THURSDAY, MARCH 29, 1900.

CELESTIAL PHOTOMETRY.

Die Photometrie der Gestirne. Von Prof. Dr. G. Müller, Observator am Königlichen astrophysikalischen Observatorium zu Potsdam. Pp. x + 556. Mit 81 Figuren im Text. (Leipzig: W. Engelmann, 1897.)

IN addition to the issue from time to time of the results of original research, the staff of the Potsdam Observatory displays its activity and manifests its usefulness by the publication of text-books, remarkable alike for their thoroughness and for the mastery of the subjects of which they treat. We recently called attention to Prof. Scheiner's work on celestial photography, and we have now to notice a book published about the same time, by his colleague, Dr. Müller, on photometry, a subject which he has made peculiarly his own, especially in researches permitting the application of the Zollner form of photometer. But if in practice he has limited himself to the use of this particular apparatus, his familiarity with the history and literature of the subject, his knowledge of the use and construction of other forms, and his intimate acquaintance with all that has been accomplished in this comparatively recent branch of astrophysics, mark him out as the proper authority for the production of a book on photometry, and constitute him a safe guide and able instructor in an inquiry that is not without its difficulties.

Dr. Müller divides his book into three main sections; the first treats of the elementary principles on which photometry is based, the second deals with photometric apparatus and methods, while the third is devoted to the detailed consideration of the results of photometry applied to celestial objects. In the first section, various elementary problems are discussed, which have been treated by numerous authors, from Lambert downwards. Over these one can rapidly pass, since they occupy, to some extent, a ground common to all photometric inquiries, and have extensive application outside astronomical research. The so-called Purkinge phenomenon and Fechner's psychophysical law are discussed at sufficient length, and may be found attractive since recent controversy has invested these subjects with considerable interest. Of more direct bearing on celestial photometry are the albedo of the planets, the distribution of light over planetary discs, the effects of phase, &c. The latest views of physicists, naturally of German physicists, on these questions are set out in great detail, and various problems which have been discussed in periodical literature are here collected into a convenient and accurate summary. In the third and concluding chapter of the first section we have presented to us a full discussion of the important, but rather thorny, inquiry into the amount of light lost in passing through our atmosphere. The theoretical treatment that this problem has received at the hands of Lambert, Bouguer, Laplace and Maurer (the last better known in this country in connection with the names of Dale and Gladstone) is copiously examined, and not only is justice done to the eminent mathematicians by whom the inquiry has been elaborated, but we have also a comparison between the results of theory and the amount actually deduced from observation by empirical methods, or, at least, by methods in which no theoretical assumptions are made. On the side of practical observation, Dr. Müller has himself worked with vigour and marked success. His table of values, which express the average amount of light extinguished by the atmosphere, as determined for the Potsdam Observatory, probably holds the most authoritative position of all the inquiries that have been instituted. In saying this, we do not leave out of sight, as Dr. Müller has apparently done in his table on p. 138, the work of Profs. Pickering and Bailey in this department of photometric inquiry. It is difficult to understand the reason for this omission. Prof. Pickering's method of observing the brilliancy of a circumpolar star at its upper and lower culmination seems to be free from objection, and has the certain advantage of confining the observations to the plane of the meridian, the plane in which all the observations are made. The amount of his material was so large that some roughness and inaccuracy in his observations would disappear in the results, and his simple formula of $-0{\cdot}25$ mag. sect. Z, applied to the observed magnitude, appears to us quite as deserving of mention as some of the results here collected. Some exception might be taken to Dr. Müller's own value given in the table (p. 138). He has himself made two determinations of the value of the coefficient of transmission of light through the atmosphere; one conducted in the presumably clearer and drier air of the Santis summit, the other at Potsdam, where the results are affected by the dust and vapour due to the proximity to a busy town, but which probably fairly represent the meteorological conditions that prevail in the neighbourhood of most observatories. To the results of the Santis investigation no exception can be taken; but, notwithstanding the general favour in which the Potsdam table is held and accepted, it has the disadvantage of containing implicitly two values of the transmission coefficient. This curious result is due to a change in the manner of observation when dealing with objects at small altitudes. Practically no difficulty will arise in the use of Dr. Müller's table of observed extinction, but we decline to follow the author in his assertion that this peculiarity is an advantage. In the first part of the table, the amount of light extinguished indicates that 81 per cent. of the incident light falling on the atmosphere is effective; while the latter part would show that 85 per cent. emerges. Both of these values cannot be correct; but the coefficient 0·835, suggested by Dr. Müller, represents not only the mean of his own observations, but accords very satisfactorily with the average of all the determinations that have been made. From the theoretical side, it is safe to say that the theory of Laplace represents the actual observations within the accuracy at present attainable.

This agreement between theory and observation leaves untouched Prof. Langley's contention concerning the total loss of radiant energy in a ray of light passing vertically through the atmosphere. While there is no question as to the legitimacy of the objection that Prof. Langley has raised, Dr. Müller is not inclined to accept the high percentage of loss demanded by the American physicist. Dr. Seeliger is quoted as an authority of equal weight, to show that the probable loss of light is only about one-third that suggested by Prof. Langley. With the view of obtaining a more correct determination, Prof. Müller has proposed, in connection with Dr. Kempf, to observe the brilliancy of a star simultaneously, as seen at sea-level and on the top of a neighbouring mountain, thus getting different lengths of atmospheric path; but the few observations that have hitherto been made with this object do not point to any trustworthy conclusion.

In the section devoted to the description of photometric apparatus, one looks for completeness rather than originality, and though Dr. Müller may have omitted some of the forms that have been recommended, yet so many examples have been illustrated and described, one would willingly hope that he has exhausted the entire catalogue. Practically there are only two principles which underlie every form of photometric measurement—extinction, and equalisation of light—but ingenuity has applied these two principles in every variety of method. This variety suggests great dissatisfaction with existing apparatus, and insinuates that a simple, convenient photometer, that would inspire confidence and ensure accuracy, has yet to be invented. After all, the eye is the real photometer; and it may be that here the inherent difficulty, physiological rather than mechanical, exists, though attempts to displace the eye, and to substitute the photographic film, have not as yet led to more satisfactory results. We cannot follow Dr. Müller through the various arrangements of diaphragms and the employment of absorbing media, by which the problem has been sought to be solved on the principle of extinction. Neither can we mention the many advocates of the method of equalisation, who have pressed into the service of photometry rotating discs, reflecting spheres, and other ingenious devices, while the applications of polarised light in various forms are legion. The special department of spectral photometry receives new additions to its apparatus every month; but if any one imagines that he has a new scheme to add to the many already proposed, we advise him to study the critical remarks of our author before venturing on a ground which is already strewn with failures, or, at least, doubtful successes.

The third and concluding section is not the least interesting. Here it is the author's purpose to show what has been accomplished by the application of photometrical methods to the planets and satellites, to the comets and stars. We not only get an historical account of what has been effected, and are thus reminded of the energy that astronomers of this century mainly have devoted to this subject, but are able to appreciate the degree of success that has attended their efforts, and see in what direction future enterprise is possible. From an observer of Dr. Müller's experience we get many critical remarks of great value, and are able to see the exact position in which celestial photometry stands. Perhaps, in the single direction of the application of photography, something more might have been added; but it must be remembered that the author's colleague, Dr. Scheiner, has already fairly occupied this ground, and some large catalogues, like the "Southern Durchmusterung" of Dr. Kapteyn, had not appeared at the time of publication. In this section, where so much is important, it is difficult to select any one topic for comment; but special mention should be made of the references to the observation of variable stars, and the relations existing between eye-estimations of star magnitude and photometric measurement. With this last subject is necessarily connected the difficult question of standards and units of measurement. Over such subjects, to which may be added the desirability of attaining uniformity of scale, some little controversy has hitherto ruled, and perhaps is not yet definitely settled, owing to the diversity in the instruments employed, and the systematic errors introduced, it may be, by abnormal vision. On the whole, Dr. Müller preserves a judicial dignity and impartiality, but some of the strictures concerning the Southern Catalogues produced under Harvard methods and auspices are, we think, undeserved. The lower altitude of the Pole at Arequipa did undoubtedly give rise to great difficulties, but any want of continuity, supposing it to exist, between the Northern and Southern Catalogues can hardly be ascribed to the method of observation. Dr. Müller's own method of establishing numerous points of reference, scattered over the sky, to serve as standard comparison stars, has much to recommend it, but it is a plan that involves long preparation before the work of real cataloguing can begin. How far the increased accuracy of the work repays for this preliminary labour, and to what extent other astronomers will avail themselves of these scattered standards, rather than to trust to a single star, are points that can hardly be decided yet; but every attempt that aims at additional accuracy must be welcomed, and on the side of accuracy we believe that the Potsdam photometric measures stand unrivalled.

AN ILLUSTRATED HISTORY OF MATHEMATICS.

Histoire des Mathématiques. Par J. Boyer. Pp. xii + 260. (Paris: G. Carré et C. Naud, 1900.)

TO write within the compass of less than 240 octavo pages an intelligible outline of the history of mathematics from the earliest times to the present day is a task so difficult that its accomplishment might reasonably be thought impossible. Yet by adopting a consistent, if modest, programme, and frankly accepting its necessary limitations, M. Boyer has achieved a considerable measure of success; possibly as much as the scope of his essay could allow. A reader whose mathematical knowledge is only moderate, or even elementary, will obtain from this book some idea, trustworthy so far as it goes, of the contributions made to the science of mathematics by the ancient Babylonians, Egyptians, Indians and Greeks; he will be able to follow the feeble course of geometry and analysis through the Middle Ages, and appreciate to some extent the glorious renascence of the sixteenth and seventeenth centuries; and he will acquire some conception of the work of the giants, from Newton to Laplace, who established the primary landmarks of modern mathematics.

Perhaps the most satisfactory chapters in the book are xiv.-xvii., which cover the period from Descartes to Laplace. It deserves to be specially remarked that M. Boyer discusses the invention of the infinitesimal calculus in an impartial and charitable spirit, and that he does substantial justice to the merits of Cauchy and Monge, who are not always appreciated as they deserve, even by their own countrymen.

As might be expected, the chapter on contemporary mathematics and mathematicians is meagre, and lacking in proportion; but it has the merit of emphasising some, at least, of the most important lines of inquiry, and it is not disfigured by any of those erroneous statements which so often appear in summaries of this kind.

One feature of the book, which will make it attractive to all classes of readers, consists in the illustrations. They comprise four facsimiles of manuscripts, one of the title-page of the "Acta Eruditorum," two plates showing the form of mathematical instruments in the seventeenth century, and nineteen portraits. The portraits are fairly representative, and are derived from authentic originals; one would gladly have dispensed with Mme. du Chatelet and Saunderson in exchange for Gauss and Abel, who are unaccountably omitted.

The appearance of so many popular histories of mathematics lately suggests a few remarks upon the purpose which they are, or should be, designed to fulfil. Their proper object is that of supplying stimulus; and there is no doubt that the interest of a mathematical student is greatly encouraged by historical notes on the subject of his reading. To the teacher, the history of mathematics is a subject not merely of interest, but of vital importance, because the psychological history of a race tends to repeat itself, in little, in the mind of the individual; hence the proper order of teaching a subject is not necessarily the most logical one; and indeed the best compromise between logical sequence and what we may call "historical sequence" is precisely the golden mean at which the teacher must do his best to aim. A popular history does good if it awakens the teacher to some idea of this: the great risk is that he may imagine that the popular history contains all the information that he requires. This is so far from being the case that he may be actually in a worse position after reading his "History" than before. Points of the highest importance are necessarily ignored in a popular treatise, either from want of space, or because the author is afraid of frightening his readers with technicalities. For instance, it is comparatively easy to ascertain the net results of Greek geometry, expressed in modern terminology; but without some acquaintance with the actual works of Euclid, Archimedes and Apollonius, *as they wrote them*, it is simply impossible to have any correct ideas of the aims and methods of Greek mathematicians. If our teachers of mathematics were really familiar even with the "Elements" of Euclid, instead of with a garbled version of a part of it, they would be far better able to discuss intelligently the question of "Euclid and his Modern Rivals."

Again, the real interest of the ante-Descartes period in Europe consists in the gradual improvement of algebraic notation, and of methods of arithmetical computation. This question of notation is of the greatest interest from every point of view. What we have now is simply the survival of the fittest, and may have to submit to modifications more drastic than any of us at present imagine; still it is far ahead of its predecessors, and our admiration of Archimedes and Fermat is greatly enhanced when we realise the wretchedly inadequate notation which they had to employ. To have traced, even in a general way, this advance of notation and method, is far more instructive and important than to know, for instance, that logarithms were invented by Napier of Merchiston, or that Newton discovered the Binomial Theorem; yet very little help in this direction is afforded by the popular history.

A really good history of mathematics in the nineteenth century has yet to be written; it would probably require the combined labour of an organised body of experts, such as those engaged on that invaluable work, the "Encyclopädie der mathematische Wissenschaften." Until such a scientific history has been composed, it is idle to expect anything worth reading in a popular treatise. It may even be questioned whether a popular writer, however competent, could profitably deal with the subject at all, unless our methods of school teaching are greatly modified. For the history of modern mathematics is not mainly that of individual discoveries, however brilliant; but that of the systematic investigation of mathematical notions, such as "number," "continuity," "function," "limit," and the like. If these technical terms are ignored, even a popular sketch of the subject becomes impossible; yet how many of these terms are even approximately understood by any but mathematical specialists? And what is the use of trying to explain the theory of doubly periodic functions to readers who are unaware that, in learning trigonometry, they were studying singly periodic functions without knowing it?

G. B. M.

SCIENTIFIC LENS-MAKING.

Theorie und Geschichte des photographischen Objectivs. Von Moritz von Rohr. Pp. xx + 435. (Berlin: Springer, 1899.)

DR. VON ROHR'S book contains much to attract students interested in the theory and development of photographic lenses. The author is one of the scientific workers attached to Zeiss' manufactory in Jena, and has a practical acquaintance with his subject.

The book is most instructive to an English reader, specially for the reason that while the debt due to the great English opticians of the present day, as well as to those of the past, is freely owned, and the author's appreciation of the value of their work is warm and cordial, yet the contrast between the methods of the English school and those of the pupils of Abbé and Schott is sharply drawn, and it is clear that in the opinion of the author the future is with the latter. Schott and Abbé began in 1881 "to study carefully, as far as possible, all chemical elements, which in any form can become constituents of amorphous compounds produced by fusion, with regard to their influence on the refraction and dispersion of the compounds." On this secure basis is raised the great Jena glass factory, to whose work the scientific world is already so deeply indebted.

We doubt if any English manufacturer would have attempted thus to improve his products; the new methods, the methods which Englishmen must adopt if England is to retain her place among the manufacturing peoples of the world, have not yet found a home among us, and unless a change is made, England must cede the place of honour, not merely in lens-making, but in every branch of manufacture.

But to return to Dr. von Rohr's book. It is divided into two parts, theoretical and historical respectively; the first, occupying about 80 pages of the whole 400 in the book, is clear and interesting so far as it goes, but it is hardly satisfying. The author has abstained, no doubt wisely from some points of view, from attempting to give the mathematical proof of most of his propositions; the result is that the reader is often brought to a standstill with the question—But how does this follow? Without a much greater acquaintance with optics than can be gained from the book, he would find much of it difficult to read with profit. It is all very well to be told, to take at random a very simple example, that the correction for chromatic aberration for two colours depends, for a "thin" lens, only on the focal lengths and refractive indices of the two lenses concerned, and not on the curvature of their faces—so long as the focal length is not altered by change of curvature—but an intelligent reader would like a proof of this.

The author starts from Gauss' theory of lenses, which is only applicable to small pencils centrically incident and inclined at a small angle to the axis; he extends this practically by the assumption that lenses can be constructed for which Gauss' theory, freed from the restriction of nearly direct incidence, would hold strictly; and then he examines the points in which actual lenses differ from this ideal system.

Each error is discussed in turn, the method of correcting it is described, and the possibility of combining the corrections for various errors is considered. Admitting the difficulty of inserting the mathematical proofs, and the probability that if they had been inserted the book would have been useless to many for whom it was intended, it may be said that all this part is well done. The result of the discussion is summed up on page 56, in the section on Seidel's five spherical errors, and the impossibility of completely removing them from a photographic object-glass. A complete freedom from spherical aberration cannot be combined with absence of distortion for all positions of his object.

A further section of this part deals with chromatic aberration; the relations between the conditions for freedom from both spherical and chromatic aberration in a thick lens are specially well treated.

The second, and by far the larger, part of the book is an historical account of the development of a photographic objective, and this is written with great insight and judgment.

It may be felt by some that a too marked prominence is given, in the account of recent years, to the work of the Jena school; such prominence is only natural considering the circumstances of the author, and it is certainly true that he shows a high appreciation of the work of the distinguished English opticians, to whom so much of the advance in photographic lenses is due.

The work is very complete; it begins with the first camera obscura made by Giambattista della Porta in 1589, and carries us down to a lens patented by E. von Hoegh in 1899. The cuts illustrating the various lenses are carefully drawn—as far as possible to scale—reduced to a common focal length of 100 mm., and the nature of the glass used is indicated by special shading. Altogether the book deserves careful study.

EXPERIMENTS ON ANIMALS.

Experiments on Animals. By Stephen Paget, with an Introduction by Lord Lister. Pp. 269; 3 illustrations. (London: Fisher Unwin, 1900.)

THERE are many people who write about experiments upon animals, but only very few who have under their constant notice the actual facts relevant to the subject. In this connection, not merely is a knowledge of fact required, but an intellectuality capable of appreciating the significance of fact. The person most competent from this standpoint is one of the Inspectors under the Act. These Inspectors are most carefully chosen by the Home Office on account of special qualifications which they possess. It must not, however, be assumed that because they are the only officers paid by the Crown they are the only men of science who serve it. Most zealous and somewhat thankless help is afforded to them by those authorities who, by virtue of their position and attainments, are regarded as competent to support the candidate in his application for a license or certificate. It would not, however, be comely for a person holding an official appointment to write a book upon the subject-matter of his office. Every vivisector, a terrible term by which to designate any one who merely pricks a guinea-pig, knows full well that no one, with the above exceptions, is more entitled to write upon the subject of animal experiments than Mr. Stephen Paget, who for twelve years was the active and long-suffering secretary of the Society for the Advancement of Medicine by Research. During this time most licensees under the Act were brought into contact with the author of the book before us.

The volume does not simply concern itself with the working of the Act, but must be regarded as a weighty contribution to the polemical literature of the subject. It shows not only that the Act is vigorously worked by the authorities, but also enters largely into the question of the justification of animal experiment.

The part of the book devoted to this subject will be of the greatest interest to the general reader, and it is sincerely to be hoped that he will take advantage of it, for, while the diatribes of those who oppose all animal experiment are thrust almost weekly into the hands of the public, the *altera pars* says but little. Mr. Paget classifies the experiments that have up to the present been performed upon animals according to the individual field of medical science enriched by their results. The most casual reader must gather from his pages how in almost every domain, medicine, using this term collectively, has learnt from animal experiment, and how the treatment of disease has by its means advanced from mediæval empiricism to its present condition. To twit workers in the medical sciences with the fact that certain discoveries in physiology, established by means of vivisection, have not so far led to the curability of apparently cognate diseases, shows a want of that intellectuality which is capable of appreciating the significance of scientific fact. We might as well deny the value of the discovery of Africa because some parts of it are uninhabitable. As hygienic science advances, and our knowledge concerning both the methods of extinction of pathogenic micro-organisms, and the neutralisation of their injurious products increases, no doubt some of the now malarial swamps will be converted into thriving colonies; and so is it with physiology, when by further experiment our knowledge of the *modus operandi* of the change from the physiological to the pathological is more complete, many facts now apparently barren will bear fruit a hundred-fold.

