THURSDAY, JULY 30, 1896.

INDUSTRIAL PHOTOMETRY.

A Treatise on Industrial Photometry, with special application to Electric Lighting. By A. Palaz, Sc.D. Translated from the French by G. W. Patterson and M. R. Patterson. Pp. x + 322. (London: Sampson Low and Co., Ltd.)

IT is remarkable, considering the importance of artificial methods of illumination, that there is so little literature dealing with the subject of light measurement treated from an industrial point of view. The introduction of electric lighting, and still more recently the success of the incandescent gas system, have aroused in the public a demand for more powerful methods of illumination than formerly were found to satisfy. But the problems involved in the economical arrangement of artificial light sources are very little understood. The scientific aspect of the subject has, indeed, hardly advanced at all during the last few years. Electrical engineers have made great efforts to increase by one or two per cent. the efficiency of dynamos intended for electric lighting purposes; but the study of arrangements of lamps and reflectors, to produce the best effect, has been almost ignored, with the result in many cases of losing quite half the usefulness of the light. There are in fact many problems involved in the economical distribution of lights, the importance of which is little recognised, and the solution hardly yet discovered. They are not to be found in text-books, and are only alluded to in isolated papers scattered about in the technical journals. The present work is a valuable compilation of facts and experiments obtained from the best technical authorities; and is the only book we know of in which this information can be obtained. The matter has not been hastily put together, since the book is a development of a long series of articles published by the author in *La Lumière Électrique* in 1887, and now enlarged and brought up to date.

The work is divided into six chapters, the first of which is introductory and deals with the principles of the subject. It contains some useful information, but is disfigured by an unfortunate choice of terms. The *candle-power* of a source of light, or the quantity of light emitted by it per unit solid angle, is here denoted by the objectionable term *total intensity*. Now the candle-power of a candle, like the horse-power of a horse, may vary from time to time; but whatever the vagaries of a so-called standard candle, the term candle-power is quite scientific. To replace this simple, accurate, and much-used expression by a vague, misleading, and unknown term, is most unfortunate, especially since, with all ordinary light sources, the candle-power varies with the direction considered, and one has to try and realise what the mean-spherical-"total"-intensity of a light source may be. The translators have, in fact, reproduced too literally the author's word intensity, which turns up over and over again in quite a needless fashion, as for instance in the phrase *intensity of illumination*, which is used to denote the quantity which English writers generally denote by the single word *illumination*. It is a great pity to make this latter word synonymous with quantity of light, as is done in this book. The ordinary idea conveyed in the assertion that an area is well illuminated, is not that there is a large quantity of light falling upon it, but that the quantity falling on it *per unit area* is considerable. The word illumination when used scientifically should correspond with this popular, and also precise, meaning. It should denote the density of the flux, not the flux itself, as the translators have unfortunately made it do.

The second chapter, on photometers, is a most valuable one. It is the longest in the book, and, like all the other chapters, is well illustrated. Photometers are divided into six classes, and over forty are described. The information given is by no means confined to instrumental details, for matters connected with the theory and practical use of the apparatus receive a good deal of attention. For instance, many useful details of various modes of making Foucault and Bunsen screens are given, and the theory of the Bunsen photometer is very fully discussed. It is remarkable how complicated this theory becomes when examined, and it does not appear that the theory given is too elaborate. As a matter of fact the optical properties of diffusing screens have been very little studied, and what has really been done in this direction seems to show that they cannot be treated in so simple a manner as is here assumed. This chapter is a useful summary of the different varieties of photometers, and leaves little to be desired. Naturally some of the instruments alluded to are of small value, while others depend on principles of a debatable character. This is especially the case in connection with heterochromatic photometry, a subject on which opinions seem to differ widely. The author is inclined to dissent from Helmholtz' assertion that "of all the comparisons effected by the aid of the eye between the intensities of different sorts of composite light, there is not one which possesses an objective value independent of the nature of the eye." Apparently many disagree with this view, since several photometers are described in which the judgment of the eye is only appealed to after the contending lights have been subjected to elaborate processes, which make it doubtful whether the decision obtained has any reference to their illuminating powers. Helmholtz' view seems to be the true one after all, and in fact it is difficult to dispute it in view of the property of the eye known as Purkinje's phenomenon, according to which the ratio of the illuminating powers of two lights of different composition varies if the intensity of each light is reduced in the same proportion. There is, no doubt, something to be said for adopting methods of comparison independent of the eye, owing to the fact that not only do two observers differ from one another, but a single observer obtains different results at different times. This, however, is largely a matter of practice, and we believe that Captain Abney, and others who have worked much at colour photometry, make little of the difficulty of judging when two illuminated surfaces of different colours are equally luminous. Certainly some of the photometers described here seem of doubtful value, in spite of the skill shown in their design, owing to the number of constants which have to be determined experimentally for each instrument; these constants apparently depending not only on the instrument, and the person using it, but also to some extent upon the nature of the lights to be compared. The most hopeful of all the instruments described for colour photometry seems to be the mixture photometer of Grosse. In this instrument the lights A and B, to be compared, are estimated not by directly measuring the ratio A : B, but by comparing two mixtures, A + *m* B and *n* A + B, where *m* and *n* are constants fixed for each instrument. Here the colour difficulty is got over by mixing the lights, and the relative illuminations of the surfaces compared is varied, partly by adjusting the distances of the lights, and partly by turning the eye-piece of the instrument, the working of which depends on the phenomena of polarisation. As far as one can judge from the description, this photometer appears to be an excellent one.

The third chapter, on photometric standards, is a full and trustworthy account of the efforts which have been made at different times towards obtaining a good standard of light. Its usefulness is increased by plentiful references to original papers, as is the case with the other portions of the book. The French work was published too soon to include the final results of the work of the English Committee on Light Standards, but almost all previous work is alluded to. A short, but good, chapter on the equipment of photometric laboratories is succeeded by one entitled "electric lights," which contains a large amount of photometric matter relating to electric lighting, that has not hitherto appeared in book form.

The sixth and last chapter of the book is reserved for the distribution and measurement of illumination. As illumination is the whole object of artificial lighting, it seems strange that the consideration of it should have been so much neglected. For every photometer that has been designed for the measurement of illumination, there are at least twenty for the measurement of candle-power. The present book is, we believe, the first to take up this subject as an important part of photometry; and the ordinary reader, who has not followed the subject in the technical press, will find here much which is quite new to him. It is a subject of increasing importance, and since the issue of the French edition of this book some valuable work on it has been done by Trotter and Blondel, which should be incorporated when a new edition is called for. It is somewhat remarkable that no allusion is made to the diffused system of lighting by inverted arc lamps, or corresponding arrangements of glow lamps, in which the lights themselves are hidden from view, the rays being directed to walls and ceilings for subsequent diffusion downwards. This system has been used on the continent and in this country, especially for the lighting of factories, and its use is extending. It is in reality one of the most efficient systems of lighting, although any one seeing it for the first time is apt to think it weaker than it really is. A description of it should certainly find a place in a book such as this. There is, however, a large amount of new matter given in this chapter, and the only fault to find is a minor one, and one for which the author is not directly responsible. In quoting the different writers on this subject, a number of terms have been introduced which are needless, confusing, and badly chosen. Such terms as "volume of illumination" and "surface of illumination" are quite needless, while the phrase "useful effect of illumination" is a very bad and misleading name for a quantity which is of doubtful importance. It follows from the definition that the "useful effect of illumination" is always greater than the illumination itself, and that it increases rapidly as the distance from the light is increased, until at an infinite distance from the light the "useful effect" of its illumination is infinite. "This result was easy to foresee," writes the author, who asserts that the criticisms to which it has been subjected "are not well founded"; and calculations are then made by means of which "the volume of useful effect," the "surface of useful effect," and the "mean useful effect" may be obtained by any one anxious to know.

In spite of a few minor blemishes, the book is a thoroughly good one. It is well printed, well illustrated, and contains a mass of valuable matter which is not to be found elsewhere. Some useful appendices by the translators conclude the work. W. E. S.

THE NOTES OF BIRDS.

The Evolution of Bird-song. By Charles A. Witchell. Pp. x + 348. (London: A. and C. Black, 1896.)

THIS little work will be heartily welcomed by all ornithologists as the first elaborate attempt to deal in a scientific spirit with the very difficult subject of the utterances of birds. Considering the great amount of careful observation necessary to the formation of any theory on the subject, and the difficulty of recording such observation correctly and intelligibly, Mr. Witchell is to be warmly congratulated on his book. It is, in fact, a very welcome and agreeable call-note, addressed to h brethren of the craft, and urging them to come and test the flavour of the food its author has discovered. Should any of them be critical of details, as indeed in such a subject they inevitably must be, it is to be hoped that the call-note will not change into an "alarm"; for however much we may differ from Mr. Witchell in detail, we shall hardly be disposed to quarrel with the main line of his argument, and we shall be grateful to him for his work as a pioneer. Personally I am glad to acknowledge that during the present season of song I have derived the greatest benefit from this book, which fortunately appeared at the very time when fresh observation was most easy and agreeable.

Starting with Darwin's theory of the origin of voice, Mr. Witchell states his belief that it was first occasioned by fear or anger in combat, and that, consequently, the earliest cries were alarm-cries. He then proceeds to show that "the first call-notes of birds were probably mere adaptations of alarm-cries, the use of which was induced by the influence of mutual aid among associated individuals." Rapid reiteration of call-notes had a tendency to produce something in the nature of song; and Mr. Witchell is able to produce several good examples of this in species which are still incapable of any true vocal effort, though he gives one or two, *e.g.* the willow-warbler, about which I feel, as yet, uncertain. Closer observation on this point is much wanted, but I can quote one case, that of the tree-sparrow (not mentioned by Mr. Witchell), which may occasionally be heard in the spring constructing a quasi-song by linking call-notes together. Mr. Witchell is careful not to commit himself to the view that *all* songs were so developed; but it is quite possible that this view may eventually hold the field, if due credit be given to the inventive and imitative powers of the bird, which, having once found a voice, would naturally make constant use of it and thus develop its resources. As regards imitation, he has much to say later on; but throughout the book he seems to me to neglect what I should call *invention*, or the varying use of the voice for the mere pleasure of using it. I am pretty confident that many birds will go on uttering "call-notes," and even songs, not for any special immediate purpose, but to express a certain sense of comfort, and for the pleasure derived from the effort. A good example of this is the greenfinch, a bird in the interpretation of whose notes Mr. Witchell surprises me; but want of space forbids me to enter into details.

Next follows a chapter on certain noticeable incidents of bird-song, which may be strongly recommended to those who are beginning to make observations of the utterances of birds. They will find their attention directed to such points as this: that variation from the specific type of song is generally to be heard at the end of the phrase, and, conversely, that the first part of the phrases sung by allied birds have the most resemblance to common types. Or, again, that the call-notes and alarms of allied birds are more alike than are their songs—a point of great importance. A theory (p. 70) to account for the fact that the most voiceful birds are, as a rule, small, is also worth consideration.

But it is in the last three chapters that we find the most original and interesting contributions to the subject. Of these, chapters vii. and ix. are on the influence respectively of heredity and imitation, and between them the author has placed a shorter one on variation. The chapter on heredity, though it would be the better for a thorough revision, both in the ordering and expression of the matter, is nevertheless of great importance. Here Mr. Witchell endeavours with success to trace the influence of the principle of heredity by comparing the notes and songs of allied species, so as to provide fresh scientific reasons for their connection in classification. As might be expected, he finds more to the purpose in call-notes and alarms than in songs, and more in the first phrases of song than in the conclusions. Some things in this chapter may astonish us, such as the statement that the buntings are more closely allied by voice to the pipits than to the finches (p. 121), and the whole of a paragraph on p. 113, in which the songs of nightingale, lesser whitethroat, and cirl-bunting are declared to resemble each other; but the general value of this part of the work is beyond question, and cannot fail to act as a fresh stimulus to many field ornithologists.

In chapter ix. Mr. Witchell discusses his second leading principle in the development of song, viz. imitation, whether of the voices of other birds, or of prevalent sounds in fields and woods. This is, I think, his favourite topic, and the one which he has most carefully elaborated by observations and records. He has gone far beyond any previous writer in the wide range of result he ascribes to this influence, and his conclusions as to the imitative capacity of many species will have to be most carefully tested. For the last two months I have been endeavouring to test them with varying result. As regards the thrush and the robin, he has already convinced me that they mimic much more than I had suspected, though I cannot detect in these songs more than an occasional imitation of which I can be *quite certain*. In others, such as those of nightingale, redstart, whitethroat, &c., I can as yet hardly detect any at all, though I have been listening to these voices constantly and carefully for more than twenty years. As far as my own experience goes, I should be disposed to think that Mr. Witchell often exaggerates superficial and accidental resemblances; but on the other hand, I can readily grant that his ear may be more accurately trained than mine for the purpose of detecting them. And in any case I must refrain from detailed criticism, which can be but the pitting of one man's experience against that of another.

The short chapter on variation might well be amplified. Not only do many birds show dialectic variation in different localities, but in the same locality the singers vary from each other, and even the individuals constantly vary from one minute to another, as I have often observed this spring. And yet the specific type is always preserved, which is owing in great degree to the peculiar tone or *timbre* of the vocal instrument of the species—a point to which I hope Mr. Witchell will turn his attention more closely than he seems yet to have done.

It gives me pleasure to sign a notice in which I hope I have done justice to the merits of this work, for in a book published a year and a half ago, I alluded to Mr. Witchell's theory, as it was then known from papers in the *Zoologist*, with somewhat scant respect. Some fanciful conclusions to which he formerly gave prominence, have in this volume retired modestly into the background.

W. WARDE FOWLER.

THE STRUCTURE OF MAN.

The Structure of Man: an Index to his Past History. By Prof. R. Wiedersheim, translated by H. and M. Bernard. 8vo, pp. xxi + 227. (London: Macmillan and Co., 1895.)

THIS book, which is a translation of Prof. Wiedersheim's "Der Bau der Menschen," by H. and M. Bernard, has the advantage of a preface and notes by Prof. G. B. Howes. As the preface states, the object of the work "is an endeavour to set forth the more salient features in the anatomy of man which link him with lower forms, and others in that of lower forms which shed special light on parts of the human organism." Such books as this give to the scientific study of anatomy much assistance by calling attention to the interesting deductions which may be made by a careful study of the different variations met with in the dissection of man and animals. In order that such deductions may be placed on a firm basis, it is necessary to have careful observations recorded in a very large number of cases, and in the English preface of Prof. Wiedersheim's book a special tribute is paid to the work carried out in the different anatomical schools through the "Collective Investigation Committee of Great Britain and Ireland." The English translation has in a great many places been added to, and brought up to date in notes by Prof. Howes. Some of these additions are exceedingly valuable in themselves, and further, their practical use is increased by the fact that they give references to the most recent literature on the subjects with which they deal. The plan of the book has been well thought out, and its arrangement is such as to render the search for information contained in it an easy one. Special chapters are set apart for the integument and tegumental organs, the skeleton, the muscular system, the nervous system, the sense organs, the alimentary canal and its appendages, the circulatory system, and the urinogenital system. The arrangement of the matter in each of these chapters is further carefully classified. In certain places the terms used lack the accuracy which is essential to a work on human anatomy, thus (p. 91) on the "comparison of the fore- and hind-limbs of man," to speak of the leg and arm of the adult as "opposite extremities" is vague and inaccurate. Again, in the description of the lower end of the humerus (p. 77) confusion is caused by the application of the term "ent-epicondylar" foramen to the occasional perforation of the olecranon fossa, instead of confining this name for the foramen partially enclosed by the ent-epicondylar process, which is sometimes present in man. The theories put forward in some parts of the book to account for facts observed in man, seem scarcely adequate; thus, for instance, on p. 38 we are told "the shifting of the centre of gravity towards the dorsal side explains why the vertebral ends of the lowest ribs are so firmly attached." Yet a very similar condition of the more posterior ribs obtains in quadrupeds, in which animals a shifting of the centre of gravity towards the spine does not occur. In another place (p. 55) it is stated that in lower races, as in the apes, the process of obliteration of the cranial sutures beginning in the frontal region and proceeding backwards "naturally causes an earlier limitation in the growth of the anterior lobes of the brain; whereas in the higher (white) races, when the fronto-parietal suture disappears only after the obliteration of the parieto-occipital one, these lobes are capable of further development." The obliteration of the sutures in the frontal region does not necessarily limit increase in growth of the frontal bones, much less that of the contained brain, and further, it has been shown that the frontal lobes do not in their growth vary with the changes in position of the fronto-parietal suture. The posterior boundary of the frontal lobe—fissure of Rolando—has a relatively constant position during brain growth, so that a relative increase in size of the frontal lobes, in white races, does not take place during the time that certain of the cranial sutures are closing, or even after birth. In the chapter on the nervous system, it is a pity that the old and superseded observations of Möller are retained, and we read, "Man differs from the Anthropoids in the preponderance of the frontal lobe and, to a lesser degree, of the occipital lobe, and in a corresponding backward extension of the temporal lobe. The parietal lobe is about equally developed in the brains of man and Anthropoids" (p. 131). As a matter of fact the great extent of the parietal lobe, together with a corresponding decrease of the occipital lobe, is a human characteristic. In the Anthropoids the upper part of the posterior boundary of the frontal lobe is relatively further back than in man. It is a curious fact that Prof. Wiedersheim's book should adhere to the old view, that a well-marked occipital lobe is a human characteristic, since it has been definitely shown that this part of the brain, which was at one time denied to apes, really attains in them its greatest relative development, and further, it is in the lower apes that a maximum is reached.

The presence of numerous illustrations, and of a glossary of the zoological terms used, in spite of its many failings, is sure to render this interesting and easily read translation of Prof. Wiedersheim's book very popular.

A. F. D.

OUR BOOK SHELF.

The Official Guide to the Norwich Castle Museum. By Thomas Southwell, F.Z.S. Pp. 294. (London: Jarrold and Sons, 1896.)

"THE value of a museum will be tested not only by its contents, but by the treatment of those contents as a means of the advancement of knowledge." This remark of Sir William Flower's is the key-note of the Committee of the Norwich Castle Museum, and in consonance with it the admirable guide-book at present before us has been constructed. The book is an interesting and useful guide to the collections in the Museum; it is not merely a catalogue, but a popular natural history in which the specimens in the cases are used as illustrations. Assisted by this guide, sightseers will pleasantly acquire a knowledge of the leading characteristics of the different groups of animals, and students will gain a large amount of sound instruction.

The scientific value of the book lies in Mr. Southwell's orderly review of the natural history specimens in the Museum. This forms the greater part of the contents; but there is also an historical account, by the Rev. Wm. Hudson, and a description of the collection of pictures, by Mr. G. C. Eaton.

The Museum was founded in 1825, and it existed as a private institution until 1894, when it was taken over by the Corporation, and established in Norwich Castle. The scheme for the conversion of the Castle, which had been condemned as a prison by the Prison Commissioners, into a museum and recreation grounds, was due to Mr. John Gurney, who died in February 1887. Mr. Gurney gave £5000 towards the scheme, which nucleus was afterwards increased by subscription to £14,389. The new home of the Museum collections was opened two years ago, and it is a credit to the Norwich Corporation and people. Very few local museums are better arranged than the one at Norwich, and in none is the educational object of the institution kept more in mind. To say that Mr. Southwell's guide is worthy of the Museum is, therefore, equivalent to stating that it possesses all the features which will make its readers appreciate to the fullest extent the *utile et dulce* of the collections.

