THURSDAY, AUGUST 20, 1896.

A SYSTEM OF MEDICINE.

A System of Medicine. By many Writers. Edited by Thos. Clifford Allbutt, M.A., M.D., F.R.S., Regius Professor of Physic in the University of Cambridge, &c. Vol. i. Demy 8vo, pp. 1008, 38 illustrations, 13 charts, 1 coloured plate. (London: Macmillan and Co., Ltd., 1896.)

IT is almost exactly thirty years ago since the first volume of the "System of Medicine," edited by Sir Russell Reynolds, was published; and the dedication of the present work to him is a touching acknowledgment, on the part of the editor, of the service rendered to medicine by the late lamented President of the College of Physicians, and his band of collaborators. Thirty years has produced not only great changes in medicine itself, but also great changes in the methods of attacking medical problems. The intimate relation in which medicine to-day stands to what may, perhaps, collectively best be termed experimental pathology, has rendered necessary in the present System, the collaboration of a band of authors whose names are better known as physiologists, pathologists, or bacteriologists than physicians, and their contributions lend a very special value to the work. The book begins with an academic introduction by the editor, who at once rightly denies any corporeal fixity to the term "System of Medicine," and defines it as "a setting forth of the whole of our knowledge, as immediate convenience and the exigencies of contemporary learning may dictate." A few brief but pregnant remarks upon classification, nomenclature, diagnosis and prognosis, lead us to Division i. of the System, which has received the name of prolegomena. Each of the articles, twenty in number, contained in this part of the work is complete in itself, and may be regarded as representing the present state of our knowledge upon the subject of which it treats.

Medical Statistics are dealt with by Dr. Billings in a most able and exhaustive manner; Anthropology and Medicine by Dr. Beddoe. Dr. Rivers, in a short essay on Temperament, points out that a valuable classification of temperaments may be expected from a study of this subject on the lines of Galton and Kraepelin. Mr. Hutchinson, in a few pages, discusses "The Laws of Inheritance in Disease." Dr. Haviland tells us succinctly what is known with regard to the relations of the physical geography of this country to heart disease, cancer and phthisis.

One of the most difficult tasks in the book has fallen to the lot of Prof. Adami, who contributes the monograph on Inflammation. The article is divided into three parts. Part i. is devoted to the comparative pathology of inflammation; chapter i. is introductory; chapter ii. contains an account of the researches of Metschnikoff, Krukenberg, Reinke, and others, on the effects of irritants on the protozoa and lower metazoa; chapter iii. describes the main forms of the process of acute inflammation in the higher animals, in which the experimental production of suppurative inflammation finds a place. Here the author considers at length the work of Cohnheim, Grawitz, and De Barry, Councilman, and others. Part ii. is devoted to the factors in the inflammatory process considered in detail, and first and foremost the part played by the leucocytes. The author here gives, with a table, a classification of leucocytes according to the different authorities. The rival theories of "phagocytosis" and "extra cellular action" are discussed at length. Dr. Adami inclines to the view that although different bacterial products may give rise to positive chemiotaxis of varying intensity, the possession by these products of actual negative chemiotactic properties is very doubtful. The author concludes by saying that however important phagocytosis may be to the organism, the extra-cellular action of active and disintegrating leucocytes may under certain conditions be even more so. The question of the inflammatory exudation, the part played by the vascular and nervous systems, the diapedesis of the leucocytes, which the author regards as active, and caused by the positive chemiotactic properties of the irritant, are each considered. Of the *rôle* played by the cells of the tissues, and of the origin of the "fibroblasts," a full account is given; and this part of the subject concludes with an exhaustive chapter on the varieties of fibrous hyperplasia and their relation to inflammation. Part iii. is devoted to classification and the systemic changes consequent upon inflammation. The author, in conclusion, defines inflammation as "the local attempt at repair of actual or referred injury." In the article just considered, Prof. Adami has certainly accomplished the task he set before himself, viz. "to bring into order the very numerous recent researches upon the inflammatory process, and to show whither they appear to tend."

The Doctrine of Fever is dealt with by Dr. Burdon Sanderson, and divided into two parts. Part i. is a historical retrospect which deals chiefly with the views of Cohnheim, Senator, Traube, Pfluger, and Leyden. Part ii. gives an account of the researches bearing on this subject since 1883. The relation between the increased proteid disintegration in fever and in inanition is discussed, as is the production of "fever," so called, by cerebral puncture and the mode of action of antipyrine and other antipyritics.

An article on the General Pathology of Nutrition is contributed by Dr. F. W. Mott. Dr. Mott first describes the physiology of nutrition, and discusses the influence of the quality of the blood, the internal secretions of the organs, the nervous system, and the inherited specific properties of the blood, upon the maintenance of a normal state of nutrition. Necrosis, atrophy and fibrosis, the degenerations, and hypertrophy are fully dealt with. The author holds the view that in the fatty degeneration of myelin, lecithin is the source of the fat, and suggests that extensive degeneration of this kind may produce an auto-intoxication. The article is well illustrated. A monograph on the General Pathology of New Growths is communicated by Messrs. Shattock and Ballance. The chief interest of this article lies in the full description and searching criticism of the parasitic theory of carcinoma. The authors have spared neither time nor trouble to put this theory to scientific tests. All attempts to cultivate the hypothetical parasite of carcinoma, either directly in various media or through the intervention of lower forms of life, were negative. The treatment of carcinoma and sarcoma by injection of a glycerine extract (50 per cent.) of the respective growth, was also without result, as was also treatment with Fehleisen's fluid.

The Principles of Drug Therapeutics are considered by Prof. Leech. Dr. Herman Weber, assisted by Dr. M. G. Forster, writes upon Climatology. The article comprises a consideration of some of the principal elements of climate, the chief climatic regions and health resorts, and the use of climate in the treatment and prevention of disease. The essay on Balneology and Hydrotherapeutics is from the same author, and is divided into two parts : (A) balneo-therapeutics, or treatment by the internal or external use of mineral waters ; under this section the chief mineral waters are described, and their therapeutic use indicated. The second (or B) section is termed hydrotherapeutics : by this is meant the therapeutic use of water considered especially in its external application to the body ; in this section the author is assisted by Dr. Parkes Weber. Artificial Ærotherapeutics is treated by Dr. Theodore Williams, the varieties of qualitative and quantitative differences in the constituents of the atmosphere are described, as are also the effects of atmospheres of varying barometric pressure. Dr. Lewis Jones writes upon the Medical Applications of Electricity. The units of measurement are explained, and the apparatus necessary for the application of electricity described. Six excellent diagrams of the motor points in the different parts of the body are given. The diagnostic use of electricity is fully discussed. The article closes with a consideration of the conditions in which the medical application of electricity has been of therapeutic value. Dr. Mitchell considers the technique, physiology and therapeutic uses of Massage. Under the head of physiology a *résumé* is given of the work of Lombard, Brunton and Tunnicliffe, Winternitz, and others. The varieties of massage, the duration and frequency of its application, are described, and the diseases in which massage has been useful enumerated. The Feeding of the Sick is the subject of an article by Sir Dyce Duckworth. The general principles of invalid dietetics are first clearly enunciated, and then all the experience of the clinician is brought to bear upon the subject of the diets suitable to various diseases. A most valuable addition to this subject comes from the pen of Dr. Eustace Smith, who writes upon the Diet and Therapeutics of Children. The very special way in which children react to different diets and forms of treatment, baths, drugs, &c., is described, and the way to obtain a maximum benefit from the remedies prescribed is clearly indicated. The editor has wisely included an article on Nursing, which is written by Miss Amy Hughes. The Hygiene of Youth is treated by Dr. Clement Dukes, in a paper which it would be well to place in the hands of every schoolmaster. The medical aspect of Life Assurance is considered by Dr. Symes Thompson. With this article Division i. of the work closes.

Division ii. is devoted to the consideration of the Fevers, and includes two parts. Part i. consists of an article from the pen of Sir Joseph Fayrer, on Insolation or Sunstroke. The Infections form the contents of Part ii. The subject is introduced by an article on the general pathology of infection, by Dr. Kanthack. The Infections are then divided as follows : (1) diseases of more or less established bacteriology, (2) diseases of uncertain bacteriology, (3) infective diseases communicable from animals to man, (4) diseases due to the protozoa. The first group is divided into local or general diseases due to pyococci, and the infective fevers, with which the volume ends.

Dr. Kanthack, in his article on the general pathology of infection, first considers the morphology of micro organisms and the conditions necessary for their existence; he then passes on to the products of bacterial activity. Ferments and enzymes, the ptomaines, toxalbumins, and the products of fermentation are fully discussed. The question whether the toxines are the result of the action of the bacilli on the tissues (Martin), or whether they are a true excretion or secretion of the bacilli themselves, is discussed, as are also the views of Klein on extracellular and intracellular poisons. A consideration of infection, contagion, and predisposition follows. The author then proceeds to the subject of natural and acquired immunity, and the article concludes with the history, principles, methods, and scope of serum therapeutics. Two admirable monographs on septicæmia and pyæmia, and erysipelas, are written by Mr. Watson Cheyne. The ætiology and general pathology of ulcerative endocarditis is dealt with by Dr. Dreschfeld ; Puerperal Septic Disease by Dr. Playfair. The articles on boils and carbuncles are written by Dr. Melsome, that on Epidemic Pneumonia by Dr. Whitelegge. Epidemic cerebro-spinal Meningitis is considered by Dr. Ormerod, who adds an appendix describing the outbreaks of the disease in this country since 1807. Dr. Goodhart writes upon influenza. The article includes a description of Pfeiffer's bacillus, and the methods for its identification and cultivation. The article on Diphtheria is divided into four parts. The clinical aspects of Diphtheria are dealt with by Dr. Gee, its ætiology and prophylaxis by Dr. Thorne Thorne, its bacteriology and pathology by Dr. Kanthack, while Dr. Herringham gives the results of the serum treatment. The subject of Tetanus is also divided between two authors—Sir G. Humphrey contributing the clinical part, Dr. Sims Woodhead the pathological. The relatively unfavourable results of the antitoxine treatment in this disease, as compared with those in Diphtheria, are explained by Dr. Woodhead as being due to the fact that the treatment in tetanus is not begun until the effects of the poison on the general system (central nervous system), as distinct from its local effects, have manifested themselves; whereas in Diphtheria, the antitoxine is injected at a period at which the disease is practically a local one. Enteric Fever is treated in an exhaustive manner by Dr. Dreschfeld. The article contains a description of Eberth's bacillus, with the characteristics distinguishing it from the *B. coli communis*, and the methods for its detection. Extensive mortality tables form an appendix. Five authors are responsible for the article on Asiatic Cholera. Mr. Ernest Hart and Dr. Smith deal with the origin and mode of propagation of the disease, Drs. Kanthack and Stephens with the bacteriology, and Dr. Kenneth Macleod with the clinical, pathological, and therapeutical aspects of the subject.

The Plague is treated by Dr. Payne. The bacteriology of the disease, in the light of Yersin and Kitasato's researches, is fully discussed, and the possibility of treatment by antitoxine (Calmette). Relapsing Fever is dealt with by Dr. Rabagliati; the bacteriology of the subject being from the pen of Dr. Westbrook. With this last article Volume i. closes. At the end of each monograph there is a list of references, which are paragraphed, according to the sub-section of the article to which they refer. An accurate and extensive index of authors and subjects greatly facilitates the use of the book.

An idea of the labour requisite to the successful editing of a work like the one before us can only be formed by the experienced few who have accomplished it. To Prof. Allbutt are due the thanks and congratulations of cosmopolitan Medicine for having produced a work which in fulness, accuracy and interest, leaves nothing to be desired.

F. W. T.

A TEXT-BOOK OF EXPERIMENTAL PHYSICS.

Lehrbuch der Experimental Physik. Von Eduard Riecke. Erster Band. Mechanik, Akustik, Optik. Pp. xvi + 418. (Leipzig: Verlag von Veit and Co., 1896.)

THE author, who is Professor of Physics in the University of Göttingen, is well and favourably known for his work in several departments of physical science, and the text-book he has written possesses the qualities we should expect to find in the work of one who is actively engaged not only in teaching physics, but in advancing the subject by research.

After an introduction on physical phenomena, physical hypotheses and theories, and elementary measurements such as those of angle, length, mass, and time, the treatise enters on the subjects of mechanics and acoustics, to which the first part of the present volume is devoted. This occupies pp. 20–261, leaving pp. 262–418 for the discussion of optics.

In opening his dynamical treatment, the author does not attempt to deal with the vexed questions of the foundations of dynamics, a procedure which is perhaps the best in the interests of beginners. The thoughtful student will be confronted with the fundamental difficulties soon enough, and according to his own ability and the skill of his dynamical adviser will be his relief from the serious mental embarrassment which he will inevitably experience. The thoughtless student need not be considered.

The author postpones the consideration of mass, and defines the unit of weight as the weight of a cubic centimetre of distilled water at maximum density under atmospheric pressure, which he calls a *gramme-gewicht*. We differ from the author as to this being the *definition* of the unit of weight in the metric system. Surely that unit is the weight of the standard kilogramme itself, or of $\frac{1}{1000}$ part of it. By the statement given the unit of weight is not immediately connected with the standard piece of matter; though it is no doubt very nearly $\frac{1}{1000}$ of the weight of the standard kilogramme. The unit of weight is, however, made the weight of a unit of mass, and is therefore, strictly speaking, a variable unit; it is used throughout the treatment of the statics of rigid bodies, with which the dynamical portion of the book begins.

The method adopted thus differs from that now usually followed, in English books at least, by considering a vertical string or bar which supports a body as stretched by a force equal to the weight of the body, as measured by the number of cubic centimetres of distilled water at maximum density which will just equilibrate the body in an accurate balance. Thus, so far as statics is concerned, a preliminary kinetic definition and discussion of force and the laws of motion are dispensed with.

Whatever opinion may be held as to the merits or demerits of this mode of presenting the subject of statics, there can be no question of the importance of referring the student on every possible occasion for illustration of principles and theorems to the great practical applications of dynamics that we have in abundance in engineering structures, and of the use where possible of graphical methods. This is an aim which the author keeps well in view, though in order to proceed to the consideration of stretching force and thrust, and ties and struts, it does seem a little hasty to dismiss the parallelogram of forces with a mere experimental proof by means of strings and weights.

The discussion of the kinetics (*dynamics* the author calls it) of solid bodies follows. This has many points of excellence. But we must rather demur to the comparison of the absolute and technical units, given at p. 67. Thus it is stated—

Absolute.	*Technical.*
Unit of mass equal to the mass of a gramme.	Unit of force equal to the weight of a gramme at latitude 45°.
Unit of force equal to the weight of $\frac{1}{981}$ gramme at latitude 45°.	Unit of mass equal to the mass of 981 gramme weights.

Whatever the somewhat disputed relative merits for different purposes of the absolute and technical system of units may be, it is universally admitted that the absolute C.G.S. unit of force is that force which gives a mass of one gramme an acceleration of 1 centimetre per second per second. To define it, or state its value as above, is to give no doubt a constant unit of force (on the supposition that gravity is constant at latitude 45°), but one differing perceptibly from that usually defined as the absolute unit, since the value of g at latitude 45° at mean sea-level is approximately 980·61 in centimetre second units. The great beauty of the absolute system as given by Gauss, surely lies in the fact that the fundamental units of length, mass, and time do all for us, and we have, so far as the *definition* of dynamical units is concerned, nothing whatever to do with gravity.

The proper definition of the unit of force it must be stated, however, is given *below* this "Gegenüberstellung" of units, so that the author probably does not offer the statement in the table as other than a comparison of values of units; but it is well to give first the definition, and rub it in with plenty of illustration. Only after the student has become perfectly familiar with the system of units he has been introduced to, is it safe to bring another under his notice.

Many interesting cases of motion are well expounded and illustrated by diagrams, for example the motion of a pendulum with a gyrostat in its bob (a case of great importance in other branches of physics), and the precession of the equinoxes.

The statics and kinetics of fluids are next dealt with, and, as was to be expected, fluid motion is well attended to. Wave motion of a fluid and the principle of Huyghens are here discussed and well illustrated graphically. Then follows a wonderfully complete account, for the space devoted to it, of molecular phenomena of solids and fluids, including a sketch of the kinetic theory of gases.

The second half of the book, which deals with acoustics and optics, we have not space to speak of in any detail. Throughout we have clear description of phenomena, of apparatus, and of experimental processes, always with neat and truthfully-drawn diagrams. There is necessarily very little in the way of mathematical exposition of these subjects, but the results of mathematical and experimental investigation are clearly stated, and the book cannot but form an exceedingly useful introduction to an extended course of physical study, such as that for which one of the more elaborate Lehrbücher, which have lately appeared in Germany, might form a basis. It is very clearly printed, and, what is a great thing in a text-book, the numerous cuts have been very well engraved and printed.

A. GRAY.

TRAVELS AMONG THE HAUSA.

Hausaland, or Fifteen Hundred Miles through the Central Soudan. By Charles Henry Robinson, M.A. Pp. 304. Map and illustrations. (London: Sampson Low, Marston, and Co., 1896.)

MR. J. A. ROBINSON, the brother of the author of the book before us, died at Lokoja on the Niger in 1891, while engaged in the study of the Hausa language, and in his memory the Hausa Association was formed in the same year with a view to carry on the work. Mr. C. H. Robinson was selected by the Association as their first "Hausa Student," and he left England at the end of April 1893, to make acquaintance with the language, and also to learn Arabic. Instead of proceeding directly to the land of the Hausas, the climate of which at the best is very trying for Europeans, he went successively to Tripoli and Tunis, where he had opportunities of conversing with many of the Hausa pilgrims on their way from the interior of the Sudan to Mecca, and with any number of Hausa slaves.

Equipped by preliminary training in the language, and accompanied by two friends, Dr. T. J. Tonkin and Mr. John Bonner, Mr. Robinson reached Lokoja, at the confluence of the Niger and Benue, in August 1894; and, after wearisome delays in obtaining carriers, set out for Kano, the commercial capital of the Hausa states. They travelled up the Benue to Loko, then struck northwards by land through Kaffa and Zaria, and reached Kano on December 23. Here the party stayed for rather more than three months, engaged in diligent study, and then returned to the Niger and to Europe.

Hausaland is not a defined geographical area, but a group of native states occupying the fertile region of the Western Sudan between the territory of the Royal Niger Company (to which they are tributary) and the Sahara desert. Sokoto is the predominant state, but the chief town of the Hausas is Kano, a trade-centre of such importance that Mr. Robinson does not hesitate to dub it the "Manchester of the Soudan." The town is described in some detail, and this description is perhaps the most important feature of the book, which in other parts suffers from "padding," including long extracts from various writers on non-essential subjects. Kano is estimated to contain about 100,000 inhabitants; it manufactures much cloth, which is largely exported, and may be purchased in Alexandria, Tunis, or Lagos, so widely are its qualities appreciated. The markets also contain European goods, which are still, for the most part, brought across the Sahara from Mediterranean ports. The Hausas are born traders, and having acquired Mohammedan education, are by no means to be viewed as savages. They are, however, inveterate slave-traders; and Mr. Robinson is of opinion that this trade cannot be seriously combated until a satisfactory currency and mechanical means of transport are introduced. At present slaves are the larger, and cowries the smaller, units of value; and as a slave is worth several hundred thousand cowries, the carriage of his value in these shells would tax the resources of any caravan. There is one coin which passes current through the whole of Northern Africa—perhaps the last in Europe which would occur to any one set to guess its nationality and date—the silver Austrian thaler coined in 1780 during the reign of Maria Theresa. Mr. Robinson urges a large importation of these coins as a measure to promote trade and discourage slavery. He has shown himself to be a diligent student and a good observer, although a somewhat diffuse writer, and the suggestion is worthy of consideration.