Those who apparently with such unctuous satisfaction point to the inability of even modern therapeutics to cope successfully with certain deadly diseases, are surely supplying an argument for more experimentation, and not for less. If medicine has not derived the full benefit possible from physiological discovery, it will do so later on. But what is regarded by the anti-vivisectors as a benefit? One of them asks quite recently, and apparently quite seriously, what benefit has accrued to medicine from a knowledge of cerebral localisation.

Mr. Paget deals with many points in detail which have formed the text of many of the more or less scurrilous essays of both varieties of antivivisectionists; he succeeds in showing that their case is only skin deep, and that when care and some erudition are applied to the elucidation of the individual instance, the facts appear in quite another light.

The last part of Mr. Paget's work is devoted to the Prevention of Cruelty to Animals Act itself. He argues, with some reason, that however efficacious the Act might have been in 1876, since then a new science, bacteriology, has practically arisen. This science for the elucidation of its problems requires a special kind of technique, simple enough, but for which the Act is ill adapted. At the conclusion of this chapter some interesting accounts are given of questions in the House of Commons concerning the working of the Act, and some interesting letters reproduced, emanating from antivivisectors, and threatening her Majesty's ministers with political destruction if they failed to use their influence against experiments on animals. The President of the Board of Agriculture seems especially to have incurred their wrath.

OUR BOOK SHELF.

Cyclopedia of Classified Dates. By Charles E. Little. Pp. vii + 1454. (New York and London: Funk and Wagnalls Co., 1900.)

IT may be doubted whether this bulky volume is of sufficient value to justify the immense amount of labour that must have been spent in its compilation. There are no less than 95,000 entries of important (and unimportant) historical events, classified geographically, chronologically, and according to their nature, so that the where, when and what of any event can be discovered. The volume is intended to be a universal history, a biographical dictionary, a geographical gazetteer and many other books combined; in short, an omniscient and international Domesday Book. The only parts with which we have any concern are the divisions of science and nature included among several other groups of events recorded for each of the seventy-nine geographical divisions, which are arranged in alphabetical order. Many of the entries appear vague and trivial, and some are misleading, if not actually incorrect. As instances of information which comes under one or other of these criticisms, the following may be cited:—1089, a widespread earthquake is felt; 1737, Dr. James Bradley discovers the variation (*sic*) of the earth's axis; 1783, Walker produces ice in summer by means of chemical mixtures; 1783, Herschel proves the binding (*sic*) rotary motion of the stars; 1787, quicksilver is frozen without the aid of snow or ice; 1827, the spectrum analysis is worked out by Herschel; 1848, William Lassell discovers the eight (*sic*) satellites of Saturn; 1852, Sir William Thompson (*sic*) discovers the dissipation of energy; 1861, Mr. Thompson, of Weymouth, photographs the bottom of the sea; 1867, nitrous oxide gas (laughing gas) is introduced; 1881, telephotography is invented by Shelford Bidwell; 1890, the bones of a hippopotamus are found imbedded in clay; and there are many others.

But the sins of commission are as nothing in comparison with those of omission. The only events recorded under science and nature in 1894 are the meeting of the British Association, and the departure of the Jackson-Harmsworth polar expedition; in 1893, an earthquake and a flood; in 1892, two earthquakes; and in 1891, the meeting of the international congress of hygiene and demography.

Judging from these facts, no serious attempt has been made to trace the progress of science in any of its branches in latter years. From our point of view, therefore, the book is of little value. In a charitable spirit we trust its merits in other sections help to make up its deficiencies in those referring to scientific matters.

Justus von Liebig und Christian Friedrich Schönbein. Briefwechsel, 1853-1868. By Georg W. A. Kahlbaum und Eduard Thon. Pp. xxi + 278. (Leipzig: Johann Ambrosius Barth, 1900.)

THE correspondence of Faraday and Schönbein, which was noticed in a former issue (Feb. 8), finds a companion volume in the work before us, which comprises 133 letters covering a period of fifteen years. Of these letters, eighty-four are from the pen of Schönbein. The same care as regards editing and annotation which was observable in the previous volume is a conspicuous feature of the present work.

The subjects dealt with by Schönbein are chiefly those which are referred to in his letters to Faraday, more especially ozone and the work arising from his investigations of that substance. Some of the letters are more or less personal and political, and will be found very interesting reading. Liebig's letters also are replete with topics of interest in the history of chemistry, and will repay detailed consideration.

Among the subjects dealt with are fermentation, food preservation, meat extract, and agricultural chemistry. It must be remembered that the views concerning fermentation, and the bearing of chemistry upon agriculture, were at that period in the polemical stage, and this imbues Liebig's statements with special interest. Both writers also from time to time soar from the commonplace recital of facts and discoveries into the higher regions of speculation and philosophy. Liebig's views on the place of Bacon in philosophy and science are referred to by Schönbein with approval (p. 166). Schönbein's views on theory as a stimulus to new discovery (p. 216) will also commend themselves to the majority of readers.

It will be gathered from this brief notice that the volume under consideration is as valuable a contribution to the history of science as its predecessor. R. M.

Cina e Giappone. By E. von Hesse Wartegg. Translated into Italian by Captain Manfredo Camperio. Pp. 536; illustrated by 168 woodblocks, 72 plates, *facsimiles* of manuscripts, and one map. (Milan: Ulrico Hoepli, 1900.)

THE opening up of China to foreign commerce has naturally created a demand for books dealing with the country and its inhabitants considered from every aspect; their religion, their treatment of missionaries, their laws, their secret societies, their educational systems, their army, their monetary systems, postal arrangements, banks, theatres, and public institutions generally. This book is a traveller's account of the country, with no pretensions to be regarded as scientific; indeed, in reading the book with a view to matters of direct scientific interest, we have found nothing more noteworthy from this aspect than the description of the Chinese methods of performing calculations and of teaching arithmetic in schools. In connection with Japan, attention is called to the serious competition on the part of the Japanese, which threatens to undermine European commerce in the East; and the translator, as president of an Association for trading with the East, corroborates this view.

Europeans have been engaged by Japanese firms to teach them European methods of manufacture, and Japan is now sending out goods precisely similar to those of European make, and with the trade marks copied on them, and is able to sell them at lower prices than the Germans. It is particularly in the competition to supply the Chinese markets that Japan seems likely to outstrip Germany most effectually.

Practical Chemistry. Part i. By William French, M.A., F.I.C. Pp. xvi+136. (London: Methuen and Co., 1900.)

An Introduction to Qualitative Analysis. By H. P. Highton, M.A. Pp. xii+170. (London: Rivingtons, 1900.)

THE many excellent elementary text-books now available for students of chemistry ought to have a very distinct influence upon chemical teaching in schools. The two books under notice differ in several respects, but each is the work of a teacher who knows the capacity of a school curriculum for science, and the limitations as well as the capabilities of the human boy. Mr. French's book follows more or less closely the chemical subjects included in the syllabus of elementary physics and chemistry prescribed for Evening Continuation Schools. The syllabus is a reasonable one, and therefore it has been possible to describe a course of work which will meet with the approval of the advocates of rational methods of instruction in chemistry. Intelligent work in experimental science is now encouraged by the authorities of the University Local Examinations as well as the Education Department; and Mr. French's book provides a course of instruction which may be adopted with advantage, not only by teachers who have the requirements of examiners and inspectors in view, but who desire also to cultivate habits of observation and reasoning in their pupils.

Mr. Highton's book contains a carefully graduated course of practical chemistry which will serve as an introduction to simple qualitative analysis. It is not so distinctly a product of the "heuristic" movement as Mr. French's book, and is largely devoted to systematic analysis. The first part contains simple qualitative experiments and preparations leading up to analysis; while the second comprises all the metals and acid radicals not included in Part i., and met with in simple qualitative analysis. The third part deals with the separation and identification of the separate parts of a mixture of two or more simple salts. After a pupil has been taught to think, a course of practical chemistry such as this may be intelligently performed; but if he is introduced to chemistry by reagents and precipitates, the educational value of his work will be very small. Mr. Highton has succeeded in making a useful course of analytical work for boys preparing for examinations in practical chemistry.

Essai de Chronologie des Temps préhistoriques. By M. Roisel. Pp. 60. (Paris: Felix Alcan, 1900.)

AN essay in which evidence for three glacial epochs is made the basis of a division of the Quaternary period into seven distinct ages, extending from the year 88,000 B.C. to 6500 A.D.

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.*]

Atmospheric Electricity.

FROM a paragraph in the "Notes" in NATURE of March 1 (vol. lxi. p. 422), it will be seen that the theory advanced by Mr. C. T. R. Wilson, of the Cavendish Laboratory, Cambridge, and recently supported by Elster and Geitel, of the origin of atmospheric electricity is gradually crystallising and becoming accepted, as might be expected when supported by such authorities. This theory is founded on the very beautiful and interesting experiments of these investigators, which show that there are ions in our atmosphere, and that these ions can form nuclei for the condensation of water vapour; and, further, that the negative ions become centres of condensation with a less degree of supersaturation than the positive ones, and consequently during condensation they will be the first to be carried down by precipitation, the positive ions being left in the atmosphere.

Before meteorologists accept this explanation of atmospheric electricity there are some points I would like to place before them for their consideration. These ions do not act as centres of condensation unless the air be highly supersaturated, whilst dust particles are active in saturated air, and some of them in air that is not quite saturated. So that before we can accept this theory of the electrification of the air we must be sure there is such a thing as dust-free air in our atmosphere; because if there is not there can be no such thing as the supersaturated air required to produce this separation of positive and negative ions.

Mr. Wilson is evidently conscious of this difficulty (*Phil. Trans.* vol. cxciii. pp. 289-308), as he adds a note to the end of his paper, in which he states that there is no evidence of supersaturation in the atmosphere; but he also says there is an equal lack of evidence against its existence; and whilst admitting it cannot exist in the lower dust-charged layers of the atmosphere, he is reluctant to give up the theory on that account, and supposes it may be possible that cloudy air may be purified of its dust as it ascends, by the dust particles becoming weighted by the vapour condensed on them, when they will fall, or the air may rise up through them and be dust-free when it escapes. Now any one who has been in clouds, or knows the slow rate at which cloud particles descend, will be aware that this process is an extremely slow one, and compared with movements of the cloud as a whole very insignificant. But suppose we give the cloud every chance to get free from its cloud particles: let it be kept quite still and free from eddies, and time allowed for all the water particles to fall out of it. Now what would be the condition of the air afterwards? Practically what it was before the cloud was formed. There would be a smaller number of dust particles in the air, but there would still be plenty to form a number of clouds in succession if the vapour supply was not exhausted. When a cloud forms in ordinary impure air, only a small proportion of the dust particles become active centres of condensation, whilst many receive no charge of vapour. On the Rigi Kulm (*Trans.* Roy. Soc. Edin. vol. xxxvii. Part iii. No. 28) I have counted as many as 3000 and 4000 dust particles per c c. in clouds, and on one occasion as many as 7700 in a dense cloud. Whilst in fog, which we may call a low-level cloud, I have observed as many as 50,000 dust particles per c.c. With thousands of dust particles in each cubic centimetre it is evident a cloud has dust nuclei sufficient for making a number of clouds should the first formed cloud particles be precipitated, as these dust particles will move with the air, whilst the cloud particles fall out.

No doubt clouds do not always have such large numbers of dust particles in them as the clouds above referred to, but cumulus clouds seem to be always pretty well supplied in that way, especially over continental areas. Then, as the quantity of water to be condensed is limited to the amount the cloud takes up with it from the surface of the earth—as it is not likely to have any vapour added to it unless it falls below the rain cloud level—there seems generally to be enough dust particles to condense all the water.

We must further remember that nature is economical in its use of these dust particles. If the cooling is taking place very rapidly, a large number of particles at once become active, but after a time a number of them lose their load of water by drying up, and only a sufficient number to do the work being retained on duty, the others going to the reserve (*Trans.* Roy. Soc. Edin. vol. xxxvii. Part ii. No. 20). It may be also as well to remember that even supposing all the dust to be precipitated out of the air, nature may be able to manufacture fresh supplies (*Trans.* Roy. Soc. Edin. vol. xxxix. Part i. No. 3). If there are any gaseous impurities in the air, such as ammonia, nitric acid, nitrous acid, peroxide of hydrogen, sulphurous acid, sulphuretted hydrogen, hydrochloric acid or chlorine, the sunshine will convert them into nuclei, which do not require a high degree of supersaturation to make them active centres of condensation.

There is another point in connection with condensation by means of ions which may be referred to here. It does not seem probable that ions could ever cause the formation of a cloud though they might give rise to rain. If the air becomes so supersaturated as to make them active centres of condensation, it will then be in an unstable condition. It will, in fact, be in an explosive state—explosive centripetally and not centrifugally as usual. Whenever any ion, in air in that condition, owing to any advantage of its constitution, or difference of temperature, pressure, or saturation in the air surrounding it becomes active before the others, it will grow extremely rapidly, and at once begin to fall; and as it will fall through highly supersaturated air, it will relieve the tension all along its path, and so grow rapidly, and soon become a rain-drop and fall to the earth. In Mr. Wilson's experiments cloudy condensation takes place with ions as nuclei, but in his apparatus the expansion is nearly instantaneous, and no particular ion has time to take advantage over the others, and many are thus formed at the same instant. There is still another point which requires consideration. What would the effect be in a rising column of air if it could not call on the reserve heat, latent in its vapour, till it had risen to a higher elevation than is necessary when dust is present.

So far as our knowledge goes, it can hardly be said there is such a thing as dust-free air in our atmosphere, and the cases in which low numbers have been observed are so extremely rare that they can hardly have any bearing on a phenomena of such widespread existence as atmospheric electricity, even though we suppose those few particles to be afterwards got rid of. We cannot suppose the positive ions will always remain in the atmosphere, because if the conditions are ever such as to cause the fall of the negative ions, the positive ones will also afterwards fall with only a slightly greater increase in supersaturation.

From what has been stated above, I think we must defer expressing an opinion on the value of this theory of the source of atmospheric electricity, and wait till some stronger evidence is produced that the air in our atmosphere is ever absolutely dust-free, so as to permit of the supersaturation becoming great enough to cause a separation of the negative and positive ions.

Ardenlea, Falkirk, March 17. JOHN AITKEN.

Escape of Gases from Planetary Atmospheres.

IN the "Astronomical Column" of last week's NATURE, on p. 501, you give an abstract of a paper by Mr. S. R. Cook, of the University of Nebraska, and quote the passage in which he points out that the present writer, when investigating the escape of gases from atmospheres, does not base his argument upon "the determination by the kinetic theory of the relative number of molecules which would have a velocity sufficient to enable them to escape from the earth or planet."

This is so: and the reason is that no such determination existed until that arrived at in the paper criticised by Mr. Cook, where data drawn from outside the kinetic theory are employed to supplement what the kinetic theory teaches. These auxiliary data are (1) that the moon has not retained an atmosphere; and (2) that the earth and Venus do retain the vapour of water in their atmospheres.

Mr. Cook supposes that Maxwell's law for the distribution of the speeds of the molecules when a gas exists under normal conditions may legitimately be employed to obtain the rate of the escape from an atmosphere. But in this he overlooks (1) the fact that the molecules that escape are emitted exclusively from that outermost layer of the atmosphere, throughout which the molecules are within striking distance of the void space beyond; and (2) the important circumstance that the molecules exist within this altogether special layer under conditions entirely remote from those which are assumed by Maxwell in the proof [1] of his law; so that Maxwell's law fails us just where we want its help, viz. in that part of the atmosphere from which the entire of the escape of molecules takes place.

This upper stratum of the atmosphere, which is probably some miles in depth, is limited on its inner side by a deeper-seated stratum of air, and on the outer side by a stratum of virtually empty space, that is, by a space tenanted only by molecules which seldom or never meet with encounters while within that space, and are therefore to be regarded as temporarily or permanently beyond the atmosphere. As to the stratum which is above spoken of as the outermost of the atmosphere, molecular encounters take place in it; and whenever an unusual speed is generated in any of the molecules which occupy it (as happens frequently to every molecule of a gas) these molecules have an opportunity of placing themselves beyond the reach of those subsequent encounters which, in gas under normal conditions, are what tone down the frequently recurring irregularities. and bring about an approximate conformity with Maxwell's law within a sufficient volume of gas of uniform density, and surrounded by gas of the same density.

Nevertheless, the numerical results obtained by Mr. Cook, though arrived at by a faulty process, are not useless. They have a certain value, inasmuch as it can be proved that the actual escape of gases from an atmosphere is more rapid than it would be if Maxwell's law governed that rate. Accordingly, Mr. Cook's numbers furnish a computed rate which we know that the actual rate must exceed, and may largely exceed. This in itself is valuable information; and would be important information, if we had no better way of investigating the problem.

Like Mr. Cook, the present writer, when he first entered on the investigation of the escape of gases from atmospheres in 1867 or 1868, hoped that Maxwell's law for the distribution of the speeds of the molecules under normal conditions would render aid; and it was only when he found that to conduct the inquiry in this way could not furnish correct results, that he cast about for some other way of approaching the problem, and finally adopted that which is developed in his memoir.[2]

G. JOHNSTONE STONEY.

8 Upper Hornsey Rise, N., March 25.

State of Practical Instruction in Physics.

WITH the view of obtaining comparative statistics of the organisation of practical instruction in physics, I have recently sent a list of questions, with table of different manipulations, to the directors of all the physical laboratories with the addresses of which I am acquainted.

Through the medium of your esteemed journal, may I ask the directors of the laboratories, who, by any reason whatever, have not received this list, kindly to inform me, so that I may at once forward the same. I should like to make the same appeal to the directors of the mechanical, electrical engineering, electro-chemical, physico-chemical, &c., laboratories where part of the practical work is of a purely physical character.

BORIS WEINBERG.

University of Odessa, Russia, March 17.

Indian Corn.

I HAVE just found in Nakamura's "Kimmō Dzui," first edition, 1666, Book xvi. fol. 7*b*, a Japanese wood-cut of Indian corn, with its Japanese and Chinese names as I gave in my previous letter (p. 392, *ante*). This figure proves that, though Kaempfer does not mention the plant in his "History of Japan," 1727, yet, through his seeing to it, he must have recognised as a fact the introduction of maize to Japan before the time of his sojourn in it; for most illustrations of the biological objects in his noted "History" (vol. i. tab. ix.–xiv.) are actually found to have been reproduced from the above-mentioned, once very popular, Japanese cyclopædia (Books xii.–xv.).

KUMAGUSU MINAKATA.

1 Crescent Place, South Kensington, S.W., March 9.

The Bacteriology of the Soil.

I SHOULD be much obliged if, through the columns of NATURE, you would give me the names of the best books in English or German which deal with soil (agricultural) bacteria.

THOS. T. WATSON.

Rosely Cottage, Collier Street, Carnoustie, March 26.

[1] See *Phil. Mag.* for January 1860, p. 22; or vol. i. of his "Collected Papers," p. 380.

[2] See *Scientific Transactions* of the Royal Dublin Society, vol. vi. Part 13; or *Astrophysical Journal* for January 1898.

THE BIRDS OF AFRICA.[1]

THE appearance of the first volume of the history of its fauna at a time when all our attention is concentrated on South Africa is doubtless in a great degree accidental, although nevertheless opportune, if only as a reminder that, when the present period of stress and anxiety has passed away, there are matters other than war and armaments demanding our attention in that part of the globe. It was a fortunate circumstance that the editor of the series—Mr. W. L. Sclater—was able to secure for his first volume the valuable services of a local ornithologist, well acquainted, not only with the birds themselves, but likewise with their habits and the localities they frequent. Unhappily, the labours in this world of Mr. Stark are ended for ever, his career having been terminated, as he stood at the door of his own house in Ladysmith, by the fragment of a shell which struck him dead almost on the spot. It is said that his last words were "Take care of my Cat.," doubtless referring to the present volume and its successor, of which latter we believe we are right in saying that the MS. was complete at the time of the author's death.

