THURSDAY, DECEMBER 24, 1891.

BOTANICAL NOMENCLATURE.

Revisio Genera Plantarum Vascularium omnium, atque Cellularium multarum, secundum Leges Nomenclaturæ Internationales, cum Enumeratione Plantarum in Itinere Mundi collectarum. Mit Erläuterungen von Dr. Otto Kuntze. Pp. 1011. (London: Dulau and Co., 1891.)

THE importance of this subject is so great, and the alterations made in this book so revolutionary (although the author pretends to be guided by "international rules"), that a brief sketch of the recent history of plant-naming is desirable in order to render any criticisms of the work generally intelligible; and it is all the more called for because Dr. Kuntze specially attacks the position taken up by a considerable section of English botanists.

From the time of the foundation by Linnæus of the binominal system of nomenclature, which cannot be said to have been consummated before the publication of the first edition of the "Species Plantarum" in 1753, down to within the last 25 or 30 years, matters proceeded with tolerable smoothness, though some influential botanists did not scruple to ignore the published names of their contemporaries, or alter them on the most trivial grounds; and there was almost universal laxity in citing authorities. But the more critical investigation of the European flora especially, and to some extent also, perhaps, the tendency to multiply species, led to a more thorough examination of the literature, resulting in the discovery that the same genus or species had often been described and named by more than one writer, the names being usually different. Furthermore the limitation of many of the genera founded by Linnæus and others was greatly modified, some by narrower circumscription, others by amplification, according to the opinions and inclinations of the writers; and of course it frequently happened that different writers dealt with the same materials independently of, and unknown to, each other. Some of these new genera and species were described or proposed in publications of merely local circulation, and were overlooked by the majority of botanists, and others seem to have been purposely neglected; so that in many instances the current and commonly accepted names were of more recent publication than those of other authors. As there appeared to be no way out of the practice of citing the author of a given combination of generic and specific names, it followed that the only fair procedure would be to adopt the name and give credit to the man who *first published* a change generally accepted; because the presumption was that it was always possible, and usually probable, that the later author was aware of the earlier publication. If an author published later than another, his names must be relegated to the synonymy. This is all very well in theory, and is not so very difficult to put into practice, so far as recent writers are concerned, once we have proved the identity of plants under different names; but when we come to the older writers, all sorts of doubts and ambiguities arise, and it seems much better to retain generic and specific names that are as well established as a thing can be in the uncertainties of the relative rank of vegetable organisms. The struggle of literary botanists to bring the law of priority into operation has, as will presently be shown, resulted in successsive changes in nomenclature, each one carrying his investigations a little further than his predecessors, and extending the backward limit of authority for the establishment of genera and species, until the whole thing has drifted into a lamentable and undignified race between persons who deal in dates, and are even prepared to make all sorts of evasions of ordinary rules in order to gratify their craze for reviving old names.

It is hardly necessary to say that these successive changes, apart from the great divergencies as to the limitations of genera and species, have a most deterrent effect on the progress of the study of systematic botany, and make it ridiculous in the eyes of persons who regard a name as merely a means to an end.

In 1867 a Botanical Congress was held in Paris, to which botanists of all countries had been invited, and the most important subject discussed was botanical nomenclature. Mr. A. de Candolle had drawn up a most carefully considered code of rules to govern botanists in their writings; and this code was submitted to the assemblage of botanists, each rule being formulated and modified as the majority deemed wise. Finally, the whole was printed and circulated. The fundamental principle of these laws was priority of publication with *adequate* descriptions, and unfortunately it was made retrospective, without any sufficiently defined statute of limitations. For reasons of their own, the Kew botanists took no part in the proceedings of this Congress; whether wisely or not it would be difficult to determine, and fruitless to discuss. Of course, their position was open to comment and criticism, which have not been wanting; and Dr. Kuntze, while expressing his admiration of the amount and quality of the work done at Kew, deplores the fact that little regard has been paid to remote and obscure priorities. So far he is fair enough; but when he imputes unworthy motives to Bentham, he commits a great mistake, and does grievous injustice to the memory of a man whose sole aim was to advance botanical science, and especially that branch to which he had devoted his life, and which is most intimately bound up with nomenclature. No doubt the authors of the "Genera Plantarum" failed to take up a large number of published generic names; and not being bound down by the law of priority, they were not always consistent, even from the point of view of expediency and convenience, as the surviving author would readily admit. But to suggest that they would not conform strictly to the rule of priority because they would have to undo much of their own work is as disingenuous as it is untrue. The first volume of the "Genera Plantarum" was not completed till 1867, the "Flora Australiensis" was less than half done, and the "Flora of British India" was not commenced; so that, if the authors had had a longing for change and cheap notoriety, they might have re-named a third of the flowering plants of the world. But their idea was to maintain genera and species, as they had been gradually built up, under current names. The opinion of the late Mr. Bentham on this point is clear from the following passage (Journ. Linn. Soc., xix., p. 19) in his "Notes on the