In the preface we are told that the Royal Geographical Society's system of spelling place-names has been followed; but this is only done in part. The French transliterations are used in some cases, and also other forms—as, for example, Abutshi, Bornou, Gandja, Soudan, Tchad; where the system referred to would require—Abuchai, Bornu, Ganja, Sudan, Chad.

It is to be hoped that the efforts of the Hausa Association will not be allowed to cease for want of money; for Mr. Robinson's linguistic work is of real value, and its importance will appear more fully when the facsimiles and translations of the Hausa MS. which he has brought home, together with his dictionary and grammar of the language, are published.

OUR BOOK SHELF.

Practical Mechanics, applied to the Requirements of the Sailor. By Thomas Mackenzie, Master Mariner, F.R.A.S., &c. Pp. xii + 175. (London: Charles Griffin and Co., Ltd., 1896.)

THIS book is one of a very useful series on nautical subjects, published in order to meet a desire on the part of the officers of the Mercantile Marine to obtain a more scientific insight into the principles of their profession.

It is becoming more generally recognised that the really "practical" man is the one who combines practical knowledge and experience with intelligent appreciation of underlying principles.

The aim of the book is to lay before sailors, in an easily comprehended manner, the principles on which the various mechanical devices employed by them are founded.

A large amount of useful information has therefore been gathered together in the small compass of this book, and rules and principles whose *general* application is explained in various text-books on practical and applied mechanics, are here specially adapted to the requirements of the sailor. To mention a few instances: we find explanations of the mechanical advantage gained by the various tackles and purchases; the construction of derricks and shears, and the weight they will support; the relative strengths of ropes; the breaking strains of spars; the floating power of spars and casks, and the weight which a raft, constructed of given materials, will bear; the effect of the wind on the sails in driving the ship ahead and in causing leeway; the effect of the water on the rudder; the extra strain thrown on slings when a ship is rolling, &c.

The principles of the composition and resolution of forces, and of the mechanical powers are somewhat fully explained in the opening chapters, and the idea of applying the traverse table, so familiar to all sailors, to the solution of the problems, is an excellent one; but more explanation of some of the rules given later, which have to be taken for granted, could be desired, as it is very difficult to retain bare rules in the memory.

The size of the book no doubt imposed limits on the amount of space to be devoted to explanation, but it provides, nevertheless, an excellent book of reference; and though it may not be necessary to make some of the calculations referred to, it is always useful to know how things are worked, and on what principles, and that the principles are in accordance with well-known physical laws. "A sailor is so often thrown on his own resources, and the more exact his knowledge is of natural forces, the more readily can he avail himself of the forces at hand."

The book contains several valuable tables, and a useful collection of rules in mensuration.

F. C. STEBBING.

Power Locomotion on the Highway. By Rhys Jenkins, M.I.Mech.E. Pp. 64. (London: William Cate, Ltd., 1896.)

THE sub-title of this publication sufficiently expresses the character of the contents; it is "a guide to the literature relating to traction engines and steam rollers and to the propulsion of common road carriages and velocipedes by steam and other mechanical power, with a brief historical sketch." The historical sketch is a concise statement of the lines along which progress in power locomotion on common roads has proceeded. Following it is a bibliography of works on mechanical carriages and traction engines, a catalogue of papers read before, or appearing in the Transactions of, scientific and technical societies, indexed under names of authors, a list of journals devoted to the mechanical carriage movement, and an index to articles on the subject in periodical literature up to the end of 1895. The periodicals indexed include those of the United States, France, and Germany, as well as of Great Britain. The author has evidently been at considerable pains to prepare his descriptive index, and his efforts deserve encouragement. It would be an immense boon if indexes of the same description were available for other branches of technology. The reception afforded to this little book will show whether the demand is sufficient to justify the publication of others of a like kind.

LETTERS TO THE EDITOR.

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

The Utility of Specific Characters.

I REGRET very much that I did not correctly remember, when writing to NATURE a month ago, what my friend Mr. Thiselton-Dyer had said at the Linnean Society's meeting. I suppose that in consequence I must not greatly complain that whilst telling us what he really did say, my friend has taken the opportunity to present a version of my views which is far from accurate. He has less excuse than I had in attempting to cite his remarks, since he has before him my printed letter of July 16. This fact also renders it easy to show wherein he is inaccurate.

I suppose that we are all agreed that it is in the highest degree interesting to know what Mr. Darwin himself thought and said on questions of the kind now under discussion. At the same time, we are none of us, I imagine, likely to attribute to Mr. Dyer a special knowledge either of Darwin's writings or of their interpretation which we do not share. Naturalists are, I believe, not prepared to accept any individual as the authoritative exponent of Mr. Darwin's teaching. Under these circumstances it is to me a matter for regret that a plain discussion of the question whether specific characters are invariably useful should be turned into a dispute as to whether the person who suggests some special application of Mr. Darwin's doctrines, or advances some subordinate hypothesis in relation to their application, can or can not be solemnly regarded as an Orthodox Darwinian. Mr. Dyer says that the Darwinian theory seems hardly to have a convinced supporter left except Mr. Wallace. He denounces my quotations from Mr. Darwin's own books as to correlation of variation as a "difficulty" brought up by me "against" the Darwinian theory; then, without more ado, assumes the *rôle* of apostle of the Darwinian theory (a part to which I cannot allow him any exclusive claim), and proceeds to tell us what "will" be found in the twenty-fifth chapter of Mr. Darwin's "Animals and Plants under Domestication," viz. that Darwin has said pretty much all that can (as yet) be said about the facts of correlation of variation. The attempt on Mr. Dyer's part to represent my citation of Mr. Darwin's own conclusions in regard to correlation of variation as unorthodox, is a little beside the mark. No one is ignorant that it "will" be found that in the chapter cited Mr. Darwin discusses "correlation"; not only that, but it *has been* so found long ago and repeatedly by many other readers besides Mr. Dyer. I expressly stated, both at the Linnean Society and in my letter printed in NATURE on July 16, that I was quoting from Mr. Darwin both as to Wells and as to other instances.

Perhaps the most unsatisfactory feature in Mr. Dyer's claim to classify his friends as heterodox and orthodox in regard to Darwinism, is that it leads him to undertake to give away the Darwinian theory. "I frankly admit," he says, "that such a case [a case of correlated variation such as that hypothetically stated by me] if completely established would give the utility of specific characters, and with it the Darwinian theory, a serious blow." I do not value this frank admission. If Mr. Dyer feels constrained to admit to some one that such a case would give a serious blow to the Darwinian theory, he must not come to me with his "admission" of a "point scored"; for I neither admit that any such blow is given, nor can I accept Mr. Dyer's good-natured offer to act as representative of the Darwinian theory. All that my friend can represent in this matter is the Dyerian theory of Mr. Darwin's theory. Mr. Darwin never asserted in so many words that specific characters are invariably "useful," and in my judgment he did not hold that opinion. But whether he did or did not, that opinion can not, I think, be shown to be a necessary outcome of the theory of natural selection, provided that we take into consideration important ascertained properties of living matter. My impression is that Mr. Wallace—whom Mr. Dyer has declared to be the only convinced supporter of the Darwinian theory now left—stated at the Linnean Society that he agreed with me as to "correlated variation" sometimes accounting for a specific character which accordingly could not be regarded as due to utility. Such also I gathered was the view of Mr. Meldola. Yet neither of these gentlemen regarded this conclusion as a serious blow to the Darwinian theory. Nor do I; on the contrary, I regard it as an important aid to the general application of the Darwinlan theory.

Apart from the objection, which I have above expressed, to the treatment of this question by Mr. Dyer as one of orthodoxy, I note with regret that he has (unintentionally no doubt) misrepresented what I have actually said in my letter. Whereas I have expressly cited Mr. Darwin's principle of "correlation of variations," Mr. Dyer writes of the "extended correlation principle of Prof. Lankester." This form of pleasantry could be itself extended, but more of it would be unworthy of your pages. As a matter of fact, what I said as to correlation was very little more than a citation of cases and their theoretical explanation given by Mr. Darwin. To represent this as in any way parallel to the independent and anti-Darwinian theories of Mr. Romanes and Mr. Bateson, as is done by Mr. Dyer, is misrepresentation. The theory of Wells as to the black races of the tropics was used by me in order to illustrate my suggestion as to correlation leading to a development of useless specific characters. I might, equally well for my purpose, have used any of the other cases collected by Mr. Darwin. Whilst I spoke of Wells' case as "striking and suggestive," I at the same time expressly referred to it as "a more or less hypothetical case." Mr. Dyer has no justification, so far as I can see, for stating that I use this case as a foundation for wide generalisation. I made no wide generalisation, but adduced the wide generalisation at which Mr. Darwin, after collecting and considering a large variety of cases, arrived, viz. that correlation of variation does occur largely and generally in the organic world. This wide generalisation is, I say, not mine; it is Mr. Darwin's. If this generalisation be accepted—and we may reasonably hope that the apostolic and orthodox do accept it—then it seems to me in the highest degree probable that an obscure specific difference of structure—highly effective as life-preserving or progeny-ensuring—will more or less frequently carry with it as a correlated variation a more obvious and measurable character in some remote part of the body, not effective, that is to say, not useful. Hence I conclude that there may be specific characters (not by any means all or always) which are not themselves *useful*, though readily observed. That is the whole of my contribution to the present discussion. It does not seem to me to involve anything rash or surprising, though such is its character according to Mr. Dyer.

I may remind the readers of NATURE that some years ago, in these columns, I adduced this same principle, viz. Mr. Darwin's principle of correlation of variations, as one sufficient to remove some of the difficulties in the way of the doctrine of natural selection brought forward by the Duke of Argyll. What the Duke called "prophetic germs" might, it seemed to me (and still seems to me), when not explicable as lapsed rudimentary structures, be accounted for as variations or new structures correlated with other useful and therefore selected variations, although not yet themselves useful. A useless variation correlated with a useful one must (it seems to me) be supposed to pass through initial stages in which it is too small, or otherwise insignificant, to be useful (its utility or harmfulness being swamped in the utility of the correlated useful character), and only after attaining considerable development becomes either useful or harmful, and therefore subject to selection, possibly under some slight change of environment.

I regret all the more my differences with my friend Mr. Dyer, because the explanations they have involved leave me so little space in which to refer to Prof. Weldon's courteous and interesting letter. I have only one point to correct in his statement of my position in relation to that taken by him. The attempt to reconcile the *dicta* of Hume or Kant, or even of Mill, with the experience and approved practice of those who make it their business to investigate natural phenomena, would be an interesting undertaking, possibly one beyond the powers of living man. It is certainly not one upon which I shall here and now embark.

The point whereon Prof. Weldon has misunderstood my contention is this. After describing a phenomenon (death-rate) preceded invariably by two or more phenomena of structure or function, he says: "Under these circumstances, Prof. Lankester thinks it legitimate to pick out one of these antecedent phenomena and to speak of it as the only effective cause of change in death-rate, the other phenomena, although equally universal, being merely unimportant concomitants of this one essential change." Prof. Weldon is mistaken in stating that I think it legitimate "to pick out" without qualification, or at haphazard, any one of these antecedent phenomena, and to speak of it as the only effective cause. I should object to such a proceeding on much the same ground as that on which I object to his calling "any and all" of those antecedent phenomena effective causes.

What I think is the reasonable course in such a case—supposing that a man wishes to ascertain, as fully as may be, the relation of these phenomena to one another, is that he should frame in his mind a hypothesis as to how any one or more of the phenomena, invariably associated with a given death-rate, can operate so as to effect an increase or decrease in that death-rate. This, no doubt, will require a large knowledge of the surrounding conditions not usually to be acquired with ease, and an analysis of the antecedent phenomena in question, often of a prolonged and laborious character. Sometimes, however, such a hypothesis will present itself very readily and with much antecedent probability. However attained, the hypothesis will remain merely a guess until it is tested. It can be tested either by experiment or by observation of appropriate natural instances. By repeated testing, involving often great ingenuity and prolonged labour, the hypothesis is either confirmed or discarded; possibly a new hypothesis is adopted, so to speak, *en route*, and established as in all probability true. When—and not until—this process has been gone through, the naturalist will be, more or less according to the extent of his work, in a position to *place* the phenomena in their true relation to one another and to the ultimate phenomenon proposed for investigation, viz. death-rate. If the study of the antecedent or associated phenomena by means of hypothesis and test-experiment has not been and cannot be carried out, the naturalist can not (it seems to me) reasonably either "pick out" one of them and assert that it is the cause of increased or decreased death-rate, nor (still less) declare that all the antecedent or associated phenomena are the causes and produce the effect of increased or decreased death-rate. If he does do so, he appears to me to be evading the task before him, which is to "explain," that is, to place in their true order and relation a complex group of related phenomena.

The appeal to analogy no doubt frequently leads naturalists to rapid conclusions as to the causal relations of the phenomena of organisms. Often (but by no means always) such conclusions are erroneous: but it is not on that account desirable to reject the argument from analogy in reasoning about such matters. It must be used with knowledge and with caution. It seems to me, that in considering a complex case in which actual experiment is as yet wanting, it is often more useful to formulate provisional conclusions as to what is cause and what only concomitant effect by the aid of argument from analogy, than to deliberately reject all attempts at analysis, and to "lump" all the constantly associated phenomena as "causes." Surely in the case of Prof. Weldon's crabs, most naturalists would take the view that the frontal measurements may *possibly* be operative in saving the life of the crab, or *may* be only a correlative of some other life-preserving structure; that its quality in this respect should be inquired into by means of hypothesis and experiment; and that, until this is done, it is *premature* to speak of a particular frontal proportion as having for its *effect* the survival of those crabs distinguished by its possession.

The chief task of the student of living things seems to me to lie in the search for such explanations, even though the task is in some cases to all appearance at first sight hopeless, and even though too hopeful and imaginative spirits may be led, not unfrequently, to propound explanations which are insufficiently supported by observed facts, or are demolished by the observation of other facts. You can not (it seems to me) reduce natural history, as Prof. Weldon proposes, to an unimaginative statistical form, without either ignoring or abandoning its most interesting problems, and at the same time refusing to employ the universal method by which mankind has gained new knowledge of the phenomena of nature—that, namely, of imaginative hypothesis and consequent experiment. I think that most naturalists will agree with Johannes Müller that "Die Phantasie ist ein unentbehrliches Gut."

E. RAY LANKESTER.

Dinard, Bretagne, August 10.

Habits and Distribution of Galeodes.

WILL you be good enough to publish in your widely-read paper the following notes on the geographical distribution of *Solfuga* (*Galeodes*) *araneoides*, as the published accounts of that arachnid are incomplete in that respect.

In India it is found in the central parts of India, in the Punjab, in Afghanistan, in Baluchistan. Probably it will be found in Rajputana and Scinde, and perhaps in some parts of the Dekhan. I have never found it in the alluvial plains of Northern India, nor in Bengal. I have just found it in the Eastern Sudan. Probably it exists in most of the deserts of Asia and Africa. The ones I have found all seem to belong to one species. They are of a light straw colour, often with some black above. A black variety is found on the Afghan frontier. The biggest specimen measured $1\frac{3}{4}$ inches from the tips of the jaws to the end of the abdomen. They feed on soft insects like moths, and live in holes on the ground. They are very rapid in their movements, and are difficult to catch when on the move. The best way to get them is to put a glass over them when they are lying still in a corner. A big match-box often acts as a good trap. They will run inside it of their own accord. They go up walls, posts, trees, in search of insects, and will jump down from a fair height.

The movements of their jaws when feeding presents a unique sight. The head is small, and the bases of the jaws are bulbous and look like a continuation of the head. When eating they move their jaws alternately, and one gets the impression that the head is jointed and that each side moves alternately. In encounters with spiders, scorpions and centipedes, they usually fare badly. Their belly is so soft, that once caught there, they are done for. Still they are very combative.

I have often tried to settle the point as to whether their bite is painful or poisonous. But I never could succeed in making one bite a human being. E. CRETIN.

Suakin Force, July 12.

[THE genus Galeodes, represented by several species, is known to extend in Africa from Algeria to Egypt, thence southwards into Somaliland ; in Europe, from Greece throughout the steppes of South Russia ; in Asia, over the whole of Asia Minor, Arabia, Persia, Baluchistan, Afghanistan and Turkestan, and thence into India, where it has been recorded from the following localities : Punjab (Kohat), Rajputana (Bikanir), Gwalior, Delhi, Secunderabad, Guntakul near Bellary, Birhbum, Bengal and Madras.—R. I. POCOCK.]

Nest-building Amphipod in the Broads.

IT will be remembered that in 1891, *Cordylophora lacustris* was found in great abundance in Heigham Sound. On the 6th inst,, after collecting in that locality and downward to Potter Heigham Bridge, I noticed at the bottom of the bottle a Crustacean, apparently a Podocerid. On floating a piece of reed stem, covered with colonies of the hydrozoon, in a vessel of water, it was evident that the colonies were thickly studded with nests, from which, in some cases, the antennæ were seen protruding. Several specimens of these Amphipods were secured at once ; and these the Rev. T. R. R. Stebbing, F.R.S., has kindly identified. They prove to be *Corophium crassicorne*, Bruzelius. A large colony of the Cordylophora has been preserved in formalin, with the nests. On the following day I met with the same Amphipod, in considerable numbers, between Acle Bridge and the Angel Inn. HENRY SCHERREN.

9 Cavendish Road, Harringay, N.

The Effects of a Strong Magnetic Field upon Electric Discharges in Vacuo.

SOME interesting experiments upon the subject of this note were described in NATURE, July 19. A small addition to those experiments serves to show the connection between the electric conductivity of the tube and the mean free-path-length of the included molecules.

Using the same form of Crookes' tube as employed by Mr. Swinton, but provided with the platinum Maltese cross to intercept the kathode rays, the shadow of the cross is rotated upon its centre through an angle depending upon the strength of the magnetic field. The motions therefore of the molecules are changed from right lines to spiral lines, and thus their free paths are lengthened—a result equivalent to a further exhaustion of the tube. The primary effect of the magnetic field, observed by Mr. Swinton, was an increase of conductivity in the tube. The two experiments therefore show that the conductivity in the tube increases with lengthening of the free paths.

By lowering the tube slowly from a few inches above the pole of the electro-magnet down to contact, the shadow of the cross is seen to rotate slowly, and to become smaller as the luminescent cone contracts to the form shown by Mr. Swinton, with the apex on the bulb.

The rotation of the shadow is reversed by reversing the poles of the magnet ; and when the current is reversed in the tube, the green fluorescence appears as a spiral band round the walls of the tube—a right-handed or left-handed helix, according to the polarity of the magnet. WALTER SIDGREAVES.

Stonyhurst College Laboratory.

THE LIVERPOOL MEETING OF THE BRITISH ASSOCIATION.