Latitude and Longitude: How to Find them. By W. J. Millar. (London: Charles Griffin and Co., 1896.)

IN this concise little book the art of navigation is treated from an elementary standpoint. Commencing by explaining the meaning of a few mathematical expressions, including triangles, the author goes on to trigonometrical ratios and logarithms, and shows how they are brought into use for the purpose of finding a ship's position. The errors that have to be corrected are explained, as well as the determination of time and the use of the sextant.

The theory of the every-day work at sea, and also of lunar distances and Sumner's method, is given, so that with a small amount of mathematical knowledge a student of navigation can master the chief problems required to find the latitude and longitude at sea.

O. L.

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.*]

The Utility of Specific Characters.

I HOPE that my friend Prof. Lankester will forgive me if I find myself unable to accept the version which he has published of a portion of the remarks which I made at the recent discussion at the Linnean Society.

I entirely agree with Mr. Mivart, that the "Problem of Utility" really involves the validity of the Darwinian theory.

I stated in my remarks what I have often stated before, that I was more and more confirmed in the belief that specific characters in flowering plants are utilitarian. I showed that this was obviously so in familiar cases. If time had allowed, I might have strengthened my position by reference to the large amount of important and convincing work which has been done in this direction in other countries. At home, for reasons which are not far to seek, this kind of research is now almost entirely neglected. The result is that the Darwinian theory of organic evolution seems hardly to have a convinced supporter left except Mr. Wallace. In its place we have the "Physiological Selection" of Dr. Romanes, the "Discontinuous Variation" of Mr. Bateson, and, last of all, the extended "Correlation Principle" of Prof. Lankester. A common feature of each is their more or less definite rejection of the principle of utility as accounting for specific characters.

I was examining with Mr. Darwin, at Kew, a collection of Pitcher-plants (*Nepenthes*). The specific differences lie mainly in the appendages of the pitchers. I hazarded the remark that it seemed hopeless to attempt to explain these on teleological grounds, and that it was difficult to believe that the differences were not due to merely fortuitous variation. Mr. Darwin replied, that he was not prepared to admit it. He gave two reasons: (1) that many plant-structures which at first sight it was scarcely conceivable could be adaptive, had been proved to be so in the most unexpected manner; (2) that to assume that phenomena were not susceptible of explanation, was to shut the door to discovery.

Now I stated, perhaps unnecessarily, that the two reasons were not on the same plane. The first implicitly asserts an inductive principle, the probability of which, it seems to me, subsequent research confirms every day; the second, I described as of a moral kind. It would have been better to have spoken of it as *ethical*, or as a counsel of scientific prudence. In point of fact, I never used the word "immorality"; that was imported into the discussion, and I think in a somewhat sarcastic spirit, by Prof. Lankester himself.

A more serious point, however, is this. Prof. Lankester quotes me as the authority for the statement that Mr. Darwin "appears to have deprecated . . . the invocation of this theory of correlation as an explanation of cases of apparently useless parts in animals and plants." Now I made no reference whatever to correlation, which I do not think enters into the particular case I referred to. The question I put to Mr. Darwin amounted simply to this:—"Is it probable that these specific differences will turn out to be adaptive?" And his reply was, in effect: "I think it is."

I confess that the use to which Prof. Lankester has turned the correlation principle fills me with some surprise. As with every difficulty which is from time to time brought up against the Darwinian theory, it will be found that Mr. Darwin has thoroughly considered the matter himself, and has said pretty much all that is to be said about it. The whole subject is exhaustively discussed in the twenty-fifth chapter of "Animals and Plants under Domestication." He treats at considerable length "the cases in which we can partly understand the bond of connection," and then gives more briefly "the cases in which we cannot even conjecture, or can only very obscurely see, what is the nature of the bond." This is characteristic of Mr. Darwin's fairness; he adopted precisely the same method in regard to cases which seemed to make for Lamarckism, and were not readily explicable by the principle of natural selection. Most of these have been since cleared up, and I do not doubt that the same thing will happen in regard to correlation. The *nexus*, which is now obscure, will sooner or later be revealed.

The animal organism is a "complex" which, in the vast majority of cases, we are far from understanding. It has undergone in a high degree what Mr. Herbert Spencer calls "integration." It is not surprising, therefore, that organic correlation has been obscured. Prof. Lankester lays it down, that "presumably also plants" are in the same predicament. But I do not admit this. In plants integration has not been carried to anything like the same extent. For many purposes of biological research I therefore hold with Mr. Darwin, that plants are better subjects of investigation than animals, because the phenomena are less complex.

The result is that in plants most cases of correlated variation are at once explicable. All the appendicular organs are homologous. A variation which affects one runs through the whole. Amongst many thousand Snowdrops from Asia Minor, grown at Kew this spring, a few had exceptionally broad leaves; this was accompanied by a corresponding dilatation of the perianth-segments. There is a variety of the common oak with marbled foliage. A tree at Tortworth has borne acorns, and these are striped. At first sight it might seem odd that a variation in foliage and fruit should be correlated. But it is not so: the marbling is due to the partial suppression of chlorophyll in those portions of the ground-tissue which are exposed to light; and this tract of tissue is continuous in the leaves and the carpels.

I cannot but think that even in animals, of which I know little, Prof. Lankester is building on a rash foundation in attempting to generalise widely from cases in which, in the light of present knowledge, the obvious but, as he thinks, useless distinctive character may (or may not) be linked with the unobvious adaptive variation.

It seems to me that when an explicable correlation persists in a species, we are not justified in assuming any part of the chain to be useless. The whole is, in fact, part of the specific character; and this was what I took to be Prof. Weldon's point. I do not see that our ignorance of the nature of the "bond" makes any difference; nor do I see how Prof. Lankester extricates himself from the effect of his admission that the parts of the chain are always subject to selection. It seems to me that we are justified in inferring that what survive as specific differences do so because they are useful.

I doubt if the case which has so impressed him is a very satisfactory one. He thinks that in "tropical regions" the colour of the skin is linked with the chemical activity of the leucocytes in the blood. Assuming that immunity from fever is due to the latter, he infers that the former is not a "useful character." This is, in the present state of our knowledge, taking a good deal for granted. But I frankly admit that such a case, if completely established, would give the utility of specific characters, and with it the Darwinian theory, a serious blow; and Prof. Lankester would have the satisfaction of arriving at the same result as Dr. Romanes, but by a different path.

But is he sure of his ground? Mr. Darwin touches on the connection between "complexion and constitution," but does not appear to think the evidence points to any definite conclusion. Nor, I confess, do I, from such facts as are within my knowledge. I have sent a good many men from Kew to Africa, and the belief of my staff is that fair men enjoy better health than dark. But I do not consider that the data are sufficient. On the other hand, men of African descent, transferred from the West Indies to Africa, are said to be more susceptible to febrile maladies than the natives. Certainly the natives of India do not appear to enjoy any immunity from fever. "It is," says Sir Clements Markham, "by far the most prolific cause of death, carrying off . . . very many more than all other disease and accidents put together" (except cholera). I cannot but be impressed with the fact, because it was to combat this state of things that the Government of India introduced Cinchona cultivation, by far the most important enterprise in which Kew ever took part.

It appears to me that the relation of a stationary population to local febrile diseases is governed by natural selection, and has possibly nothing to do with epidermal pigment. The more susceptible die off, the more immune survive. Variation in the phagocytes would do the whole business. In this way disease and population reach an equilibrium. In some cases a disease actually attenuates, to recover its virulence when, as in the case of measles in Fiji, it reaches new ground. At any rate, I think Wells's theory can hardly be accepted as a scientific fact. But it does not follow that epidermal pigment is useless because one explanation of it seems to fail.

I must add a few words about specific differences. Some one has recently observed that Mr. Darwin has given no definition of a species. I do not propose to attempt the task. But the majority of botanists demand that a "good species" shall be distinctly marked off from every other by definite and tangible characters. The members of the group may either conform pretty closely to a common type, or exhibit a good deal of variation amongst themselves, and this variation may be sometimes indifferent, sometimes adaptive. Such a variable group is generally considered to contain species "on the make"; but the indifferent variation will be remorselessly, if slowly, brought to book by natural selection. A botanical species is then a discontinuous group marked off by characters which I believe to be adaptive.

I have a strong suspicion that zoologists have a different conception of a species from that of botanists. I once heard Prof. Huxley say roundly, at a meeting of the Linnean Society, that there were no such things as species at all. The subject under discussion was a group of *Salmonidæ*, and he said that if the forms were arranged in a row, it was a purely arbitrary matter how any one chose to cut it up into species. But the same state of things might be paralleled amongst plants—as, for example, in *Hieracium*. The occurrence of such cases is not incompatible with the fact that the majority of species probably admit of being sharply defined, and are, in other words, discontinuous. This is, at any rate, the case with plants, and I do not see why it should not be equally so with animals. If, however, zoologists cut their species finer, it is intelligible that they may find difficulty in recognising the distinctive characters as adaptive. Prof. Poulton, in the discussion at the Linnean Society, went the length of saying that he saw no objection to giving a name to every distinct form, leaving it to be afterwards decided if it were or were not entitled to specific rank. Such a proceeding, if general, would throw taxonomy into a state of chaos. It has been adopted by a few botanists; but by common consent their writings, though not without a certain interest as studies in variation, have been excluded from serious taxonomic literature. W. T. THISELTON-DYER.

Kew, July 20.

IN his letter, published in NATURE of July 16, Prof. Lankester has formulated with great clearness his views concerning the utility of specific characters; and he explains that his chief object in doing so is to draw attention to certain statements of mine, which he declares to involve a serious logical fallacy. While I am grateful for the courtesy with which Prof. Lankester has tempered his condemnation of my logic, I am still unconvinced; and the point at issue is so important that I am anxious to state, as clearly as I can, what my own position is. I may perhaps conveniently begin by quoting in full a passage from a former paper. Last year I wrote as follows:

"In order to estimate the effect of small variations upon the chance of survival, in a given species, it is necessary to measure *first*, the percentage of young animals exhibiting this variation; *secondly*, the percentage of adults in which it is present. If the percentage of adults exhibiting the variation is less than the percentage of young, then a certain percentage of young animals has either lost the character during growth, or has been destroyed. The law of growth having been ascertained, the rate of destruction may be measured; and in this way an estimate of the advantage or disadvantage of a variation may be obtained" (*Roy. Soc. Proc.*, vol. lvii. p. 381).

Prof. Lankester objects to this passage; and, if I understand him rightly, his objection may be stated in this way:—Admitting it to be proved that variation in a certain dimension, among young animals of a species, is associated with change in the death-rate, so that when this dimension increases the death-rate increases, and when it diminishes the death-rate diminishes; so that by ascertaining the magnitude of this dimension in a young animal you can accurately measure its chance of becoming adult;—admitting this relation to hold through a range of experience sufficient to form the basis of a reasonable induction, you have still no right to say that change in the observed dimension is a cause of the subsequent change in death-rate; for the two observed phenomena, namely the change in the observed dimension and the subsequent change in the death-rate, may alike be due to variation in some unobserved character, which alone is effective in causing change of death-rate.

In other words, you have a phenomenon, namely death-rate, preceded invariably by two or more phenomena of structure or function; and these are so associated, that from a known change in the antecedent group of phenomena, affecting always every member of the group, you can infer a change of known magnitude in the death-rate. Under these circumstances, Prof. Lankester thinks it legitimate to pick out one of these antecedent phenomena, and to speak of it as the only effective cause of change in death-rate, the other changes, although equally universal, being merely unimportant concomitants of this one essential change. He further finds something extraordinary in my logical position when I disagree with him, and considers every member of the group of correlated changes which invariably precedes change in death-rate as one of the causes of that change.

I have ventured to restate Prof. Lankester's position in my own words, in order to show what I believe him to mean. If I have in any way misrepresented him, I trust he will forgive me.

My own view seems to me identical with that held by a large number of persons, from Hume onwards; and for that reason I hope Prof. Lankester will not think I am indulging in an "empty wrangle" if I ask whether he accepts the following statement:

"We may define a cause to be *an object, followed by another, and where all the objects, similar to the first, are followed by objects similar to the second*, or in other words, where, *if the first object had not been, the second never had existed*. . . . We may . . . suitably to experience, form another definition of cause, and call it, *an object, followed by another, and whose appearance always conveys the thought to that other*. But though both these definitions be drawn from circumstances foreign to the cause; we cannot remedy this inconvenience, nor attain any more perfect definition, which may point out that circumstance in the cause, which gives it a connection with the effect. We have no idea of this connection; nor even any distinct notion of what it is we desire to know, when we endeavour at a conception of it" (Hume: "Inquiry concerning Human Understanding," § vii.).

When I have spoken of cause and effect, I have always endeavoured to use the words in accordance with the definition given in this passage or in Kant's extension of it; but Prof. Lankester seems to go beyond it. At least, the process of selecting one out of a group of universal antecedents, and calling that one alone the effective cause of the consequent, seems to me to involve precisely that knowledge which Hume and all his followers disclaim. For unless he knows "that circumstance in the cause which gives it a connection with the effect," how does Prof. Lankester pick out that one of the universal antecedents of an event which he chooses to call the cause? Such selection would have been impossible to Hume; and if Mill had regarded it as possible, he would hardly have defined a cause as "the sum total of the conditions, positive and negative taken together; the whole of the contingencies of every description, which being realised, the consequent immediately follows."

It is the assumption of the right to choose one out of a number of universal antecedents, and to regard this as the only cause of the consequent, which I have ventured to call illogical: and since Prof. Lankester has quoted Mill against me, I would ask him to read Mill's opinion of such a proceeding.

The prevalence of this practice in biological speculation tends more than any other habit to that neglect of the real complexity of the phenomena of life which Prof. Lankester himself so justly deprecates. For example, the contraction of the body of an amœba has been discussed of late years in two ways. Observers, following Prof. Lankester's method, have discussed the question, how much of the body of an amœba is the effective cause of its contractility? It is possible roughly to divide the body of an amœba (neglecting the nucleus) into an apparently more solid net-work or sponge-work, and an apparently more fluid substance in the meshes of this sponge-work. The question has been hotly debated, which of these two substances should be regarded as the essentially contractile element, the other being an unimportant concomitant. Each alternative has had its advocates, and neither party has convinced the other. Readers of NATURE are aware that a short time ago Prof. Bütschli attacked this question from the standpoint which I am here advocating: that is to say, he regarded each of the substances, invariably antecedent to contraction, as one of the causes of the contraction. By considering the changes in the relation between the two, Prof. Bütschli was at least enabled to make a dead model which imitates with remarkable exactness the phenomena of amœboid movement; while the suggestion that such movement falls into the same category as the change in surface-tension at the boundary between two not-living liquids with change in the constitution of either, is a most important step in the "explanation" of contractility in general.

Here then is a phenomenon which had for years been rendered more obscure by the attempt to fix upon one of its two universal antecedents as its effective "cause"; while some kind of explanation was at once forthcoming when both antecedents were taken into account. It would be easy to multiply examples of this kind; but perhaps the foregoing may suffice.

I would only now reiterate my hope that in trying to make plain my own position I have not in any way misrepresented that adopted by Prof. Lankester. W. F. R. Weldon.

Marine Biological Laboratory, Plymouth, July 18.

It appears to me that Prof. Weldon's argument, referred to in Nature of July 16 (p. 245), is accurately represented in the following illustration. It might be an established fact, although it is not in reality, that there was a constant correlation between baldness and short-sightedness. Suppose that it were so, and that in a country where conscription was enforced, short-sighted men were exempt from military service; that is to say, let us suppose that a test was applied to the eyes of all men at a certain age, and that those whose vision was not normal were rejected and allowed to return to the peaceful pursuits of civilian life. These rejected men would, on the hypothesis, be all more or less bald, and according to Prof. Weldon's position, it would be quite as correct to say that they were not in the army because they were bald, as to say they were rejected on account of myopia. Now it is quite true that the officers of the army medical staff might save themselves trouble by rejecting all bald-headed men, because, on the hypothesis, all such men would be short-sighted; but it would be obviously wrong to conclude that a good development of hair was essential to military efficiency.

Prof. Weldon argues that it is enough to prove that individuals of a species are selected according to a certain character, and that it is unnecessary to discover whether survival depends directly on this character, or on some other with which it is correlated. He seems to have concentrated his attention on the attempt to demonstrate directly the occurrence in nature of individual selection, in this peculiar sense, and to be temporarily indifferent to all other questions.

Prof. Lankester suggests that specific characters would be explained if it were proved that they were correlated with adaptive characters. It is of course perfectly true that if there were such constant correlations, then the survival of adaptive variations would involve also the survival of the indifferent characters connected with them. But the difficulty is to prove that in many cases there are *any* important differences of adaptation between allied species. It is easy enough to define the specific differences and specific characters; but to find any differences which correspond to differences in the mode of life, is often exceedingly difficult. It is true we find in most cases some differences in the conditions of life of closely allied species, but we do not usually find peculiarities of structure which can be said to be adapted to those differences. Who, for instance, can say what adaptation is present in the pilchard or sprat differentiating either from the other or from the herring? The question, therefore, is not whether indifferent specific characters are correlated with useful characters, but whether species of a single genus are distinguished from one another by any characters which can be proved to be useful or adaptive. The tongue and hyoid of the woodpeckers are beautiful adaptations; but are there any differences of selection value between one species of woodpecker and another? The denial of the utility of specific characters means, not merely that some specific characters are indifferent while others are adaptive, but that adaptations are not in the great majority of cases distinctive of species at all. Therefore, as the late Mr. Romanes often ably demonstrated, natural selection is not a theory of the origin of species, but only a theory of the origin of adaptations. The further objection, that a theory of selection is only of secondary importance in comparison with a theory of the origin of variations, I will not enter upon on this occasion. J. T. Cunningham.

College of Surgeons, July 17.

The Position of Science at Oxford.

Will you allow me a few lines in which to express my entire agreement with your recent article on this subject, if only to emphasise the fact that I am not the author of the article, and that the opinions there expressed are not those of an isolated individual. The reason for the comparative neglect of natural science at Oxford is that, however well-disposed some individuals may be, the college tutors and lecturers as a rule dislike it. They dislike it for two reasons. First, because it cannot be taught in the college parlours called lecture-rooms; and second, because they are, as a rule, ignorant—owing to their own defective education—of the nature and scope of the immense field of study comprised under the head "natural science." They do not know either the enormous educational value of natural science, or its vital importance to our national life and development.

And lastly, if they did know, there is no conceivable motive which could operate so as to induce them to sacrifice some of the rewards and educational domination, which are at present enjoyed by the long-established classical and historical studies, to newer lines of work in which the present beneficiaries and their academic offspring can have no share.

The situation is a "dead-lock," and only an intelligent Parliamentary Commission (if such is possible) can put matters on to a fair and healthy basis. Probably the scandal of the present paralysis of our beloved Oxford will have to become even greater and more outrageous than it is at this moment, before the necessary remedy is applied.

