elected at the meeting for the ensuing year:—President: H. Woodward, F.R.S. Vice-Presidents: Prof. A. H. Green, F.R.S., Dr. G. J. Hinde, Prof. J. W. Judd, F.R.S., R. Lydekker. Secretaries: J. E. Marr, F.R.S., J. J. H. Teall, F.R.S. Foreign Secretary: J. W. Hulke, F.R.S. Treasurer: Prof. T. Wiltshire. Council: H. Bauerman, Dr. W. T. Blanford, F.R.S., Sir John Evans, F.R.S., Prof. A. H. Green, F.R.S., Dr. J. W. Gregory, Alfred Harker, Dr. G. J. Hinde, T. V. Holmes, W. H. Hudleston, F.R.S., J. W. Hulke, F.R.S., Prof. J. W. Judd, F.R.S., Prof. C. Lapworth, F.R.S., R. Lydekker, Lieut.-General C. A. McMahon, J. E. Marr, F.R.S., H. W. Monckton, Clement Reid, F. Rutley, J. J. H. Teall, F.R.S., Prof. T. Wiltshire, Rev. H. H. Winwood, Dr. H. Woodward, F.R.S., H. B. Woodward.

ON Saturday, February 24, at four o'clock, a meeting will be held in Queen Elizabeth's Lodge, Chingford, Epping Forest, in support of a proposed Epping Forest free local museum. For many years the idea of a museum to illustrate the natural history, history, archæology, &c. of the forest has been in the minds of residents of the district, and the Queen Elizabeth's Lodge seems to be admirably suited to contain a collection of the kind indicated. The Council of the Essex Field Club have expressed their willingness to undertake the gathering together of specimens, and the curatorship and scientific superintendence of the collections, as a branch of their central museum at Chelmsford. The specimens and exhibits which it is proposed to place in the museum would include such as the following:—(*a*) Specimens of the natural history and geology of the forest district—the quadrupeds, birds, fishes, reptiles, insects, trees, wild flowers, fungi, fossils, &c. (*b*) Instructive preparations to illustrate the variety of form colour, structure, habits, transformations, and development, &c., of the above, with examples of galls and other plant disease and injuries. (*c*) The antiquities of the forest districts; illustrations of the camps, and other earthworks; prehistoric implements and other remains, &c. (*d*) Plans, maps, photographs, pictures, models, &c. relating to the district; illustrations of the history of the forest, and its scenic beauties; the architectural and archæological features of the district, &c. (*e*) A small collection of books—guides, histories, manuals of natural history, &c.—useful to those wishing to learn something about the district before taking rambles therein. A local museum of the kind proposed would be a source of interest and utility to all lovers of nature, and might be made of considerable educational value.

IN the early part of this week a very severe frost set in over the midland, eastern, and southern parts of England, accompanied by piercing easterly winds; the night minima in the shade fell to 16° at Loughborough, and to about 25° at Shields; while in London the temperature on the grass was as low as 14°, and fog occurred over the inland parts of England. These conditions were due to an area of high atmospheric pressure which lay over Denmark, the Netherlands, and south of Scandinavia, where the barometer readings were as high as 30·6 inches, with lower readings further south. But our extreme north and west coasts were under the influence of low pressure areas, and a south-westerly gale was blowing at Stornoway on Monday evening; consequently the temperature in these parts was higher.

IN Dr. Wild's *Annalen des Physikalischen Central Observatoriums* for 1892, just received, it is recorded that at Werchojansk, Lat. 67° 34′ N., Long. 133° 51′ E., the temperature fell in February to – 69°·8 C. or – 94°·6 F. This is absolutely the lowest temperature of the air hitherto observed anywhere on the surface of the earth.

IN *Ciel et Terre* of the 1st inst. M. A. Lancaster contributes an interesting paper "On the commencement and end of winter," as determined by the first and last occurrence of snow and frost at Brussels. He gives tables showing these dates for sixty-one years, from 1832–3 to 1893–4 (the data for the first and last of these years being incomplete). On an average, the first frost occurs about November 10, and the first snow about five days later, while the first frost of much intensity (below 20° F.) occurs about six weeks afterwards. At times these phenomena occur much earlier or later; the first frost occurred in 1864–5 and in 1881-2 on October 5, while in 1877-8 no frost occurred until December 10. The last frost occurs, on an average, about April 4; in 1885-6 there was a frost as late as May 1, while in 1835-6 the thermometer did not fall below 32° after February 24. The fall of snow is much more irregular; it fell seventeen times in May, and once in June (in the year 1866). A paper of a imilar nature was published for Sweden in 1880, by M. Hildebrandsson.

THE practice of spraying fruits with certain mineral compounds, such as salts of copper and arsenic, to destroy insects and fungi, has called out discussion in regard to the ripened fruit after such spraying, and its fitness for food. The first condition for intelligent discussion of any subject is to know the facts in the case, so experiments have been made on the matter at the State Agriculture College, Michigan, and *Bulletin* No. 101 contains the results. In these experiments, extending over two years, the minerals used in spraying the fruits were found in appreciable quantities in every instance, though the amount was small in all cases except when the spraying had been purposely excessive. The question naturally arises whether the sprayed salts merely adhere to the surface or penetrate the substance of the fruit. Experiments made to test this showed that while most of the copper salts, in the case of a solution containing copper sulphate, adhered to the surface of pears sprayed with the solution, a portion found its way into the body of the fruit. Dr R. C. Kedzie, who has made the analyses, remarks that the use of poisons in horticulture is largely in excess of the amount required for a fungicide. One-half or even one-third of the amoun usually employed would probably give as good results. To be on the safe side, no fruits should be sprayed with solutions of mineral salts during the period of ripening, for though the amount found in a single pound of fruit may be very small, repeated doses of the poison might produce slow poisoning.

THE new theory of light-sensation devised by Christine L. Franklin, and intended to avoid the difficulties involved in the acceptance of the two chief theories in the field at present, known as Helmholtz's and Hering's theory respectively, is expounded in the last two numbers of *Mind*. While the Young-Helmholtz theory supposes that the judgment picks out of a mixture of colours all the even red-green-blue sensations, and deceives itself into thinking them to be a new sensation called white, the new theory assumes an independent retinal process as ground for the latter sensation, therein agreeing with Hering's theory. But while Hering supposes that some parts of the spectrum produce construction, and others destruction of the tissue of the retina, Miss Franklin considers that the sensations of the black-grey-white series must be regarded as the fundamental ones, and attributed to the dissociation of certain molecules, which she provisionally calls the grey molecules. The atoms thus dissociated have different periods of vibration, and in the more highly developed visual organs—those capable of colour-sensations—these colour-atoms differ in behaviour according to the wave-length of the light beating upon them. Thus some atoms would only be torn off by red light, and would give rise to the sensation of red. The prevalence of such colour molecules would coincide with the predominance of the structures known as cones in the fovea of the retina, while the "rods" are endowed chiefly with grey molecules. This is simply translating into the language of the theory the well-known fact that the colour sense is chiefly confined to the centre of vision, as anybody may prove by looking at a coloured object through the corner of the eye. This distribution, says Miss Franklin, offers a perfect analogy with that of the organs of hearing. In the ear we have a very simple apparatus for hearing noise only, and also a highly differentiated structure for the discrimination of notes of various pitches.

IN 1881 M. Blondlot gave the results of some experiments he had made on the velocity of propagation of Hertzian waves. The velocity was determined by calculating the period of the electrical vibrations from the dimensions of the resonator, and measuring experimentally the wave-length. The results obtained, while they indicated that the velocity is always approximately that of the propagation of light, showed that as the wave-length increased the velocity diminished. In a note, communicated at a recent meeting of the Académie des Sciences (Paris) (*Comptes Rendus*, No. 6, 1894), M. Mascart has shown that a more accurate calculation of the frequency gives a remarkable agreement between the different experiments. In this note the author gives the formula for the self-induction of a rectangle of wire, and applies it to the reduction of M. Blondlot's observations. He finds that the values obtained for the velocity of propagation show no systematic variation with the wave-length within the limits of observation, that is, between wave-lengths of 9 and 35 metres. The mean of all the experiments gives the value 303,200 kilometres per second as the velocity, while, if the results obtained with one of the resonators which M. Blondlot thinks are less trustworthy are omitted, the mean becomes 302,850 kilometres per second, the maximum variation obtained from this mean amounting to 2·5 per cent. The author also points out that it is interesting to note that the mean value of the velocity of propagation of electro-magnetic waves obtained is about one per cent. higher than the velocity of light. The difference he considers to be due to the fact that the calculated value of the self-induction is too small, for the radius of the wire is an important factor, which may be estimated too large, either owing to errors in measurement or to the fact that the current in the wire is not exclusively confined to the external surface of the wire (as the formula employed supposes), but penetrates some distance into the wire. The employment of wires of larger section, he thinks, might perhaps lead to a better result.

WITH reference to some recent experiments on the railway between Beuzeville and Havre, the *Electrician* says :—"When, about three years ago, a scheme was announced for building a locomotive on which a high-speed engine was to drive a three-phase alternator, which was in turn to drive motors, it met with a little ridicule, and the two sets of tests which have been recently made on the Chemin de Fer l'Ouest at Havre have raised a smile, but only where the reasons of this roundabout system have not been understood. The two chief difficulties in obtaining higher speeds than from 70 to 80 miles an hour with ordinary express locomotives are want of balance and want of space. The impossibility of avoiding the superfluous vertical action of balance weights on an ordinary single-wheel locomotive is alone sufficient to reduce adhesion, and to allow slip at speeds a little over 80 miles an hour. All these difficulties are reduced, if not avoided, in the Heilmann locomotive, though not without the introduction of others, and it remains to be seen how the balance of advantage works out."

