THURSDAY, DECEMBER 13, 1894.

DILETTANTISM AND STATISTICS.

The Growth of St. Louis Children. By William Townsend Porter. Vol. vi. No 12: Transactions of the Academy of Science of St. Louis.

THE anthropometrical researches of Mr. Francis Galton have had a very widespread influence, and, under his inspiration, a large mass of material has been, and is being collected, which cannot fail to be of service in elucidating a number of knotty problems. In particular, very comprehensive measurements of children have been made in America by Bowditch, Sargant, and Porter, the results being tabulated by aid of Mr. Galton's method of percentiles.

It is not only in the form of statistics of measurements on man, but also of measurements on the lower animals and on plants, that material for the study of variation and correlation is accumulating with almost alarming rapidity. We say with almost alarming rapidity, for not only is theory lagging somewhat behind the needs of statistical experience, but, what is far worse, many collectors have no real insight into the theory which does exist. They are tabulating results in a form which will be of no permanent service, or neglecting to publish the very details which alone would enable us to test any development of statistical theory. The ignorance of the elements of mathematics, to say nothing of the theory of chance, is, indeed, singularly characteristic of many investigators, who think statistics can be handled, and conclusions drawn from them, without the least preliminary training. For example, in a recent paper on variation in the flowers of buttercups and clover, by Herr de Vries, we are introduced to our old friend the normal curve of errors as a definite symmetrical polygon; no attempt is made to fit it by aid of the probable error of the observations, but a particular form of it, apparently selected at random, is plumped down on the top of another polygon representing the observations. The odds against the thus selected theoretical polygon (curiously called the "Galton Curve") expressing the real distribution of variation, are, it is needless to say, many thousands to one. Still more curiously, sets of observations giving theoretically very good frequency curves of the type which occurs in infantile mortality, the valuation of houses, &c., are termed "Half-Galton-Curves," although the type, so far from being represented by the half of the normal curve of errors, corresponds to a curve asymptotic to the ordinate of maximum frequency! We might pass these eccentricities by, were not statistics thus handled used to support some vague theory of heredity, or to question some principle of variation.

Mr. Porter is not quite so wild in theory as Herr de Vries, but for him also the normal curve of errors appears to be a polygon, and he tells us, on p. 277, after a table of its ordinates—which are quoted from Thoma, and given only to the units, and *not to decimals*—"that there is a limit beyond which no deviation occurs." It is, perhaps, needless to say that if we start only with a knowledge of the theory of chance, such as may be found in works like those of Thoma and Stieda, we are hardly likely to avoid bad theoretical blunders. Mr. Porter is no worse than many writers of memoirs in the *Journal of the Royal Statistical Society;* but the time has come when statistics, as well as archæology, must be taken out of the hands of the dilettanti, and when a man, if he takes upon himself to publish statistical researches, must, like the physicist, show his credentials in the form of a fair mathematical training.

Unfortunately, Mr. Galton's method of percentiles, useful as it is in the right place, is now acting as a distinct check to statistical theory in a somewhat unexpected manner. Before that theory was disseminated the dilettante statistician was compelled to publish his raw material, and his remarks on it did not impair its value to the trained scientist. Now, however, the percentile method of dealing with statistics seems so intelligible, that the non-mathematical anthropologist or physiologist grasps it at once, reserves his raw material, and publishes tables of percentiles. It is hard to conceive anything more disastrous for true statistical progress.

The reason for this is easily explained. As is well known, any normal curve of errors can be reduced to any other by uniform stretches (or squeezes) parallel to its base and its axis. The area of any portion of any normal curve up to a deviation of given magnitude cannot be given by a finite series; it can by stretching be reduced to an integral, the values of which are tabulated, but which is not itself finitely expressible for an indefinite value of the deviation. The tables formed of this integral, however, enable us to pass from selected percentiles to the constants, and so to the form of the frequency curve. Now, innumerable frequency curves in biological, just as in economical statistics, are *not* of the normal type. A generalised frequency curve can be theoretically obtained fitting closely the observations in many, perhaps in all such cases, but owing to its asymmetry or skewness, its limited range and other features, it neither possesses the stretch property of the normal curve, nor can its areas be expressed by tables of one entry. The tables required are, according to the type, of two or three entries, and such tables are not likely to be prepared for a long time to come, and if prepared, would be of little service except for the case when the statistics are given in percentiles. In other words, the use of percentiles may suffice to demonstrate the skewness of the statistics, but at once precludes the use of any theory higher than that of the normal curve, by which the skewness could be accounted for. This is the fundamental defect of Mr. Porter's labours. He gives us a most extensive system of measurement on boys and girls from six to twenty years of age—ample material, if properly dealt with, to provide solutions for innumerable problems in correlation and selection with age. The material, however, is only given in the form of percentiles or in diagrams of the "ogive" curve corresponding to the integral of the frequency curve. These diagrams fully confirm the results of Bowditch—*that the statistics are skew, and that the degree of skewness varies with the age.* But all means of scientifically treating this skewness disappear, because we have only percentile results. The skewness of the original frequency curve cannot be deduced from nine or ten points on a curve, the equation of which is only expressible by an unintegratable form containing the constants to be determined. This is all the more tantalising, as the variation with age (or with selection) of the skewness of the frequency curve for a given organ is very probably a most important factor in the mathematical treatment of evolution, while it is precisely *human* material which enables us to record the age, too often an unknown quantity in investigations on the lower animals. It must not be supposed, however, that Mr. Porter realises that skewness in the percentiles marks want of normality in the frequency curve. On the contrary, he takes the raw material for the height of 2192 girls aged eight, and fits it, not very accurately, to a normal frequency distribution. Although the measurements better fit a skew than a normal distribution, the divergence from the normal is not very marked, and Mr. Porter writes:

> "It is not necessary to make this comparison at more than one age, or in more than one dimension, for it is known that if one series in a group like that with which we have to deal shows this agreement, the other series will be found to do the same." (p. 290.)

Now the whole point of the measurements, if properly interpreted, is to ascertain the varying degree of skewness with age, *i.e.* the varying amount of divergence from the normal curve. To emphasise this divergence when remarking on his raw material, but to fail to recognise what bearing it has on theory, is typical of the untrained statistician.

As we have seen, the use of percentiles renders Mr. Porter's material of small value for the problem of selection. The same remark again applies in the case of correlation. The ten organs measured would have been most valuable material for the problem of correlation; the percentile results alone being given, nothing can be done. In Chapter ix. Mr. Porter does develop a theory of correlation, which, however, seems to us absolutely erroneous. As he makes it the basis of a scheme for the school physician to testify whether the pupil's strength is equal to the strain put upon it, this want of accurate theoretical knowledge appears doubly unfortunate.

We have ventured to point out these failings in Mr. Porter's work, not because they are peculiar to him—they are characteristic of much work of a statistical kind which is now being turned out in both Europe and America—but rather because they point to a new need, which the public has hardly yet recognised. There has been up to the present time—with the honourable exception of courses of lectures by Dr. Venn in Cambridge—no teaching of statistical theory in England. There is no chair of statistics in any English university, and the Newmarch lectureship at University College, except for the year of its tenancy by Prof. Edgeworth, has been held by economists, and not by mathematically trained statisticians. We want a centre, which shall not only contain a statistical museum, but embrace as well a statistical laboratory and workshop. In such a centre students might not only receive a mathematical training in dealing with raw statistics, but also be exercised in the methods of collecting and tabulating, which must precede mathematical reduction. To such a centre the biologist and anthropometrist could send their measurements to be "fitted," the physicist his observations to be dealt with, and the economist or sociologist his price or labour statistics to be analysed. At the same time, absolute measurements might be made bearing on the problems of evolution, disease and national economy. In this manner a number of efficient young statisticians might be trained, who would not only find a life-work ready for them in craniology, zoology, botany, and economics, but who, passing into government departments, census offices and labour bureaus, might remove from us the reproach of a recent continental writer, that nowhere were statistical dilettanti so rampant as in England.

KARL PEARSON.

WATER SUPPLY AND WATER-WORKS.

The Principles of Water-works Engineering. By J. H. Tudsbery Turner and A. W. Brightmore. (London: Spon, 1893.)

The Water Supply of Towns. By W. K. Burton. (London: Crosby Lockwood and Son, 1894.)

WITH the constant growth of population, and the increasing tendency of the population to congregate into large towns, the provision of an ample supply of pure water to towns becomes every year of greater importance; whilst, as the nearer sources of supply become inadequate for the ever-growing demands, a sufficient supply becomes more difficult to obtain, and has to be sought at considerable distances. Accordingly, the subject of water-supply has assumed an enhanced importance within recent years; and till these books appeared, there was a dearth of comprehensive text-books on this branch of engineering. As the sources of water-supply, and the works for its provision and distribution are comprised within very definite limits, both books necessarily traverse much the same ground, and deal with similar classes of works; but, nevertheless, they exhibit differences in arrangement, and in the method of treating the various subjects. Thus, whereas "Water-works Engineering" extends over 420 pages, separated into only eight chapters, and illustrated by one hundred and twenty figures in the text, "The Water Supply of Towns" is subdivided into twenty-two chapters, occupying only 272 pages, somewhat larger in size, but illustrated by two hundred and fifty-seven drawings, several of which are put into forty-three large folding-plates. The first book, moreover, is subdivided into numbered sections, the numbers of which are merely used for reference, and instead of the pages in the index; whilst the various subjects in the second book are clearly indicated by black-letter headings. Altogether, "The Water Supply of Towns" is much better arranged for reference; and though containing slightly less matter, it is much more fully illustrated, is in larger print, and has a somewhat longer index than "Water-works Engineering." Neither book gives a summary of contents at the head of each chapter, which is often serviceable for reference.

In "Water-works Engineering," the sources, measurement, collection, storage, purification, conveyance, and distribution of water, and the maintenance of water-works, are successively dealt with in the eight chapters. The most logical sequence would be to describe in regular order the various processes to which water is subjected, from its source up to its delivery to the consumer.

This has only been partially accomplished in "Water-works Engineering," for, owing to the small number of chapters, such dissimilar subjects as the available rainfall and the gauging of the flow of streams have been put in the same chapter, near the beginning of the book, with water-meters, which latter subject should properly have been dealt with at the end, in connection with consumption and waste. Moreover, impounding reservoirs and dams are grouped with service reservoirs, tanks, and house cisterns; whilst the filtration, purification, and softening of water, are considered before the preliminary process of conveyance from the source of supply by aqueducts and conduits. Though the grouping of dissimilar subjects has been avoided in "The Water-Supply of Towns" by the multiplication of chapters, the natural sequence of processes has not been always maintained; for the chapters on pumping machinery, and the flow of water in conduits and open channels, follow after the chapters on purification; whereas the flow of water is intimately connected with the conveyance of the supply from impounding reservoirs; and purification and softening are more needed for waters pumped out of rivers, or from wells, than for waters impounded in a reservoir in a mountainous uninhabited district. In this book, however, the descriptions of water-meters are given in the chapter on the prevention of waste of water, to which subject they properly belong; and cisterns are referred to in this connection, as well as in relation to distribution.

The most notable difference between these two books consists in the use made of mathematical calculations and formulæ. Prof. Burton rarely introduces any formulæ, contenting himself, in the case of high masonry dams, with diagrams of the theoretical profiles proposed by Prof. Rankine and Mr. Wergmann, in addition to the actual sections of the Vyrnwy dam in Wales, and the Tytam dam at Hong Kong; whilst he refers his readers to Fanning's "Treatise on Hydraulics and Water-works Engineering" for formulæ relating to the flow of water. A less cursory treatment of these important subjects, in relation to water-supply, might reasonably be expected in a book which is stated, on its title-page, to be "a practical treatise for the use of engineers and students of engineering"; but perhaps Prof. Burton exercised a wise discretion in this matter, for in the calculation of the cross-section of a stream, on page 28, he falls into an obvious error in including the depths at both extremities to obtain the average depth. The authors of "Water-works Engineering," on the contrary, incline towards the other extreme, and devote more attention to mathematical solutions than to practical considerations. Some amount of mathematical treatment is unquestionably expedient in dealing with the flow of water and the design of masonry dams; and formulæ based upon experience, together with diagrams, furnish almost indispensable aids in the consideration of these matters, and for their practical application. It is, however, an undue extension of the province of mathematics, in such a treatise, to use them to prove that impalpably fine matter in suspension in water would never fully settle (pp. 266–7), which is a subject for physical experiment; and the result would vary with the nature of the material and its specific gravity. Moreover, considering the uncertainties that exist as to the precise distribution of the pressures over the masonry of a reservoir dam, it appears somewhat superfluous to devote over two pages in calculations proving that wind-pressure against the outer face of a masonry dam has only a very slight influence in modifying the position of the line of resultant pressures, with the reservoir empty, under the exceptional conditions of a high wind blowing up the valley, and the reservoir being empty. The somewhat free use of mathematical processes, and the occasional introduction of the integral calculus, will unfortunately be liable to deter persons, who have not received a regular mathematical training, from consulting "Water-works Engineering"; though the greater part of the book deals clearly and concisely with the practical matters and works relating to this branch of engineering. More frequent references to executed works, by way of illustration, would have added interest to the book, and would have served to exhibit the methods of application of general principles, and their results; but the authors explain in their preface, that they considered descriptions of works outside the scope of their book. Owing doubtless to their having lived in cities supplied from wells and impounding reservoirs, they condemn the quality of river waters in stronger terms than the inhabitants of London, deriving a large proportion of their supply from the Thames, would be prepared to admit as correct. They say: "The quality of river-water is seldom unexceptionable, and is frequently bad. . . . Rivers are generally charged with impurities of vegetable, animal, and mineral origin, to such an extent as to render them frequently a most desirable source of water for irrigation purposes, but not for domestic supply." After this it is only a slight consolation to be told that, "still, by certain processes, even the most unpromising river-water is often rendered generally serviceable," especially as, shortly after, they quote the statement of the Rivers Pollution Commission of 1868, that "there is no river in the United Kingdom long enough to effect the destruction by oxidation of sewage put into it at its source," and add, in conclusion, that "the result of the aëration produced by even the Falls of Niagara is so insignificant as to be insensible so far as regards any purifying effect upon the water."

As the author of "The Water Supply of Towns" is Professor of Sanitary Engineering in the University of Tokyo, he has naturally inserted some matters relating more particularly to Japan, such as, for instance, certain special provisions for the extinction of fires which are very common in Japan, owing to the light construction of the houses, and more especially the probable effects of earthquakes on water-works, and the precautions to be taken against them. Most, however, of these special matters have been relegated to foot-notes and an appendix; whilst some particulars of general interest relating to the Tokyo water-works are given in the text.

Both books contain a fairly complete exposition of the principles relating to the water supply of towns, and a general description of the construction of water-works; and they should both prove valuable to the engineer and the student, in relation to this special branch of engineering. Whilst, however, the engineering student, who has had the advantage of a mathematical and scientific training, will doubtless prefer the more scientific metho exhibited in "The Principles of Water-works Engineering," the practical engineer will obtain fuller information, with less trouble, from the more clearly arranged and better illustrated pages of "The Water Supply of Towns."

HAMILTON'S PATHOLOGY.

A Text-book of Pathology, Systematic and Practical. By D. J. Hamilton, M.B., F.R.C.S.E., F.R.S.E., Professor of Pathology, University of Aberdeen. Vol. ii. Parts 1 and 2. (London: Macmillan and Co., 1894.)

THESE two handsome volumes complete the laborious task which Prof. Hamilton set himself many years ago, of producing a text-book of pathology, which should include not merely morbid anatomy and histology, but should deal also with clinical medicine, and those new but rapidly developing sciences, pathological chemistry, pathological physiology, and bacteriology.

It is true that the many varied problems which come before a pathologist require for their solution a more or less extensive acquaintance with each and all of these branches. So many, however, are the workers, and so vast has the literature become in each branch, that it is already impossible for anyone to be equally proficient in all, and it is necessary to specialise.

If this is the case with the individual, it is much more so with the text-book. It is impossible to compress within even 1800 pages, a tithe of our present knowledge of all these branches, and opinions will differ widely as to how the selection should be made.

The task of making such a selection is herculean enough to daunt any man, and the thanks of all interested in pathology are due to the Professor for the very valuable addition he has made to our literature on the subject. The amount of original work is considerable, and the volumes are copiously illustrated by drawings, the majority of which are by the author.

An extremely useful feature in the book consists in the bibliography of both the English and foreign literature at the end of each section. The convenient system is also adopted of indicating the title of any publication referred to in the text by a number in Roman letters, while the full titles and the corresponding numbers are given in a list at the end of the book. The first volume, which was reviewed in these columns in 1889, met with general appreciation. This volume commenced with full directions for making systematic post-mortem inspections, and for the practical bacteriology and histology of the tissues removed. Nearly 200 pages were devoted to a full account of inflammation and suppuration, and of tumours and new formations, while the remainder of the book was occupied by an account of the heart, the blood, and the vascular system generally.

The two parts of the second volume now issued are bound up separately. The first part of the present volume is devoted to a systematic account of the pathology of the respiratory organs, the liver and the kidney; a good solid piece of work containing much original research, in which are embodied the well-known papers of the author on diseases of the lungs, and on cirrhosis of the liver. Among the many observations, we would draw attention to the important conclusion that in nephritis the fatty granular remains of the convoluted epithelium are absorbed by the lymphatic system, and are not, as is generally supposed, mainly washed out by the urine.

The drawings, illustrating the author's views, are worthy of all praise; they are well executed, and are sufficiently diagrammatic to impress the conclusion to be indicated, while their reproduction also reflects great credit on the publishers.

The remainder of the volume deals with the diseases of the digestive, the nervous, the osseous and the cutaneous systems, finishing with a short statement of the various malformations, and a good summary of systematic bacteriology and of human parasites.

The present volume is overweighted by 150 pages of normal anatomy and physiology, which preface the accounts of the various organs, nearly fifty pages and twenty illustrations being devoted to the nervous system. This material is surely out of place in a book, in which the author has been compelled by the exigencies of space and time to reduce his account of the pathology of many conditions to too brief an account to be of value, so that often there is no indication as to which lesions and causes are common and important to remember, and which are exceptional; frequently a mere list of them is given.

It is also to be regretted that it has been thought desirable to devote valuable space to the description of the various tests for sugar and albumen in the urine, as this information is contained in the ordinary standard books on clinical medicine.

The author apologises for the lapse of five years since the appearance of the first volume; and we cannot help regretting that Prof. Hamilton did not publish only the first part of the present volume, and allow himself another year or more in which to have worked up the second part to the high standard reached in the earlier portion of the book. Although five years may appear a long time, for the production of the second volume of this book, still the amount of labour required was so extreme that more time was really necessary. The lack of space also has necessitated so much compression, that often the pathology is reduced to a mere explanation of the terms used; while the numerous omissions of important facts suggest that this part has been produced at very high pressure, so that the facts are not presented in their due relation to one another, while there are several statements to which we would take exception.