But happily the vitality of Oxford is indestructible. The misused and monopolised resources of Oxford will assuredly some day be devoted to the true purposes of a great University.

E. Ray Lankester, Linacre Professor, Oxford.

There are some points in the article on this subject in Nature of July 9, which call for comment. The defects pointed out are not, I believe, due to the causes mentioned by your correspondent. The fault lies mainly in the public schools. The lower forms of public schools are, as a rule, mainly classical, the division into sides, classical, modern and science, only beginning when a boy has finished about half his school career. The choice of sides is chiefly left by the parents to the masters, and since in the lower forms these masters have, as a rule, little sympathy with any kind of work which is not purely classical, boys of ability are drafted as a matter of course into the classical side. The boys who enter the science side are often the failures of the classical side, and unless special care is taken by the science masters, even they are kept at classics until it is hopeless to make them into respectable science scholars. Naturally there are many exceptions; some clever boys have enlightened parents, and others, early developing a taste for scientific matters, persuade their parents to allow them to give up the dead languages. There are also some classical men who admit that other subjects than their own have educational value. But the rule is for the able boy to be kept at classics, while his less favoured brother is sent to science. I know that this is the case at the five public schools with whose working I am familiar, and I have little doubt that the science masters of other public schools have the same experience. Occasionally able boys are recruited from the modern side, and it is these boys who are practically shut out from Oxford. However small the knowledge of Greek required for passing responsions may seem to a classical man, it is no light matter for a boy who has it all to learn in little more than a year, and who has much other work to do during the time. At Cambridge the necessary knowledge of Greek is almost nominal, and it is a pity it is not abolished altogether. If both Universities would substitute a good knowledge of German—so necessary for every scientific student—for the very imperfect and quite useless modicum of Greek which they now require, it would result in a great saving of time to many science students, and ultimately in raising the science standard at both Universities.

H. B. Baker.

In your article on "Science at Oxford," in Nature for July 9, you say: "It may be objected that every public school has one or more science masters of tried capacity, and that science is a compulsory subject in nearly all."

The first part of this statement may be correct, but I venture to demur to the second. Certainly at one school I could name, where over £10,000 has been spent on scientific equipment, science is not a compulsory subject; and, in fact, it is practically impossible to get science teaching on the classical side.

It would be interesting to know how far science is compulsory in the various public schools. I have no doubt there are others, but I only know of two—Eton and Clifton—where it is.

"A PARENT."

Discharge of an Electrified Body by Means of the Tesla Spark.

IT has been shown that a body charged with electricity may be discharged by means of the rays from a Röntgen bulb. I find, also, that an electrified body is rapidly discharged by the influence of a high-frequency spark, such as that produced by the Tesla apparatus. The discharging action was shown in this way. A high-frequency spark was produced between two rather blunt points, one inch apart in air, no bulb being used. A gold-leaf electroscope, placed far away from the influence of the spark, was used to test the electrical condition of the charged bodies—viz. a stick of sealing-wax and a rod of glass. The sealing-wax was rubbed, and the electroscope indicated that it was well charged. It was again rubbed, and then brought to within a foot of the points, and by means of a key in the battery circuit the Tesla coil was thrown into action for an instant. On testing the sealing-wax rod with the electroscope, it was found to be entirely discharged. A similar experiment was next made with a glass rod; the glass rod was entirely discharged by the Tesla spark. From a previous experiment, it was seen that the electrification of the rods was dissimilar. The influence, then, of the high-frequency spark is to discharge electricity of either sign.

FREDERICK J. SMITH.

Oxford, July 17.

On the Occurrence of the Pelagic Ova of the Anchovy off Lytham.

HITHERTO no free eggs of this species have been procured in Britain, though Mr. Jackson found ripe females off Southport in June 1878. Day observes, in the "Brit. Fishes," that the anchovy spawn off our coasts in September and December; though in June specimens have been found with enlarged ova, and so tender that they burst on the slightest interference. On June 26 last, however, Mr. R. L. Ascroft, of Lytham, obtained certain ova in the tow-nets used off Lytham Pier, which he courteously sent for examination in a solution of formalin. These eggs agree in all respects with the descriptions and figures of Hoffman, Wenckebach, and Raffaele, though somewhat larger.

Prof. Hoffman found that the anchovies of the Zuyder Zee were ripe in the months of June and July, and that the eggs were of an oval form, and about 1 mm. in length. In July 1886, Wenckebach captured the same eggs in his tow-nets, and hatched them on the third day. The egg is ovoid, and the yolk reticulated as in other clupeoids, such as the sprat. Raffaele procured the same egg at Naples—from May to September—and also ascertained that hatching took place after two or three days. He gives the long diameter of the egg as 1·15 to 1·25 mm., and the shorter at 0·5 to 0·55 mm. The larva is provided with a reticulated yolk of little depth, but of great length, extending, indeed, considerably beyond the middle of the body, while the notochord is unicolumnar. In two or three days after hatching the yolk had greatly diminished, the pre-anal fin-membrane was augmented, and the dorsal had likewise passed much further forward. The buccal aperture had also opened, and four branchial arches were visible. The yolk had completely disappeared about the fourth or the fifth day, and pigment occurred in the eye and along the dorsum. The post-pyloric portion of the gut was transversely ridged.

The eggs sent by Mr. Ascroft were, for the most part, advanced; the embryo occupying the long axis of the egg, as usual in such cases, and as shown by Raffaele. The long diameter ranged from 1·295 to 1·447 mm., the shorter diameter being almost constant at 0·685 mm.; they are thus larger than those from the Mediterranean and the Zuyder Zee.

Interesting accounts of the occurrence of anchovies off the British shores have been given by Prof. Ewart and Mr. Cunningham, and they would seem to be by no means so rare as at one time supposed. Prof. Hoffman thought that very rapid growth occurred during the first year of the life of the anchovy, so that those spawned in June and July reached a length of 12 cm. at the end of October, and Dr. Hoek appeared to agree with him. Ehrenbaum, however, asserts that the young anchovies referred to are in their second year; and this would be more in harmony with what is known of the herring, the pilchard, and the sprat. This author considered that the anchovy breeds when two years old.

W. C. M.

Gatty Marine Laboratory, St. Andrews, July 16.

Information on Scientific Questions.

DR. BROWN GOODE is quoted in NATURE of July 16 (p. 252), as saying "he cannot think of any scientific subject regarding which a letter, if addressed to the scientific bureaus in Washington, would not receive a full and practical reply." I infer from this that the replies are prompted by the courtesy of the officers of the various departments, and that the public of the United States possess no right to demand them. If this is so, surely Dr. Brown Goode's scoff at British Government departments is disingenuous, to say the least of it.

But though we have no *right* to apply for information to Government departments, it must have been the experience of great numbers of people that information may be most readily obtained, and that only very exceptionally does a public officer fail to reply to any reasonable inquiry relating to his own branch of science—or art. I have myself made frequent inquiry of officers of the British Museum, both at Bloomsbury and South Kensington, and in every case (save one) have had courteous and satisfactory replies from those to whom my inquiries were addressed. I have received similar treatment from the Department of Science and Art, from the Society of Antiquaries, from the Board of Trade, from the Agricultural Department, from the Royal Academy, from the School of Mines, and from other bodies which, though in this country they are of a pseudo-private character, would in the States probably come under some public department.

It may be that the experience of others has been less favourable than mine; but this I find it difficult to believe, and unless Dr. Brown Goode means us to understand that the Washington bureaus may be peremptorily applied to for information, it would be seemly to withdraw the implied charge of discourtesy which he has levelled at our public officers.

I would further point out that in many of our country towns and cities there exist municipal museums, to which local inquiries are first addressed, whence information may be obtained, at, I confess, considerable inconvenience to the curators. I have myself had inquiries on all sorts of questions from agriculturists, medical men, colonists, genealogists, artificers, tradesmen, youthful collectors, and the general public, some of which have taken me two or three hours, and occasionally a microscopic examination of specimens, to solve, and doubtless many other persons could tell of similar experiences. It is quite remarkable how entirely the public have adopted the view that a curator of a museum is a fit and proper person to consult upon any and every subject; but my experience leads me to think that curators have brought this condition of things upon themselves.

Exeter, July 20. JAMES DALLAS.

Horary Variation of Meteors.

DR. DOBERCK, of whose paper an interesting abstract was given on June 25, informs us that shooting-stars decrease in average magnitude from evening to morning, their duration and length of path decreasing with the magnitude, while the velocity increases as the magnitude diminishes.

This larger evening magnitude is said to be "owing to the fact that the meteorites are heated to incandescence nearer the earth in the evening than in the morning," a fact deserving further explanation.

In the morning we stand on the front of the earth in her orbital motion; the earth then generally meets the meteors with the double velocity of their two motions. In the evening the meteors are overtaking the earth with a slower motion, the difference of their velocities. In the morning, therefore, the meteors enter the atmosphere with double velocity, and are burned up before nearly reaching the earth. In the evening, the slower motion enables them to penetrate further through the atmosphere before becoming incandescent.

So also most aerolites fall in the evening hours, although shooting-stars are most numerous in the morning.

The impalpable air shields the earth from those meteorites whose impact would be dangerous, burning them up by their very velocity, while giving passage to those whose slower motion renders them comparatively harmless.

G. C. BOMPAS.

London, July 18.

AUGUST KEKULÉ.

BY the death of this eminent chemist at the age of sixty-six, which took place on July 13, science loses one of her most distinguished votaries. It is only four years ago since a remarkable demonstration was held in Bonn in celebration of the twenty-fifth year of Kekulé's professorship in that University. Two years previously, in March 1890, a similar rejoicing had been held in Berlin in honour of the twenty-fifth anniversary of the promulgation of the benzene theory by its illustrious author. It appears that Kekulé was intended by his father to have been an architect, and for that purpose he was sent to Giessen to become proficient in the subject after having undergone a preliminary training of the ordinary kind at the Darmstadt Gymnasium. At Giessen he came under the powerful spell of Liebig, and having attended some lectures on chemistry by that great master, his inclination towards the adoption of this science as a profession instead of architecture appears to have received a strong impulse. After a short period of probation at the Darmstadt Polytechnicum, where he tells us he learnt chemistry under Moldenhauer, and spent his leisure in lathe-turning and modelling in plaster, he returned to Giessen and entered as a student under Liebig and Will. Even at this stage of his career he appears to have been capable of rendering material assistance to his master in the experimental work being carried on in connection with the familiar "Letters on Chemistry," in which Liebig includes the name of Kekulé among those of many other chemists now well known in science, in acknowledgment of the services rendered by the future founder of structural organic chemistry. That Liebig thought highly of his pupil may be inferred from the circumstance that he very nearly received the appointment of assistant in the Giessen laboratory, then renowned throughout Europe for the chemical work being carried on there. Instead of remaining at Giessen, however, young Kekulé went to Paris, and having sat at the feet of Regnault, Frémy and Wurtz, he was casually attracted by a course of lectures on chemical philosophy advertised by Gerhardt, who had resigned his professorship at Montpellier, and was giving private courses of instruction in the French capital. Gerhardt appears also to have recognised the capabilities of his student, and an intimate personal friendship sprang up between them. It is probable that this contact with Gerhardt acted as a stimulus in developing the particular faculty as a theoriser which must have been inherent in Kekulé, and which found expression in all his later work. From Paris, where he declined an invitation to become Gerhardt's assistant, he went for a short time to Switzerland as assistant to Von Plantu in the Castle of Reichenau. After this Swiss sojourn, and chiefly at the instigation of Bunsen, he accepted an offer from the late Dr. Stenhouse, then at St. Bartholomew's Hospital, and for a time this country had the honour of fostering young Kekulé. The bent of his mind in the direction of chemical theory is well brought out by his confession in later life that he did not derive much profit from his experience at St. Bartholomew's; but having become acquainted with Williamson, who had just completed his classical work on etherification, he appears to have found a more congenial outlet for his energies in the school of thought being evolved by that investigator and Odling, and which he declared, in 1892, to have been an excellent school "for the encouragement of independent thought." While in this country an offer was made to Kekulé that he should remain here as a technologist, but the Fatherland had greater attractions for him; his great ambition was to become attached to a German University, and he started a small laboratory in the house of a corn merchant in the main street of Heidelberg, where he received pupils. In these days of palatial laboratories, it is interesting to recall that in this little kitchen Kekulé carried out his work on the fulminates, and that Baeyer, then one of his pupils, conducted his researches on cacodyl. It is not the laboratory that makes the chemist!

Kekulé's first call as ordinary professor was to Ghent, where the Belgian Prime Minister was instrumental in getting him a modest laboratory; and here for nine years he worked with a success that can be measured by the fact that, in addition to Baeyer, he numbered among his pupils Ladenburg, Victor Meyer, Wichelhaus, and others whose names are as household words in the annals of chemical science. From Ghent he was "called" to Bonn, in which University the magnificent laboratories grew under his inspiring influence, and where he remained till the last, adding to the lustre of his reputation and shedding the light of his intellect over that country in which modern chemistry appears to have found its headquarters.

As an experimentalist, Kekulé's contributions to science are not great as compared with the enormous influence which his genius for theorising has exerted upon the development of the science of the century. His greatest and most precious gift was his power of penetrating into the inner mysteries of molecular constitution, and it is through this work that his name will ever be revered. It was Kekulé who first gave definite form to Frankland's conception of valency, and his application of this idea to the study of the carbon compounds was nothing less than epoch-making. Out of this conception grew the famous theory of cyclic compounds, which has been prolific to an extent almost unparalleled in the history of pure science, and which from the practical side has made Germany what it is in the domain of organic chemical technology. If the life-work of any chemist of our age need be quoted as a standing protest against the *cui bono* attitude of mind which we in this country are still suffering under, and which relegates abstract theoretical studies to the realms of "academic" thought remote from human interests, let the speculations of August Kekulé be put forward as an answer crushing and complete for all time.

The present writer never had the privilege of coming into personal contact with the master-thinker who has so recently passed away. His geniality of disposition appears to have endeared him to all who came under his influence. The chemists of this country join with those of the Fatherland in mourning over the gap that has been caused in their ranks. R. M.

NOTES.

THE International Geological Congress will hold its seventh session at St. Petersburg at the end of August next year, under the acting presidency of Dr. A. Karpinsky, and with the Grand Duke Constantine as honorary president. The session will continue about a week, and the proceedings of the Congress will not be divided into sections, as at Zürich, but will be devoted chiefly to the discussion of broad principles. Extended excursions are announced, the most important being to the Ural Mountains before, and to the Caucasus after, the meeting at St. Petersburg. Shorter excursions have also been arranged to Finland and elsewhere. Geologists who propose to attend this meeting should send notice of the excursions in which they wish to participate, before next October, to the General Secretary of the Congress. The Emperor of Russia has decided, on the favourable report of the Minister of Public Ways, to grant to all geologists duly enrolled for the meeting, free first-class railway tickets during their sojourn in Russia.

THE forty-second annual meeting of the German Geological Society will be held at Stuttgart, on August 9-12. Another important annual meeting is that of the German Anthropological Society, which takes place on August 3-6, at Spires.

The Royal Institution of Science, Letters, and Arts of Venice offers a prize of 3000 lire for the best essay on the alluvial matter brought down from the Alps by one of the principal rivers of Venetia. The competition remains open till December 31, 1896.

An effort is to be made to induce the Prince of Wales to place himself at the head of the movement for celebrating at Bristol, in June next year, the 400th anniversary of the discovery of North America by John and Sebastian Cabot, who sailed from Bristol. It is hoped that the foundation-stone of the memorial will be laid by the Prince of Wales simultaneously with one laid in Canada.

The International Congress of Applied Chemistry was opened in Paris on Monday. Sixteen hundred delegates were present, of whom six hundred were from other countries. M. Berthelot, Perpetual Secretary of the Academy of Sciences, was elected President of the Congress, and delivered a powerful inaugural address, in the course of which he dwelt particularly on the relation between pure and applied science, the remarkable results obtained by the alliance of physics and chemistry, and the beneficent part that science has played in the history of the past three-quarters of a century.

The Russian Geographical Society has awarded this year its Constantine medal to M. A. Rykacheff, for his work in the domain of physical geography. Beginning in the year 1874 with a work on the distribution of atmospheric pressure in Russia, he continued to publish a series of researches on the diurnal variations of pressure, the prevailing winds of the Caspian and the White Seas, the tides in the atmosphere, the freezing and thawing of the Russian rivers, the variations of the levels of rivers in Middle Russia, in connection with variations in the amounts of rain and snow, the diurnal variations of temperature over the tropical oceans, &c. A full list of M. A. Rykacheff's works, mostly written in German and French, is given in the yearly Report of the Society. The Count Lütke medal has been awarded to Admiral Makaroff, for his work on the temperature and density of water in the Northern Pacific, based on the measurements made in 1886–89 on board the *Vityaz*. His maps of the distribution of surface temperature in August, and of temperature at a depth of 400 metres, are especially worthy of notice. The Prjevalsky prize was awarded to M. Berezovsky, for his explorations of the northern borderlands of Tibet. A Prjevalsky medal was awarded to J. A. Schmidt, for his twelve geodetical expeditions to different parts of Central Asia and Siberia; and one to Dr. H. A. Fritsche, for his magnetic measurements in China, Mongolia, Siberia, and Russia. Two small gold medals were awarded to F. F. Müller, for his magnetical work in East Siberia; and to A. A. Lebedintseff, for his researches into the chemical composition of water in the Black and Azov Seas. Eighteen silver medals were awarded for various works of lesser importance.

A full description of the cell invented by Dr. W. W. Jacques, for the production of electricity by the consumption of carbon, is given in the July number of the *Engineering Magazine*, by Mr. G. H. Stockbridge. The apparatus consists of a pot of pure iron surrounded by a suitable furnace and containing caustic soda, in which hangs a rod or cylinder of carbon. The carbon must be in such a state that will serve as a good conductor of electricity. Gas, carbon, and charcoal are available without special treatment; but anthracite coal has to be baked to give it the requisite conductivity, and bituminous coal needs, for the same purpose, to have some of its hydrocarbons driven off. Commercial caustic soda can be used without expelling the usual impurities. Air is forced through the caustic soda by means of an air-pump. To set the cell in operation the furnace and its enclosed generator containing the caustic soda is brought to a temperature of 400° to 500° C., and the air-pump is put in action. The caustic soda takes up oxygen from the air, and releases it at the carbon. The carbon is attacked by the oxygen, and suffers a gradual consumption as long as the operation continues, an electric current being produced as a result of the action, the poles being the iron pot and the carbon rod. Some electricians say the current is only a thermo-electric one. The cell is said to have an efficiency of eighty-five or ninety per cent. This efficiency does not, however, take into account the expenditure of heat for maintaining the cell at a suitable temperature, or of the power used in running the air-pump which supplies it with oxygen. Dr. Jacques' cell is an interesting addition to the list of others devised to obtain electricity as a direct result of the consumption of carbon; but whether it will become of practical value, cannot yet be decided.