Gramineæ"—the last of the natural orders elaborated for the "Genera Plantarum":—

"Much has been done, however, for the elucidation of the order in local Floras. Already at the close of the last century and the commencement of the present one, several Continental botanists proposed new genera for anomalous European grasses; but these were published in works which entered but little into general circulation, and were overlooked by Beauvois, Persoon, Willdenow, and other systematists. Several of the same genera have since been re-established, but under other names which have now been so long and so universally adopted, that they must be considered as having acquired a right of prescription to overrule the strict laws of priority. It would indeed be mere pedantry, highly inconvenient to botanists, and so far detrimental to science, now to substitute *Blumenbachia* for *Sorghum*, *Fibichia* for *Cynodon*, *Santia* for *Polypogon*, or *Sieglingia* for *Triodia*."

It is idle to argue that two or three persons have no right to make laws; for any corporation, however small, has that right, and is justified in exercising it if it has the power to carry them into effect. But, after all, the main question is, whether the Kew botanists acted in the interest of science in declining to be guided by the rules passed by another body of botanists; and I think any unprejudiced outsider would agree that they did, and that the course events have taken has strengthened their position.

It should be remembered that most of the advocates of priority, and especially those advocates of almost unqualified priority, such as Dr. Kuntze, have no responsibility beyond literary accuracy, and even that cannot be maintained for such uncertain quantities as orders, genera, and species of plants. On the other hand, the botanists of Kew have grave responsibilities towards the general public. It is not too much to say that Kew is almost exclusively responsible for the botanical nomenclature current in gardens, and in English and colonial literature dealing with plants or the products of plants, to say nothing of the vast named collections at Kew. The labour of renaming the plants in accordance with the investigations of successive reformers would have been as nothing to the folly of doing so, though it would have been a herculean task, and a recurring task, as each older name was disinterred. The idea of giving a gardener, or a manufacturer, or any person interested in vegetable products, one of these resuscitated generic names with a specific name tacked on to it by a person who has done nothing else except put his initials to it, is too absurd. All the literature connected with the plant is under another name, all the figures likewise, and, one might add, all the persons almost who know anything about the plant, know it by the old name. Yet, forsooth, we are asked to sacrifice everything that belongs to the present for the sake of a "principle" that involves endless confusion, and feeds the vanity of the living more than it honours the dead. Of course priority in current work is a totally different thing; but if it had been the intention of the promoters of the new "Index to Plant Names," on which Mr. Daydon Jackson and his assistants have been engaged for some ten years, to restore these old generic names, and enumerate the species thereunder, it would now be necessary to cite some 30,000 of them as the combinations of O. K. (Dr. Kuntze). It is no disparagement to the literary researches of Dr. Kuntze to say that Mr. Jackson was in a position to do this infinitely better than Kuntze, if it had been desirable to do it. But it was never a part of the plan that the compiler should reduce synonymy, and amend the nomenclature of plants. His task has been to prepare an index, and as such its value will far exceed any attempts at finality in synonymy. To have proceeded on the lines of Steudel would have only resulted in the addition of many thousands of names devoid of all authority. Nevertheless, Dr. Kuntze, being so impressed with the importance of his precious names, declares that the index will have no scientific value unless it include the 30,000 specific names appropriated by "O. K." without more labour than a mere transfer. Dr. Kuntze worked at Kew for several years, and enjoyed the usual privileges of the establishment, and the exceptional privilege of consulting the index in question; and he now very magnanimously dedicates a genus to the compiler, and patronizingly tells him he hopes he will take proper advantage of the researches and superior wisdom of the author.

