oxides and hydrides, acids, bases and salts, halogen compounds and sulphides.

The preparation of the nitrides of calcium and boron strikes us as strange. The calcium or boron are heated in the air, and so one obtains a mixture of oxide and nitride. As, however, the oxide and nitride cannot be separated, there seems very little point in the experiment, except that the student's attention is directed to ammonia from the air. Certainly, but if the air is first passed over red-hot copper, thus freed from oxygen, and subsequently passed over the heated calcium or boron, surely the experiment is much more striking, and, further, the pure nitride is prepared. This method of preparation would also lead up to a discussion of argon and similar gases.

The book will undoubtedly be of great use to teachers of inorganic chemistry and others who wish to study the subject from a preparatory point of view, but it is rather too full for the average student, who would certainly require very careful direction, or he would be inclined to wander along in a rather aimless fashion. F. M. P.

The Bacteriological Examination of Disinfectants. By William Partridge. With a preface by Major C. E. P. Fowler. Pp. 66. (London: The Sanitary Publishing Co., Ltd., 1907.) Price 2*s.* 6*d.* net.

THE subject of disinfectants has lately attracted considerable attention, and Mr. Partridge's little book forms a very useful summary of the methods employed for testing bacteriologically the germicidal value of disinfectants. The Rideal-Walker or "drop" method is rightly that most favoured, and the major part of the book is devoted to it. We doubt if the explanation given on p. 17, that a forty-eight hours' culture of *B. typhosus* is less readily killed by a disinfectant than a twenty-four hours' one, because it is more vigorous, is correct; we should ascribe the fact rather to the greater number of bacilli and to clumping in the older culture. On p. 18 it is said that while a broth having a reaction of +1·5 is suitable for the culture of the typhoid bacillus, for the diphtheria and cholera organisms a "neutral or alkaline broth must be substituted." The broth named is quite suitable for these organisms, for it is alkaline in the ordinary acceptation of the term; though acid to phenolphthalein, it is still alkaline to litmus. On p. 34 an experiment is quoted to show that an organism from different sources may have a different resisting power from a disinfectant. Doubtless this is so, but the experiment does not prove it. The experiment shows that two strains of the typhoid bacillus, with strengths of carbolic of 1 in 70 and 1 in 100 respectively, are killed in between 5 and 7½ minutes; obviously the one might have been killed in 5¼ minutes, the other in 7¼ minutes, and actually there might have been little difference between them. Everyone has his own method of manipulating tubes for inoculation, but we do not like either method depicted in Figs. 3 and 4. Major Fowler, R.A.M.C., contributes a useful introduction.

R. T. HEWLETT.

Ergebnisse und Fortschritte der Zoologie. Edited by Dr. J. W. Spengel. Vol. i., part i. (Jena: Gustav Fischer.)

UNDER the above title Mr. Gustav Fischer is issuing a new zoological journal, of which a variable number of parts are to appear each year, the whole to form an annual volume at the price of sixty marks. As no prospectus is issued with the part now before us, we are unable to indicate the ground which the publication is specially intended to cover. The present part contains 238 somewhat closely printed 8vo pages, illustrated by fifty text-figures; and from this we presume that plates do not enter into the scheme of the new venture. The name of the editor is a sufficient guarantee that only papers of a high order will be accepted for publication, this being fully borne out by the contents of the initial number. These comprise a discussion on chromosomes by Mr. Valentin Häcker, of Stuttgart; an article by Dr. Richard Heymons on the various types of insect metamorphosis, and their relation to the metamorphoses of other arthropods; and another, by Mr. O. Maas, of Munich, on the scyphomedusæ. The new enterprise has our best wishes for success. R. 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.]*

Seismographs and Seismograms.

As I have had occasion to study and compare the records of nearly all the types of seismographs for recording distant earthquakes which are now in use, I may perhaps be permitted to add something to Prof. Milne's letter in NATURE of January 2. The nature of the records, and the relative merits of different types, of seismographs, are not generally apprehended in England, and appear to be misunderstood in Strassburg, from whence much of the recent seismological literature has been inspired.