It seems as if the Boltzmann-Maxwell distribution of the kinetic theory of gases is likely to become an everlasting bone of contention. A short time ago we chronicled in these columns an attack on Maxwell's original proof by M. Bertrand in the *Comptes rendus*, on the ground that the proof assumed independence of the frequencies of distribution of the velocity-components of a molecule of the gas resolved in three directions at right angles. This paper called forth a rejoinder from Prof. Boltzmann, inviting M. Bertrand to examine Maxwell's later proofs. M. Bertrand replied that he considered these even worse than the first. Dr. Carlo del Lungo, writing in the *Atti dei Lincei* in defence of the proof first objected to by M. Bertrand, now endeavours to prove that the assumed independence of distribution of velocities is a necessary consequence of the principles of conservation of momentum and of energy. It certainly seems impossible to give a rigorous proof of the Boltzmann-Maxwell distribution without making *some* assumption beyond the ordinary principles of pure dynamics (*e.g.* the assumption that the only intermolecular forces are those due to impact). But Dr. del Lungo also upholds the view that the general evidence in favour of the law lies in the fact that the distribution in question satisfies so many conditions which are not satisfied by any other distribution, and which represent more or less closely the phenomena present in gases.

Dr. A. Lampa, working in the laboratory of Prof. Franz Exner, has determined the refractive indices of a number of substances for electric waves of very small length. The experiments, which form the subject of a communication to the *Wiener Sitzungsberichte*, were made with electromagnetic radiations of 8 mm. wave-length; this number being ascertained both from the dimensions of the excitor and by diffraction observations. The wave-length in question corresponds to the frequency $N = 37 \cdot 500 \times 10^6$, and Dr. Lampa gives the following values for the index n: Paraffin, 1·524; ebonite, 1·739; crown glass, 2·381; flint glass, 2·899; sulphur, 1·802; benzole, 1·767; glycerine, 1·843; oil of turpentine, 1·782; oil of vaseline, 1·626; oil of almonds, 1·734; absolute alcohol, 2·568; and distilled water, 8·972.

M. L.-A. Marmier, of the Pasteur Institution, has communicated to the Société Française de Physique the results of an interesting series of experiments on the action of currents of high frequency on microbian poisons. It had been previously announced by MM. d'Arsonval and Charrin that such currents affect these poisons to a considerable extent, and M. Marmier has examined whether this modification is a new phenomenon, or merely a secondary result of well-known effects of the current. It is found that the alteration in question only occurs when the liquids are allowed to become heated to a temperature which would alone suffice to modify the poisons. When the liquids are kept cool by suitable precautions, no modification takes place, and M. Marmier concludes that currents of high frequency do not themselves affect microbian poisons. The experiments were performed on the toxin of diphtheria, that of tetanus, and the venom of the cobra snake.

IN the June number of the *Annalen der Hydrographie* there is an interesting discussion, by H. Haltermann, of the occurrence of St. Elmo's Fire at sea, based upon observations in the log-books received at the Deutsche Seewarte. The tables contain full details as to position, conditions of weather, &c. During more than 77,000 days of observation, the phenomenon was observed 164 times, 87 times in north, and 77 times in south latitude. Its occurrence differs very considerably in different parts of the ocean, *e.g.* in the ten-degree square lying between the equator and 10° N. lat., and between 20° and 30° W. long., St. Elmo's Fire was observed about three times per 1000 days, while in the two squares lying between 50° and 60° S. lat. and 60° and 80° W. long. it occurred six times per 1000 days. The more frequent occurrence at sea than on land is attributed to the fact that the accumulating electricity is more easily conducted by the numerous objects projecting into the air over the land.

ON July 2, Prof. Wiesner presented to the Vienna Academy of Sciences an investigation on the important relation of plant-life to photo-chemical climate, based on observations made at Vienna, Buitenzorg (Java), and Cairo. The measurements of the chemical intensity of light were made by a process corresponding in principle to the photographic method of Bunsen and Roscoe. The following are the principal results arrived at: (1) The greatest chemical intensity of light at Vienna amounted (in Bunsen-Roscoe units) to 1·500, and at Buitenzorg to 1·612. (2) The average ratio of the noon intensity to the daily maximum at Vienna was as 1 : 1·08, and at Buitenzorg as 1 : 1·22. (3) At Vienna the yearly noon intensity varied in the proportion of 1 : 214, and at Buitenzorg, in the proportion of 1 : 124. (4) As a rule the daily maximum at Vienna occurred about noon, and at Buitenzorg in the late forenoon. This explains the relatively high maxima at Vienna, and the relatively low maxima of Buitenzorg. In clear or uniformly cloudy weather, the maximum occurred generally at noon at both places. (5) At Cairo a strong depression of the daily curve of intensity was observed at noon, during a perfectly clear sky. This depression was also observed on rare occasions at Vienna, but to a smaller extent. (6) At Buitenzorg the chemical intensity of light was generally greater in the forenoon than in the afternoon. At Vienna this excess was greatest in June and July; the morning intensities were generally higher than the corresponding evening intensities, even when the sky was similarly clouded.

GENERAL PYEVTSOFF, who has had great experience in the measurement of altitudes in Central Asia with the barometer, publishes in the *Memoirs* of the Russian Geographical Society a very valuable paper on barometrical levellings, in which he points out once more the degree of precision that can be obtained from such measurements. He discusses separately the errors in the calculated altitudes which are due to disturbances in the atmosphere resulting from cyclonic and anticyclonic air-movements, to the error in the determination of the average temperature of the air between the two stations at which the barometer has been simultaneously observed, &c., and he gives their relative importance under different circumstances. The most valuable part of the author's inquiry is the comparison which he has made between the real differences of altitudes of twenty-eight different meteorological stations, situated at distances of from 67 to 270 miles from each other, and the altitudes which are obtained day by day from a comparison of the readings of the barometer at the stations, taken in pairs. It appears, as a rule, that if the readings on the days of great atmospheric disturbances are not taken into account, the results are most satisfactory, and that the errors, due to an unequal distribution of pressure at the two stations, seldom exceed 100 feet, and only occasionally attain 140 feet, even for stations taken so wide apart as 100 and 270 miles. As to the altitudes determined in Central Asia, they very seldom exhibit errors exceeding 300 to 400 feet, which evidently is, for separate places, a quite sufficient approximation. At the end of his memoir, General Pyevtsoff gives new tables, based on Babinet's formula, for the calculation of altitudes without the aid of logarithms, which tables combine great accuracy with rapidity, and are very practically arranged.

THE extension of the use of pure yeasts has not unnaturally caused a good deal of attention to be of late bestowed upon the most efficient methods, both for their successful preservation and transmission. Pure yeast cultures can be purchased much as any other article of commerce at the present day, but a great deal depends upon how these so-called stock yeasts can be stored. Experience has shown that, in general, solutions of cane-sugar answer far better for this purpose than wort-gelatine or infusions of wort. In saccharine solutions, yeasts have been preserved in perfect condition for as long as fourteen years. There are, however, exceptions to this rule, for Hansen reports that the *Saccharomyces Ludwigii* dies off in from two to three years when kept in saccharine solutions; whilst Dr. Holm has quite recently described a particular variety of yeast obtained from some Jamaica molasses, which could not be persuaded to exist beyond twelve months in such solutions, whilst in wort-infusions it was still alive after the lapse of two and a half years. With these exceptions, however, saccharine solutions answer the purpose perfectly; but it is of great importance that vessels containing these stock yeasts, whilst occupying very little space, should obviate as far as possible the evaporation of the contents. This evaporation has caused no little trouble in the past; but, thanks to an ingenious device of Jörgensen's, it appears to be happily overcome. The apparatus employed has been recently described by Dr. Holm, to whom the conduct of the experiments involved in determining this point were entrusted, and his paper appears in the *Centrablatt für Bakteriologie*, part ii. Not only may the special flasks described be used for liquids, but they have been found also of great service in preserving gelatine cultures from drying up, and Dr. Holm tells us that, even six months after its preparation, the gelatine in these flasks still retained its soft consistency, and was not in this respect distinguishable from freshly prepared gelatine. This should overcome a difficulty with which bacteriologists are frequently troubled.

IN the summer of 1893, two of von Rebeur-Paschwitz's horizontal pendulums were erected at the observatory of Charkow, and the reports on the observations made with them are being issued by Prof. G. Lewitsky, now director of the Dorpat Observatory. The first pamphlet, consisting of sixty-three pages, is one of the most valuable contributions to the study of earthquake-pulsations so far published. It contains detailed records of 139 series of pulsations between August 4, 1893, and October 9, 1894, the time and amplitude of each marked phase being given, as well as the epochs of the beginning and end of the movement. The duration of some of the disturbances is extraordinary. Thus, the oscillations due to the Greek earthquakes of April 20 and 27, lasted for 14h. 50m. and 12h. 35m. respectively, and those from the Constantinople earthquake of July 10 for 13h. 26m. In these cases, however, the epicentre was not at a great distance. Turning to others of remoter origin, we find that the Japanese earthquake of March 22 affected the pendulum for 10h. 14m., and the Venezuelan earthquake of April 28 for as much as 10h. 30m.

WE notice in the Moscow geographical review, *Earth Knowledge* (*Zemlevedenie*), a very valuable paper, by M. Alboff, "On the Vegetation of West Caucasia." The paper is an introduction to his recently-published great work, "Prodromus Floræ Colchidæ."

THE Director of the Geodynamic Section of the Observatory of Athens has sent us the *Bulletin Mensuel Seismologique* for February, March, and April of the present year. The fact that these three numbers contain records of 147 earthquakes in Greece and the Ionian Islands, shows what an admirable field for study is placed before Dr. Papavasilion. Many of these shocks occurred in districts which have been recently visited by severe earthquakes, no less than sixty-six having been felt in Zante alone.

THE *Proceedings* of the Edinburgh Mathematical Society for the Session 1895–96 have just been published. Among the papers is one having an application to optical instruments, viz., "A summary of the theory of the refraction of thin approximately axial pencils through a series of media bounded by coaxial spherical surfaces, with application to a photographic triplet, &c.," by Prof. Chrystal. Prof. G. A. Gibson contributes to the volume an interesting descriptive review of Prof. Cantor's "Vorlesungen über Geschichte der Mathematik," with special reference to the rise of the infinitesimal calculus, and the Newton-Leibnitz controversy. There is also a paper on "The number and nature of the solutions of the Apollonian contact problem," by Mr. R. F. Muirhead, and others on "Symmedians of a triangle and their concomitant circles," by Dr. J. S. Mackay, and "On deducing the properties of the trigonometrical functions from their addition equations," by Mr. Muirhead.

FOUR parts of the *Transactions* of the Royal Society of Edinburgh, embracing the period 1893–95, are among recent publications. As we regularly print reports of papers read at the Edinburgh Royal Society, it is unnecessary to do more now than refer to a few of those published in the present parts of the *Transactions*. A paper, by Mr. Aitken, on dust particles, and the relation between them and meteorological phenomena, appears in part iii. of vol. xxxvii.; which also contains papers on the fossil flora of the South Wales coal-field, by Mr. R. Kidston, and on the variations of the amount of carbonic acid in the ground-air, by Dr. C. Hunter Stewart. The following part of the same volume has papers on the partition of a parallelepiped into tetrahedra, by Prof. Crum Brown; on the manganese oxides and manganese nodules in marine deposits, by Dr. John Murray and Mr. Robert Irvine; and on the chemical and bacteriological examination of soil, with special reference to the soil of graveyards, by Dr. J. Buchanan Young. The greater portion of part i. (vol. xxxviii.) is taken up with Dr. H. R. Mill's elaborate paper on the distribution of temperature in the Clyde Sea area. The paper is accompanied by thirty-two plates, most of them containing several coloured diagrams, and it is altogether a monument of painstaking observation and careful work. Among the remaining contents of the same publication are papers on bird and beast in ancient symbolism, by Prof. D'Arcy Wentworth Thompson, jun.; two glens (Glen Aray and Glen Shira), and the agency of glaciation, by the Duke of Argyll; and on the relation between the variation of resistance in bismuth in a steady magnetic field, and the rotatory or transverse effect, by Mr. J. C. Beatie. In part ii. of vol. xxxviii. are two extremely important papers, one on specific gravities and oceanic circulation, by Dr. Alex. Buchan; and the other on deep- and shallow-water marine fauna of the Kerguelen region of the great Southern Ocean, by Dr. John Murray. Dr. Buchan's paper is accompanied by nine maps, showing the specific gravity of the oceans at observed temperatures from the surface to a depth of 2000 fathoms. An immense amount of material has thus been brought together for the benefit of oceanographers.

AN important source of vanadium compounds has lately been discovered in South America. In the high plateaus of the Andes, at a height of about 16,000 feet, there exists a mine of anthracite containing vanadium. The coal from this mine, which is easily worked, burns easily, leaving about two per cent. of ash. This ash contains one-seventh to one-quarter of its weight of vanadium, besides some silver, with traces of zirconium and platinum. The extraction of the vanadium on the large scale has been accomplished by M. K. Hélouis, who has applied it to the preparation of aniline black, to the colouring of porcelain, and in metallurgy. The vanadium used by M. Moissan in the preparation of vanadium carbide came from this source.

IN the current number of the *Berichte* there is an account, by Mr. O. Piloty, of a new method of preparing the salts of hyponitrous acid, for which it is claimed that the yield is in advance of all methods previously described. Hydroxylamine hydrochloride, by treatment in alcoholic solution with sodium ethylate and benzene-sulphonic chloride, is converted into benzene sulphonehydroxylamine, $C_6H_5.SO_2.NH.OH$, and this, on treatment with concentrated potash solution, gives the potassium salts of benzene-sulphonic and hyponitrous acids, which can be separated without difficulty. The mechanism of the reaction is precisely analogous to the production of hyponitrite from potassium hydroxylamine mono-sulphonate, discovered by Divers and Haga (*Jour. Chem. Soc.*, 1889, p. 760). In a subsequent note, referring to the preliminary note by Hantzch, stating that anhydrous hyponitrous acid can be obtained, Mr. Piloty describes the results obtained by him in this direction. Silver hyponitrite, suspended in ether and treated with hydrogen chloride, gives silver chloride and a solution of $H_2N_2O_2$ in ether. Rapid evaporation of the ether causes the deposition of the acid as an oil, which solidifies to a crystalline mass in a freezing mixture. As might be expected, both the oil and solid possess highly explosive properties. Of the numerous isomerides of the formula $H_2N_2O_2$ theoretically possible, this is the second to be isolated, the nitramide NH_2NO_2 of Thiele and Lachman being the first.

THE additions to the Zoological Society's Gardens during the past week include a Barbary Ape (*Macacus inuus*, ♀) from North Africa, presented by Mr. E. G. Walls; a Bonnet Monkey (*Macacus sinicus*, ♀) from India, presented by Mr. P. Clarke; two Macaque Monkeys (*Macacus cynomolgus*, ♂ ♀) from India, presented by Mrs. Williamson; an Ocelot (*Felis pardalis*) from Trinidad, presented by Mr. H. O. Nicholls; a Black-tailed Flower-Bird (*Anthornis melanura*) from New Zealand, presented by the Hon. Walter Rothschild; a New Zealand Parrakeet (*Cyanorhamphus novæ-zealandiæ*) from New Zealand, presented by Miss A. Malcolm; a Bare-eyed Cockatoo (*Cacatua gymnopis*) from South Australia, presented by Mrs. M. E. Huntley; a Boobook Owl (*Ninox boobook*) from Australia, presented by Dr. R. Broom; a Raven (*Corvus corax*), British, presented by Mr. William Soper; a Rook (*Corvus frugilegus*), British, presented by the Rev. A. Greaves; six Purplish Death Adders (*Pseudechis porphyriaca*), three Brown Death Adders (*Diemenia textilis*), six Short-headed Death Adders (*Hoplocephalus curtus*) from Australia, a Yellow-headed Conure (*Conurus jendaya*) from South-east Brazil, deposited; six Garter Snakes (*Tropidonotus ordinatus*), six Dekay's Snakes (*Ischnognathus dekayi*), three Spotted-headed Snakes (*Ischnognathus occipito-masculatus*), three Grass Snakes (*Cyclophis vernalis*), a Hog-nosed Snake (*Heterodon platyrhinos*) from Montreal, received in exchange; two Patagonian Cavies (*Dolichotis patachonica*), two Ypecaha Rails (*Aramides ypecaha*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

NOVEMBER METEORS.—With the July number of *Monthly Notices R.A.S.* a circular is issued by G. Johnstone Stoney, calling the attention of astronomers to the approach of the great secular maximum of the Leonids, which is due about 1899 or 1900. It is probable that this swarm was drawn into the solar system by the planet Uranus about February or March A.D. 126, and careful observations during the next few years may furnish materials for confirming or rejecting this hypothesis. Photography should be employed as widely as possible, and wherever practicable the time of appearance of each meteor recorded. Accurate simultaneous observations from different stations will be of exceptional use. The radiant-points and times of apparition of all meteors should be exactly noted, commencing a few nights before, and continuing some nights after, November 14 and 15.

PLUMB-LINE DEVIATIONS.—M. Messerschmitt, who has been for some time engaged in the determination of latitude and azimuths of a series of points in the Swiss Triangulation, has communicated (*Ast. Nach.*, No. 3365) the results of his most recent investigations. It may be recalled that M. Messerschmitt's first determinations were made in West Switzerland, and these were followed by further researches in the north of the country, which corroborated his previous results. The present paper is concerned with observations made on a line drawn approximately north and south through the centre. Collecting his results into a table showing the difference between geodetic and astronomical latitude, and arranged in order of increasing distance from the equator, a systematic deviation from the vertical is clearly shown. In the midst of the mountains (around Andermatt for example) these deviations are quite small. Going south they increase rapidly, and attain a negative maximum of 20″ (astronomical—geodetic) towards Lugano. A positive maximum occurs about Goschenen, the entrance to the Gotthard Tunnel; and still further north, the difference diminishes again, and changes sign about the latitude of Zürich. Schaffhausen shows again the position of negative maximum. The position of the mountain chains generally explains these variations.

An investigation, founded on these deviations of the plumb-line, of the form of the surface of the earth for a meridian length of about 200 km. through the Gotthard district, discloses the fact that the ellipsoid sinks everywhere below the geoid. Selecting as a zero point that position where no deviation from the vertical exists (47°·1 15′ lat.), the greatest difference between the two surfaces occurs near Airolo (the southern exit of the tunnel), where it amounts to nearly five metres. Going southwards from this point the surface sinks gradually, and approaches the ellipsoid before the valley of the Po is reached.

THE HAMBURG OBSERVATORY.—Prof. Rumker's report of the observatory work during the year 1895, shows that the activity of the various departments is fully maintained. The observations with the equatorial have mainly consisted in deriving the positions of small planets and comets, and of the fainter stars with which the nebulæ, whose places have been published in a communication from the observatory, have been compared. With smaller instruments attention has been given by Dr. Hänig to variable stars and occultations by the moon. With the meridian instrument, in addition to observations required for the accurate distribution of time signals, arrangements have been made for observing stars in the degree 80–81 N. Decln. down to 9·3 mag. In addition to this varied work, the attention of the staff has been called by Dr. Auwers and others to discrepancies between the places of stars in the Hamburg catalogue, and those recently obtained in the "Astronomischen Gesellschaft" zone catalogue. This has necessitated much searching of old records, and in some cases the detection of errors, which will be published in a communication from the observatory.