The fact that Mr. Stark's last thoughts were for the safety of the scientific labours on which he had expended so much time and care confers a pathetic interest on the appearance of the volume before us, and should go far to disarm hostile criticism, were such otherwise called for. Fortunately, however, under no circumstance would there apparently be much, if any, room for unfavourable comments, as the execution of the work seems, from all points of view, excellent and praiseworthy.

As stated in the editorial preface, the series, of which the present volume forms the first instalment, is intended to deal with the fauna of that portion of Africa lying to the southward of the Zambesi and Cunene Rivers; and therefore includes, not only the very peculiar and restricted assemblage of animals characteristic of Cape Colony and the other districts south of the Orange River, but likewise embraces a large stretch of country whose animals have a wider geographical range. The present volume treats of about one-half the total number of Passerine birds met with in the area under consideration. It is satisfactory to find that in the treatment of his subject the author has seen fit to follow, so far as practicable, the plan of arrangement and description adopted by Mr. Oates in his contribution to the "Fauna of British India," under the editorship of Mr. Blanford.

Accordingly we have, so far as the present volume and its successor are concerned, the great advantage of having the faunas of the two great old-world dependencies of the British Empire described on a similar plan, so that they are readily comparable with one another. And how extraordinarily different is the avian fauna of South Africa—especially as regards its host of peculiar generic types—from that of Peninsular India, can be ascertained at a glance by comparing the systematic index in Mr. Oates' volumes with that in the one now before us.

And yet, in spite of this great general faunistic difference, there are in birds, as in mammals, a certain number not only of generic, but likewise of specific types common to the two areas. A case in point is afforded by the Spotted Creeper (*Salpornis spilonotus*), the African and Indian forms of which are regarded by the author as worthy only of subspecific distinction. And we are glad to notice that, not only does the author give an unqualified adherence to the manifold advantages offered by trinomial nomenclature, but that he has not been frightened by the bugbear of "absence of connecting links" out of regarding closely allied, but widely separated, forms as local races. Consequently we have the African representative of the Spotted Creeper appearing as *Salpornis spilonatus salvadorii*, while the Indian form would be distinguished as *typicus*.

As regards the general scheme of classification for the "orders," the author adopts the one proposed by Dr. P. L. Sclater in 1880; and although there are doubtless some respects in which this scheme is susceptible of emendation, it is a very workable one, and has the great merit of simplicity.

A somewhat instructive parallel may be drawn between the avian and mammalian faunas of South Africa. Attention has been already drawn to the large number of genera of birds peculiar to Africa, notable examples occurring in the families of the Starlings (*Sturnidae*) and Weaver-Birds (*Ploceidae*); and these may be regarded as the analogues of the many genera of Antelopes likewise characteristic of the country. But the parallelism by no means stops here. Many of the species of the Antelopes (as well as other mammals) are restricted to the area south of the Orange River, but are represented by kindred types in the districts to the northward of that river. Among the Passerines we may notice the Red-shouldered Glossy Starling (*Lamprocolius phaenicopterus*), the Cape Weaver-Bird (*Sitagra capensis*), the Cape Long-tailed Sugar-Bird (*Promerops cafer*), and the Cape Sun-Bird (*Cinnyris chalybeus*), as well-known species exclusively confined to the Cape, but represented in Natal or in districts still further north by allied species or subspecies, precisely in the same manner as are Antelopes. Evidently, therefore, we have to deal with some deep-seated cause which has modified a large portion of the Cape fauna; but the exact nature of this cause has yet to be worked out.

So far as we have had an opportunity of testing them, Mr. Stark's diagnoses and "keys" to the various genera and species he describes are all that the most exacting ornithologist can desire. But the work is very imperfectly described, as in the lamented author's last words, as a mere "Catalogue." On the contrary, it contains some delightful and interesting descriptions of the habits and mode of life of African birds, of which we know far too little. Witness, for instance, the author's description of the "showing-off" of the male of the Cape Long-tailed Sugar-Bird,[1] the illustration of which we are enabled by the courtesy of the publisher to reproduce. "Towards the end of April or beginning of May," he writes, "the males, when not feeding, fighting, or chasing one another with shrill cries, may be usually seen perched on the summit of some prominent bush or young pine-tree, their long, flexible and curved central tail-feathers blowing about in the wind, often in a reversed curve over the bird's head. At intervals one of them will mount twenty or thirty feet in the air, incline his body backwards, violently jerk his tail up and down, and at the same time rustle the feathers together, and bring his wings with sharp, resounding 'claps' against his sides, before returning to his perch to indulge in an outburst of song. Occasionally a male may be seen to throw the longer tail-feathers into a double curve. At the same season the hens amuse themselves by flying round and round in a small circle."

Special attention has been devoted to nidification, and the illustration on page 76, in which the nests of three species of Weaver-Birds are shown in a single tree, is highly noteworthy. Still more remarkable are the three enormous dome-shaped nests of the Sociable Weaver-Bird depicted in the photogravure on page 117. Before dismissing the subject of illustrations, it may be mentioned that these are in the main restricted to the head and wing, which afford the best diagnostic features of the species described. The mere fact that they were drawn by Mr. Grönvold is a sufficient guarantee of the excellence of their execution.

[1] "The Fauna of South Africa. Birds." Vol. i. By A. C. Stark, M.B. Pp. xxx + 322, illustrated. (London: R. H. Porter, 1900.)
"The Birds of Africa, comprising all the Species which occur in the Ethiopian Region." By G. E. Shelley. Vol. i. 1896. Vol. ii. Part i. (London: R. H. Porter, 1900.)

[1] The only error we have hitherto noticed in the book is that this illustration is lettered *Promerops capensis* instead of *P. cafer*.

In conclusion, we can scarcely bestow a higher meed of praise on the labours of the late Mr. Stark than the expression of the hope that the subsequent volumes of this valuable series will be equal in merit to the one before us.

Captain Shelley's work covers a much wider field than that of Mr. Stark, embracing all the birds of Africa south of the Tropic of Cancer, together with those of Madagascar and other islands off the African coast. In other words, it describes the avi-fauna of the Ethiopian region in its more extended sense. A notable feature of the second part is the beautiful series of coloured plates with which it is illustrated; the portraits of the birds, as in all Mr. Grönvold's work, being remarkably true to nature, and at the same time forming artistic pictures.

The first volume, which made its appearance four years ago, consists of a classified list of the genera and species of African birds, with references to the works in which the names first appeared. With the second volume commences the descriptive portion of the work; the first part, in addition to containing the Angola Pitta and the two other Ethiopian representatives of the "Oligomyodæ," being devoted to the beautiful Sun-Birds, or *Nectariniidae*.

With reference to the plan of the work, it is stated in the Introduction that it will "consist of a series of handy volumes complete in themselves," and that the second volume "will be an acceptable work to the Field Naturalist, for whom many of the notes will be specially intended." From these statements, we venture to think, it may be inferred that the work is intended to supply all the needs of the African ornithologist when working alone in the wilds, far away from a library. But, on examination, we doubt whether this is altogether the case. For example, when the genus of a bird has been changed, there is in most cases no possibility of finding out the name under which it was originally described; the references in the first volume merely giving the name of the author of the species, and the place and date of publication, without mention of the genus. Neither can we approve of the mode of arrangement of the references themselves. We have, for instance, on page 89, the following, viz.:—*Chalcomitra senegalensis* (Linn.), Shelley, B. Afr. i. No. 47 (1896); *Cinnyris senegalensis*, Shelley, Mon. Nect. p. 267 (1878); *Nectarinia senegalensis*, Bocage, J. f. O. 1876, p. 435. Apart from the omission of the references to the Linnean genus and place of publication, the arrangement of these references is, we venture to submit, totally unjustifiable, and they should have been put in just the reverse order, when they would accord with their chronology. As a matter of fact, references to the author's previous works are, we think, a great deal too prominent. Moreover, the complicated system on which the author makes his references is liable to lead to great confusion in the event of any typographical error. For example, the omission of a couple of brackets on page 95 of the first volume would lead the reader to believe that a certain bird was described as *Newtonia brunneicauda* three years before the date of publication of the generic name!

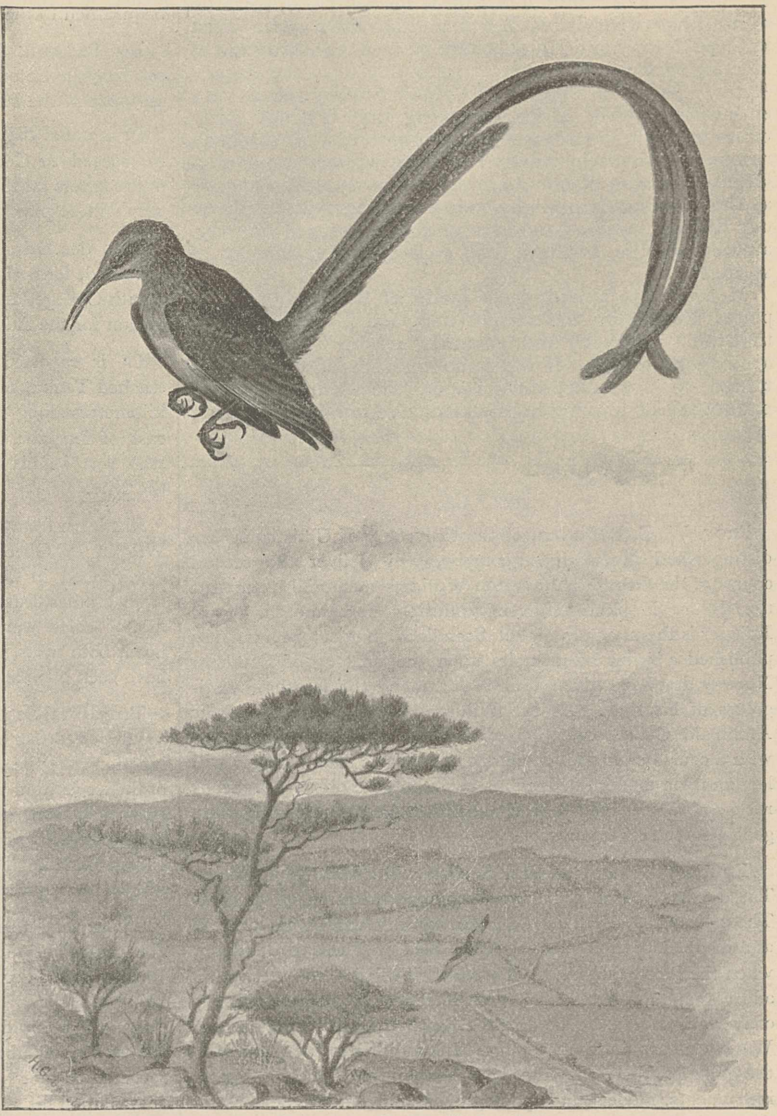

FIG. 1.—Male of the Cape Long-tailed Sugar-Bird "showing-off." From Stark's "Birds of South Africa."

In regard to the arrangement of the families of Passeres, it is a matter for regret that there is much divergence between the present work and that of Mr. Stark. In the latter the arrangement is from the highest to the lowest, commencing with the *Corvidae*, and concluding with the "Oligomyodæ." Captain Shelley, on the other hand, adopts the opposite plan, commencing with the "Oligomyodæ"; and in this he is no doubt perfectly justified, although we are at a loss to ascertain why he follows on immediately with the *Nectariniidae*, which are usually placed near the middle of the series. Apart from this, we have no hesitation in saying that, in a matter which is really of no importance at all, it would be a great convenience if ornithologists could agree to follow the same method of arrangement of the families. And this reminds us that there is another difference between Captain Shelley and

Mr. Stark; the former adding the termination "formes" to the names of the orders. Here again, although we regard the addition of the termination in question as totally superfluous, and at the same time ugly, we should be quite prepared to sacrifice our personal prejudices for the sake of uniformity.

The descriptive portion of the work appears to be carefully executed, and the "keys" seem to be well drawn up. While quoting from the publications of the numerous field-naturalists who have written on African ornithology, Captain Shelley is by no means dependent altogether on the observations of others for his accounts of the habits of many of the birds he describes, since he himself has twice visited Egypt, and has likewise travelled in Cape Colony and Natal, where he had the advantage of meeting such well-known local ornithologists as the Messrs. Ayres. As a "bird-country" Captain Shelley speaks very enthusiastically of Africa, observing that it "may fairly claim to be the metropolis of the song-birds, for the bush resounds with their melody; it is the winter home of a large proportion of our most attractive small birds, such as the nightingale and the many warblers which enliven our English gardens and surrounding country in summer, as well as the swallow, our well-known harbinger of spring."

Mention has already been made of the beauty of the plates illustrating the second part, and it may be added that the typography and general "get-up" of the work are beyond praise. If the same high standard be maintained in the succeeding issues, the complete work cannot fail of proving highly attractive to all bird-lovers.

R. L.

NOTES.

PROF. W. C. BRÖGGER, of the University of Christiania, the distinguished Norwegian geologist, will deliver the second course of the George Huntington Williams memorial lectures at the Johns Hopkins University during next month. Prof. Brögger is the most prominent Scandinavian geologist, and has published a series of memoirs upon the geology of Southern Norway that have given him rank among the leading investigators of his time. As the Williams lecturer, he follows Sir Archibald Geikie, who opened the lectureship two years ago with a course upon the founders of geology. Prof. Brögger will lecture upon modern deductions regarding the origin of igneous rocks, a subject that has commanded the attention of many geologists in recent years.

THE Royal Meteorological Society will attain its jubilee on Tuesday next, April 3, having been founded on April 3, 1850. The celebration of this fiftieth anniversary will be commenced at a commemoration meeting to be held on Tuesday afternoon, when the President, Dr. C. Theodore Williams, will deliver an address, and delegates from other societies will be received. A conversazione will be held at the Royal Institute of Painters in Water Colours in the evening. In addition to the pictures in the galleries, there will also be an exhibition of meteorological instruments, models and photographs, and lantern demonstrations will be given by Colonel H. M. Saunders, Mr. T. C. Porter and Mr. W. Marriott. On Wednesday, April 4, there will be an excursion to the Royal Observatory, Greenwich, and a dinner at the Westminster Palace Hotel. As a memento of the jubilee of the Society, a bronze commemoration medal, bearing on the obverse a portrait of Luke Howard, F.R.S., has been struck.

THE New York *Electrical Review* states that the North German Lloyd has decided to equip all its swift steamships with wireless telegraphy apparatus to announce their proximity to the German coast. The *Kaiser Wilhelm der Grosse* has been equipped with the necessary instruments, and a similar outfit has been installed on an island near the mouth of the Ems in the North Sea. The ship will thus be able to exchange signals with the mainland long before she is sighted, or has passed out of view when outward bound. The question of installing wireless telegraphy apparatus on the Nantucket Shoals Lightship, off the Massachusetts coast, is under consideration by the Lighthouse Board. Incoming steamships, similarly equipped, could thus be reported many hours before they could be sighted at Fire Island.

M. CREVAT-DURAND, who recently died at Fontainebleau, bequeathed to the Pasteur Institute the sum of 100,000 francs.

DR. PATRICK MANSON, professor of medical pathology at the English Colonial School of Medicine, has been elected an associate of the Paris Academy of Medicine.

IT is stated that Dr. Edward Ehlers, of Copenhagen, is about to proceed to Crete to make arrangements for the segregation of the lepers on the island. There are about 2000 of these, and they will be placed on a small island off the north coast.

THE death is announced, at New York, of Dr. Oliver P. Hubbard, formerly professor of chemistry and geology in Dartmouth College, and one of the founders of the American Association for the Advancement of Science.

IT is reported that the Lemaire scientific expedition has reached Tenka, after a successful and peaceful journey of 3000 kilometres along the border of the Congo State. Three days east of Lualaba Mission the expedition met Major Gibbons, who was on his way to Tanganyika, *via* Lafoi, and thence to the Nile.

WE learn from *Science* that under the direction of Prof. A. A. Wright, of Oberlin College, systematic excavation has been commenced in Brownhelm, Ohio, near Lake Erie, and about twelve miles from Oberlin, to recover mastodon remains, the first of which were discovered several years ago. The jaws and head, both tusks, together with a number of ribs and vertebræ, have been obtained in a good state of preservation.

THE Royal Scottish Geographical Society proposes to organise a purely Scottish expedition to the South Pole to work in conjunction with the British and German expeditions. The sphere of the expedition will be the Weddell sea quadrant, south of the Atlantic Ocean, while the British expedition will explore to the south of the Pacific Ocean and the Germans to the south of the Indian Ocean. The leader will be Mr. William S. Bruce, who visited the Antarctic regions in 1892 and 1893.

AT the last meeting of the Paris Société d'Encouragement, the president, M. Carnot, referred to the death of Prof. S. Jordan, a member of the council of the society. Prof. Jordan was professor of metallurgy at the École centrale for many years, and was widely known among metallurgists and engineers. He was a member of the Comité des Arts chemiques, and represented more particularly the metallurgy of iron and steel. It was upon his report that the society awarded the Lavoisier medal to M. Osmond in 1897, for his excellent researches on the microstructure of steel.

IT was mentioned last week (p 498) that a gentleman had given the Scottish Meteorological Society a donation of 300*l.* to be spent during the next two years in the discussion of the results of the observations made on Ben Nevis and at Fort William since 1881. By the patriotic generosity of Mr. Mackay Bernard, of Dunsinane, whose three donations amount to 1500*l.*, the observations will be carried on to the end of next year. By the 1000*l.* presented by the Royal Societies of London and Edinburgh, the hourly and other observations will be printed *in extenso*, filling three volumes of the *Transactions* of the Royal Society of Edinburgh; and by the gift of 300*l*. just received, the necessary clerical assistants will be engaged to enable Dr. Buchan and Mr. Omond to lay before the scientific world the results of the great experiment in meteorology undertaken by the Directors of the Ben Nevis Observatories in 1881.

THE Society for the Protection of Birds had a year of active work to report upon at the annual meeting held a few days ago. Several new publications were issued during 1899, fifty-nine lectures were delivered, and petitions were sent to various responsible officials and authorities asking for their sympathy and assistance. Steps have been taken to prevent the exportation of Birds of Paradise and others from British New Guinea; the wearing of osprey plumes by the officers of the Hussar and Rifle Regiments, and of the Royal Horse Artillery has been discontinued; and turbans have been substituted for the caps with birds' plumes formerly worn by the body-guard of the Viceroy of India. Orders for the protection of rare birds are in force in many parts of the British Isles, but they are numerous and complicated instead of being few and simple. Efforts are now being made to obtain Parliamentary approval of a Bill designed to simplify and consolidate the present law relating to the protection of wild birds, and practically introducing but one change, viz. to extend protection during a close time to all species and their eggs, leaving it to the various county councils to obtain orders exempting from protection within their administrative areas those species which are destructive or too numerous. Such a law would be far easier to understand and administer than the present intricate and varying regulations.

AN admirable summary of the methods used to preserve food in ancient as well as in modern times was given by Dr. S. Rideal in a paper read before the Society of Arts on March 21, and published in the current number of the Society's *Journal*. There are only a few early allusions to the use of salt, vinegar, and allied substances, to keep food from putrifying, and none of much importance. After either smoking, salting or drying, the characters of fresh food cannot be restored. It was not till the middle of the nineteenth century that it was discovered that small quantities of certain antiseptics would enable the original qualities to be retained, and prevent decay for a considerable period, with less influence on digestion than the old curing processes. Utilising the fact that fermented liquids remain stable for long periods, Bethell, in 1848, patented a process for preserving milk, which consisted in first boiling the milk to expel all the air contained in it, and then saturating with carbon dioxide; when so treated, the liquid remains fresh for a long time after being opened. More recently, compressed oxygen and sterilised air have been tried for preserving milk. Butter, when kept in carbonic acid, at a pressure of six atmospheres, often remains unchanged for four or five weeks. But experiments have shown that carbonic acid, though generally effective for mineral waters, will not of itself prevent changes in milk or meat. Moreover, with regard to the sterilising effect of carbonic acid in mineral waters, Dr. Otto Hehner has examined many such waters and found them swarming with germ life. A great variety of substances containing boric acid are used as preservatives at the present time, especially for the preservation of milk, cream and butter. There is, however, a wide difference of opinion as to the effects of preservatives upon the consumer of the food containing them, and the whole matter is being inquired into by a Departmental Committee.