The two leading problems of seismology, as it stands at present, are the determination *firstly*, of the exact nature and amount of the movement which takes place, and, *secondly*, of the time requisite for the transmission of the different types of disturbance from the origin, to various distances, and in various directions, through the earth, or along its surface. The first of these is naturally the special object of purely seismological stations and observatories, and for it no single instrument or type of instrument will be sufficient. From the mathematical and experimental investigations of the mechanics of seismographs by Prince Galitzin, Prof. Rudzki and others, it has been conclusively established that no form of instrument, having a pendular period of vibration of its own, however perfectly the oscillations may be damped, can possibly record with exactitude an undulatory movement of the soil such as is caused by earthquakes. As every instrument giving a continuous record must necessarily be of the nature of a pendulum of some sort or other, owing to the necessity for bringing the recording point back to the zero line of the record, it is obvious that no single instrument can suffice for this purpose, and that the only way, by which an understanding of the nature of the movement of the soil can be arrived at, is by installing a number of instruments, of different types and varying response to movements in diverse directions and of unlike period.

For the second purpose a totally different set of conditions comes in. It is no longer necessary to attempt an exact, or even an approximate, representation of the actual movement of the ground, so long as the instruments give records in which the different phases of wave motion can be recognised with reasonable certainty; but, since the solution of this problem involves the collection of numerous records from many stations, it is necessary to obtain the cooperation of astronomical, physical, meteorological, and other observatories, and, consequently, certain conditions, which may be ignored in a specially seismological station, have to be taken into consideration. These are:—

(1) The instrument must not be unduly cumbrous or bulky; it must be easy of transport, occupy only a moderate floor space, and not require special and expensive foundations.

(2) It must run without much attention, and at as moderate a cost as possible.

(3) It must be sufficiently sensitive and consistent in its action to give records capable of interpretation as a general

rule—no instrument has yet been devised which will invariably do this—but should not be too sensitive, or the record of important disturbances may be lost.

(4) The records should be capable of easy and rapid reproduction.

Of the instruments which have been designed or suggested for this purpose, four types are in use to a greater or less extent.

The Wiechert so-called astatic pendulum is an inverted pendulum with a bob weighing more than a ton, kept in position by two springs, and provided with an ingenious system of air-damping of its vibrations. This instrument has been recommended for general use, because its supposed astatic nature is believed to make it record the movement of the soil in an accurate manner; as already pointed out, this condition is immaterial, and, moreover, cannot be completely fulfilled. The instrument is undoubtedly a fine one, and gives valuable records, but its proper place is in an observatory specially devoted to seismology; for general use it is too heavy, requires too much attention, and gives records which are not adapted for ready and rapid reproduction.

The Rebeur-Ehlert instrument is a horizontal pendulum, of the form devised by v. Rebeur-Paschwitz, combined with a recording arrangement devised by Prof. Ehlert. This instrument is an extremely sensitive one, and there seems to be none better for recording small disturbances; in the case of large earthquakes the record is apt to be lost. The record is photographic, and the seismograms are readily reproduced by photography. Its cost of maintenance and too great sensitiveness are the points in which it fails to meet the requirements of an instrument for general adoption.

The so-called Omori pendulum is a horizontal pendulum presenting no special peculiarities, and is a modification, in details only, of a type of instrument in very general use. It fulfils all the first three requirements, being moderate in size, needing little attention, and gives good records, easy of interpretation and measurement. It fails in the fourth requirement only; the record, being taken on smoked paper, is not readily reproduced by photography, and is on too small a scale to obviate the risk of introducing error when copied by tracing.

The fourth type of instrument is the Milne pendulum, a horizontal pendulum with photographic record on a principle quite different from that adopted in any other instrument. This fulfils all the first three requirements, and the fourth too; the seismograms are easily reproduced by contact printing on to the same photographic paper that is used for recording, and the copies are practically as serviceable as the originals. This alone, if the instrument had few other merits, would mean much; but in addition to this I have found its seismograms the most convenient of any for determining the exact time of any point on the record, and had it not been for the general adoption of this type of instrument, and the ease with which its records can be reproduced, a considerable part of what seismological work I have been able to do could not have been attempted. The only improvement I have ever desired to see is an increase in the rate of movement of the recording surface, and this has now been introduced. I have examined and studied hundreds of records of this instrument from different stations; from Victoria, Toronto, Cape Town, Bidston, Paisley, and many other places, its records are consistently good; at a few stations, whether from a defect in the particular instrument, a want of proper adjustment, or, more probably, something in the foundations or the subsoil, its records are less satisfactory, but from none do they seem to be so bad as at Strassburg; having never seen a seismogram of that instrument—it is not easy to get copies from Strassburg—it is impossible to hazard a suggestion of the reason for the failure of the instrument at this station.