MR. H. WORK DODD has investigated the question as to a relationship between epilepsy and errors of refraction in the eye, and the current number of *Brain* (part lxiv.) contains his results. He has examined the eyes of one hundred cases of true epilepsy, and compared the refractions with those of apparently normal eyes. It appears that of simple hypermetropia there were twenty-eight cases per cent. less in the epileptic than in the apparently normal class. Of astigmation of all kinds, there were twenty-six cases per cent. more in the epileptic division than in the normal one. These and other differences lead Mr. Dodd to conclude that, given a certain condition of instability of the nervous system: (1) errors of refraction may excite epilepsy; (2) the correction of the errors of refraction will, in combination with other treatment, in many cases cure or relieve the epileptic condition; and (3) that in some cases, when the refraction error has been corrected, the epilepsy will continue, generally in a modified form, in consequence of other irritation, even though the error of refraction may have been the exciting cause of the fits in the first instance. Mr. Dodd is strongly of opinion that in every case of epilepsy—in addition to general treatment and the investigation of other organs—the eyes should be carefully examined under a mydriatic with a view of correcting any error of refraction that may exist by the use of proper spectacles.

THE bacterial contents of ice from various sources has been very exhaustively investigated, but only a few experiments have been made on the vitality of particular micro-organisms in artificially frozen ice produced by means of freezing mixtures. Prudden exposed various bacteria to 24° of cold, and amongst these the typhoid bacillus was found still present in large numbers after 103 days of continuous exposure to this low temperature; if, however, the freezing was interrupted during the twenty-four hours by three separate thawings, they were entirely destroyed at the end of three days. Prudden also showed very clearly that the resistance of an organism depends upon its initial vitality, for whereas the *staphylococcus pyogenes aureus* taken from a fresh agar cultivation was present in very large numbers at the end of sixty-six days, if an old and half dried-up agar culture was used for the original infection, none were found after seven days. Renk (*Fortschritte der Med.* No 10, 1893) has quite recently examined the behaviour of the cholera organism in ice artificially prepared from sterilised river Saale water, and finds that five days uninterrupted exposure to a temperature of from − 0·5 to − 7° C. is sufficient to entirely destroy these bacilli; but contrary to Prudden's experience, he found that if the freezing was interrupted, which took place when the vessels containing the organisms were removed for examination, a *longer* time (6–7 days) was necessary for their annihilation. When unsterilised Saale water was used, the cholera organisms disappeared at the end of three days, and the ordinary water bacteria present were reduced in 24 hours from 1,483,000 to 62,445 per c.c. whilst after three days only 4480 were found. Prudden's experiments with the typhoid bacillus, together with those on the cholera organism, indicate how important it is that ice for consumption should only be prepared from sterilised water, or from water the source of which is altogether beyond suspicion of contamination.

THE Société d'Encouragement pour l'Industrie Nationale has issued its *Annuaire* for 1894.

WITH the present year the bi-monthly cryptogamic journal, *Hedwigia*, published at Dresden, and edited by Prof. G. Hieronymus, commences the publication of a periodical synopsis of cryptogamic literature.

WE have received a copy of "Bourne's Handy Assurance Directory" for 1894. The work appears for the first time under the imprimatur of Mr. William Schooling, who will doubtless sustain the reputation for accuracy earned for it by the late editor, Mr. William Bourne.

DR. M. BARATTA has prepared a series of maps showing the topographical distribution of earthquakes in Italy for each year from 1887 to 1891. The maps, which originally appeared in the *Annali dell' Ufficio Centrale di Meteorologia e Geodinamica*, should be of great interest to seismologists.

THE second volume of Sir David Salomons' "Electric Light Installations," dealing with apparatus, engines, motors, governors, switches, meters, &c. will be shortly issued in Messrs. Whittakers' "Specialists' Series." The third and concluding volume is now in the press, and will deal with the application of electricity.

MR. W. THYNNE LYNN'S "Celestial Motions" (Edward Stanford) has reached the eighth edition. The first edition of this useful little book was published ten years ago. Another little treatise by the same author, "Remarkable Comets," has just passed into a second edition. Both books have been revised and brought up to date.

AN important report on the Ainu of Yezo, Japan, prepared for the U.S. National Museum, by Mr. Romyn Hitchcock, has been received. It is profusely illustrated from photographs taken by the author, and contains a mass of detail concerning the remnant of a once numerous people in Yezo and on the islands Kumashiri and Zeterof.

MESSRS. BLISS, SANDS, AND FOSTER announce that they have made arrangements with the editor of "A Son of the Marshes," and with Prof. Boulger, for the joint production of twelve monthly volumes to be entitled "The Country, Month by Month." Mr. Lockwood Kipling has supplied a design for the cover. The first number will appear on March 1, and will be descriptive of that month.

A NEW work is announced by Mr. Leland, bearing upon his favourite subject—practical education. The manual deals with elementary metal work, including bent iron, repoussé, cut metal, and easy silver work. It is written primarily for manual training classes in elementary and preparatory schools, but will probably be found interesting to any one who has a mechanical bent. Mr. Karl Krall has revised the work while passing through the press. The publishers are Messrs. Whittaker and Co.

THE first part of the new journal, *Novitates Zoologicæ* has been issued. It is a large 8vo, with 266 pages and four coloured plates, while six others are deferred, to appear in part ii. An excellent memoir, by Dr. Forsyth Major, on the small lemurs of Madagascar (*Microcebus*, &c.), commences the work; then follow articles by Mr. Rothschild (on a new pigeon, and on some new sphinx-moths), and by his two assistants, Dr. E. Hartert and Dr. K. Jordan, on various birds and insects. The organ of the Tring Museum has made a good start, and promises to be of great interest to zoologists.

LOVERS of nature will be glad to know that the supposed dissolution of our old contemporary, *Science Gossip*, after nearly thirty years' prosperity, proves to be only a case of suspended animation, and that its familiar face will again be seen in public after the 25th inst. In future *Science Gossip* will be under the editorship of Mr. John T. Carrington and Mr. Edward Step. The character of the paper as a medium between amateur naturalists, and for the recording of observations, will be fully maintained; at the same time, it is intended to give it a higher educational value by enlisting the aid of the leading men in every department of natural science. Messrs. Simpkin, Marshall, and Co. will in future be the publishers.

IN our issue of November 9, 1893, we gave a description of some Hindoo dwarfs photographed by Colonel A. T. Fraser. Dr. A. E. Grant afterwards suggested that the dwarfs were afflicted with the disease known as pseudo-hypertrophic paralysis. Colonel A. T. Fraser writes to us, however, as follows:—"On observing Dr. A. E. Grant's letter in NATURE for January 4, I lost no time in sending him a copy of the dwarfs' photograph, to which his reply states—'It is evident they are true dwarfs, and not subjects of the disease I alluded to. Their heads and trunks *appear* to be of normal size, whilst their limbs are stunted and deformed.'"

UNDER the title, "Climates of the United States," Dr. Charles Denison has prepared a revised edition, in a condensed form, of his annual and seasonal climatic charts of the United States. The book is published by the W. T. Keener Co., Chicago. It consists of twelve charts and eleven tables representing the climatic statistics of different sections of the United States. The annual rainfall and temperature are shown on one chart, the former by means of broken lines, and the latter by the usual isothermals. A chart is devoted to the illustration of annual cloudiness, and one to regimal elevations. Upon the four charts exhibiting the isothermal lines for the four seasons of the year, a number of arrows of three different kinds are drawn, showing not only the directions of the prevailing winds, but also the directions of winds likely to be followed by rain or snow, and the directions of those that usually herald fine weather. The average atmosphere humidities during different seasons of the year are clearly shown in eight degrees of colour. Altogether the book presents in a handy form a mass of climatological information.

THE late Prof. Hertz could have no more permanent monument than that afforded by his work on the propagation of electric energy through space, reviewed in these columns on October 5, 1893. An English edition of the collected papers contained in that volume has recently been published by Messrs. Macmillan and Co., under the title "Electric Waves." Prof. D. E. Jones is the translator, and he had the advantage of Dr. Hertz's supervision and advice while the book was passing through the press. In a preface, Lord Kelvin briefly describes the development of the idea as to action at a distance, and concludes by pointing out that "absolutely nothing has hitherto been done for gravity either by experiment or observation towards deciding between Newton and Bernoulli, as to the question of its propagation through a medium, and up to the present time we have no light, even so much as to point a way for investigation in that direction." Lord Kelvin also calls attention to the experimental work on electromagnetic waves done previous to the publication of Hertz's researches, but which do not detract in the least from their merit. The English reading public will doubtless fully appreciate Prof. Jones' translation of one of the most important works of this century.