The extent to which these changes have been made may be gathered from the author's own summary, in which he states that he has reduced 151 genera; separated off 6 genera; re-named 122 genera, because they bore names homonymous with other genera; restored 952 genera in accordance with the laws of priority; and re-named upwards of 30,000 species belonging to these genera! How he justifies these changes may be learnt from a few examples, selected to illustrate the various extraordinary devices employed by a writer who professes to be animated by a sincere desire to reform and consolidate botanical nomenclature. We may waive for the moment another phase of the question—how far can botanists accept these identifications, even if they are prepared to accept the principle? *Astragalus*, a genus of more than a thousand species, is to be superseded by *Tragacantha*, because the latter name was published by Linnæus in his earlier crude "Systema" (1735), though in his revised and improved work he preferred and employed the former. Kuntze says, in fact, that no author can be permitted to revoke any previously published name of his own making, any more than those of another person; and accordingly he transfers page after page of names from *Astragalus* to *Tragacantha*, with the appended authority, "O. K." Other familiar large genera treated in the same way are: *Erica*, which becomes *Ericodes*, on an even less tenable ground; *Pelargonium* has to cede to *Geraniospermum;* and *Clematis* receives an additional syllable, and in future we must say *Clematitis*. Recent authors have combined *Rhododendron* and *Azalea* under the former, but Kuntze now gives them all names under the latter. Proceeding to examples of more far-fetched changes, it may be noted that *Cleistanthus* is to be *Kaluhaburunghos*, though it was only the other day that Dr. Trimen discovered that a plant in Herrmann's herbarium, bearing this name, which was taken up by Linnæus in his "Flora Zeylanica," was the same as *Cleistanthus acuminatus*. Dr. Trimen also identified *Gaedawakka* as of the same origin with *Chætocarpus*, therefore Kuntze restores the former. Another excuse for changing names is the existence of two of the same derivation. Thus *Glaucium* cannot be tolerated by the side of *Glaux*, and Kuntze takes the opportunity of dedicating the genus to his "dear sister Mary and her husband Franz Mosenthin," and we get the new name *Mosenthinia*. Some other names of the same derivation are sufficiently distinct to avoid confusion, yet Kuntze says they must be treated as homonyms. To this category belong *Hydrothrix* and *Hydrotriche;* consequently the former is re-named *Hookerina*, though a *Hookera* exists and is accepted by our author, who also invents a *Sirhookera!* Failing any of the foregoing reasons, an old name may be modified to conform to modern rules, and then replace a current name. For example, *Katoutsjeroe* goes through this process, and is issued as *Catutsjeron*, otherwise *Holigarna*. In the same way *Anil* becomes *Anila*, and supplants *Indigofera; Caju* is lengthened to *Cajum*, and supersedes *Pongamia;* and *Kauken* to *Kaukenia*, swallowing up *Mimusops*. A still more exasperating kind of change is the transfer of a familiar generic name to some other familiar genus; such as *Armeria* to *Statice*. It may be mentioned in passing that the *Plumbaginaceæ* have fared badly at the hands of this wholesale reformer. *Acantholimon* is referred to *Armeriastrum; Armeria* to *Statice; Vogelia* to *Dyerophyton*, O. K.; *Limoniastrum* to *Limonioides*, altered by O. K. to *Limoniodes*.

Lovers of orchids will probably be long before they adopt the numerous changes effected in the generic names of their favourites. *Dendrobium* is superseded by *Callista*, *Eria* by *Pinalia*, *Saccolabium* by *Gastrochilus*, *Bulbophyllum* and *Cirrhopetalum* by *Phyllorchis*, *Pleurothallis* by *Humboldtia*, and *Angræcum* by *Angorchis*—the last by mistake, it would seem, for *Angræcum* is really older than the substitute. Why *Epidendrum* does not fall is not explained; for as now limited it does not contain one of the species of Linnæus's original *Epidendrum*: and I believe that *Vanilla* would have to be named *Epidendrum* on the principle adopted by Kuntze.

There is another confusing element in these changes. Dr. Kuntze reinstates a number of Aublet's neglected or previously unrecognized genera, with modified spellings. In this way *Coumarouna* and *Tounatea* become *Cumaruna* and *Tunatea*, giving them a widely distant position in an index. On the other hand, Dr. Taubert has recently adopted the original spellings, and appropriated all the species, so that each species is now saddled with at least three names, in order that justice should be done to Aublet, who described one species of each genus!

But Dr. Kuntze is not the only person who believes,—and conscientiously, I am convinced—that botanical nomenclature can only be established on a firm basis by absolute adherence to the rule of priority. As an instance of the extremes to which some of the American reformers and champions of priority and fixity go, I may refer to the writings of Prof. E. L. Greene. With regard to the authorship of species, he contends (*Pittonia*, i. p. 183) "that according to an acknowledged general principle which governs men, or ought to govern them, in all literary work, whether scientific or general," any binominals now in use in the same form that they happen to occur in pre-Linnæan works, such as those of Ray, Bock, Dodoens, Fuchs, and others, should be credited in all modern books, not to Linnæus, but to such of these sixteenth century authors who first employed the combinations; and he enumerates forty-eight examples taken from Ray's "Catalogus Plantarum circa Cantabrigiam nascentium." This, not because these authors had any idea of a binominal nomenclature, but because the ordinary diagnostical phrase of the period happened to be reduced to two words. Of course, if we admit species on this ground, we cannot logically date the genera later; and the same writer ("Flora Franciscana") carries out the same principle for genera, and ascribes *Lupinus* to Catullus, *Linum* to Virgil, *Euphorbia* to Pliny, and *Amygdalus* to Theophrastus!