While such common causes of optic neuritis as plumbism and anæmia are omitted, excessively rare ones, such as pneumonia and measles, are given; again, while optic neuritis is discussed, optic atrophy is omitted. Among the causes of cerebral hæmorrhage, such well-recognised ones as glioma and aneurysm are omitted.

Through oversight the following conflicting statements have been allowed to appear. Under cysts of the liver, the association of such with similar ones in the kidney is said to be fortuitous; while under the head of renal diseases, it is noted that out of sixty-two cases of cystic kidneys, seventeen were associated with similar cysts in the liver. On one page, in discussing the growth of typhoid bacilli, great doubt is thrown on whether they spore; on the next page, it is stated that they invariably spore when grown under suitable conditions.

The excellence of the work, and it is great, lies in the histological and bacteriological portions, and in the numerous and ingenious theories and suggestions; but the necessity of finishing the book soon, and of keeping it within reasonable size, appear to be responsible for the less satisfactory accounts of the morbid anatomy and of the etiology of the lesions in certain of the sections. This, however, will probably be remedied in later editions.

OUR BOOK SHELF.

The Mechanism of Weaving. By T. W. Fox. Pp. 464. (London: Macmillan and Co., 1894.)

ONE result of the application of the "beer money" to education is the increasing production of technical hand-books similar to that before us. Technical Instruction Committees found, very soon after their responsibilities were thrust upon them, that there were few competent teachers of technology, and that the literature of arts and crafts was very limited. Many books have been made for the purpose of supplying the need, some good and others of doubtful utility. The new conditions have been favourable to the development of technological books and teachers, and we must not complain if a few monstrosities occur in the case of each, for they are more than counter-balanced by many admirable examples on the other side. The book under review is one of these new guides to industries, and nothing but good can be said of it. It deals with a branch of weaving that has been almost ignored by previous writers. Much has been written on designing and fabric structure, but practically nothing on the mechanical processes of the weaving trade, though new machinery and new processes have been increasing ever since the power-loom supplanted the hand-loom. This gap is admirably filled by Mr. Fox's treatise. The leading types of weaving machinery are clearly described, and the numerous illustrations (256 in all) of machines, appliances, and constructions pertaining to the textile industry are most instructive. We have no hesitation in saying that Mr. Fox, who is the lecturer on textile fabrics at the excellent Municipal School at Manchester, has produced a practical handbook of great value.

Memorials of Old Whitby. By Rev. J. C. Atkinson, D.C.L. Pp. 326. (London: Macmillan and Co., 1894.)

FICTION, "like the baseless fabric of a vision, leaves not a wrack behind," because it has no foundation in fact. Stories that are commonly classified as "fabulous" usually have, however, a nucleus of scientific import. In the words of Canon Atkinson: "The myth, the fable (of the mystic sort), the legend, has always a base, a substratum or foundation of some sort. Like the Pentacle, with its mystic application and use, or the Svastika, Fylfot, or Hammer of Thor, the Monolith or Standing-stone—from Jacob's stone at Bethel, and before and since—it has always had its own something to rest upon, to spring from its actual material 'base' or occasion." Scientific inquiry is required to reveal this base; but by this we do not mean that facts of physical or of natural science are necessarily involved in every marvellous story, but rather that the investigator of legendary lore should conduct his research in a scientific spirit, discarding speculative evidence, and reducing the problem to its simplest appearance. This is the spirit in which Canon Atkinson has investigated the myths, legends, and traditions connected with Whitby. His treatment of the Caedmon legend is worthy of special mention. It will be remembered that Caedmon produced his great sacred poem at Whitby. According to Baeda, he was a common cowherd or oxherd, to whom the gift of poesy was miraculously, or at least suddenly, given, and this story has been generally accepted in spite of its great improbability. Canon Atkinson rids the story of its miraculous element, and justifies the dictum *poeta nascitur, non fit.* He shows that it is largely mythical, and that Caedmon was probably a homely native poet of some genius, but undeveloped, before the Abbess Hild took him up. This view is practically clinched by the evidence that Caedmon's name is of Celtic origin, and that therefore he doubtless possessed the fervid imagination and vivid fancy of the Celtic temperament. It need hardly be said that the miracle described by Baeda would have been eliminated from the story at once by a man of science. To us it seems that Canon Atkinson comes to the only conclusion possible after a careful consideration of historical records, and a common-sense view of the case. Other stories and customs connected with Whitby are discussed with a similar broad-mindedness, and in a manner which local historians generally would do well to follow.

Die Schöpfung der Tierwelt. Von Dr. Wilhelm Haacke. Pp. 552. (Leipzig and Vienna: Bibliographisches Institut, 1893.)

IT is just as well to state at once that this is a scientifically-arranged description of the animal kingdom, profusely and beautifully illustrated with twenty coloured plates and 469 figures in the text. The illustrations are certainly among the finest of their class, and one cannot help regretting that, as the text is in German, the work can only have a limited sale in England. We can console ourselves, however, with Mr. Lydekker's "Royal Natural History," now being published, and which is rather more popular than the volume under review. The order in which Dr. Haacke treats his subject is uncommon. The book is divided into two parts, one dealing with the various forms of animal life from the point of view of their development, while in the second part the characteristics of different groups of animals are described. The first part is thus chiefly concerned with embryology and palæontology in their relations to zoological affinities; with the functions of organs and the influence of environment; and with the distribution of animal life upon the earth. In the second part, invertebrated and vertebrated animals occupy two separate sections, and life is traced from the protozoa up to the higher forms. From this brief sketch it will be seen that the book has the theory of evolution as the basis of its construction. It is therefore a work in which the facts of natural science are presented in scientific order, and as such deserves high commendation.

The Vaccination Question. By Arthur Wollaston Hutton, M.A. Pp. 128. (London: Methuen and Co., 1894.)

MR. HUTTON is among the mistaken people who distrust vaccination, and advocate the repeal of the compulsory laws relating to it. His book is in the form of a letter addressed to Mr. Asquith, in the hope of converting him to the opinion that compulsory vaccination is indefensible. It would be futile for us to discuss the subject, or to attempt to show the fallacy of much of the evidence adduced against vaccination. We may, however, point out that, to be consistent, the anti-vaccinationists must oppose the treatment of diphtheria by anti-toxic serumt; but this they are doubtless ready to do.

Dr. William Smellie and his Contemporaries. By John Glaister, M.D. Pp. 360. (Glasgow: James Maclehose and Sons, 1894.)

DR. GLAISTER'S book throws some new light upon the history of obstetrics in Great Britain and France, during the eighteenth century, in addition to tracing the career of one of the founders of scientific midwifery.

Smellie's work, however, was so purely medical in character, that a review of it would be of little interest to most of the readers of NATURE.

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.*]

Dr. Watt's Dictionary of the Economic Products of India.

THE notice of this important undertaking in a recent number of NATURE (November 1, p. 4), seems to me somewhat unsympathetic, and scarcely to do justice to its undoubted merits. At any rate, the Government of India, at whose instance the Dictionary was prepared, might draw the conclusion from the review that the work was more open to criticism than I believe to be really the case. A British Government is never too ready to undertake an enterprise of this kind, and anything of the nature of a disappointment, when it has made the experiment, is little likely to induce it to make another.

As I warmly encouraged the inception, and have taken a keen interest in the progress of the Dictionary, I feel bound to express my opinion that it is one of the most important aids which has yet been given to the material advancement of India.

As the reviewer correctly remarks, "the large proportion of the products" of that country "are of vegetable origin." It is, however, astonishing how little they are known in Europe. I can speak with some confidence on this subject, because, in 1880, the India Office transferred to Kew the entire economico-botanical collections forming part of the India Museum at South Kensington. Their incorporation with the existing contents of the Kew Museum was carried out under my supervision. I was struck, as every one has been who has had to do with the subject, with the profusion of products for which some useful purpose ought to be found in the arts. As Kew undertook the duty of acting as referee with regard to these matters, it became necessary to accumulate information with regard to them. I therefore formed in my office a sort of rough dictionary, in which I posted up every paper, document, and scrap of information which I could collect about Indian vegetable products. This enabled me to deal rapidly with a great number of commercial inquiries. But for this purpose, Dr. Watt's Dictionary is an infinitely superior instrument. It is in constant use in my office, and I am at a loss to conceive how the day's work could now be got through without it. As I am continually testing its contents, I can only express my surprise at the degree of accuracy which Dr. Watt has attained. I am quite satisfied that the catalogue of not very important blemishes which the reviewer has managed to detect, must have cost him no small labour. The criticisms, with one exception, I do not propose to discuss. I cannot help, however, regretting that the reviewer both begins and ends his article with something like a sneer—in the one case at the size of the book, in the other at the paper on which it is printed.

Such an encyclopædia of economic products as the Dictionary affords, has long been needed. Indirectly I regard it as one of the outcomes of the Famine Commission, the second part of whose Report was presented to Parliament in 1880. In this Report (p. 175) the Commission point out that "at the root of much of the poverty of the people of India . . . lies the unfortunate circumstance that agriculture forms almost the sole occupation of the mass of the population." "Facilities for obtaining profitable markets for all sorts of produce" is a necessary means of remedying this state of things. But it is obvious that the markets will not come into existence till the products are brought into demand by better knowledge.

The bulk of the book and the length of the articles was, I imagine, the result of a deliberate plan on the part of the Department of Revenue and Agriculture under whose authority the Dictionary was issued. One object in view was to afford to Indian officials, who cannot be expected to take a miscellaneous library about with them, a standard book of reference. The want of something of the kind led Mr. Edwin T. Atkinson, of the Bengal Civil Service, to commence a sort of industrial survey of the north-western provinces. The work is only a fragment, as Dr. Watt's Dictionary has since covered the ground. The preface to the first part (Allahabad, 1876) puts the needs of Indian officials so clearly, that it will be useful to quote from it:

"There is no subject perhaps regarding which so much has been written and spoken as 'the development of the resources of the country.' The phrase is a pretentious one, and has a rounded, rolling sound worthy of many of the ideas launched in its name, but really expresses what must form one of the first duties of every civilised Government. As early as June 1804 a Mr. Gott was deputed to examine the forests of Rohilkhand and Gorakhpur, with the view of ascertaining what local products could be advantageously brought to the notice of the European world. . . . He was followed by Laidlay and others, who examined into many of the questions which even now are made the subjects of inquiry. Their reports lie unused and unknown amid the twelve hundred volumes of records composing a portion of the immense library of our Board of Revenue, and surely it would be a saving of money and labour if these reports were given to the official and non-official public. My own inquiries have shown me that it is the tendency of all these questions to crop up in cycles, with the same result, the same perfunctory procedure; and were official memory long enough to recollect what has already been done, and know where to find it, one half of the erratic circulars which now puzzle and worry the district officers, might be answered by a reference to the correspondence on the same subject. . . . A review of the efforts made in the past to attain a knowledge of the resources of the country is not unmixed with regret. They have all gone by without leaving any trace behind them, and without advancing our knowledge one single step, because they wanted organisation."

In criticising Dr. Watt's Dictionary, a statement like this should be borne in mind. Dr. Watt has, in fact, and it is one of the great obligations we owe to him, gutted and boiled down the colossal and inaccessible official literature of India. For the first time, an Indian official has at hand all that is practically to be said about any local product with which he has to deal.

These points were, I think, not clearly brought out in the review in NATURE, and I think it is only due to the Government of India that they should be stated.

There is one detailed criticism on which I absolutely take issue with the reviewer. He devotes some space to the expression of the opinion that with regard to "drugs and therapeutics, the details are out of place in a description of economic products." He particularly objects to the inclusion of plants, "solely because of some medicinal, or supposed medicinal, use, frequently by ignorant people." To me the subject seems, on the contrary, of the deepest importance. It is the empirical knowledge acquired by "ignorant people" which is at the base of almost all our knowledge of drugs. And the extension of modern therapeutics must in the main depend on following up the beginnings in new directions which ignorant people have made. In this respect the Dictionary is a mine of information available for the guidance of future research. In India itself a trustworthy account of the properties of the plants used by the natives for therapeutic or even criminal purposes, must, it seems to me, be of every-day utility.

W. T. THISELTON-DYER.

Royal Gardens, Kew, December 8.

IN your recent review of the above work, attention is drawn to the startling statement that "Diamond dust is known to be a powerful mechanical poison." This statement, occurring as it does in an official work, issued by the Government of India, aroused my amazement when I first had occasion to consult the Dictionary. It occurred to me then, and I still think, that the author would have done better to have quoted the words of Colonel Wilks on the subject ("South of India," vol. ii. p. 197), namely, "Whatever doubts may be entertained of the fact, there is none regarding the belief [by the Mahommedans of Southern India in the power of diamonds as a poison], and the supposed powder of diamonds is kept as a last resource like the sword of the Roman, but I never met with any person, who from his own knowledge could describe its visible effects, &c."

Better still perhaps, instead of giving what, as you have pointed out, is an erroneous account of the celebrated Gaikwar's case, Dr. Watt might have quoted the emphatic words of the Commissioners who tried the Gaikwar, that "Diamond dust according to the best authorities has no injurious effect on the human body" ("Commission Report," p 223).

With some inconsistency—although Dr. Watt quotes an account of the reputed medicinal qualities of prepared diamond

—he does not follow up the first statement by including it in his list of reputed poisons (vol. vi. p. 309.) It may be added that Dr. Watt's selection of authorities, generally, appears to be somewhat capricious. He does not appear to be acquainted with Garcia de Orta's famous work on Indian drugs, for he gives Linschoten and others credit for observations not originally made by them, but by Garcia, *e.g.* art. *Manna*.

Again, when writing of ambergris, surely he might have found some more direct source of information regarding a product derived from *whales*, than a work, excellent though it be, which deals properly with the products of the Punjab.

I write as one not wishing to find fault, especially as I recognise the good services done by Dr. Watt, but because I believe such a work so brought out, should be a faithful summary of recorded facts, which, if hitherto only known to comparatively few, should be so stated as not to mislead the many who may have occasion to refer to the Dictionary. V. BALL.

Science and Art Museum, Dublin, November 19.

Drift-Bottles in the Irish Sea.

IN NATURE for Nov. 8 (p. 35) mention is made of the travels of some drift-bottles in the south seas. It may be of interest to put on record the results so far obtained of the distribution of bottles set free by the Liverpool Marine Biology Committee in order to get further information in regard to those currents in the Irish Sea which would affect small floating bodies. The objects we have had in view are: (1) A purely scientific matter, the source and distribution of the plankton; and (2) the probably utilitarian object of determining the movements of the food of fishes, and so one of the causes of their migrations, and also the drift of the floating ova and embryos of food fishes. The tidal currents of the area in question are to a considerable extent known, and marked on the charts and given in books on "Sailing Directions"; but to these currents have to be added the modifying influence of prevalent winds, and what we want to get at is the resulting average effect. We want to know in what direction an object set floating at any spot will probably be carried at various times of the year in ordinary weather. The surface organisms are such feeble swimmers, if locomotory at all, that any results obtained from small floating bottles may reasonably enough be regarded as holding good for the plankton.

The form of bottle we have chosen is cheap, buoyant, strong, and well corked. It measures 7·5 cm. in length over all, and 1·8 cm. in diameter. Inside it is placed a printed paper requesting the finder to fill in date and locality, in spaces left for the purpose, and post it back to myself. The papers are numbered, and are folded in the bottle in such a way that the distinguishing numbers can be read through the glass, so as to ensure that the bottles are set free in consecutive order. After the bottle has been corked up, the end is immersed a couple of times in melted paraffin, so as to close up the pores in the cork. None of the papers that have been returned show signs of water having got into the bottles.

As to the distribution, I sent off the first few dozen myself from steamers crossing between Liverpool and the Isle of Man, dropping a bottle over every fifteen or twenty minutes between the Bar and Douglas, and also from a steam-trawler while dredging between Port Erin and Ireland. Mr. A. Holt has had a number of bottles distributed for me from his outward-bound steamers on their course between Liverpool and St. George's Channel, and from the Mull of Galloway round to the Mersey. The Lancashire Sea-Fisheries steamer has set free another series along various lines up and down the Lancashire coast, and finally some have been set free at equal intervals of time during the rise and fall of the tide from the Morecambe Bay Light vessel in the northern part of the district, and from the Liverpool North-west Light vessel in the southern part. The distribution has now been going on since the beginning of October, and a very fair proportion (about one out of every three) of the papers have already been returned to me duly filled in and signed. They have come from various parts of the coast of the Irish Sea—Scotland, England, Wales, Isle of Man, and Ireland. Some of the bottles have gone quite a short distance, having evidently been taken straight ashore by the rising tide. Others have been carried an unexpected length—*e.g.* one (No. 35) set free near the Crosby Light vessel off Liverpool at 12.30 p.m. on October 1, was picked up at Saltcoats in Ayrshire on November 7, having travelled a distance of at least 180 miles (probably far more) in 37 days or less; another (H 20), set free near the Skerries, Anglesey, on October 6, was picked up at Ardrossan, Ayrshire, on November 7, having gone at least 150 miles in 31 days.

It would be premature as yet, until many more dozens or hundreds have been distributed and returned, to draw any very definite conclusions. It is only by the evidence of large numbers that the vitiating effect of exceptional circumstances, such as an unusual gale, can be eliminated. However, I may state, as provisional results so far, that nearly fifty per cent. of the bottles found have been carried across to Ireland, and they are chiefly ones that had been set free in the southern part of the district (between Liverpool and Holyhead) and off the Isle of Man. The bottles set free along the Lancashire coast and in Morecambe Bay seem chiefly to have been carried to the south and west—to about Point of Ayre, in North Wales, and Douglas, in Isle of Man. It is apparently only a few that have been carried out of the district through the North Channel. The most interesting point, so far, is that so many of the bottles have been stranded on the Irish coast, although they were sent off for the most part much nearer to the English and Welsh coasts, showing no doubt the influence of the spell of easterly winds in October. W. A. HERDMAN.

University College, Liverpool, November 29.

The Explosion of Gases in Glass Vessels.

WHEN Prof. Lothar Meyer was visiting Manchester a few years ago (on the occasion of the meeting of the British Association), he surprised me by saying that it was his custom in lecture to explode mixtures of ethylene and other hydrocarbons with oxygen in glass cylinders, some 10 to 12 inches long by 1½ to 2 inches in diameter (if I remember rightly), and that he had never had an accident. I suppose I did not sufficiently conceal my surprise, for he immediately demanded that we should go to the laboratory and repeat the experiment. Not having a mixture of ethylene and oxygen ready, I could not accept the challenge on the spot. The issue was therefore changed. Prof. Lothar Meyer said that he would fire a mixture of hydrogen and oxygen in a thin glass test-tube without breaking it. I confess I was sceptical, until I saw him do it time after time without injury. He argued that if the thin test-tube would withstand the explosion of hydrogen and oxygen, a thick glass cylinder would withstand the more violent explosion of a hydrocarbon. Nevertheless I ventured to warn him against trying the experiment with either acetylene or with cyanogen, the two gases I had found to explode more violently than any others, *especially with a small quantity of oxygen*. Prof. Meyer's recent accident with acetylene and oxygen has led him to warn chemists against the danger of that mixture. I would wish to add to that warning that the danger is equally great, if not greater, with a mixture of cyanogen and oxygen.