THE detailed local arrangements are now progressing rapidly. St. George's Hall, where the reception room and offices will be situated, has just been re-painted and decorated by the Corporation at great cost. The old wooden flooring has been removed so as to expose the beautiful tiled pavement, which has not been seen for many years, and which will not be covered up again until after the meeting. The buildings and rooms allotted to the various Sections were mentioned in the former article. The sectional fittings have now been planned out, and will be commenced at once. Separate electric and oxy-hydrogen lanterns will be provided for all the Sections that desire them. A large lecture theatre, holding over six hundred people, at University College, will be available for joint discussions between the Sections.

The details of most of the excursions have now been arranged, and a special "Excursions Guide," in addition to the "Handbook," has been prepared. Amongst the excursions, that to the Isle of Man at the conclusion of the meeting will probably take a foremost place, both on account of its general attractiveness and of its special scientific interest. This excursion will extend over five days—from Thursday, September 24, to Monday, September 28, inclusive ; and the party will break up into four Sections : (1) Archæologists, (2) Geologists, (3) Zoologists, and (4) Botanists, to be conducted by competent leaders over those parts of the island which offer special attractions for scientific study. The geology of the island is varied and interesting, especially as regards the dynamic alteration of the older Palæozoic rocks, the volcanic series and the richly fossiliferous limestones of Carboniferous age, and the wide developments of the glacial deposits; the Prehistoric, Scandinavian, and other early remains are celebrated ; the marine fauna and flora are abundant, and the presence of the Liverpool Marine Biological Station at Port Erin affords facilities for dredging expeditions and other biological work.

The detailed programme for the four Sections, which follows, has been arranged by a Committee of the Isle of Man Natural History and Antiquarian Society, acting along with representatives appointed by the Liverpool Executive Committee ; and a special appendix to the Liverpool "Handbook," containing an account of the geology, antiquities, and natural history of the island, illustrated by a geological map and a chart, has been drawn up by Mr. P. M. C. Kermode, Mr. G. W. Lamplugh, and Prof. Herdman.

SECTION I.—ARCHÆOLOGISTS.

Leaders : Arthur J. Evans, P. M. C. Kermode.

Thursday, 24.—Arrive by steamer from Liverpool about 3 p.m. Reception by his Excellency the Right Hon. Lord Henniker, Lieut.-Governor, at Government House. Headquarters at Douglas.

Friday, 25.—Carriages at 9.30 a.m. for Braddan (see ancient crosses and alignments), St. Trinian's, then Tynwald Hill (see mound, cist, Runic cross, &c.). Lunch at Creg Malin Hotel, Peel, at 1 p.m. See Peel Castle, round tower, cathedral, and crosses. Drive to Crosby, Ellerslie, and Ballingan Chapel. See Glen Darragh Circle, and then back to Douglas.

Saturday, 26.—Train at 9.30 a.m. to Castletown; arrive Castle Rushen 10.30. See Castle collection of antiquities, &c. Train 12.22 for Port Erin. Lunch 1 p.m. Visit Liverpool Marine Biological Station. Walk to Neolithic Circle on Meayll Hill, then to Cregneish (see chasms), then to Port St. Mary (see Oghams and standing stones). Train to Douglas.

Monday, 28.—Train at 9.35 a.m. to Ramsey. Stop at Sulby Glen at 10. 42. Climb Cronk Sumark (see ancient fort). Train to Ramsey, arriving at 1.10 p.m. After lunch visit Masonic Rooms (see casts of crosses, flint implements, &c.) Carriages to Laxey (see King Orry's grave). Electric railway to Baldrine (see ancient fort and "cloven stones"), Keeil Killane (lintel graves), then on to Douglas. The four Sections will dine together at the Sefton Hotel, Douglas, on Monday evening.

SECTION II.—GEOLOGISTS.

Leaders: Prof. W. Boyd Dawkins, G. W. Lamplugh.

Thursday, 24.—Reception at Government House, &c., as before. Headquarters at Douglas.

Friday, 25.—Train at 9 30 to Castletown, walk to Castle Rushden, and then on to Stack of Scarlet, and thence to Poyllvaaish (see carboniferous limestones and contemporaneous volcanic series). Meet carriages at Poyllvaaish, lunch at George Hotel, Castletown, and drive to Langness (see base of carboniferous rocks and Skiddaw slates), and then on to railway station at Ballasalla. Train to Douglas.

Saturday, 26.—Electric Railway at 9.30 a.m. to Laxey, and on to Snaefell. (General view of island, and metamorphism of Skiddaw slates). Meet carriages near the Hut, and drive to Tholt-y-Will. Lunch 1 p.m., drive down the Glen, stopping at various points (see crush-conglomerates of Skiddaw rocks, &c.) on the way to Ramsey. Steamer at 6 p.m. back to Douglas.

Monday, 28.—Carriages at 9.30 a.m. for Crosby, Rockmount (see intrusive dykes in Skiddaws), Lhoob-y-Reeast, Peel (see red sandstones, &c.). After lunch see Peel Castle, &c. Drive to Foxdale (see lead mines and granite outcrop), and then on to Douglas. Final dinner with the other Sections.

SECTION III.—ZOOLOGISTS.

Leaders: Prof. W. A. Herdman, I. C. Thompson.

Thursday, 24.—Reception at Government House, &c., as before. Train at 5.10 p.m. for Port Erin.

Friday, 25.—If the weather is suitable, the day will be spent in dredging, &c., from a steamer, probably to the west of the Isle of Man. If dredging is impossible there is shore collecting, tow-netting in the bay, and work in the Biological Station to fall back upon.

Saturday, 26.—Train at 10.40 to Castletown (see Castle Rushen). Return to Port Erin. Lunch at Bellevue Hotel. Take Section I. over Biological Station. Walk with Sections I. and IV. to Neolithic Circle on Meayll Hill. See Cregneish, chasms, &c., and return to Port Erin.

Monday, 28.—If weather is suitable take steamer to Ramsey, dredging on the way along the east side of Island. Lunch at Ramsey, 1 p.m. (If time permits, join Section I. in seeing collection at Masonic Rooms.) Dredge from steamer on way back to Douglas. Final dinner, and stay night at Douglas.

SECTION IV.—BOTANISTS.

Leaders: Prof. F. E. Weiss, Rev. S. A. P. Kermode.

Thursday, 24.—Reception at Government House, &c., as before. See Mr. Okell's garden and collection of Veronicas. Train at 5.10 (with III.) to Port Erin.

Friday, 25.—Carriages 9.30; drive by "Round Table" to Peel over the mountains. Lunch (with I.); see Castle, &c. Carriages to Foxdale, Malew, and back to Port Erin.

Saturday, 26.—Train (or walk, by shore) to Castletown. See Castle Rushen (with I. and III.). Train to Port Erin. Lunch (with I. and III.). Visit Biological Station and Port Erin Shore. Walk with I. and III. to Neolithic Circle on Meayll Hill, on to chasms, and back to Port Erin. There is good shore-collecting at Port Erin, at Port St. Mary, and at various intermediate points.

Monday, 28.—Train to Douglas; carriages to Laxey. Electric railway to Snaefell. Meet carriages near Hut; drive to Tholt-y-Will. Lunch 1 p.m. Drive down Glen and through Curraghs (marsh plants) to Ballamooar, Jurby (gardens, conifers, &c.), back through Curraghs to Ballaugh railway station. Train to Douglas. Final dinner, and stay night at Douglas.

Some changes are taking place in the list of foreign guests. A few of those who had accepted, find themselves unable to be present; but others who were not expected, or were doubtful, are now coming, including some foreign Professors of medicine, surgery, and allied medical subjects—no doubt out of compliment to the President-elect. The local medical men are organising various arrangements in honour of Sir Joseph Lister.

The Local Secretaries hope to secure Dr. Nansen's presence at the meeting. Before he sailed in the *Fram*, Nansen promised a Liverpool shipowner that he would visit him immediately on his return. He has now been reminded of that promise by telegram to Vardö.

It is becoming possible now to forecast to a considerable extent the scientific work which will be brought before this meeting of the Association, and in a further article next week we shall give a sketch of what will probably be the leading features of the various Sections.

W. A. HERDMAN.

COUNTY COUNCILS AND AGRICULTURE.

THE allocation of public money to County Councils under the Local Taxation (Customs and Excise) Act of 1890 has now been in progress for half-a-dozen years. It was understood, though not expressly stipulated, that this money—the proceeds of an additional tax placed upon beer and spirits—should be devoted to the furtherance of technical education, and in the case of most County Councils it is to this object that the money has been applied. With reference to the permanency of the grant, the Duke of Devonshire (then Lord Hartington), addressing a meeting on December 5, 1890, said: "The best way of securing the fund will be to see that it is used for the purpose for which it was originally granted." And on the previous day, in the House of Commons, Mr. Goschen, at that time Chancellor of the Exchequer, said, in reply to a question: "If County Councils set themselves heartily to work to utilise the grants for important educational purposes, it will probably be difficult for any Minister to persuade Parliament to divert them." Excepting in a few cases where some or all of the grant has been applied to the relief of local rates, the County Councils appear to have loyally adhered to the understanding in accordance with which the money was originally directed into the new channel. The particular form of "important educational purposes" to which aid has been extended has necessarily varied somewhat in different localities, but, on account of the universal pursuit of the industry, agriculture perhaps has received more wide-spread recognition than any other art. The result has been the establishment within recent years of new agricultural colleges and schools, or the grafting of an agricultural department upon educational institutions already in existence. To a third group may be assigned various organisations which were in operation before the days of County Councils, but to which these latter have felt it right to hold out a helping hand. To what extent these different bodies are carrying out the objects for which they were instituted, is a fair subject of inquiry.

In most cases evidence is forthcoming of two main lines of activity, which, though independent of each other, are nevertheless related. These are on the one hand the instruction of students, and on the other the prosecution of investigations which should prove of interest not only to students but to all who are engaged in agricultural pursuits. Under the first head there is not much room for novelty. The model which was set up when, more than fifty years ago, a small group of far-seeing men—the Prince Consort amongst them—unfurled upon the Cotswold Hills the banner of agricultural education, is the one that, consciously or unconsciously, has always been followed. In all attempts that have since been made to formulate a fairly comprehensive scheme of agricultural tuition, the germs of every system will be found in the curriculum of the Royal Agricultural College, Cirencester. Nevertheless, this curriculum, far from being stereotyped or crystallised, is and always has been susceptible of such modifications as are called for by the exigencies of the times, as was exemplified by the manner in which the dairying industry, at the time of its renaissance, received prompt and adequate recognition. Its permanence, indeed, is due to its elasticity. Many hundreds, perhaps thousands, of students are now receiving in agriculture a good type of technical instruction which a score of years ago could be obtained nowhere else in this country than at Cirencester.

It is to that section of their work in which institutions fostered by County Councils are brought more immediately under the notice of the general public that criticism may be usefully directed. Most agricultural colleges and schools, and probably the agricultural departments of all university colleges which possess them, are engaged in pursuits which may or may not deserve to be dignified by the name of research. In the majority of cases, however, the work is nothing else than demonstration, and it usually takes the form of differential manuring experiments upon various crops in the field. Periodically, reports are published embodying the results. These are noticed in our columns, but we are not often able to point to any work that rises above the level of demonstration, of the same type as the example fields and example crops that are conducted under Government auspices in France. In most instances the results can approximately be stated beforehand. If it is necessary to demonstrate in a number of localities the effects of nitrogen according as it is applied to a crop in the form of nitrate of soda or of sulphate of ammonia, or to show the different effects of basic slag, bones, and superphosphate of lime as sources of phosphorus—to cite these as simple illustrations—then, no doubt, these many-duplicated field experiments serve their purpose. Nevertheless, they do not alter the fact that the best experimental farm —the one that is capable of teaching the most useful lessons—is a farmer's own occupation, for in this case the conditions are known to him with, perhaps, a minuteness of detail that cannot be approached in connection with field experiments in which he is hardly likely to take more than a sort of academical interest. What have the County Councils, through the medium of the institutions to whose support they contribute, yet done towards teaching the farmer to read aright the lessons which he may learn all the year round in his own fields, and the capacity to make correct inferences from which would be invaluable to him?

It is noteworthy that, with hardly an exception, the work of these institutions is restricted to crops and cropping. The fascinating problems associated with animal nutrition have mostly failed to attract them. Perhaps these are considered too difficult, possibly they may be thought too costly. In one or two cases the domain of bacteriology has been invaded, particularly in connection with dairying. A good illustration of the general character of the work undertaken is provided in the current report of the Board of Agriculture on the distribution of grants for agricultural education. In the financial year 1894-95 the Board distributed the sum of £7400 amongst seventeen institutions. It is not very obvious why these institutions and none others were selected, but it is a fact that all, or most of them, are also in receipt of County Council grants. It is stated that, in at least twenty counties of England and Wales, "demonstrations by experimental work in field plots are now undertaken," and *résumés* are given of the work recently done at the institutions which have received grants from the Board.

Altogether it would seem that, whilst the institutions under notice are undoubtedly useful as instruments of agricultural education, their value in other directions might be increased were their labour less diffuse. The boast that a given centre has more fields of demonstration scattered over a larger number of counties, and that its officials have travelled a greater aggregate of miles in the discharge of their duties, than in the case of any other centre, may be gratifying to local pride, but it is not a high object to aim at. There may possibly exist an ambition to make a centre a second Rothamsted, but it must be remembered that it is the "continued effort along a given line," associated with "the limited number of lines undertaken, although the work extends over fifty years," that has secured for Rothamsted its unique reputation. The warning has already gone forth officially to the United States agricultural experiment stations, that concentration of energy upon a few specific objects of investigation is preferable to the diffuse expenditure of force which has hitherto characterised many of the stations. There is no coordinated effort amongst our own institutions; each goes its own way, independent of, and practically ignoring, the others—unless, perchance, there be rivalry. A connecting link, possibly a controlling influence, is needed. Youth is on their side, and they have furnished many proofs that they are not lacking in energy. Quality rather than quantity, however, is the goal at which they should aim in the future conduct of their work.

THE ECLIPSE OF THE SUN.

THE bad news which we published last week regarding the almost general failure of the eclipse observations is tempered by the telegrams which have since been received regarding the weather in Novaya Zemlya and in Siberia.

A telegram from Hammerfest reports success at the former station, though details are yet lacking. As this expedition was organised at the last moment, very little has been said about the instruments to be employed. It may be stated, therefore, that Sir G. Baden-Powell took with him Dr. Stone, of the Radcliffe Observatory, who proposed to make spectroscopic observations, and Mr. Shackleton, one of the computers employed at the Solar Physics Observatory, South Kensington, who observed the eclipse of 1893 in Brazil. Mr. Shackleton was provided by Mr. Norman Lockyer with a powerful prismatic camera with two 3-inch prisms of 60°, and careful testings gave great hopes of its performance.

It was, therefore, to be employed chiefly in investigating the special spectrum of the corona found on the photographs of 1893. As a subsidiary instrument, a telescope of four inches aperture and somewhat long focal length was also arranged to photograph the corona. Both instruments were to be fed with light by a Foucault siderostat.

It is hardly necessary to remark that these instruments are capable of furnishing results of the highest value to solar physicists. We therefore note with satisfaction the still later Reuter telegram from Hammerfest, dated August 17:

Sir George Baden-Powell's yacht *Otaria* has arrived here all well with the members of the British Eclipse Expedition. The party made excellent and valuable observations of the eclipse in Novaya Zemlya. The corona and spectrum were clearly visible, and very satisfactory photographs were taken.

The following telegrams from Russia inform us that one of the parties from Pulkowa, including the excellent spectroscopist Belopolsky, has also been successful.

St. Petersburg, August 15.

According to a telegram from Tiumen, in Western Siberia, the solar eclipse was very successfully observed at that place, and one particularly good photograph was taken. Some stars even are visible.

August 16.

A despatch from Khabarovka, which is the residence of the Governor-General of the maritime territory in the extreme east, states that the astronomical observations of the eclipse taken in the village of Orlovski, on the river Amur, were thoroughly successful. The weather was fine during the eclipse. The astronomers, MM. Belopolsky, Vitram, and Orbinsky, have returned to Khabarovka.

August 18.

A telegram from Khabarovka gives further details of the astronomical observations of the solar eclipse taken on the Amur. The sky at the time was overcast, but during totality the corona and several stars of the first magnitude were distinctly visible through the telescope. The darkness was not complete. Six photographs were taken illustrating the different phases of the eclipse.

We referred last week to the partial success on the west coast of Norway. Mr. John Dover has communicated a letter to Tuesday's *Times*, from which we make the following extract:—

It was thought that the best view would be obtained from a village, Brevik, about twenty miles south-east of Bodö. Leaving Bodö on Saturday evening by steamboat, we passed through the "Saltström" at low tide, and waited near to watch [the waters rushing into the fiord as the tide rose. We then proceeded to the village of Brevik, where we landed at two o'clock on Sunday morning. A climb of about twenty minutes brought us to a suitable elevation above the fiord of about 250 feet from whence there was an uninterrupted view to the north-east and east for some miles. Only one small cloud was visible, and that in the west; otherwise the sky was quite clear. There was a perfect sunrise at 3.14 on Sunday morning. The partial eclipse began at 4.1 a.m. The total eclipse began at 4.54, and lasted for 92 secs. At Vadsö the totality would have been 105 secs. The partial eclipse ceased at 5.51. At 4° 54′ 45″, the middle of the eclipse, the sun being completely hidden, the corona around it assumed a distinct form. The corona to north-east was about the length of the sun's diameter and very distinct. On the western edge the corona was about two-thirds the length of the sun's diameter, while to the south-east it was about half a diameter. To the north the corona was very slight indeed, being about one-tenth of a diameter. On the south-western edge of the sun appeared a large red spot which was visible until the totality of the eclipse had quite ceased. A Dutch professor near me observed two small spots on the eastern side, but these escaped my notice. I glanced away from my telescope for a moment to see if any stars were visible, and observed Jupiter and Venus. Mercury and Regulus were also seen by others present. The colour of the moon in front of the sun seemed of a dull grey, while the corona around the sun was of a light cream colour. The sky to north and east appeared of a pale orange colour, while to the west it was of light yellow shade. Two photographers—one from Flensborg, Schleswig, the other an amateur from Oxford—were at work, so that I hope a good photograph of the eclipse may be produced. My great regret was that I did not see any one present with thoroughly good scientific instruments.

We shall publish next week a letter received from Mr. Norman Lockyer, containing an account of the preparations he made to observe the eclipse on the Island of Kiö, assisted by the officers and men of H.M.S. *Volage*. Sir Robert Ball contributes to Wednesday's *Times* a long letter on the observing parties and stations at Vadsö and in the neighbourhood, and Dr. Rambaut makes a similar contribution to the *Daily Chronicle* of the same day. We must also mention that the correspondent of the *Daily Telegraph* gives some interesting notes on the characteristics of the shadow.

NOTES.