THE polymeric modifications of acetic aldehyde form the subject of an interesting and important communication by Messrs. Orndorff and White to the January issue of the *American Chemical Journal*. These remarkable substances, paraldehyde and metaldehyde, have furnished the theme of many investigations, but their nature and their relation to common aldehyde has not hitherto been definitely established. In the older treatises upon organic chemistry, no less than five different polymeric forms of aldehyde are mentioned, but the researches of Kekulé and Zincke resulted in the existence of only two being established, the liquid paraldehyde and the solid metaldehyde. It was shown that carefully purified aldehyde suffers no change on heating or cooling, or on being kept for a length of time, and that polymerisation is always connected with the presence of certain substances, such as hydrochloric and sulphuric acids or carbonyl chloride. In most cases both forms are simultaneously produced, a low temperature, particularly below 0°, favouring the formation of metaldehyde, and a higher temperature being more favourable to the production of paraldehyde. The vapour density of paraldehyde was further shown to correspond to the triple formula $(C_2H_4O)_3$, and it was assumed that three molecules of ordinary aldehyde unite to form the closed chain compound, paraldehyde. The constitution thus arrived at for the liquid polymer of aldehyde has since received remarkable confirmation from the spectrometric work of Brühl, who found that the molecular refraction of paraldehyde corresponded to that calculated upon the assumption of the triple formula. Metaldehyde only differs from paraldehyde in its physical properties; chemically, the two compounds behave precisely alike. The vapour density of metaldehyde cannot be directly determined owing to its partial dissociation into ordinary aldehyde when heated, hence its formula has not hitherto been definitely known. Hanriot and Œconomedes succeeded, however, in determining its density by introducing a correction for the amount of ordinary aldehyde produced, and their results indicated that the formula of this solid polymer was the same as that of the liquid paraldehyde. Orndorff and White have here taken up the subject, and show that determinations of molecular weight by Raoult's method, using phenol and thymol as solvents, point irresistibly to the same conclusion, the molecular weight found being always in the neighbourhood of 132, corresponding to three times 44, the molecular weight of aldehyde. They have also repeated and extended the vapour density determinations of the former observers, and have definitely settled the fact that paraldehyde and metaldehyde are isomers, both possessing the molecular composition

$(C_2H_4O)_3$. They further show that metaldehyde is by no means so stable as has been supposed; it decomposes completely in a few days' time, the products of decomposition being paraldehyde and a new polymer, tetraldehyde $(C_2H_4O)_4$. The latter substance, whose composition has been definitely established by vapour density and cryoscopic determinations, is a solid of similar appearance and properties to metaldehyde. It is finally shown that paraldehyde and metaldehyde are in all probability stereo-isomers, like maleic and fumaric acids, the more stable paraldehyde corresponding to the fumaroid or so-called "cis-trans" form, and the less stable metaldehyde to the maleinoid or "cis" form.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*, ♂) from India, presented by Mr. James Carter; two Vulpine Phalangers (*Phalangista vulpina*, ♂ ♀) from Australia, presented respectively by Mrs. Percy Morton and Mr. W. Hughes; two Garden Dormice (*Myoxus quercinus*) European, presented by Dr. R. B. Sharpe; a Goshawk (*Astur palumbarius*) European, presented by Mr. Duncan Parker; a Jackdaw (*Corvus monedula*) British, presented by Mrs. Dixon Brown; two Striped Hyænas (*Hyæna striata*) from North Africa, a Mitred Guinea Fowl (*Numida mitrata*) from Madagascar, deposited.

OUR ASTRONOMICAL COLUMN.

SUN-SPOTS AND MAGNETIC DISTURBANCES.—The *Memoirs* of the Società degli Spettroscopisti Italiani (vol. xxii. p. 189) contains a paper by Dr. L. Palazzo on the magnetic disturbances of August 1893, considered in relation to the extent of solar spots. When the very large spot, or rather group of spots, was passing the central meridian on August 6 and 7 of last year, the bifilar magnetometer of the Roman College Observatory was considerably disturbed. On August 18, that is, when the spots were again near the plane of the central meridian, but on the other side of the sun, all three magnetic elements suffered a disturbance. Another magnetic storm was recorded at the Marine Observatory of Pola on August 12 and 13. Dr. Palazzo has collected all the facts connected with these three disturbances, and discusses them with the idea of determining the relation, if any, between them and sun-spots. From the paper it appears that the magnetic perturbation of August 6 commenced at 4·7 hours, when the double spot was about 15°·4 from the central meridian. The middle point of the pair passed the central meridian at 8·5 hours on the following day. It would be interesting to know whether the sun was under observation at any place east of Rome at a time corresponding to that given for the commencement of the brusque magnetic disturbance described by Dr. Palazzo, and if so, whether any strange phenomenon was observed. The disturbances of August 12 preceded by about twelve hours the transit of the largest spot visible upon the sun at the time. On August 18, however, no spot could be seen near the central meridian when the magnetic needles were recording a perturbation, while neither when the double-spot again appeared on the sun's limb, nor when it passed the central meridian on September 2, did the magnetic needles flutter. We have, therefore, spots without disturbances, and disturbances without spots, thus indicating that there is no connection between the phenomena. Prof. Ricco's discussion of the relation between solar spots and disturbances of terrestrial magnetism (*Mem. degli Spettrosc.* vol. xxi. p. 153, 1892) led him to believe that magnetic disturbances occur, on the average, about 45·4 hours after the transit of spots over the central meridian of the sun. M. Marchand (*Comptes Rendus*, 1887, p. 133) showed that such disturbances occurred when groups of spots or faculæ were near the centre of the sun's disc, and Dr. Veeder has given evidence to prove that the appearance of spots on the sun's eastern edge is the signal for magnetic fluctuations. Dr. Palazzo, however, believes that the position with respect to the earth of the solar region disturbed is really unimportant.

STONYHURST COLLEGE OBSERVATORY.—Father Sidgreaves' report on the meteorological, magnetic, and solar observations made at Stonyhurst College Observatory during 1893 has been issued. We extract from it the chief points of astronomical interest.

The ordinary work of the solar chromosphere was practically suspended during the year on account of the anticipated dismounting of the telescope for the erection of the Father Perry Memorial. But the sun-spot drawings have been continued, and were carried on with the six-inch objective which was mounted on the equatorial during the absence of parts of the eight-inch telescope. The new objective, with its mountings, was erected on November 6. It has a clear aperture of 14⅝ inches, and was worked by Sir Howard Grubb, of Dublin. It is valued at £650, and constitutes the substantial tribute to the memory of the late Father Perry, raised by the generosity of his many friends.

The large grating spectrograph has been employed upon the solar spots and faculæ with the result that 175 photographs were obtained of spot-spectra in the green-yellow region, and ninety-two plates of faculæ-reversals of the H and K lines.

The night-work with the equatorial has been confined to stellar photographic spectra. In May, it was decided to make use of every opportunity upon the variable star β Lyræ; and as the exposures upon this were necessarily long, and there were many failures, other stars were let alone. Out of the whole number of exposures forty-five plates of β Lyræ proved to be available for careful measurements, and the results are published in the December number of the Monthly Notices of the Royal Astronomical Society.

THE "ANNUAIRE" OF THE BUREAU DES LONGITUDES.—A copy of the *Annuaire* of the Bureau des Longitudes, for the present year, has been received. Every year sees an increase in the quantity of matter compressed into that veritable *vade mecum*. To the present volume has been added notes by Prof. Cornu on the physical aspect of the sun, solar spectroscopy, and the spectra of comets and nebulæ. The descriptive note on stellar spectra, began in the 1893 issue, is completed, and an account is given of recent observations of β Lyræ, and the spectrum of Nova Aurigæ. The articles include one by Prof. Poincaré, on light and electricity, according to Maxwell and Hertz; another, on the origin and use of the compass, by Contre-Amiral Fleuriais; and a third, in which Dr. Janssen describes four days of observation on the summit of Mont Blanc. Altogether, the 1894 *Annuaire* adds to the reputation gained by its predecessors; it is a volume which no astronomer can afford to be without, and which every student of physical science will find useful.

THE SPECTRUM OF NOVA NORMÆ.—A telegram received at Kiel on February 15 announces that Nova Normæ was observed by Prof. Campbell at the Lick Observatory on February 13, and found to have fallen to magnitude 9·5 (*Astr. Nachr.* 3211). The spectrum was seen to consist of four bright lines of the same relative intensity and position as those shown by Nova Aurigæ in August, 1892 (see NATURE, vol. xlviii. p. 524). Like this new star, therefore, Nova Normæ has descended to the condition of a planetary nebula.

THE SMITHSONIAN INSTITUTION REPORT.

THE report of Prof. S. P. Langley, Secretary of the Smithsonian Institution, for the year ending June 30, 1893, has just been published. Its contents refer, not only to the Smithsonian Institution, but also to the work of the U.S. National Museum, the Bureau of Ethnology, the Bureau of International Exchange, the Zoological Park, and the Astro-Physical Observatory. To do justice to the many and various operations of all these sections is impossible within the limits of space at our disposal, but some idea of the work may be obtained from the following abstract:—

Research.

It appears to be an essential portion of the original scheme of the government of the Smithsonian Institution that the secretary should be expected to advance knowledge, in letters, or in science, by personal research. Prof. Langley has continued the traditions of the Institution and the usage of former secretaries by contributing to the objects stated, as far as his increasing administrative duties would permit. During 1893 he continued the researches, of which a portion was published in 1891, in a treatise entitled "Experiments in Aerodynamics." Interesting results have since been reached, which appear to be of wide utilitarian importance, but though Prof. Langley hopes soon to be able to make some communication of them to the public, they are not yet complete. In this same connection, in pursuit of an investigation begun some years ago, he has made experiments upon the variations continually going on in the atmosphere, in what is regarded for ordinary meteorological purposes as a steady wind. Specially light anemometers have been constructed and mounted upon the north tower of the Smithsonian building, and connected with a suitable recording apparatus. The complete results, which promise conclusions of practical importance, are being collated and will be published at a later date. (See NATURE, January 18.)

The extensive investigations carried on in astro-physics are referred to in the adjoining column.

As in previous years, aid to a limited extent has been given to original investigators who are not immediately connected with the Institution. Prof. E. W. Morley has continued his determinations of the density of oxygen and hydrogen, for which special apparatus has been provided by the Institution.