Prof. Lothar Meyer asks how we can account for the violence of the explosion of acetylene, when the velocity of its explosion is so little greater than the velocity of explosion of marsh gas and of ethylene, while it is far less than that of hydrogen? It is important to bear in mind that the explosion-wave is not set up at once; when a gaseous mixture is ignited at the open end of a tube the flame starts comparatively slowly. The violence of an explosion in a short tube depends mainly on whether the explosion-wave is set up or not. I think the immunity so long enjoyed by Prof. Meyer's cylinders depends on the fact that the wave was not set up. I have found that pieces of strong combustion tubing, which will stand an hydraulic pressure of twenty-five atmospheres, are broken by the explosion-wave of hydrogen and oxygen. It requires exceptionally strong glass tubes, capable of bearing at least 120 atmospheres, to withstand the shock of the explosion-wave with cyanogen or acetylene. With both these gases it is the incomplete combustion which occurs with the greatest rapidity and violence. According to the hypothesis I have published, viz. that the explosion-wave travels with the velocity of sound in the burning gases, the pressures existing in the explosion-wave of cyanogen and acetylene with their own volume of oxygen are 117 and 105 atmospheres respectively. Quite apart from this hypothesis, the pressures may be calculated from Riemann's equation for the propagation of a wave of constant type, since we know the density of the unburnt gases and the rate of propagation of the wave. According to Riemann's equation the pressure in the cyanogen explosion is 140 atmospheres, and in the acetylene explosion 114. Some experiments I have recently made in conjunction with Mr. J. C. Cain confirm these calculated pressures. When the explosion-wave was propagated through a mixture of equal volumes of cyanogen and oxygen it broke soda-lime tubing of 18 m.m. external diameter and 2·5 m.m. thickness. Pieces of this tubing broke at a mean hydraulic pressure of 70 atmospheres. Green glass tubing of 2·8 mm. thickness withstood the explosion; it broke at a pressure of 140 atmospheres. More exact results were obtained when the gases were diluted with an equal volume of nitrogen:—$C_2N_2 + O_2 + 2N_2 = 2CO + 3N_2$.

Pieces of the tube which were broken by the explosion were broken hydraulically at 63 atmospheres; pieces of the tube which withstood the explosion were broken hydraulically at 84 atmospheres.

Pressures in the Explosion Wave.

Gaseous mixture.	Calculated pressures.		Observed pressures.
	Riemann,	Dixon.	
$C_2N_2 + O_2$	140 At.	117 At.	70 – 140
$C_2N_2 + O_2 + 2N_2$	73·5 At.	57 At.	63 – 84

When oxygen is added to these mixtures the rate of explosion is diminished and the pressure falls. For instance, according to Riemann's equation, the pressures produced in the explosion of acetylene with increasing quantities of oxygen are as follow:—

Gaseous mixture.	Calculated pressure.
$C_2H_2 + O_2$	114 At.
$C_2H_2 + O_3$	98 At.
$C_2H_2 + O_5$	78 At.

In the same way the pressures produced in the explosion of ethylene with different quantities of oxygen may be calculated:—

Gaseous mixture.	Calculated pressure.
$C_2H_4 + O_3$	98 At.
$C_2H_4 + O_4$	91 At.
$C_2H_4 + O_6$	78 At.

The lowest of these pressures is probably sufficient to break the cylinders used by Prof. Lothar Meyer. As Prof. Thorpe says in NATURE, the danger of acetylene lies in the rapidity with which the explosion-wave is initiated, even when the air alone is used as "tamping." Safety lies not in thickening the glass, but in shortening the tubes. H. B. DIXON.

Owens College, December 1.

The Kinetic Theory of Gases.

I HAVE to thank Mr. Culverwell for his reply to my letter on the discussion at Oxford. To quote his own words (in answering Mr. Burbury, p. 105), Mr. Culverwell's letter was "exactly the kind of letter that I hoped to elicit," as I had not been able to recall the exact purport of Prof. Fitzgerald's "onslaught." Although Prof. Boltzmann made no attempt to answer Prof. Fitzgerald's objections in the short space of time available after the other speakers had concluded, he several times mentioned the question to me after the debate as one which had not been hitherto satisfactorily cleared up. In preparing my Report, the question of the spectra of gases came prominently before me, but I purposely refrained from expressing my own opinions on a subject about which so little had been written in a report which was intended to be chiefly a record of work actually done. My frequent allusions to the question of the *uniqueness* of the Boltzmann-Maxwell Law were intended, however, to pave the way, if possible, for an explanation of the discrepancies alluded to by Prof. Fitzgerald, and I should like now to attempt to answer some of his objections.

According to Mr. Culverwell, Prof. Fitzgerald asked why the ether, the solar system, and the whole universe were not subject to the Boltzmann-Maxwell Law? Let us take the solar system first.

The law is obviously inapplicable to a *single* system (as I pointed out in my Report, and hope to prove still more conclusively shortly). In order to apply it, Prof. Fitzgerald would have to take *an infinitely large number of solar systems*, each consisting of similarly constituted planets differing, however, in their motions. What the law states is that, if the coordinates and momenta of the different systems were at any instant distributed according to the Boltzmann-Maxwell distribution (*i.e.* with frequencies proportional to e^{-hE}), they would be so distributed at any subsequent instant. In the absence of mutual action between the various solar systems, this would *not* be the only permanent distribution, nor would there be any tendency to assume such a distribution. If, however, the different solar systems were to collide with or encounter one another *at random* in such a way that transference of energy was liable to take place between any of the coordinates of any one system and any of the coordinates of any other system, the Boltzmann-Maxwell distribution *would* probably be unique, and there would be a tendency to assume such a distribution as the ultimate result of a great number of encounters taking place. Will not Prof. Fitzgerald agree to this?

With regard to the ether, I notice that Mr. Culverwell emphasises Prof. Fitzgerald's contention that the investigations ought to take "ethereal" as well as "molecular" coordinates and momenta into account. But here I agree with Prof. Boltzmann that the *onus probandi* lies with physicists. If they will give us a clear and definite statement as to *what are the coordinates and momenta of the ether*, and *how transference of energy takes place between these and the molecules*, and if they will show that the Boltzmann-Maxwell Law is violated under conditions under which we have proved it to be unique, a "true bill will have been found."

At present all we assert is that if the "ethereal coordinates and momenta" satisfy a determinantal relation similar to that proved on p. 22 of Dr. Watson's new edition, the Boltzmann-Maxwell distribution, *if it ever once existed*, will be permanent *in the absence of disturbing influences*. But the test case in which molecules are regarded as smooth solids symmetrical about an axis (see my Report, § 45, case iii.) affords an instance in which partition of energy does not take place between *all* the coordinates of a system, the angular velocity of each molecule about its axis of symmetry being constant and unaffected by collisions, and therefore independent of the Boltzmann-Maxwell Law. And why should not a similar explanation be applicable to the ether? At any rate, this hypothesis is supported by the views advanced by Prof. Oliver Lodge at the Nottingham meeting of the British Association ("Nottingham Report," p. 688). G. H. BRYAN.

Cambridge, November 30.

IT appears to me that the difficulty raised by recent critics against Maxwell's law of partition of energy in the theory of gases, and Boltzmann's minimum theorem relating thereto, by consideration of the effect of a complete reversal of the motions, is capable of direct explanation; and that whatever weak points the theory may have, they are not in that direction. Indeed, if that were not so, the criticism would apply equally against the Second Law of Thermodynamics.

The theorem in question is that there exists a positive function belonging to a group of molecules, which as they settle themselves into a steady state maintains—on the average derived from a great number of configurations—a steady downward trend; that the Maxwell-Boltzmann steady state is that one for which this function has finally attained its minimum, and is thus the unique steady state; it still being borne in mind that this is only a proposition of averages derived from a great variety of instances in which nothing is conserved in encounters except the energy, and that exceptional cases may exist, comparatively very few in number, in which the trend is, at any rate temporarily, the other way.

Such an exceptional case is in fact the very striking one, pointed out by Maxwell and Helmholtz, in which the motions of the system are all at some stage precisely reversed, so that it retraces its previous history backwards, and the trend of the reversed system is therefore in the opposite direction to the one which would lead towards the steady state. Now it has been assumed, at first sight plausibly, that there are just as many cases of this reversed motion as there are of the direct motion; and if that were so, it would undoubtedly go hard with the distribution theorem. But a fallacy underlies such an assumption, as indeed the other accepted proofs of simple cases of the distribution theorem would lead us to expect. Consider an arbitrary distribution of velocity and configuration of the system to begin with; and let it settle down for a time towards the final state, whatever that may be. Suppose at the end of that time that its velocities are all reversed; the system will retrace its course up to the initial state, but when it has got there, it will presumably go on settling down towards the final state by another route, because there is no longer any reason for exceptional behaviour. Thus there will be only a temporary aberration in the course of the reversed system; and further, if the original progress towards a steady state was at all rapid, this aberration will be sensible only for a brief time, the remainder of the history of the reversed motion corresponding nearly to the steady state. It is true that if the whole universe were thus reversed, the aberration would be permanent; but then the whole universe is a permanently dissipative system, and there is no question of a steady state being attained by it in measurable time. For a finite system, like a mass of gas imagined as bounded by a rigid envelope, the case would be different.

Thus even if these reversed states amounted to half the possible states, there would still be a preponderating, though not immense, probability in favour of a final settling down. But are these reversed states half the total number? The characteristic of such a state is that it is derived from an entirely fortuitous initial distribution by a process of change which is in the direction of the final steady state, whatever that may be, and much in that direction if the time concerned be considerable. It seems then that the number of configurations which can retrace their history for a sensible time is very much more limited than the total number of possible configurations, and that they are simply the exceptions which do not disprove the rule. For a theorem of average, derived from a very great number of instances, is of course not invalidated by picking out a comparatively small number of instances which depart widely from the average. J. LARMOR.

Cambridge, December 4.

Peculiarities of Psychical Research.

MR. H. G. WELLS disposes very aptly of most of the claims set up by Mr. Podmore and his colleagues to be real scientific investigators. But, I think, he rather disguises the significance of the card-drawing experiments to which he refers. The experiments of M. Richet and those of the S.P.R. belong to two very different categories. In the former case, 789 correct guesses were made in 2927 trials, or a deviation from the most probable result of 57 or 58; this is about 2·4 times the standard deviation, or the odds against a deviation *in excess* of this amount are only about 100 to 1, or odds of only about 50 to 1 of a deviation of this magnitude either way.

On the other hand, in the S.P.R. trials we have a deviation from the most probable of 347, about six times the standard deviation. That is to say, the odds against such a result are in round numbers about 2,000,000,000 to 3! Now, this is of a totally different order to that given by M. Richet's numbers. I have obtained odds as great as 100 to 1 against the results of very carefully conducted lottery experiments. There is in reality nothing significant about such odds. But the odds against the S.P.R. experiments are almost equal to the odds against the Monte Carlo roulette returns! The experiments are significant, very significant—not to my mind, however, of telepathy, but of the want of scientific acumen in the psychical researchers. The interesting point as to whether an abnormal distribution was also in the cards turned up as well as in the percipient, does not appear to have been recorded. Mr. Wells has, however, passed over the difference between the two cases, and given, I fear, the psychical researchers the chance of a little self-glorification on their due appreciation of the significant.

University College, December 8. KARL PEARSON.

Chronometer Trials.

THE Mersey Docks and Harbour Board have, by an Order dated November 29, 1894, modified the regulations under which they are prepared to issue certificates to those who deposit chronometers and other scientific apparatus at their Observatory for test and examination. Under the new regulations, instrument-makers can re-submit their apparatus within a twelvemonth of first deposit, without any additional fee. In the case of chronometer-makers this concession will probably be welcomed for the following reasons. Hitherto, the certificates granted have simply been regarded as a protection to the public, and the makers have had to apply their own tests to ensure accurate performance before submitting them to independent examination. But it will now be possible for makers to spare, in some degree, their own rigorous control, since the certificate granted will show the direction in which correction must be made, and a second certificate will be granted without fee to the improved instrument. If no alteration be needed, the time required for the additional trial is of course saved. Another modification, which will be appreciated by those who seek certificates for watches, is that affecting the condition under which these certificates are granted. The alteration will be best shown by an example. Suppose a watch to have a normal rate in the first position of trial of nine seconds a day, and under the various tests to which it is submitted the rate increase more than a second daily. Such a watch or chronometer, under the old regulations, would be refused an "A" certificate because the daily rate increased to more than ten seconds from mean time. But the watch might evidently be superior to one with a normal rate of two seconds, and which varied some five or six seconds in the various stages of its trial. The alteration in the regulations sanctioned by the Board will now permit the variations to be reckoned from the normal rate, and not from mean times. WILLIAM E. PLUMMER.

Liverpool Observatory, December 10.

Indo-Malayan Spiders.

IN your issue of November 29, Mr. R. I. Pocock, reviewing Mr. and Mrs. Workman's book on "Malaysian Spiders," states: "But the pine-apple is a native of South America, and has only of *late* years been introduced into Singapore; &c." Now, twenty years ago it was as common at Singapore as any other fruit, more so than many indigenous ones. How long before it may have been introduced I am unable to say, but that also should surely be stated "Before such a conclusion, however, can be looked upon as an established fact." I quite grant that in all probability the plant and spider were introduced simultaneously.

New Club, Grafton Street, W. B. A. MUIRHEAD.

Death-feigning in Snakes.

IN NATURE of November 29, p. 107, L. C. Jones asks whether death-feigning is, among snakes, confined to *Heterodon platyrrhinus*.

A writer in the *American Naturalist*, November 1894, pp. 966–8, tells almost precisely the same story of the "Moccasin" snake (*Ancistrodon*) and of "a black or blowing viper."

He also finds "letisimulation" in the toad, and in certain arthropods, worms, and protozoa.

December 1. GERARD W. BUTLER.

The alleged Absoluteness of Motions of Rotation.

PROF. GREENHILL'S remarks on my letter on this subject (NATURE, November 29), admitting that he is unconverted, and throwing out a suggestion that further arguments or explanations are desirable, appear to open a wider question than can be effectively dealt with in the limits of an ordinary letter. I propose therefore to deal with the subject in a special article to be written shortly. A. E. H. LOVE.

St. John's College, Cambridge, December 3.

Gravitation.

MAY I ask Dr. Joly whether Newton himself did not point out that a graduated tension excited by matter in a continuous inextensible medium, of an intensity proportional to the mass and the inverse distance, would account for gravitation; and whether he did not refrain from further elaborating this idea because there seemed at that time no adequate way of explaining the existence of such a tension? OLIVER J. LODGE.

"Outlines of Quaternions."

MAY I make a short explanation on one or two points on which my reviewer (NATURE, November 22) does not appear to have understood me?

(1) I mentioned Prof. Hardy and Dr. Odstrčil's names in one or two places, because I quoted their language verbally. I found it was better than any language I could devise.

(2) The extraordinary oversight on p. 76 would never have seen the light had I had either good health, or the assistance of a friend, in correcting the proofs of a MS. which was written at odds and ends of time, at places as widely separated as Norway, Gibraltar, and India.

(3) My reviewer says eq. 8 of p. 40—$i = \sqrt{-1}$—"plays sad havoc with one's very definitions." Having defined i as a right versor on p. 37, and explained on p. 39 that i^2 means ii, I deduced in the usual manner the eq. $-i^2 = -1$, or—

$$ii = i^2 = -1 = \sqrt{-1}\sqrt{-1}.$$

Hence I concluded that $i = \sqrt{-1}$; and I fail to see how I have played havoc with my definitions in doing so. Had I begun by explaining that Hamilton built up his system by treating $\sqrt{-1}$ as a right versor perpendicular to the line it operates on, I might have been open to criticism; but I did not do so. I took another course, which may not have been the best one; but that is a different thing from violating one's own definitions.

H. W. L. HIME.

24 Haymarket, S.W., November 26.

[A pointed reference to a scientific writer usually implies one of three things—that the writer is an authority on the particular subject under discussion, or has made a noted discovery in connection with it, or has been guilty of a serious blunder. Dr. Odstrčil's corollary hardly comes under the second category, and Prof. Hardy's statement differs in no essential word from Hamilton's own language in the "Lectures" (p. 83).

Equation 8 (p. 40, in "The Outlines of Quaternions") asserts the equality of $i, j, k, -i, -j, -k$, and as these symbols are by definition all different, the said equation is inconsistent with the definitions. To speak of $\sqrt{-1}$ as an indeterminate right versor, that is, an operator which rotates any vector through a right angle about an indeterminate axis—a most difficult operation for the mind even to imagine—may be permitted as a figure of speech; but to equate this backboneless thing to a real unit vector or right versor with all its powers of action, is making a serious demand upon the credulity of the student. After *defining* i, j, k as symbols involving both axis and angle, what right or reason has Colonel Hime thus arbitrarily to annihilate the axis? Is it not playing havoc with the very props of the calculus?—THE REVIEWER.]

THE WARBLE FLY.[1]

IT is only within comparatively recent years that much attention has been paid to the insect pests of the farm and garden. It is true that when these assume unusually devastating proportions, especially when they make their appearance suddenly, as in the case of locust-swarms, the attention of whole nations is called to them for the moment; but the loss caused by less obtrusive creatures may proceed unchecked and almost unsuspected for years, without attracting the notice even of those who suffer from it most. But there are now many entomologists, among whom Miss Ormerod deserves special notice in England, and Prof. Riley in America, who have been working zealously for years to diminish the loss and injury caused by injurious insects; and the pamphlet before us, with its clear descriptions and statistics, and excellent illustrations, conveys a mass of information, in a very handy form, which certainly deserves the most serious attention of all who are interested in the cattle and leather trades, whether as graziers, butchers, or tanners.