THE current number of the *Comptes rendus* of the Paris Academy of Sciences contains a statement, by M. Berthelot, with reference to the present condition of the scheme to erect a monument to Lavoisier by international subscription. A Committee to take this matter in hand was formed, in 1894, of members of the Institute of France, and representatives of the French Government, of the Municipal Council of Paris, and of various scientific bodies. A special Committee was nominated to obtain subscriptions, and the result of their appeal has now been made known by the publication of a list of subscribers in a fasciculus issued with the *Comptes rendus*. The amount already received is 47,553 francs (nearly £2000), and subscriptions are still coming in. The Emperor of Russia has authorised the opening of a subscription list in Russia, and has headed the list with a sum of two thousand roubles (£313). The French Minister of Public Instruction will give six thousand francs (£240), and the City of Paris £400. Alsace has contributed 2475 francs (nearly £100) to the fund, Germany about £160, England £130, Austria-Hungary £100, Belgium £24, United States £20, Greece £7, Italy £40, Mexico £4, Netherlands £40, Portugal £24, Roumania £14, Servia £40, Norway and Sweden £80, Switzerland £34. The construction of the monument has been entrusted to M. Barrais.

A STRONG and representative Committee is being formed in connection with the proposal to inaugurate a memorial to commemorate the inception and extension of submarine telegraphy. Amongst many other influential persons who have agreed to act are:—Viscount Peel, Lord Kelvin, Lord Selborne, the Lord Chief Justice, Mr. Joseph Chamberlain, Sir Richard Webster, Lord George Hamilton, Sir Juland Danvers, Sir Samuel Canning, Sir Eyre Shaw, Sir John Robinson, Admiral Sir Henry J. C. D. Hay, Admiral Sir Anthony Hoskins, Mr. Hubert Herkomer, Mr. Herbert de Reuter, Mr. J. C. Lamb, Mr. W. H. Preece, Dr. John Hopkinson (President of the Institution of Electrical Engineers), Dr. Alexander Muirhead, Mr. Alexander Siemens, and Mr. W. S. Silver. An executive will no doubt be formed from the larger Committee, and the *Electrician* makes a suggestion for their consideration. The introduction and extension of telegraphy are almost exactly coincidental with Her Majesty's reign—the magnetic needle telegraph having been patented by Cooke and Wheatstone on June 12, 1837, and the first telegraph line from Paddington to West Drayton being constructed in 1838–9. The suggestion of our contemporary is that the memorial in question should be inaugurated at the time of the celebration of the sixtieth anniversary of the Queen's reign, on or about June 20 next.

THE fourth meeting of the International Congress of Hydrology, Climatology, and Geology will be held at Clermont-Ferrand, Puy de Dôme, from September 28 to October 4.

IT is announced in *Science* that, if certain conditions are fulfilled by the City of Chicago, the Field Columbian Museum is to receive two million dollars as an endowment fund from Marshall Field, the founder of the Institute.

THE death is announced of Dr. H. E. Beyrich, Professor of Geology and Palæontology, at Berlin.

WE are sorry to see the announcement of the death of Prof. H. A. Newton, of Yale College, whose work in pure mathematics and mathematical astronomy is well known to men of science.

THE list of new Fellows elected by the Reale Accademia dei Lincei of Rome, with their special subjects of study, is as follows:—Ordinary Fellow, Giovanni Briosi (Botany); Corresponding Fellow, Giacinto Morera (Mechanics); Foreign Fellows, Carl Neumann and Hugo Gyldén (Mechanics), Ludwig Boltzmann and Alfred Cornu (Physics).

THE *British Medical Journal* states that the members of the Liverpool Medical Institution have invited Sir Joseph Lister, President-Elect of the British Association, to a banquet to be given in his honour on September 19. The banquet will take place in the Philharmonic Hall, in which the dinner of the British Medical Association was held on the occasion of the meeting in Liverpool in 1882. The Lord Mayor of Liverpool (the Right Hon. the Earl of Derby) will also be the guest of the Institution, and upwards of one hundred of the more distinguished members of the British Association have been invited to be present. The Blue Hungarian Band has been engaged for the evening, and no effort is being spared by the Dinner Committee to make the festival worthy of the guests, the British Association, and the Medical Institution.

As announced on July 23, the autumn meeting of the Iron and Steel Institute will commence at Bilbao, on Monday, August 31. The detailed programme has now been distributed, from which it appears that the following papers have been offered for reading:—On the Spanish iron industry, by Don Pablo de Alzola; on the estimation of sulphur in iron ores, by R. W. Atkinson and A. J. Atkinson; on the present position of the iron ore industries of Biscay and Santander, by William Gill; on a new water-cooled hot-blast valve, by William Colquhoun; on the manganese ore deposits of Northern Spain, by Jeremiah Head; on the missing carbon in steel, by T. W. Hogg; a note on the presence of fixed nitrogen in steel, by F. W. Harbord and T. Twynam; further notes on the Walrand process, by G. J. Snelus, F.R.S.; on the roasting of iron ores with a view to their magnetic concentration, by Prof. H. Wedding.

WE regret to notice the report that Herr Lilienthal, whose experiments in artificial flight have on several occasions been described in these columns, has been accidentally killed. According to a Central News telegram, he made an experimental journey on August 11, starting from Gömberg, in the province of Brandenburg. He had flown along safely for over two hundred yards, when a gust of wind suddenly caught and carried him upwards, causing him to lose control over his wings, with the result that he fell to the ground, broke his spine, and died soon afterwards. Herr Lilienthal was reported some time ago to have met with a fatal accident, but happily the news then proved to be incorrect. There does not, however, seem to be any grounds for doubting that he has now actually met his death while carrying out one of his intrepid experiments which have been of such assistance in developing the knowledge of the conditions of flight. It is worth remark that Mr. S. E. Peal, in a letter which we published on August 6, prophesied the probability of the occurrence of an accident such as that which has just ended fatally. He said: "Herr Lilienthal is probably on the right trail. I see he desires to turn and meet the breeze; but in this movement, I fancy the upper central aëroplane—so high above the centre of gravity—will turn him over in a strong wind." Unfortunately the suggested accident has happened, and has deprived science of an enthusiastic experimenter in aërial navigation.

A CORRESPONDENT at Johannesburg has sent us a report, from the Johannesburg *Star*, of some amusing speeches recently made in the Volksraad on the subject of rain-making experiments. It would be a pity to let the richness of the utterances of the members of the Raad be the cause of merely local merriment, so we subjoin the report, trusting that sacrilegious meteorologists will give it their consideration. The report is as follows: "The debate on the memorials from Krugersdorp, requesting the Raad to pass an Act to prevent charges of dynamite being fired into the clouds for rain, was continued. Mr. A. D. Wolmarans spoke in favour of his proposal, and denounced the action of certain persons at Johannesburg as invoking the wrath of God. Mr. Birkenstock said there was nothing irreligious or sacrilegious in these experiments; they were purely scientific experiments. When lightning-conductors were first invented, the same objections were raised against their use. This was not a subject for the Raad to deal with, and he moved as an amendment that the Report of the Memorial Committee to decline to interfere be adopted. The Chairman said it was a monstrous thing to shoot into the clouds; it was nothing less than defiance of the Almighty; it should be made a criminal offence. Mr. Labuschagne was of the opinion that the offenders should be imprisoned. After a further discussion it was resolved, by fifteen to ten votes, to instruct the Government to draft a law to prevent such things happening in future, and submit it this session."

THE Committee of the British Association on Zoological Bibliography and Publication desire to draw attention to the following statement:—It is the general opinion of scientific workers, with which the Committee cordially agrees, (1) that each part of a serial publication should have the date of actual publication, as near as may be, printed on the wrapper, and, when possible, on the last sheet sent to press. (2) That authors' separate copies should be issued with the original pagination and plate-numbers clearly indicated on each page and plate, and with a reference to the original place of publication. (3) That authors' separate copies should not be distributed privately before the paper has been published in the regular manner. The Committee, however, observes that these customs are by no means universal, and asks that they shall be more generally put into force. The Committee further asks for co-operation in the following matter. There are certain rules of conduct upon which the best workers are agreed, but which it is impossible to enforce, and to which it is difficult to convert the mass of writers. These are: (4) That it is desirable to express the subject of one's paper in its title, while keeping the title as concise as possible. (5) That new species should be properly diagnosed and figured when possible. (6) That new names should not be proposed in irrelevant foot-notes, or anonymous paragraphs. (7) That references to previous publications should be made fully and correctly, if possible in accordance with one of the recognised sets of rules for quotation, such as that recently adopted by the French Zoological Society. The Committee points out that these and similar matters are wholly within the control of editors (*rédaction*) and publishing committees, and any assistance in putting them into effect will be valued, not merely by the Committee, but, it is believed, by zoologists in general. Any remarks on the above matters may be addressed to Mr. F. A. Bather, Secretary of the Committee, at the Natural History Museum, Cromwell Road, London, S.W.

AN account of experiments, conducted by G. W. and E. G. Peckham, for testing (1) the range of vision and (2) the colour-sense of spiders, published in a late volume of the *Transactions* of the Wisconsin Academy, is given in the *American Naturalist*. The evidence offered by the authors is based upon a study of twenty species of Attidæ. This study extended over eight successive summers, during which notes were made of many hundreds of observations. The movements and attitudes of the spiders of the group chosen are wonderfully vivid and expressive. The males, in the mating season, throw themselves into one position when they catch sight of a female, and into quite another at the appearance of another male. This power of expression through different attitudes and movements was of great assistance in determining not only its range of sight, but also its power of distinct vision. The results of these experiments are summed up as follows:—"The Attidæ see their prey (which consists of small insects) when it is motionless, at the distance of five inches; they see insects in motion at much greater distances; they see each other distinctly up to at least twelve inches. The observations on blinded spiders, and the numerous instances in which spiders were close together, and yet out of sight of each other, showing that they were unconscious of each other's presence, render any other explanation of their action unsatisfactory. Sight guides them, not smell."

WE learn from *Science* that the U.S. Weather Bureau has issued what it calls a "souvenir" map of the St. Louis tornado of May 27. On one side there is a map showing the weather conditions over the United States on the evening of that day, with the tornado districts indicated by red crosses, and with a brief descriptive text beneath. On the reverse side is an explanation of the wind, weather, and temperature signals of the Bureau.

THE Pilot Chart of the North Atlantic Ocean for the month of August shows the probable routes followed by eighty-two bottle-papers thrown from vessels and returned to the U.S. Hydrographic Office between December 1, 1895, and June 1, 1896. Some of the individual drifts are very noteworthy, but the general course of the bottles clearly illustrates the two main features of the general surface circulation of the waters of the North Atlantic: (1) a vast but gentle eddy extending from the equator to the parallel of 48° N., and completely enclosing that portion of the ocean lying between the trades and the anti-trades, in which the currents are feeble in force and variable in direction; and (2) the so-called extension of the Gulf Stream, which proceeds north-eastward and skirts the shores of Iceland on the one hand, and Scotland and Norway on the other. The principal meteorological feature during July was the large areas of fog reported. Several westward-bound vessels met the fog near 25° W., and, with the exception of short intervals, did not have clear weather again till reaching the American coast.

REFERRING to M. Marmier's paper on the action of currents of high frequency on microbic poisons (noticed in NATURE for July 30), Dr. d'Arsonval writes to the *Société Française de Physique* pointing out, in the first place, that he has succeeded in attenuating poisons when frozen, and, in the second place, that he has destroyed the virulence of the venom of the cobra snake by means of currents of high frequency at a temperature not exceeding 40°, although, according to MM. Phisalix and Bertrand, this venom does not ordinarily lose its virulence till it has been heated to 150° in sealed tubes. Dr. d'Arsonval has been led to the conclusion that the action is not due to any heating effects, but is rather of an electrolytic nature. It is not here a matter of ordinary electrolysis with liberation of chemical constituents in the neighbourhood of the electrodes, but rather a series of alternating decompositions and recombinations following each other in rapid succession from molecule to molecule without giving rise to any free products.

THE goats of Anatolia seem to be remarkable for their susceptibility to a particular form of pneumonia which, although in some respects resembling the pleuro-pneumonia to which calves are addicted, is yet quite distinct in its microbic origin. M. Nicole, formerly assistant at the Pasteur Institute in Paris, and now director of the Imperial Bacteriological Institute in Constantinople, has made a careful study of this disease, in conjunction with one of his assistants, Rêfik-Bey. Constantly associated with the disease the authors have found a bacillococcus which, as its name indicates, is polymorphic in appearance, and might by some be regarded as belonging to a group of micro-organisms known under the collective title of the "bacteria of hæmorrhagic septicæmia," of which the fowl-cholera microbe is usually taken as the type. This group owes its origin to a hypothesis of MM. Nocarde and Leclainche, who regard the various organisms associated with hæmorrhagic septicæmia as mere variations from one parent form, the ovoid bacterium. This hypothesis, although attractive, requires further experimental confirmation before it can be unhesitatingly accepted; and M. Nicole considers that he has shown by his investigations that the organism which he has discovered to be the pathological agent in this pneumonia affection of Anatolian goats, is entirely distinct from that associated with the closely-allied pneumonic disease to which calves are subject.

A GREAT deal has been written about the difficulties surrounding the production of diphtheria toxines of efficient strength in a reasonable space of time. Some investigators have stated that an abundant supply of air is essential in the culture flasks, and specially constructed vessels admitting a current of air have been devised; others, again, assert that meat which has almost commenced to putrify is far more effective than fresh meat. M. Nicole, however, now tells us, in a recent number of the *Annales de l'Institut Pasteur*, that he can procure a powerful toxine in the simplest and briefest possible manner by employing the juice of beef only a few hours after it has been killed, with addition of peptone and salt, the whole being brought to the boiling-point, then filtered, rendered strongly alkaline, and heated for ten minutes at 120° C. After being again filtered, it is distributed in any sort of vessel which may be to hand, and sterilised by being subjected for a quarter of an hour to a temperature of 115° C. The diphtheria microbe is introduced, and the cultures are kept for five days at 37° C., when a toxine of high toxic quality is ready for use. This simple process ought to recommend itself to all who have the preparation of diphtheria toxines to superintend, for M. Nicole tells us that the results he has obtained have never failed, but, on the contrary, have been absolutely constant.

A LETTER from Messrs. P. and F. Sarasin to the Freiherr von Richthofen, announcing their return from an exploration of the south-east arm of Celebes, is published in the *Verhandlungen der Gesellschaft für Erdkunde zu Berlin*. The explorers report the examination of two lakes hitherto almost unknown to Europeans, the Lakes Matanna and Towuti. These lakes lie in an S-shaped depression between two ranges of mountains. Lake Matanna is of great depth, a sounding in the middle giving 480 metres without bottom. Remains of a prehistoric village built on piles, and now submerged, were discovered, many of the bronze and pottery articles found being very similar to those obtained in such villages in Europe. The surface of Lake Matanna is about 400 metres above sea-level, that of Lake Towuti about 350 metres.

PROF. DR. KARL FUTTERER contributes to the *Verhandlungen der Gesellschaft für Erdkunde zu Berlin* an extremely interesting paper containing a detailed comparison of the Ural and Caucasus Mountains. Discussing the similar origin of the two ranges, and the similarity of conditions affecting the Caucasus and the southern Urals, the various progressive stages of erosive action are carefully traced, and the results taken to illustrate a number of important points in the new science of geomorphology, especially in connection with such examples as the Alleghany Mountains, and the extinct range known to geologists as the "Central German Alps," which now forms the Thuringian and Black Forests, and the Harz.

THE Geodynamic Section of the Meteorological Observatory of Constantinople has just completed the publication of its first year's work, and in the last monthly *Bulletin* (for December 1895) the Director, Dr. Agamennone, summarises the results. The area studied is not confined to the Ottoman Empire, but includes all the countries bordering the eastern end of the Mediterranean. Nevertheless, out of 753 shocks recorded in the *Bulletins* (an average of more than two per day), 400 belong to Turkey; while 236 occurred in Greece, and 55 in Bulgaria. Of the Greek earthquakes, more than two-thirds were felt in the island of Zante. From Persia, the Caucasus, and the region beyond the Caspian Sea, the reports are few in number, but this is probably owing to the want of observers. The slight shocks of course greatly predominate, amounting to 519; 225 were strong, or very strong, and nine disastrous. Most of the records are very brief, but more detailed accounts are given of four earthquakes—those of the Caspian Sea on July 9, the Adriatic Sea on August 9, Pergama on November 13-14, and Salonica on December 2. The value of Dr. Agamennone's work will be evident when we compare this ample chronicle with the list of 49 shocks in the Ottoman Empire during the previous year.

THE difficulty of recognising the direction from which a sound proceeds is a well-known obstacle to the full utilisation of acoustic signals at sea. It often happens that a gun signal, or especially a steam whistle, is supposed by one of the watch to be on the port side, while another hears it to starboard. M. E. Hardy, in *La Nature*, describes an apparatus which should prove effective for determining the direction of the sound waves. Two microphones are placed on board at as large a distance apart as possible, say 100 yards. Each of the microphones is connected with a telephone. The observer holds the forward telephone to his right ear, and the stern telephone to his left. Then, when a signal is given by a vessel straight ahead, he will hear it first in his right ear and then in his left, and the interval will be that required by the sound wave to travel from one microphone to the other. In the case supposed, the interval will be about one-third of a second. When the strange vessel is just abeam, the sounds will strike the microphones at the same instant, and the observer will hear them as coincident. When it is just astern, the left ear will be the first to hear the signal. This method, while capable of fixing the angle between the keel of the vessel and the direction of the stranger, does not decide the port-or-starboard question. This might be done by a similar auxiliary apparatus amidships. Another method described by the same author is based upon the interference of sound waves, the sound being received by a tube dividing into two branches whose ends are placed at a distance apart equal to half the length of the sound wave, and are attached to the ends of a bar capable of rotating in a horizontal plane. When this bar points in the direction whence the wave proceeds, and only then, will the sound heard through the tube vanish by interference. But the wave may be proceeding along the bar either forwards or backwards, so that here again we have an ambiguity. But it must be borne in mind that the choice between two exactly opposite directions is comparatively easy. Considering the differences of pitch of the various bells, whistles, and fog-horns in use, we should prefer the first method. But the sound rays rarely proceed in straight lines from the source, and there are many objective difficulties besides the subjective one mentioned.

IN the May number of the *Records* of the Indian N.W. Geological Survey, Dr. W. T. Blanford sums up the results of recent investigations on the ancient geography of Gondwána-land, the great southern continent of which Australia, peninsular India, Southern Africa and South America are the now isolated remnants. He points out how, one by one, each of these great southern land-masses has been found to contain remains of the peculiar Gondwána flora so different from the contemporaneous (Carboniferous and early Mesozoic) floras of the northern hemisphere, and how in each case a peculiar boulder-bed, accompanied by unquestionable evidence of its glacial origin, has been found associated with them. Recent discoveries in South America have completed the chain of resemblance, and show the great extent of the Gondwána continent. At the same time, if the South American boulder-bed be as truly glacial as that of the other areas, any attempt to explain the occurrence of this glaciation within the Tropics by a shifting of the earth's axis must be finally abandoned. It does not necessarily follow that an unbroken continental tract extended at one and the same time from South America through Africa and India to Australia, but the whole region must at least have been mainly land, at a time when the Pacific Ocean was already as important a terrestrial feature as it is now. This continental mass, too, with its peculiar flora, must have been separated from the northern lands, on which the *Lepidodendron* and *Sigillaria* of our coal-measures flourished, by some barrier, probably the Tethyan Ocean of Suess, of which our present Mediterranean and Caribbean seas are the shrunken remnants. Two recently recorded facts of hydrography are mentioned by Dr. Blanford, as throwing an interesting side-light on the difference now existing between the ancient basin of the Pacific and the modern oceans which occupy part of the site of Gondwána-land. The first is the much slower rate at which the Krakatoa wave of 1883 was propagated through the shallower waters of the South Atlantic than through the deep Pacific. The other is the warm temperature of the bottom-waters of the North-west Indian Ocean, which indicates that they (like those of the Mediterranean) are isolated by some barrier from the cold bottom-currents, and such a barrier must run in precisely the direction required for the ancient connection of India and Southern Africa. Thus evidence from all directions converges to indicate the fundamental difference between Atlantic and Pacific Oceans, and in this, bound up as it is with the history of Gondwána-land, it may well be that the key to many a geological puzzle will yet be found.