A paper by Prof. A. A. Michelson, upon the "Application of interference methods to spectroscopic measurements," with a view to increased precision in measuring specific wave-lengths of light, has been published in connection with his work upon a universal standard of length. Mr. F. L. O. Wadsworth was detached from the observatory staff, and sent (at the expense of the Smithsonian fund) to the Bureau Internationale des Poids et Mésures, near Paris, to assist Prof. Michelson during a stay of six weeks in preparation of this standard.

The Hodgkins Fund.

Numerous applications, which are referred to the advisory committee for consideration, have already been made for grants from the Hodgkins Fund to aid original investigations upon the nature of atmospheric air and its properties. Two have been approved, a grant of 500 dollars having been made to Dr. O. Lummer and Dr. E. Pringsheim, members of the Physical Institute of the Berlin University, for researches on the determination of an exact measure of the cooling of gases while expanding, with a view to revising the value of that most important constant which is technically termed the "gamma" function. Drs. Lummer and Pringsheim were recommended for this work by Dr. H. von Helmholtz, of Berlin.

A second grant of 1000 dollars has been made to Dr. J. S. Billings, U.S.A., Army Medical Museum, Washington, and to Dr. Weir Mitchell, of Philadelphia, for an investigation into the nature of the peculiar substances of organic origin contained in the air expired by human beings, with a specific reference to the practical application of the results obtained to the problem of ventilation for inhabited rooms.

The Naples Table.

In the spring of last year, a petition, signed by nearly two hundred biologists, who represented some eighty universities and scientific institutions, was presented to Prof. Langley, asking that a table be maintained by the Smithsonian Institution at the Naples Zoological Station, for the benefit of American investigators. This step was favourably decided upon, and in April last an advisory committee was appointed, at Prof. Langley's request, in order to obtain opinions as to the best administration of the table. The four members of this committee are:—Major John S. Billings, U.S.A., nominated by Prof. O. C. Marsh, President of the National Academy of Sciences; Dr. E. B. Wilson, Professor of Zoology, Columbia University, nominated by Prof. Chittenden, President of the Society of American Naturalists; Dr. C. W. Stiles, Zoologist, Bureau of Animal Industry, U.S. Department of Agriculture, nominated by Prof. C. O. Whitman, President of the American Morphological Society; Dr. John A. Ryder, Professor of Embryology, University of Pennsylvania, nominated by Prof. Allen, President of the Association of American Anatomists. Dr. J. S. Billings, U.S.A., has been designated chairman, and Dr. C. W. Stiles secretary of the committee.

Satisfactory conditions as to the occupancy of the table have been arranged with Dr Dohrn, the director of the station at Naples, and a contract has been signed and completed.

Numerous applications for the occupancy of the table have been received, but at the close of the fiscal year sufficient consideration had not been given them to render it possible to make any definite assignment.

The Astro-Physical Observatory.

Prof. Langley has continued his important investigations with the bolometer. The instrument, as now constructed, is a minute strip of metal barely $\frac{1}{500}$ of an inch wide, and less than $\frac{1}{5000}$ of an inch thick. Through this frail thread of metal a current of electricity is continually kept flowing. When the spectrum, visible or invisible, is thrown upon it, the thread is warmed and the current decreased by an amount corresponding to the intensity of the effect received, while novel instruments specially mounted and constructed are in electric connection with the thread, and now automatically record every minute change in this current.

With late improvements these instruments are so delicate that a change of temperature of one-millionth of a degree is readily detected and even measured, and it is easy to see that as a consequence of this delicacy the greatest care must be taken in their use. Thus the laboratory must be almost completely darkened, and closed tightly, so as to exclude all draughts and to keep it at as nearly a uniform temperature as possible, while for other reasons it must be kept under constant hygrometric conditions.

In spite of numerous difficulties, most of which are due to the very temporary and inefficient nature of the small wooden building in which the work is carried on, and its proximity to the traffic-laden streets, the expectations of last year have been largely realised, and a detailed publication of the work, accompanied by charts showing several hundred new and before unknown lines, will shortly be issued.

The result of the year's work has been the discovery and approximate determination of position of about 150 or 200 new lines in the hitherto unexplored region of the solar spectrum. Important as these results are, they are but the beginning of of what Prof. Langley hopes will be accomplished.

In addition to the bolometric work proper, experiments on three special methods of investigation of the infra-red spectrum have formed a considerable portion of the year's work:—(1) Preliminary experiments on the measurement of wave-lengths in the invisible spectrum by interference methods. (2) Experiments on photographing the invisible spectrum by the aid of phosphorescent films. (3) Preliminary experiments on bolometric investigation of the infra-red normal spectrum. What might almost be said to have been the chief work of the observatory for the year, has been the improvement of the apparatus and instrumental conditions of working.

Lunar Photography.

Prof. Langley has been interested for a considerable time in the possibility of preparing a chart of the moon by photography, which would enable geologists and selenographers to study its surface in their cabinets with all the details before them which astronomers have at command in the use of the most powerful telescopes. Such a plan would have seemed chimerical a few years ago, and it is still surrounded with difficulties, but it is probable that within a comparatively few years it may be successfully carried out. No definite scale has been adopted, but it is desirable that the disc thus presented should approximate in size one two-millionth of the lunar diameter; but while photographs have been made on this scale, none of them show detail which may not be given on a smaller one.

The work has been favoured with the co-operation and interest of the directors of the Harvard College Observatory, of the Lick Observatory, and others, who in response to a letter addressed to them on February 10, 1893, have furnished many valuable suggestions.

The preparation of a series of enlargements of lunar photographs taken at the Kenwood and Lick Observatories, has been undertaken at the Astro-Physical Observatory. Some attempts at solar photography have been made at this observatory, but the atmospheric conditions prove to be very unfavourable to any satisfactory work in this direction.

The National Museum.

The National Museum suffers from the want of funds for the improvement of the collections by purchase. It is pointed out that in respect of this provision, the museum stands at the foot of all American museums, being surpassed even by every municipal museum of note in the United States. The disadvantage in which it stands (Prof. Langley remarks), when compared with what are now its competitors in the national collections of the leading countries of Europe, has grown painfully obvious. Important collections made in America of the objects illustrating the vanishing life of its own native races of men and animals—collections which can never be made again, and never be replaced—are being permanently withdrawn to enrich the museums of Europe. This has already gone so far that it is necessary in order to study the past life of the Mississippi Valley to come to England, while for that of southern Alaska Americans must go to Berlin, and for the Californian coast they have to go to Paris, and so on. It is already then, in European capitals more than in those of the United States, that the most important characteristics of the American races have to be studied, and at the present rate, within a few more years, when the American collector has nothing more left to gather and to sell abroad, it will be in Europe, and not in America, that the student of past American history must seek for nearly everything that most fully illustrates the ancient life and peoples of the American continent.

The Bureau of Ethnology.

As during previous years, the work of the Bureau of Ethnology has been conducted with special reference to the American Indians in their primitive condition, with a view of securing the largest possible amount of information, both in the form of records for print and in the form of material objects for preservation and future study in the National Museum.

One of the most interesting questions ever raised concerning the early peoples of America relates to the artificial mounds scattered abundantly over the Mississippi Valley, and with less abundance over most of the United States. Many investigators have given attention to these works of a vanished race; and it came to be a general opinion that the builders of the mounds were a distinct people antedating the native races found in possession of the land on the advent of the Europeans. Within the last five years extended surveys of the mound territory have been made by collaborators of the Bureau under immediate instructions from the director and by Dr. Cyrus Thomas. An elaborate report on this subject has been prepared during the year, and is now in press. It is the united opinion of the officers of the Bureau that this document contains the solution to the mystery of the mounds; very greatly to the surprise of the investigators who began the work, they have been led to believe that the mounds and the art products contained therein are in no wise distinct from the works of the modern Indians, and that the distribution of tribes can now be studied from the mounds themselves as well as from other aboriginal records.

Many other important investigations have been carried on, one of the chief being the means of interchanging ideas among the American Indians, including gesture, speech, and picture writing, as well as spoken language. The primitive modes of expression by means of gestures or pantomime, and by means of glyphs or pictures, are held by students as of special interest in that they represent the beginnings of language.

Smithsonian International Exchange Service.

As an illustration of the extent of this special part of the Institution's activities, it may be stated that it has now about 24,000 active correspondents, of whom 14,000 are in Europe, 200 in Africa, 500 in Australia, and about 9000 in the various countries of the Western Hemisphere. In the course of this work, the Institution has gathered at Washington an immense collection of books, found nowhere else to so great an extent, bearing chiefly upon discovery and invention, which, with others, now occupy nearly 300,000 titles. Over 100 tons of books passed through the exchange office during the fiscal year 1892–93, and while the service is used almost exclusively for the transmission of printed matter of a scientific nature, natural history specimens having no commercial value are occasionally transmitted under special permission, when they cannot be conveniently forwarded by the ordinary means of conveyance.

The National Zoological Park appears to be in a satisfactory condition, and fulfils the chief purpose for which it was made, viz. to keep from extinction species of American animals, several of which are now upon the point of vanishing from the face of the earth, and would vanish for ever if something were not done to preserve them.

In conclusion we must say that the report covers so many branches of science, and so much has been done to advance each of them, that in the above abstract it has only been possible to mention a few of the investigations. Sufficient has been said, however, to show that considerable contributions to knowledge have been made.

THE GREENLAND EXPEDITION OF THE BERLIN GEOGRAPHICAL SOCIETY.