No one would wish to see one pattern of instrument adopted to the exclusion of all others, nor has it ever been pretended that the pattern adopted by the organisation which has grown up under the auspices of the British Association is faultless; but for the purpose of securing a large number of records for comparison with each other, and thereby determining the rate of transmission of earthquakes across, through, and around the earth, it is no more faulty than any other pattern, and has one crowning merit which they do not possess. Can it be to this, to the ease of reproduction of its records, which renders unnecessary the centralisation of seismological research, that we must attribute the continuous vilification of a valuable type of instrument? R. D. OLDHAM.

An Early Acoustical Analogue of Michelson's Echelon Grating.

IN the "Œuvres complètes." of Christiaan Huygens (tome x., p. 571) occurs the note given below. It was destined for Ph. de la Hire, and of date November, 1693. Huygens's remarkable observation and his ingenious explanation of the musical note produced by reflection from a large flight of steps of the noise of a fountain in the park of Chantilly will be read with interest also by those who, though having no ready access to the "Œuvres complètes," are still concerned with the (reflecting) echelon grating:—

"Je veux adjouter icy au sujet de la réflexion du son une observation assez singulière, que j'ay fait autrefois estant à la belle maison de Chantilly de la Cour où est la statue Equestre on descend avec un degré large de . . . marches dans le parterre ou il y a une fontaine de celles qu'on appelle gerbe d'eau, qui fait un bruit continuel. Quand on est descendu en bas et qu'on se tient entre le degré et la fontaine on entend du costé du degré une résonance qui a un certain ton de musique qui dure continuellement, tant que la gerbe jette de l'eau. On ne scavoit pas d'où venoit ce son ou en disoit des causes peu vraisemblables ce qui me donna envie d'en chercher une meilleure. Je trouvay bientost qu'il procédoit de la réflexion du bruit de la fontaine contre les pierres du degré. Car comme tout son, ou plustost bruit, réitéré par des intervalles égaux et très petits fait un son de musique, et que la longueur d'un tuyau d'orgue détermine le ton qu'il a par sa longueur par ce que les battements de l'air arrivent également dans les petits intervalles de temps que ses ondoiements emploient à faire deux fois la longueur du tuyau sçavoir quand il est fermé par le bout, ainsi je concevois que chaque bruit tant soit peu distingué qui venoit de la fontaine, estant réflechi contre les marches du degré, devoit arriver à l'oreille de chacune d'autant plus tard qu'elle estoit plus éloignée, et cela par des différences de temps justement égales à celuy que les ondoiements de l'air employent a aller et venir autant qu'estoit la largeur d'une marche. Ayant mesuré cette largeur qui estoit de 17 pouces, je fis un rouleau de papier qui avoit cette longueur, et je trouvai qu'il avoit le mesme ton qu'on entendoit au bas du degré.

"Je trouvay comme j'ay dit que la gerbe n'allant point l'on cessoit d'entendre ce ton. Et aiant eu occasion d'aller a Chantilly pendant l'hyver, qu'il estoit tombé beaucoup de niège qui ostoit la forme aux marches, je remarquay que on n'entendoit rien quoyque la gerbe allast et fit du bruit à l'ordinaire."

A slight confusion is caused by Huygens's first referring in his note (apparently only drafted) to a closed organ-pipe and later to an open one. Taking a pouce=2·7 cm., the depth of the steps becomes $17 \times 2{\cdot}7 = 45{\cdot}9$ cm. At 10° C., the corresponding sound of about 368 vibrations per second would be given by an open pipe of 46 cm.

The effect of gratings on impulsive motion of light is now well understood, thanks to the labours of Lord Rayleigh, Gouy, Schuster, and others. It remains interesting, however, to contrast the opinion concerning the supposed regularity of white light, held by some high authorities before these discussions, with Huygens's statement that the regularity in the nature of the sound which he observed has been impressed upon it by outside influence. P. ZEEMAN.

Amsterdam, January 6.

The Inheritance of "Acquired" Characters.

MAY I ask for information upon the interpretation of two sets of facts?

(1) Prof. Henslow states that the garden parsnip "known in the trade as 'The Student' was raised from seed of the wild plant by Prof. J. Buckman in 1847 at the Agricultural College, Cirencester," and also that M.