THE second and concluding part of Mr. C. D. Sherborn's *Index to the Genera and Species of the Foraminifera* (NON—Z) has been published by the Smithsonian Institution. It shows signs of the most scrupulous care in preparation, and should prove a boon to future workers on the Foraminifera.

THE Jubilee of the Chemical Society of London was celebrated in 1891, and it formed the subject of articles in these columns at the time. A record of the proceedings, together with an account of the history and development of the Society, has now been published in a souvenir volume. Translations are given of the addresses sent by foreign societies, and of the speeches made by the foreign delegates. A translation is also given of M. Dumas' Faraday lecture, as well as abstracts of the five other Faraday lectures. The Society boasts of being the first which was formed solely for the study of chemistry, and success has attended it from its foundation. It soon became a centre of the chemical life of this country, and by its publications it has played the chief part in the advancement of chemistry. The volume just published will make Fellows of the Society proud of their Fellowship, and will arouse a spirit of emulation among chemists in many parts of the world.

STUDENTS of meteorology will be glad to know that three important essays on Australian weather have, by the generosity of the Hon. Ralph Abercromby, been brought together and published in book form. The first essay, on "Moving Anti-cyclones in the Southern Hemisphere," by Mr. H. C. Russell, F.R.S., was originally read before the Royal Meteorological Society, and published in the Society's *Journal*. The leading fact brought out in this paper is that Australian weather south of lat. 20° S. is the product of a series of rapidly moving anticyclones, which follow one another with remarkable regularity, and are the great controlling force in determining local weather. These anticyclones travel eastward at the average rate of four hundred miles per day, and they do so with such regularity that the prospect is held out of weather predictions being made some weeks in advance, or even for longer periods. The second essay in the volume is the one, by Mr. H. A. Hunt, on "Southerly Bursters," which won the prize offered by the Hon. Ralph Abercromby. This essay was noted in NATURE in January 1895 (vol. li. p. 230). The third essay, which is also by Mr. Hunt, has for its subject "Types of Australian Weather." This discussion throws much new light upon the source of the greater part of Australian rain, and at the same time forms an important contribution to the study of weather in the southern hemisphere generally. The volume containing these essays is published by Mr. F. W. White, Sydney.

THE additions to the Zoological Society's Gardens during the past week include four Malabar Squirrels (*Sciurus maximus*) from Southern India, presented by Mr. W. J. Stillman; two Sclater's Curassows (*Crax sclateri*) from Minas Geraes (Brazil), presented by Mr. E. Sumead; a Temminck's Stint (*Tringa temmincki*), British, presented by Mr. E. C. Sprawson; a Golden Eagle (*Aquila chrysætus*) from Spain, presented by Mr. F. Leathly Holt; three Common Blue-birds (*Sialia wilsonii*) from North America, presented by Mr. A. T. Binny; two Stone Curlews (*Œdicnemus scolopax*), British, presented by Mr. W. J. Kidman; two Common Blue-birds (*Sialia wilsonii*) from North America, presented by Mr. Percy Cockshut; three Common Adders (*Vipera berus*), British, presented by Mr. A. Old; three Peruvian Snakes (*Tachymenis peruviana*) from Peru, presented by Mr. A. H. Jamrach; a White-browed Amazon (*Chrysotis albifrons*) from Honduras, purchased; a Wapiti Deer (*Cervus canadensis*, ♀), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

LUNAR PHOTOGRAPHS.—Prof. Weinek, whose artistic skill in the enlargement of lunar photographs cannot but be admired, and who has co operated with the staff of the Lick Observatory in reproducing from their negatives the more interesting features of the moon, has recently made a further contribution to the Vienna Academy of fifteen enlargements of certain details on the lunar surface, as seen at the third quarter. Also, Aristarchus and Herodotus have been drawn with the shadows thrown on the western side as a companion picture to an earlier enlargement in which the shadows are thrown to the east. Dr. Weinek now takes the opportunity of calling attention to the fact that, in many instances, his drawings, indicating the existence of minute detail, have been confirmed by Dr. Gaudibert from optical examination of the moon itself. This remark refers to drawings from original negatives taken both at the Lick and Paris Observatories. The differences between the photographic reproductions and Schmidt's map are admitted, and according to the description furnished by Dr. Gaudibert, it is a little difficult to explain some of the omissions from this well-known authority.

DISTRIBUTION OF BINARY-STAR ORBITS.—Miss Everett gives in *Monthly Notices*, June 1896, pp. 462-466, the results of an attempt to discover if the planes of the orbits of binary stars have any relation to the plane of the Milky Way. To do this, the most accurate values of the elements of fifty-five orbits were taken, and from these the galactic longitudes and latitudes of the poles of the orbits were calculated and tabulated. Gould's value of the position of the galactic northern pole was assumed, viz. R.A. 12h. 42m. 4s. (190° 31′), Decl. +27° 16′ (Epoch 1890). This gives the obliquity of the central line of the Galaxy as 62° 7′, and the position of the ascending node is at R.A. = 18h. 42m. (280° 31′), from which the galactic longitudes are reckoned. On examining the tables, it appears that equal surface zones contain nearly similar numbers of orbit poles, and it is concluded that there is no decided tendency on the part of the poles of the orbits to favour any special region of the celestial sphere, and hence that the planes of the orbits cannot be regarded as having any definite relation to the mean plane of the Galaxy.

COMET 1890 VII.—The orbit of this comet, which was first seen by Dr. Spitaler of Vienna, while searching for one recently discovered by M. Zona of Palermo, has been submitted to a thorough examination by the original discoverer, with the result that the period of six and half years has been confirmed. Consequently, its return to perihelion may be looked for next spring, and Dr. Spitaler has prepared ephemerides to facilitate its search. The most favourable time for observation will be next month, when the comet will be in opposition, but the theoretical brilliancy will be only about one-fourth that possessed at the time of discovery in 1890. The considerable southern declination of the comet will render its detection in these latitudes still more difficult. The next return in 1903 will be still more unfavourable, and though 1909 may offer good chances for observation, the error of position will be larger. Dr. Spitaler thinks that the ephemeris he has prepared for this return is trustworthy to about five minutes of R.A. and forty minutes of Declination.

PHOTOGRAPHY OF SOLAR CORONA.—Count de la Baume Pluvinel has recently discussed the conditions necessary for successfully obtaining photographs of the corona (*Bulletin de la Soc. Ast. de France*, July 1896). The difficulty of the problem lies in the varying intensity of the several parts of the corona, the delicate details being lost in long exposures on the inner region, while in short exposures the outer corona is almost absent. During the eclipse of April 1893, the author attempted to determine the best value of the "photographic action" necessary for depicting the coronal structure without allowing the light from the surrounding sky to produce any deteriorating effect. The term "photographic action" is defined as being proportional to the product of the intensity of the image and the duration of exposure, and is accepted as being constant within certain limits. For this purpose he employed a compound camera having nine object-glasses, with apertures varying from 5 mm. to 155 mm., and average focal length of 1·5 metres. The time of exposure for all was 230 secs., and consequently the photographic action had values varying from 0·24 to 250. From the various photographs obtained he concluded that, for that particular climate (Joal) a photographic action of about 4 was best. From other photographs taken in Brazil, he recommends a value of 10 to be used in future eclipses, this value to be diminished or augmented as the sky light is greater or less than that in 1893. The above law of photographic action ceases to hold beyond certain limits; as the intensity of the light decreases, the time of exposure must be enormously increased, and this fact has led the author to suggest a method of photographing the corona without an eclipse. It involves the design of a telescope with such a ratio between aperture and focal length that the sky illumination will be too feeble to affect the plate, while the slightly greater intensity of the corona will allow of its being photographed with a long exposure.

NANSEN'S POLAR EXPEDITION.

DR. NANSEN arrived at Vardö, Norway, on Thursday, August 13, after an absence of three years. A Reuter telegram says that he left the *Fram* with a companion on March 14, 1895, in lat. 84° N., in order to push further north into the Polar Sea than the *Fram* could penetrate. The expedition accomplished its object in traversing the Polar Sea to a point north of the New Siberia islands. The most northerly point attained was lat. 86° 14', which is nearly 200 miles further north than had previously been reached. No land was sighted north of 82°. Dr. Nansen and his companion then went south to Franz Josef Land, where they passed the winter, subsisting on bears' flesh and whale blubber. Here they fell in with the *Windward*, of the Jackson-Harmsworth expedition, which brought them to Vardö. It is expected that the *Fram* will eventually arrive at Spitbergen.

With most commendable enterprise, the *Daily Chronicle* published on Saturday, August 15, Dr. Nansen's own narrative of his expedition, telegraphed from Vardö. The narrative is in the highest degree interesting, as well as a striking testimony to the hardihood and indomitable spirit of Dr. Nansen and Lieut. Johansen, who for seventeen months, cut off from all means of retreat, travelled over nearly 700 miles and carried on polar explorations. The telegram published in the *Daily Chronicle* is abridged below; and we are glad to express our acknowledgments to that newspaper for the opportunity afforded us of placing before the readers of NATURE the salient points in this account of Dr. Nansen's explorations of polar regions.

The *Fram* left Jugor Strait August 4, 1893. We had to force our way through much ice along the Siberian coast. We discovered an island in the Kara Sea, and a great number of islands along the coast to Cape Cheljuskin. In several places we found evidences of a glacial epoch, during which Northern Siberia must have been covered by inland ice to a great extent.

On September 15 we were off the mouth of Olenek River, but thought it too late to go in there to fetch our dogs, as we would not risk losing a year. We passed the New Siberian Islands on September 18.

On September 22 we made fast to a floe in latitude 78° 50' N., and longitude 133° 37' E., and allowed the ship to be closed in by the ice.

As anticipated, we were gradually drifted north and north-westward. The sea was up to ninety fathoms deep south of 19° N., where the depth suddenly increased, and was from 1600 to 1900 fathoms north of that latitude. This will necessarily upset all previous theories based on a shallow Polar Basin. The sea-bottom was remarkably devoid of organic matter. During the whole drift I had good opportunities to take a series of scientific observations—meteorological, magnetic, astronomical, biological soundings, deep-sea temperatures, examinations for salinity of the sea-water, &c. Under the stratum of cold ice-water covering the surface of the Polar Basin, I soon discovered the warmer and more saline water due to the Gulf Stream, with temperatures from 31° to 33°. We saw no land, and no open water, except narrow cracks, in any direction.

As anticipated, our drift north-westward was most rapid during the winter and spring, while northerly winds stopped or drifted us backwards during the summer. On June 18, 1894, we were on 81° 52' N. lat., but drifted then southward only. On October 21 we passed 82°. On Christmas Eve, 1894, latitude 83° N. was reached, and a few days later 83° 24', the farthest north latitude previously reached by man.

As I anticipated that the *Fram* would soon reach her highest latitude to the north of Franz Josef Land, and that she could not easily fail to carry out the programme of the expedition, viz. to cross the unknown Polar Basin, I decided to leave the ship in order to explore the sea north of her route. Lieut. Johansen accompanied me. On March 3 we reached 84° 4' N. Johansen and I left the *Fram* on March 14, 1895, at 83° 59' N. lat., and 102° 27' longitude East of Greenwich. Our purpose was to explore the sea to the north, reach the highest latitude possible, and then go to Spitzbergen *viâ* Franz Josef Land, where we were certain to find a ship.

On March 22 we were on 83° 10' N. lat. The ice now became rougher, and the drift contrary. On April 3 we were at 85° 50' N., constantly hoping to meet with smoother ice. On April 4 we reached 86° 3' N., but the ice became rougher, until on April 7 it got so bad that I considered it unwise to continue our march in a northerly direction. We were then at lat. 86° 14' N.

I then made an excursion on *ski* further northward in order to examine the possibility of further advance, but I could see nothing but ice of the same description, hummock beyond hummock to the horizon, looking like a sea of frozen breakers, the whole time. We had had a low temperature during nearly three weeks; it was in the neighbourhood of 40° below zero. On April 1 it rose to 8° below, but soon sank again to 38°. The minimum in March was 49° and the maximum 24°. In April the minimum was 38° and the maximum 20°.

On April 8 we began our march towards Franz Josef Land. On April 12 our watches ran down, and we were after that date uncertain of our longitude, but hoped that our dead reckoning was fairly correct. We expected daily to find land in sight, but we looked in vain.

On May 31 we were in 82° 21' N.; on June 4 in 82° 18' N.; but on June 15 we had been drifted north-west to 82° 26'. No land was to be seen, although, according to Payer's map, we had expected to meet with Petermann Land at 83° N. These discrepancies became more and more puzzling as time went on.

We did not reach land until August 6, at 81° 38' N. lat. and about 63° E. long. This proved to be entirely ice-capped islands. In our "kayaks" we made our way westward in open water along these islands.

On August 12 we discovered land extending from south-east to north-west. The country became more and more puzzling, as I could find no agreement with Payer's map. I thought we were in a longitude east of Austria Sound; but if this were correct, we were now travelling straight across Wilczek Land and Dove Glacier, without seeing any land near us.

On August 26 we reached a spot in 81° 13' N. and 56° E., where we wintered. The spring came with sunshine and much open water to the south-west, and we hoped to have an easy voyage to Spitzbergen over floe ice and open water. On May 19 we were at last ready to start, and came to open water on May 23, in 81° 5' N., but we were retarded by storms until June 3. A little south of 81° we found land extending westward, and the open water reached west-north-west along its north coast. But we preferred to travel southward over ice through a broad Sound. We came on June 12 to the south side of the islands, and found much open water, trending westward. We sailed and paddled in this direction in order to proceed across to Spitzbergen from the most western cape, but Payer's map is misleading.

We left Franz Josef Land in the *Windward* on August 7, and had a short and very pleasant passage, thanks to the masterly way in which Captain Brown brought his ship through the ice, and thence in the open sea to Vardö.

BACTERIA AND CARBONATED WATERS.

THE new methods of bacteriological research were early called into requisition to determine what hygienic importance from a bacterial point of view could be ascribed to the gaseous aëration of water.

A large number of experiments have from time to time been carried out, and various points of interest have been investigated, but nevertheless considerable divergence of opinion exists as to the precise hygienic value with which the carbonation of water can be credited.

Some authorities state that in such waters the number of bacteria steadily declines, whilst others again have observed as distinct a multiplication of the bacteria present.

The possibility of these two contingencies is, however, quite conceivable without necessarily impugning the accuracy of the results obtained in either case. In the first place it must be remembered that widely different types of water serve for the manufacture of artificial aërated waters, the bacterial contents of which are likewise widely divergent both qualitatively and quantitatively.

Here, then, in the first instance is a source of discrepancy, for the behaviour of bacteria in carbonated waters, as also under other conditions, primarily depends upon the particular varieties of bacteria which have to be dealt with.

It has been shown that whereas some bacteria rapidly disappear in aërated waters, others again are endowed with fabulous powers of multiplication and longevity.

Thus in one instance a sample of carbonated water was found to contain, one hour after its manufacture, 8350 microbes per cubic centimetre; these figures rose, however, after the lapse of 210 days, to the considerable number of 212,400 per c.c.; later on, however, at the end of 428 days, there were only 46 per c.c.

Again, as regards the duration of vitality of ordinary water microbes under these circumstances, we read of as many as 91 being found per c.c. in a sample of water which was considerably more than two years old.

It is obvious, therefore, that as regards the bacterial contents of a particular sample of aërated water, the results are in the first instance dependent upon the bacterial quality of the original water employed, and the nature of the particular microbes present, whilst it must be acknowledged that a considerable element of chance is introduced into the results, inasmuch as they so greatly depend upon the time at which the examination happens to be made.

Thus in the above example, where such enormous multiplication was observed, no one would hesitate, on the strength of such figures, to condemn that water from a bacterial point of view, whilst if its examination had been longer postponed until it yielded only 46 microbes per c.c., as unhesitating a favourable verdict might have been pronounced upon it. As regards the influence of the bacterial purity of the original water upon the finished article, we have frequent evidence of the paucity of bacteria present when the raw water employed has been deprived of all microbial life by boiling or distillation; but even when such precautions are taken in the first instance, we often find that very considerable numbers of bacteria are present in this water after aëration, a fact which is to be ascribed to the bacterial contamination which subsequently obtains in the process of manufacture. Such contamination may be due to various causes; the storage of the water in reservoirs in the factory has been shown in some cases to be responsible for this result, whilst Dr. Abba has recently called attention to the condition of the bottle-syphons used for the distribution of aërated waters as frequently contributing to bring about this condition of things.

These syphons, he states, in his important report on the aërated waters supplied to Turin, are not only left unsterilised after use, but they are neither washed out nor even emptied completely; hence a deposit is always present, which furnishes ample material for the bacterial contamination of the freshly added water. Another factor which controls to a certain extent the bacterial contents of aërated waters is the amount of carbonic anhydride which is present. This point has been well illustrated by Slater, and Dr. Abba has confirmed his results. Thus:—

Amount of carbonic anhydride present per litre. (Grammes.)	Bacteria per c.c.
15·08	299
12·10	388
11·74	435
9·07	1207
8·01	1354
6·90	1580
6·03	2032

Whether the above rise in the bacterial contents on the release of the gas present is due to the diminution of the pressure or to the specific action of the gas being modified, it is difficult to say; probably both causes co-operate in bringing about the result. At present we have no authoritative experimental observations to decide this point.

That carbonic anhydride is capable of exercising very specific action in the case of some micro-organisms in the absence of pressure, was shown some years ago by Dr. Percy Frankland in his experiments on the influence of carbonic anhydride and other gases on the development of micro-organisms (*Proc. Roy. Soc.*, 1889). Three microbes were experimented with—Koch's cholera bacillus, Finkler-Prior's bacillus, and the *Bacillus pyocyaneus*, an organism frequently found in green pus.

These bacteria were exposed on gelatine surfaces to the action of the gas in closed vessels, and after a time they were transferred to vessels containing air only.

In the case of Koch's bacillus and Finkler-Prior's bacillus, no growth whatever appeared in the carbonic anhydride vessel, neither did any sign of vitality make its appearance when the bacilli in question were subsequently transferred to the air vessel.

The case was, however, different with the green-pus bacillus, for although no growths appeared in the presence of the gas, on being removed to the air-vessel, growths did manifest themselves, showing that the carbonic anhydride had not succeeded in destroying the bacilli as it had done the two others.

Here, then, we have an example of the specific action of the gas being controlled by the character of the particular microbe to be dealt with. Some authorities ascribe the action of carbonic anhydride simply to the bacteria being deprived of oxygen by its means, but the absence of oxygen can also not be held entirely responsible for the deleterious action of carbonic anhydride; thus, in an atmosphere deprived of oxygen by means of hydrogen, Dr. Percy Frankland found that the growth of Koch's cholera bacillus was not interfered with at all, but we have seen how fatally it was affected in the absence of oxygen by the carbonic anhydride. Here, then, clearly the presence or absence of oxygen would appear to have no voice in the results obtained.