PARTICULAR interest is felt by the Geographical Society of Berlin in the results of an expedition to the north of Greenland, which they fitted out some two years ago. At the sitting of the Society held on November 4, 1893, Dr. Erich von Drygalski and Dr. E. Vanhöffen communicated papers on the work of the expedition, Dr. Drygalski giving a general account of their life in Greenland.

On June 27, 1892, they reached Umanak, a Danish colony on the shores of North Greenland, and selected as their base of operations a position some distance inland at the head of the Umanak Fjord. They placed their house in the hollow of a great ice-cirque. East and west were the ice-streams of the Great and Lesser Karajak, behind them stretched the bare expanse of the ice-sheet of the interior, in front lay the open water of the narrow fiord. Dr. Stade had charge of the meteorological station; Dr. Drygalski and Dr. Vanhöffen made journeys into the interior and along coastal regions of glacier and moraine.

At first, when they ascended the Karajak, none of the Greenlanders were willing to accompany them, as they are full of superstitions about the ice-wastes of the interior. Three ultimately consented, and overcame their fears so far as to enter with spirit into the difficulties of the tour. Bamboo canes were fixed as marks in the ice, and the "interference area" studied where the upper ice of the Karajak streams meets the inland ice. In the winter months, Dr. Drygalski, with two trusty Greenlanders, explored the Great Karajak glacier. He took measurements on the relative rate of movement in the smoother and more cleft parts of the glacier. He tells how, as the big blocks of ice tumbled down, fine ice-dust was raised, which hung like a transparent veil around the ice-pillars and hummocks, sometimes catching the sun-rays and glancing with colour effects. Ice-grottoes were found, the remnants of old water-channel in those the temperature was wonderfully high, and the ice-wa quite moist.

From February until June, Dr. Drygalski and Dr. Vanhöffen were engaged in a long sleigh journey to the most northerly part of the Upernivik colony, in Lat. N. 73°. At this latitude the outer margin of the great ice mantle of the interior extended to the sea level. Another tour which they attempted in June had to be given up on account of the warm Föhn wind. Before their final departure from Karajak, they ascended the ice once more to take observations on the bamboo marks previously set. Dr. Drygalski attributes the movement of the ice-streams to their content of water, and says there would be no motion whatever unless the melting temperature were reached. Farther, the increase of temperature in summer, due to the downward passage of heated surface-water, is much greater than the decrease of temperature in winter. The warming effect of the water is at its maximum in the deepest layers of ice, where also the movement is most marked. Microscopic examination of the ice also proved that it was thoroughly penetrated with water. It will be some time before the expedition can publish their results in detail. Dr. Vanhöffen's work was mainly biological.

THE SUN-SPOT PERIOD AND THE WEST INDIAN RAINFALL.

THE irregularities of the rainfall from year to year are so large that apparently there is no connection whatever between the sun-spot period and the Jamaica or any other rainfall; but if we smooth down these irregularities by taking the mean for three years as the rainfall for the middle of those years—that is to say if we take the mean of the rainfall during 1866, 1867, and 1868 as applying to the middle of 1867, the mean of the rainfall during 1867, 1868, and 1869 as applying to the middle of 1868, and so on—we shall then get a series which rises to a maximum about the time of a solar minimum, and which falls to a minimum about the time of a solar maximum.

It is now about a year ago since this connection was found between the sun-spot period and the Jamaica rainfall, and my article on the subject appeared in the *Journal* of the Jamaica Institute, No. 5.

The Barbados, Antigua, and Trinidid rainfalls have been subjected to the same treatment with the same results; but it will be noticed in the following table that the smoothed Jamaica rainfall rises and falls with much greater regularity than the smoothed rainfall in Barbados, Antigua, and Trinidad; the irregularity in the last island is due to the circumstance that we are dealing with the rainfall at one station only, namely the Botanic Gardens, instead of the rainfall deduced from many stations, as in the other islands.

Year (middle of).	Sun-spot period.	JAMAICA. Rainfall, 90 stations.	JAMAICA. Average for 3 years.	BARBADOS. Rainfall, 90 stations.	BARBADOS. Average for 3 years.	ANTIGUA. Rainfall, 47 stations.	ANTIGUA. Average for 3 years.	TRINIDAD. Rainfall, 1 station.	TRINIDAD. Average for 3 years.
		in.	in.	in.	in.	in.	in.	in.	in.
1843	Min.			45·31	—				
44				74·45	54·56				
45				43·91	61·39				
46				65·82	52·61 Min.				
47				48·10	59·23				
48	Max.			63·77	54·88				
49				52·77	61·47				
50				67·88	60·02				
51				59·40	62·02				
52				58·77	62·34				
53				68·84	59·50				
54				50·88	65·68 Max.				
55				77·31	58·89				
56	Min.			48·49	62·23				
57				60 90	51·54				
58				45·22	53·45 Min.				
59				54·22	52·45				
60	Max.			57·91	61·98				
61				73·82	63·97				
62				59·27	58·49			63·15	—
63				42·38	53·61			66·80	64·28
64				59·19	56·74			62·90	71·66
65				68·64	62·50			85·28	72 01 Max.
66		53·65	—	59·68	66·08 Max.			67·86	73·23
67	Min.	64·47	61·95	69·93	58·07			66·56	63·54
68		67·74	62·53	44·60	54·35			56 21	58·74
69		55·37	70·85 Max.	48·52	51·10			53·46	59 67
70	Max.	89·43	64·96	60·17	50·05			69·35	66·13
71		50·09	61·57	41·46	50·06			75·58	64·96
72		45·18	52·78 Min.	48·55	47·23 Min.			49 95	56·52
73		63·06	59·06	51·69	53·15			44·02	56·75 Min.
74		68·94	61·47	59·22	57·54	31·16	—	76·28	60·40
75		52·42	64·24	61·71	57·89	28·78	33·97	60·90	73·04
76		71·35	64·06	52·73	62·85	41·98	39·94	81·95	71 65
77		68·40	72·06	74·10	66·64	49·05	46·05	72·10	71·43
78		76·42	77 89 Max.	73·10	73·83	47·11	52·55	61·24	66·26
79	Min.	88·84	73·57	74·30	72·79 Max.	61·50	52·77	65·43	69·67
80		55·44	70·96	70·98	71·91	49·69	54·98 Max.	82·34	71·16 ?
81		68·60	60·64	70·45	63·83	53 75	45·49	65·72	67·02
82		57·87	61·91	50·06	61·21	33·04	47·43	52 99	63·07
83		59·26	58·01	63·12	57·04	55·51	44·13 Min.	70·50	60·12
84	Max.	56·90	58·67 Min.	57·95	55·05 Min.	43·98	47·63	56·88	56·87 Min.
85		59·86	69·12	44·08	61·61	43·39	45·05	43·22	62·31
86		90·61	73·71	82·81	65·30	47·78	44·95	86·82	64·71
87		70·66	77·79 Max.	69·01	73·64	43·68	45·23	64·09	72·12
88		72·11	72·31	69·09	71·67 Max.	44·23	53·83 Max.	65·44	67·77
89	Min.	74·15	70·23	76·92	66·18	73·59	50·27	73·79	74·04 ?
90		64·42	74·42	52·53	65·25	33·00	52·20	82·90	70·14
91		84·70	74 03	66·30	—	50·01	40·51	53 74	75·93 ?
1892		72·98	—	—	—	38·53	—	91·14	—

The Barbados rainfall was discussed by Sir Rawson W. Rawson in 1873,[1] and indeed it neither was, nor yet is easy to make out the connection between the years 1843 and 1863; but since 1863 it is all plain sailing, especially when aided by Jamaica on one side and Antigua on the other.

I have written to Mr. Hart, the superintendent of the Botanic Gardens, Trinidad, asking him to assist me in getting the Trinidad rainfall into better form.

MAXWELL HALL.

[1] NATURE, vol. viii. pp. 245, 547; vol. x. p. 263; and vol. xi. p. 327.

ON PREPARING THE WAY FOR TECHNICAL INSTRUCTION.

SIR PHILIP MAGNUS discoursed on methods of technical instruction on February 14, at the College of Preceptors. In the course of his address he pointed out that our intermediate schools were generally described as in a state of chaos, and it could scarcely be expected that so nebulous a system would be largely influenced by the definite movement in favour of technical education. As a fact, they had been much less affected than the institutions above and below them, and probably in consequence of the recognised absence of organisation. It might be that the Royal Commission about to be appointed would introduce order into this chaos, and that when each school knew exactly its position in the school hierarchy—its relation to the schools above and below it, and the special and particular purpose it was required to serve—our intermediate schools, both first and second grade, would become more efficient than they now were in preparing the way for that technical education which, in every branch of professional and commercial life, was being recognised as indispensable.

In the New Education, the most important subject of instruction was science. It was the development of science, and its application to the varied work of life, that had changed to a great extent, and would change still more in the near future, the entire character of our school teaching. In an address given in 1876, Sir Philip remarked upon the inadequate attention given to the teaching of science in our endowed schools. Out of one hundred and twenty-eight schools which furnished replies to the Commissioners at that time, there were only sixty-three schools in which any kind of science was taught, and of these only thirty devoted any regular time to scientific study. Since then a great change had taken place; but the change was more marked in the elementary than in the secondary schools. And the right of science to be included in the school curriculum had only recently been generally recognised.

The advance was very satisfactory; but the important question was whether, with the increase in the number of schools in which science was taught, there had been any corresponding improvement in the *method* of science teaching?