THOUGH we have as yet no agricultural experiment stations comparable with those of the United States, Canada, and elsewhere, a large amount of valuable work is being done in connection with the agricultural departments of some of our colleges, and by other organisations. Two reports upon work of this kind are before us—one containing the results of experiments made by the agricultural department of the Yorkshire College, Leeds, and the other the *Proceedings* of the Agricultural Research Association, N.B. One of the papers in the former describes the effect of various fertilisers upon the production of meadow hay, which is one of the chief objects of farmers in the West Riding. Among the conclusions arrived at as the result of experiments on clover are:—"Nitragin, a preparation used for inoculating soil with the bacteria that are found in the nodules of clover roots, did not increase the crop."

THE work of the Agricultural Research Association covers a very wide ground. Among the useful results of the investigations carried on under its auspices are the following:—The demonstration that insoluble finely ground mineral phosphate is as effective as soluble phosphate. This led to the use of coprolite and phosphatic slag instead of soluble phosphate, and thereby reduced the cost of phosphate to the farmer; discovery of aperture in root hairs, giving explanation of the absorption into the plant of insoluble matter; determination of the relative values of different forms of nitrogenous, phosphatic and potash manures—leading to economy in the selection of manures for turnip, grain and grass crops; the cause of finger and toe disease in turnips, and means for its prevention. Inquiries on grasses and clovers which indicated the most economical grass mixtures to use, explained the poorness of rye grass pastures and showed the remedy, and led to the suggestion of a ten-year rotation. This rotation is being gradually adopted in practice. The chief subject of investigation at present on hand is an inquiry into the natural cross-fertilisation of oats. Most satisfactory confirmation has been got, during the past season, that an increase is attainable in the yield of the oat crop by means of such natural cross-fertilisation. It now appears that the increase that may be expected by this method is substantial, and it is one which does not involve any outlay. Experiments are now being made to discover the cross which gives the best crop. Several other inquiries are being proceeded with, but they have not reached the stage for being reported upon. We regret to see that certain of these inquiries have had to be stopped for want of funds, but it is hoped that an opportunity may soon arise to admit of their being taken up again.

WE have received from Mr. R. C. Mossman a paper on the price of wheat at Haddington from 1627 to 1897, read before the Scottish Society of Economists. It is a rather laborious compilation, for, although dealing with local wheat prices, it necessarily has to take into consideration many conflicting interests outside the district under discussion. The results are clearly shown in a diagram, in which the mean annual temperature of the air in Edinburgh since 1764 is also given. Previous to this date the observations were non-instrumental, and have been extracted from such works as Lowe's "Natural Phenomena." The relation between temperature and prices during most of the period for which the information is available is clearly seen on looking at the two curves. The most extraordinary prices occurred in 1800 and 1812 (134*s*. 9*d*. per quarter). In the first case heavy rains and a low temperature in August were the chief causes. In the second case the high price was due to the deficiency of the 1811 crop, without the means of obtaining any from abroad. Since 1873, when the price was 64*s*. 8*d*. per quarter, there has been a rapid and almost uninterrupted fall in price, the most potent factor being the great increase in the wheat area of the United States, the lowest figure being 26*s*. 6*d*. in 1894. The rise in 1879 (53*s*. 6*d*.) was due to the unparalleled severity of the weather, and the rise in 1891 (46*s*. 7*d*.), although the wheat harvest of the world was much above the average, was due to a severe and extensive rye famine in Russia, and to the supply in two preceding years being inadequate to meet the demand.

FROM a lengthy "note" on the food supply of the United Kingdom, Belgium, France and Germany, by Mr. R. F. Crawford, published in the *Journal* of the Royal Statistical Society, we learn that the average dietary of an inhabitant of the United Kingdom contains a much larger quantity of meat than that of a Belgian, Frenchman or German, but a smaller proportion of bread and potatoes. In Belgium more bread and less meat are consumed than in any of the other countries named, while in France a noteworthy feature is the apparently small consumption of milk. In the case of potatoes, the consumption per head in Belgium and Germany is about three times that in Great Britain, but the considerable requirements are largely accounted for by their use in the manufacture of starch and spirits.

A FRESHWATER chert from Asia Minor is the text of Mr. W. T. Haydon's presidential address to the Liverpool Microscopical Society, published in the thirty-first annual report of the society. The investigation was based on the examination of some "worked flints" from Hermanjik imported in cargoes of horse beans, which flints were doubtless used as teeth in threshing boards. Thin sections of the flints were remarkably rich in organic remains, chiefly vegetable, including mosses, hepaticæ, pine pollen grains, ferns and grasses; while remains of mollusca, diptera, spiders, &c., were also found. In endeavouring to explain the origin of the silica forming this deposit, an examination was made of some chalk-rock from the same neighbourhood, and this was found to contain great numbers of unmistakable remains of *diatoms*, but all traces of these disappeared in hydrochloric acid. Mr. Haydon's conclusion is that the flint deposits derived their silica from the diatoms, and that the forms of the latter, as represented in the calcareous rocks, were "pseudomorphs" whose silica was replaced by calcite.

A NOTE on the use of formalin as a preventive of silkworm disease is given by Prof. G. Gianoli and Dr. E. Zappa in the *Rendiconti del R. Istituto Lombardo* (xxxiii. 2, 3). From experiments, it appears that the diffusion of formic aldehyde in the form of vapour through the breeding chambers containing the silkworms is not a certain preventive of *Botrytis bassiana*, a fact which the authors attribute in some degree to its high chemical activity, which causes it to be absorbed by various objects, such as the leaves on which the silkworms feed. On the other hand, the presence of this vapour produces a diminution in the weight of the cocoons, and a deterioration in the quality of the silk.

IN the March number of the *Zoologist* Mr. J. H. Gurney gives a plate of one of the so-called "Chestnut Partridges" which have recently excited so much interest in Norfolk. It is stated that a local race of these abnormally coloured birds has now been established; the figured specimen being the twelfth example.—The same number contains the conclusion of Mr. Distant's article on "Mimicry," to which allusion has been previously made in these columns. "The theory of mimicry," writes the author in his concluding paragraph, "is probably the still imperfect recognition of a great truth which is struggling to survive a mass of more or less irrelevant evidence too frequently offered in its support. . . . Whatever view be held, this alone is certain, that the theory in either its demonstrated or suggestive enunciation has been the means of a vast record of facts pertaining to the life-histories of animals and plants which would otherwise have remained either unobserved or disregarded." Can more be demanded in favour of any working hypothesis?

MISS ELEANOR ORMEROD'S Twenty-third Report on Injurious Insects and Common Farm-Pests has just been issued. From this it appears that during 1899, in addition to the ordinary insect infestations, there occurred developments of species which had hitherto been but little noticed. None of the infestations were of a very serious nature, with the exception of the so-called Turnip-Fly, which was very prevalent in some parts of the country. A feature of the year was the marked absence of injury to root-crops by visitations of Caterpillars and Mangold-leaf Fly; while orchard-haunting Caterpillars, as well as those of the Gooseberry and Currant Moth, were likewise conspicuous by their absence. Hessian Fly was recorded from one district; and in this connection the author draws the attention of farmers to the extreme importance of destroying wheat-screenings containing the pupæ of this noxious insect, in the form of the so-called "flax-seeds." A section of the Report is devoted to the Grouse-Fly, which appears to be more interesting to the entomologist than harmful to the prospects of the sportsman, as there is no evidence that it does any appreciable injury to the birds it infests. The remarkable structure of the foot of this insect is illustrated by a tinted plate.

A *Bulletin* of the U.S. Department of Agriculture, by Dr. L. O. Howard, on "Some Results of the Work of the Division of Entomology," bears testimony to the attention bestowed on the mitigation of insect ravages to crops and trees on the other side of the Atlantic. A large portion of this *Bulletin* is devoted to the life-history of two scale-insects of the genus Pulvinaria, which infest maples, and occasionally do much harm to these valuable shade-trees in various parts of the Eastern United States. The second article treats of the insects which (together with the newspapers, as it is significantly remarked) were responsible for the so-called "kissing-bug scare" of the past summer; the account being published mainly to satisfy a shoal of inquirers as to the truth of the newspaper stories. Reports on the devastation caused by locusts in the Western Territories during 1899 are of especial interest at the present time on account of the recent arrival of flights of the true Rocky Mountain Locust (*Melanoplus spretus*) in certain districts of the North-west. Housekeepers in many parts of the world will be interested to hear of a simple Australian remedy for cockroaches, consisting of a mixture of flour and plaster of Paris, which is greedily eaten by the insects and rapidly sets in their stomachs.

YET another serial dealing, in part, with insect pests is to hand, in the form of the February number of the *Agricultural Gazette of New South Wales*. The contents of this number are varied and interesting, ranging from such subjects as the proper mode of stretching barbed wire and the pruning of plants, to the incubation of eggs and the bacteria found in milk and butter. It is undoubtedly a healthy sign when it is considered advisable to teach poultry-raisers the changes which take place in eggs during incubation; and the excellent figures with which these changes are illustrated, as also those displaying milk and butter bacteria, are worthy of the highest commendation.

M. E. LAURENT has an interesting note, in the *Bulletin* of the Royal Botanical Society of Belgium, on the distribution of the mistletoe in that country. While almost entirely absent from Holland, its distribution in Belgium is local, and appears to have some connection with the nature of the soil, preferring that of a calcareous character. The trees on which it either grows naturally, or has been artificially inoculated, are *Salix viminalis* and *grandiflora* (?), the fig, the olive, *Eucalyptus globulus*, the oleander, the hawthorn, the apple, the pear, the medlar, the quince, and *Malus spectabilis*. On *Ficus elastica*, *Spartium junceum*, and some varieties of the pear, the berries of the mistletoe appeared to produce a poisonous effect. Somewhat similar results as to the influence of the nature of the soil on distribution were obtained with the dodder.

UNDER the title of "The Temple Cyclopædic Primers," Messrs. Dent and Co. have commenced the publication of a series of little volumes on scientific and other subjects. "An Introduction to Science" is written by Dr. Alex Hill; and an "Ethnology" has been translated by Mr. J. H. Loewe from the German of Dr. Michael Haberlandt. The books are both attractive in appearance and instructive in contents; and they should bring a large public in touch with scientific work and thought. Students of science as well as general readers will find the volumes well worthy of consideration.

THE advantages of quartz as a thermometric substance as compared with glass are well known, and since the well-known experiments of Mr. Boys on the behaviour of quartz near its melting point, numerous attempts have been made to produce quartz tubes. M. Dufour, who describes his experiments in the current number of the *Comptes rendus*, has been able to constructs quartz thermometers, two of which are described, one carrying tin as the thermometric liquid, and hence suitable for temperatures from 240° C. upwards. The second instrument contained mercury, and was constructed with the view of comparing its zero residues with those of a glass thermometer.

THE limiting value for the molecular depression of the freezing point in solutions of non-electrolytes, when these solutions become infinitely dilute, is a constant of great importance in the van 't Hoff theory of solution. Raoult, Abegg and Loomis are among the best known workers in this field, and in the current number of the *Zeitschrift für physikalische Chemie* is a contribution by the last named in which the numerous difficulties surrounding the apparently simple operation of taking a freezing point are discussed. The limiting value in extreme dilution for the molecular depression of a considerable number of non-electrolytes of different constitution is the same, 1·86. This is exactly the figure which is obtained from van 't Hoff's well-known formula, if the latent heat of fusion of ice be taken as 79·3 thermal units.

THE additions to the Zoological Society's Gardens during the past week include a Sooty Mangabey (*Cercocebus fuliginosus*, ♀) from West Africa, presented by the Rev. A. Christopher; a Jaguar (*Felis onea*) from Brazil, presented by Mr. Rodriques; two Martinican Doves (*Zenaida aurita*) from the West Indies, presented by Mr. G. R. Phillipps; a Great Bustard (*Otis tarda*, ♂), European, presented by Mr. E. G. B. Meade-Waldo; a Pine Marten (*Mustela martes*) from the Spanish Pyrenees, a Snowy Owl (*Nyctea scandiaca*), European; a Pin-tailed Sand Grouse (*Pterocles alchata*), a Slender-billed Gull (*Larus gelastes*) from Southern Europe, deposited.

OUR ASTRONOMICAL COLUMN

ASTRONOMICAL OCCURRENCES IN APRIL.

April 2. 13h. Venus in conjunction with the moon. Venus 0° 46′ N.
2. 13h. 2m. Jupiter's Sat. IV. in conjunction with N. pole of planet.
3. 8h. 47m. to 9h. 44m. Moon occults B.A.C. 1373 (mag. 5·7).
3. Venus 2° S. of the Pleiades.
4. 9h. 30m. to 10h. 22m. Moon occults *o* Tauri (mag. 4·8).
8. 11h. 46m. to 12h. 45m. Moon occults *α* Cancri (mag. 4·3).
15. Illuminated portion of disc, Venus = 0·576; Mars, 0·985.
17. 12h. 56m. Minimum of Algol (*β* Persei).
18. 10h. 36m. to 11h. 42m. Moon occults 24 Ophiuchi (mag. 5·6).
19. 15h. 9m. to 16h. 28m. Moon occults B.A.C. 6088 (mag. 6·0).
20. 9h. 45m. Minimum of Algol (*β* Persei).
20. 12h. 37m. to 13h. 30m. Moon occults 33 Sagittarii (mag. 6·0).
20. 14h 27m. to 15h. 42m. Moon occults ξ^2 Sagittarii (mag. 3·5).
20. Epoch of the April meteoric shower (Lyrids, radiant 271° + 33°).
20. Saturn. Outer major axis of outer ring, 40″·18.
20. Saturn. Outer minor axis of outer ring, 17″·52.
22. 12h. 12m. to 13h. 49m. Transit of Jupiter's Sat. III.
23. 15h. 23m. to 15h. 57m. Moon occults c^1 Capricornii (mag. 5·2).
24. 14h. 20m. to 15h. 12m. Moon occults *κ* Aquarii (mag. 5·5).
28. 12h. Venus at greatest elongation, 45° 30′ E.
28. Perihelion passage of Giacobini's comet (1900*a*).
29. 15h. 37m. to 17h. 14m. Transit of Jupiter's Sat. III.

COMET 1899 V.—Herr S. K. Winther continues his ephemeris of this comet in the current issue of the *Astronomische Nachrichten* (Bd. 152. No. 3631).

Ephemeris for 12h. Berlin Mean Time.

1900.		R.A. h. m. s.		Decl. ° ′
April 2	...	21 59 40	...	+55 38·0
4	...	22 3 59	...	56 20·9
6	...	8 18	...	57 3·4
8	...	12 40	...	57 45·5
10	...	17 3	...	58 27·2
12	...	22 21 28	...	+59 8·5

NEW SYSTEM OF SPECTRUM PHOTOMETRY.—In the *Astrophysical Journal*, vol. xi. pp. 6–24, January 1900, Prof. D. B. Brace, of Nebraska University, describes a new system of spectral photometric work, the foundation of which depends on a novel method of bringing into juxtaposition the two illuminated areas, the intensities of which are to be compared. This is done by using for the dispersion-piece of the spectro-photometer a compound prism, one-half of which has been silvered along a narrow strip of its face before being cemented to the other. Two collimators are then used, one sending light through the prism in the customary manner, the other so arranged that after internal reflection from the silvered strip and subsequent refraction, the resulting spectrum is received in the same telescope as that from the first source. The two spectra will thus be seen in absolute juxtaposition, and perfect equality will be denoted by the disappearance of the junction line between them. The instrument is stated to be so sensitive that good comparisons could be made on the star Capella, using a 4-inch telescope, so that with larger telescopes the spectra of all the brighter stars could be accurately compared.

VARIABLE STARS OF THE ALGOL TYPE.—In *Popular Astronomy* for March 1900, Mr. H. C. Wilson, of Goodsell Observatory, Minnesota, U.S.A., gives a very complete synopsis of the eclipse theory of the Algol type of variables. In the computations of the possible light curves due to eclipses, formulæ are given for both circular and elliptic orbits, and for direct and oblique line of sight. Using Vogel's values for the radial velocity of the star during the period, there is some little disagreement in the resultant curves, which is too systematic to be put down to errors of observation. Dr. Vogel considering that part of this might be due to the possibly existing atmospheres surrounding the stars having been neglected, Dr. Wilsing calculated anew the resultant light curve, which is so nearly in accordance with the actual one observed as to leave little doubt of the accuracy of the assumption. In this particular Mr. Wilson makes an interesting suggestion with respect to the light curve of the class of variables represented by *β* Lyræ, showing how, by considering certain values of distance, intensity, and extent of atmosphere of two bodies, a curve of light variation may be found very closely agreeing with that deduced spectroscopically.

COMPUTATION OF ORBITS OF SPECTROSCOPIC DOUBLES.—Dr. K. Schwarzschild, of Munich, contributes to the *Astrono-*

mische Nachrichten (Bd. 152, No. 3629) an extension of the method put forward by Lehmann-Fildes (*Astr. Nach.* No. 3242) for the determination of the orbit of a spectroscopic double from the observation of the velocities in the line of sight. The chief characteristic of the present method is that the solution by mechanical quadratures formerly necessary is here dispensed with, thus somewhat simplifying the problem.

MALARIA AND MOSQUITOES.[1]

OUR knowledge of the disease called malarial fever first emerges from chaos in the seventeenth century, when, owing to the recent discovery of quinine, the great Italian physician, Torti, was able to differentiate this malady from other fevers, and to describe its symptoms with accuracy. Next century Morton, Lancisi, Pringle and others observed the connection of the disease with stagnant water and low-lying ground, and first emitted the theory—which in one form or another has found general acceptance up to the present date—that the fever is due to a miasm which rises from the soil or water of malarious localities. The next great advance was made in the middle of the nineteenth century by Meckel, Virchow and Frerichs, who ascertained that the distinguishing pathological product of the disease is a black substance, which is distributed in collections of minute coal-black or brown granules in the blood and organs of patients, and which is called the *malarial pigment* or *melanin*. This line of research culminated in the great discovery of Laveran in 1880—to the effect that the melanin is produced within the bodies of vast numbers of minute parasites which live in the red blood-corpuscles of the patient.

Ray Lankester had already opened the science of the parasitology of the blood by his discovery of *Drepanidium ranarum* in frogs; and it was at once apparent that the parasites found by these two observers are somewhat nearly allied—that is, that Laveran's parasite is a *Protozoal* organism, and not a vegetable one like the pathogenetic organisms recently discovered by Pasteur, Lister, Koch and many others. And our knowledge of the subject was quickly increased by the discovery of similar hæmatozoa in certain species of reptiles, birds, monkeys and bats, and in cattle, by Danilewsky, Kruse, Labbé, Koch, Dionisi, Smith and Kilborne. In 1885 a further advance was made by Golgi, who ascertained that the human parasites propagate within the body of the host by means of ordinary asexual spore-formation; that the exacerbations of fever in a patient are coincident with the disruption of the clusters of spores produced by the organisms; and that there are at least three varieties of the parasites in man in Italy. These observations were confirmed and extended by a large number of persons working in various parts of the world—most prominent among whom are Marchiafava, Celli, Vandyke Carter, Grassi, Osler, Bignami, Antolisei, Councilman, Mannaberg, Romanowsky, Labbé, Koch, Manson, Thayer and MacCallum. In short, the work of all these observers, and of many others scarcely less meritorious, has not only absolutely established the fact that the parasites are the cause of malarial fever, but has given us a very thorough knowledge both of the parasites themselves and of their pathological effects, direct and indirect; until the science of malaria —for it may almost be described as a science in itself—has become a brilliant exemplar of the modern methods of research as regards the science of disease in general.