As regards the behaviour of pathogenic bacteria in carbonated waters, the results so far obtained are decidedly more unanimous.

There is no doubt that a very general impression prevails that a barrier of no mean obstructive power is placed between the consumer and zymotic disease, by the substitution of aërated waters for ordinary drinking-water, at any rate during times of epidemics.

This impression is to a certain extent justified by investigation, but can at the same time only be encouraged to a moderate extent, as the following researches will sufficiently show.

When anthrax bacilli are introduced into ordinary seltzer water, they do not live more than from fifteen minutes to one hour; when the spores, however, are similarly treated, they survive upwards of 154 days.

As, however, anthrax in the condition of bacilli devoid of spores is only very exceptionally met with, we cannot derive much comfort from using seltzer water; fortunately, however, so far the communication of anthrax is not associated with drinking-water, and from a hygienic point of view the above results may be regarded as of, perhaps, more theoretical than practical interest.

Our position with reference to cholera germs and water is, however, on quite a different footing, and it is extremely reassuring to learn, on the authority of such investigators as Hochstetter, Slater, and Abba, that in ordinary seltzer and soda water, cholera bacilli cannot live longer than three hours. Dr. Abba records some curious results, in which he states that in *sterilised* tap-water gaseously aërated cholera bacilli persisted as long as forty-eight hours, whilst if such sterilised aërated water is rendered alkaline by the addition of 1 per 1000 bicarbonate of soda, their life was prolonged for as much as twelve days.

It would appear that sterilisation, or the removal of competing water bacteria, materially assisted the life of cholera bacilli; and this impression is confirmed by another experiment with alkalised water, in which the water was not sterilised first, and in which the vitality of the bacilli, instead of reaching twelve, was cut down to seven days.

Unfortunately, as regards typhoid infected water, we cannot resort with any degree of security to carbonated waters, unless we have proof that the manufactured article has been stored for at least a fortnight before use.

Slater observed typhoid bacilli alive in ordinary aërated water as long as eleven days, and both Abba and Hochstetter record a vitality of five days. In some cases, however, they appear to disappear much more rapidly, and doubtless a great deal depends upon the initial vital condition of the particular cultivation of typhoid bacilli employed.

Here again Dr. Abba finds in *sterilised*, alkalised, aërated water, that the persistence of the typhoid bacilli is superior to that observed in similar waters not deprived of their bacterial life.

Dr. Abba has also experimented in a similar manner with the *B. coli communis*, and finds that, beyond its exhibiting the customary character of superior hardiness under adverse circumstances to its near relative, the typhoid bacillus, its behaviour resembled that of the latter. Although storage even for such considerable periods of time as over two years cannot, as we have seen—at any rate in some cases—secure the entire elimination of ordinary water microbes, yet storage of considerably shorter duration is of undoubted service in the destruction of disease germs, as far as our information at present goes.

It would appear reasonable, therefore, to make a practice of storing such waters before distribution, a measure recommended many years ago by Duclaux, and which, in the absence of preliminary precautions, such as the removal of all bacteria present by boiling, distillation or efficient filtration, would appear to be a measure of great hygienic importance.

G. C. FRANKLAND.

A NEW OXYACID OF NITROGEN.

ALTHOUGH rapid strides have been made recently in the chemistry of nitrogen and its inorganic derivatives, since the discovery of hyponitrous acid by Dr. Divers in 1871, no new oxyacid of nitrogen has been described. In the current number of the *Gazzetta Chimica Italiana* (July 31), there is an account by Dr. A. Angeli of the preparation and properties of the sodium and barium salts of a new acid, $H_2N_2O_3$, which fills the gap between hyponitrous and nitrous acids. The method of obtaining this acid is simple and elegant. An alcoholic solution of free hydroxylamine is prepared in the usual manner from hydroxylamine hydrochloride and sodium ethylate, an excess of the latter being taken, and to the solution after filtering off the precipitated salt is cautiously added the theoretical quantity of ethyl nitrate. The reaction proceeds according to the equation $C_2H_5.ONO_2 + NH_2OH = C_2H_5.OH + H_2N_2O_3$, the white sodium salt of the new acid commencing to separate out at once. From this salt, which on analysis proved to be $Na_2N_2O_3$, the barium salt is readily obtained in a pure state by adding barium chloride to the dilute aqueous solution. These salts are both moderately stable in the dry state, but are easily decomposed, on boiling the aqueous solution, into the hydrate of the metal and nitric oxide. The same gas is given off quantitatively on acidifying the aqueous solution, and hence all attempts to isolate the free acid have failed. As regards the composition of this acid, from its mode of formation, the formula $(NO_2)NH.OH$, or nitro-hydroxylamine, naturally follows; and this is to some extent confirmed by the fact that the yellow silver salt, momentarily obtainable from the aqueous solution, is reduced in a few seconds to metallic silver, and Fehling's solution is also readily reduced. The existence of an acid of this composition has already been indicated by Dr. A. Thum, who showed that hydroxylamine salts when oxidised by potassium permanganate in hot alkaline solution take up exactly as much oxygen as corresponds to the formation of $Na_2N_2O_3$, the formula of which might be $O\left\langle\begin{matrix}N.ONa\\ |\\ N.ONa\end{matrix}\right.$. This salt, not yet isolated, is clearly isomeric with the sodium salt above described, since it suffers no further oxidation on boiling with excess of potassium permanganate. It would also yield only one ethyl derivative, whilst nitro-hydroxylamine might be expected to give the isomers $(NO_2).NEt.(OH)$ and $(NO_2).NH.OEt$, which would be readily distinguishable, and this point is being followed up by Dr. Angeli. He has also applied the same reaction to amyl nitrite and to nitrobenzene (*Berichte*, July 27). The amyl nitrite formed a sodium salt, the aqueous solution of which gave a yellow silver salt resembling silver hyponitrite. Nitrobenzene gave a substance which is probably $C_6H_5.NO : N(OH)$, identical with the nitroso-phenylhydroxylamine, $C_6H_5.N(NO).OH$ of Bamberger. The further development of this work, which may be expected to throw light on the constitution of Traube's isonitramine derivatives, Frankland's dinitroethylic acid, and of Pelouze's salt, will be looked for with much interest.

G. N. H.

A RESEARCH ON THE LIQUEFACTION OF HELIUM.[1]

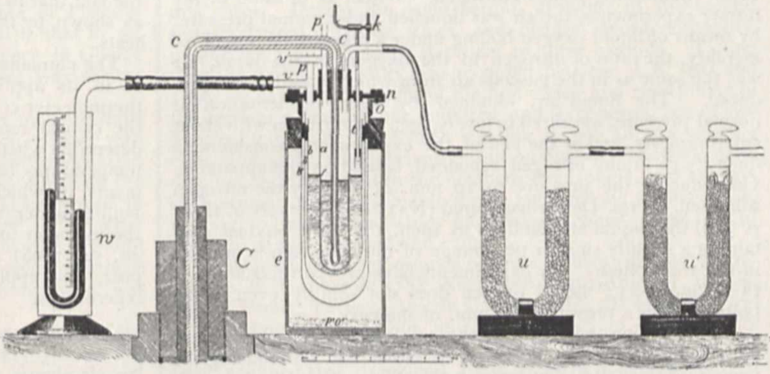
MY experiments on the liquefaction of helium were carried out with a sample of that gas, sent to me by Prof. Ramsay from London, in a sealed glass tube holding about 140 ccm. I take this opportunity of rendering him my most sincere thanks. In his letter Prof Ramsay informed me that the gas had been obtained from the mineral clèvite, and that it was quite free from nitrogen and other impurities, which could be removed by circulation over red-hot magnesium, oxide of copper, soda-lime, and pentoxide of phosphorus. The density of the gas was 2·133 and the ratio of its specific heats (Cp/Cv) 1·652; the latter figure indicating that the molecule of helium was monatomic, as had already been found to be the case with argon.

Prof. Ramsay further informed me that the gas was only very slightly soluble in water; 100 ccm. of water dissolving scarcely 0·7 ccm. of helium.

From the results of my earlier experiments I had been led to expect that it would be only possible to liquefy helium at a very low temperature; the small values obtained for the density and solubility of the gas, together with the fact that its molecule is monatomic, indicating a very low boiling-point. For this reason I did not consider it necessary to use liquid ethylene as a preliminary cooling agent, but proceeded directly to conduct my experiments at the lowest temperature that could be produced by means of liquid air. The apparatus employed in these investigations is figured in the accompanying diagram.

The helium was contained in the glass tube, *c*, of the Cailletet's apparatus, C. The tube, *c*, reached to the bottom of a glass vessel, *a*, which was intended to contain the liquid air. The vessel, *a*, was surrounded by three glass cylinders, *b*, *b'*, and *b''*, closed at the bottom and separated from one another. The outer vessel, *b''*, was made just large enough to fit into the brass collar, *o*, which supported the lid, *u*, of the apparatus. The tube, *a*, fitted into an opening in the centre of the lid; the tube, *t*, connected with an apparatus delivering liquid oxygen, passed through a hole on the right. The vessel, *b*, was also connected with a mercury manometer and air-pump by means of a T-tube, *p*, *v*, one arm of which passed through the third hole in the lid of the apparatus. The tube, *a*, was closed by a stopper, through which passed the tube, *c*, of the Cailletet's apparatus, a tube connected with the drying apparatus, *u*, *u'*, and one limb of a T-tube, by means of which the manometer and air-pump could be put in connection with the interior of the vessel. The lower part of the whole apparatus was enclosed in a thick-walled vessel, *e*, containing a layer of phosphorus pentoxide.

By turning the valve, *k*, the vessel, *b*, could be partially filled



with liquid oxygen, which, under a pressure of 10 mm. of mercury, boiled at about −210° C. Almost immediately the gaseous air began to condense and collect in the tube, *a*; a supply of fresh air was constantly maintained through the drying tubes, *u* and *u'*, which were filled with sulphuric acid and soda-lime respectively. When the quantity of liquid air ceased to increase, the tap on the U-tube, *u*, was closed, the T-tube, *p' v'*, was connected with the manometer and air-pump, and the liquid air was made to boil under a pressure of 10 mm. of mercury. In order to protect the liquid air from its warmer surroundings, a very thin, double wall tube, *f*, reaching to the level of the liquid in the outer vessel, was placed inside the tube *a*. When, as in some of my experiments, liquid oxygen was used in the inner vessel, this part of the apparatus was dispensed with.

Using the apparatus I have just described, I carried out two series of experiments, in which liquid air and liquid oxygen were employed as cooling agents. The tube of the Cailletet's apparatus was thoroughly exhausted by means of a mercury pump, and then carefully filled with dry helium. In the first series of experiments the helium, confined under a pressure of 125 atmospheres, was cooled to the temperature of oxygen boiling, first under atmospheric pressure (−182°·5), and then under a pressure of 10 mm. of mercury (−210°). The helium did not condense under these conditions, and even when, as in subsequent experiments, I expanded the gas till the pressure fell to twenty atmospheres, and in some cases to one atmosphere, I

[1] Translated from the original paper, by Prof. K. Olszewski, in the *Bulletin de l'Académie des Sciences de Cracovie* for June 1896, "Ein Versuch, das Helium zu verflüssigen," by Morris Travers.

could not detect the slightest indication that liquefaction had taken place. The first time that I compressed the gas I had, indeed, noticed that a small quantity of a white substance separated out and remained at the bottom of the tube when the pressure was released. Possibly this may have been due to the presence of a small trace of impurity in the helium, but it could not have constituted more than 1 per cent. of the total volume of the gas.

In the second series of experiments I employed liquid air, boiling under a pressure of 10 mm. of mercury. The helium was first confined under a pressure of 140 atmospheres, and then allowed to expand till the pressure fell to twenty atmospheres, or in some cases to one atmosphere The results of these experiments were also negative, the gas remained perfectly clear during the expansion, and not the slightest trace of liquid could be detected. The boiling point of liquid air was taken, from my previous determination, to be − 220° C. (*Comptes rendus*, 1885, p. 238). This number cannot, however, be taken as a constant, as the liquid air, boiling under reduced pressure, becomes gradually poorer in nitrogen. Further, the quantity of nitrogen lost by the liquid air on partial evaporation, varies not only with the rate of boiling, but even according to the manner in which it has been liquefied.

If air, under high pressure, be cooled first to the temperature of boiling ethylene, and then to − 150° C., it liquefies, and, on reducing the pressure slowly, liquid air is obtained boiling under atmospheric pressure During the process a considerable quantity of the liquid air evaporates, and the proportion of nitrogen to oxygen in the remaining liquid is less than in air liquefied under high pressure. If the liquid air, obtained by this process, be made to boil under a pressure of 10 mm. of mercury, the proportion of nitrogen in the mixture continues to decrease, but, on account of the large quantity of oxygen present, the liquid does not solidify, although its temperature is some 6° below the freezing point of nitrogen. When, as in some of my former experiments, the air was liquefied under normal pressure, by means of liquid oxygen boiling under a pressure of 10 mm. of mercury, the ratio of nitrogen to the oxygen in the liquid air was the same as in the gaseous air from which it had been produced. The liquid air, obtained by direct condensation at normal pressure, appeared to lose oxygen and nitrogen with about equal rapidity, and at the end of the experiment a considerable quantity of liquid nitrogen remained behind in the apparatus. On reducing the pressure to 10 mm. of mercury the nitrogen solidified. Prof. Dewar has stated (NATURE, February 6, 1896, p. 329) that liquid air solidifies as such, the solid product containing a slightly smaller percentage of nitrogen than is present in the atmosphere My experiments have proved this statement to be incorrect; liquid oxygen does not solidify even when boiling under a pressure of 2 mm. of mercury.

After carrying these experiments to a successful conclusion. I found that it was yet necessary to prove that, on reducing the vapour pressure of boiling oxygen to a minimum, no corresponding fall of temperature takes place. The vessel, *c*, was partially filled with liquid oxygen, and, by means of a small syphon, a small quantity of the liquid was allowed to flow into the tube, *a*. The inner vessel, *a*, was then connected with the air-pump and manometer, and the pressure was reduced to 2 mm. of mercury. The oxygen remained liquid and quite clear. In a second experiment the temperature of the liquid oxygen, boiling under 2 mm. of mercury pressure, was measured by means of a thermometer. The temperature indicated lay above − 220° C., a temperature easily arrived at by means of liquid air. I therefore concluded that liquid air was a much more efficient cooling agent than liquid oxygen, and that it would be quite unnecessary to make further experiments on the liquefaction of helium.

In every single instance I have obtained negative results, and, as far as my experiments go, helium remains a permanent gas, and apparently much more difficult to liquefy than even hydrogen. The small quantity of the gas at my disposal, and, indeed, the extreme rarity of the minerals from which it is obtained, compelled me to carry out my investigation on a very small scale. Using a larger apparatus, and working at a much higher pressure, I could have submitted the gas to greater expansion. Further, I should have been able to measure the temperature of the gas at the moment of expansion by means of a platinum thermometer, as I did when working with hydrogen; but to make such experiments I should have required 10, if not 100 litres of the gas. As I was unable to determine the temperatures to which I cooled the gas, by any experimental means, I have been obliged to calculate them from Laplace's and Poisson's formula for the change of temperature in a gas during adiabatic expansion.

$$T/T_1 = (p/p_1)^{k-1/k}$$

Where:—

T, p are the initial temperature and pressure of the gas.
T_1, p_1 are the final temperature and pressure of the gas.
k is the ratio (cp/cv) which, for a monatomic gas, is 1·66.

In the first series of experiments the gas, under a pressure of 128 atmospheres, was cooled down to − 210° C.

p	T	p_1	T_1	
At.	°	At.	°	°
125	− 210 C.	50	− 229·3 C.	43·7 A.
—	—	20	− 242·7 C.	30·3 A.
—	—	10	− 250·1 C.	22·9 A.
—	—	5	− 255·6 C.	17·4 A.
—	—	1	− 263·9 C.	9·1 A.

The results of these calculations tend to show that the boiling-point of helium lies below − 264° C., at least 20° lower than the value I have found for the boiling-point of hydrogen. If the boiling-point of a gas be taken as a simple function of its density, helium, which, according to Prof. Ramsay's determination, has a density 2·133, more than double that of hydrogen, should liquefy at a much higher temperature. Both argon and helium have much lower boiling-points than might be expected, judging from their densities. This anomalous condition may be accounted for by the fact that in each case the molecular structure is monatomic, as shown by the values obtained for the ratios of their specific heats.

The permanent character of helium might be taken advantage of in its application to the gas thermometer. The helium thermometer could be used to advantage in the determination of the critical temperature and boiling-point of hydrogen. To determine whether the hydrogen thermometer is of any value at temperatures below − 198° C., I carried out a series of experiments, in which I measured the temperature of liquid oxygen boiling under reduced pressure. I made use of the identical thermometer tube employed by T. Estreicher (*Phil. Mag.* [5] 40, 54, 1898) as a hydrogen thermometer for the same purpose, and applied the same corrections as were made in his experiments.

Pressure.	Temperature.	
	Helium thermometer.	Hydrogen thermometer.
mm.	°	
741	− 182·6 C.	− 182·6 C.
240	− 191·8 C.	− 191·85 C.
90·4	− 198·7 C.	− 198·75 C.
12	− 209·3 C.	− 209·2 C.
9	− 210·57 C.	− 210·6 C.

The results of these experiments prove that the coefficient of expansion of hydrogen does not change between these limits of temperature, and that the hydrogen thermometer is a perfectly trustworthy instrument even when employed to measure the very lowest temperatures.

I have already pointed out (*Wied. Ann.*, Bd. xxxi. 869, 1887) that the gas thermometer can be used to measure temperatures which lie even below the critical point of the gas with which the instrument is filled. For instance, the critical temperature of hydrogen, which I have found to be − 234·5° C. (*Wied. Ann.*, 56, 133; *Phil. Mag.* [5] 40, 202, 1898) can be determined by means of a hydrogen thermometer. The helium thermometer could be used at much lower temperatures, and would probably give a more exact value for the boiling-point of hydrogen than it is possible to obtain by means of a platinum thermometer.

ON PERIODICITY OF GOOD AND BAD SEASONS.[1]

I FEEL some reluctance in coming forward to-night, with the result of my investigations into the periodicity of good and bad seasons—floods and droughts if you will—because they must come to you as a surprise, and they will make a claim on your confidence which at first sight you will probably not be disposed to grant. For myself I know that some years ago, if any one had come to me stating that it was possible to forecast the seasons many years in advance, I should have received the statement with incredulity. The difficulty in getting the facts together is very great. I have had to ask from history records of passing phenomena which it has been the habit of the historian to neglect; however, there will be before you a mass of evidence in support of my proposition, *that there is a periodicity in weather*. The weak point in the evidence is that history has not kept a regular and continuous account of droughts, but only recorded them when they became very prominent. The strong point is that all the data that history does give us is in favour of the nineteen years' cycle.

And it may be explained that the word drought is not used here in the sense in which it is often used in England and elsewhere, that is, to signify a period of a few days or weeks in which not a drop of rain falls; but it is used to signify a period of months or years during which little rain falls, and the country gets burnt up, grass and water disappear, crops become worthless, and sheep and cattle die.