The total loss caused by the warble fly in the United Kingdom alone is estimated at something like £8,000,000 per annum; an enormous amount, but which the facts given in Miss Ormerod's pamphlet fully appear to bear out. When hides are sometimes so deteriorated that the loss on each may be as much as from twenty-five to thirty shillings, to say nothing of hides rendered utterly worthless; cattle killed, or the best parts of the carcase destroyed, and diminished yield of milk, the importance of the matter becomes very apparent. And beyond this, there remains a very serious question which Miss Ormerod has not touched upon at all: how far the milk of badly-infested cows, or the apparently sound portions of a carcase, even when all the obviously diseased part has been conscientiously removed, may be liable to cause disease in man—disease, possibly, of a nature the origin of which is at present absolutely unknown and unsuspected by medical men. And yet we remember once to have met with the statement that the best hides generally contained warbles. This, however, if true in any sense, could only mean that the fly attacks the strongest and healthiest animals in preference to weaker ones, thereby of course increasing the mischief produced by its attacks.

Although the insect is so abundant that as many as 500 maggots have been found in a single hide, yet the fly is rarely seen. When the cattle are attacked by it, they gallop wildly about, with their tails in the air, and seek the shelter of trees or sheds, or rush into the water; and in any of these situations, the fly does not appear to follow them. Cattle will act in the same manner when attacked by true gad-flies, one of the largest British species of which, *Tabanus bovinus*, is likewise noticed and figured by Miss Ormerod in her pamphlet. The gad-flies, however, simply pierce the skin of the cattle, and suck their blood, but inflict no permanent injury; and their larvæ are subterranean, and not epizootic.

According to the observations of Prof. Riley in America, the egg of the warble fly is deposited on, and not under, the skin. In the earliest stage of the maggot, which Miss Ormerod has herself observed, it is a small blood-red worm-like creature, scarcely visible to the naked eye, embedded in a slight swelling, composed of blood-red tissue, through which a fine channel, no wider than a hair, passes up to the surface of the skin (Fig. 1). In the very young stage, the maggot, which always rests with its head at the bottom of the sore, and the breathing apparatus, which is at the opposite extremity of the body, directed towards the opening which communicates with the external air, is provided with two forks or diggers, probably used for piercing through the substance of the hide. In this stage, too, the maggots are capable of inflating themselves with fluid which they have apparently no means of discharging, and become so hard that they can scarcely be compressed with the fingers, thus forming living and growing plugs, which act the part of setons, and which cannot be pressed back out of the wound, more especially as they are furnished with short bands of prickles along a portion of the back. Having penetrated the hide, the

[1] "Observations on Warble Fly or Ox Bot Fly (*Hypoderma bovis*, De Geer)." By Eleanor A. Ormerod, F.R.Met.Soc., &c. (London: Simpkin, Marshall, Hamilton, Kent, and Co., Ltd., 1894.)

maggot rests in the sore, and presently assumes a more pear-shaped form.

When about one-third grown, a great change takes place in the structure of the creature, which, while it was forcing its passage, was "little more than a bag of fluid, with a large proportion of the space *occupied by breathing-tubes*." At this stage, however, "the hard tips necessary, or at least serviceable for forcing a passage up the hide, are no longer needed, and they are exchanged for a broad form of spiracle, and the internal organs become suited to provide material for the development of the fly, which will presently form in the dry husk of the maggot, which serves as the chrysalis-case."

The further development of the maggot is so well known that we scarcely need trace its course until it reaches its final shape of a hairy two-winged fly, not very unlike a small humble-bee in general appearance, nor need we go into the elaborate accounts of the enormous loss which is frequently caused by it to all persons interested in living or dead cattle. The fly

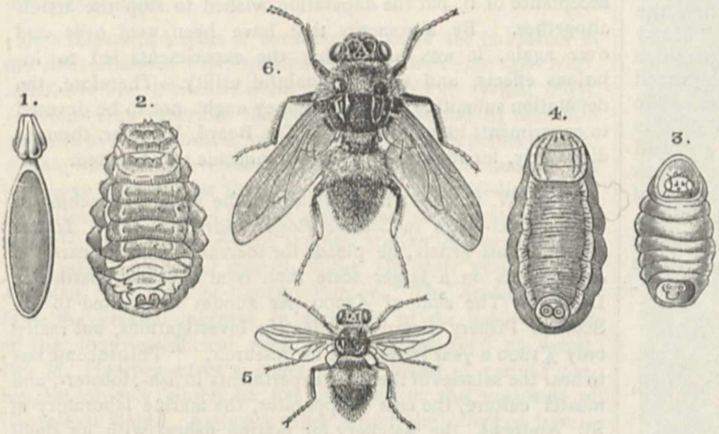

FIG. 1.—*Hypoderma bovis.* 1, egg; 2, maggot; 3 and 4, chrysalis-case; 5 and 6, fly; 3 and 5 natural size, after Bruey Clark; the other figures, after Brauer, and all magnified.

appears to be found in most parts of the world, but is a much greater pest in some countries than in others; and it is worthy of notice that, while goats appear to suffer from the warble as much or more than cattle, horses seem never to be attacked by it.

Miss Ormerod, however, gives several easy, harmless, and efficacious methods by which the mischief may be abated or removed; and the fly appears to be sluggish, and not to stray far from where it lived as a maggot, for after a few years' careful destruction of the maggots, the pest seems to disappear, without the farm being liable to fresh incursions from surrounding farms where similar precautions have not been taken to exterminate the maggots. Miss Ormerod has evidently done her best to show the farmers how they may best exterminate the pest; and if they do not avail themselves of the information which she has been at so much trouble to collect and to disseminate, it will not be her fault. The accompanying illustration is from her useful pamphlet.

W. F. KIRBY.

FERDINAND DE LESSEPS.

THE death of M. de Lesseps, on Friday last, removes from the world one of its more prominent men. To say that it was his indefatigable energy which brought to a successful termination the scheme to pierce the Isthmus of Suez, is but to repeat what is known to every schoolboy. With the affairs that during the last two years have obscured his fame to the political eye we have nothing to do. The work which earned for him the title of "Le Grand Français" is sufficient to command the admiration of every man of science. The appreciative obituary notice in the *Times*, running into nearly four columns, deals largely with de Lesseps' diplomatic career, but this does not concern us. We are indebted to the notice, however, for some of the following particulars of interest to our readers.

Ferdinand de Lesseps was born at Versailles, November 19, 1805. His early public life was spent in the diplomatic service, for which he manifested the same predilections as his ancestors. But so far back as 1841, the project for cutting through the Isthmus of Suez had occurred to him. It was not until 1854, however, that he first revealed the scheme that will be most lastingly associated with his name. Two years later, Said Pasha, the Viceroy of Egypt, granted him a formal letter of concession. In the same year de Lesseps published a clear and definite exposition of his views in his pamphlet "Percement de l'Isthme de Suez. Exposé et documents officiels." Many eminent engineers questioned the practicability of the scheme; nevertheless a capital of two millions of francs was subscribed, and in 1859 the works were commenced.

Difficulties and disputes of a most serious character cropped up from time to time, but they were overcome, with the result that a canal, having sufficient water to admit of the passage of steamboats, was opened on August 15, 1865. The channel was widened and deepened by special machinery, and in March, 1867, small ships were able to make use of the Canal. The waters of the Mediterranean mingled with those of the Red Sea in the Bitter Lakes on August 15, 1869, and the event was commemorated by grand *fêtes* at Suez. On November 20 following, the Canal was formally opened at Port Said amid a series of brilliant festivities. The Canal is about 100 miles long, with a bottom width of upwards of 200 feet, and a depth of 28 or 29 feet.

Honours poured in upon M. de Lesseps after the successful opening of the Canal. In February, 1870, the Geographical Society of Paris awarded him the Empress's new prize of 10,000 francs. He gave it as a contribution to the Society's projected expedition to Equatorial Africa. He was appointed to the rank of Grand Cross of the Legion of Honour, and received the cordon of the Italian Order of St. Maurice. The honorary freedom of the City of London was presented to him, and Queen Victoria created him an honorary Knight Grand Commander of the Order of the Star of India. In July, 1873, the Paris Academy of Sciences elected M. de Lesseps a member, in the place of the late M. de Verneuil. In 1875 he published his "Lettres, Journal, et Documents pour servir à l'Histoire du Canal de Suez." For this work the French Academy awarded to him the Marcelin Guérin prize of 5000 francs. In June, 1881, he was elected President of the French Geographical Society, in the place of Admiral de la Roncière-le-Noury. The Broad Riband of the Persian Order of the Lion and the Sun was presented to him in 1883.

M. de Lesseps promoted the project of the Corinth Canal, and made a journey in Algeria and Tunis to study the scheme of Commandant Rondaire for the creation of an inland sea in Africa—a scheme of which he formed a favourable opinion. Gradually, however, he became wholly absorbed in the undertaking which was to prove his ruin—the Panama Canal. All the world knows how this ended. After the humiliating drama had been played out at the beginning of last year, the central figure sunk into a state of stupor, and in this condition—almost oblivious of everything that went on around him—he remained until he passed into the silence of death.

But let us forget these events. However dark they may seem to be, they cannot hide from us the wonderful applications of science we owe to Lesseps.

We learn from the *Times* that Mdme. de Lesseps has received many messages of condolence. The Emperor of Germany telegraphed to her, "All intellectual and scientific people mourn over the tomb of one of the greatest minds and of a genius which embraced the universe." Lord Dufferin has conveyed the sincere grief of the Prince of Wales, as also the sympathy of Lord Kimberley. It is understood that a committee of the Suez Canal Board is to propose to the company that a statue be erected to M. de Lesseps at the expense of the company, and that the city of Paris is to be asked to grant a site in one of the public squares.

These expressions show the high regard in which de Lesseps was held. A still more striking testimony is afforded by the fact that the Paris Academy of Sciences adjourned on Monday in sign of mourning, when the president, M. Lœwy, is reported to have said: "Many storms had latterly broken over the head of the illustrious veteran. It is, perhaps, not to be too much regretted that his declining strength had for several years made him almost a stranger to the melancholy affairs of this world. His name will for ever be linked with a grand work, the success of which is due entirely to his glorious efforts, and will be a memorable date in the history of civilisation."

Scientific posterity will remember the Academy's act of respect to de Lesseps, and M. Lœwy's words of tribute to his colleague's memory.

NOTES.

WE are glad to see the report that M. Pasteur, who has been poorly for some little time, is improving in health.

PROF. G. LEWITZKY, the Director of Charcow Observatory, has been appointed Director of Dorpat Observatory, in succession to the late Prof. L. Schwarz. Dr. L. Struve goes to fill Prof. Lewitzky's place at Charcow.

THE Academy of Sciences of Berlin has granted a subsidy of 1200 marks to Dr. P. Kuckuck, to aid him in the investigation of the alga-flora of Heligoland.

WE learn from the *Botanisches Centralblatt*, that Herr W. Siehe, of Berlin, has undertaken a botanical exploration of the almost unknown region of Cilicia Trachæa.

CONSIDERABLE changes have recently been made in the scientific department of Smith College, U.S.A. The botanical department has been reorganised, and Dr. W. F. Ganong appointed professor. Miss Grace D. Chester, formerly instructor in botany, has been appointed instructor in cryptogamic botany.

OUR correspondent at Cambridge has sent us the following notes:—The Council of the Senate have appointed Dr. R. D. Roberts to be a governor of the Royal Holloway College, Egham. Dr. H. H. Tooth, of St. John's College, has been appointed an additional examiner in medicine. The Walsingham Medal for biological research has been awarded to H. Burkill, assistant curator of the Herbarium. The following awards for Natural Science have been made at St. John's College: G. S. West, Royal College of Science, London, Foundation Scholarship of £80 a year; E. F. Hudson, Dulwich College, and T. F. R. McDonnell, St. Paul's School, Minor Scholarships of £50 a year; A. C. Ingram, Felsted School, Exhibition of £30.

A SOCIÉTÉ des Amis des Explorateurs français has recently been formed at Paris. Its object is to collect and administer funds for the purpose of aiding travellers, particularly in their return, and to contribute to the progress of geography by the publication of the results of explorations, and other contributions to geographical science. The Society is in connection with the Paris Geographical Society.

WE thought it would come. The anti-vaccinationists, anti-vivisectionists, and kindred souls have, if only for the sake of consistency, protested against the new treatment for diphtheria. At the ordinary fortnightly meeting of the Metropolitan Asylums Board, on Saturday last, a deputation, headed by Lord Coleridge, waited upon the Board to present a memorial against experiments being carried out in the hospitals in the Metropolitan Asylums district in the use of the anti-toxin cure for diphtheria. The Royal College of Physicians had offered to supply serum for the treatment of diphtheric patients in the Board's hospitals, and the General Purposes Committee advised the acceptance of it, but the deputation wished to stop the action altogether. By arguments that have been used over and over again, it was urged that the experiments led to injurious effects, and were of doubtful utility. Therefore, the deputation submitted, "public money ought not to be devoted to experiments in physiology." The Board, however, thought differently, for the report of the Committee was adopted.

PROF. W. C. MCINTOSH writes on the artificial hatching of marine food-fishes in *Science Progress* for December. In the course of his article, he pleads for increased funds to carry on experiments on a larger scale than is at present possible at Dunbar. The sum of £3000 per annum is granted to the Scottish Fishery Board for scientific investigations, but really only £1800 a year is available for research. "This income has to bear the salaries of the staff, experiments in fish-, lobster-, and mussel culture, the cost of apparatus, the marine laboratory at St. Andrews, the hatchery for marine fishes, with its small laboratory at Dunbar, and the carrying out of other scientific fishery work." Leaving England out of consideration, compare this with what is done on the other side of the Atlantic. "The United States spends annually £70,000 on fish-culture and scientific investigations, and employs two large steamers and a sailing vessel exclusively for the work. Besides this large sum, the Fish Commissioners of the various States also disburse considerably on the development of their fisheries. Canada, again, expends £100,000 yearly on her fisheries, of which a sum of about £10,000 is devoted to fish culture."

IT was pointed out by Lord Rayleigh in these columns in 1883 (vol. 27, p. 535), that whenever a bird pursues its course for some time without moving its wings, we must conclude either (1) that the course is not horizontal, (2) that the wind is not horizontal, or (3) that the wind is not uniform. Prof. S. P. Langley's recently published memoir on the internal work of the wind shows that the condition represented by the third cause, which Lord Rayleigh believed must sometimes operate, always exists. The investigations described in the memoir distinctly prove that the wind is never a homogeneous current, but consists of a continued series of rapid pulsations varying indefinitely in amplitude and period. These pulsations undoubtedly help a bird to maintain its flight without working its wings or expending energy. This may be accomplished by a succession of ascents and descents; the ascents being made during the wind-gusts, and the descents during the lulls. Prof. George E. Curtis, of Washington, has lately investigated mathematically whether the third case of Lord Rayleigh's analysis is the one actually employed by a bird in soaring; that is to say, he has tested whether the pulsations of the wind, in addition to being qualitatively applicable, are also quantitatively sufficient to produce the result. (*Annals of Mathematics*, vol. viii. No. 6.) With this application in view, he has determined the course in the air of a free heavy plane subjected to definite wind pulsations. For a period of action of ten seconds, the following result was obtained "without any internal source of energy, during the 10 seconds of alternate calm and wind, the plane has in the first 2·75 seconds made a descent of 36 feet, and in the remaining 7·25 seconds has risen 46·9 feet, travelling at the same time horizontally a distance of 251 feet, of which 154 feet is made against the wind. In addition, the relative velocity of the plane and wind (58·9 feet per second) at the end of this period is sufficient, if the wind continue with the same velocity, to yield a considerable further ascent before the vertical component of pressure is reduced to such an extent that it no longer exceeds the weight of the plane under the constant angle of inclination." Though the conditions of the problem solved by Prof. Curtis are seldom met with in nature, the results he has obtained have a very important bearing upon mechanical flight.

A NUMBER of papers of considerable value are published in the *Mittheilungen der Prahistorischen Commission der Kais. Akademie der Wissenschaften.* (Bd. 1, No 3. Wien, 1893). J. Szombathy describes the ceramics discovered by him in a tumulus at Langenlebarn, in Lower Austria. He points out that double and multiple vessels were common in the finds of the bronze period and first iron age of the eastern Mediterranean countries, especially in Cyprus and Troas, somewhat rarer at a later time in the graves of the first Italian iron age, and still more rare in the later graves of the Hallstatt period of the Austrian Alps and of Central Europe. The doubling arose from the gradual increase in diameter of the lower portion of the long cylindrical neck of an earlier type of vessel. Dr. M. Hoernes gives a study of the forms of various prehistoric objects which he has observed in the museums of North-east Italy. Amongst other objects he refers to bronze and iron knives in the museum at Padua. The resemblances in the handles and ornamentation of certain clay vessels leads him to the conclusion that the culture of the bronze age of North-eastern Italy extended to the East, and included Bosnia-Herzegovina and Istria; but from this he does not deduce an ethnical relationship. In the bronze period of Eastern Upper Italy not only were pile-dwellings inhabited, but mounds as well, as in Istria and Bosnia-Herzegovina. Even if related pile-structures are not to be found in the north-west of the Balkan Peninsula, the difference in the mode of life is counterbalanced by other considerations. The evolution of the Italians and the Illyrians later diverged in the historically well-known way. He would add the Ligurian people as belonging to the same culture group of that age; this culture-unity separated these people from other peoples living further north and south of this zone. Dr. Hoernes also describes some triangular and other ornaments, human and animal forms, especially those with associated birds, as well as bronze and iron fire-dogs. Prof. R. Trampler writes on the oldest graves in the Brünnen Valley district; and F. Heger, on finds from prehistoric and Roman times in Lower Austria. The author of this well-illustrated paper states that there was a pure bronze age in the district of Upper and Lower Austria, which had points of relationship with the Western finds, and especially with those in the North and East.

A CURIOUS formation is illustrated in the first annual report of the Iowa Geological Survey, published a few months ago. Good exposures of limestone rocks are found all along the Mississippi from Keokuk to Burlington, in south-eastern Iowa. The limestone often stands out in overhanging cliffs over the softer shale beds beneath, and gives the appearance of a cascade, as shown in the accompanying illustration, which is reduced from a plate in the report. The Survey was only established

in 1892, but the report shows that a large amount of useful information, of great economic interest to the people of the State, has already been collected.

THE volume referred to in the foregoing note showed us that the publications of the Iowa Geological Survey were to be of a high character. The second volume, which reached us a few days ago, goes to confirm this view. It is a description of the coal deposits of Iowa, by Dr. C. R. Keyes, and is a model of what a general report should be. With text running into more than five hundred quarto pages, eighteen full-page plates of a high quality, representing interesting formations in connection with the coal-measures, and over two hundred figures in the text, the volume is an attractive handbook for the coal-miners of Iowa. It is not a detailed account of the geological features of the coal districts—that will follow when sufficient facts have been accumulated; it is rather a preliminary report, somewhat general in character, but sufficient to supply temporarily the demand for information pertaining to the coal deposits of the State. Separate volumes will be devoted to practical mining in Iowa, and to a description of the uses and properties of Iowa coals. We offer our congratulations to Dr. Keyes and the geological corps with which he is associated. May their intentions with regard to future work be satisfactorily realised.