But I am not here concerned with questions of pathology in malarial fever. At the conclusion of the labours to which I have just referred, we had, it is true, grasped the nature of the disease itself; but a question of the greatest moment still required an answer. We had studied side by side the morbid process and the parasites which cause it; but we had still to find out how infection is caused, how these parasites effect an entry. We had ascertained the life-history of the parasites within man, and of the kindred parasites within other animals; but, even after all these investigations, the life-history of the parasites *outside* man and *outside* other vertebrate hosts remained to be discovered. Until this was done our knowledge was not complete. It is now my privilege to describe the interesting theories and investigations which led to the solution of this great and difficult problem.

The *importance* of the problem need not be enlarged upon. In the British army in India during the year 1897, out of a total strength of 178,197 men, no less than 75,821 were admitted into hospital for malarial fever! Fortunately the death-rate of the disease is low in most places; but on the other hand the cases are so numerous that in the aggregate the mortality from malarial fever is very large indeed. For instance, in India alone, among the civil population (who do not take adequate treatment) the mortality from "fevers" during the single year 1897 amounted to the enormous total of 5,026,725—over five million deaths—being nearly ten times that due to any other disease. Although undoubtedly thousands of deaths are wrongly attributed to fever in these statistics, such figures can point only to a very great mortality due to malaria. Yet India on the whole is not nearly so malarious as many localities—such, for instance, as places on the coasts of Africa. In short, next perhaps to tuberculosis, malarial fever is admittedly the most important of human diseases.

But if the problem to which I refer was an important one, its solution presented difficulties which I, for one, formerly thought to be insuperable. It has been mentioned that Lancisi and Pringle connected the disease with stagnant water; and their views have been generally endorsed by innumerable observations made since their time—by the general experience of mankind, by statistics, and by the fact that malaria can often be actually banished by means of drainage of the soil. But Laveran had now shown the disease to be due to a parasite of the blood. How reconcile these facts? There appeared to be but one way of doing so—namely, by supposing that the organism lives a free life in the water or soil of malarious places, from which it enters man by the respiratory or digestive tracts. To prove this it was necessary to discover it in the water or soil of malarious places. But how make this discovery? The organism is not a bacterium, but an animal parasite. It cannot be taken from the living blood and sown on the surface of a gelatine film. Experiments have proved that it can be inoculated from man to man by the intravenous injection of fresh infected blood; but this is a very different thing to cultivating it in an artificial medium. At all events, experiments in this line have always failed and are not in the least likely to succeed. The parasites simply perish when taken from their natural habitation, the blood. It was therefore extremely unlikely that we should ever be able to follow up their life-history by this means—which has proved so successful as regards the bacteria. It remained only to find them in the soil or water by direct search. But how identify them among the host of Protozoa which live in these elements? Certainly not by their form or appearance. As known to us at that time, they were simply minute amœbæ ensconced in the red corpuscles and accurately adapted for such a life. Now red corpuscles do not exist in soil and water; if the parasites live in the latter, they must possess some other form to that which they possess in the blood, and the clue afforded by identity of appearance fails us. The only remaining method open to us would have been to attempt to produce infection by each one in turn of the numerous species of Protozoa found in the water and soil of malarious places—a task of great magnitude, and one which we now know would have failed. Indeed, it was actually attempted by several observers, and actually did fail.

Such was the state of things up to the end of the year 1894. Speaking for myself, I can well remember the hopeless feelings with which I then regarded the problem. Fortune, however, was to be kinder to us than I had dared believe. At this very moment the key to the solution of the problem had already been indicated by Dr. Patrick Manson.

I have said that since the original discovery of Ray Lankester numerous hæmatozoa—or rather hæmocytozoa—have been found in man and various animals. All these are generally classed by zoologists in Leuckart's order of the Sporozoa, and are usually divided into three groups—groups which are not very closely related, except for the fact that all the organisms concerned are parasites of the red corpuscles of the blood. One group—found in reptiles—consists of parasites closely allied to the Gregarinidæ; another is found in oxen, and is the cause of Texas cattle-fever; the third—for which I adopt the name of Hæmamœbidæ, Wassielewski—is found in man, monkeys, bats and birds. It is to this third group—the Hæmamœbidæ—to which we must now direct our attention, because it includes the parasites of malarial fever. There are, at least, two known species found in birds, two in bats, one in monkeys, and three in man. The human parasites are those which respectively

[1] A lecture delivered at the Royal Institution of Great Britain on March 2, by Major Ronald Ross, D.P.H., M.R.C.S., Lecturer in Tropical Medicine, University College, Liverpool.

cause the three varieties of malarial fever—quartan, tertian, and remittent or pernicious fever. For these three species I adopt the names *Haemamoeba malariae* (quartan), *Haemamoeba vivax* (tertian), and *Haemomenas praecox* (remittent fever).[1] According to Metchnikoff the group belongs, or is allied, to the Coccidiidæ. All the species have a close resemblance to each other, and all contain the typical melanin of malarial fever. The youngest parasites are found as minute *amoebulae* living within the red corpuscle and generally containing granules of this melanin (which, indeed, is derived by the parasite from the hæmoglobin of the corpuscle within which it makes its abode). The amœbulæ grow rapidly in size, until, after one or more days (according to the species), they reach maturity. At this point many of them become *sporocytes*—that is, give rise to ordinary spores by vegetative reproduction. These spores presently attach themselves to fresh corpuscles, become fresh amœbulæ, and so continue the life of the parasites indefinitely within the vertebrate host. Others of the amœbulæ, however, instead of becoming sporocytes like the rest, become *gametocytes*.

Now it is to these gametocytes that an extreme interest attaches, because it is to them, and to Manson's study of them, that we owe the solution of the malaria problem. Numerous observers had examined them before Manson's time, but all had failed in arriving at a correct idea as to their function It had been often observed that they circulate in the blood of the vertebrate hosts without, apparently, performing any function at all. As soon, however, as they are drawn from the circulation—as when the blood containing them is made into a fresh specimen for microscopic examination—they undergo the most remarkable changes. They swell up and liberate themselves from the enclosing corpuscle; and then some of them are suddenly seen to emit a number of long *motile filaments*. These filaments can easily be watched struggling violently, and may sometimes be seen to break from the parent cell and to dart away among the corpuscles, leaving the residue of the gametocyte, with its melanin, an inert and apparently dead mass.

Now it is not to be supposed that such an extraordinary phenomenon as this—which was observed by Laveran during his first investigations—could be witnessed without exciting the liveliest curiosity. As a matter of fact a hot controversy rose regarding it. Laveran, Danilewsky and Mannaberg maintained that the phenomenon is a vital one—that the motile filaments are living organisms, and constitute a stage in the history of the parasite. Antolisei, Grassi, Bignami and others of the Italian school fell back upon the old theory—which we always like to employ when we cannot explain a phenomenon—that it is a regressive phenomenon, a disintegration of the parasite due to its death *in vitro*. Here, however, the controversy practically stayed. While the Italians, in conformity with their views, attached no signification to the motile filaments, Laveran, Danilewsky and Mannaberg, who held an opposite opinion, did not expressly or exactly state what their signification is. Mannaberg, indeed, held that they are meant to lead a saprophytic existence, but did not explain how they could escape from the body in order to do so.

It was reserved for Manson to detect the ultimate (though not the immediate) function of these bodies. He asked why the escape of the motile filaments occurs only after the blood is abstracted from the host (a fact agreed upon by many observers). From his study of these filaments, of their form and their characteristic movements, he rejected the Italian view that they are regressive forms; he was convinced that they are living elements. Hence he felt that the fact of their appearance only *after* abstraction of the blood (about fifteen minutes afterwards) must have some definite purpose in the life-scheme of the parasites. What is that purpose? It is evident that these parasites like all others must pass from host to host; all known parasites are capable not only of entering the host, but, either in themselves or their progeny, of leaving him. Manson himself had already pushed such methods of inductive reasoning to a brilliantly successful issue in discovering by their means the development of *Filaria nocturna* in the gnat. He now applied the same methods to the study of the parasites of malaria. Why should the motile filaments appear only after abstraction of the blood? There could be only one explanation. The phenomenon, though it is usually observed in a preparation for the microscope, is really meant to occur *within the stomach cavity of some suctorial insect, and constitutes the first step in the life-history of the parasite outside the vertebrate host.*

[1] NATURE, August 3, 1899.

It is perhaps impossible for any one, except one who has spent years in revolving this subject, to understand the full value and force of this remarkable induction. To my mind the reasoning is complete and exigent. It was from the first impossible to consider the subject in the light in which Manson placed it without feeling convinced that the parasite requires a suctorial insect for its further development. And subsequent events have proved Manson to have been right.

The most evident reasoning—the connection between malarial fever and low-lying water-logged areas in warm countries—suggested at once that the suctorial insect must be the *gnat* (called *mosquito* in the tropics); and this view was fortified by numerous analogies which must occur at once to any one who considers the subject at all, and which it is not necessary to discuss in this place.

Needless to say, since Manson's theory was proved to be right it has been shown to be not entirely original. Nuttall, in his admirable history of the mosquito theory, demonstrates its antiquity. Eleven years before Manson wrote, King had already accumulated much evidence, based on epidemiological data, in favour of the theory. A year later (1884), Laveran himself briefly enunciated the same views, on the analogy with *Filaria nocturna*. Koch, and later, Bignami and Mendini, were also advocates of the theory—partly on epidemiological grounds and partly because of a possible analogy with the protozoal parasites of Texas cattle-fever which Smith and Kilborne had shown to be carried by a *tick*. Hence many observers had independently arrived at the same theory by different routes. But I feel it most necessary to point out here that there is a difference between a fortunate guess and a true scientific theory. Interesting and suggestive as were many of the hypotheses to which I have just referred, they were to my mind far from convincing. *Filaria nocturna*, and even *Apiosoma bigeminum*, are not in close enough relationship with the Hæmamœbidæ to admit of very forcible analogies in regard to the respective life-histories. The epidemiological arguments of King and Bignami (some of which were also used by Manson) were scarcely solid enough to support by themselves a theory of any weight. All these were hypotheses—little more: I can scarcely conceive a practical man sitting down to laborious researches on the strength of arguments like these. On the other hand, Manson's theory was what I have called it—an *induction*—a chain of reasoning from which it was impossible to escape.

I have wished to defend this work of Manson's because it has been much misunderstood and much misrepresented, and even (in a somewhat amusing manner) completely ignored by some who, though they once strongly opposed his theory, now, as soon as it has done its work, wish to forget it. It is true that he endeavoured to predict the history of the parasites a little too far, and that he was in error (as will presently appear) regarding the immediate nature of the motile filaments; but the centre of his theory was invaluable. I have no hesitation in saying that it was Manson's theory, and no other, which actually solved the problem; and, to be frank, I am equally certain that but for Manson's theory the problem would have remained unsolved at the present day.

To leave these interesting theories and to return to actual observations—I should begin by remarking that Manson thought the motile filaments to be of the nature of zoospores—that is, motile spores which escape from the gametocytes in the stomach cavity of the gnat, and then occupy and infect the tissues of the insect. In this he was proved, two years later, to have been wrong. The motile filaments are not spores, but *microgametes*—that is, bodies of the nature of spermatozoa. I have said that some of the amœbulæ in the blood-corpuscles of the host become sporocytes, which produce asexual spores (nomospores); while other amœbulæ become gametocytes, which have no function within the vertebrate host. As soon, however, as these gametocytes are ingested by a suctorial insect they commence their proper functions. As their name indicates, they are sexual cells—male and female. About fifteen minutes after ingestion (in some species) the male gametocyte emits a variable number of microgametes—the motile filaments—which presently escape and wander in search of the female gametocytes. These contain a single *macrogamete* or ovum, which is now fertilised by one of the microgametes, and becomes a *zygote*. We owe this beautiful discovery to the direct observation of MacCallum (1897), confirmed by Koch and Marchoux, and indirectly by Bignami. Metchnikoff, Simond, Schaudinn and Siedlecki have also demonstrated what are practically sexual elements in some of the Coccidiidæ. Directly MacCallum's discovery was announced Manson saw the important bearing of it on the mosquito theory. Admitting that the motile filaments themselves do not infect the gnat, he at once observed that it was probably the function of the zygote to do so—and this time he was perfectly right.

I must now turn to my own researches. Dr. Manson told me of his theory at the end of 1894, and I then undertook to investigate the subject as far as possible. I began work in Secunderabad, India, in April 1895; and should take the present opportunity for acknowledging the continuous assistance and advice which I received both from Dr. Manson and from Dr. Laveran, and later from the Government of India. Even with the aid of the induction, the task so lightly commenced was, as a matter of fact, one of so arduous a nature that we must attribute its accomplishment largely to good fortune. The method adopted—the only method which could be adopted—was to feed gnats of various species on persons whose blood contained the gametocytes, and then to examine the insects carefully for the parasites which by hypothesis the gametocytes were expected to develop into. This required not only familiarity with the histology of gnats, but a laborious search for a minute organism throughout the whole tissues of each individual insect examined—a work of at least two or three hours for each gnat. But the actual labour involved was the smallest part of the difficulty. Both the form and appearance of the object which I was in search of, and the species of the gnat in which I might expect to find it, were absolutely unknown quantities. We could make no attempt to predict the appearance which the parasite would assume in the gnat; while owing to the general distribution of malarial fever in India, the species of insect concerned in the propagation of the disease could scarcely be determined by a comparison of the prevalence of different kinds of gnat at different spots with the prevalence of fever at those spots. In short, I was forced to rely simply on the careful examination of hundreds of gnats, first of one species and then of another, all fed on patients suffering from malarial fever—in the hope of one day finding the clue I was in search of. Needless to say, nothing but the most convincing theory, such as Manson's theory was, would have supported or justified so difficult an enterprise.

As a matter of fact, for nearly two and a half years my results were almost entirely negative. I could not obtain the correct scientific names of the various species of gnats employed by me in these researches, and consequently used names of my own. Gnats of the genus *Culex* (which abound almost everywhere in India) I called "grey" and "brindled" mosquitoes; and it was these insects which I studied during the period I refer to. At last, the persistently nugatory results which had been obtained with gnats of this genus determined me to try other methods. I went to a very malarious locality, called the Sigur Ghat, near Ootacamund, and examined the mosquitoes there in the hope of finding within them parasites like those of malaria in man. The results were practically worthless (except that I observed a new kind of mosquito with spotted wings); and I saw that I must return to the exact method laid down by Manson. The experiments with the two commonest kinds of Culex were once more repeated—only to prove once more negative. The insects, fed mostly on cases containing the crescentic gametocytes of *Haemomenas praecox*, were examined cell by cell—not even their excrement being neglected. Although they were known to have swallowed living Hæmamœbidæ, no living parasites like these could be detected in their tissues—the ingested Hæmamœbidæ had in fact perished in the stomach cavity of the insects. I began to ask whether after all there was not some flaw in Manson's induction; but no—I still felt his conclusion to be an inevitable one. And it was at this very moment that good fortune gave me what I was in search of.

In a collecting bottle full of larvæ brought by a native from an unknown source I found a number of newly-hatched mosquitoes like those first observed by me in the Sigur Ghat—namely, mosquitoes with *spotted wings* and *boat-shaped eggs*. Eight of these were fed on a patient whose blood contained crescentic gametocytes. Unfortunately I dissected six of them either prematurely or otherwise unsatisfactorily. The seventh was examined, on August 20, cell by cell; the tissues of the stomach (which was now empty owing to the meal of malarial blood taken by the insect four days previously being digested) were reserved to the last. On turning to this organ I was struck by observing, scattered on its outer surface, certain oval or round cells of about two to three times the diameter of a red blood-corpuscle—cells which I had never before seen in any of the hundreds of mosquitoes examined by me. My surprise was complete when I next detected within each of these cells *a few granules of the characteristic coal-black melanin of malarial fever*—a substance quite unlike anything usually found in mosquitoes. Next day the last of the remaining spotted-winged mosquitoes was dissected. It contained precisely similar cells, each of which possessed the same melanin; only the cells in the second mosquito *were somewhat larger than those in the first*.

These fortunate observations practically solved the malaria problem. As a matter of fact, the cells were the *zygotes of the parasite of remittent fever growing in the tissues of the gnat*; and the gnat with spotted wings and boat-shaped eggs in which I had found them belonged (as I subsequently ascertained) to the genus *Anopheles*. Of course it was impossible absolutely to prove at the time, on the strength of these two observations alone, that the cells found by me in the gnats were indeed derived from Hæmamœbidæ sucked up by the insects in the blood of the patients on whom they had been fed—this proof was obtained by subsequent investigations of mine; but, guided by the presence of the typical and almost unique melanin in the cells, and by numerous other circumstances, I myself had no doubt of the fact. The clue was obtained; it was necessary only to follow it up—an easy matter.

The preparations of the stomachs of the two *Anopheles* were sealed, and were afterwards examined by Drs. Smyth, Manson, Thin and Bland-Sutton; and an account of the work and of the observations of these gentlemen was published a little later. Unfortunately, my labours now met with a serious interruption; but not before I had succeeded again in finding the zygotes in two other mosquitoes—one, another species of *Anopheles*, also bred from the larva, and also fed on a case containing crescentic gametocytes; the other, a "grey mosquito" (*Culex pipiens* type), which had been caught feeding on a case of tertian fever, and which I now think had become previously infected from a bird with *Haemamoeba relicta*.

Early in 1898, mainly through the influence of Dr. Manson, Sir H. W. Bliss and the United Planters' Association of Southern India, I was placed by the Government of India on special duty in Calcutta to continue my investigations. Unable to work with human malaria—chiefly on account of the plague-scare in Calcutta—I turned my attention to the Hæmamœbidæ of birds. Birds have at least two species of Hæmamœbidæ. I subjected a number of birds containing one or the other of these parasites to the bites of various species of mosquitoes. The result was a repetition of that previously obtained with the human parasites. Pigmented cells precisely similar to those seen in the *Anopheles* were found to appear in gnats of the species called *Culex fatigans*, Wiedemann, when these had been fed on sparrows and larks containing *Haemamoeba relicta*. On the other hand, these cells were never found in insects of the same species when fed on healthy birds or on birds containing the other parasite, called *Haemamoeba danilewskii*.

It will be evident that this fact was the crucial test both as regards the parasitic nature of these cells and as regards their development from the hæmocytozoa of the birds; and it was not accepted by me without very close and laborious experiment. The actual results obtained were as follows:—

Out of 245 *Culex fatigans* fed on birds containing *H. relicta*, 178, or 72 per cent., contained "pigmented cells." But, out of 41 *Culex fatigans* fed on a man containing crescentic gametocytes, 5 on a man containing immature tertian parasites, 154 on birds containing *H. danilewskii*, 25 on healthy sparrows, and 24 on birds with immature *H. relicta*—or a total of 249 insects, all carefully examined—not one contained a single "pigmented cell."

Another experiment was as follows:—Three sparrows, one containing no parasites, another containing a moderate number of *H. relicta*, and the third containing numerous *H. relicta*, were placed in separate cages within three separate mosquito-curtains. A number of *Culex fatigans*, all bred simultaneously from larvæ in the same breeding bottle, were now liberated on the same evening partly within the first mosquito-netting, partly within the second, and partly within the third. Next morning many of these gnats were found to have fed themselves on the birds during the night. Ten of each lot of gnats were dissected after a few days, with the following result:—

The ten gnats fed on the healthy sparrow contained no "pigmented cells." The ten gnats fed on the sparrow with a moderate number of parasites were found to contain altogether 292 "pigmented cells"; or an average of 29 in each gnat. The ten gnats fed on the sparrow with numerous parasites contained 1009 "pigmented cells"; or an average of 100 cells in each gnat. These thirty specimens were sent to Manson in England, who made a similar count of the cells.