THE DUNSINK OBSERVATORY.—The seventh part of the astronomical observations and researches made at Dunsink, and published under the direction of Prof. Rambaut, contains the meridian places of 717 stars, of which upwards of 2000 observations have been made. These stars have been selected on account of suspected large proper motions, and the observations, interrupted as they have been from several causes, have been spread over eleven years. But, nevertheless, the accuracy maintained throughout is of a very high degree. From an examination of the separate results, the probable errors in R.A. and Declination are, respectively, $\pm$ 0″·037, and $\pm$ 0″·505. This error is probably increased by the uncertainty of the proper motion in many cases, and does not fully express the accuracy of the work. The Pistor and Martin's meridian circle, with which the observations have been made, has been frequently reversed in the course of the work, and the determination of latitude, on which great care has been bestowed, is slightly different in the two positions. With Clamp West the resulting latitude is 53° 23′ 13″·05, and with Clamp East three-tenths of a second less. The value used in the final reduction is 53° 23′ 13″·00, and the results, it is believed, coincide very closely with Auwers' fundamental system. The cause of this systematic difference in the latitude, however, has not been satisfactorily explained.

OBSERVATORY OF MOSCOW.—The last issue of the *Annals* of this Observatory (series 2, vol. iii. part 2, 1896) contains several papers of general interest. The director, W. Ceraski, contributes the following articles: (1) "Photometry of the star cluster χ Persei," in which he gives the measures of the magnitude of seventy stars of the group, determined with a Zollner photometer on a 10-inch refractor. One star he finds to be variable, and recommends its further study. (2) "Observations of eclipses of Jupiter's satellites without photometric appliances," using eye estimates of relative magnitude compared with some neighbouring star of known brightness. (3) "On the temperature of the sun," in which he gives the inferior limit to be about 3500° C. (4) "A new method for the electrical comparison of pendulums."—P. Sternberg discusses the photographs he obtained during the transit of Mercury on May 9, 1891, and also contributes an important description of his determination of the variation of latitude at Moscow.—B. Modeston gives a full description of the calculation of double-star orbits by the methods of Kowalski and Encke respectively. —S. Blajko, as the result of thirteen photometric measures of the magnitude of Mira Ceti during the winter of 1894–5, finds evidence of a secondary maximum in its light curve, occurring about a month previous to the highest maximum, the magnitudes at the secondary and principal maxima being about 3·5 and 3·16 respectively.

THE SOLAR ECLIPSE OF APRIL 16, 1893.

M. DESLANDRES has now issued his report on the work accomplished by the French expedition to Fundium, Senegambia, for observations of the total solar eclipse of April 16, 1893. Some of the results obtained have already been made known, and these are now brought into connected order and discussed. A full account is also given of the general objects and organisation of the expedition. The programme decided upon included the photography of the corona, a photographic study of the coronal spectrum, especially in the ultra-violet, and a spectroscopic study of the movements of the corona.

The report is of special importance in view of the approaching eclipse, for the reason that reference is made to several points which may serve as a guide in future operations. For example, M. Deslandres' experience indicates that for the corona pictures plates of moderate sensitiveness give better results than the plates of greater rapidity. Another practical suggestion is that at least two cameras should always be employed in the search for an intra-mercurial planet; M. Deslandres found it impossible to determine whether certain spots on the single plate which he obtained represented stars or photographic defects.

The general results relating to the coronal spectrum are thus stated: (1) The continuous spectrum of the corona, which forms the greater part of its light, is most intense on the red side, relatively to the spectrum of the disc, and the difference appears to become greater as the point considered is further removed from the photosphere. (2) The spectrum of dark lines, under very favourable conditions, did not appear at 5′ from the sun's limb; at this height the light diffused by the coronal particles is still too feeble with respect to their own light. (3) The luminous gases of the corona, indicated by the fine lines, have not the same intensity or composition in different parts of the corona, or at different heights; further, they most frequently do not correspond to elements known upon the earth.

Special interest attaches to the investigation of the rotation of the corona by observing or photographing the displacement of lines in the spectrum at some distance from the limb on each side of the equator. No photographic impression was secured with the fourth order spectrum of a diffraction grating, adjusted for H and K, and, although the eclipse occurred at a maximum of sun-spots, 1474 K was too feeble in the second order spectrum to permit any trustworthy measures to be made visually. A successful photograph of the H and K lines was obtained, however, with a 3-prism spectroscope attached to a 6-inch refractor, one half of the slit being exposed on the west and the other on the east side of the corona. The measured velocity of 6·8 km. per sec. has led M. Deslandres to conclude that the equatorial part of the corona moves very nearly with the same angular velocity as the photosphere. This result must be received with caution until confirmed by further researches, as the photographs taken at the same moment by Mr. Fowler give no indications of the presence of H and K in the true coronal spectrum. It is pointed out that this research may be simplified in future by making only one exposure, placing the slit radially, so that the velocities may be determined from the inclination of the lines, as in the recent researches on Saturn's rings.

In the last chapter of the report, various hypotheses as to the nature of the solar atmosphere are reviewed, and an electrical theory is propounded. It is pointed out that, notwithstanding the diversity of appearances, there is really a great similarity between the solar and terrestrial atmospheres, and the report ends as follows: "Terrestrial meteorology and solar physics, which are separated by the necessity for the division of work, are in reality connected sciences, which, by the nature of things, ought to be studied together."

THE RÖNTGEN RAYS.[1]

PROF. RÖNTGEN, of Wurzburg, at the end of last year published an account of a discovery which has excited an interest unparalleled in the history of physical science. In his paper read before the Wurzburg Physical Society, he announced the existence of an agent which is able to affect a photographic plate placed behind substances, such as wood or aluminium, which are opaque to ordinary light. This agent, though able to pass with considerable freedom through light substances, such as wood or flesh, is stopped to a much greater extent by heavy ones, such as the heavy metals and the bones; hence, if the hand, or a wooden box containing metal objects, is placed between the source of the Röntgen rays and a photographic plate, photographs such as those now thrown on the screen are obtained. This discovery, as you see, appeals strongly to one of the most powerful passions of human nature, curiosity, and it is not surprising that it attracted an amount of attention quite disproportionate to that usually given to questions of physical science. Though appearing at a time of great political excitement, the accounts of it occupied the most prominent parts of the newspapers, and within a few weeks of its discovery it received a practical application in the pages of *Punch*. The interest this discovery aroused in men of science was equal to that shown by the general public. Reports of experiments on the Röntgen rays have poured in from almost every country in the world, and quite a voluminous literature on the subject has already sprung up.

In view of the general interest taken in this subject, I thought that the Röntgen rays might not be an unsuitable subject for the Rede Lecture.

Before discussing these rays themselves, I think it may perhaps make matters clearer if I call your attention to one or two of the phenomena which accompany the discharge of electricity through gas at a low pressure. I have here a bulb from which the air has been taken until the pressure has been reduced to about 1/10000 part of the atmospheric pressure. When the electric discharge passes through this bulb you see that there is considerable luminosity in the gas in the bulb, except in a region round this terminal—the negative one; this region, where the luminosity is so deficient, is called the negative dark space. In this bulb there is no phosphorescence on the glass, and I may say no emission of Röntgen rays. If I were still further to reduce the pressure of the gas in this bulb, this dark space would expand and encroach on the luminous part of the discharge, and would, when the pressure got very low, reach the walls of the tube; the expansion of the dark space diminishes the luminosity in the gas, but we find that where the dark space reaches the glass of the tube the glass itself becomes luminous, until finally at very low pressures we get to the state of things shown by this tube, where the luminosity is all on the glass, and little or none is to be observed in the gas. Röntgen rays are produced by this bulb, though not by the other.

There is one feature in this tube to which I must call your attention: you see that there is a shadow on part of the tube; this shadow is thrown by a mica cross fixed between the negative electrode and the wall of the tube, and if we observe the shape of the shadow we see that any point of the tube is in shadow if the line joining that point to the negative electrode passes through the mica cross. We thus conclude that we have something starting from the negative electrode, travelling in straight lines, and producing phosphorescence when it reaches the glass, and, further, that this something is stopped by the mica cross. This something which travels in straight lines from the kathode is called the kathode rays: these rays are of great interest in relation to the subject of this lecture, for the kathode rays seem to be the parents of the Röntgen rays. Let me call your attention to the effect produced by a magnet on these rays: you see that when the magnet is brought near, the shadow of the cross is displaced; this shows that the direction of the rays casting the shadow have been deflected by the magnet, thus the kathode rays are deflected by a magnet. We shall see later on that the Röntgen rays, on the other hand, are not so affected. This is one of the most striking differences between the parent—the kathode rays—and the child, the Röntgen rays. The effects of the kathode rays inside the tube were discovered more than twenty years ago by Crookes and Goldstein; it is only quite recently, however, that any effects produced by these rays outside the tube have been observed. In 1894 Lenard, using a tube of the kind shown in the diagram (Fig. 1), where the kathode

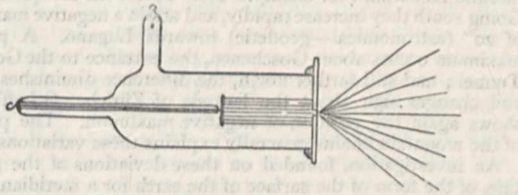

FIG. 1.

rays struck against a window made of very thin aluminium, found that if he placed outside the tube in front of the window a screen covered with a phosphorescent substance, pentadecaparatolyketon, it became phosphorescent; he found, further, that a photographic plate placed behind the window was affected—nay, that this plate was affected even though he placed in front of it a plate of aluminium or a thin quartz plate—in fact, he took a photograph through aluminium and quartz; he thus obtained two of the most prominent phenomena shown by the Röntgen rays. In fact, we know from the researches of Röntgen that the Röntgen rays must have been present and played a part in these experiments. Lenard himself ascribed the effects he observed to kathode rays which had penetrated the aluminium window, and indeed it would seem that something in addition to the Röntgen rays must have been present, as Lenard found that the position of the phosphorescent patch was affected by a magnet, while the Röntgen rays themselves are, as we shall see, not influenced by such an agent.

I now come to the consideration of the Röntgen rays themselves, and shall endeavour to repeat some of the experiments by which Röntgen established their existence. The apparatus consists of a tube exhausted to such a low pressure, that when the electric discharge passes through it there is an abundant supply of kathode rays; these rays strike against a metal plate in the bulb. This metal plate is not essential for the production of the rays, and was not present in the bulbs used by Röntgen; it, however, considerably increases the efficiency of the bulb.

When the electric discharge passes through this bulb, the region round it is the seat of some very remarkable phenomena. I have here a screen coated with a phosphorescent substance,

[1] The Rede Lecture, given at the University of Cambridge, on June 10, by Prof. J. J. Thomson, F.R.S.

potassium platinocyanide; though this screen is opaque to ordinary light, you will see that it phosphoresces when placed in the neighbourhood of the bulb. This phosphorescence is due to something radiating from the bulb, because when I place this piece of metal between the bulb and the screen, a sharp shadow of the metal is thrown on the screen. The metal is opaque to these radiations. If, however, I place a piece of wood, about an inch thick, between the bulb and the screen, you will hardly be able to see a shadow; so that this board, though opaque to ordinary light, allows those rays to pass through with considerable freedom. The lighter the substance the more easily is it penetrated by these rays; thus the very light metal aluminium is very transparent, as you will see by the poor shadow it casts upon the screen. This property has been used to detect real gems from paste, as the diamond, consisting of the light element carbon, is much more transparent than an artificial one made of heavy silicates. Since light objects are, roughly speaking, transparent, while the heavy ones are opaque, if we have a mixture of heavy and light objects between the screen and the bulb, the heavy ones will throw a shadow, the lighter ones will not. We can thus detect dense objects even when surrounded by opaque ones, provided the latter are light. [Experiments throwing shadows of jewellery in cases, hands, &c., upon the screen.] Prof. Lodge has in this way been able to see through a yard of timber. We seem here to have the realisation of Sam Weller's aspiration after an optical arrangement which would enable one to see through "a flight of stairs and a deal door."

I will now endeavour to show that in order to have Röntgen rays you must have kathode rays to start with. I will produce in the bulb, which I have used for the production of the Röntgen rays, a discharge of another kind, viz. an electrodeless discharge in which the discharge, instead of travelling between metallic terminals in the gas, travels round a closed circuit in the gas. In this way we have no kathode and no kathode rays; you see that though a bright discharge passes through the bulb, far brighter than in the previous case, no luminosity is produced on the screen.

One very remarkable property discovered by Röntgen of these rays, is that they are not bent when they pass from one medium to another. We can show this in the following way. I place in front of the bulb this thick plate of metal, in which a vertical slit has been cut; the metal stops the rays, so that we get on the screen a bright luminous vertical band. Now I place between the slit and the screen this wooden prism, which covers up the lower, but not the upper, half of the slit; if the rays which came through the slit were refracted, then the lower part of the bright band would no longer be in the same straight line as the upper part. You see, however, that the two halves still remain on the same line; the only effect produced by the wooden prism has been to make the lower half somewhat dimmer than it was before.

Again, these rays are not deflected by a magnet; to prove this, we throw the shadow of two brass tubes on the screen, and observe the shadows before and after a horse-shoe magnet has been introduced into the tubes; you see that no appreciable effect is produced by the introduction of the magnet.

The absence of refraction leads us to expect that there would be little regular reflection of the Röntgen rays, and this conclusion has been confirmed by numerous experiments. At grazing insistence, however, Joly of Dublin has been able to detect a small amount of regular reflection. Though there is but little regular reflection there is an appreciable amount of what, to avoid any speculation as to its nature, has been called by Sir George Stokes "diffuse return" of the rays; this was discovered by Röntgen himself, and is rendered very evident by an experiment of Lord Blythswood. We do not know yet, however, whether the rays coming off from the metal plate are of the same kind as those which fall upon it, or whether they are slightly different. If they are of the same kind, then the effect would resemble the diffuse reflection from a piece of ground glass; if they are different, it would indicate that the piece of metal illuminated by these rays became itself a source of rays not quite of the same kind as those which fall upon it, just as when a solution of quinine is exposed to the invisible ultra-violet light it emits not ultra-violet light like that which fell upon it, but visible blue light. This point might be settled by measurements of the rates of absorption of the incident and "diffusely returned" light.

That the Röntgen rays are not all of the same kind, has been shown in several ways, of which, however, I have only time to mention one. Mr. McClelland, working in the Cavendish Laboratory, found that if he took a plate of tinfoil and a layer of water, and adjusted the thicknesses so that they exerted the same absorption on the Röntgen rays given out from one bulb, they did not necessarily produce the same absorption in the rays from another bulb, showing that the rays from the one bulb were not the same as those from the other.

Röntgen discovered that the rays not only made certain substances phosphorescent, but that they affected a photographic plate; so that if we replaced the phosphorescent screen in our experiment by a photographic plate, we should get a permanent impression of the picture, which would be thrown on a phosphorescent screen placed in the position of the photographic plate. To obtain these photographs all that is necessary is to protect a photographic plate from ordinary light by thick cardboard or aluminium, and place the object to be photographed between the bulb and the plate; after an exposure varying with the nature of the object and the state of the bulb, photographs of the kind which are now so well known can be obtained.

One very marked feature of these photographs is the sharpness of the detail; this shows that the origin of the rays must be confined to a comparatively small region. If these rays came from an area comparable with that occupied by the phosphorescence on the walls of the bulb used to produce the rays, the luminosity from one part of the screen would throw one pattern on the screen, while the rays from another portion would throw another pattern; the superposition of these patterns would produce a blurred image. To illustrate this point, I have here two photographs of the same thing—one taken by the Röntgen rays, the other taken by an incandescent lamp with walls of frosted glass, of about the same size as the bulb used to produce the Röntgen rays, and placed in the same position; you see that the photograph taken by the Röntgen rays is quite sharp, while that taken by the electric lamp is much blurred. This shows that the Röntgen rays do not come from an area nearly so extended as the phosphorescent part of the glass. We can investigate the place of origin of these rays in various ways, by observing the law of diminution with the distance of the effects due to these rays, by taking pin-hole photographs, by observing the direction of the shadows cast by a series of opaque bodies; the result of such observations shows that Röntgen rays are produced when the kathode rays strike against a solid obstacle. Cases have been observed by Lord Blythswood and by Rowland, which seem to show that this is not the only source of these rays.

The experiments made on these ray have not led to any result absolutely decisive as to their nature, but we can profitably discuss the question whether the facts known about these rays oblige us to regard them as due to a new form of energy, or whether they are consistent with these rays being a variety of some form of energy already known to us; before calling in a new form, we ought to be quite sure that it is necessary to abandon the old. The rectilinear propagation of these rays, their powers of producing phosphorescence and of affecting a photographic plate, their insensibility to a magnet, suggest that of the old forms of energy light is the one to which these rays are most closely allied. We are acquainted with so many varieties of light (by light I mean transverse vibrations propagated with a definite velocity) with such widely different properties, that we can well contemplate the existence of other kinds with still different properties. We know, for example, the ultra-violet light of very small wave-length, the subject of classical researches by Sir George Stokes, which, though it affects a photographic plate, does not affect the retina, and passes through bodies with such difficulty that the most ultra-violet kind is quenched after passing through a few millimetres of air; then we have the visible light able to affect the retina, and able to pass through great lengths of some substances which are opaque to the ultra-violet rays though stopped by very small thicknesses of others; then we have the longer waves of radiant heat given out by a hot body below the temperature at which it becomes luminous. These are not visible, have but little effect on a photographic plate, and are able to traverse substances opaque to both ultra-violet and visible light. Then we have the waves emitted by vibrating electrical systems, which neither affect the retina nor a photographic plate, which, as Mr. Rutherford has shown, are able to traverse the walls of the houses and the bodies of the inhabitants of about three-quarters of a mile of a densely populated part of Cambridge, and which are so different in properties from ordinary light that it required the genius o Clerk-Maxwell to recognise them as light at all. I shall have to call your attention to another kind of light, discovered lately by Becquerel. It will doubtless be urged that widely as these kinds of light differ from each other in some respects, they all are bent when they pass from one substance to another, while the Röntgen rays, as we have seen, are not refracted. This objection to the possibility of the Röntgen rays being a kind of light, formidable as it appears at first sight, loses all its force when more closely examined. We know cases in which light passes through substances without being refracted; thus Kundt found that certain rays could pass through gold without being refracted, while other rays were bent the wrong way. Stenger has lately found that certain blue rays can pass through fuchsin, and other slightly different ones through Hofmann's violet, without being bent. Perhaps, however, the most striking testimony to there being nothing inconsistent in the idea of a kind of light which is not refracted, is afforded by one of the last investigations undertaken by von Helmholtz, and published about three years ago. Von Helmholtz investigated what, on the electromagnetic theory of light, would be the bending experienced by light of different frequencies passing through an ideally simple substance, one whose spectrum consists of only one line. The result of his investigation is shown in this curve (Fig. 2), where the abscissæ represent the frequency of the light, the ordinates the refractive index. On the part of the curve from *f* to *g*, you see that the refractive index increases as the frequency increases; this corresponds to the normal spectrum where the blue rays are more refracted than the red. After passing *b* the curve dips down; this means that the greater the frequency the less the bending, in other words, the blue rays tend to be bent more than the red. We know many instances of this, it is called anomalous dispersion. Then we get to

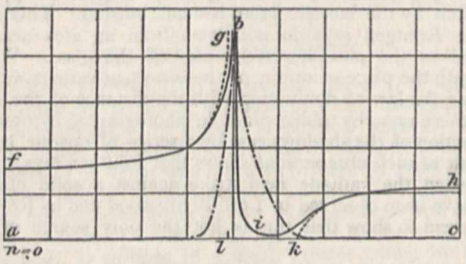

FIG. 2.

the part of the curve about *i*, where the refractive index is less than 1; that is, where the rays are bent the wrong way. We have examples of this, as Kundt has shown, in the case of gold, silver and copper; but the most interesting part of the curve for our purpose is the last part, where, after dipping below the line of no-bending for a short distance, it approaches it and practically coincides with it for all frequencies greater than *a h*; so that, on this theory there would practically be no bending for all waves whose frequency exceeds a certain value. Thus, so far from the absence of bending being a proof that the Röntgen rays are not light, this absence of bending is exactly what we should expect if these rays were light of very great frequency.