IN the current number of the *Zoologischer Anzeiger* (No. 462), Dr. Arnold Graf records some novel observations made by him in the course of some experiments on the effects of compression on the segmentation of the egg of the sea-urchin *Arbacia*. Driesch has already shown that compression of the segmenting egg between slide and cover-slip leads to the formation of a flat plate of blastomeres, and Dr. Graf's observations coincide completely with those of Driesch up to the 32-cell stage. At this point the author determined to remove the pressure exerted by the cover-slip, and to notice the effect produced on the egg by this reversion to normal conditions. To his astonishment he found that, after removing the pressure (by gradually adding a surplus of water between slide and cover-slip) the various blastomeres began to fuse together again. In this way a plate of 15 cells was produced, each cell containing two and, in a few cases, three nuclei. Shortly afterwards the separate nuclei in each cell reunited. The last transformation observed by the author was the conversion of the plate into a 12-cell stage, consisting of eight macromeres and eight micromeres. The results of the experiment are full of interest, and well worthy of renewed trial. The author is certain that the fusion of the blastomeres took place quite regularly: the two or three daughter-cells of one mother-cell reunited to form the single uni-nucleated equivalent of the mother-cell again.

CERTAIN obscure phenomena connected with the mingling of two masses of liquid are dealt with by Herr E. Kaiser in the current number of *Wiedemann's Annalen*. Two soap-bubbles or two jets of water when brought into immediate juxtaposition will not always mingle at once, and sometimes they will not do so at all. Impurities in the water or soap solution will encourage fusion, and so will a difference of potential. Whether the influence of the latter may be explained on the supposition of sparks breaking down the intervening layer of air, is a question which has been answered in the affirmative. But Herr Kaiser's experiments tend to show that in reality the difference of potential simply increases the pressure on the intervening air, and forces it out at the sides, thus diminishing the distance between the two surfaces down to the radius of molecular action. He suspended a circular film of Terquem's sugar-soap solution in a wire ring by a delicate spring balance, and brought a bubble to bear against it from below. The film and bubble were placed in electric connection through their supports, and differences of potential due to 1, 2, 3 and more Daniells were subsequently introduced. With the films investigated, the time necessary for fusion was about 3·2 seconds with no difference of potential, 1·4 seconds with one Daniell, and 0·4 with two. With more Daniells fusion took place instantly, and the films burst in most cases. The pressure was 1·016 gr. The displacement of air from between the films was studied by the aid of the Newton's rings formed between them. The rings widened out, rapidly at first, then more slowly, and the mingling was heralded by the appearance of the grey-blue of the first order. A difference of potential simply accelerated this process.

IT has been said by someone, that a whole set of meteorological predictions may be disturbed by a cowboy carelessly throwing down a burning match upon a prairie. The match starts a prairie fire which causes changes extending throughout the atmosphere, and so an accident may upset the most carefully prepared forecast. This is, of course, a far-fetched case, but it is worth while knowing what effects great forest fires have upon the atmosphere. To this end Prof. Cleveland Abbe, in the *Monthly Weather Review*, determines some of the meteorological results of the extensive forest fires in the United States during July and August of this year. It appears that, with regard to their influence upon atmospheric moisture, the experience of the past summer is sufficient to show that forest fires are not necessarily followed by rain, and are not a practical method of inducing rain in dry seasons. Comparing the normal maximum effect of solar heat with the computation of the effect of burning forest, Prof. Abbe finds that, in the locality where it occurs, a forest fire can heat its atmosphere more than the hottest sun of June, in the ratio of 10,000 to 750. Fortunately, however, the general influence of the forest fire upon the whole atmosphere is much smaller than that of the sun, because the fire is of small extent, while the sun affects the whole earth. The actual area covered by the forest fires of August in Minnesota, Wisconsin, and Michigan did not exceed five thousand square miles, whereas the area covered by the smoke, and, therefore, hot air, from these fires before the heat was all lost by radiation, was not less than one million square miles. A comparison of the solar effect over this large area and the more intense forest fire effect over the small area, shows that the former is fifteen times that of the latter. Prof. Abbe's calculations thus afford no foundation for the belief that forest fires have important meteorological sequelæ. The story of the connection between careless cowboys and atmospheric circulation is, therefore, no longer worth telling.

THE current number of *L'Elettricista* contains a description of a new method for measuring small resistances, due to Dr. Pasqualini. This method, which requires no special apparatus that cannot be easily set up in the laboratory, consists in having a coil, composed of a few turns of wire, wound double, so that there are two similar circuits. This double coil is fixed to the case of an ordinary galvanometer, so as to act on the needle. The main current sent through the resistance to be measured passes through one of the circuits of this auxiliary coil. A shunt circuit to the resistance is formed by the second circuit of the coil, the galvanometer, and a resistance-box. The connections are so arranged that the main current and the shunt current in the galvanometer coils tend to turn the needle in opposite directions. The resistance of the shunt circuit is varied till the galvanometer deflection is zero. Suppose K is the galvanometer constant, while K_1 is the constant which expresses the effect of either of the circuits in the auxiliary coils on the needle, and if I and i are the total main current and the fraction which passes through the shunt circuit respectively. Then $K\,i = K_1\,I - K_1\,i$, or if R is the resistance of the shunt circuit, the resistance to be measured $= \frac{K_1}{K}R$. The value of the constant $\frac{K_1}{K}$ can be obtained by performing the experiment on a known resistance. With an auxiliary coil consisting of two similar circuits containing four turns each, and a Wiedemann galvanometer of 8 ohms resistance, the author finds he can measure a resistance of ·0002 ohms within $\frac{4}{1000}$ I, using a standard ohm to determine the constant $\frac{K_1}{K}$.

IN a paper communicated to the *Physical Review*, Mr. S. H. Brackett gives an account of some experiments he has made on the magnetic properties of iridium. The samples used contained 98 per cent iridium, with some platinum, and a trace of phosphorus, but no iron. A bar of the metal 13·3 m.m. long, 3·2 m.m. wide, and 0·9 m.m. thick, when brought into the field of an electromagnet, set itself at right angles to the lines of force, and became permanently magnetised in a transverse direction, and when suspended, freely set itself E. and W. The permeability of a larger bar was tested, and was found to be unity; and although the specimen was sharply struck while in the magnetising coil, no induced magnetisation was produced.

A NEW illustrated descriptive price list of electric and magnetic apparatus, for use in colleges and scientific institutions, has been issued by Messrs. King, Mendham, and Co., Bristol.

WE have received a *Jahrbuch* containing the results of meteorological observations made at the observatory of the

Magdeburgischen Zeitung during 1893. The volume is edited by Herr A. W. Grützmacher, and is in its thirteenth year.

THE *Bulletins* of the Michigan State Agricultural College Experiment Station, for October, contain an exhaustive paper on the insects which attack clover, by Mr. G. C. Davis, and one on rape as a forage-plant, by Mr. C. D. Smith and Mr. F. B. Mumford.

THE eighth part of "The Natural History of Plants" has been published by Messrs. Blackie and Son. It deals with the genesis of plant-offspring, one section being devoted to asexual reproduction, while the other refers to reproduction by means of fruits.

MM. J. B. BAILLIÈRE ET FILS, of Paris, following other firms of booksellers and publishers, have commenced the issue of a monthly list of new books, under the title *Bibliographie des sciences naturelles.* The list for November comprises the titles and prices of important works on protozoa, sponges, cœlenterata, echinoderms, &c.

THE *Transactions* of the Academy of Science of St. Louis, U.S.A., include a paper on *Sclerotinia Libertiana*, a fungus parasite of the sunflower, by Prof. L. H. Pammel, to which is appended a bibliography of the fungus diseases of roots, covering thirty-seven pages; also a further instalment of Mr. C. Robertson's observations on the relationship between flowers and insects in the process of pollination, the present paper treating of American species of *Rosaceæ* and *Compositæ.*

A VOLUME containing a number of important papers and reports relating to the gold fields of New Zealand has just reached this country. The mining industry has formed a large factor in the advancement of the colony, and the statement issued by the Government shows the direction in which further developments should be made. The volume comprises reports on the gold fields and coal mines, geological reports on the older quartz-drifts in Central Otago, and a report on deep quartz-mining in New Zealand.

MR. EDWARD STANFORD has acted wisely in reissuing Dr. A. R. Wallace's standard work on "Australasia" in a revised and enlarged form. The first volume of the new edition, dealing with Australia and New Zealand, was published a few months ago. The second volume, which has just appeared, has for its subject Malaysia and the Pacific Archipelagoes, being an enlargement of the part devoted to that region in the original work. Dr. F. H. H. Guillemard is responsible for the new work, and he has performed his task so thoroughly that the present volume occupies nearly twice the number of pages previously allotted to the region with which it deals. The work is certainly the most interesting and accurate account extant on the tropical portion of the Eastern Archipelago.

STUDENTS of the anatomy of the horse will be glad to learn that the second volume of the elaborate "Topographische Anatomie des Pferdes," by Drs. W. Ellenberger and H. Baum, has been published by Paul Parey, Berlin. The first volume, dealing with the structure of the limbs of the horse, appeared in the spring of last year. The volume just received has for its subject the head and shoulders; while the third, which the publisher informs us will be issued early next year, will be devoted to the trunk. We shall review the work when it is completed. So far as we can at present judge, it promises to be a useful contribution to the literature of the subject, possessing both scientific and practical importance.

PROF. W. NERNST'S important treatise on "Theoretical Chemistry, from the Standpoint of Avogadro's Rule and Thermodynamics," the German edition of which was enthusiastically received last year, has been translated by Prof. C. S. Palmer, of the University of Colorado, and the translation will shortly be published by Messrs. Macmillan and Co. The work is a development of Prof. Nernst's introduction to the "Handbuch der Anorganischen Chemie" of Dr. O. Dammer. It is, however, quite an independent text-book for students of the new physical chemistry, and includes the results of all recent investigations bearing upon that science. The book will serve at once both as a treatise in itself, and also as an introduction to, and companion in, the larger field covered by the *Zeitschrift für Physikalische Chemie* and the related literature.

ANOTHER scientific work which Messrs. Macmillan will shortly issue, is "Steam and the Marine Steam Engine," by Mr. John Yeo. The book is chiefly intended for naval officers, and for students of engineering in the earlier part of their training. It aims especially at giving a sound general knowledge of the propelling machinery of ships, and of various matters connected with its use and management.

IN "Darwinism and Race Progress," which Messrs. Swan Sonnenschein and Co. will shortly publish, Prof. Haycraft shows how the racial deterioration which would of necessity ensue upon our modern care of the sickly and enfeebled, may be counteracted by a keener public conscience. Our preservation of unworthy types by public and private charity is strongly animadverted upon, and with regard to intellectual development, it is pointed out that the present democratic movement, while it gives a chance to the clever and capable of becoming educated and well-to-do, entails upon them conditions which generally imply late marriages and relative sterility. Without supplementary action, nothing could be devised which would more effectually breed capacity out of the race.

TWO volumes have lately been added to the comprehensive engineering division of the Encyclopédie Scientifique des Aide-Mémoire, published jointly by Gauthier-Villars and Masson. They are "Les Chronomètres de Marine," by M. E. Caspari, and "Torpilles Sèches," by M. E. Hennebert. As is the case with all the volumes in the same series, the two new ones are practical handbooks of a very instructive character. In the former, the construction of chronometers, and the theory relating to it, are clearly described, and a deal of space is devoted to the effects of various conditions upon the rate, and also to the determination of the coefficients of the rate-formula. The determination of longitude by means of chronometers, and the methods of testing and comparing the instruments, are concisely described. Both M. Caspari's work, and that by Lieut.-Colonel Hennebert on torpedoes, bring together a lot of scattered information of great use to students of the subjects with which they respectively deal.

COMMERCIAL geography is just the kind of subject to be fostered by County Councillors, hence, from the time that the Technical Instruction Act came into operation, there has been an increasing demand for text-books upon it. The first edition of Dr. H. R. Mill's "Elementary Commercial Geography," in the Pitt Press Series (Cambridge University Press), appeared in 1888, when the boundary line of commercial geography was in a more or less nebulous condition. Thanks to the impetus that has been given to the subject during the past few years, the book has reached a second edition. The new issue is not, however, merely the original work reprinted, for it has almost entirely been rewritten. To quote the preface: "The book has been revised throughout by the aid of official publications, and the facts are as far as possible brought down to date. It is enlarged by treating more fully of the principles of commercial geography, by describing the African possessions of the European powers in greater detail, and by many small additions in every chapter."

These additions have enlarged the book from 132 to 181 pages, and have correspondingly increased its value. It would be difficult to find an elementary class-book of commercial geography constructed on better lines, or in which the information is more concisely and accurately stated.

The additions to the Zoological Society's Gardens during the past week include a Spotted Owl (*Athene brama*), four Grey Francolins (*Francolinus ponticerianus*), three Rain Quails *Coturnix coromandelica*), an Indian House Sparrow (*Passer domesticus*), two Red-headed Buntings (*Emberiza luteola*), two Nutmeg Finches (*Munia rubro-nigra*), a Spotted Turtle Dove (*Turtur meena*) from India, presented by Mr. E. W. Harper, a West African Love Bird (*Agapornis pullaria*) from West Africa, presented by Mrs. Robinson; a Reticulated Python (*Python reticulatus*) from Malacca, presented by Mr. Sigismund Bruzaud; an American Black Bear (*Ursus americanus*, var. *cinnamonea*) from the Rocky Mountains, two Common Cassowaries (*Casuarius galeatus*) from India, a Red-vented Parrot (*Pionus menstruus*), two Orange-flanked Parrakeets (*Brotogerys pyrrhopterus*) from South America, a Scops Owl (*Scops* ——) from Formosa, deposited; a Short-billed Toucan (*Ramphastos brevicarinatus*) from Central America, received in exchange.

OUR ASTRONOMICAL COLUMN.

Motion and Magnitude.—Students of elementary astronomy often believe that stellar motions in the line of sight soon produce changes in the magnitudes of stars. Motion towards the earth involves, of course, a certain increase of magnitude, and a motion of recession must carry with it a decrease, but the amount in the case of a star is far too small to be measurable, even if the magnitudes are observed during many generations. At the meeting of the Amsterdam Academy on November 24, Prof. Oudemans communicated the results of an investigation to determine exactly how long stars of which the velocity in the line of sight are known would have to go on moving, in order to produce a change of 0·1 magnitude. From his own list of parallaxes in vol. 122 of the *Astr. Nachrichten*, and Vogel and Scheiner's list of proper motions in the line of sight ("Potsdam Observations," vii. i. p. 153, 154), fourteen stars were selected, four of them receding from, and the remaining ten approaching, the solar system. Adopting a solar parallax = 8″·815, and the logarithm of the proportion of the increase of brilliancy for one magnitude = 0·400, he found that the period required is given by the formula

$\frac{6195}{\text{parallax} \times \text{motion}}$ years for stars that are receding from,

and $\frac{5916}{\text{parallax} \times \text{motion}}$ years for those that are approaching

to the solar system; the parallax being expressed in seconds, and the motion in geographical miles (1 geographical mile = 4·61 English miles). Aldebaran proved to be the only star of which the brightness could, since Ptolemy's time, have experienced a loss of 0·1 magnitude by its radial motion, provided that the parallax 0″·52, found by Otto Struve, is trustworthy. Elkin found a value = 0″·12, and adopting this value, the period becomes 4½ times longer. For the other stars the result was 5500 years at least for Procyon; while most of them gave tenthousands of years as the result.

The Recent Transit of Mercury.—Details of several of the American observations are published in *Astronomical Journal*, No. 330. At most of the stations, some of the contacts could not be observed on account of clouds, but, on the whole, a fair amount of success attended the observers. Prof. Young observed the first and second contacts, and reports that there was a sort of "hardening" of the sun's limb close to the expected point of first contact a few seconds before the actual contact took place; he has now observed this phenomenon four times, and states that "it may be due to the planet's obscurvation of the brightest part of the chromosphere close to the disc of the sun, or to some diffraction effect at the limb of the planet." A large "black drop" was observed two seconds before the second contact. Prof. Todd failed to observe the contacts on account of unfavourable weather, but obtained some results during the passage across the disc. He says:—"The planet appeared perfectly circular, sharply defined about the limb, unattended by any halo or atmospheric ring, and of the same colour as the umbræ of the spots on the sun. No attendant satellite of Mercury was seen, nor could any bright spot be discerned upon the centre of the disc, although intently looked for. Any satellite smaller than 100 miles would have escaped detection."

The New Achromatic Object-glass.—*Engineering* for November 23 and 30 contains a full account of a fine 12½ inch equatorial that has just been built by Messrs. Thomas Cooke and Sons, York, for the new observatory at Rio de Janeiro, together with a sketch of the Buckingham Works at York. The description is accompanied with twenty-nine detailed drawings of the instrument. It appears that the works are the only ones in this country where every part of a modern astronomical telescope is constructed; with the exception of the actual making of the glass, everything is done on the premises, even the heavy cast-iron pillars being made in the firm's own foundry. The new instrument, with Messrs. Cooke's standard form of equatorial mounting for large refracting telescopes, is fully equipped for all branches of astronomical research. The clock, too, has all the latest improvements for adjustment. The object-glass is of the three lens form, invented by Mr. Taylor, who deserves the thanks of astronomers for producing an objective which, whilst made of durable kinds of glass, and of moderate focal length, is perfectly achromatic. How this end is attained was described in these columns on March 15 (vol. xlix. p. 464). A section of the lens is shown in Fig. 1. The first, L_1, is made of baryta light flint, having a refractive index of 1·5637 for the D ray, and its reciprocal of dispersive power is 50·6. The second lens, L_2, is made of a new glass known as boro-silicate flint. This glass has a refractive index of 1·54685 for the D ray, and the reciprocal of its dispersive power is 50·2. The third lens, L_3, is made of light silicate crown glass having the following characteristics: refractive index for D ray, 1·5109; reciprocal of dispersive power 60·4. By suitably proportioning the various radii of the lenses, the middle negative lens may be made to almost exactly compensate for the dispersion effected by the other two, and this without necessitating any exceptionally great focal length. If necessary this may be made so small as fifteen times the aperture, and there is no difficulty whatever in making a glass with a focal length of eighteen apertures. A small space is provided between the second and third lens, the object of this being to make the correction for spherical aberration as complete as possible; and when the thickness of this space is properly proportioned, there is an entire absence of spherical aberration for all colours of the spectrum. Object-glasses made on this plan have been tested by astronomers for visual and photographic observations. In both cases the stellar images showed no indications of residual colour, the lenses behaving just like the mirror of a reflector.