I may mention one more out of several experiments of the same kind. A stock of *Culex fatigans*, all bred from the larva, were fed on the same night partly on two sparrows containing *H. relicta*, and partly on a crow containing *H. danilewskii* (placed, of course, under separate mosquito-nettings). Out of 23 of the former lot, 22 were found to have pigmented cells; while out of 16 of the latter, none had them.

Hence no doubt remained that the "pigmented cells" really constitute a developmental stage in the mosquito of these parasites; and this view was accepted both by Laveran and Manson, to whom specimens had been sent. In June 1898, Manson published an illustrated paper concerning my researches, and showed that the pigmented cells must in fact be the zygotes resulting from the process of fertilisation discovered by MacCullum.

It remained to follow out the life-history of the zygotes. For this purpose it was immaterial whether I worked with the avian or the human parasites, since these are so extremely like each other. I elected to work with the avian species, chiefly because the plague-scare in Bengal still rendered observations with the human species almost impossible. By feeding *Culex fatigans* on birds with *H. relicta* and then examining the insects one, two, three and more days afterwards, it was easy to trace the gradual growth of the zygotes. Their development briefly is as follows:—After the fertilisation of the macrogamete has taken place in the stomach-cavity of the gnat, the fertilised parasite or zygote has the power of working its way through the mass of blood contained in the stomach, of penetrating the wall of the organ, and of affixing itself on, or just under its *outer coat*. Here it first appears about thirty-six hours after the insect was fed, and is found as a "pigmented cell"—that is, a little oval body, about the size of a large red corpuscle, and containing the granules of melanin possessed by the parent gametocyte from which the macrogamete originally proceeded. In this position it shows no sign of movement, but begins to grow rapidly, to acquire a thickened capsule, and to project from the outer wall of the stomach, to which it is attached, into the *body-cavity* of the insect host. At the end of six days, if the temperature of the air be sufficiently high (about 80° F.), the diameter of the zygote has increased to about eight times what it was at first; that is, to about 60 μ. If the stomach of an infected insect be extracted at this stage, it can be seen, by a low power of the microscope, to be studded with a number of attached spheres, which have something of the appearance of warts on a finger. These are the large zygotes, which have now reached maturity and which project prominently into the mosquito's body-cavity.

All this could be ascertained with facility by the method I have mentioned; and it should be understood that gnats can be kept alive for weeks or even months by feeding them every few days on blood—or, as Bancroft does, on bananas. But a most important point still required study. What happens after the zygotes reach maturity? I found that each zygote as it increases in size divides into *meres*, each of which next becomes a *blastophore* carrying a number of *blasts* attached to its surface. Finally, the blastophore vanishes, leaving the thick capsule of the zygote packed with thousands of the blasts. The capsule now *ruptures*, and allows the blasts to escape into the body-fluids of the insect.

These blasts, when mature, are seen to be minute filamentous bodies, about 12–16μ in length, of extreme delicacy, and somewhat spindle-shaped—that is, tapering at each extremity. Just as the zygotes recall the shape of the Coccidiidæ, so do these blasts recall the "falciform bodies." Prof. Herdman and I have adopted this word "blast" for these bodies after careful consideration—but others prefer other names. They are, of course, *spores*; but spores which have been produced by a previous sexual process—and are in fact the result of a kind of *polyembryony*. Just as a fertilised ovum gives rise to blasts, which produce the cluster of cells constituting a multicellular animal, so, in this case, the fertilised ovum, or zygote, gives rise to blasts, each of which, however, becomes a separate animal. Prof. Ray Lankester suggests for the blasts of the Hæmamœbidæ the simple term "filiform young."

At this point the investigations took a turn of extreme interest and importance, scarcely second even to what attached to the first study of the zygotes. Since the blasts are evidently the progeny of the zygotes, they must carry on the life-history of the parasites to a further stage. How do they do so? What is their function? Do they escape from the mosquito, and in some manner, direct or indirect, set up infection in healthy men and birds? Or, if not, what other purpose do they subserve? It was evident that our knowledge of the mode of infection in malarial fever—and perhaps even the prevention of the disease—depended on a reply to these questions.

As I have said, the zygotes become ripe and rupture about a week after the insect was first infected—scattering the blasts into the body-cavity of the host. What happens next? It was next seen that by some process, apparently owing to the circulation of the insect's body-fluids (for the blasts themselves appear to be almost without movement), these little bodies find their way into every part of the mosquito—into the juices of its head, thorax, and even legs. Beyond this it was difficult to go. All theory—at least all theory which I felt I could depend upon—had been long left behind, and I could rely only on direct observation. Gnat after gnat was sacrificed in the attempt to follow these bodies. At last, while examining the head and thorax of one insect, I found a large gland consisting of a central duct surrounded by large grape like cells. My astonishment was great when I found that many of these cells were closely packed with the blasts (which I may add are not in the least like any normal structures in the mosquito). Now I did not know at that time what this gland is. It was speedily found, however, to be a large racemose gland consisting of six lobes, three lying in each side of the insect's neck. The ducts of the lobes finally unite in a common channel which runs along the under surface of the head and *enters the middle stylet, or lancet, of the insect's proboscis*.

It was impossible to avoid the obvious conclusion. Observation after observation always showed that the blasts invariably collect within the cells of this gland. It is the *salivary* or *poison* gland of the insect, similar to the salivary gland found in many insects, the function of which, in the gnat, had already been discovered—although I was not aware of the fact. That function is to *secrete the fluid which is injected by the insect when it punctures the skin*—the fluid which causes the well-known irritation of the puncture, and which is probably meant either to prevent the contraction of the torn capillaries or the coagulation of the ingested blood. The position of the blasts in the cells of this gland could have only one interpretation—wonderful as that interpretation is. The blasts must evidently pass down the ducts of the salivary gland into the wound made by the proboscis of the insect, and *thus cause infection in a fresh vertebrate host*.

That this actually happens could, fortunately, be proved without any difficulty. As I had now been studying the parasites of birds for some months, I possessed a number of birds of different species, the blood of which I had examined from time to time (by pricking the toes with a fine needle). Some of them were infected, and some, of course, were not. Out of 111 wild sparrows examined by me in Calcutta, I had found *H. relicta*—the parasite which I had just cultivated in *Culex fatigans*—in 15, or 13·5 per cent. As a rule, non-infected birds were released; but I generally kept a few to use for the control experiments mentioned above, and the blood of these birds had consequently been examined on several occasions, and had always been found free from parasites. At the end of June I possessed five of these healthy control birds—four sparrows and one weaver-bird. All of them were now carefully examined again and found to be healthy. They were placed in their cages within mosquito-nets, and at the same time a large stock of old infected mosquitoes were released within the same nets. By "old infected mosquitoes" I mean mosquitoes which had been previously fed repeatedly on infected birds, and many of which on dissection had been shown to have very large numbers of blasts in their salivary glands. Next morning, numbers of these infected gnats were found gorged with blood, proving that they had indeed bitten the healthy birds during the night. The operation was repeated on several succeeding nights, until each bird had probably been bitten by at least a dozen of the mosquitoes. On July 9, the blood of the birds was examined again. I scarcely expected any result so complete and decisive. Every one of the five birds was now found to contain parasites—and not merely to contain them, but to possess such immense numbers of them as I had never before seen in any bird (with *H. relicta*) in India. While wild sparrows in Calcutta seldom contain more than one parasite in every field of the microscope, those which I had just succeeded in infecting contained ten, fifteen, twenty and even more in each field—a fact due probably to the infecting gnats having been previously fed over and over again on infected birds, a thing which can rarely happen in nature.

The experiment was repeated many times—generally on two or three healthy birds put together. But I now improved on the original experiment by also employing controls in the following manner. A stock of wild sparrows would be examined, and the infected birds eliminated. The remainder would then be kept apart, and at night would be carefully secluded from the bites of gnats by being placed within mosquito nets. These constituted my stock of healthy birds. From time to time two or three of these would be separated, examined again to ensure their being absolutely free from parasites, and then subjected to the bites of "old infected mosquitoes," and, of course, kept apart afterwards for daily study. Thus my stock of healthy birds was also my stock of control birds. Until they were bitten by gnats, I found that they never became infected (except in a single case in which I think I had overlooked the parasites on the first occasion), although large numbers of healthy birds were kept in this manner. The result in the case of the sparrows which were subjected to the bites of the infected gnats was different indeed. Out of 28 of these, dealt with from time to time, no less than 22, or 79 per cent., became infected in from five to eight days. And, as in the first experiment, all the infected birds finally contained very numerous parasites.

It was most interesting to watch the gradual development of the parasitic invasion in these birds; and this development presented such constant characters that, apart from other reasons, it was quite impossible to doubt that the infection was really caused by the mosquitoes. The course of events was always as follows. The blood would remain entirely free from parasites for four, five, six or even seven days. Next day one or perhaps two parasites would be found in a whole specimen. The following day it was invariably observed that the number of the organisms had largely increased; and this increase continued until in a few days immense numbers were present—so that, finally, I often observed as many as seven distinct parasites contained within a single corpuscle! Later on, many of the birds died; and their organs were then found to be loaded with the characteristic melanin of malarial fever.

I also succeeded in infecting on a second trial one of the six sparrows which had escaped the first experiment; and also a crow and four weaver-birds; and, lastly, gave a new and more copious infection to four sparrows which had previously contained only a few parasites.

These experiments completed the original and fundamental observations on the life-history of the Hæmamœbidæ in mosquitoes. The parasites had been carried from the vertebrate host into the gnat; had been followed in their development in the gnat; and had finally been carried back from the gnat to the vertebrate host. The theories of King, Laveran, Koch and Bignami, and the great induction of Manson, were justified by the event: and I have given a detailed historical and critical account of these theories, and of my own difficulties and experiences, in the hope of bringing conviction to those who might, perhaps, otherwise think the story to be too wonderful for credence.

But work of great importance remained to be done. I had intended, immediately after making this study of one of the parasites of birds, to extend the investigation more fully to those of man—a work which now presented no difficulty, since both the kind of mosquito hospitable to them (*Anopheles*) and the form of the parasites in the mosquito were well known to me. Unfortunately I was obliged to attend to other and less important duties, which kept me fully occupied for several months—an interruption which practically put an end to my own study of the mosquito-theory at a very interesting point. No time, however, was really lost. In December 1898, Dr. Daniels, of the Malaria Commission of the Royal Society and the Colonial Office, arrived in Calcutta to examine and report upon my results. After carefully repeating the various experiments, he fully confirmed the statements made by me.[1] At the same moment, the work was taken up with great brilliance and success by Dr. Koch and by Prof. Grassi and Drs. Bignami and Bastianelli, in Italy. I must now describe the investigations of these observers—though I have scarcely space to do so at the length they deserve.

Ever since the discoveries of Laveran and Golgi, the Italian observers of the Roman school had done much important work on malaria, facilitated by the well-known prevalence of the disease near Rome—work, if not of much originality, yet full of careful detail. More recently, however, this work had been practically arrested by their theory—wholly gratuitous, but which they accepted as a dogma—that the motile filaments are forms of disintegration *in vitro*. When Manson propounded his theory, Bignami, for instance, rejected it on this ground. But at the same time he evolved a gnat-theory of his own—a theory that malarial fever is inoculated by gnats which carry the parasite from marshy areas. The arguments he used were the epidemiological ones already advanced by King, and which can scarcely be said to amount to more than a plausible hypothesis: the only solid basis for the theory—that of Manson—was opposed by him. Later, however, the work of Simond, Schaudinn, Siedlecki, MacCallum and myself, explained by Manson, rendered the Italian position concerning the motile filaments quite untenable; and Bastianelli, Bignami and Grassi now undertook a study of the mosquito-theory on sound principles. My own results, with descriptions of the technique employed and with illustrations of the zygotes, had been published from time to time; a summary of them had been given by Manson in June 1898, and another, including the infection of healthy birds, before the British Medical Association, early in August; and there could therefore be no difficulty in following up the observations therein recorded. In September, Grassi published a paper in which he described certain investigations made in Italy with a view to ascertaining the species of gnats which are associated with the prevalence of malaria in that country. Such investigations are not, I think, trustworthy; and as a matter of fact two out of the three species of gnat then selected by Grassi as being malaria-bearing ones have now been rejected by him. The third species was an *Anopheles*, namely *A. claviger*, Fabr.

At the same time Bignami resumed his study of the subject. Some years previously, following his theory, he had endeavoured to infect healthy persons by the bites of gnats brought from malarious places. He had failed, and abandoned his efforts—and I believe that his method would of itself never have led to a solution of the problem. In the autumn of 1898, however, he renewed his efforts; but was again unsuccessful until he used a number of *Anopheles claviger*, brought from a house containing infected persons. The result was successful, the subject of the experiment becoming infected after some time. This important experiment gave the first confirmation with human malaria of my previous inoculation experiments with the malaria of birds; but since other species of gnats as well as *A. claviger* had been employed, it failed to fix suspicion entirely on the latter. In order to obtain this result, these observers were finally obliged to resort to the correct method of Manson and myself—namely that of direct cultivation of the parasites in the gnat. Success was now immediate. The zygotes and blasts of the parasites were found, exactly as previously described by me, in the tissues of *A. claviger*; and, lastly, healthy persons were infected by the bites of these insects. Pushing forward with admirable rapidity, the Italian observers next found that all three species of the human Hæmamœbidæ are cultivable in *A. claviger*; and not only in this, but in other Italian species of *Anopheles*; while, like me, they failed in cultivating the parasites in *Culex*.

Almost simultaneously Koch repeated and confirmed with the weight of his authority most of the results which had been obtained as regards both the human and avian parasites. In August 1899, the malaria expedition sent to Sierra Leone by the Liverpool School of Tropical Medicine (of which expedition I was a member) found the human parasites in two species of *Anopheles* in that colony, namely *A. costalis*, Loew, and *A. funestus*, Giles. I hear also that the same result has been obtained with *Anopheles* in two other parts of the world; so that it would appear that something like nine species of *Anopheles* have now been inculpated—while as yet every species of Culex which has been tried has failed to give positive results.

From this point it becomes impossible to follow in detail the researches carried out in connection with the mosquito-theory

[1] NATURE, August 3, 1899.

in various parts of the world. The facts already collected would fill a small volume; and every month witnesses additional publications on the subject. I shall therefore, in conclusion, content myself with a brief reference to three points of leading importance.

I shall first try to indicate how completely the recent discoveries explain the well-known laws regarding the diffusion of malaria. As mentioned at the beginning of this lecture, malarial fever has long been known to be connected with the presence of stagnant water. That is to say, we generally, though not invariably, find that the disease is associated with low-lying flat areas, where water tends to collect to a considerable extent. It was indeed the general appreciation of this law which led to the old miasma-theory of the disease—the theory on which the word "malaria" was based. We assumed that the poison is one which rises from marshy areas in the form of a mist, and which thence infects all living within a given distance. Later, when the pathogenetic parasite was discovered in the blood of febricitants, many observers, still clinging to this conception, thought that the parasite is an organism which in its free state dwells in such places, and diffuses itself in such mists. It is interesting to note how near to the truth this almost instinctive conception took us. It is right in idea, wrong in fact. It is not the parasite itself which springs from the marshy ground, *but the carrier of the parasite.*

This was one of the many interesting points made by King in his mosquito-theory of seventeen years ago. But King fell into an error which could have been used as a powerful argument against his hypothesis. He seemed to have assumed that all mosquitoes rise from marshes. Hence, he said, malaria exists in the presence of marshes; hence it is a disease of the country rather than of towns, and so on. As a matter of fact, mosquitoes as a rule do not rise from marshes at all; they do not all even rise from pools of water on the ground; the commonest species, at least of those which habitually annoy human beings, spring from tubs and pots of water in the vicinity of houses, and are indeed more common in cities than in country places, at any rate in the tropics. Now it is not the least interesting feature of recent researches that they have shown where the error lay. As soon as I had succeeded in cultivating the human parasites in my "dappled-winged mosquitoes," which were really gnats of the genus *Anopheles*, I began to study the habits of these insects, and soon ascertained the remarkable fact that while gnats of genus *Culex* generally breed, in India, in vessels of water round houses, gnats of genus *Anopheles*, which I had just connected with malaria, breed in *small pools of water on the ground.* This point was made the subject of a special investigation by the recent expedition to Sierra Leone; and we found that the law holds good there as in India. While *Culex* larvæ were to be seen in almost every vessel of water, or empty gourd or flower-pot in which a little rain-water collected, in only one case did we find *Anopheles* larvæ in such. On the other hand, *Anopheles* larvæ occurred in about a hundred small puddles scattered through the city of Freetown—puddles mostly of a fairly permanent description, kept filled by the rain, and not liable to scouring out during heavy showers. What was almost equally significant, the larvæ seemed to live chiefly on green water-weed. Hence it follows that while *Culex*, the apparently innocuous genus of gnats, are essentially, or at least often, domestic insects, *Anopheles*, the malaria-bearing genus, are essentially gnats which spring from stagnant water on the ground. And numerous other facts in the history of malaria can be explained by the same discovery. It is supposed, for instance, that malaria originates from freshly-turned earth; now we actually noted examples where railway embankments and the like had produced *Anopheles* pools; and it is easy to see that disturbance of the soil may often produce depressions in the ground capable of holding a little rain-water suitable for the larvæ of these insects. Again, malarial fever often appears on board vessels which have touched at malarious ports; as an explanation of this we ascertained that *Anopheles* visit ships from the shore. In short, on studying the matter from every point of view, I must confess to being ignorant of any well-established fact about malarial fever which is not explained by the mosquito-theory.

This brings me to the subject of *objections* to the mosquito-theory. In view of the exact and copious microscopical and experimental evidence which has now been collected in proof of the theory, it is no longer permissible to doubt the main facts; and the objections which one still finds, both in the lay and the medical press, are generally based on a complete ignorance of these facts, and need not be discussed here. But there is one objection—frequently made, in spite of corrections as frequent, by persons who reside in malarious places—which deserves comment. This is, that malaria exists where there are no mosquitoes, and that so-and-so has had fever without being bitten by gnats at all. Generally speaking, we must always remember that malarial fever is a disease in which relapses occur perhaps for years after the first infection, and that it is this first infection and not the relapses which are due to the bite of *Anopheles.* It is thus possible to suffer from any number of attacks of fever without being bitten by *Anopheles* (except on one occasion), and without invalidating the theory—a fact of which those who argue in this manner are generally ignorant. Again, it is well known that one may be bitten without perceiving it; that some persons are singularly callous to the punctures of these insects; and, lastly, that many others have very limited powers of observation. I may say at once that, personally, I cannot accept any statement to the effect that gnats are absent in any locality in the tropics, until such a statement is made by a competent observer after direct search; because I have never been in any place in the tropics—and I have been in a large number—where there were no gnats. On the other hand, I have often found numerous gnats in localities where I was previously told there were none. I was once actually informed that there are no mosquitoes in Sierra Leone! The fact is that those who will trust the statements of the general public on such matters must be very credulous.

I turn lastly to the all-important subject of *prevention*, but can do no more than touch upon it here. Two methods suggest themselves at once. I need not refer to that of guarding against the bites of these insects by the use of mosquito-nets and so on—an obvious and, I believe, an exceedingly useful measure, which may reduce the chances of infection to a small fraction. Unfortunately such methods will never be employed on a large scale in the majority of malarious localities; and we must resort to the *destruction of malaria-bearing species of gnats.* Early in 1892 I reported to the Government of India that it may be possible to exterminate *Anopheles* in *some* localities—especially some towns, cantonments and plantations—owing to the habit the insects have (in some places) of breeding only in selected pools. Since then, a considerable literature has already grown up round the subject. Reviewing this literature, it seems probable that we may be able to exterminate *Anopheles*, or at least largely reduce their numbers, in towns where, owing to the conformation of the ground, the low level of the subsoil water or the small rainfall, surface pools suitable for the insects are comparatively few. The methods which can be adopted against the larvæ are numerous—such as brushing out the pools with a broom, draining them away, filling them up, or treatiug them with various *culicides*, such as paraffin and numerous other substances (recently investigated by Celli and Casagrandi). On the whole, the most promising method which suggests itself is the employment of some cheap solid material or powder which dissolves slowly, which kills the larvæ without injuring higher animals, and which renders small pools uninhabitable for the larvæ for some months. If, for instance, a cartload of such a material would suffice to extirpate the larvæ from a square mile of a malarious town, the result would be a large gain to its healthiness. Dr. Fielding-Ould has lately reported favourably on *tar.* Grillet recently reports a case in France where a large district was rendered free of malaria by the extensive use of *lime* for agricultural purposes. *Gas-lime*, or even common *salt*, may be suggested. In short, though the question of the possibility of attacking these insects with success is still entirely in the experimental stage, we may reasonably hope that the mosquito-theory of malaria may some day prove to be as useful to humanity as it certainly has proved interesting to the student of science.