Drought is, however, not wholly made by a deficiency of rainfall. Its most important factors are great heat and drying winds. As an illustration we may look to the year 1895; in the latter part of winter and in spring there were many falls of rain, which would have made grass in ordinary seasons, although there was not as much as usual, but it had no sooner fallen than a dry north-west wind and burning sun dried it all up. This great and burning heat was a well-known feature in historical droughts, and some authorities say that the fable of Phaëton driving the chariot of the Sun so close to the earth that he set it on fire, is a poetical setting of an actual experience in Greece when the sun became so powerful that the heat was almost beyond endurance.

Before 1895 all the diagrams I used had been made to show quantities of the various elements, and their relation in time, with a view to seeing if there was any periodicity. Recently it occurred to me that it would be useful to have a diagram in which all the droughts, without regard to their intensity, should be placed in their order of time; not only was this desirable for seeing what the relation in time was, but it had become evident that it would be impossible to see the relation between our droughts and those in other countries, unless some such pictorial arrangement was made.

As a preliminary to making the diagram, the particulars of the weather in this colony from all sources, for every year of our history, were carefully examined, and the years simply classed as good or bad; that is, having sufficient or insufficient rainfall. A form was then prepared with a vertical space for each year, and across these a zero line was drawn to divide the good from the bad; and, beginning with 1895, I filled in for that year, and below the line, a convenient length of the column in red ink; the length was simply to catch the eye. Then for 1894, a good year, I filled in with black ink, above the line, a space equal to the red in the vertical space for 1894. The two years were thus contrasted simply as good and bad; the question of how good, or how bad, was purposely left out. The diagram was then completed, each year being treated in the same way back to 1788. It was at once apparent that a drought lasting from three to seven years was most regular in its occurrence. A vertical red line was then drawn between the first and second years of each of these dry periods, and it was found that the interval between two successive lines was regular and exactly nineteen years. The centres of another set of dry periods, more intense and relatively shorter than the first series, were found also to recur at intervals of nineteen years. One of these droughts falls regularly between a pair of the more extensive droughts previously referred to.

In the whole period, from the foundation of the colony of New South Wales to the present year, *i.e.* 108 years, it is certainly very noteworthy that the most pronounced droughts recur with great regularity—that is, at every nineteen years throughout the 108 years. Indian droughts seem to have coincided with Australian ones in many instances.

The investigation had become interesting, and seemed to promise to show the exact year of the great drought in this country, of which there was abundant evidence when the colonists landed here, both in the fact that to the south of Sydney all the very large trees were dead, and between them were growing young trees; and the story of the blacks, who said that the river Hunter dried up; that all the great trees died, and most of the blacks; that those who survived had obtained drinking-water from the mountain springs. I had long wanted to find out when this terrible drought in this colony took place, and the Indian record showed that the extensive drought had been repeated in 1769-70, which probably fixes the date; for the middle of the eighteenth century was very dry, generally, all over the world.

But, if we can carry the nineteen years' period in this way back beyond our history, the idea immediately presents itself, where are you going to draw the limit, is there any limit? It was evidently not a question for argument, but for proof or disproof by figures. Tables were prepared showing every date on which droughts of the first class recurred back to A.D. 1, and the same for droughts of the second class. I am not going to weary you by going through the list, but will give you the result. History says very little about droughts prior to A.D. 900. Between that date and this, a drought has, on the assumption, occurred at every nineteen years. In this interval of nine hundred and ninety-six years there have been fifty-two repetitions of drought, and the question is what has history to say about its droughts. Well, it shows that these droughts have been repeated at various places on the earth on forty-four of the fifty-two dates; of these eight missing droughts, no less than six of them occurred between 1000 and 900 A.D., an interval when history was less complete on these matters. So far as I have gone, history furnishes us with seventy-eight droughts in different countries, all of which fit into the first series. During the same period, droughts of the second series recurred fifty-one times, and history records droughts, numbering eighty-nine, on thirty-six of these periods. Taking then the droughts history has recorded between A.D. 900 and 1896, we have seventy-eight of the first series and eighty-nine of the second, a total of one hundred and sixty-seven, out of two hundred and eight on record; but this is not all, for another class of drought, which is irregular in Australia, seems to be more definite and important in the northern hemisphere, and twenty-six more out of the two hundred and eight belong to this series, making up the number to one hundred and ninety-three out of the total of two hundred and eight.

In estimating the importance of these figures, it must be remembered that, before 1788, North and South America, Russia, China, Persia, Turkey, Austria and Australia, all subject to frequent drought, yet did not, however, furnish to the numbers quoted more than you could count on your fingers; and it may be fairly assumed that if we had these records, and especially if history had made a point of recording droughts, we should have had drought recorded on every recurrence of the nineteen years' cycle, of the two chief series; but I think the evidence that history furnishes one hundred and ninety-three recorded droughts, every one of which fits into the cycle, justifies us in assuming that the nineteen years' cycle has been running for at least one thousand years, and may be trusted to continue and justify forecasts based upon it for some time to come.

Having got so much from the study of droughts in the Christian era, it seemed desirable to see if there were any recorded in B.C. times. Records of twenty B.C. droughts were found, all of which, with one exception, fit into our nineteen years' cycle. If these dates are examined apart from their connection with Australian droughts, we find that the intervals between them are multiples of nineteen years, which shows that droughts then, as now, occurred in cycles of nineteen years, which is very strong evidence in favour of our theory, the more so when it is remembered that all the B.C. droughts I have been able to collect, except one, do fit in: they do not form a series of droughts selected for the purpose of supporting it; again, taking the dates given in the various works, the intervals between all these B.C. droughts and those in Australia are multiples of nineteen years.

If it be objected that chronologists have grave doubts about the accuracy of B.C. dates, I reply, that it is quite certain that chronologists did not arrange the dates to make them fit into an

[1] Abridged from a paper read before the Royal Society of New South Wales, June 3, by H. C. Russell, C.M.G., F.R.S.

unknown cycle running amongst these droughts, or in connection with Australian droughts, the dates of which have been unknown until now. These dates are points in history, and the fact that they fall into a cycle of weather, itself supported by all the available drought dates of the last thousand years of history, is in strong confirmation of the accuracy of these B.C. dates.

These intervals in which our droughts are found repeated are surprising, but I am not unaware of the differences of opinion in regard to chronology, but take the dates as given, and it is remarkable how exactly they fit in. But there is another point of importance hidden in these dates, and probably you have not noticed it. Pharaoh's drought was predicted, and a Jew was made chief man under the king, and he was doubtless versed in much of the wisdom of the priests, and carried that wisdom to the Jewish priests, who did not forget it, as the figures make manifest, and if warning of such evils could be depended upon, it is not likely they would forget it. The figures show that Elijah's prediction was a repetition of Pharaoh's drought 42 × 19 years after it; also Elisha's prediction was nineteen years after Elijah's, and it is noteworthy that the drought in David's time, although it does not appear to have been predicted, was 19 × 36 after Pharaoh's. This seems to me to be very strong evidence in favour of the view that the Egyptians knew of the nineteen years' cycle, and that the Jews brought the knowledge away with them.

Those learned in Assyrian antiquities tell us that the book containing "the Observations of Bel," the oldest astronomical book of that part of the world, was ordered to be kept by the king 3800 years B.C.; that book shows that they kept a record of astronomical and all other events, that they had discovered the nineteen years' cycle of eclipses, and we are told that it was a doctrine with them, that one event caused another, and all astronomical and meteorological observations were thus bound up together. Under such conditions I do not think it would be possible for them to avoid finding in the droughts a similar period to that in the eclipses, *i.e.* nineteen years; but even if they did, it would have been impossible for those who kept the Nilometer in Egypt to avoid finding it in the heights of the Nile floods, which were of such vital importance and so carefully recorded.

Since I have been working at this subject there have been a number of red rain storms noted in New South Wales, and the latest, on April 10, suggested to me this line of investigation. Red dust is obviously a proof of drought somewhere, otherwise the dust could not rise; and since these proofs of drought are entirely apart from the others, and recorded not as droughts but as marvels, which in days gone by created no little alarm, it will be worth while to see how far they support or contradict the nineteen years' cycle. The result of this resolution came as a surprise to me, because it was so unexpected; I had no idea there were so many records of red rains, or that they went so far back in history.

There are altogether sixty-nine recorded instances of the fall of red rain; of these I have recorded six for New South Wales. The first was fourteen years after the foundation of the city of Rome, that is in B.C. 738, and there are nine others B.C., all of which fit into the nineteen years' cycle; between 538 B.C. to 582 A.D. I can find no record of red rain, but from 582 to 1896 there are fifty-nine recorded falls of red rain, and all of them fit into the nineteen years' cycle. We have here, then, ten B.C. droughts which go with the eight mentioned before to make eighteen B.C. droughts in support of the cycle, the remainder, fifty-nine, are included in the previous list.

I have endeavoured to put before you some of the reasons which have convinced me that there is a cycle in weather; but the necessity for brevity in order to keep within the limits of one address, has rendered it necessary to express in a few sentences the results of many separate investigations, and the evidence does not seem so strong when thus condensed as it does when a number of facts one by one are brought to light from diverse sources, all of which individually support the proposition. I can assure you that the evidence was far more convincing when taken in detail; but want of time to get these details into one address, make this course impossible. Enough appears to have been said to prove that the cycle does exist, and to show the very great importance of this re-discovery of a law of climate which, there are many reasons to think, was well known to the Jews, the Egyptians, and other ancient peoples; they at least knew how to forecast droughts successfully, and in Egypt, like sensible people, made provisions for them.

An examination of the weather of one hundred years of New South Wales has shown that certain features recur every nineteen years; we have seen that the droughts of history—the great and conspicuous droughts I mean—all drop into this same cycle: both those that happened before the birth of Christ, and those that have occurred in our era; for instance, Elijah's drought happened in B.C. 908, that is, 2736 before our great drought in 1828, and the interval is 19 × 144. Great hurricanes, the great frosts of history, all the red rains, and all the droughts that history records, with a very few exceptions, likewise are included in this cycle, and the level of great lakes in Palestine, South America, and New South Wales are subject to the same mysterious influence that controls our weather.

As my investigation proceeded, the weight of evidence gradually converged upon the moon as the exciting cause. I have never had any sympathy with the theory of lunar influence upon weather, and received, rather against my will, the evidence that presented itself; still the logic of facts left no alternative but to accept the moon as prime motor. There has not been time to complete this investigation, and when finished it must form another paper. Meantime I may say that, so far, the comparison of the moon's positions in relation to the sun and earth and droughts shows that when the eclipses congregate about the equinoxes, that is, in March and September, they do so in the years which give us great droughts. Further, that when the eclipses accumulate in February and March, that is, at the vernal equinox and the month before it, and September, the autumnal equinox, and the month before it, August, we have the more intense and relatively shorter droughts of the second series, with heat, gales and hurricanes; on the other hand, when they accumulate about March and April, that is, the month of equinox, and the one following, and about September, the month of equinox, and October following it, we have droughts of the first series that are less severe, but much longer than the droughts of the second series. I have spoken chiefly of droughts; though, so far as our own history is concerned, it would have served the purpose just as well if I had taken up the periodicity of wet years, but outside Australia it would have been very difficult to get the necessary data, for history has much more to say about the horrors of drought than the abundance of wet seasons.

SNAKE VENOM AND ANTI-VENOMOUS SERUM.[1]

I HAVE already recorded in a series of memoirs published since 1892 in the *Annales de l'Institut Pasteur* and in the *Comptes rendus de l'Académie des Sciences*, the results of my researches on the venom of snakes, on the immunisation of animals against this venom, and upon the production of an anti-venomous serum. Prof. Fraser has confirmed the facts that I have published, and has successfully repeated almost all my experiments. I bring before you, to-day, a series of new facts relative to the same question. I may say at the outset, in contradiction of the opinion recently expressed by certain physiologists, that it is fully proved that the venoms of the various species of snakes produce physiological phenomena which have certain features common to all, and that the actions of these venoms only differ as regards their local effects. It is now possible to separate, artificially, the substances which produce the local phenomena from those which produce the bulbar intoxication. This separation may be effected by means of heat. If any sample of venom be thrown into watery solution and heated at 85° C. for a period of fifteen minutes, the albumin contained in the solution is coagulated and the phlogogenic substances are destroyed, whilst the toxicity of the substance is entirely unaffected. MM. Phisalix and Bertrand have already demonstrated this fact in the case of the venom of the viper found in France. After heating at 85° C. and filtration, all venoms, both viperine and colubrine, produce the same effects; they only differ as to the degree of their toxic activities. Similarly all are destroyed by the hypochlorites of the alkalis and by chloride of gold, the use of which substances I have suggested—particularly the hypochlorite of lime in a solution of

[1] Abridged from a paper read before the Section of Pathology and Bacteriology of the British Medical Association, by Prof. A. Calmette, Director of the Pasteur Institute, Lille.

1 in 60—for the local treatment of snake bites, to prevent the absorption of the venom.

Quite recently M. Phisalix, assistant in the Paris Museum, has announced that he has succeeded in separating the vaccinating substance of venom by filtering it through a Chamberland filter. The animals into which this experimenter inoculated the filtered venom did not die, and he found that they were vaccinated against the inoculation of a lethal dose of non-filtered venom. I have repeated these experiments with the greatest care, but the results that I have obtained are very different from those obtained by M. Phisalix. When a solution of normal venom is filtered through a Chamberland bougie a great part of the venom is kept back by the porcelain, just as is the case when microbic toxines are similarly filtered. It is certainly found necessary to use two and a half times more of the filtered venom than of the non-filtered venom in order to kill animals of the same weight; but if before filtration care is taken to separate the albumin of the venom by heat, it is found that the porcelain no longer keeps back any of the toxic substance. The animals are killed by the same dose of solution both before and after filtration. It follows very evidently, therefore, if the venom which has not been freed from albumin is less toxic after filtration than before, that this must be due to the fact that the albumin adheres to the porous wall of the filter, so forming a perfect dialysing membrane through which the venom can pass only with very great difficulty. I have been able to prove this by restoring a certain proportion of albumin by means of the addition of normal serum to venom that had previously been deprived of its albumin by heat. On filtering this venom containing the added albumin, I found that the liquid which passed through the filter was again considerably less toxic. Animals which have received filtered venom, and which have not succumbed after the lapse of three days, resist a minimal lethal dose of venom—*i.e.* they do not die; they are already vaccinated, just as are those that have been injected with a less than lethal dose of normal venom. There is, I believe, no reason to suppose that, as has been maintained by Phisalix and Bertrand, there is brought about by heat or by filtration of venom any separation of two substances, the one toxic and the other vaccinating, which are supposed to be found together in normal venom. This hypothesis does not appear to me to be justified by experiment, and it is absolutely certain that if one inoculates an animal with a quantity of heated venom, or of filtered venom of which the toxicity has been modified in sufficient quantity to kill the animal, it will react exactly as if it had been injected with a dose of normal venom a little below that required to produce death. In both cases, and in the same time, the animal acquires through this inoculation a state of resistance which enables it, at the end of several days, to receive with impunity an amount of venom capable of killing animals of the same weight. The serum of animals vaccinated against one species of very active venom, such as the venom of the cobra for example, is perfectly antitoxic as regards the venom of all other species of snakes, and also, as I have been able to prove, against the venom of scorpions.

The best method of procedure for the purpose of vaccinating large animals destined to produce anti-venomous serum consists in injecting them from the outset with gradually increasing quantities of the venom of the cobra mixed with diminishing quantities of a 1 in 60 solution of hypochlorite of lime. The condition and the variations in the weights of the animals are carefully followed in order that the injections may be made less frequently if the animals do not thrive well. Quantities of stronger and stronger venom are in turn injected, first considerably diluted and then more concentrated, and in order that the animals (horses) may give a serum equally active for the various phlogogenic substances which determine the various local actions it is necessary, when they have already acquired a sufficiently perfect immunity, to inject the venoms derived from as large a number of different species of snakes as possible. The duration of the treatment is of considerable length, at least fifteen months, before the serum is sufficiently active to be used for the purposes of treatment. The serum that we actually prepare at the Institut Pasteur, Lille, is active to the degree of 1/200,000th, that is to say, it suffices to inject, as a prophylactic dose, into a rabbit a quantity of serum equal to the 200,000th part of its weight in order to protect it against a dose of venom killing an animal of equal weight in three or four hours. If this serum be injected after the venom, it is sufficiently active in a dose of ½ c.c. given thirty-three minutes after the inoculation of a dose of venom lethal in three or four hours to prevent the death of the animal. Large quantities of this serum have been sent during the last few months to India, to Cochin China, to Australia, and to other countries where venomous serpents are most frequently met with, and we have already been able to collect certain interesting observations on people bitten. It is, however, very difficult in the greater number of instances to obtain information as to the species of venomous snake that has inflicted the wound. It has seldom been found possible to kill or capture on the field the snake inflicting the bite, so that all the statements as to the species of the snake which have not been so determined must be considered as open to some suspicion. I have already published one case, a most conclusive one, that of an Annamite bitten very severely in the hand by a cobra at the Bacteriological Laboratory of Saigon, who was cured by a single injection of 10 c.c. of serum. I have quite recently received the report of another very interesting case, for which I am indebted to Mr. Hankin, director of the laboratory of Agra in India. The patient in this instance was bitten by one of the most dangerous reptiles found in India, the Bungarus.

It has, indeed, been fully demonstrated, both by experiments on animals and by the actual treatment of snake bites in the human subject, that we have in anti-venomous serum a "specific" remedy which is very efficacious against venomous bites. It is, therefore, surely necessary to hasten to distribute it in all those countries where dangerous snakes are found. The only real difficulty consists in procuring sufficient quantities of venom for the immunisation of large animals, such as horses, to furnish adequate quantities of serum. The Pasteur Institute at Lille actually possesses enough venom, and horses completely immunised numerous enough for the most pressing wants. Serum prepared in an absolutely pure condition can be preserved for more than a year without losing any of its curative properties. In all countries where snakes claim their numerous victims, and especially in India, where the annual number of deaths resulting from venomous bites rises to about 22,000, it would surely be expedient that the various Governments should take steps to establish depôts, at least, in the principal agricultural, forest, and mining districts, where medical aid may be afforded as early as possible to every person bitten who comes to seek treatment. Each of these posts should be supplied with (1) a stock of serum, renewed each year; (2) hypodermic injection syringes; (3) a perfectly freshly prepared solution of hypochlorite of lime, and other medicaments and instruments necessary for the dressing of wounds. The expense of effecting such an organisation would be very slight. I ask you, gentlemen, to pass unanimously a resolution that may have the effect of inducing, or affording justification to, the Indian Government to realise this humanitarian scheme.

Dr. Calmette delivered a lecture, with experimental illustrations, in the Laboratories of the Conjoint Board of the Royal College of Physicians and Surgeons, on "The Treatment of Animals poisoned with Snake Venom by the injection of Anti-Venomous Serum." In the course of his lecture he said:

I have to-day the opportunity of giving you the results of experiments that have been performed under Dr. Woodhead's licence, but under my direct personal supervision, so that they may be depended upon as affording direct proof of the value of my method. Those animals that have been successfully treated you may examine for yourselves; others that have been poisoned with the snake venom, but have not received the serum, have succumbed; these latter serve as control experiments with which to compare the results obtained when the serum has been given.