A characteristic feature of all varieties of light is the existence of polarisation, and polarisation is indisputable evidence of transversal vibration; hence, many experiments have been made to see whether any polarisation of these rays could be detected. All these experiments have practically been confined to seeing whether the Röntgen rays could traverse two plates of tourmaline more freely when the axes of the two plates are parallel than when they are crossed; there is a great difference in the transparency to ordinary light in the two cases. The results of these experiments are somewhat conflicting. Prince Galitzine and M. de Karnojitsky are of opinion that they have succeeded in detecting a slightly greater absorption of the rays when the axes are crossed than when they are parallel; on the other hand, Becquerel, Mayer, and I were not able to detect any appreciable difference in the two cases. If the result of Prince Galitzine should be confirmed, it would prove beyond cavil that these Röngten rays were light; but even if the presence of polarisation is not definitely established in this case, it does not follow that these rays can not be polarised—the methods for polarising one kind of light may not be successful when used for another. For example, a wire bird-cage will polarise the long electrical waves, but will not affect the shorter waves of radiant heat, much less those of visible light. By winding exceedingly thin wires close together on a framework, Rubens and Du Bois were able to polarise the waves of radiant heat, the wave-lengths of which are long compared with those of light. This arrangement, however, is much too coarse to polarise visible light, much less ultra-violet light. And it is possible, and indeed likely, that the structure of the tourmaline, though fine enough to polarise ordinary light, may not be fine enough to polarise the Röntgen rays.

So far, I have confined myself to showing that there is nothing in the effects known to be due to these rays inconsistent with their being a variety of light. I must now pass on to some evidence of a more positive character. Since the discovery of the Röntgen rays, Becquerel has discovered a new kind of light, which in its properties resembles the Röntgen rays more closely than any kind of light hitherto known. Becquerel found that certain uranium salts emitted, after being exposed to the sunlight, radiations which, like the Röntgen rays, could pass through plates of aluminium or of cardboard, and affect a photographic plate behind. I have here a photograph of a perforated piece of zinc, which has been taken by Becquerel's method by simply scattering over the zinc plate powdered uranium nitrate, and placing it over a photographic plate well protected from ordinary light. After a long exposure of from twenty to forty hours, the photograph now on the screen was taken. Becquerel has shown that the radiation from the uranium salts can be polarised, so that it is undoubtedly light; it can also be refracted. It forms a link between the Röntgen rays and ordinary light, it resembles the Röntgen rays in its photographic action in power of penetrating substances opaque to ordinary light, and in the characteristic electrical effect, while it resembles ordinary light in its capacity for polarisation, in its liability to refraction. The persistence of the radiation is very remarkable. Becquerel found that the potassium-platinum compound of uranium went on emitting these radiations with nearly undiminished zeal for fifteen days after it had been exposed to the sunlight. It would seem that under the influence of sunlight some change in the chemical or physical nature of the substance occurred, and that after the sunlight was cut off, the substance gradually went back to its original state, and that while doing so it emitted this peculiar radiation. The radiation from the uranium salts is of especial interest from another point of view. Sir George Stokes has shown that in the case of phosphorescence caused by sunlight or the arc lamp, the light emitted by the phosphorescent body is of longer wave-length than the light causing the phosphorescence; in the case, however, of the phosphorescence discovered by Becquerel, the light emitted is of a shorter wave-length than the incident light. The case resembles that called calorescence by Tyndall, when the body placed in a focus of dark radiant heat becomes luminous and gives out the shorter luminous waves.

From this discovery of Becquerel, we may conclude that besides the vibrations emitted by luminous bodies with which we have hitherto been acquainted, there are others having a much greater frequency and, it may be, arising in a different way.

To sum up, we may say that though there is no direct evidence that the Röntgen rays are a kind of light, there is no known property of these rays which is not possessed by one or other of the forms of light.

One of the most remarkable phenomena connected with these rays is the way in which the absorption depends upon the density of the body; if we measure the transparency of a series of bodies, we find that the order of opacity is the same as the order of their density. No other factor in the constitution of the body seems comparable in importance with density. In this respect, the relation between the opacity and the other properties of a body in the case of the Röntgen rays is simpler than that for luminous waves or electric waves. There seems no simple relation between the density of a body and its transparency to visible radiation or electrical vibration; in the case of the Röntgen rays, however, it seems the greater the density the greater the opacity. This appears to favour Prout's idea that the different elements are compounds of some primordial element, and that the density of a substance is proportional to the number of the primordial atoms; for if each of these primordial atoms did its share in stopping the Röntgen rays, we should have that intimate connection between density and opacity which is so marked a feature for these rays.

I now pass from the consideration of the rays themselves to some of the effects they produce on bodies through which they pass.

There seems considerable evidence that the energy associated with these waves is small. I am not acquainted with any effects produced by them which involve the expenditure of an amount of energy comparable with that emitted in a second by a candle. They do not produce any appreciable rise in temperature when they fall on the thin metallic strips of a bolometer. Mr. Skinner has found that they exert no appreciable effect on the combination of hydrogen and chlorine, though this is a good test of the intensity of very faint light; and, what is more unfortunate, they do not exert any of those deleterious effects on bacteria which are fortunately associated with ultra-violet light. Some of the other effects exerted by ultra-violet light seem to be associated with these rays; thus some observers who have had undue curiosity about their bones, and have in consequence exposed their hands frequently to these rays, have found that the hand so exposed became sunburnt. There seems considerable evidence, too, that these rays are not good for the eyes, though it is difficult to disentangle any distinctly injurious effect due to the rays from the bad effect that may be produced by the straining of the eye in the endeavour to see only a faintly luminous object.

There is one property of substances which seems peculiarly suitable for testing if these rays affect the substance through which they pass; it is the property of transmitting electricity.

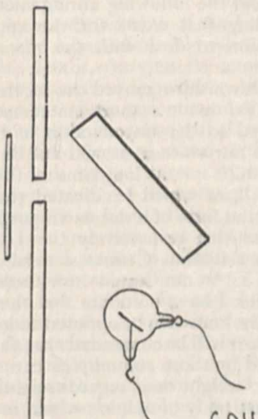

FIG. 3.

When we investigate the effect of the Röntgen rays on this property, we find the remarkable result that bodies which, when shielded from these rays, insulate to all appearance, perfectly allow electricity to pass through them when exposed to the action of these rays. I will, first of all, show an experiment illustrating this property in the case of gases which in their normal state are of all substances the most perfect insulators. The details of the experiment are shown in the diagram (Fig. 3). The coil and bulb are placed in this box, lined inside with tin-plate, and covered over the top with sheet-lead. A hole is cut in the box just over the bulb, and this hole is covered with a plate of aluminium, which is transparent to these rays. The air space between the electrodes is placed over this hole. One electrode is connected to one pair of quadrants of the electrometer, the other electrode is connected to one terminal of a battery, the other terminal of which is to earth; the two pairs of quadrants of the electrometer are connected together and with the earth, and the connection between them broken. If there is no leakage across the air space, the needle of the electrometer will remain at rest. You see it does so when the coil is not in action. As soon, however, as the coil is turned on, the spot of light moves rapidly across the scale, showing that electricity is passing across the air space. The rapidity of movement of the spot of light is a measure of the rate of leak. Now the electrical leakage produced by these rays depends on the nature of the gas. The gas I have just used was air. I will now replace the air by another gas—chlorine. Again you see the leak, but it is now much faster than before. Mr. McClelland and I have investigated the rate of leak in different gases, and we find that they can be arranged in the following order: hydrogen, coal gas, ammonia gas, air, carbonic acid gas, sulphuretted hydrogen, chlorine, mercury vapour.

That the gas itself is put into a peculiar state by the passage through it of these rays—a state which it attains for an appreciable time—is shown by the following experiment, which I described some time ago in NATURE. I have here an electrode shielded from the direct action of these rays. I charge it to a high potential, and even though the rays are on, it does not leak. I now blow some of the air through which the rays have passed on to the electrode, and you see at once we get a rapid leak. The rate at which electricity passes through the gas

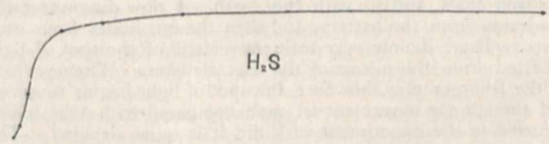

FIG. 4.

depends upon the pressure; the lower the pressure the slower the leak. Mr. McClelland and I found that for an air space of about 1 cm. the rate of leak over a considerable range of pressure varies as the square root of the pressure. In some experiments recently made by Mr. Rutherford and myself, we found that using a constant potential difference the rate of leak was smaller across a very thin plate of air than across a thicker one; it thus appears that the process of conduction through a gas is one that requires a considerable amount of room.

Another very interesting point about the rate of leak is the connection between the rate of leak and the electromotive force. This can, perhaps, be most easily understood by means of a curve (Fig. 4). The ordinate represents the rate of leak, the abscissa the electromotive force. At first, when the E.M.F. is small, the curve is a straight line, showing that the current is proportional to the electromotive force; in other words, that the conduction of electricity through the gas, like the conduction through metals and electrolytes, obeys Ohm's law. But it is

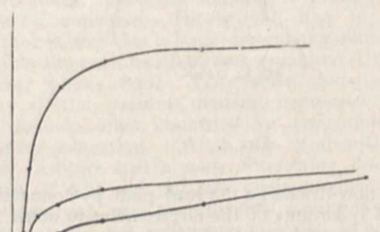

FIG. 5.

only when the E.M.F. is small that the curve is straight. We soon get to a stage where the current increases more rapidly than the E.M.F.; beyond this, again, we reach a part of the curve where the current increases but slowly as the electromotive force increases, and we finally reach a stage where the current seems independent of the E.M.F., and is, to borrow a term from magnetism, "saturated." I have here a diagram (Fig. 5) of three curves taken for the same gas, but at different distances from the bulb. You see that the first ascent is much steeper near to the bulb—that is, when the rays are strong than when it is far away and the rays are weak, and practical saturation is attained sooner when the rays are strong than when they are weak. These curves bear a remarkable resemblance to those which represent the relation between the magnetisation of a piece of iron and the magnetic force acting upon it. When the rays are strong, the curve is like that of soft iron; when the rays are weak, it is like steel.

Gases are not the only substances that conduct when transmitting these rays; solids also conduct, though the conductivity obeys different laws and only lasts for a short time. The conduction through solids very closely resembles the phenomenon called "electric absorption," a well-known example of which s the residual charge of a Leyden jar.

I have here some experiments which illustrate the effect of the Röntgen rays on solids. In the first of these we have a lead cylinder with a thin base made of aluminium. At the bottom of the cylinder there is a thin layer of solid paraffin; on the top of this, and sticking to it, there is a large leaden disc, over which paraffin has been poured, so that the disc is entirely embedded in the paraffin (Fig. 6). This cylinder rests on the aluminium window in the iron chest containing the coil and the tube, this window being very much smaller than the lead plate in the paraffin. I now connect the lead plate to one pair of quadrants of a highly charged electrometer, and then connect the two pairs of quadrants together and with one of the poles of a battery of 200 small storage cells, the other pole of which is connected with the iron chest, and so with the earth. I now disconnect the quadrants from the battery, and then the quadrants from each other. There is now very little movement of the spot of light reflected from the mirror of the electrometer. When we turn on the Röntgen rays, however, the spot of light begins to move, and though the movement is small compared with that which occurred in the experiment with air, it is quite decided. The rapidity with which the spot of light moves soon, however, begins to decrease, and after a short time becomes almost

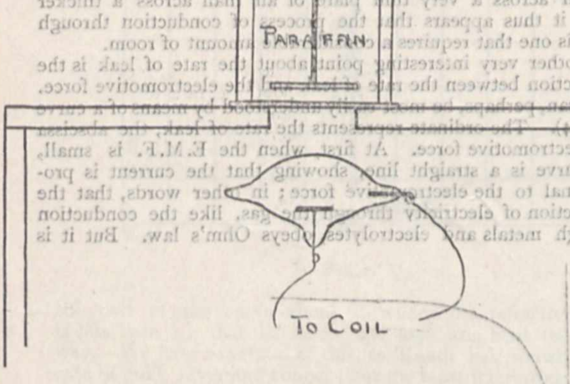

FIG. 6.

inappreciable. I now discharge the lead plate by connecting it and both pairs of quadrants of the electrometer to earth for a short time, then keeping one pair of quadrants connected with the earth, and leaving the other connected with the lead plate, we see that when the rays are off there is a very slight movement of the spot of light in the opposite direction to the original deflection; this is due to the leaking out of the "residual charge." This movement is, however, greatly increased as soon as the rays are turned on, and continues until we get quite a large deflection; "the residual charge," or polarisation in the paraffin, has then been enormously increased by the rays. The conductivity of the paraffin under these rays resembles in its properties that of the insulating sheath of a telegraph cable. In testing the resistance of such a sheath, the current passing through it does not remain constant, it rapidly falls off in intensity; and if after the electromotive force has been applied for some time it is removed, and the inside and outside of the sheath connected with the terminals of a high-resistance galvanometer, a current flows through the galvanometer, and this current is in the opposite direction to that which originally flowed through the sheath.

Ebonite shows the effect of the Röntgen rays in increasing the conductivity even better than paraffin. I have here a plate of ebonite about 1 mm. thick, coated on both sides with tinfoil. I put this on the aluminium window, and on the top of the ebonite plate I place a lead disc, which is much larger than the aluminium window; the object of this disc is to prevent the Röntgen rays from striking against the wire connected with the disc, and so discharging the disc through the air. That it is effectual in doing this, is proved by there being no leak when the rays are on, and the wire (raised to a high potential) disconnected from the disc. If we now repeat with this plate of ebonite the experiments we previously tried with the paraffin, we get similar but decidedly larger results. I may mention that different specimens of ebonite vary considerably in the magnitude of this effect. There is one variation of the preceding experiments which is of some interest. I will charge up the ebonite plate without putting the Röntgen rays on at all; on discharging, you see that the electrometer indicates that the "residual charge" is coming out. I keep discharging the disc until the residual charge is almost inappreciable. I now for the first time put on the rays, and you see that the residual charge or polarisation, which could not previously be detected, now becomes quite marked. These experiments show how greatly the properties of bodies are modified by the Röntgen rays, and show that by their discovery physical science has received an agent which promises to be of the greatest service in investigating some of the properties of bodies which are now most urgently pressing for explanation.

LONDON UNIVERSITY COMMISSION BILL.

THE second reading of the London University Commission Bill was agreed to by the House of Lords on Thursday last. A full report of the debate upon the Bill was given in the *Times* of Friday, and the following abridgement of it will show the favourable feeling that exists for the appointment of the Statutory Commission to deal with the reconstitution of the University.

The Duke of Devonshire moved the second reading of this Bill. He said: As I made a short statement of the circumstances that have led to the introduction of this Bill when I moved for leave to introduce it, it will not be necessary for me to detain your lordships for any long time on this occasion. The opposition to the Bill, of which I indicated the possibility, has manifested itself in the form of a statement purporting to proceed from two bodies entitled respectively the University Defence Committee and the Gresham Commissioners' Scheme Amendment Committee. It is not stated how those committees are composed, and whilst I have no doubt that they fairly represent those parties who are known to be opposed to legislation on those lines, I do not think it will be contended that the body of opinion which is represented by those committees can be compared for a moment, either in weight or as regards scientific or educational experience, with that body of opinion which in various ways has given expression to its adoption of the principles upon which this Bill is founded. I think that in moving the second reading it may be sufficient if I say that, in my opinion, the arguments which are brought forward in this case do not establish any reason why the Bill should not be read a second time. There may be some points which are referred to in that case which may be worthy of attention in Committee, and I think that some of the statements may be eminently deserving of the attention of the Statutory Commission if it should be appointed under this Bill. Lord Davey has expressed his willingness to accept the position of chairman of this Commission if it should be appointed, and I trust that before the Bill leaves your lordships' House, or at all events as soon as there appears to be any possibility or probability of its being passed through the other House, I shall be in a position, in conjunction with him, to state the names of those gentlemen who it is proposed shall form the entire Commission. With this explanation I beg to move that this Bill be read a second time.

Lord Herschell: As I have the honour to be Chancellor of the University of London, it is only natural that I should desire to say a few words on the present occasion. The objections to the measure may, I think, be put under two heads. It is alleged that the scheme of the Commission of which Lord Cowper was chairman, even when subjected to the scrutiny and modification of the proposed Statutory Commission, would involve two consequences—that it would lower or tend to lower the standard of the degrees, and that it would be unfair or tend to unfairness towards those students who sought to obtain a degree without having been connected with any college or collegiate instruction. The opponents to the scheme, both in the statement they have recently made and in previous statements, always seem to me to assume that those will be the consequences. Their statement is founded upon assumption rather than any proof or evidence. If the members of the Senate shared the view of the opponents of the scheme that the consequences which they assumed would, in fact, necessarily result, I venture to say that the Senate of the University would have been found in the front rank of opposition to the scheme, and if they support the scheme it is not because they are indifferent either to the standard of education or to the interests of the external students, but because they believe that the present work of the University may be made even more valuable than it has been without any such risk as the opponents of the scheme consider must necessarily attach to it. The fear seems to be that the scheme which has been proposed would give the teachers in London schools and colleges more power in the direction of examinations and course of study than they possess at present, and that a likely consequence of their obtaining that greater power would be a lowering of the standard of education at the University. But here again we are not without experience. First let me say that the high standard that has been maintained has been largely due to the examiners. Who have the examiners been who have thus maintained the standard of the examinations? They have very largely consisted of the teachers of the London schools and colleges. That is a matter of experience which is of much more value than any mere assumption. There is a very large consensus of opinion amongst these teachers, who have had much greater experience than can be claimed by any body of graduates, in favour of the proposed changes. The Royal Commission have impartially considered the views of those who are in favour of the scheme and of those who are opposed to it, and they have arrived at the conclusion that the scheme is one which is likely to be of public advantage and will be detrimental to no one. I only desire further to remark that I think that the scheme of the Cowper Commission, although on the whole an admirable one, is susceptible of improvement in its details, leaving its general principles untouched. The very object of appointing the Statutory Commission is to carry out those recommendations, and that the details should be looked at by a body of able men, and that the weight and force of the objections raised to those details should be fully considered and, where necessary, modified. I know that some of the opponents of the present scheme desire to create another University in London alongside of the University of London. That is a question that has been considered by men of great weight and authority, who have very largely pronounced against the proposal. The House of Commons has emphatically pronounced against it, and I believe that the country has also pronounced against it in an equally emphatic manner. Under these circumstances, I believe the best hope for the solution of this question and for the increase, even, of the valuable work which the University of London has done, lies in the direction proposed by the noble Duke.