In conclusion, however, I should add that this result is not likely to be attained unless we, as a nation, determine to pay more attention to scientific discoveries in the field of tropical medicine than hitherto we have done. During the last fifty years discovery after discovery in this field has been made without finding any adequate reflex in medical and sanitary practice in our tropical possessions. The discoveries, for instance, of Lösch, Davaine, Dubini, Bilharz, Bancroft, Koch, Laveran, Manson, Carter and Giles, though nearly concerned with the lives of thousands of human beings, have been generally treated either with scepticism or neglect—have been neither sufficiently followed in the laboratory nor sufficiently acted upon in the region of practical sanitation.

AUSTRALIAN EXPERIMENTAL FARMS.

THE importance attached by the various Australian Governments to the encouragement of agriculture is shown in the fact that in most of the colonies a department of agriculture has been established, the official head of which is a member of the colonial ministry. In New South Wales a site suitable for a central establishment was selected at Ham Common, near the town of Richmond, in the Hawkesbury district, about 39 miles from Sydney, where an area of about 4000 acres was resumed for the purpose. The college and farm are now in their seventh year of existence. Accommodation is provided for ninety-six resident students, and during 1898 there was a full roll. Theoretical as well as practical instruction is imparted by experts in every branch of agriculture, and experimental work is carried on with cereal and other crops. There is an orchard, 30 acres in extent, and a vineyard, 10 acres in extent, and the cultivation of plants for the production of scent has also been begun. There are also experimental farms at Bomen, 304 miles from Sydney, in the Murrumbidgee district; and at Wollongbar, 366 miles from Sydney, in the Richmond River district. The former is near the town of Wagga Wagga, and embraces an area of 2460 acres, of which 1200 acres are in cultivation, 1000 acres being devoted to growing cereals, of which 500 acres are for seed wheat; 85 acres to fruit trees and grape-vines, and 80 acres to forage plants; while 8 acres are under olive trees; the remaining portion being taken up by irrigation plots, nursery and experimental plots. Quarters have been provided for twenty-five students. At the Wollongbar Farm experiments have been made with sugar-canes obtained from New Guinea, sugar cultivation being a staple industry on the Clarence, Richmond, and other northern rivers. Experiments with grasses for the grazing of dairy cattle have been carried on, and steps taken to assist the dairying industry, which is greatly on the increase in the northern parts of the colony. Other trials are being made with citrus fruits, pineapples, bananas, and various other tropical and semi-tropical fruits. The total area of the farm is 263 acres. The experimental farm at Bathurst, 145 miles from Sydney, is largely devoted to the cross-breeding of sheep, irrigation, fruit-growing, cereal culture, and general mixed farming. The area of the farm is 596 acres, to which leased areas of 176 acres have been added. The area under cultivation is 370 acres. There are 1000 sheep and lambs on the farm; and nine students have been enrolled. Another farm is situate at Coolabah, in the dry country, about 424 miles from Sydney, where there are about 200 acres in cultivation, trials being systematically made with various kinds of wheat, maize, sorghum, cow-peas, grasses, fodders, and so on. There is also a travelling instructor, whose duty it is to visit the rural districts and give personal advice and practical demonstration in all matters connected with agriculture. Under the direction of the Government pathologist, investigations are carried out at the laboratories at the Sydney, Bathurst, and Wagga Wagga farms. At a laboratory at Pymble, a few miles from Sydney, the diseases of citrus plants have formed the subject of special inquiry. Operations at Bathurst are not specially directed to agriculture, but are confined more to the diseases of stock; but at Wagga Wagga the work of the laboratory is mainly in connection with wheat and other farm crops.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

A SCHOOL of forestry is to be established at Yale University. The large estate bequeathed by the late Prof. O. C. Marsh will be used as a school of botany, and will also be used for the present as a school of instruction in forestry.

BESIDES the degrees recognised by the State, the universities of France can grant degrees exclusively scientific, but which confer none of the rights or privileges belonging to the State degrees, and which must in no case be declared as equivalent to them. We learn from the *Lancet* that the Nancy faculty of medicine is not content with its power to present candidates for the university doctorate as regards medicine, but desires the right to present for a degree persons who have shown their especial knowledge in biological science. The council of the University of Nancy has agreed to this proposition, and accepted it at a special meeting. The resolution has just been approved by the Minister of Public Instruction, and so, starting during the current scholastic year, the faculty of medicine at Nancy is authorised to present candidates for the new degree, which is the first of the kind to exist in France.

THE unsatisfactory condition of the teaching of geography in this country should afford plenty of scope for the work of the Geographical Association, which aims "to improve the teaching of geography by spreading the knowledge of all such methods as call out the pupil's intelligence and reasoning powers and make geography a real educational discipline, instead of merely loading the memory with names and isolated facts." The membership of the Association has hitherto been limited to teachers in secondary schools and others interested in public school education. At the recent annual meeting, its boundaries were extended, and the Association is now open to all teachers of geography, and to other persons desirous of encouraging improved methods of geographical instruction. Geography as it is usually taught ought to be banished from our schools, for it is of no scientific value whatever, and benumbs a child's intellect instead of developing it. When the authorities which supervise and examine the work done in primary and secondary schools take a wider view of geography than at present exists, when, in fact, they make geography mean physiography, there will be hope for the rational methods of teaching which the Geographical Association seeks to encourage.

A SHORT time ago it was proposed to form a Bureau or School of Research in Washington, under the supervision of the Smithsonian Institution. The Regents of the Institution are in sympathy with the scheme, but they consider that their present powers are scarcely broad enough to embrace the work proposed. They may, however, decide to ask Congress to provide the means for organising the scientific work of the various Government departments, and for co-operating with the universities and colleges of the United States in systematic research work. The Bureau would be in connection with the proposed National University, upon which subject a sub-committee of the National Educational Association has just presented a report. The committee suggests that if the Smithsonian Institution is unable to take the initiative in the matter, the Bureau of Education shall become the administrative centre of the Bureau of Research. Under the terms of either of the plans proposed, it is assumed that the persons admitted to carry on research will be graduates of a college or university in good standing, or will have had an equivalent training. The committee point out that such a bureau of research, whether it be placed under the care of the Smithsonian Institution or under that of the Department of Education—which would supersede the existing Bureau of Education—would be a source of strength to the higher education of the United States and a great advantage to the Government in its work of promoting the progress of science and the useful arts, and in applying the result of scientific investigation to the development of the natural resources of the country, of agriculture, of manufactures, and of commerce.

SCIENTIFIC SERIALS.

American Journal of Science, March.—Hot water and soft glass in their thermodynamic relations, by C. Barus. Glass shares the property of colloids, of being soluble in a liquid when the latter is hot enough. Glass is dissolved in water heated under pressure to 210°. Every glass at a sufficiently high temperature must eventually show complete solubility in water. Such solutions are, however, unstable at ordinary temperatures. The solubility of silicates in very hot water has an important bearing upon natural phenomena. Sea-water more than 200 metres below the surface of the ocean will remain liquid at 200°. If, therefore, water from anywhere below that depth penetrates into the earth as far as the isotherm for 200°, the rock there, if of the character of glass, will become liquefied, apart from pressure. The hydrated silicate is thus virtually fluid 8 kilometres below the surface, and the level of aqueous fusion is five times as near the surface as that of igneous fusion.—An electrical thermostat, by W. Duane and C. A. Lory. The thermostat, which is of very high efficiency, consists of a wooden trough containing an ordinary salt solution, which is heated by an electric light current introduced through zinc plates at the ends of the trough. The regulating device is a set of brass tubes filled with alcohol, whose expansion depresses a thread of mercury in one arm of a U-tube, and thus makes contact in the other arm. The circuit thus established works a relay which inserts a resistance in the heating circuit, and thus automatically reduces the temperature. The action is remarkably prompt, the regulating circuit being made and broken two or three times per second. The temperature of the thermostat remains constant to within $\frac{1}{2000}$th of a degree C., even when the surrounding temperature changes suddenly by some 12 degrees.—Explorations of the *Albatross* in the Pacific, (iii.), by A. Agassiz. The deepest trawl haul yet made was made about 75 miles east of Tonga-Tabu. It was at 4173 fathoms. The bag brought up a number of large fragments of silicious sponge, belonging probably to the genus Crateromorpha, which had been obtained by the *Challenger* at depths of only 500 fathoms. The bottom consisted of light brown volcanic mud mixed with radiolarians.—Illinois Gulch meteorite, by H. L. Preston. This siderite was found in Montana last year, on the bed rock about four feet below the surface. It weighs $2\frac{1}{2}$ kilograms, and consists of 92 5 per cent. iron, 6·7 per cent. nickel, and traces of cobalt, silicon, phosphorus and carbon. It shows no figures on etching, but greatly resembles the Morrodal siderite of Norway.—The Silurian-Devonian boundary in North America, by H. S. Williams. This first article deals with the Chapman sandstone fauna. It must be regarded as the equivalent of the topmost fauna of the Welsh Silurian system. This classifies the Lower Helderberg formation in the Silurian system.

THE *Physical Review* for January contains the first part of a paper, by Prof. R. A. Fessenden, bearing the title of "A determination of the nature of the electric and magnetic quantities, and of the density and elasticity of the ether."—Mr. B. E Moore, in the same number, deals with electrolytic polarisation; and Mr. H. V. Carpenter with the comparison of two self-inductances.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, February 1.—"On the Influence of the Temperature of Liquid Air on Bacteria." By Allan Macfadyen, M D.

The experiments of Dr. Horace T. Brown and Mr. Escombe (*Roy. Soc. Proc.* vol. 62, 1898, p. 160) have shown that no appreciable influence is exerted upon the germinative power of seeds when exposed for 110 hours to the temperature of liquid air (−183° C. to −192° C.). The results were equally negative in the recent experiments of Sir W. Thiselton-Dyer (*ibid.* vol. 65, 1899, p. 361), in which seeds survived exposure for upwards of six hours to the temperature of liquid hydrogen (−250° C. to −252° C.).

The following investigation on the influence of the temperature of liquid air on bacteria was carried out at the suggestion of Sir James Crichton Browne and Prof. Dewar. The necessary facilities were most kindly given at the Royal Institution. The experiments were conducted under the personal supervision of Professor Dewar, and he has asked me to put the results on record, although it must be acknowledged that the essential features of the investigation are due to him.

Ten organisms were used for the experiments, viz.:—*B. typhosus*, *B. coli communis*, *B. diptheriae*, *Spirillum cholerae Asiaticae*, *B. proteus vulgaris*, *B. acidi lactici*, *B. anthracis* (sporing culture), *Staphylococcus pyogenes aureus*, *B. phosphorescens* and *Photobacterium balticum*.

The cultures were simultaneously exposed to the temperature of liquid air for twenty hours (−182° C. to −190° C.). They were then carefully thawed and examined. The results may be briefly stated. In no instance, whether on solid or in liquid media, could any impairment of the vitality of the micro-organisms be detected. The fresh growths obtained from the exposed tubes were normal in every respect, and the functional activities of the bacteria were equally unaffected. The colon bacillus produced its typical effects—such as the curdling of milk, the fermentation of sugar and the production of indol; the *Staphylococcus pyogenes aureus* retained its pigment-producing properties, and the anthrax spores their pathogenic action, on animals. The photogenic bacteria preserved their normal luminous properties. These photogenic properties are intimately connected with the functional activities of the cells. The cells emit light which is apparently produced by a chemical process of intracellular oxidation, and the phenomenon ceases with the cessation of their activity. These organisms therefore furnished a very happy test of the influence of low temperatures on vital phenomena. Their cultures, when cooled down in the liquid air for twenty hours, became non-luminous, but on rethawing the luminosity returned with unimpaired vigour as the cells renewed their activity. Watery emulsions of the photogenic bacteria, on immersion in liquid air for a few minutes, ceased to emit light, but on withdrawal the luminosity reappeared in a very short time. Strips of filter paper soaked in the watery emulsions and brightly luminous were immersed directly in the liquid air with similar results. The sudden cessation and rapid renewal of the photogenic properties of the cells, despite the extreme changes of temperature, was remarkable and striking.

The above experiments show that bacteria may be cooled down to −190 C. for a period of twenty hours without losing any of their vital properties.

Further experiments are in progress with the above-mentioned, and with other micro-organisms exposed to the temperature of liquid air for still longer periods of time, as well as to that of liquid hydrogen. These experiments will form the subject of a future communication.

March 15.—"The Theory of the Double Gamma Function." By E. W. Barnes, B.A., Fellow of Trinity College, Cambridge. Communicated by Prof. A. R. Forsyth, Sc.D., F.R.S.

Physical Society, March 23.—Prof. W. E Ayrton, F.R.S., Vice-President, in the chair.—A paper on some experiments illustrating syntony was read by Mr. P. E. Shaw. The experiments described in this paper have been devised for the purpose of showing in a lecture-room the principles of magnetic space telegraphy, the distance between the sending and receiving circuits being about fifteen yards. A current flowing in a main circuit was interrupted by a tuning-fork of 100 vibrations per second, and a fraction of the current was passed through the sending coil. The sending coil was placed in series with a coil of adjustable self-induction, and the two coils were shunted with a condenser of variable capacity. By suitable adjustments an oscillation of frequency 400 could be maintained in the sending circuit. The receiving coil was in series with a variable self-induction and a variable capacity, and was tuned to respond to the waves given out by the primary. The current induced in the secondary coil was passed round a light drum fastened to a wire tuned to 400 vibrations per second. The drum was placed in a strong magnetic field, and the electrical oscillations caused mechanical vibrations of the drum. On to the drum was attached one carbon of a microphone, and the induced oscillations were thereby considerably magnified in the microphone circuit. This circuit was also arranged in the same way as the former, and by means of another microphone the vibrations were transferred to another circuit where their intensity was sufficient to actuate the diaphragm of an ordinary telephone-receiver to such an extent as to render the sound perfectly audible. Mr. Watson described some experiments which he had shown to illustrate syntony, both by obtaining galvanometer deflections and sparks in the secondary circuit. Dr. Lehfeldt asked how the circuit was tuned when it contained both a variable capacity and a variable self-induction. Mr. Shaw said that the values of the capacity and self-induction were connected with the vibration frequency by a formula given by Dr. Lodge. Starting with a known capacity, the necessary self-induction was calculated and small alterations produced by means of an iron core.—Mr. Shaw then read a paper on an electrical micrometer. In this paper the motion of the centre of a telephone diaphragm was measured by means of a system of levers and a spherometer screw. The screw, which had a pitch of 0·5 mm. and a head divided into 500 parts, pressed against the long arm of an aluminium lever. The short arm of this lever pressed against the long arm of another, and so on through three levers. In this way any motion of the spherometer screw was transmitted to a fine platino-iridium point close to a small platino-iridium disc fastened to the centre of the telephone diaphragm. Since the head of the spherometer could be accurately read to 0·1 of a division by means of a telescope, and since the system of levers minified any motion a hundred-fold, it follows that an accurately observable twist of the spherometer head corresponds to a movement of a millionth of a millimetre or 1 $\mu\mu$ of the fine point. To test the action of the levers, the point was removed and a convex lens substituted. This lens formed one of a system by means of which Newton's rings were produced and observed. By means of an optical experiment, the author has found that 0·1 of a division on the graduated head equals 1·033 $\mu\mu$ at the platino-iridium point. The point and the diaphragm then formed part of a circuit containing an ordinary telephone, and the levers were so adjusted that the point just touched the diaphragm. A sharp click was then heard in the telephone. A small current was then sent through the electro-magnets of the original telephone, and the displacement of the diaphragm measured by turning the spherometer screw until the point just touched it and a second click was heard. By carrying out a series of experiments of this description, a curve has been drawn showing the relation between current strength and diaphragm displacement. It is then interesting, by exterpolation from the curve, to find the movement which corresponds to the least audible sound. The author has done this, and finds that he cannot hear sounds if the amplitude is less than 0·37 $\mu\mu$. A motion of 50 $\mu\mu$ gives comfortable sounds, 1000 $\mu\mu$ uncomfortable sounds, and 5000 $\mu\mu$ sounds unbearably loud. Throughout the experiments it was necessary to get rid of extraneous vibration by means of indiarubber balls and door-spring suspensions, and by working at night. Prof. Everett expressed his interest in the delicacy of the system of measurement, and asked if the micrometer had been used to determine the form of the plate when vibrating. Mr. Phillips asked if experiments on the smallest sound audible had been made on different people, as it would be physiologically interesting to know if this minimum value were constant. Mr. Campbell asked if the sound was expected when heard. Mr. Shaw said he had not conducted experiments on the form of the plate when vibrating, although he had investigated its law of damping. He said the small sounds were expected and the limit varied. The chairman said he found it easy to rid galvanometers and electrometers from extraneous disturbance by placing them on a block of stone resting on a thickness of three or four feet of slag wool contained in a hollow brick pillar.—The Society then adjourned until April 27, when the meeting will be held at eight o'clock in the Solar Physics Observatory of the Royal College of Science.

Chemical Society, March 8.—Prof. Thorpe, President, in the chair.—Prof. Warington delivered a lecture on recent researches on nitrification.

March 15.—Prof. Thorpe, President, in the chair.—The following papers were read:—The vapour densities of dried mercury and mercurous chloride, by H. B. Baker. Carefully dried mercurous chloride seems to have the molecular composition Hg_2Cl_2 at 448°, thus differing from the undried material, which is known to dissociate at this temperature. Carefully purified and dried mercury is monatomic at 448°.—The preparation of pure hydrobromic acid, by A. Scott. Pure hydrobromic acid is very conveniently prepared by the action of sulphurous acid upon bromine, the product being easily separated, by two or three distillations, from the sulphuric acid simultaneously formed.—A new sulphide of arsenic, by A. Scott. On allowing an arsenate to react with phosphorus trichloride and sulphurous acid at ordinary temperatures a dark brown precipitate is formed, which consists of a new arsenic sulphide having the composition As_3S.—The action of iodine on alkalis, by R. L. Taylor. The author shows that the action of iodine upon alkalis in the cold is always the same in the first instance, and consists in the formation of hypoiodite and iodide; the hypoiodite, however, decomposes more or less rapidly, according as the solution is more or less concentrated, into iodate and iodide.—The interaction between sulphites and nitrites, by E. Divers and T. Haga.—The sym-dipropyl, sym-diisopropyl- and $\alpha\alpha'$-propylisopropyl-succinic acids, by W. A. Bone and C. H. G. Sprankling. The authors show that, contrary to the received view, each of the above alkyl-substituted succinic acids exists in two stereoisomerides; both *cis*- and *trans*-isomerides yield their own anhydrides with acetic chloride, and the anhydrides give characteristic anilinic acids with aniline.—Mannogalactan and lævulo-mannan; two new polysaccharides, by J. L. Baker and T. H. Pope. The Indian clearing nut (*Strychnos potatorum*) yields an amorphous manno-galactan on extraction with hot dilute alkali; the new substance gives a mixture of two parts of galactose to one of mannose on hydrolysis. The ivory nut (*Phytelephas macrocarpa*) similarly yields a lævulo-mannan which on hydrolysis gives a mixture of twenty parts of mannose with one of lævulose.—Hydrolysis of semicarbazones, by G. Young and E. Witham.—The dissociation constant of azoimide, by C. A. West. Determinations of the electrical conductivity of aqueous solutions of azoimide show that this substance resembles acetic acid in acid character.—Racemisation occurring during the formation of benzylidene, benzoyl and acetyl derivatives of dextro-ac-tetrahydro-β-naphthylamine, by W. J. Pope and A. W. Harvey. During the formation of the benzylidene, benzoyl and acetyl derivatives of dectrotetrahydro-β-naphthylamine nearly, but not quite, all of the material undergoes racemisation.