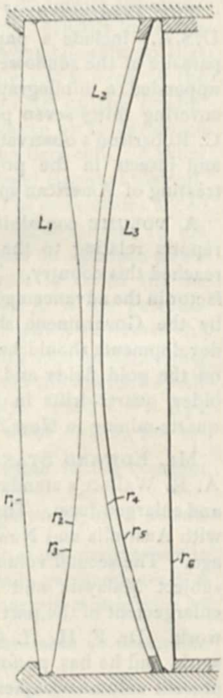

Fig. 1.—Section of the New Object-glass.

Ephemeris for Swift's Comet.—The surmise noted in these columns last week has proved to be correct. M. Schulhof shows pretty conclusively, in the current *Comptes-rendus*, that Swift's new comet is really identical with De Vico's comet 1844 I. The subjoined ephemeris is from one given by Prof. Lamp in the *Astronomische Nachrichten*, No. 3266.

Ephemeris for Berlin Midnight.

1894.	R.A. (app.) h. m. s.	Decl. (app.) ° ′	Brightness.
Dec. 13 ...	23 21 36 ...	−5 33·9	
,, 15 ...	26 46 ...	4 55·7 ...	0·53
,, 17 ...	31 52 ...	4 17·9	
,, 19 ...	36 54 ...	3 40·5 ...	0·48
,, 21 ...	41 53 ...	3 3·5	
,, 23 ...	23 46 48 ...	−2 26·9 ...	0·43

The brightness of the comet on November 21 has been taken as unity.

A NEW STAR?—The Rev. T. E. Espin has announced that a very red star of the eighth magnitude, not in the Bonn Durchmusterung, was found by him on November 29, in R.A. 17h. 54·3m. Decl. + 58° 14′. The spectrum belongs to Secchi's Type IV.

PROF. VICTOR MEYER'S NEW METHOD OF DETERMINING HIGH MELTING POINTS.

A DESCRIPTION of improved apparatus for the determination of high melting points, by his admirable new method, is contributed to the current *Berichte* by Prof. Victor Meyer, in conjunction with his students Messrs. Riddle and Lamb. The simplicity of the method will doubtless cause it to take rank immediately among the standard processes for the determination of physical constants, and alongside the universally popular method of determining vapour densities, which we likewise owe to the distinguished Heidelberg professor. Naturally, however, operations at temperatures higher than those at which the hardest varieties of glass soften, must perforce be conducted in apparatus constructed of platinum, just as in the cases of the determinations of vapour density at the same high temperatures. One of the main advantages of the method is that it only necessitates the use of a very small quantity of the substance whose melting point is to be determined, thus enabling it to be extended to compounds of the most extreme rarity.

The method is based upon the principle of measuring the temperature by means of a miniature air thermometer constructed of platinum, the air contained in which is expelled, at the moment when the fusion of the substance under investigation occurs, by means of a soluble gas into a gas-measuring vessel filled with a liquid capable of dissolving the expelling gas. The substance whose melting point is to be determined is placed in a small and very narrow platinum tube, which is fixed to the bulb of the air thermometer during the operation, and both are immersed in a bath of a fused salt whose melting point is considerably below that of the substance under investigation. Hence the operation of determining a high melting point by this method is perfectly analogous to that usually adopted in determining ordinary melting points lower than the temperature of boiling mercury.

The air thermometer is simplicity itself. It consists of a spherical platinum bulb of about 25 c.c. capacity, from which rise parallel to each other two relatively long capillary tubes, also of platinum. One of the tubes passes down into the interior of the sphere, almost touching the opposite inner surface, while the other only just pierces the envelope. Both are bent at right angles at their upper extremities, in opposite directions. In order to eliminate all errors due to the capillary tubes a compensator is also employed, consisting of a long capillary U-tube of the same bore and bent at right angles at the extremities, so as to form an exact counterpart of the capillary portion of the air thermometer. The small tube containing the substance is firmly fixed by means of stout platinum wire so that its lower portion is in close contact with the sphere; the walls of the tube are of the same thickness as those of the sphere. The salt employed for the purposes of a bath is contained in a capacious platinum crucible, supported over a table furnace in a miniature basket of platinum gauze. One of the capillary tubes of the air thermometer is ready to be connected with an apparatus for generating pure carbon dioxide, and the other is attached to a gas-measuring burette similar to the well-known Schiff nitrogen apparatus, but somewhat narrower, and surrounded by the outer tube of a Liebig's condenser, through which a stream of cold water is continually passed. This arrangement enables the air to be collected and measured in the proximity of the furnace. The measuring burette is filled with a concentrated solution of caustic potash. The temperature of the water-jacket is measured by a thermometer immersed in a small accessory reservoir, through which the water passes immediately after leaving the jacket. A very simple device has been adopted for determining the exact moment when fusion occurs. Before the experiment the little test-tube is heated until the substance melts; a fine platinum wire, furnished with a thickened end, is then inserted in it, and allowed to become fixed by the solidification of the substance. The fine wire is then passed over a pulley some distance overhead, and the free depending end is attached to a weight; just below the weight a bell is hung.

When everything is ready for the actual operation of determining a melting point, the salt in the crucible is fused, the lower part of the air thermometer and its attached substance-tube are inserted in the bath of liquid, as is likewise the compensator, connection with the measuring burette is made, and the carbon dioxide apparatus is arranged to be delivering the pure gas. When the temperature of the bath at length attains that of the melting point of the substance, the portion of the latter in immediate contact with the walls of the platinum tube fuses, and instantly the wire is released, and the weight falls and strikes the bell. The moment the sound is heard, connection with the carbon dioxide apparatus is established, and the air contained in the thermometer is displaced and driven into the measuring burette. The compensator is similarly treated, and the quantity of air which it contained deducted from that contained in the thermometer. From the resulting volume, together with the knowledge previously obtained concerning the capacity of the thermometer and compensator and the known expansion of air, the melting point is obtained by a very simple calculation.

Four groups of interesting results have already been obtained by use of the new method, indicating the dependence of the melting point upon atomic weight. They are as follows:

Salt.	Melting point.	Salt.	Melting point.
Potassium chloride	800°·0	Potassium iodide	684·7
Potassium bromide	722·0	Rubidium iodide	641·5
Potassium iodide	684·7	Cæsium iodide	621·0
Sodium chloride	815·4	Calcium chloride	806·4
Sodium bromide	757·7	Strontium chloride	832·0
Sodium iodide	661·4	Barium chloride	921·8

It will be observed that in the halogen salts of both sodium and potassium a diminution of melting point accompanies a rise in the atomic weight of the halogen; also that a lowering of the melting point accompanies a rise in the weight of the metallic atom in the case of the iodides of the alkali metals potassium, rubidium, and cæsium, while the reverse occurs with respect to the chlorides of the alkaline earthy metals calcium, strontium, and barium. Whether there is rise or fall of the melting point with ascending atomic weight, however, the salt of intermediate molecular weight invariably exhibits an intermediate melting point.

A. E. TUTTON.

SCIENCE IN THE MAGAZINES.

THERE are very few articles on purely scientific subjects in the magazines received by us this month. Apparently the magazine-reading public thinks a scientific pabulum unsuitable for Christmas reading; or is it that men of science are too deeply engrossed in their researches to cultivate the art of writing interestingly upon the wonders of nature? Literary men frequently play fast and loose with natural phenomena and laws, and are often pilloried for doing so; but, on the other hand, many men of science do not pay due regard to the literary polish which is essential to an attractive style.

The first number of the *Fortnightly* under the new editor, Mr. W. L. Courtenay, contains two articles of interest to our readers, one on "A True University of London," by Mr. Montague Crackenthorpe, and the other on "The Spread of Diphtheria," by Dr. Robson Roose. Mr. Crackenthorpe deals broadly with the whole question of the expediency of establishing in London a University which shall teach as well as examine. He defines the work of a true metropolitan University as follows: (1) To do the work of the higher teaching by its own professorial staff, and to superintend and aid its being done by other educational agencies in the metropolis. (2) To examine and to grant degrees, but to grant them as a mark of success in regular and systematic courses of study, rather than in the display of hastily acquired, and, therefore, ill-digested knowledge. (3) To stimulate scholarly and scientific research by means of well-equipped libraries, laboratories, and other like apparatus, and by the institution of public lectures of an advanced character, like those of the Sorbonne and the College de France. The scheme drawn up by the Gresham Commissioners satisfies most of these requirements, and a *deus ex machina* in the shape of a Statutory Commission is all that is wanted to establish it.

So much attention has recently been given to the new treatment of diphtheria, that Dr. Roose's sketch of the history of the complaint, and the circumstances which tend to promote its spread, comes very opportunely. His description of the measures calculated to check the prevalence of the disease, and of the remedy lately introduced, is clear and concise, whilst the following statement, though commonly known in the scientific world, will remove the misapprehension that exists in the minds of a large section of the general public:—"Löffler and Klebs discovered the microbe of diphtheria, and studied its life-history; Roux and Yersin demonstrated that the bacillus was capable of evolving toxic material, and Behring crowned the edifice by discovering the antidote."

An address by Prof. G. W. Prothero, on "Why should we learn History?" contained in the *National*, would at first hardly seem to be a subject for comment in these columns. There is, however, much in the address worth noticing here, for Prof. Prothero shows that history, if not strictly speaking a science, may be taught in a scientific way. Let us briefly state his argument. There are many gaps in history, but in every science there is a lack of information on certain points. Even astronomy, the most exact of the sciences, has its dark spots, and there are shady places in evolutionary biology. Thus, so far as imperfection of knowledge goes, history and science only differ in degree. A greater difficulty, perhaps, is that the historian cannot employ experiments either to discover facts or to test observations; but here again it suffers in company with geology and other branches of natural knowledge concerned with the past. History is therefore not disqualified from being a science because it is not experimental. The infinite variety and extent of historical phenomena, and the presence of the human element are, however, "obstacles which, it must be allowed, check history on the threshold of science. If indeed the term science is to be restricted to the knowledge and application of general laws—if that alone is science which can foretell with certainty the occurrence of certain results—if science deals with no phenomena but such as can be exactly weighed and measured—then history is not a science at all. But this is surely to restrict science within too narrow limits. All sciences are not equally exact or equally capable of generalisation. . . . There is, in fact, a regular gradation from the sciences of abstract reason and mathematical formulæ, through the phenomena of the inanimate and the animate world to the world of man." But, *pace* Prof. Prothero, if history be granted a place among the sciences, it must be scientific in the ascertainment of its facts. Take the Black Death as an illustration. The vague and exaggerated statements of certain chroniclers of its ravages may be taken as evidence, or the more laborious process of searching the registers of the time may be explored. The difference is that one of the ways is scientific, while the other is unscientific. In the drawing of conclusions, also, "there is the same distinction between scientific and unscientific work as there is in the ascertainment of historic facts. For instance, Buckle, in illustrating his theory that national character depends largely upon food, attributes the weakness of the Hindoos to an almost exclusive diet of rice. A striking but misleading generalisation, for, as Sir H. Maine has pointed out, the great majority of the Hindoos never eat rice at all. . . . There is, then, a scientific way as there is an unscientific way of studying history. If treated one way, its results are guess-work and delusion; if treated another way, if industry, reason, and sober judgment are brought to bear, its results are in many cases matter of certainty, in many others matter of at least high probability. And, if we except the science of mathematics, what more can be said of any science?" The main object of Prof. Prothero's address was to show that historical study exerts considerable influence upon the mind and character. This is certainly the case, and if the student is trained on the lines indicated in the foregoing, intellectual results of the highest order must follow.

The great landslip which caused the formation of the Gohna Lake, Gurhwal, in the central Himalayas, has led Mr. W. M. Conway to write on "Mountain Falls" in the *Contemporary*. This catastrophe, however, is only used as a peg upon which to hang an account of the great Alpine landslip which buried part of the village of Elm, in Canton Clarus, thirteen years ago. The *Contemporary* also contains a metaphysical article by Emma Marie Caillard, in which cosmic and ethical processes are discussed, and Prof. Huxley's opinions on evolution and ethics are criticised. Prof. Huxley is also involved in a paper on religion and science, contributed by A. J. Du Bois to the *Century*. Another article for abstract philosophers is Prof. Seth's second paper on "A New Theory of the Absolute."

Sir Robert Ball contributes to *Good Words* another of his interesting—albeit superficial—papers on great astronomers, the subject being Sir John Herschel. *Chambers's Journal* contains the usual complement of chatty articles, among which we notice one on smoke absorption, descriptive of Colonel Dulier's patent system of removing the soot and sulphurous acid from the products of combustion, by treatment, before passage into the chimney, by both steam and water; and another on remarkable hailstorms.

A passing reference will suffice for the remaining papers on scientific topics. The result of prematurely releasing a chrysalis from its cocoon, a subject on which we published a letter by Dr. L. C. Jones in our issue of November 22 (p. 79), serves Mr. W. C. Wilkinson as the theme of a poem in the *Century*. St. George Mivart writes popularly on "Heredity" in the *Humanitarian*, his paper dealing chiefly with Prof. Weismann's speculations. A posthumous paper of Richard Jefferies' appears in *Longman's Magazine*, and Mr. Phil Robinson contributes a facetious paper on rattlesnakes to the *English Illustrated*. In addition to the magazines and reviews named in the foregoing, we have received *Scribner*, *Cassell's*, and the *Sunday Magazine*; but these do not contain any articles that can be commented upon here.

OYSTER CULTURE ON THE WEST COAST OF FRANCE.[1]

At the request of the Lancashire Sea-Fisheries Committee, I spent some time, last June and July, in investigating the various methods of shell-fish culture in use along the western coast of France from Arcachon in the south to Brittany in the north. There can be no doubt that there are extensive and flourishing shell-fish industries along the French coast, and one is struck very forcibly with the admirable manner in which the people seem to make the best of unfavourable conditions, and to take full advantage of any opportunity given to them by nature. Few places on any coast could look more desolate and forbidding than the vast mud swamps of the Bay of Aiguillon, and yet by means of the "bouchot" system many square miles of this useless ground have been brought under cultivation, and an industry established which supports several villages. Then again, the neat little enclosures along the beach at many places, carefully tended by the owners at low tide, remind one constantly of market gardening, and enforce the truth of the idea, long familiar to the biologist, and now beginning to be more generally recognised, that the fisherman should be the farmer, not the mere hunter of his fish, and that aqui-culture must be carried on as industriously and scientifically as agriculture.

In addition to industry and care on the part of the fisher-folk, women as well as men, the success of the shell-fish industries in France is largely due to the encouragement and wise assistance of the Government, especially in the regulation of general oyster-dredging and the reservation of certain grounds for supplying seed. The practical question—and one of enormous importance—is: Is there anything special in the conditions in France, either of the land or of the water, which would render their methods inapplicable to our more northern shores? I do not believe that the question can be satisfactorily and finally answered until some experimental cultures on a fairly large scale have been tried; but a consideration of the details and results of the French methods will at least give us some idea of which experiments are worth trying, and of the localities which might be cultivated with most prospect of success.

[1] Abstract of a report, by Prof. W. A. Herdman, F.R.S., to the Lancashire Sea-Fisheries Committee.

The leading characteristics of French oyster culture are (1) that the whole is under the regulation and supervision of the State, concessions of ground being given to individuals or companies (*e.g.* at about 30 francs the hectare at Arcachon) for the purpose of forming oyster "parcs"; (2) that certain grounds are set aside or preserved as banks of breeding oysters to supply the spat; and (3) that the whole process of raising the spat, and rearing and fattening the oyster, is not carried on at one locality, but is subdivided and specialised, spat-production taking place best at one locality, such as Arcachon, and fattening for market at another, such as Marennes.

Two species of oyster are cultivated, *Ostrea edulis*, the ordinary rounded flat oyster of north-west Europe; and *O. angulata*, the elongated Portuguese oyster. The latter, although increasing in numbers in some places, and becoming of considerable commercial importance, is not so highly thought of as is *Ostrea edulis*.

There are now only two places on the coast of France where spat is produced in sufficient quantity to be of commercial importance. These are Auray, in the north, and Arcachon, in the south, and these two localities supply all the other oyster culture centres in France, and even export to other countries. One merchant I met at Arcachon told me that he had already sent eleven millions of oysters to London that season.[1] I visited in all about ten different oyster-culture centres; but as several of these showed nothing special, they need not be mentioned. The arrangements for the capture and rearing of spat were best seen at Arcachon, and for the further rearing and the fattening of the adults at La Tremblade and Marennes on the estuary of the Seudre. At Arcachon I was fortunate in enjoying the hospitality of the excellent Biological Station, which was established nearly thirty years ago by La Société Scientifique d'Arcachon, and which was made use of by Paul Bert in 1867 for his observations on the physiology of marine animals.

Arcachon presents remarkable facilities for the study of shallow-water marine forms, and is of great interest to the biologist, as well as to the ostreiculturist. Its vast inland sea (Bassin d'Arcachon), which is about 80 kilom. in circumference, contains at high tide about 15,000 hectares of area, and is, over the greater number of the channels, about 5 to 10 fathoms in depth, while two-thirds of the whole area dries at low tide. In the middle of the "bassin," and north of the town of Arcachon, is a small island, Ile des Oiseaux, and on the shores of this, and on various other flat shallow parts which are exposed at low tide, and which are called "crassats," are situated the oyster "parcs." Some of these areas are reserved by the State for the purpose of producing spat, a wise precaution, although some of the parqueurs think the State reservations unnecessary, as there are so many adult oysters over the ground that plenty of spat is sure to be produced. Certainly during the time of my visit, which was just when the free-swimming embryos were settling down, the water over the parcs seemed to be swarming with them, and the spat was making its appearance over all sorts of suitable submerged objects. The summer of 1893 was, however, a particularly good season, which the parqueurs attributed to the great heat. Probably calm weather and absence of rain during the critical period when the young oysters are free-swimming, and then settling down, has as much to do with a heavy fall of spat as the actual temperature.