The progress in this direction had not been as marked as one might have wished. The correct methods of science teaching were only very gradually being understood. It was largely owing to the usefulness of the information which the study of science involved, that the value of the study as a means of education had been lost sight of. It should be remembered that "acquirement of every kind has two values—value as *knowledge* and value as *discipline*"; and, in early education, the latter was by far the more important. With the first feeling of intoxication which the breathing of the atmosphere of science excited, there was a strong reaction against the teaching of subjects apparently useless, as mere instruments for mental gymnastics. There was a loud cry for useful information; and the scientific lecture, with its platform experiments, served both to awaken the interest of the pupil and to afford such information. But, gradually, better views prevailed, and it was recognised, although very slowly, that information was not the first object of science teaching, and that, valuable as was the information which science conveyed, such information was of little use unless the process of informing served to train and discipline and educate the faculties. Accuracy in thought and expression, the power of arranging and co-ordinating facts, and of acquiring, retaining, and reproducing in logical order, new ideas, and the habit of deliberation in arriving at conclusions, were educational ends of far more real value than any amount of mere knowledge which the student of science might gain. The recognition of this educational truth had rudely shaken methods of teaching, and even of examining in scientific subjects.

Herbert Spencer, in his well-known essays on Education, had said:—"It would be utterly contrary to the beautiful economy of nature if one kind of culture were needed for the gaining of information, and another kind were needed for mental gymnastics. Everywhere throughout creation we find faculties developed through the performance of those functions which it is their office to perform; not through the performance of artificial exercises devised to fit them for those functions. . . . The education of most value for guidance must at the same time be the education of most value for discipline." The method of teaching science must therefore be carefully considered, so that the training of the faculties might be steadily kept in view as the aim and object of the instruction, rather than the mere acquisition of knowledge. This change of method involved the substitution, from the very commencement, of practical work on the part of the pupil for the ordinary lecture or lesson.

At the outset, the practical exercises should be of the very simplest kind. The pupil must take nothing for granted. It was clear, therefore, that he must commence with simple exercises in measurement. In physics they were always dealing with quantities, and could not understand what is meant by a quantity except by measuring it. The first measurements to be made were those of length. In making such measurement, certain standards had to be considered, and different systems (the English and the metric systems) should be compared. These comparisons involved easy exercises in arithmetic, which might be practised in connection with such concrete examples. Various objects should be actually measured, and the length calculated by multiplication or division of other measurements. But the pupil should be made thoroughly familiar with his standard of measurement before passing away from this exercise. This should be followed by measurement of areas, the consideration of which was fruitful in useful exercises. In country schools the actual measurement of the areas of fields, by simple methods of surveying, might be usefully attempted; in town schools there was generally a playground which would afford opportunities for similar exercises. Then the methods and results of all such measurements should be carefully and neatly transferred to paper, and the pupil should be thus incidentally exercised in elementary drawing. The measurement of volume would follow, with more varied and more difficult problems.

Immediately connected with the measurement of volume was that of mass. There, of course, a difficulty arose, owing to the close connection between *mass* and *weight*, and the difficulty of distinguishing between them. But the explanation of this difficulty might be postponed, and the pupil could be allowed to use ordinary weights as measures of mass. At this stage he was introduced to a balance, and, with a view of inducing habits of accuracy, he should at once use a fairly good balance. The exercises were very numerous which the pupil could practise with a good pair of scales. From this point the order of any elementary series of lessons could be varied at the discretion of the teacher. The balance suggested experiments, to be done by the pupil, on the use of the lever, whence the principle of the lever could be obtained. From the common balance to the Roman balance, and to other modes of weighing, the steps were very gradual. The relative volumes of bodies of the same material could then be ascertained by the balance, and former exercises in measurement be verified and repeated. The pupil should not only do the actual work himself, but should write out clearly a description of what he had done, thus learning to connect action, thought, and words. From these exercises the pupil might pass to the consideration of the difference in homogeneous bodies of the same volume and of different weight, and so on, by very easy stages, to methods of ascertaining relative weights of different substances. Exercises in finding specific gravities of solids, powders, and liquids, gave opportunity for very valuable instruction, and prepared the way for the use of instruments of precision, and for knowldege of interesting properties of different kinds of bodies. The value of these lessons consisted in the accuracy of measurement, and in the clearness and correctness of the written record, as regards the statement of facts, the sequence of reasoning, the numerical calculations, and the use of words and phrases.

It was, of course, essential that these written exercises should be carefully corrected, as are exercises in Latin or Greek composition. The aim of the instructor, in compiling such an introductory course as that suggested, should be to include those subjects, an acquaintance with which was required to enter upon the systematic study of any one branch of science, and which were practically common to all branches. The character of an introductory course might be influenced by the consideration of the special science which, in any particular school or district, would be likely to be studied, or indeed by the special taste of the instructor. A knowledge of the use of simple measuring instruments, including the thermometer, the barometer, the hygrometer, having been acquired, the pupil might pass by carefully suggested experiments to the determination of simple physical laws, and to the discovery of the composition of common substances, such as air, water, salt, lime, &c.; and it was needless to say that such exercises would open up wide views of the elementary facts and laws of different branches of science, and would prepare the way for the specialised teaching which more properly belongs to technical education.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—Prof. August Weismann will deliver the Romanes Lecture in the Sheldonian Theatre, on Wednesday, May 2.

Prof. H. H. Turner has selected "The International Photographic Chart of the Heavens" for the subject of his inaugural lecture as Savilian professor of astronomy, to be delivered to-morrow.

CAMBRIDGE.—The Vice-Chancellor has appointed Mr. J. W. Clark, Registrary and formerly Superintendent of the Museum of Zoology, to the office of Reader on Sir Robert Rede's foundation for the present year, in succession to Prof. Foster.

The Special Board for Medicine report that in consequence of the great increase in the number of candidates for the M.B. degree (in 1893 there were 224 to be examined), it is necessary to increase the staff of Examiners to four in Medicine and four in Surgery.

THE Somerset County Education Committee announce that three Senior County Scholarships will be offered for competition in June 1894. They will be tenable for two years in the scientific or technical department of a university college, the Royal College of Science, South Kensington, or some other college or institution approved by the County Education Committee. The annual value of each scholarship will be from £50 to £60, according to the place of instruction chosen, and, subject to the maximum limit, will be fixed at a sum sufficient to cover the cost of instruction, together with £30 per annum toward's the scholar's maintenance. The competition will be open to any boy whose parents or guardians are *bonâ fide* residents in the administrative county of Somerset, and who has regularly attended any secondary school (public or private) within the county for two school years preceding August 1, 1894, provided that every candidate is over 15 and under 17 years of age on July 1, 1894, and that his parents are in receipt of an income of not more than £400 a year from all sources. Six intermediate County Scholarships will also be offered for competition in June 1894. They are of the annual value of £30, and will be tenable for two years at some public secondary school approved for the purpose by the County Committee.

SCIENTIFIC SERIALS.

American Journal of Science, February.—On the chemical composition of staurolite, and the regular arrangement of its carbonaceous inclusions, by S. L. Penfield and J. H. Pratt. A careful analysis of several specimens gave the formula $HAL_6FeSi_2O_{13}$, which may be written as a basic orthosilicate. The aluminium is partly replaced by ferric iron, and the ferrous iron by magnesium and manganese. Basal sections of the rhombic prism show the carbonaceous inclusions to be disposed in the form of a rhombus parallel to the outline, with the corners joined together. This figure develops into a simple cross towards the centre, whereas towards the ends the rhombus widens out until it coincides with the outline. This proves that the inclusions are arranged in the surface of a double pyramid with its apex in the centre, and also in planes joining the edges of this pyramid with those of the prism.—Additional species of pleistocene fossils from Winthrop, Mass., by R. E. Dodge. Three more species of preglacial shells have been found in the drumlin in Boston Harbour, known as Winthrop Great Head. They are *Lunatia Grœnlandica*, Stimpson, *Scapharca transversa*, Adams, and *Buccinum undatum*, Linné. These fossils give additional evidence of the higher temperature of Massachusetts Bay in pre-glacial as compared with the present time.—On the basalts of Kula, by H. S. Washington. These basalts occur near Kula, about 125 km. east by north of Smyrna, where they from cones and streams of a fresh and unaltered appearance. The lavas are to be classed as hornblende-plagioclase basalts, distinguished by the constant presence and great relative quantity of the hornblende, its peculiar magmatic alteration, the small quantity of both plagioclase and olivine, and the large amount of glass basis. The name *Kulaite* is proposed for them.—The fishing banks between Cape Cod and Newfoundland, by Warren Upham. If a portion of the continental border from Cape Cod to the Grand Bank south east of Newfoundland could be uplifted, we should behold nearly as much diversity of valleys, ridges, hills, plateaus, and all the forms of subaërial land erosion, as is exhibited by any portions of the adjacent New England states and eastern provinces of Canada. The submerged channels of outlet from the Gulfs of Maine and St. Lawrence, and the less profound valleys that divide the fishing banks from each other, prove that this region during a comparatively late geologic time was a land area, its maximum elevation being at least 2000 feet higher than now.

Bulletin of the New York Mathematical Society, vol. iii, No. 4, January.—"Modern Mathematical Thought," the presidential address, delivered by Prof. Newcomb, before the New York Mathematical Society (pp. 95–107), has been printed in our columns (see NATURE, vol. xlix. pp. 325–329). "Recent Researches in Electricity and Magnetism" (pp. 107–111) is a review, by G. O. Squier, of Prof. J. J. Thomson's "Notes." The reviewer feels assured that this "supplementary" volume will take its proper place beside Maxwell's great treatise in the library of every true student of electrical science. "Notes" and "new publications" occupy pp. 112–118.