Earl Cowper, speaking as chairman of the Commission that considered this question, said that when the work of that body first began he was prepossessed in favour of the Gresham scheme, because he thought everybody would admit that, if there was to be a second University, that scheme would have been at least as good as any other which could have been devised. But he found that the large majority of his fellow Commissioners were of a contrary opinion, and as the evidence proceeded he became more and more convinced that the great bulk of opinion throughout the country, and more particularly in the metropolis, was not in favour of a second University, but in favour of one. He could not help feeling pretty sure that everybody who went through the voluminous mass of evidence would gradually come to the same conclusion as that at which he had arrived.

Lord Playfair said that he introduced a Bill last year for the purpose of converting the present London University into a teaching University, and as the noble Duke had accepted the Bill he would strongly urge that the Government should take the matter up in earnest, considering the enormous amount of support which they now had in regard to the scheme. This scheme had been under the consideration of educationists for a whole generation. Three times the Convocation of the existing London University had met and discussed the principles of this Bill, and by increasing and finally by an overwhelming majority had pronounced in their favour. The minority of the Convocation, and individual graduates in the country, refused to accept their defeat, and were still alarmed at the proposed changes in the constitution of the present University. At the basis of their opposition was the fear that the new University might lower the value of degrees, and thus lessen the honour in which the existing graduates were now held. This fear did not seem to be shared to any extent by graduates who had the highest degrees. They had never had it explained why an organised teaching University should think it to their interest to lower the value of the degrees. One would say that their interest was to keep up the degrees to the highest value, and he thought the graduates, when they considered the question, would gradually come to this view. London was the only large town—he would not say the only capital—which did not possess an organised teaching University. It was a most melancholy fact—a fact that was a disgrace to the metropolis, that, although the towns of great population possessed organised teaching Universities, the London University did not yet do so. It was impossible that the London University, with its present powers and its present charters, could constitute a teaching University in accord with the science of the time. Teaching by verbalism was more and more going out in science. Lecturers were of far less importance than experimental work in laboratories. For this purpose well-equipped colleges were absolutely necessary, and the object of the University would be to raise itself continually up to the level of science. The object of this scheme, for which educationists had been agitating for so many years, was to produce this result. The Bill would provide a system of education capable of raising itself continually to the heightening and advancing state of knowledge. It did not provide the means, however; but if they erected an organised University of which Londoners and the people of this country would be proud, he was perfectly sure that the funds would not be lacking. He would give one instance of why they should have that confidence. The late Royal Commission appointed a small committee, consisting of Prof. Burdon-Sanderson and himself, to consider the scientific part of the report; and they recommended the foundation of research laboratories for chemistry and physics, independent of the existing colleges, but open to any of the graduates who showed the power of advancing the boundaries of science by original researches. Their recommendation was adopted after some hesitation on the part of their colleagues, because they thought they were asking too much, for no funds were in view for building and equipping such laboratories or for maintaining them when equipped. The generosity of one scientific manufacturer —Mr. Mond—had already founded these laboratories, which two years ago looked so hopeless of accomplishment. Like results would follow in regard to other recommendations of the Commission. He would like to point out how important it was that a large community like that should put itself into the position of having organised University teaching as other places had. They were doing nothing in this country at the present moment compared with what was being done in other countries for the promotion of higher University education. After the Franco-German war the French Institute had a most interesting discussion upon the question, "Why did our late crisis produce no great people in this country?" and the universal feeling in the Institute was that France had not sufficiently attended to her higher University education. Renan, in summing up the whole debate, said:—"It is German science that won the day at Sadowa and Sedan. The German national spirit is a product of the German Universities, and the German Fatherland is a product of that spirit." Inspired by these views France, since the war, had spent nearly 100 millions of francs in equipping her higher colleges, so that they might suffice for modern scientific requirements; and it now spent annually about as much as Germany in higher education. Germany had not stood still. When she acquired Strasburg as a result of the war, she spent upon that small town no less than £711,000 sterling in the building of a new University and its scientific laboratories, and annually voted above £50,000 sterling for their maintenance. The future competitions of the world would not be determined by armies and navies alone, but would be mainly governed by the intellectual development of the people. In the presence of these facts, surely England could not allow its great capital to remain the only large town, either in the United Kingdom or abroad, which had no means by which organised University teaching could be given to her people.

Lord Reay said that the main purpose of the Bill was to put an end to an anomaly. London had a variety of institutions in which University education was given, but which had not the power of conferring degrees; and, on the other hand, London had an examining Board unconnected with the teaching institutions. The institutions had no crown to their edifice; the University had no foundation. The object of the scheme of the last Royal Commission was to constitute a corporate body out of these scattered fragments, and recognition was given on well-defined and broad lines to University teaching wherever it existed. The aim of the Bill was not merely educational. It had a much wider bearing. What was the cause of the increased expenditure on higher education on the continent? It was the consciousness that wealth and military power were insufficient; that higher education must provide the intellectual capital which agriculture, industry, and trade required. If we were to hold our own in this race we must use the same means. A London University would not be a mere local institution; it would eventually be an Imperial institution, profiting all classes throughout the Empire. The progress of the Bill was anxiously watched by scientific men at home as well as abroad. There was, indeed, practical unanimity among all those who had the higher interests of the country at heart that failure to give London a teaching University would be nothing less than a national disgrace.

Lord Kelvin felt that the reasons already put before their lordships for accepting the Bill were overwhelmingly strong, and he only wished to intervene because he had been mentioned as one apparently partially opposed to the provisions of the Bill. As a member of Lord Cowper's Commission he joined with Sir George Stokes and Mr. Weldon in a note expressing a preference for a separate teaching University. They had some doubts as to whether or not the functions of a teaching University could practically be added to the duties so well performed by the University of London of examining for degrees and conferring degrees upon students who had not had the benefit of instruction in colleges of universities in any part of the world. They felt the gravity of the objection that might be held to establishing another university—a rival university—beside the University of London; but when it seemed, as it did then seem to them, hopeless that the University of London could be got to undertake the duty of organising and carrying on a teaching University, they felt that the paramount object of having a teaching University in London should not on that account be given it. On his own behalf, and, he believed, on behalf of his colleagues in the note, he could say they would only have been too glad to have accepted what was now proposed by this Bill. Their doubts and hesitation had been completely set aside by what had passed. Personally he thoroughly approved of the Bill. He believed that an immense addition to the usefulness of the existing colleges in London would result from the passing of the measure. It was an anomalous state of things that there was no teaching University in London. It was not only London, but the United Kingdom, and, indeed, the whole world, that would benefit by the passing of the Bill, and therefore his desire was strong and evident, not only that the Bill might pass speedily through their lordships' House, but that it would be taken up by the House of Commons and made an Act of Parliament before the close of the present Session.

The Earl of Kimberley said that some years ago he had the honour to be President of University College, and at that time there was put forward a scheme for a separate University such as Lord Kelvin thought might be the only alternative. He then felt it would be a great misfortune if there were set up two rival universities in London, and therefore, he need hardly say, how greatly he rejoiced that they had arrived at last at a point where they seemed to have in view a conjunction of teaching and examining in the University of London. He was glad to see the noble Duke had inserted in the Bill the clause that the Commissioners were to see that provision was made for securing adequately the interests of collegiate and non-collegiate students respectively. That ought to reassure those who had placed themselves in opposition to the Bill, because an impartial Statutory Commission such as the noble Duke intended to appoint would be perfectly able to see that the statutes of the University were so framed that there would be no chance of any portion of the University work being impaired by a wrong administration of its powers.

The second reading was then agreed to.

SCIENCE FOR SECONDARY SCHOOLS.

THE Reports of the United States Commissioner of Education are known to be the most valuable publications on educational statistics and methods in the English language. The Report (1892–93), just distributed, may appear to be somewhat belated, but the contents are so instructive and exhibit so many special features, that the delay of publication may be forgiven. There are two volumes, running altogether into 2153 pages, and the amount of information contained in them is marvellous. Taking the volumes in order, we find in the first elaborate tables of statistics referring to the schools of the United States, and statistics of illiteracy for each of the States and for Europe. Then follow surveys of the educational system of Belgium, the elementary schools of Great Britain, the systems of education developed in the British Colonies, the French educational system, and a most instructive chapter on developments in the teaching of geography in Central Europe. The chapter on child-study, which practically concludes Part I. of the first volume, contains a number of interesting contributions from leading American representatives of this modern movement.

The second part of the first volume is devoted entirely to reports which were called forth on the occasion of the World's Columbian Exposition. Among these reports are detailed criticisms of American educational methods, by eminent French and German educationists. There is a survey of medical instruction in the United States, as presented in the reports of two French Commissioners appointed to make a special study of the subject, and an English version of a report on American technological schools, prepared by Prof. Riedler, of the Royal Polytechnicum at Charlottenberg. The remainder of the first volume is taken up with papers read at the Library Congress held during the Columbian Exposition, and notes on the educational exhibits.

The second volume contains the third and fourth parts of the Report. Prof. Hinsdale contributes to it a series of rare documents illustrative of American educational history, and there is incorporated in it the report of the Committee of Ten, appointed to take up the important subject of courses of instruction in secondary schools, and papers relating thereto. The chief interest for us in the volume lies in this valuable educational document.

The Committee, which was appointed by the National Council of Education, organised conferences of leading teachers of the principal subjects which enter into the programmes of secondary schools in the United States. Each of nine subjects was considered and reported upon by a conference consisting of ten members, who were selected on account of their scholarship and experience. Among the subjects discussed were four concerned with groups of sciences; viz. (1) mathematics; (2) physics, astronomy, and chemistry; (3) natural history (biology, including botany, zoology, and physiology); (4) geography (physical geography, geology, and meteorology). As a result of the conferences, a great number and variety of important changes in the scope and method of science teaching were recommended. All the conferences on scientific subjects agreed that laboratory work by the pupils was the best means of instruction, and dwelt upon the great utility of the genuine laboratory note-book; and they all declared that teachers of science in schools need at least as thorough a special training as teachers of languages or mathematics receive.

The most important recommendations made by the scientific conferences are summarised in the following pages. But all who are interested in scientific education should read the entire reports, for each is so full of suggestions and recommendations, that it is impossible to present adequate abstracts of them.

On one very important question of general policy, which affects the preparation of all school programmes, the Committee of ten, and all the conferences organised by it, were absolutely unanimous. Among the questions suggested for discussion in each conference was—"Should the subject be treated differently for pupils who are going to college, for those who are going to a scientific school, and for those who, presumably, are going to neither?" This question was answered unanimously in the negative by all the conferences; so that we have the fact that nearly one hundred eminent teachers agree that every subject which is taught at all in a secondary school should be taught in the same way and to the same extent to every pupil so long as he pursues it, no matter what the probable destination of the pupil may be, or at what point his education is to cease.

MATHEMATICS.

The form of the report of the conference on mathematics differs somewhat from that of the other reports. This report is subdivided under five headings: (1) General conclusions; (2) the teaching of arithmetic; (3) the teaching of concrete geometry; (4) the teaching of algebra; (5) the teaching of formal or demonstrative geometry.

The first general conclusion of the conference was arrived at unanimously. The conference consisted of one Government official and university professor, five professors of mathematics in as many colleges, one principal of a high school, two teachers of mathematics in endowed schools, and one proprietor of a private school for boys. The professional experience of these gentlemen and their several fields of work were various, and they came from widely separated parts of America; yet they were unanimously of opinion "that a radical change in the teaching of arithmetic was necessary." They recommend "that the course in arithmetic be at once abridged and enriched; abridged by omitting entirely those subjects which perplex and exhaust the pupil without affording any really valuable mental discipline, and enriched by a greater number of exercises in simple calculation, and in the solution of concrete problems." They specify in detail the subjects which they think should be curtailed or entirely omitted, and they give in their special report on the teaching of arithmetic a full statement of the reasons on which their conclusion is based. They map out a course in arithmetic which, in their judgment, should begin about the age of six years, and be completed at about the thirteenth year of age.

The conference next recommend that a course of instruction in concrete geometry with numerous exercises be introduced into the grammar schools, and that this instruction should, during the earlier years, be given in connection with drawing. They recommend that the study of systematic algebra should be begun at the age of fourteen; but that, in connection with the study of arithmetic, the pupils should earlier be made familiar with algebraic expressions and symbols, including the method of solving simple equations. "The conference believe that the study of demonstrative geometry should begin at the end of the first year's study of algebra, and be carried on by the side of algebra for the next two years, occupying about two hours and a half a week." They are also of opinion "that if the introductory course in concrete geometry has been well taught, both plane and solid geometry can be mastered at this time." Most of the improvements in teaching arithmetic which the conference suggest "can be summed up under the two heads of giving the teacher a more concrete form, and paying more attention to facility and correctness in work. The concrete system should not be confined to principles, but be extended to practical applications in measuring and in physics."

In regard to the teaching of concrete geometry, the conference urge that while the student's geometrical education should begin in the kindergarten, or at the latest in the primary school, systematic instruction in concrete or experimental geometry should begin at about the age of ten for the average student, and should occupy about one school hour a week for at least three years. From the outset of this course, the pupil should be required to express himself verbally as well as by drawing and modelling. He should learn to estimate by the eye, and to measure with some degree of accuracy lengths, angular magnitudes, and areas; to make accurate plans from his own measurements and estimates; and to make models of simple geometrical solids. The whole work in concrete geometry will connect itself on the one side with the work in arithmetic, and on the other with elementary instruction in physics. With the study of arithmetic is therefore to be intimately associated the study of algebraic signs and forms, of concrete geometry, and of elementary physics. Here is a striking instance of the interlacing of subjects which seems so desirable to every one of the conferences.

Under the head of teaching algebra, the conference set forth in detail the method of familiarising the pupil with the use of algebraic language during the study of arithmetic. This part of the report also deals clearly with the question of the time required for the thorough mastery of algebra through quadratic equations. The report on the teaching of demonstrative geometry is a clear and concise statement of the best method of teaching this subject. It insists on the importance of elegance and finish in geometrical demonstration, for the reason that the discipline for which geometrical demonstration is to be chiefly prized is a discipline in complete, exact, and logical statement. If slovenliness of expression, or awkwardness of form is tolerated, this admirable discipline is lost. The conference therefore recommend an abundance of oral exercises in geometry—for which there is no proper substitute—and the rejection of all demonstrations which are not exact and formally perfect. Indeed, throughout all the teaching of mathematics the conference deem it important that great stress be laid by the teacher on accuracy of statement and elegance of form as well as on clear and rigorous reasoning. Another very important recommendation in this part of the report is to be found in the following passage: "As soon as the student has acquired the art of rigorous demonstration, his work should cease to be merely receptive. He should begin to devise constructions and demonstrations for himself. Geometry can not be mastered by reading the demonstrations of a text-book, and while there is no branch of elementary mathematics in which purely receptive work, if continued too long, may lose its interest more completely, there is also none in which independent work can be made more attractive and stimulating." These observations are entirely in accordance with the recent practice of some colleges in setting admission examination papers in geometry which demand of the candidates some capacity to solve new problems, or rather to make new application of familiar principles.

PHYSICS, CHEMISTRY, AND ASTRONOMY.

The members of this conference were urgent that the study of simple natural phenomena be introduced into elementary schools, and it was the sense of the conference that at least one period a day from the first year of the primary school should be given to such study. Apparently the conference entertained the opinion that the present teachers in elementary schools are ill prepared to teach children how to observe simple natural phenomena; for their second recommendation was that special science teachers or superintendents be appointed to instruct the teachers of elementary schools in the methods of teaching natural phenomena. The conference were clearly of opinion that from the beginning this study should be pursued by the pupil chiefly, though not exclusively, by means of experiments and by practice in the use of simple instruments for making physical measurements. The report dwells repeatedly on the importance of the study of things and phenomena by direct contact. It emphasises the necessity of a large proportion of laboratory work in the study of physics and chemistry, and advocates the keeping of laboratory note-books by the pupils, and the use of such note-books as part of the test for admission to college. At the same time the report points out that laboratory work must be conjoined with the study of a text-book and with attendance at lectures or demonstrations, and that intelligent direction by a good teacher is as necessary in a laboratory as it is in the ordinary recitation or lecture room.

The great utility of the laboratory note-book is emphatically stated. To the objection that the kind of instruction described requires much time and effort on the part of the teacher, the conference reply that to give good instruction in the sciences requires of the teacher more work than to give good instruction in mathematics or the languages; and that the sooner this fact is recognised by those who have the management of schools the better for all concerned. The science teacher must regularly spend much time in collecting materials, preparing experiments, and keeping collections in order, and this indispensable labour should be allowed for in programmes and salaries. As regards the means of testing the progress of the pupils in physics and chemistry, the conference were unanimously of opinion that a laboratory examination should always be combined with an oral or written examination, neither test taken singly being sufficient. There was a difference of opinion in the conference on the question whether physics should precede chemistry, or chemistry physics. The logical order would place physics first; but all the members of the conference but one advised that chemistry be put first for practical reasons which are stated in the majority report. A sub-committee of the conference has prepared lists of experiments in physics and chemistry for the use of secondary schools, not, of course, as a prescription, but only as a suggestion, and a somewhat precise indication of the topics which the conference had in mind, and of the limits of the instruction.

NATURAL HISTORY.

The conference on natural history unanimously agreed that the study of botany and zoology ought to be introduced into the primary schools at the very beginning of the school course, and be pursued steadily, with not less than two periods a week, throughout the whole course below the high school. In the next place, they agreed that in these early lessons in natural science no text book should be used; but that the study should constantly be associated with the study of literature, language, and drawing. It was their opinion that the study of physiology should be postponed to the later years of the high school course; but that in the high school, some branch of natural history proper should be pursued every day throughout at least one year. Like the report on physics, chemistry, and astronomy, the report on natural history emphasises the absolute necessity of laboratory work by the pupils on plants and animals, and would have careful drawing insisted on from the beginning of the instruction.

As the laboratory note-book is recommended by the conference on physics, so the conference on natural history recommends that the pupils should be made to express themselves clearly and exactly in words, or by drawings, in describing the objects which they observe; and they believe that this practice will be found a valuable aid in training the pupils in the art of expression. They agree with the conference on physics, chemistry, and astronomy that science examinations should include both a written and a laboratory test, and that the laboratory note-books of the pupils should be produced at the examination. The recommendations of this conference are therefore very similar to those of the physical conference, so far as methods go; but there are appended to the general report of the conference on natural history sub-reports which describe the proper topics, the best order of topics, and the right methods of instruction in botany for schools below the high school, and for the high school itself, and in zoology for the secondary schools. Inasmuch as both the subject-matter and the methods of instruction in natural history are much less familiar to ordinary school teachers than the matter and the methods in the languages and mathematics, the conference believed that descriptive details were necessary in order to give a clear view of the intentions of the conference. In another sub-report the conference give their reasons for recommending the postponement to the latest possible time of the study of physiology and hygiene. Like the sixth conference, the conference on natural history protest that no person should be regarded as qualified to teach natural science who has not had special training for this work—a preparation at least as thorough as that of their fellow teachers of mathematics and the languages.