The oyster reproduces at Arcachon between May and the beginning of July, and the young animal leads a free swimming existence for about a week before settling down as spat. The parqueurs examine carefully the condition of the spawn in the old oyster, and at what they consider to be the proper time (generally about the end of June) for catching the deposit of spat, they place their "collectors" in position. It is of importance that the collectors should not be put in the water too soon, as they are liable to become coated with slime and sediment, which will prevent the young oyster spat ("naissain") from adhering. The collectors are crates ("gabarets" or "ruches") of earthenware tiles, coated with a limy cement which gives them a whitewashed appearance. The tiles are like ordinary roofing tiles, about fourteen inches long by six inches at one end and five at the other, and half an inch in thickness. The cement with which they are coated is made of lime mixed with sea-water and a certain amount of sand, so as to form a creamy paste. Different proprietors use slightly different proportions of lime and sand. The process of coating ("chaulage") adds from one-sixteenth to one-eighth inch in thickness to each side of the tile. It has to be done with some care, so that the limy layer may be of the right nature, sufficiently strong and adhesive, and yet readily detachable when the time for "détroquage" comes, so that the young oysters may be removed from the tiles without injury, and without the necessity of breaking up the tiles, as used to be the case. By the present method the little oysters and film of cement can be flicked off rapidly by a skilled hand with a square-ended knife, and the tiles preserved for use again the following year. A dozen or more millions of these tiles are probably employed each year at Arcachon. The prepared tiles are arranged in alternating rows lengthways and breadthways inside cases made of strips of wood, so that the water may flow in readily between and around them. The cases of collectors I measured were about 6 feet × 2 feet and 3 feet in height, each holding 120 tiles arranged in ten layers. The alternating arrangement of the layers is intended to break up the currents of water as the tide runs through the "ruche," form eddies, and so give the young oysters a better opportunity of affixing themselves to the tiles. The tiles are all placed with the convex surface upwards, as it is very important that there should be as little opportunity as possible given for the collection of any fine sediment in which the young spat might be smothered. The arrangement of tiles above described is now considered the best at Arcachon. Various other arrangements have been tried, and may be suited to special conditions of bottom or depths of water.

I was very fortunate in seeing some of the tiles just after the young oyster spat had been deposited, and photographed such a tile covered thickly with the minute amber-coloured specks. There may be several hundred such young oysters on one side of a tile. During my stay at Arcachon I found that the temperature of the water varied from 74° F. to over 80° F., and the specific gravity from 1·022 to 1·024. However, it is known that no such high temperatures are really required for spat production, since, *e.g.*, Captain Dannevig has had an abundant deposit of spat in his pond near Arendal in Norway, where the July temperature of the water was only 63° F. To compare these with our own district, we find that in the same week of July 1893 the water off the south end of the Isle of Man was on the average about 60° F. with a specific gravity ranging from 1·025 to 1·026, while shore pools near the Biological Station at Port Erin, comparable as to exposure with the oyster parcs at Arcachon, reached as high a temperature as 76° F. Dr. Bashford Dean[1] and other authorities state as their opinion, that a low specific gravity is necessary for a good deposit of spat, but there is no unanimity on this point amongst the practical men at Arcachon (some of whom are keen observers, and are in the habit of taking the temperature and specific gravity).

After removal from the tiles, those young oysters which are not sold to "éleveurs" away from Arcachon are kept for another year or two in the parcs. They are placed at first in flat trays having a floor and lid of close galvanised wire netting, of about half-inch mesh, and these trays ("ambulances" or "caisses ostreophiles") are placed between short posts in the water on the oyster parc, so that the tide can run freely through them, supplying the oysters with food and oxygen. They measure about 6 feet by 4, and are 6 inches deep. They serve to keep the young oyster, during the early period of its life, out of the sediment, and they also protect it from its numerous natural enemies, such as the starfishes and crabs, which manage to suck or pick out the soft body, and whelks, such as *Purpura*, *Murex* and *Nassa*, which can bore a hole through the shell. The ambulances are constantly looked after by the oyster men and women, who come at low tide, when they are exposed, open the lids, and pick over the contents, removing enemies and impurities which may have got in, taking out any dead oysters, and rearranging those that are left, so that all may have a fair chance of obtaining food and growing normally. The young oysters grow rapidly in the ambulances, and soon have to be thinned out. The larger ones are removed, and if large enough are thrown into the open areas of the parc. In this way, by

[1] Two-year olds, measuring 5 to 6 cm. across, cost at the rate of 12 francs per 1000, and somewhat older ones, measuring 6 to 7 cm., 25 francs per 1000. These prices include packing and carriage as far as Bordeaux, where they meet the steamer. On an average only 1 per cent. die on the journey. From the middle of March to the middle of April is the best time to send young oysters for stocking purposes to England. Before that it is liable to be too cold in England, and later it is too hot in Arcachon for transportation to be effected safely.

[1] Various important papers in Bulletin U.S. Fish Commission.

thinning out, rearranging, and adding fresh supplies, relays of young oysters in their first year may occupy the ambulances for eight months, although an individual oyster may only be in for a month or so.

Eventually all the oysters not sold to éleveurs get transferred from the ambulances to the open rectangular areas, like little fields, which make up the rest of the parc. The low banks bounding these areas are formed of two parallel rows of close-set vertical bunches of the local heath, *Erica scoparia*, with the space between, a foot or more wide, filled in with masses of a tenacious clay obtained from the Ile des Oiseaux. Planks of wood and stakes, to strengthen the boundary, are also used in places, and at one corner a sluice is formed, so that the water at low tide may either be retained to a depth of 6 or 8 inches, or allowed to run off as required. About one million oysters can be accommodated in each little field, which is about at the rate of 125 to the square metre. Going thoroughly over a parc, partly in a boat and partly by wading, gives one an excellent idea of the extensive and profitable system of aquiculture practised at Arcachon.

Between neighbouring oyster parcs, and surrounding the "concessions" of the various proprietors, run lanes of water about 4 metres wide. These give ready access to the parcs, and can be traversed by the long gondola-like boats of the parqueurs. The lanes are bordered by rows of tall saplings with bunches of twigs left on. These are called "pignons." They keep waving in any slight breeze, and give a characteristic appearance to the scene. The oyster men declare that they are of use in frightening away fish, and especially the voracious ray *Myliobatis*, which might otherwise do great damage in the preserves. Possible depredations of another kind are guarded against by the "pontons," or large barges, moored at the corners of the parcs in which the "gardes des pêches" live.

Great numbers of the oysters bred and reared through their early stages at Arcachon are sent to Marennes and La Tremblade, in the flat district on both sides of the estuary of the Seudre, to be fattened in a *parc d'élevage*, and "greened" by feeding upon the diatom *Navicula fusiformis*, var. *ostrearia*. Wide canals from the estuary lead the sea-water inland, and supply the numerous "claires," which are merely shallow artificial ponds excavated in the clay and marly soil. In spring and early summer the muddy floor of the claire undergoes a good deal of preparation by digging, cleaning, draining, and exposure to sun and air, in order that later on, when sea-water is readmitted, at first in small quantity, it may be in what has been found by experience the most favourable condition for the growth of the desired kinds of lower algæ. These soon cover the floor with a dense green growth, which the éleveurs recognise as being of great importance to the nutrition of the oysters. Samples of the growth which I collected from the bottoms of several claires consisted of *Cladophora flavescens* and *C. expansa*, along with *Spirulina tenuissima* and a *Lyngbya* and little tufts of *Calothrix*, while a more detailed examination with the microscope shows that these plants are teeming with small animals and other forms of life, and nearly everything is covered with innumerable diatoms. Probably the larger green algæ, thought so much of by the éleveurs, are only of importance in oyster culture in providing points of attachment and shelter or favourable environment for the microscopic forms of life, and especially for the diatoms. It is well known that diatoms form a most important constituent of the food of oysters, and that the greenish blue tint of the celebrated Marennes oysters is due to the presence in the claires of enormous quantities of *Navicula fusiformis*, var. *ostrearia*, upon which the oysters feed. This form is found in our own fishery district in the estuary of the Dee (and probably elsewhere), although not abundantly; but it is probable that there are various other allied diatoms that would do equally well for rearing and fattening oysters on, and as a matter of fact the examination of the contents of an oyster's stomach shows that the food consists of a number of different kinds of diatoms as well as other minute organisms.

Altogether, all the evidence I was able to collect shows, I think, that the bottom of a claire is teeming with microscopic life, and that it is probably this rich feeding *alone* which is necessary in order to bring the oysters, in a very short period—a few weeks usually, sometimes ten days or a fortnight is sufficient—to the desired condition of fatness and flavour. The autumn and early winter months are said to be the best for fattening and greening.

I shall have to omit all reference to the industries at Pointe le Chapus, at the Island of Oléron, at La Rochelle, at Les Sables d'Olonne, and at Le Croisic—except a brief explanation of the basins of "dégorgement" seen at Le Chapus and elsewhere. These are shallow tanks, high up on the beach, with smooth bricked or tiled floors, so that they can be kept clean and free from mud. Their purpose is to enable the oysters taken fresh from the parcs and claires, and which naturally have some fine mud and food-matters of a decomposable nature clinging to them, both externally and internally, to lie for a few days in clean water, and so get rid of their impure mud and excreta before being packed up and sent off on a journey. The oysters also become accustomed in these basins, which can be emptied and filled with water periodically, to close their shells and stand prolonged exposure to air.

I do not see that the French shores are, in any important respects better fitted for shell-fish culture than some parts of our own Lancashire and Cheshire coast.[1] The deposits, both littoral and submarine, are, on the whole, much the same, the fauna, both macroscopic and microscopic, is scarcely appreciably richer, and although the temperature of the water is decidedly higher in the south—probably on the average about 10° F. higher in summer—I do not think that that is an essential condition, *so long as the winter temperature of our water does not get too low*. It would certainly be necessary, I think, to keep our oysters completely submerged during the winter months; but there are several places in the estuaries of the Dee and the Ribble, and in the Barrow Channel near Roe and Peil Islands, where "littoral" cultivation in summer might be combined with "bedding out" in winter—somewhat as is done at present with American oysters in the estuary of the Wyre, near Fleetwood. As to the other conditions—of bottom, of water, and of food, several places in the Barrow Channel and in the Dee estuary seem to me to be well fitted for oyster culture.

ENDOWMENT FOR SCIENTIFIC RESEARCH AND PUBLICATION.[2]

I.

TWENTY years ago Prof. Tyndall delivered in New York and in other cities of this country a series of lectures upon light. The last of the series was an impressive plea for a more thorough prosecution of original research in pure science; and incidentally, for the need of endowments to maintain it. I was fortunate in having the opportunity to listen to that remarkable course of lectures, and to that plea for science. Its impression has never left me. The impression was the deeper, because Tyndall set upon it the seal of self-denial. Some 30,000 dols., nearly the entire net proceeds of his lectures in the United States—money for which he undoubtedly had abundant use in his own affairs, or at least in the prosecution of researches in his own country, and which by all precedent and the example of other lecturers he would have taken with him—this he has given to the science of this country, endowing therewith, in 1885, three scholarships for the prosecution of original research in physics, one under the direction of Columbia College, one under Harvard, and a third at the University of Pennsylvania.

The truths uttered and the example set by this self-denying master have already many times borne fruit. The late President Barnard, of Columbia College, who was a warm supporter of Prof. Tyndall when here, bequeathed to Columbia upon his decease a few years since the sum of 10,000 dols. for the endowment of another fellowship for the encouragement of scientific research, upon substantially the same terms as those of the Tyndall scholarships. In other parts of the country there have been some other endowments for similar purposes. In the last year Columbia has also received 100,000 dols., the munificent bequest of Mr. Da Costa, for the establishment of the department of biology. Although this bequest is not primarily for the prosecution of original research, it is not restricted by hampering conditions, and will to some extent, it is hoped, admit of a direct and continuous support of the highest and most advanced studies.

The appeal made by Tyndall has been often renewed by

[1] This, being a report to the Lancashire Sea-Fisheries Committee, is only concerned with localities within the Fishery District.

[2] Address delivered by Mr. Addison Brown, at a meeting of the Scientific Alliance of New York. (Reprinted from Smithsonian Report, 1892.)

scientific men; by the heads of universities; by the presidents of scientific associations, here and abroad; and by none, perhaps, more eloquently than by Dr. Edwin Ray Lankester, in his address before the biological section of the British Association at Southport, in 1883.

What shall we say to the call and the examples of such men? Was the gift of Tyndall based only upon an idle fancy? Or was it the result of a clear perception of a profound truth, viz. America's need of that money as a stimulus and support to more scientific research; the call on him being felt to be the more imperious, because the need of it was so plain to him, while obscure to others; and making his act, therefore, a noble instance of self-renunciation in an unappreciated cause?

"To keep society as regards science in healthy play," he says, "three classes of workers are necessary:

"(1) The investigator of natural truth, whose vocation it is to pursue that truth and extend the field of discovery for truth's own sake, without reference to practical ends.

"(2) The teacher, to diffuse this knowledge. . . .

"(3) The applier of these principles and truths to make them available to the needs, the comforts, or the luxuries of life. . . .

"These three classes ought to coexist and interact. The popular notions of science . . . often relate, not to science strictly so-called, but to the application of science."

The great discoveries of scientific truth, he continues, are "not made by practical men, and they never will be made by them; because their minds are beset by ideas which, though of the highest value in one point of view, are not those which stimulate the original discoverer."

In a chance conversation, a few weeks since, I received a confirmation of these words, so direct and unexpected, that it may bear citation. I was talking with an electrical expert who had made several very interesting and important inventions. I asked him of how much importance he conceived that the scientific men of the closet, the original investigators, so-called, had been in working out the great inventions of electricity during the last fifty years—the telegraph cables, telephones, the electric lighting, and the electric motors; and whether these achievements were not in reality due, mainly, to the practical men, the inventors, who knew what they were after, rather than to the men of science, who rarely applied their work to practical use?

"Not at all," he said, "the scientific men are of the utmost importance; everything that has been done has proceeded upon the basis of what they have previously discovered, and upon the principles and laws which they have laid down. Now-a-days we never work at random. Look at that electric light! Of the energy expended in producing it, only 7 per cent. appears as light; the rest, 93 per cent. is wasted, mainly in heat. We are all now trying to prevent this enormous waste. I want to reverse that proportion; but if I can reduce the waste to only 33 per cent. a patent of my invention will be worth millions of dollars for its economy in production. In seeking this we do not work at random. I go to my laboratory; study the applications of the principles, facts, and laws which the great scientists like Faraday, Thompson, and Maxwell have worked out, and endeavour to find such devices as shall secure my aim."

This is but an expression, in another form, of what Tyndall said twenty years ago: "Behind all our practical applications there is a region of intellectual action to which practical men have rarely contributed, but from which they draw all their supplies. Cut them off from that region, and they become eventually helpless."

What is true in one department of natural science is, I apprehend, equally true in all. The practical men do not work at random, but upon the basis of what scientific research and publication have previously put within their grasp.

It is evident, therefore, that not only the advancement of knowledge itself, but all possibility of any continuous advance in those great improvements which are to mitigate the sorrows, and promote the health, the conveniences, and the comforts of men, is vitally dependent upon the progress of scientific research. In recent years how marvellous have these improvements been! Besides those that are most common and familiar to all, what miracles, almost, have been achieved through the photograph, the spectroscope, the microscope; by the discovery of the sources of fermentation and of putrefaction; by the discovery of anæsthetics and the application of antiseptic methods in surgery, and in the treatment of other lesions! These latter discoveries alone have ameliorated beyond expression the sufferings of man; they save more lives than war and pestilence destroy, surpassing even in that regard the safety lamp of Sir Humphrey Davy—an invention which at the time it was made, was said to have exceeded every previous discovery as a means of saving human life, except, possibly, inoculation for smallpox.

This vital relation between the advancement of knowledge and the welfare of man furnishes an all-sufficient reason for the continuous and never-ending prosecution of original research. Of necessity the original work of discovery must always lead; that must always precede the practical applications. The necessity for such research must therefore continue so long as science and human society endure. As there is no limit to the advance of knowledge, so there can be no limit to the benefactions it is capable of conferring upon mankind. The more rapid the advance, the more speedy the enjoyment of its fruits. In this relation alone the need of ample provision for scientific progress is one that addresses itself equally to the nation, to the State, to philanthropists, and to all who would advance the welfare of man on the broadest and most enduring lines.

How shall such research be maintained and extended? The investigator of pure science does not work for profit. His discoveries are not marketable. The law allows no patent upon a principle of nature or the discovery of a new truth. Newton could not patent the law of gravitation, nor Volta the galvanism of the voltaic pile; nor Ehrenberg and Schwann the discovery of the widespread influence of bacteria; nor Faraday, nor Henry, electro-magnetism; nor Joule, his correlation of forces; nor Jackson, his anæsthetics; nor Lister, his antiseptic treatment; nor Koch, nor Pasteur, their discoveries of the bacilli, the destruction of which may lead to the cure or amelioration of terrible diseases. To the practical men and to the inventors, on the other hand, who apply to the specific wants of men the truths and principles which the men of science have made known to them, the law, in the form of a patent, gives a monopoly of from fourteen to twenty-one years. They thus obtain, as a rule, a reasonable, and, in some cases, even an excessive, pecuniary reward. In this country alone nearly 500,000 patents have been issued; they are increasing at the rate of about 25,000 per year. In the extreme multiplication of patents affecting a large part of everything we use, the whole world, it might almost be said, is paying tribute to the inventors and practical men; while to the original discoverers, who have made so much of all this possible, there is no promise of pecuniary reward.

This is not said by way of complaint. In the nature of things it is scarcely avoidable. The aims, the motives, the methods, and the genius of the two classes of minds are and ever must be widely distinct. Original discoverers cannot be turned aside from their special work to become mechanics and inventors without infinite loss. Prof. Henry had one form of the electric telegraph in actual use some years before Morse conceived it.[1] But how great would have been the loss to science, without any corresponding gain, had Prof. Henry in 1830 turned away from pure science to do the subsequent work of Morse in adapting the telegraph to common and valuable use!

Research in pure science can never be made a self-supporting pursuit. It can never, therefore, be carried forward broadly, and continuously, and effectively, except through men sustained by some form of stipend or endowment. Occasionally, it is true, men of independent fortune, like Harvey, and Darwin, and Lyell, and Agassiz, have devoted themselves to original research upon their own means, and have accomplished most important results. But these instances are rare. Many other persons, too, with aptitudes and tastes for research, though not following a scientific career, have carried on private researches in the intervals of leisure stolen from the exacting demands of professional or business life; and these have, in the aggregate, added no small amount to the common stock of knowledge.

It is no disparagement, however, of these subordinate workers to say that nearly all the great discoveries, and nearly all the great advances along the lines of knowledge, have been achieved by men who in the main have devoted their lives to the work, and have been supported through institutions or endowments which made this devotion possible. Government appointments, professorial chairs, or salaried positions in scientific institutions of some kind, have been and must continue to be our chief dependence. And it is manifest that these can only be maintained by Government aid, or by the bounty of private in-

1 "Smithsonian Report," 1878, pp. 159, 262

dividuals. The former is mainly the European system; the latter, in the main, is ours. There, universities are founded by the Government; here, chiefly by the people.

In Germany there are twenty-one universities maintained by the Government. In each of these, as Dr. Lankester states, there are five independent establishments in the department of biology alone, viz. in physiology, anatomy, pathology, zoology, and botany. At the head of each of these establishments there is a professor, with two paid assistants, making altogether about 300 for biological research in Germany; and he estimates about one-quarter of that number in the same department in England. In all the sciences, therefore, there would probably be found in Germany from 800 to 1000 persons of high scientific attainments, supported by the Government in the universities, who are regularly and systematically engaged in the discovery of new scientific truth. For it is there made both the object and the duty of the professors of natural science to carry on original investigations by work in the laboratory. Their positions are obtained through previous distinction in such investigations, and it is for this work that their small but fixed stipend is paid by the Government.