Geological Society, March 7.—J. J. H. Teall, F.R.S., President, in the chair.—Notes on the geology of Gilgit, by Lieut.-General C. A. McMahon, F.R.S. Briefly stated, the author's conclusions are as follows:—That at one period in the elevation of the Hindu Kush the strata were thrown into a series of folds and compressed into a series of uniclinal beds with a vertical dip. That the direction of the main drainage of the area was determined before, or at the commencement, of the last series of earth-movements that crumpled up the strata. The sedimentary rocks were profusely invaded by granite and diorite, and profoundly metamorphosed by contact-action. As regards the age of the rocks, the author gives his reasons for identifying the Gilgit limestones with the conformable Carbo-Triassic series of the Himalaya.—The rocks of the south-eastern coast of Jersey, by John Parkinson. In this paper the author has continued the study of the deep-seated rocks of Jersey, begun in a communication presented to the Society last session, entitled, "On an intrusion of granite into diabase at Sorel Point (Northern Jersey)." A great resemblance exists between these rocks in the north and south of the island, and it is concluded that they represent parts of the same magma; but in the south-east additional complications arise, owing to the intrusion of another rock before the invasion of the granite.—The rocks of La Saline (Northern Jersey), by John Parkinson.

Anthropological Institute, March 13.—Mr. C. H. Read, President, in the Chair.—A photographic slide, presented by Mr. Sidney Hartland, was exhibited, representing the figure of a War God from Boma, in the Congo State (now in the museum of Leyden), into which numerous nails have been driven, probably in registration of the prayers or vows of worshippers. The President compared a similar figure in the possession of Miss M. H. Kingsley, in which the nails were explained as records of lives taken through the magic power of the God.—Mr. A. L. Lewis read a paper on "Stone Circles in Scotland," which he classified according to local types as follows: (1) the Western type, consisting of a single ring of stones with a cist or grave within the enclosure; (2) the Inverness type (found also locally along the east coast, north of Inverness, and easily accessible thence by sea) with two concentric rings, of which the inner formed the retaining wall of a cairn, under which was a stone lined sepulchral chamber accessible by a stone lined passage; (3) the Aberdeen type, which differs mainly from that of Inverness in the presence of a large slab set vertically between the two largest stones of the outer ring at a point opposite to the passage leading to the chamber. The more irregular circles and alignments, such as Callernish and Brogars, which the author regarded as not primarily sepulchral, and explained as "sun and star" circles, on the ground of their aspect, and of certain proportions which were found to exist among their dimensions. He insisted upon the ethnological value of the various local types, and upon the importance of testing this by applying a similar classification to the stone circles of England. In discussion, Mr. W. Gowland pointed out that failure to find traces of an interment within a circle did not prove that that circle was not a sepulchral monument originally; and emphasised the points of agreement between the Western, the Inverness, and the Aberdeen types of circle. Dr. J. G. Garson discussed the modes of determining the age of stone circles, in view of the work of the Stone Circles Committee of the British Association. Mr. G. L. Gomme protested against the premature adoption of an astronomical interpretation of individual monuments. Mr. Lewis briefly replied; and the President, in returning thanks, dwelt on the necessity of collecting the local traditions as to the original use of these monuments, and at the same time of distinguishing, as in the case of the Yorkshire "Danes Graves," between aboriginal and immigrant sources of tradition.—Mr. J. L. Myres exhibited and described a series of photographs of the megalithic buildings of Malta and Gozo, and pointed out the inapplicability of certain current theories of their origin.

EDINBURGH.

Royal Society, February 19.—Prof. McKendrick in the chair.—Sir John Sibbald read a paper on the statistics of suicide in Scotland. The various tables were arranged to bring out such features in the statistics of suicide as the influence of sex and age, of season, of locality, of town and country, and so on. The prevalent idea that statistics proved an increasing tendency in suicide was shown to be a too hasty deduction from the figures. When the statistics of suicide by hanging—the one method in which there was very little chance of mistake—were compared for the last fifteen years and for the preceding period of fifteen years, the number of suicides per million was exactly the same. Then a careful scrutiny of the returns for deaths by accident showed that the apparent increase in suicide by such methods as drowning, shooting, poisoning, &c., was balanced by a decrease in deaths by accident due to the same causes. It would, therefore, appear that the apparent increase in suicides in the last fifteen years was due, not to a real increase in suicide, but to improved methods of discriminating between suicide and accident. The statistics clearly established the fact that the suicidal rate was less in the western than in the eastern counties of Scotland, a fact which Dr. Clouston, in the after-discussion, explained as being in all probability due to difference of race, the greater Celtic element in the west producing, not necessarily a less suicidal disposition, but a less determined carrying out of the deed of self-destruction.

March 5.—Prof. Duns in the chair.—A paper, by Dr. Thomas Muir, on certain aggregates of determinant minors, was taken as read.—Mr. John Aitken, F.R.S., communicated a paper on the dynamics of cyclones. Attention was first drawn to the conditions under which cyclonic motion was developed both in air and in water; and the dynamic principles underlying the production of the phenomenon were illustrated by means of a neat arrangement of balls hung at the ends of two parallel wires, the whole being capable of rotation about a central vertical axis. When drawn together by pulls along threads which passed through the axis of rotation, the two balls were made to spin round one another with a rapidly increasing angular velocity, thus illustrating the important principle of the conservation of moment of momentum. By a simple modification, the apparatus could be made to illustrate the principle of the conservation of energy. Mr. Aitken emphasised the importance of giving increased attention to the anti-cyclonic distributions which in a sense may be regarded as playing, relatively to the accompanying cyclonic distributions, the same *rôle* as is played by the condenser relatively to the boiler of a steam engine. The direction and rate of movement of a cyclone was shown to be determined by the position and configuration of the region where the isobars were closest; a cyclone whose isobars form a set of concentric circles having little or no translatory motion. This characteristic was explained by the author as due to the direct influence of the anti-cyclonic vortex. Many of the features of cyclones were illustrated by means of an ingenious apparatus in which the necessary upward draught was produced in a tall chimney, the whirls of air developed beneath being made visible by the use of sal-ammoniac fumes. The crossing of currents at different heights was beautifully demonstrated. In conclusion, Mr. Aitken referred to the physiological effects observed in the front area of a cyclone, and thought that these might be explained as due to the impure air rising from the ground. In the after-discussion, Prof. Crum Brown drew attention to the experiments by which Prof. Hunter Stewart had established the fact that the soil breathed out a great deal of carbonic acid gas, and no doubt other emanations as well. Mr. Omond pointed out that the dissimilarity as regards relative dimensions between Mr. Aitken's model and the real cyclone should make us very cautious in applying the results obtained with the model to the explanation of cyclonic effects. Dr. Buchan said that, although most of the storms of the north-west of Europe travelled westwards and were characterised by high westerly and south-westerly winds, there were occasionally cyclones which travelled eastwards, and these were always characterised by high east winds. Dr. Knott took the opportunity of protesting against the tendency of speaking of a cyclone as something independent of the winds that really constituted it. Given a cyclonic condition moving through the air, it is obvious that the strongest winds will be on the whole in the direction of that movement, and consequently the isobars will be closest where the associated wind has this direction. It is merely expressing the same truth in different ways to say that a westward travelling cyclone in our latitudes has its isobars closest to the south and is characterised by high west winds, that a slow moving or motionless cyclone has a symmetrical arrangement of isobars, and that an eastward travelling cyclone has its isobars closest on the north and is characterised by high east winds.—Dr. W. G. Aitchison Robertson read a note on the activity of saliva in diseased conditions of the body, being a continuation of a previous paper. In many diseases the digestive activity of saliva on starchy foods underwent a great diminution. This was particularly the case in disorders of the stomach, and the importance of selecting a proper dietary in such cases was insisted on. In many instances a more thorough examination of the saliva than is customary would almost certainly lead to valuable conclusions, and an examination of this kind had the great merit of being extremely easy.

PARIS.

Academy of Sciences, March 19.—M. Maurice Lévy in the chair.—Forces related to the state of perfect elasticity that dynamic contraction creates in the muscle substance. The physiological work intimately constituted by this creation, by M. A. Chauveau.—On linear partial differential equations of the second order with constant coefficients, by M. J. Coulon.—On differential systems with fixed critical points, by M. Paul Painlevé.—On multiplex telegraphy, and a differential telemicrophonic relay, by M. E. Mercadier. A description of a new microphonic relay by means of which it is possible to send a large number of messages simultaneously over the same wire. Between the two end stations, intermediate stations working together with the extremes can be interposed without any difficulties arising.—Relations between the electrolytic conductivity and internal friction of saline solutions, by M. P. Massoulier. Solutions of sulphate of copper in solutions of glycerine and water of various strengths were employed. The resistances were measured both by the Lippmann electrometric method and by the telephone with alternating currents, the viscosity by Poiseuille's method. At 15° the rises of resistance and viscosity with increase of glycerine are proportional, but this does not appear to hold at 0° C.—On a quartz thermometer for high temperatures, by M. A. Dufour. Two quartz thermometers have been prepared, one containing tin, capable of measuring temperatures between 240° and 580° C., and another containing mercury. The study of the zero residues in the quartz-mercury thermometer is under consideration.—Fluorescence of certain metallic compounds when submitted to the Röntgen and Becquerel rays, by M. Paul Bary. Numerous salts of the metals of the alkalies and alkaline earths are divided into two groups according as they were found to be fluorescent or non-fluorescent in the X-rays. The substitution of a radio-active substance for the Crooke's tube showed that all bodies which fluoresce with the X-rays present the same phenomenon with the Becquerel rays. The division proved to be somewhat arbitrary, no general relation between the position of a salt and its chemical composition being apparent.—On the hydrated peroxides of barium, by M. de Forcrand. A calorimetric study of the action of solutions of hydrogen peroxide upon baryta.—On the separation of the rare earths, by M. R. Chavastilon. In the separation of thorium and cerium from lanthanum and didymium, the author reverses the method of M. Urbain and keeps the thorium in solution. Two methods are proposed; in the first the solution of the rare earths is poured into an excess of neutral sodium sulphite, the cerium, lanthanum and didymium being precipitated as sulphites, the thorium remaining in solution. In the second method, the solution of the rare earths is precipitated by adding ammonia and hydrogen peroxide, from which precipitate alkaline bicarbonates extract the thorium and ceriux oxides only.—Chemical reactions produced in a solution; vapour tension of the solvent, by M. A. Ponsot. A mathematical discussion of the conditions under which the vapour pressure of a liquid increases, when reactions occur between substances dissolved in it.—On the detection, estimation and variations in cystine in polluted water, by M. H. Causse. The reagent used is the chloro-mercurate of sodium *p*-diazobenzenesulphonate, which produces with cystine a yellow-orange coloration, the depth of which is proportional to the quantity of cystine present. The author has been able to trace a direct connection between the presence of cystine in a drinking water and an outbreak of typhoid fever.—On certain phenomena presented by nuclei under the action of cold, by MM. L.

Matruchot and M. Molliard. In the plant studied (*Narcissus Tazetta*) the action of cold produced nuclear deformations, which are evidently related to the respective positions of the nucleus and the cell fluid. The most obvious phenomenon is an orientation, generally bipolar, of the chromatic portion with a more or less complete condensation of the chromatin in the equatorial region.—On the toxicity of the compounds of the alkaline earths with respect to higher plants, by M. Henri Coupin. Of the salts tried, barium chlorate proved to be the most injurious. The iodine compounds of all three alkaline earths possess a much higher toxic effect than the other halogen compounds, and for a given acid the toxicity increases with the atomic weight of the metal.—On the pure culture of a green algæ: formation of chlorophyll in the dark, by M. Radais. Comparative experiments on the cultivation of *Chlorella vulgaris* in the daylight and in the dark showed that the multiplication of the cells was the same in both cases. After about a fortnight's growth at 25°, the green tint was also the same in both, the identity of the green colouring matter formed in the dark with chlorophyll being proved spectroscopically.—The andesitic volcano of Tifarouine (Algeria), by M. L. Gentil.—Specific heat of the blood, by M. H. Bordier. The measurements were made by the method of cooling, the upper starting temperature being 45°. Arterial blood, defibrinated blood and serum gave ·901, ·920 and ·932 respectively. The specific heat of arterial blood is greater than that of venous blood (·893). It follows from these figures that the specific heat of the organism taken as a whole must be nearer 0·7 or 0·8 than 1·0 as usually assumed.—Restoration of the functions of the heart and central nervous system after complete anemia, by M. Fréderic Batelli.—Method for the examination and measurement of taste, by MM. Ed. Toulouse and N. Vaschide. Four solutions were employed of salt, sugar, quinine bromhydrate and citric acid, which were systematically diluted. Special precautions as to temperature and mode of contact with the tongue were made, the start being made with a tasteless solution, the strength of which was gradually increased until the taste became perceptible.—Some considerations concerning the freezing of water, by M. F. Bordas.

DIARY OF SOCIETIES.

THURSDAY, MARCH 29.

ROYAL SOCIETY, at 4.30.—On the Retinal Currents of the Frog's Eye, excited by Light and excited Electrically: Dr. Waller, F.R.S.—Observations on the Electromotive Phenomena of Non-medullated Nerve: Miss Sowton.—Variation: Prof. Ewart, F.R.S.—Certain Laws of Variation: Dr. H. M. Vernon.—(1) Data for the Problem of Evolution in Man. IV. Note on the Effect of Fertility depending on Homogamy. (2) Mathematical Contributions to the Theory of Evolution. VII. On the Inheritance of Characters not capable of Exact Quantitative Measurement: Prof. K. Pearson, F.R.S.

ROYAL INSTITUTION, at 3.—Equatorial East Africa and Mount Kenya: H. J. Mackinder.

CHEMICAL SOCIETY, at 3.—Annual General Meeting.——At 8.30.—Bunsen Memorial Lecture: Sir Henry Roscoe, F.R.S.

SOCIETY OF ARTS (Indian Section), at 4.30.—The Manufacture and Use of Indigo: Christopher Rawson.

FRIDAY, MARCH 30.

ROYAL INSTITUTION, at 9.—Facts of Inheritance: Prof. J. A. Thomson.

SATURDAY, MARCH 31.

ROYAL INSTITUTION, at 3.—Polarised Light: Lord Rayleigh.

MONDAY, APRIL 2.

SOCIETY OF ARTS (Foreign and Colonial Section), at 4.30.—The Century in our Colonies: Right Hon. Sir Charles Wentworth Dilke, Bart., M.P.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.—Explorations in Central Asia: Captain H. H. P. Deasy.

VICTORIA INSTITUTE, at 4.30.—North Polar Thalassography: Cavaliere Jervis.

TUESDAY, APRIL 3.

ROYAL INSTITUTION, at 3.—Structure and Classification of Fishes: Prof. E. Ray Lankester, F.R.S.

SOCIETY OF ARTS, at 8.—Process Engraving: Carl Hentschel.

ROYAL METEOROLOGICAL SOCIETY, at 3.—Commemoration Meeting.—Address by Dr. C. Theodore Williams.

ZOOLOGICAL SOCIETY, at 8.30.—On *Mus sylvaticus* and its Allies. Subspecies, and Geographical Variations: G. E. H. Barrett-Hamilton.—Notes on the Mammals of Siam and the Malay Peninsula: Stanley S. Flower.—On a Remarkable New Piece of Skin from Cueva Eberhardt, Patagonia: Dr. Einar Lönnberg.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Economical Railway Construction in New South Wales: Henry Deane.—The Tocopilla Railway: Robert Stirling.

ROYAL PHOTOGRAPHIC SOCIETY, at 8.—F. P. Ambrano will show Slides, Old and New.

MINERALOGICAL SOCIETY, at 8.—Hamlinite, Florencite, Plumbogummite (Hitchcockite), Beudantite and Svanbergite as Members of an Isomorphous Group: G. T. Prior.—On the Optical Properties of Chalybite and Diallogite: Dr. A. Hutchinson.—Ægirine (and Riebeckite) Anorthoclase Rocks related to the "Grorudite-Tinguaite" Group from the Neighbourhood of Adoa and Axum, Abyssinia: G. T. Prior.—The Chemical Composition of the Mount Zomba Meteorite: L. Fletcher, F.R.S.

WEDNESDAY, APRIL 4.

SOCIETY OF ARTS, at 8.—Cotton Supplies: John A. Banister.

GEOLOGICAL SOCIETY, at 8.—Additional Notes on some Eruptive Rocks from New Zealand: F. Rutley.—On the Discovery and Occurrence of Minerals containing Rare Elements: Baron A. E. Nordenskiöld.

ENTOMOLOGICAL SOCIETY, at 8.

SOCIETY OF PUBLIC ANALYSTS, at 8.—Note on the Influence of Temperature and Concentration on the Saline Constituents of Boiler Waters: Cecil H. Cribb.—On an Improved Absorption Apparatus for Use in the Analysis of Essential Oils: Alfred C. Chapman and H. E. Burgess.—On the Composition of Danish Butters: H. Faber.—The Composition of Milk and Milk Products: H. Droo Richmond.

THURSDAY, APRIL 5.

ROYAL SOCIETY, at 4.30.—*Probable Papers*: On the Weight of Hydrogen desiccated by Liquid Air: Lord Rayleigh, F.R.S.—Combinatorial Analysis: The Foundations of a New Theory: Major MacMahon, F.R.S.—Über Reihen auf der Convergenzgrenze: Dr. E. Lasker.—Extinct Mammalia from Madagascar. I. *Megaladapis insignis*, sp. n.: Dr. C. J. Forsyth Major.—The Kinetic Theory of Planetary Atmospheres, Part I.: Prof. E. H. Bryan, F.R.S.—Observations on the Effect of Desiccation of Albumin upon its Coagulability: Prof. J. B. Farmer.

ROYAL INSTITUTION, at 3.—Equatorial East Africa and Mount Kenya: H. J. Mackinder.

MATHEMATICAL SOCIETY, at 5.30.—The Orthoptic Loci of Curves of a Given Class: A. B. Basset, F.R.S.

LINNEAN SOCIETY, at 8.—*Sphenophyllum* and its Allies, an Extinct Division of the Vascular Cryptogams: Dr. D. H. Scott, F.R.S.

CHEMICAL SOCIETY, at 8.—(1) The Liquefaction of a Gas by "Self-Cooling": A Lecture Experiment; (2) Note on Partially Miscible Aqueous Inorganic Solutions: G. S. Newth.—The Decomposition of Chlorates. II. Lead Chlorate: W. H. Sodeau.—The Interaction of Mesityl Oxide and Ethyl Sodiomethylmalonate: A. W. Crossley.—The Bromination of Benzeneazophenol: J. T. Hewitt and W. G. Aston.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.

RÖNTGEN SOCIETY, at 8.—The Influence of the X Rays upon the Growth and Development of Micro organisms: Dr. Norris Wolfenden and Dr. Forbes Ross.

FRIDAY, APRIL 6.

ROYAL INSTITUTION, at 9.—Solid Hydrogen: Prof. J. Dewar, F.R.S.

GEOLOGISTS' ASSOCIATION, at 8.—Zonal Features of the Kentish Chalk-Pits between London and the Medway Valley: G. E. Dibley.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Experiments on Struts with and without Lateral Loading: H. E. Wimperis.

SATURDAY, APRIL 7.

ROYAL INSTITUTION, at 3.—Polarised Light: Lord Rayleigh.

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