These experiments are easily carried out, and are absolutely painless. In rabbits, as in the human subject, the first symptom indicating the action of snake poison is slight somnolence, which, becoming more and more marked, is gradually succeeded by a condition of unconsciousness associated with, first, muscular contraction and then with loss of motor power, which, commencing in the hind limbs, passes forwards until the respiratory centres are affected, the cardiac centre being the last attacked. When the animal dies, the heart is found in a condition of diastole. The venom may be injected in two ways—intravenously, when a comparatively small dose acts with great rapidity; and subcutaneously, when the dose also acts powerfully but more slowly. A lethal dose of cobra poison injected subcutaneously is about 1 milligramme of dried substance, which proves lethal in about twelve hours. Twice this quantity injected into the veins kills a rabbit of about 1500 grammes in sixteen minutes. Five times as much introduced subcutaneously proves fatal in about three and a half hours. I may, however, give you the results of experiments devised to bring out the exact action of the anti-venomous serum, which experiments have been followed by those who are working in these laboratories.

To exhibit the efficiency of protective injections, at nine o'clock this morning four rabbits, weighing between 1450 and 1770 grammes, were injected intravenously in the lateral aural vein, each with 3 c.c. of the anti-venomous serum. This afternoon these rabbits have been injected intravenously with 2 milligrammes of dissolved dried venom sufficient to kill the animal in sixteen or seventeen minutes. None of these animals show any symptom of sleepiness, and it is evident that the venom will have little, if any, effect upon them. At the time that these animals were injected with the two lethal doses, two control rabbits, weighing 1340 and 1275 grammes respectively, were similarly injected intravenously with 2 milligrammes of the venom; these both succumbed to the symptoms above-mentioned, one in about sixteen minutes and the other in seventeen minutes. We have here, then, ample evidence of the great protective power that the serum exerts when injected into the body before the venom is introduced. In a second series of experiments, carried out to demonstrate the curative properties of this serum, six rabbits were similarly treated with 5 milligrammes of venom injected under the skin. Half an hour afterwards two of these animals received 3 c.c. of the serum intravenously; neither of them showed any symptoms of poisoning, and remained perfectly well. Two others of these poisoned animals, one hour after the venom had been introduced, were similarly injected intravenously with 3 c.c. of the serum; they also remained well. Two of the other rabbits should have been left for one and a half hours, but the dose of poison was so large that one of the animals succumbed at the end of an hour and twenty minutes; the other animal was immediately injected with the same dose of serum as above, with the result that it is now well, although the dose of venom was so large and had been allowed to act for so long a time—long enough, indeed, to kill the other animal injected at the same time. This is a very striking proof of the efficacy of the serum.

Although the anti-venomous serum does not act directly upon the toxin, but only through the cells, it begins to exert its influence immediately it is introduced into the body. This fact is well brought out by the following experiments:—Three c.c. of the serum were injected into the lateral vein of the left ear of a rabbit weighing 1280 grammes; fifteen minutes later this animal received into the lateral vein of the right ear 2 milligrammes of the venom, sufficient to kill it in less than twenty minutes had it not received the serum. The animal has remained perfectly well, and still shows no evidence of poisoning by snake venom. A more striking experiment still is one of which I give a description. A rabbit having received intravenously 2 milligrammes of venom, two minutes later is injected with 5 c.c. of the anti-venomous serum in the vein of the opposite ear. The animal has remained perfectly well.[1] Such an experiment shows that the venom does not destroy the cellular elements at once, and that even when the poison has already found its way to the circulation these cells may be rendered insensible to the action of the poison by means of the action of the serum.

[Dr. Calmette then gave extracts from the paper which he brought before the British Medical Association at Carlisle, and concluded by asking Dr. Woodhead to read the following.]

Gentlemen, the experiments that have been described to you concerning the efficacy of the "anti-venomous serum," the results of which you have before you, prove that the said serum really constitutes a specific remedy against venomous snake-bites. The use of this serum must necessarily become generalised at no distant date in all countries where venomous snakes are found, in order that both men and domestic animals may be protected. Is it not advisable, therefore, for the British or Colonial Governments, which are deeply interested in this matter, to take rigorous measures to prevent the sale in England and in its colonies of serums for which no absolute guarantee of efficacy and purity is given? I have the honour to propose that you will adopt the following propositions, and bring them in some way before the Government at as early a date as possible:—

(1) That there be instituted in London and in each British colony where there are found venomous snakes a sanitary committee, to be entrusted with the duty of testing the efficacy of anti-venomous serums offered for sale or sent out to be delivered gratuitously by druggists and others.

(2) That no bottle shall be sold or distributed unless bearing the mark of such control.

(3) That this control be effected according to the sole, simple, and rapid method which alone presents every guarantee of accuracy.

(4) The method proposed is the following:—A standard solution of venom will be placed at the disposal of the appointed experts. The toxic unit of this solution will be based on the quantity of venom necessary to kill a rabbit of 2 kilogrammes in twenty minutes by intravenous inoculation in the marginal vein of the ear, the above quantity corresponding on an average to 2 milligrammes of cobra venom (weighed dry) and to 4 milligrammes of rattlesnake venom. An anti-venomous serum, to be sufficiently active for therapeutic use, must be a preservative in a minimum dose of 2 c.c. on intravenous injection into a rabbit of 2 kilogrammes against an intravenous injection of the toxic unit of venom. The preventive inoculation must be made fifteen minutes only before the inoculation of the venom. The testing of the serum is thus effected in less than one half-hour.

(5) That stations provided with serum and all the necessary apparatus for its application be established in the principal centres of agriculture and in the mining and forest districts of the colonies infested with venomous snakes, such as Australia, Burmah, and India, so that every person bitten may be able to come at once and receive treatment.

[1] All these animals were still alive and in excellent health eight days later.

REPORT OF THE DEPARTMENT OF SCIENCE AND ART.

THE forty-third Report of the Department of Science and Art, dealing with the work of the Department during the past year, has just been issued in the form of a Blue-book. The report may be taken as a statement of the condition of elementary science teaching in this country; therefore, some of the facts and opinions contained in it are worth recording.

In the science division it is pointed out that in the decennial period, from 1886 to 1895, the number of schools has increased from 1682 to 2673, of classes from 5862 to 9545, and of students from 94,838 to 193,404. Of the 193,404 pupils under instruction in 1895, 188,380 come within the category of those on account of whose instruction payments on the results of examinations are made by the Department. Of the schools examined, 2139 were in England and Wales, 366 in Scotland, and 168 in Ireland. There were 113,398 individual students examined, and 52,079 were successful in passing in one or more subjects. The payments to Science Schools, exclusive of those made to Training Colleges on the results of the examinations for the year 1895, amounted to £142,543, an increase of more than £2000 on the preceding year.

Of the 2673 Departmental Science Schools in 1895, 115 were Organised Science Schools, that is to say, schools in which organised courses of instruction are followed. A new scheme of work for such schools came into force last year, and so far it appears to have worked satisfactorily. Practical physics was made obligatory in these schools by the new scheme, and the result is that while only a few years ago a physical laboratory was a rarity, one will shortly be found in every school in which science forms a proper place in the curriculum.

Mr. C. A. Buckmaster, one of the senior inspectors, places his finger upon a weak point in the education of teachers when he says, "the great failing of the elementary teacher as a science instructor is not want of knowledge, but want of ability to experiment." The reason is that few Training Colleges provide facilities for courses of experimental work, though such scientific practice should be an essential condition for the teaching of science subjects in the Elementary School Code.

Throughout the reports of the inspectors the welcome information is made known that experimental work in science is becoming more common, but there is still much room for improvement. The supply of apparatus is being largely increased, and laboratory accommodation is being extended. The chief difficulty to be contended with at the present time is the insufficient education of the students who join the evening classes. Especially is there a lack of knowledge of scientific principles, and there is a difficulty in getting students to take up subjects which lie at the bottom of all technical subjects. On this point Dr. H. H. Hoffert says: "It is much to be desired that as Technical Institutes multiply, and permanent staffs of well-qualified teachers become appointed, more encouragement may be given to students of evening classes to take up definite courses of study. Such students too frequently attempt the study of the more purely technical and applied subjects, without having the necessary knowledge of the underlying sciences, and in consequence of this the teaching is largely based on rule-of-thumb methods of practice, and is lacking in scientific generality and educational value. There is an undue disproportion in number between classes on such subjects as applied mechanics, steam, and mining, and those in theoretical mechanics, elementary physics, chemistry and geology."

In addition to the reports on instruction in science and art, the Blue-book just issued contains as appendices reports on the Royal College of Science, the South Kensington Museum, and other museums in connection with the Department of Science and Art, supported by the State. There is also in it the Report of the Director-General of the Geological Survey of the United Kingdom and the Museum of Practical Geology, and a Report to the Solar Physics Committee on the work done in the Solar Physics Observatory at South Kensington.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

THE appointments, recently advertised, at the Northampton Institute, Clerkenwell, have been filled as follows:—Mr. John Ashford, Lecturer on Engineering at the Birmingham Technical Schools, to be Head of the Mechanical Engineering and Metal Trades Department; Mr. John Williams to be Head of the Artistic Crafts Department; Mr. C. V. Drysdale to be Chief Assistant in the Applied Physics Department; and Miss Mary A. H. Gibbs to be Head Teacher in the Domestic Economy School.

THE Technical Education Board of the London County Council has addressed a letter to the Councils of University and King's Colleges on the subject of the financial assistance to these institutions during the forthcoming session. It is pointed out in this letter that the Board cannot undertake to ensure regular annual grants towards either of these colleges. It is further recommended that the Councils of the two colleges should confer together before making any application for assistance, with a view to coordinating the work now specially carried on in connection with Oriental languages. A question has been raised with regard to King's College, as to whether the Board can legally make a grant to an institution of a denominational character. But since the discussion of these questions will take some time, it is proposed to continue the grants of £1500 to University College and £1000 to King's College for next year, on the understanding that such a conference shall be held.

THE following complaint, which has been made by *The Local Government Journal*, is not, we think, borne out by the reports of the technical education committees of those County Councils which administer the affairs of the agricultural counties, and which have been sent to us from time to time. The paragraph runs thus: "If technical education committees would bestir themselves and give lessons in thatching, hedging, ditching, sheep-shearing, and so on to the men, instead of providing an afternoon's amusement for labourers' wives in showing them how to make butter without having a cow to produce the milk, and similar instruction for farmers' wives and daughters when the ladies of the farm have no intention of making butter, or of bending their backs to skim the milk, much more good would be done than is accomplished at present, and a great waste of treasure would be obviated." More than one committee in charge of technical instruction would be grateful to our contemporary for some successful method of getting farm-labourers together for the purpose of agricultural instruction, though we have our opinion of the wisdom of teaching the subjects named, even if these arts are not included in the well-known restriction of the Technical Instruction Act.

SCIENTIFIC SERIALS.

American Journal of Science, August.—Molluscan archetype considered as a *veliger*-like form, by A. E. Verrill. In the form of molluscan larva known as *veliger*, and in its slightly younger stages, we have organisms that swim free, often seek their own food, and seem to have claims to be considered the nearest living representatives of the ancestral molluscan archetype, or archetypes, for it is quite probable that the different classes of Mollusca have descended from distinctly differentiated *veliger*-like organisms. In general, it may be stated that nearly all Gastropoda, except certain terrestrial and fresh-water forms, pass through *veliger* stages. The same may be said of Bivalvia, Scaphopoda, and Pteropoda. Cephalopoda, on the other hand, seem to have an abbreviated development, like terrestrial Gastropoda, and leave the egg with the general structure of the adult. It is probable that each of these great classes were originally small, free-swimming forms, furnished with a ciliated locomotive organ similar to the velum of modern veligers. The primitive Cephalopoda had probably a similar origin from a *proveliger* like that of some pteropods and gastropods. On the other hand, it seems impossible to derive a cephalopod or a bivalve from a creeping chiton-like archetype such as Lankester has proposed.—An apparatus for the rapid determination of the surface tensions of liquids, by C. E. Linebarger. The apparatus is based upon Jäger's method of employing two capillary tubes of different bore immersed in the liquid, and measuring the difference of the depths to which they were plunged when air bubbles forced out of them at the bottom required the same air pressure. The tubes employed had bores ranging from 0·1 to 1·5 mm. Two tubes were mounted in clamps in a stand over a test tube containing the liquid, and immersed in a water or glycerine bath. Air pressure was applied, and the orifices were shifted until the liquid was pushed down to the orifices, and there the heights were carefully adjusted until equal streams of bubbles issued from both orifices. The surface tensions were found by the formula

$$\gamma = chs + s^2$$

when γ is the surface tension in dynes per cm., c the apparatus constant, h the distance between the ends of the tubes, and s the specific gravity.—Wardite, a new hydrous basic phosphate of alumina, by J. M. Davison. Mr. Packard's "variscite" from Utah occasionally leaves on decomposition some cavities in the nodules, and encrusting these cavities is a hydrous basic phosphate of alumina, which appears to be a new mineral. It is a light green or bluish green, with vitreous lustre, concretionary structure, hardness about 5, and density 2·77. Its formula is $Al_2(OH)_3PO_4$, and it forms a series with Peganite and Turquois.—On the existence of selenium monoxide, by A. W. Peirce. The author has been unable to find evidence of the existence of the monoxide, either gaseous or solid, and his experiments go to show that the peculiar smell of decayed cabbage, attributed by Berzelius to the monoxide, is only developed when selenium is heated in presence of moisture, if only a mere trace, and is probably due to selenium hydride.

Bulletin of the American Mathematical Society, vol. ii. No. 9, June.—The motions of the atmosphere, and especially its waves, is a translation, by Prof. Cleveland Abbe, of an address by Dr. E. Hermann, which was delivered before the Meteorological Section of the Association of German Naturalists at the annual meeting held in Vienna, September 25, 1894. The author states that the inadvisability of the views according to which the motions of the atmosphere consist in the development of independent cyclones and anticyclones is, of late years, more and more plainly recognised. This conclusion has been arrived at, not so much through a severe criticism of the fundamental basis upon which these erroneous views had been established, as by the power of the facts that resisted introduction into this artificial system. He traces this change of view to the influence of a memoir by Hann, published some ten years since, and then points out that the idea was further developed in von Helmholtz's "Mechanics of the Earth's Atmosphere." He then expounds his own method, and closes with the hope that it may lead meteorology out of the region of vacillating ideas that now control it into a broader field, and "place it among the exact sciences, where everything is reduced to numerical computation, and thus, to an important extent, further its application to daily practice."—Prof. Osgood writes on some points in the elements of the theory of functions.—On the motion of a homogeneous sphere or spherical shell on an inclined plane, taking into account the rotation of the earth, by Prof. A. S. Chessin, discusses some interesting illustrations akin to Foucault's experiments with the pendulum and the gyroscope.—From the Notes we learn that the Council have arranged for a colloquium in connection with their summer meeting at Buffalo, at which are to be delivered two courses of six lectures each, viz. on the subject of linear differential equations and their application, by Prof. M. Bôcher, and on the Galois theory of equations, by Prof. J. Pierpont.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 10.—M. A. Cornu in the chair.—Researches on cyanic acid, by M. Berthelot. By studying the reaction between acetic acid and potassium cyanate it was found to be possible to separate the heat evolved by the simple replacement of the cyanic by acetic acid, from the heat evolved in the subsequent decomposition into ammonia and carbon dioxide. The value thus found for the heat of neutralisation of cyanic acid (12·25 calories) was confirmed by the observation that no reaction takes place between potassium cyanate and boric acid (heat of neutralisation, 11·6 calories).—Researches on the volatility of lævulic acid, by MM. Berthelot and André. Lævulic acid is slightly volatile *in vacuo* at ordinary temperatures. Analysis of the residual acid showed that its composition was not quite the same as the original acid, possibly owing to the formation of an anhydride.—On the reactions taking place in the cold between phosphoric acid and ether in the presence of water, by MM. Berthelot and André.—M. Marcellin Langlois presented two memoirs on thermochemistry.—On the lunar photographs offered to the Academy by M. Weinek, Director of the Prague Observatory, by M. Lœwy.—On the part played by the dielectric in the discharge by the Röntgen rays, by M. Jean Perrin.—Photography in the interior of a Crookes' tube, by M. G. de Metz. By the use of the method previously described, it has been found possible to draw up tables of relative permeabilities to X-rays and kathode rays. With the exception of lead, which is slightly more transparent for the kathode rays than for the X-rays, the two tables are identical, and even this exception appears to be capable of explanation. The kathode rays, like the X-rays, appear to be non-polarisable.—Remarks on the preceding communication, by M. H. Poincaré. In the experiments described in the previous paper, the kathode rays have to traverse a piece of card. It has still to be shown that this card does not give out X-rays.—Researches on the principles of vegetable digestion, by M. V. Poulet. The carefully-cleaned root-hairs of a number of plants gave, on pulverising and extracting with dilute acid, traces of ferrous tartrate. This appears to play an important part in the process of vegetable digestion; and it is suggested that, in the absence of iron in the soil, it is the non-formation of this salt which causes etiolation: that chlorophyll itself in the pure state contains no iron, being now well established.—On a new property of the corpuscle of the silk-worm disease, by M. J. M. Krassilschtchik.—On the heterogamic fertilisation of the algæ *Ectocarpus secundus*, by M. C. Sauvageau.—Alteration in the elimination of phosphates, under the influence of the Röntgen rays, by M. L. Lecercle. The rate of elimination of phosphorus appears to be increased.

NEW SOUTH WALES.

Linnean Society, June 24.—Mr. Henry Deane, President, in the chair.—A new family of Australian fishes, by J. D. Ogilby. In this paper the author proposed to segregate in a new family, under the name *Melanotæniidæ*, certain small fresh-water percesocoid fishes belonging to the Austrogæan region, which differ from all other members of that group by the structure of the first dorsal fin, which consists of a single stout and pungent ray followed by two or more flexible unarticulated rays; by the thoracic insertion of the ventral fins, &c.—New genera and species of Australian fishes, by J. D. Ogilby.—On the Australian *Clivinides* (Fam. *Carabidæ*), by T. G. Sloane. Thirty-one new species of *Clivina* were described, bringing the total for Australia up to eighty-three, divisible into thirteen groups.—On the bag-shelters of certain lepidopterous larvæ of the genus *Teara*, by W. W. Froggatt. A general account is given of the curious bag-like diurnal shelters fabricated by the gregarious larvæ of moths of the genus *Teara*, with particulars of the life-history of *T. contraria* bred from nests obtained near Sydney.—Diatomaceous-earth deposits of the Warrumbungle Mountains, by Prof. T. W. E. David.—In the neighbourhood of the diatomaceous-earth deposits two formations are represented: (1) the permo-carboniferous coal measures and (2) trachyte lavas, dykes and tuffs, with which last are associated the deposits of diatomaceous-earth, and a seam of lignite. At one of the outcrops, fossil leaves (*Cinnamomum Leichhardtii*, Ettingsh.) occur on a horizon immediately above and intimately associated with the diatomaceous-earth. The latter is largely made up of diatoms (the genus *Melosira* predominating) and sponge spicules; and the age of the deposit is provisionally set down as early Eocene or late Cretaceous. The author emphasised and discussed the significance of the fact that all the diatomaceous deposits hitherto found in New South Wales occur in association with volcanic rocks.

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