SOCIETIES AND ACADEMIES.

LONDON.

Physical Society, February 9.—Annual general meeting. Prof. A. W. Rücker, F.R.S., President, in the chair.—The annual report of the Council was read by the President. Dr. Atkinson read the Treasurer's Report, and also an obituary notice of the late Prof. Tyndall. The adoption of the Reports was moved by the President, and carried *nem. con.* Dr. Chichester Bell and Mr. Griffiths were appointed scrutators, and subsequently declared the following gentlemen duly elected to form the new Council:—President, Prof. A. W. Rücker, F.R.S. Vice-Presidents: Walter Baily, Major-General E. R. Festing, F.R.S., Prof. J. Perry, F.R.S., Prof. S. P. Thompson, F.R.S. Secretaries: H. M. Elder, 50 City Road, E.C., and T. H. Blakesley, 3 Eliot Hill, Lewisham, S.E. Treasurer: Dr. E. Atkinson, Portesbery Hill, Camberley, Surrey. Demonstrator: C. Vernon Boys, F.R.S., Physical Laboratory, South Kensington. Other members of Council: Shelford Bidwell, F.R.S., W. E. Sumpner, Prof. G. Fuller, J. Swinburne, G. Johnstone Stoney, F.R.S., R. E. Baynes, Prof. G. M. Minchin, L. Fletcher, F.R.S., Prof. O. Henrici, F.R.S., Prof. S. Young, F.R.S. Prof. Reinold proposed a hearty vote of thanks to the Lords of Committee of Council on Education, for the use of the rooms and apparatus in the Royal College of Science. This was seconded by Prof. J. V. Jones, and carried unanimously. Votes of thanks were similarly accorded to the auditors, Mr. A. P. Trotter and Mr. R. Inwards, on the motion of Mr. Watson, seconded by Prof. Fuller; and to the officers of the Society, on the motion of Dr. Barton, seconded by Mr. Trotter. At an ordinary science meeting then held, Mr. Owen Glynn Jones read a paper on the viscosity of liquids, and exhibited the apparatus used in his experiments. The method employed consists in measuring the speed at which a small sphere travels through the liquid under the action of gravity. As Prof. Stokes had shown, the velocity of a sphere falling in an infinite liquid becomes constant, this velocity being given by the equation

$$V = \frac{2}{9}ga^2\frac{\sigma - \rho}{\mu},$$

where a is the radius of the sphere, σ its density, ρ the density of the liquid, and μ its viscosity. If sliding friction exists between the sphere and liquid, the equation becomes

$$V = \frac{2}{9}ga^2\frac{\sigma - \rho}{\mu}\frac{\beta a + 3\mu}{\beta a + 2\mu};$$

where β is the coefficient of friction. In making the experiments, small spheres (usually of mercury) were allowed to fall through a burette containing the liquid, and the time taken to travel the distance between two marks about 50 c.ms. apart noted. The radii of the spheres being small, it was considered better to deduce this from the mass. Direct determination of such small masses being difficult, a larger mass (M) was taken, weighed, and divided into, say, ten or twelve parts, and the speed of falling of each part observed in a liquid of constant viscosity. The velocity V, with which a sphere containing the whole mass would have fallen, was deduced from the equation

$$V^{\frac{3}{2}} = \Sigma v^{\frac{3}{2}}.$$

Similarly, the mass of any part which falls with a velocity v is given by

$$m = \left(\frac{v}{V}\right)^{\frac{3}{2}} . M.$$

In this way the author had been able to determine the mass of a sphere weighing only about 0·003 grammes to four significant figures. Referring to experiments made with a view to ascertaining whether sliding friction existed, the author said the divergence from the simpler formula did not exceed experimental errors. In determining viscosity, changes of temperature were found to be of great importance, especially in the case of glycerine, whose viscosity varies as much as 10 per cent. for 1° C. Small differences of temperature between different parts of the liquid are, however, not very serious, provided the mean temperature be known, for the mean speed observed is shown to be that corresponding to the mean temperature. To determine viscosity accurately at a given temperature, very delicate thermometers must be employed. Most of the liquids experimented on were bad conductors of heat, and hence required considerable time for the temperature to become uniform. Differences in temperature could readily be detected by observing if the speed of descent of a small sphere varied at different parts of its path. The author suggested that this fact might be used to determine the thermal conductivity of liquids heated at the top by Forbes' method. The falling sphere would form a thermometer of almost infinitesimal thermal capacity. For most oils, spheres of water coloured with eosin could be employed to determine the viscosity. A water-drop of 1 mm. radius was found to fall one inch per hour in castor oil at 8° C. To determine the variation of viscosity with temperature a special apparatus was used, with which observations could be made in rapid succession by simply inverting the tube containing the liquid and the falling sphere. In Mr. Trouton's viscosity experiments, which were somewhat analogous to those described, surface tension complicated the results considerably; the author's aim had been to eliminate such disturbing influences. Prof. Everett, in a written communication, suggested that the motion of the liquid spheres be checked by using beads of quartz or glass. Lord Rayleigh pointed out, in a letter, that the formula employed related to a solid sphere, and thought it not legitimate to use it for liquid spheres, for the tangential forces at the surface would set the interior liquid in relative motion, and modify the resistance experienced. He also thought the existence of a finite coefficient of sliding friction between two fluids an impossibility. Mr. Watson said temperatures might be kept constant for days together by Ramsay and Young's vapour jacket. Dr. Sumpner thought the surface tension of such small spheres of mercury was so very large that they would act practically like solids. The want of solidity might be of importance when the two liquids were very nearly alike in density and other properties. Mr. Blakesley said that at high velocities the falling sphere might get a palpitating motion, in addition to the gradual descent, and this might introduce errors. Prof. Perry considered that the experiments on the velocity of a small sphere, and those of the two parts in which it was divided, which showed that $V^{\frac{3}{2}} = v_1^{\frac{3}{2}} + v_2^{\frac{3}{2}}$ proved the simple formula used to be correct. Mr. Boys inquired if any tests had been made on the constancy of dimensions of the spheres used. He would expect that in the case of water and oil, for example, that mutual contamination would take place. Speaking of the indirect method of determining the masses of small spheres, he thought direct weighings might be made, for, as the President and Prof. Poynting had shown, the balance might be immensely improved. Prof. S. P. Thompson suggested that small globules of aluminium or slag might be used. Dr. C. V. Burton thought Lord Rayleigh's criticism important, and that large corrections might be necessary. He failed to see how the large surface tension mentioned by Dr. Sumpner could prevent internal circulation. Mr. Trotter said Lord Rayleigh's point might be tested by using a sphere of oiled wax. Mr. Boys mentioned that Lord Rayleigh had shown in the case of soap rings that variation of surface tension due to stretching or compression produced stability. The same phenomena would probably retard internal circulation. The President said there was little doubt that internal circulation, as mentioned by Lord Rayleigh, would modify the velocity. In his reply, Mr. Jones said he could not imagine how in pure liquids internal motion in the falling spheres could be set up. In answer to Mr. Boys, he had found slight changes in the masses of the water spheres after being used many times, but this was a question of days. During an ordinary series of observation the dissipation was too small to be observed. After the meeting had been adjourned, Mr. Boys and Dr. Burton considered the question of internal circulation, and the latter pointed out that with perfectly liquid spheres there would be infinite slip, and the coefficient of sliding friction β would be zero. The velocity of descent would therefore be $\frac{3}{2}$ times that given by the first equation.