GEOGRAPHY.

Considering that geography has been a subject of recognised value in elementary schools for many generations, and that a considerable portion of the whole school-time of children has long been devoted to a study called by this name, it is somewhat startling to find that the report of the conference on geography deals with more novelties than any other report, exhibits more dissatisfaction with prevailing methods, and makes, on the whole, the most revolutionary suggestions.

It is obvious, on even a cursory reading of the majority and minority reports, that geography means for all the members of this conference something entirely different from the term "geography" as generally used in school programmes. Their definition of the word makes it embrace not only a description of the surface of the earth, but also the elements of botany, zoology, astronomy, and meteorology, as well as many considerations pertaining to commerce, government, and ethnology. "The physical environment of man" expresses as well as any single phrase can the conference's conception of the principal subject which they wish to have taught. No one can read the reports without perceiving that the advanced instruction in geography which the conference conceive to be desirable and feasible in high schools cannot be given until the pupils have mastered many of the elementary facts of botany, zoology, geometry, and physics. It is noteworthy also that this conference dealt avowedly and unreservedly with the whole range of instruction in primary and secondary schools. They did not pretend to treat chiefly instruction in secondary schools, and incidentally instruction in the lower schools; but, on the contrary, grasped at once the whole problem, and described the topics, methods, and apparatus appropriate to the entire course of twelve years. They recognised that complete descriptions would be necessary in all three branches of the subject—topics, methods, and equipment; and they have given these descriptions with an amplitude and force which leave little to be desired.

More distinctly than any other conference, they recognised that they were presenting an ideal course which could not be carried into effect everywhere or immediately. Indeed, at several points they frankly state that the means of carrying out their recommendations are not at present readily accessible, and they exhibit the same anxiety which is felt by several other conferences about training teachers for the kind of work which the conference believe to be desirable. After the full and interesting descriptions of the relations and divisions of geographical science, as the conference define it, the most important sections of their report relate to the methods and means of presenting the subject in schools, and to the right order in developing it. The methods which they advocate require not only better equipped teachers, but better means of illustrating geographical facts in the schoolroom, such as charts, maps, globes, photographs, models, lantern slides, and lanterns. Like all the other conferences on scientific subjects, the ninth conference dwell on the importance of forming from the start good habits of observing correctly and stating accurately the facts observed. They also wish that the instruction in geography may be connected with the instruction in drawing, history, and English. They believe that meteorology may be taught as an observational study in the earliest years of the grammar school, the scholars being even then made familiar with the use of the thermometer, the wind vane, and the rain gauge; and that it may be carried much further in the high school years, after physics has been studied, so that the pupils may then attain a general understanding of topographical maps, of pressure and wind charts, of isothermal charts, and of such complicated subjects as weather prediction, rainfall and the distribution of rain, storms, and the seasonal variations of the atmosphere.

Their conception of physiography is a very comprehensive one. In short, they recommend a study of physical geography which would embrace in its scope the elements of half a dozen natural sciences, and would bind together in one sheaf the various gleanings which the pupils would have gathered from widely separated fields. There can be no doubt that the study would be interesting, informing, and developing, or that it would be difficult and in every sense substantial.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

PROF. G. B. MATHEWS has resigned the chair of Mathematics in the University College of North Wales, in order to be able to devote more time to study and research.

THE Executive Committee of the City and Guilds of London Institute have appointed Mr. W. E. Dalby, since 1891 University Demonstrator of Mechanism and Applied Mechanics at Cambridge, to the Professorship of Mechanics and Applied Mathematics at the Institute's Technical College, Finsbury, rendered vacant by the resignation of Prof. Perry.

Science reports the dedication, at the University of Vermont, of two new buildings—Converse Hall, a dormitory presented to the University by John H. Converse at a cost of £25,000; and a science building presented by Dr. Edward H. Williams, which, with its equipment, will cost about £40,000. The dormitory was formally presented to the University by Mr. Converse; and the science building, in the absence of Dr. Williams, by his son, Prof. Edward H. Williams, jun., of Lehigh University. On the front of the latter building are three medallions with the heads of Agassiz, Henry, and Prof. Marsh. The building contains ample accommodation for the departments of physics, chemistry, biology, electrical engineering, and metallurgy.

EARL SPENCER, in distributing the prizes on Monday to the successful pupils at Northampton School, spoke of the absolute necessity of a sound primary education for a sound secondary technical and even University education. In Japan, and in Canada, too, he found that both secondary and University education were secured to the people. The fact that England should be behind was rather curious, and he took it that a great deal of it was due to the old grammar schools and the dislike of Parliament, with these schools existing, to create a national system of secondary education in England. That more secondary and University education was required was illustrated by the fact that, while Germany, with a population of 45,000,000, had 24,000 people using her Universities, England, with 30,000,000, had only 5500 at the University.

WE learn from *Science* that a State Veterinary College has been established in New York. It is pointed out that the animal industry of the State is so important and extensive, and the relations of animal diseases so intimately interwoven with human health and well-being, that the financial and sanitary interests of the State will derive benefit from the knowledge and continued investigations of the body of experts which the College will bring together. The following have already been appointed upon the staff of the College:—Director and Professor of Veterinary Medicine, Principles and Practice, Zymotic Diseases, and State Medicine, Dr. James Law; Professor of Veterinary and Comparative Pathology and Bacteriology, Dr. V. A. Moore; Assistant Professor of Veterinary and Comparative Physiology, Materia Medica and Pharmacy, Dr. P. A. Fish; Assistant Professor of Veterinary Anatomy and Anatomical Methods, Dr. G. S. Hopkins; Professor of Microscopical Technology, Histology and Embryology, S. H. Gage; Instructor in Microscopy, Histology and Embryology, Dr. B. F. Kingsbury; Assistant in Veterinary Bacteriology, Dr. R. C. Reed.

SCIENTIFIC SERIALS.

Wiedemann's Annalen der Physik und Chemie, No. 7.—Polarised fluorescence, by L. Sohncke. The polarisation of fluorescent light is capable of giving hints concerning the manner in which the molecules of a solid substance vibrate, and its study may form the basis of the kinetic theory of solids. Theoretically, all doubly-refracting crystals should emit polarised fluorescence. This is found to be the case. Crystals of the regular system are the only crystals which do not. The author has investigated the fluorescence of a large number of substances in confirmation of this view.—Uniformities in the spectra of solid bodies, by F. Paschen. The author investigates the distribution of energy in the spectrum of glowing iron oxide at various temperatures. Of the formula hitherto proposed for its expression, that of Weber most closely approaches the reality. It gives a nearly parabolic curve in which the energy declines on both sides from a maximum which decreases in wave-length as the temperature rises. But the want of symmetry in Weber's curve is greater than in reality. The author finds a new formula, for which he claims that it covers all the observations.—The electrical behaviour of vapours from electrified liquids, by G. Schwalbe. The author finds that the vapours rising from electrified liquids are not capable of bearing away with them any portion of the electric charge, and that Exner's theory of atmospheric electricity must therefore be abandoned.—The damping action of magnetic fields upon rotating insulators, by William Duane. Cylinders and discs of glass, sulphur, paraffin, ebonite, or quartz, oscillating between the poles of a magnet with their axes vertical and at right angles to the lines of force, experience a damping action proportional to the field intensity and to the speed of rotation. This is not due to an action on the suspending threads, nor on the viscosity of the air, nor an electrostratic effect from the current in the coils, nor to induction currents in the substance, as was proved by test experiments and calculations. It must therefore be regarded as a hitherto unobserved magnetic effect upon the insulators in question.—Effect of magnetism upon electromotive force, by A. H. Bucherer. The author finds that in solutions of neutral ferrous salts no E.M.F. exceeding 0·00001 volt can be produced by the magnetisation of one of the two iron electrodes. The E.M.F.s observed by Gross and others must be attributed to changes of concentration produced by the magnetised electrode during its solution.—On the measurement of flame temperatures by thermo-elements, especially the temperature of the Bunsen burner, by W. J. Waggener. The temperatures were determined by various thermo-couples in different parts of the flame. The highest temperature, 1700° C., was indicated in the lower portion of the external mantle. But an infinitely thin thermo-element free from conduction would probably indicate over 1770°. A wire 0·05 mm. thick still suffers from conduction, and it is actually fused in the hottest portion. A more refractory metal is required for these measurements.

Bollettino della Società Sismologica Italiana, vol. ii., 1896, No. 1.—Velocity of propagation of the Paramythia (Epirus) earthquake of the night of May 13–14, 1895, by Dr. G. Agamennone. From time-observations obtained at several places near the epicentre, at six Italian observatories and at Nicolaiew, it appears that the early tremors travelled with a velocity of 1·94 km. per sec., and the oscillations constituting the maximum phase at the rate of 1·42 km. per sec. There is no evidence of any change in the velocity with the distance from the epicentre.—Vesuvian notes (July–December 1895), by Prof. G. Mercalli.

THE last number of the *Izvestia* of the Russian Geographical Society (1895, vi.) contains a new map of Lake Onega, in which last year's measurements of the depths of the lake are embodied. The greatest depths are in its western part, where they attain from 31 to 68 fathoms. This last depth is reached in the branch by which the lake protrudes towards the north-west. A narrow valley is thus formed at its bottom, and runs north-west to south-east, in the direction of the glacial striation in that region. Another great depth is found at the top of the other fjord-like bay in the northern portion of the lake, also directed to the north-west.

WE find in the last numbers of the *Izvestia* of the East-Siberian branch of the Russian Geographical Society (1895, Nos. 1 to 5) a very good sketch of the Yakutes of Verkhoyansk, by S. Kovalik; and an interesting note on the little-known customary hunting laws of the Buryates, by M. Croll; as also a full translation, from the Mongolian, of the renowned Buddhist "Mirror of Wisdom," which gives the "History of the Kingdom of Sukawadi."—M. Prein's preliminary article on the presence of the lime-tree in the neighbourhood of Krasnoyarsk is especially interesting. It is known that that tree does not appear to the east of the Urals, and only reappears in the Amur region on the very slopes of the high central plateau. But it was lately found in the Kuznetsk Alatau mountains, and has now been discovered further to the north-east, in the neighbourhood of Krasnoyarsk.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 18.—"Magnetisation of Liquids." By John S. Townsend.

The experiments on the coefficient of magnetisation of liquids were made with a sensitive induction balance. Both circuits were commuted about sixteen times a second, so that very small inductances could be detected by the galvanometer in the secondary circuit. The principle of the method consisted in balancing the increase of the mutual induction of the primary on the secondary of a solenoid arising from the presence of a liquid in the solenoid against known small inductances. Thus, if the sum of the inductances be reduced to zero, as shown by the galvanometer in the secondary giving no deflection, the balance will be disturbed to the extent $4\pi k M$, due to the insertion of a liquid into the solenoid whose coefficient of magnetisation is k, and the galvanometer in the secondary circuit will give a deflection when the commutator revolves. An adjustable inductance is then reduced by a known amount, m, till the deflection disappears; so that we get

$$4\pi k M = m \qquad \therefore k = m/4\pi M,$$

where m and M are quantities easily calculated.

Since the formula does not contain either the rate of the rotation of the commutator or the value of the primary current, no particular precautions are necessary to keep these quantities constant.

In all the determinations the magnetising force was varied from 1 to 9 centigram units, and in no case was there any variation in k. The densities of the salts in solution were also varied over large ranges, and showed that the coefficient of magnetisation for ferric salts in solution depended only on the quantity of iron per c.c. that was present, giving the formula

$$10^7 k = 2660\ W - 7\cdot 7$$

for ferric salts, where W is the weight of iron per c.c., the quantity $-7\cdot 7$ arising from the diamagnetism of the water of solution.

A similar result was obtained for ferrous salts, the corresponding formula being

$$10^7 k = 2060\ W - 7\cdot 7,$$

the temperature being 10° C.

Experiments were also performed to find the effect of heating, and they showed a great diminution in the value of k as the temperature increased, thus letting $k = k_0 (1 - \alpha t)$ the coefficient α is the same for ferrous and ferric salts, being a function of the temperature only, its value at the lower temperatures between 5° and 25° C. being about ·0055, and at the higher temperatures between 65° and 75° C. its value is ·0035.

PARIS.

Academy of Sciences, July 20.—M. A. Chatin in the chair.—Laws of uniform flow to the second approximation in circular tubes and in semicircular canals, by M. J. Boussinesq. A continuation of previous papers on the same subject.—Study of lanthanum carbide, by M. H. Moissan. This carbide, which is obtained from the oxide and carbon in the usual manner, forms a transparent yellowish crystalline mass, of the composition LaC_2. Water rapidly decomposes it at the ordinary temperature, giving acetylene, ethylene, and methane, with traces of solid and liquid hydrocarbons.—The relations between the expenditure of energy of a muscle and the amount of shortening it undergoes, by M. A. Chauveau. The method of the respiratory exchanges was used in this as in previous work on the same subject. For a given amount of external work done by the muscle, the energy used up is smaller as the muscle is nearer to its maximum length.—Report on a memoir of M. Jäderin, concerning a new method of measuring a base line, by M. Bassot. By the substitution of wires for steel tape, there is a gain in speed and also in the initial expense. The results obtained in working over well-established base lines, although agreeing amongst themselves to 1/100,000, disagree by 1/25,000 from the mean results of other methods.—Mirages and refractions observed on Lake Leman, by M. F. A. Forel.—On the photography of the sounds of the heart, by M. A. de Holowinski. The sounds are transmitted by a sensitive microphone to an optical telephone, the diaphragm of which produces Newton's rings, which are photographed.—On a new method of treating tuberculosis, by M. Fr. Crôtte.—The Secretary announced the death of M. A. Kékulé von Stradonitz, Correspondent in the Section of Chemistry, on July 13.—On the summations of Gauss, by M. P. de Séguier.—On the definite quadratic forms of M. Hermite, by M. Alfred Lœwy.—On an electroscope with three gold leaves, by M. L. Benoist. The instrument described has the advantages of increased sensibility and greater certainty in the measurement of the angle of deflection.—On metallic alloys, by M. H. Gautier. Giving fusibility curves for cadmium-silver, zinc-silver, and tin-silver alloys.—On the oxygen salts of mercury, by H. Raoul Varet. A thermochemical study of the condition of some mercuric salts on solution in dilute acids.—On the action of the halogen compounds of phosphorus upon iron, nickel, and cobalt, by M. A. Granger. The phosphides Fe_4P_3, Ni_2P, and Co_2P, together with the chlorides, were obtained.—On some combinations of iodic acid with other acids, by M. Paul Chrétien. An account of the salts of molybdo-iodic, metatungsto-iodic, and phospho-iodic acids.—Action of ammonia upon the paratungstates of potash and soda, by M. L. A. Hallopeau.—Action of reducing agents upon the nitroso-compounds of osmium, by M. L. Brizard. By the reduction of $Os(NO).OK$ in acid solution with stannous chloride an amido-compound, $Os(NH_2).Cl_3.KCl$, is obtained.—Fermentation of uric acid by micro-organisms, by M. E. Gérard. Under certain conditions it is possible to split up uric acid in such a manner that the whole of the nitrogen appears as urea, no ammonia being formed.—Action of the chloride of sulphur upon penta-erythrite, by M. J. Bougault. A chlorhydrin and sulphurous ether are formed simultaneously.—On the determination of the freezing-point of dilute aqueous solutions, by M. A. Ponsot. A discussion of the correction recently proposed by M. Raoult.—Estimation of alcohol in the blood after direct injection into the veins, or the introduction of alcoholic vapour into the lungs, by M. N. Gréhant.—Coagulating action of the prostatic fluid upon the contents of the seminal vesicles, by MM. L. Camus and E. Gley.—The influence of lecithine upon the growth of warm-blooded animals, by M. B. Danilewsky. The injection of lecithine in small quantities causes an acceleration in growth.—On the dorsal apodeme of the Araneida, by M. Causard.—On the tubercle disease of the vine, by M. F. Lataste. The tubercles are shown to be very contagious. It is necessary to destroy by fire all infected stocks to stamp out the disease.—Direct estimation of ethyl alcohol in solutions where it is diluted in proportions between 1/500th and 1/3000th, by M. Maurice Nicloux.

PHILADELPHIA.

Academy of Natural Sciences, June 16.—The following papers were presented for publication:—"On a collection of fishes obtained in Swatow, China, by Miss Adele M. Fielde," by Cloudesley Rutter; "On a collection of fishes made by the Rev. Jos. Seed Roberts in Kingston, Jamaica," by David Starr Jordan and Cloudesley Rutter.—Prof. Edward D. Cope continued his report on the vertebrate remains from the Port Kennedy bone-fissure. Among the Mustellidæ were five new species of the genera Lutra, Mephitis, Osmotherium, and Putorius. They were represented by at least forty individuals, and were described and named. Remains of the largest known tortoise from this section of the country were described as belonging to a new species of Clemmys. *C. insculpta* was also represented, together with a new box-tortoise belonging to the genus Toxaspis. A close ally of the black snake was also described.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Catalogue of the Fossil Bryozoa in the Department of Geology, British Museum (Natural History). The Jurassic Bryozoa: Dr. J. W. Gregory (London).—Annuario p.p. Observatorio do Rio de Janeiro, 1896 (Rio de Janeiro).—Report on the Work of the Horn Scientific Expedition to Central Australia. Part 3. Geology and Botany (Dulau).—Practical Mechanics applied to the Requirements of the Sailor: T. Mackenzie (Griffin).—Handbuch der Gewebelehre des Menschen: Prof. A. Koelliker, Sechste Umgearbeitete Auflage, Band 1 and 2 (Leipzig, Engelmann).—Everybody's Guide to Chess and Draughts: H. Peachey (Saxon).—Everybody's Cycling Law: S. Wright and C. W. Browne (Saxon).—Erkenntnistheoretische Grundzüge der Naturwissenschaften, &c.: Dr. P. Volkmann (Leipzig, Teubner).—Three Essays on Australian Weather: Hon. R. Abercromby (Sydney, White).—Essai de Paléontologie Philosophique: Prof. A. Gaudry (Paris, Masson).—Die Mikrotechnik der Thierischen Morphologie: Dr. S. Apáthy, Erste Abthg. (Braunschweig, Bruhn).

PAMPHLETS.—American Museum of Natural History, Annual Report, 1895 (New York).—History of Modern Mathematics: Prof. D. E. Smith (Chapman).—The X-Rays: A. Thornton (Lund).—Das Parallelogramm der Kräfte: Dr. J. Sperber (Zürich, Speidel).

SERIALS.—Lloyd's Natural History. Butterflies: W. F. Kirby. Part 3 (Lloyd).—Proceedings of the Academy of Natural Sciences of Philadelphia, 1896, Part 1 (Philadelphia).—Royal Natural History, Part 33 (Warne).—History of Mankind: F. Ratzel, translated, Part 10 (Macmillan).—Himmel und Erde, July (Berlin, Paetel).—American Journal of Psychology, Vol. 7, No. 4 (Worcester, Mass.).

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