In the College de France, also maintained by the Government, there is the same requirement, though with a larger salary to the professors, and with the added duty imposed on them to deliver to the students about forty lectures yearly upon the subjects of the professors' researches; while in Germany the professors also receive from each student who attends their lectures, a moderate fee, which serves to increase their meagre stipend, as well as to stimulate their activity and usefulness. Under this system, Germany has become the greatest school of science, and the resort of the whole world.

In this country the opposite system prevails. The colleges and universities are mainly private foundations, dependent on private gifts and endowments. The colleges are unwisely multiplied. All are more or less cramped for money. This limits the number of professors and assistants appointed for instruction, and crowds them with routine work. The result is that in all but a few colleges, and in these until comparatively recently, the duties of instruction have left to the professors but little time or opportunity for the prosecution of original investigations; and these with but poor equipment and inadequate means.

In not one of all our colleges and universities, so far as I have been able to ascertain, is there a single professorship endowed or founded, even in part, for the avowed object of original scientific research. Instruction, not discovery, is the only avowed object. It is to the great credit of American professors and teachers that, with so much routine work on their hands, and so little leisure for research, they should have accomplished by purely voluntary studies so much as is shown in their contributions to our scientific publications.

To what is said above, perhaps a virtual exception should be made as respects our astronomical observatories, in which, the labours of instruction being less, original work has been perhaps expected, and has been accomplished with most signal success. To some extent this may possibly apply to our medical schools also. And in other departments, generally, wherever time and opportunity have been afforded, much original work has been done by our professors; some of it of the first class. This is attested, not to mention living instances, by the work of Prof. Henry at Princeton, Dr. Torrey at Columbia, Dr. Silliman at Yale, Dr. Gray at Harvard, and many others that might be named. In a number of the States, also, and at Washington, there have been maintained by the State or nation a number of scientific men, in connection with certain State or national interests, who have accomplished most important results; of these, Dr. James Hall, of this State, is a conspicuous instance. At Harvard and at other colleges some noble opportunities for special study have been also provided in their scientific schools and museums; notably in the zoological museum, the Jefferson Physical Laboratory, and the Peabody Museum of Archæology at Cambridge, and also in the department of hygiene at the University of Pennsylvania. But in most of these the great complaint is the lack of necessary endowments to make possible the active advanced work in original discovery for which those institutions are designed. In the Peabody Museum there was in 1891 a gift of 10,000 dols. by Mrs. Hemenway to establish a post-graduate fellowship; and also a gift of like amount by Mr. Wolcott, for the general support of the museum's work. New York also has within a few years past seen spring up almost as by magic, through the efforts of a single leading spirit, seconded by other public-spirited men and women, and by municipal aid, a museum of natural history that bids fair to stand in the front rank of scientific opportunities; but the endowments of fellowships and professors necessary to make its opportunities available in active research are as yet wanting.

England holds a position midway between the United States and Germany. Her scientific men lament her deficiencies. They are striving to increase their means for scientific work, and are doing so yearly.

If experience teaches anything, it is that no broad and general development of scientific work of the first class is possible, except either through independent establishments for special work, or else by the university system, in which professors in science and their assistants are first selected on account of their previous distinction in original research, and are then appointed to continue that work, and in the teaching of students, to transmit to them the zeal of discovery and the true methods of advance.

It matters little whether the support of the university or of special institutions for research comes from the Government or from private endowment, provided the provision is adequate and constant. The difficulty with us has been, and still is, that funds are insufficient, the means and equipment inadequate, and the time allowed to the professors for research insufficient. There has been too much of the schoolmaster, and too little of the real professor. Too great absorption of the professor's time in the work of instruction is injurious to both teacher and pupil. The most stimulating of teachers is he who by daily experiment is in vital touch with nature—he who brings from the fires of the laboratory the warmth, the illumination, and the inspiration of his own researches.

This is now well recognised; and so far as their means will permit, the leading colleges are by degrees relieving their professors of the work of elementary instruction, so that they may the better prosecute original researches, and at the same time become best qualified for the highest work of instruction. This system will doubtless demand watchfulness and discrimination. To prevent abuses, regulation and responsibility may have to be imposed. But it involves the appointment of additional instructors. It requires added means. And this is indispensable as a part of the transition of our leading colleges to the university system. It is indispensable, also, if we are to have in this country any considerable systematic prosecution of original research. We must use existing instrumentalities and existing institutions. And all experience shows that outside of the few Government positions, and in the absence of special institutions for research, the professorial chairs are best adapted to such investigations. No greater service could be done to science than to make such endowments as should insure systematic and continuous research by the professors as a part of the new university system.

Endowments for the same object, and operating in the same line, might also take a different form, viz. the endowment of several professorial fellowships, each, say, of 1000 dols. annual income; to be controlled and awarded by some independent scientific body (such as this alliance might afford) for distinction in active scientific investigations, either within the country or within the State. I know of no more quickening impulse to original scientific research than such as would be given to it by those means.

How backward we have been in this country, through the lack of proper endowments, in making use of the best existing opportunities for research, may be illustrated by a single instance. Some twenty years ago a school was established at Naples for the prosecution of marine biological research. It is most thoroughly equipped, and, being a general resort, is the most advantageous for study in the world. It is maintained by a charge of 500 dols. per year upon each table occupied, each occupant being entitled to all the advantages of the institution. Of these tables, the German States for several years have taken thirteen; Italy, eight; Austria, Russia, Spain, and England, each three; Switzerland, Belgium and Holland, each one; the United States, until 1891, none, except one table supported by Williams College for two years, and one by the University of Pennsylvania for one year. Prior to that time about fifteen other American students in all had obtained places at the tables taken and paid for by other nations. In 1890 this arrangement was prohibited by the administration of the institution;

and the right to a table in 1891, was secured to Americans, only through the private benefaction of Major Alex. Henry Davis, of Syracuse. For the year 1892 the use of a table has been secured through a subscription started by the American Association for the Advancement of Science, toward which the Association itself granted out of its scanty funds 100 dols. and was the means, I believe, of procuring the rest.[1]

We have not, however, been wholly without some such means of study in this country through the marine biological laboratories established some years ago at Newport and at Wood's Holl, by Prof. Alex. Agassiz. The former has been now enlarged so as to accommodate eight advanced students, besides the professor and his assistant.[2] The Johns Hopkins University also has supplied some opportunities of this kind by its summer school, formerly at Beaufort; later, at Jamaica; but at present, as I understand, it is without any permanent location.

Our neighbour, the Brooklyn Institute, has organised similar investigations, on a minor scale, during the summer months at different places on Long Island. But what is needed for the most effective work is suitable endowments for professors and advanced students, in connection with an adequate biological laboratory, such as the Newport one enlarged might afford, equal in means and equipment to that at Naples, or at least to that recently completed, largely through private enterprise, at Plymouth, England.[3]

(*To be continued.*)

SCIENTIFIC SERIALS.

Bulletin de l'Académie Royale de Belgique, Nos. 9 and 10.—On the conversion of black mercuric sulphide into red sulphide, and on the density and specific heat of these bodies, by W. Spring. As a general rule, if a body is capable of existing in two allotropic states with different densities, it is possible to convert the lighter into the heavier kind by compressing it to the higher density, the pressure depending upon its compressibility. Sometimes this conversion is only possible above a certain "critical temperature." In the case of the sulphide of mercury the conversion of the black into the red variety (vermilion) involves a compression of 9 per cent., and would require a pressure of 35,000 atmospheres, which is not at present attainable. But M. Spring has succeeded in obtaining a new form of black HgS which only requires 2500 atmospheres. It is obtained by sublimation of ordinary black HgS in an atmosphere of nitrogen or CO_2. Its density is 8·0395, while that of vermilion is 8·1587, and of ordinary black HgS 7·6242. A curious side result of the investigation is that the black sulphide hitherto known, after being made to expand by heat and then cooled, takes about a day to return to its original density.—Vapour tension and hygrometric state, by Dr. J. Verschaffelt. A new hygrometer may be based upon the fact that the hygrometric state of the atmosphere may be taken as the ratio of the vapour tension inside a solution to the highest possible vapour tension of water at the same temperature, if the tension inside the solution is equal to that in the atmosphere, *i.e.* when the solution does not evaporate or gather moisture from the air. The ratio mentioned is independent of the temperature, and hence the humidity is simply a function of the concentration of the solution. In practice, Dr. Verschaffelt moistens a weighed piece of blotting-paper with a weighed quantity of a solution of lithium chloride of known concentration, exposes it to the atmosphere, and weighs it again. From the last weight the "equilibrium concentration" may be calculated, and from this the humidity with the aid of Dieterici's data for this salt. The apparatus might be made self-registering.

[1] See *Proceedings American Association A. S.* 1891, vol. xl. p. 449-451.

[2] *Report Harvard College*, 1891, p. 182.

[3] In his address before the American Association for the Advancement of Science, in 1891, President Prescott, referring to this general subject, said: "To nurture investigation in science is the largest opportunity before the American people. Research, systematic and wisely directed, requires good organisation and strong support, the support of many powers. It must have the support of able and persistent men. It needs the conference of workers, and the dissemination of knowledge in societies like this. It wants the interest and the confidence of the public. It asks and will always obtain the constant, helpful use of the press. It requires distinct provision in colleges, and in the institutions of higher education. It ought to be sustained expressly by the Government, both in the several States and under the United States, and sustained on broad and permanent foundations. Still, it needs private benefactions. Research is the growth of years. Let it be the demand of all, and let this call find utterance everywhere."—*Proceedings American Association*, 1891, vol. xl. p. 440.

Bulletin of the American Mathematical Society, vol. i. No. 2. (New York, Macmillan, November, 1894.)—On the problem of the minimum sum of the distances of a point from given points, is the translation, by A. Ziwet, of a paper presented to the Society at its summer meeting (August 15), by Prof. V. Schlegel (pp. 33-52). This frequently discussed problem (see references given by Sturm, *Crelle's Journal*, vol. 97), is considered by the author to offer room for further treatment. He discusses the best method of investigating the question, and in the end treats it by means of the simplest methods of Grassmann's "Ausdehnungslehre." Prof. Cajori collects a number of authorities in confirmation of a statement in his "History of Mathematics" (p. 218), that it is *not* true that the binomial theorem is engraved on Newton's monument in Westminster Abbey. The latest additional authority for his statement is contained in a letter from the present Dean of Westminster, whom Prof. Cajori calls "Dr. Granville"!—The only other matters are the notes and new publications.

SOCIETIES AND ACADEMIES.

CAMBRIDGE.

Philosophical Society, November 26. — Prof. J. J. Thomson, President, in the chair.—On Benham's artificial spectrum, by Prof. G. D. Liveing. Prof. Liveing exhibited one of Benham's "artificial spectrum tops" (see NATURE, November 29, p. 113), and a variety of discs with figures in black disposed on a white ground, and with white figures on a black ground, which, when revolved in a bright light showed remarkable bands of colour of various shades of red, green, and blue. The general result of his observations of these discs was that if a succession of black and white objects were presented to the eye with moderate, but not too great, rapidity, then, when black was followed by white, an impression of a more or less red colour was perceived, while when white was succeeded by black a more or less blue colour was perceived. If the succession of black and white was very rapid the appearance presented to the eye was of a more or less neutral green or drab. The explanation offered by Prof. Liveing was based on the known facts that the impression produced on the retina by a bright object remained for an appreciable time after the light from the object had been cut off, and that the duration of that impression was different for different colours; and on a supposition, which he did not know to have been as yet verified experimentally, that the rapidity with which the eye perceives colours was greater for one end of the spectrum than for the other. From this point of view the explanation of the blue colour seen when white is followed by black would be that the impression of blue on the retina lasts a little longer than that of the other colours; while the red colour seen when white succeeds black is due to the greater rapidity with which the eye perceives red light than that with which it perceives blue. If, however, the alternations of white and black succeed each other with sufficient rapidity, the new impression of a white patch will be produced before that of its predecessor has vanished, and there will be an overlapping of impressions, and the sensation will be that of a mixture of colours, or of a more or less neutral tint. So far as he could test the theory by his own eyes it appeared to him that the residual impression, left when the light from a white object was suddenly cut off, was at first green and faded out through a more or less blue or slate colour.—On a simple test case of Maxwell's law of partition of energy, by Mr. G. H. Bryan.

PARIS.

Academy of Sciences, December 3.—M. Lœwy in the chair.—The reduction of alumina by carbon, by M. Henri Moissan. The author describes the reduction of pure corundum by means of his now well-known electric furnace. Liquid alumina is not reduced by carbon; the reduction only takes place when the vapours of these substances are carried to a very high temperature, metallic aluminium is then produced and partially combines with carbon.—Reply to M. Mayer-Aymar concerning his defence of *Saharien* as a name for the latest geological period, by M. A. Pomel.—A letter from Prof. R. Fresenius was read announcing the formation of a German committee in connection with the Lavoisier monument. The Academy appointed Prof. Fresenius delegate for this work. Prof. G. Hinrichs was similarly appointed delegate for the

United States.—On the identity of the new comet with De Vico's comet, by M. L. Schulhof (see our Astronomical Column, December 6, p. 132).—Observations of the planet B H 1894, discovered by M. Borrelly at Marseilles Observatory, November 19, 1894; by M. Borrelly.—On the distribution of planets between Mars and Jupiter, by M. E. Roger. A mathematical paper in continuation of a paper on the same subject in the previous number.—On quasi alternate permutations, by M. Désiré André.—On the temperature of the electric arc, by M. J. Violle. The conclusion is drawn, from a spectroscopic study of the poles and the arc itself, that the temperature of the arc is generally higher than that of the positive carbon, and that it increases with the electric energy employed.—On the solubility of ozone, by M. l'Abbé Mailfert. At a pressure of 76 mm. water dissolves two-thirds of its volume of ozone at 0° C., at 12° about one-half. The solubility of ozone in water acidified with sulphuric acid is the same as its solubility in pure water up to about 20° C.; more ozone is dissolved by the acid solution above this temperature. The suggestion is made that ozonised water might be employed as a disinfectant and antiseptic.—On the superposition of the optical effects of different asymmetric carbon atoms in the same active molecule, by MM. Ph. A. Guye and M. Gautier. For the determination of this point the authors have used in the present instance amyl valerate. The ester produced by combining inactive amyl alcohol with active valeric acid gives $[\alpha]_D = +1·08°$, the corresponding compound with active amyl alcohol and inactive valeric acid gives $[\alpha]_D = +4·26°$; the ester obtained from active alcohol and active acid gives $[\alpha]_D = +5·32°$, while the sum of the two former is $+5·34°$. Theoretically a better agreement should be obtained by using the racemic in place of the inactive forms; in this case the sum is 5·62°. The difference is probably due merely to experimental errors.—Experimental researches on the crystallisation point of some organic substances, by M. Raoul Pictet. The crystallisation points of a number of organic substances are given, and the results are embodied in a number of general conclusions confirming previous work.—On the emission of a saccharine liquid by the green parts of the orange-tree, by Dr. M. Büsgen. The author calls attention to the part played by aphides and similar parasites in the production of saccharine liquids from plants, and includes the orange-tree among the cases of this kind.—Osteomyelitis of the inferior maxillary in the kangaroo, by MM. Lannelongue and Achard.—On the action of the toxine from the pyogenous Staphylococcus on the rabbit, and on the secondary infections which it determines, by MM. Mosny and G. Marcano. The toxine does not confer immunity against the attacks of the living microbe.—Action of high pressures on some bacteria, by M. H. Roger. Notable differences were observed between different bacteria in regard to their behaviour under pressure. The virulence of the anthrax bacillus was very much diminished by a pressure of 3000 kgms.—On the disinfection of fœcal matter, by M. H. Vincent. At about 16° C. the disinfection of normal fœcal substances is brought about in twenty-four hours by 6 kgms. of copper sulphate per cubic metre. Eberth's bacillus is destroyed in typhoidal refuse by 5 kgms. per cubic metre, and the cholera bacillus by 3·5 kgms. of copper sulphate per cubic metre after twelve hours contact.—Marine muds and their classification, by M. J. Thoulet.

BERLIN.

Physical Society, November 2.—Prof. du Bois Reymond, President, in the chair.—The President alluded to the death of Prof. Pringsheim, and drew attention to his important researches on the fertilisation of algæ.—Dr. C. H. Wind gave a comprehensive review of the researches carried on by Dutch observers with reference to Kerr's phenomenon. He then discussed Lorentz's theory, and described the elaborate experiments made by Sissingh and Zeeman and by himself, which had yielded results for iron, nickel, and cobalt, which were not quite in accord with the theory. Since the other theories as to this phenomenon, as, for instance, that of Drude, are still less in accord with experimental facts, the speaker had extended Lorentz's theory so as to take into account the results obtained by Sissingh and Zeeman, and to bring the phenomenon of Kerr into relation with that of Hall. This extension of the theory had been accepted by Lorentz, and Dr. Wind is now engaged on the investigation of certain phases of Hall's phenomenon.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—In the Guiana Forest: J. Rodway (Unwin).—The Electro-Plater's Handbook: G. E. Bonney, 2nd edition (Whittaker).—Lehrbuch der Botanik für Hochschulen: Drs. Strasburger, Noll, Schenck, and Schimper (Jena, Fischer).—Lehrbuch der Zoologie: Dr. R. Hertwig, Dritte Auflage (Jena, Fischer).—Climbing in the Himalayas; Maps and Scientific Reports: W. M. Conway (Unwin).—Ottica: Prof. E. Gelcich (Milano, Hoepli).—The Dynamics of Life: Dr. W. R. Gowers (Churchill).—The Planet Earth: R. A. Gregory (Macmillan).—Britain's Naval Power: H. Williams (Macmillan).—The Warwick Shakespeare. "As you like it," edited by J. C. Smith (Blackie).—The Teacher's Manual of Lessons in Elementary Science: H. Major (Blackie).—Handbuch der Stereochemie: Drs. Walden and Bischoff, ii. Band (Frankfurt a.M., Bechhold).—Forty-three Graphic Tables or Diagrams for the Conversion of Measurements in Different Units: Prof. R. H. Smith (Griffin).—Torpilles Sèches: E. Hennebert (Paris, Gauthier-Villars).

PAMPHLET.—Gehirn und Seele: Prof. A. Forel (Bonn, Strauss).

SERIALS.—Observatory, December (Taylor and Francis).—Companion to Observatory (Taylor and Francis).—Himmel und Erde, December (Berlin, Paetel).—Geographical Journal, December (Stanford).—Natural History of Plants: Kerner and Oliver, Part 8 (Blackie).—Yule Tide Annual (Cassell).—Science Progress, December (Scientific Press).

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