Geological Society, February 7.—W. H. Hudleston, F.R.S., President, in the chair.—Mr. C. J. Alford, in explanation of specimens of auriferous rocks from Mashonaland exhibited by him, stated that several of them were vein-quartz occurring as segregations in the slates, generally forming veins between the cleavage-planes. Another specimen was a mass of chromate of lead, with pyromorphite and other lead minerals, occurring in masses in decomposed and dislocated talcose slate in the Penhalonga Mine near Umtali, and probably resulting from the alteration of masses of galena by weathering, as a broken vein of galena was found in close proximity. This crocoisite was supposed to be a somewhat rare mineral, but he had found it and also the native red oxide, minium, in several places in South Africa. The most interesting specimen, was, however, a mass of diorite showing visible gold throughout the rock, an assay of which gave upwards of 130 ounces of gold per ton. From information obtained from the prospector who made the discovery, he gathered that the deposit was a dyke of diorite running for a considerable distance, about 8 feet in width, flanked on one side by granite and on the other by slates. There were extensive ancient workings extending to a depth of about 60 feet, and the prospecting shafts had not gone much below that depth, so not much information was obtainable at present. The diorite showed a development of epidote, but little or no quartz; and the gold appeared to enter in an extraordinary manner into all of the composing minerals. Mr. Alford hoped, after his next visit to Mashonaland, to be in a position to lay before the Society more definite information regarding these interesting rocks.—The following communications were read:—On some cases of the conversion of compact greenstones into schists, by Prof. T. G. Bonney, F.R.S. By the path leading from the Bernina Hospice to the Grüm Alp (Engadine), some masses of compact green schist are seen, intercalated in a rather crushed gneiss. They prove to be intrusive dykes modified by pressure. Microscopic examination of specimens from these revealed no trace of any definite structure indicating an igneous rock; a slice, cut from one of the masses within an inch or so of a junction, showed it to be a foliated mass of minute chlorite or hydrous biotite, with granules of epidote (or possibly some sphene) and of a water-clear mineral, perhaps a secondary felspar. An actual junction showed a less distinct foliation and some approach to a streaky structure. A slide from the middle of another dyke (about 18 inches thick) exhibited a more coarsely foliated structure and minerals generally similar to the last, except that it may contain a little actinolite and granules of hæmatite (?), and the clear mineral, in some cases, seemed to be quartz. The structure and most of the minerals appeared to be secondary. Chemical analysis showed the rock to have been an andesite. A specimen from a third dyke was generally similar, but was rather less distinctly foliated. A somewhat similar, but rather larger intrusive mass by the side of the Lago Bianco showed more actinolite and signs of primary felspar, with other minerals. Here the rock retained some likeness to a diabase. The resemblance of certain of these rocks to somewhat altered sediments is remarkable. The author considered the bearing of this evidence upon other and larger masses of "green schist" which occur in the Alps, and expressed the opinion that their present mineral structure may be the result of great pressure acting on more or less basic igneous rocks.—The Waldensian gneisses and their place in the Cottian sequence, by Dr. J. Walter Gregory. The lower part of the sequence of the Cottian Alps has been universally divided into three series, of which the lowest has been regarded as a fundamental (basal) Laurentian gneiss. It was the object of the present paper to show that this rock is really intrusive in character and Upper Tertiary in age. The writer endeavoured to prove this by the following line of argument:—(1) The gneiss consists of only isolated outcrops instead of a continuous band, and these occur at different positions and not always at the base of the schist series; (2) the gneiss is intrusive, because (*a*) it includes fragments of the overlying series instead of *vice versa*, (*b*) it sends off dykes of aplite into the surrounding schists, (*c*) it metamorphoses the rocks with which it is in contact, and (*d*) the schists are contorted near the junction; (3) the gneisses were further shown to be later than the igneous rocks intrusive into the "pietre verdi" series, as these never traverse the gneiss. No positive opinion as to the age of the overlying schists was expressed, though it was pointed out that the recent discovery of radiolarian muds in the series may necessitate their inclusion in the Upper Palæozoic. The freshness of the gneisses, the fact that these have not been affected by the early Tertiary earth-movements, and the absence of authentic specimens of the gneiss in the Cretaceous, Eocene, and Miocene conglomerates, renders their late Tertiary age highly probable. The nature of the contact-metamorphism and the origin of the gneissic structure were discussed, and a classification offered of the earth-movements in the Cottian Alps. A discussion followed, in which the President, Prof. Judd, Mr. Barrow, Prof. Bonney, Mr. A. M.

Davies, Mr. Vaughan Jennings, and Dr. G. J. Hinde took part. The author briefly replied.

PARIS.

Academy of Sciences, February 12.—M. Lœwy in the chair.—On a theorem connecting the theory of synchronisation with the theory of resonances, by M. A. Cornu. A demonstration is given leading to the theorem: "A very small periodic force, varying with the time according to any law capable of development by Fourier's series, is equivalent in its action on a vibrating system, damping slowly and of almost the same period, to the simple pendular force represented by the terms of the first order of the series." The general character of synchronisation is pointed out, and the necessity of considering it where resonance is an important property is insisted on.—New experiments on the production of artificial diamonds, by M. Henri Moissan.—On interior pressure in gases, by M. E. H. Amagat. The author defines interior pressure as $\pi = \left(T\frac{dp}{dt} - p\right)$ and traces its value for carbon dioxide, ethylene, oxygen, nitrogen, air, and hydrogen. With the more perfect gases, π as the volume decreases first increases to a maximum, and then decreases; the maximum for hydrogen is reached at a comparatively low pressure, and π for this gas continues decreasing through zero to a negative value. With ethylene and carbon dioxide the maximum has not been reached.—On the time of departure for the Iceland fishery, by M. Jean Sicard.—Note on the solar observations made at the Lyons Observatory Brunner equatorial) during the second quarter of 1893, by M. J. Guillaume.—On rectilinear congruences and on Ribaucour's problem, by M. E. Cosserat.—On a characteristic property of the linear element of spiral surfaces, by M. Alphonse Demoulin.—On some points in the theory of functions, by M. Émile Borel.—On a theorem concerning harmonic functions of several real variables, by M. G. D. d'Arone.—Researches on the mode of combustion of ballistic explosives, by M. Paul Vieille. The author has studied the character of explosions under pressure, and will publish the results in a coming paper.—On the conductibility of discontinuous conducting substances, by M. Edouard Branly. Two hypotheses appear to explain the experimental results:—(1) The insulator interposed between the conducting particles becomes a conductor by the passage of a current of high potential, and the observed phenomena characterise the conductibility of the insulating substance. (2) It may be regarded as demonstrated that it is not necessary for the particles of a conductor to be in contact to allow the passage of even a feeble electric current; the distance for which the conductibility persists depends on the energy of the anterior electrical effects. In this case, the insulator serves chiefly to maintain a certain interval between the particles.—On the fusibility of isomorphous saline mixtures, by M. H. Le Chatelier. Tables and curves are given showing the temperatures of crystallisation of varying mixtures of pairs of isomorphous salts, – K_2CO_3 : Na_2CO_3, – Na_2SO_4 : K_2SO_4, – K_2CrO_4 : K_2SO_4, – Na_2SO_4 : Na_2CO_3, – K_2CO_3 : K_2SO_4, – NaCl : KCl, – KI : KCl.—On the assimilation of gaseous atmospheric nitrogen by microbes, by M. S. Winogradsky.—On the antitoxic property of the blood of animals vaccinated against viper poison, by MM. C. Phisalix and G. Bertrand. If viper poison be mixed with certain proportions of the defibrinated blood of an inoculated animal it fails to poison on injection.—Researches on the anatomy and development of the female genital armature in lepidopterous insects, by M. A. Peytoureau.—Observations on hypermetamorphosis or hypnodia among the cantharidiæ. The stage called pseudo-chrysalis, considered as a phenomenon of encystment, by M. J. Künckel d'Herculais.—Salivary glands of hymenoptera of the family of the Crabronidæ, by M. Bordas.—On some parasites of the Lepidodendra of Culm, by M. B. Renault.—Observations on the character of the relationship between platinum and its mother-rock, by M. Stanislas Meunier. The author points out the agreement between the views of M. Inostranzeff (*C. R.* January 29) and his own previously published observations. The metallic platinum must have been deposited in the interstices of peridotic masses by the interaction of hydrogen and platinic chloride vapours at a red heat, much below the temperature of fusion of the rock.—On a bed of apophyllite in the environs of Collo (Algeria), by M. L. Gentil.—Eruption of the volcano Calbuco, by M. A. F. Noguè.—Remarks on the earthquakes in the island of Zante during 1893, by M. A. Issel.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—The Student's Introductory Handbook of Systematic Botany: J. W. Oliver (Blackie).—A School Course in Heat 5th edition: W. Larden (S. Low).—Remarkable Comets, 2nd edition: W. T. Lynn (Stanford).—Celestial Motions, 8th edition: W. T. Lynn (Stanford).—The Voices of the Stars: J. E. Walker (Stock). The Universal Electrical Directory, 1894 (Alabaster).—Bourne's Handy Assurance Directory, 1894: W. Schooling (London).—Social Evolution: B. Kidd (Macmillan).—Manures and the Principles of Manuring: C. M. Aikman (Blackwood).—Société d'Encouragement pour l'Industrie National Annuaire pour l'Année 1894 (Paris).—A Memorial Work, chiefly on Botany and Zoology, in Commemoration of the Ninetieth Anniversary of Keisuke Ito, 2 vols.: T. Ito (Nagoya, Japan.

PAMPHLETS.—Imitation: a Chapter in the Natural History of Consciousness: Prof. J. M. Baldwin.—Stonyhurst College Observatory. Results of Meteorological and Magnetical Observations, 1893 (Clitheroe).—Erinnerung an Eilhard Mitscherlich, 1794–1863 (Berlin).—Guide to the Examinations in Elementary Agriculture and Answers to Questions, Elementary Stage, 1884–93 (Blackie).—The Internal Work of the Wind: S. P. Langley (Washington).—Revision of the Japanese Species of Pedicularis, L.: T. Ito (Nagoya, Japan).—Note on the Burmanniaceæ of Japan: T. Ito (Nagoya, Japan).—The Development of the Skeleton of the Limbs of the Horse: Prof. J. C Ewart.

SERIALS.—Journal of the Chemical Society, February (Gurney and Jackson).—Memoirs and Proceedings of the Manchester Literary and Philosophical Society, 1893–4. Vol. viii. No. 1 (Manchester).—Botanische Jahrbücher für Systematik, Pflanzengeschichte und Pflanzengeographie, Achtzehnter Band, 1 and 2 Heft (Williams and Norgate).—Brain, part 64 (Macmillan).—Engineering Magazine, February (New York).—Medical Magazine, February (Southwood).—Verhandlungen der Gesellschaft für Erdkunde zu Berlin, Band xx. No. 10 (Berlin).—American Journal of Mathematics, Vol. xvi. No. 1 (Baltimore).—Proceedings of the Royal Society of Edinburgh Session 1892–3. Vol. xx. pp. 97–160 (Edinburgh).—Transactions of the Royal Society of Edinburgh, Vol. xxxvii. parts 1 and 2 (Edinburgh).—Himmel und Erde, Februarv (Berlin).—Bollettino della Società Geografica Italiana, Serie 3, Vol. 6, fasc. 10 and 11 (Roma).—Indian Museum Notes, Vol. vii. No. 7 (Calcutta).—Journal of the Asiatic Society of Bengal, Vol lxii. part 2, No. 3 (Calcutta).—Journal of the Anthropological Institute, February (K. Paul).—Harvard University Bulletin, January.—American Meteorological Journal, February (Ginn).

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