In a more recent article (*Pittonia*, ii. p. 185), Prof. Greene proposes new names for a number of what he terms "revertible generic names"—that is, names which have at some period been applied to some other plants than those for which they are now current, no matter how remote the chance of revivals. On this principle he supersedes *Pickeringia*, Nutt., *Nuttallia*, Torr. and Gr., *Darlingtonia*, Torr., *Crantzia*, Nutt., *Torreya*, Arnott, and others; and, as he asserts, with great regret.

One might go on multiplying instances of these unnecessary changes, but it would only be wearisome. Still, I may give one or two examples of repeated changes, and we are not sure that we are at the end. Sir Ferdinand Mueller, the eminent Australian botanist, reduced *Candollea*, Labill., to *Hibbertia* (Dilleniaceæ), and replaced *Stylidium* by *Candollea*, whilst *Marlea*, in Cornaceæ, was replaced by the older name for the same genus, *Stylidium*. Kuntze now discovers that *Karangolum* is an older name for *Marlea*, therefore he reinstates *Stylidium* for the plants generally known under that name, and *Candollea* of Dilleniaceæ is relegated back; though in the meantime another compiler had invented the name *Eeldea* for it, in spite of its having been reduced to *Hibbertia*. One more instance: *Nymphæa* and *Nuphar* are names familiar in their application to a large number of persons outside of botanical circles, and there was no objection to them until recently, when Mr. J. Britten found that *Nuphar* ought to be *Nymphæa*, and the latter *Castalia*, and he believed he had reached finality in the matter; but Kuntze now says that *Castalia* must fall, because the name *Leuconymphæa* was employed by Ludwig in 1737. And so these changes go on.

On the whole, I think it will be admitted that the Kew botanists have exercised a wise discretion in employing current and familiar names in preference to these uncertain and endless revivals; and I may say that the same policy will be pursued in the immediate future. If the advocates of change succeed in popularizing their ideas of "right" and "justice" in the matter, then, no doubt, Kew would follow, and not unwillingly.

There are endless difficulties in the way of taking up genera anterior to the first edition of Linnæus's "Species Plantarum," and it seems only rational and consistent that binominal nomenclature should be based upon the foundation of the system, and upon Linnæus's completed work, rather than upon his, or other authors', earlier imperfect works. It is no breach of confidence to say that Mr. Daydon Jackson, who has been ten years engaged on Darwin's "Index to Plant Names," has come to the conclusion that any attempt to adopt genera of an earlier date will lead to hopeless confusion, to say nothing of inconvenience.

There are some genuine cases of priority that one would rather not admit, because there is no advantage gained by them and much confusion is caused, inasmuch as one change often involves several others, and the re-naming of large genera. According to the strict law, *Pimelea* should be *Banksia*, and so Kuntze re-names the latter *Sirmuellera*.

It remains for botanists, who really write for the public, to decide whether, in a general way, it is not better to employ current names; because it is perfectly ridiculous to vapour about the "scientific" value of names. We might as well attempt to purify the English language. All we want is to know what plant is designated by a given name, and that is no easy matter, apart from other complications.

Since the foregoing was written, I have seen an article (*Botanical Gazette*, November 1891, p. 318), by Mr. E. L. Rand, on "Nomenclature from the Practical Standpoint," in which he recommends the course followed by the Kew botanists, without any reference to them, however, or to Dr. Kuntze, whose work could not have reached America at that time. W. BOTTING HEMSLEY.

APPLIED THERMODYNAMICS.

Thermodynamics of the Steam Engine and other Heat Engines. By Cecil H. Peabody, Associate Professor of Steam Engineering, Massachusetts Institute of Technology. (London: Macmillan and Co., 1889.)

SUCH an important work as the present, on the invention which has completely changed in the course of this century the conditions of human life, should not have remained unnoticed so long, and an apology is due to the author; our excuse must be that the scope and power of the book are such as to arrest attention and to excite interest in all its various details.

The work forms a noble companion to the "Applied Mechanics" of Prof. Lanza, the author's colleague; and the students of the Massachusetts Institute of Technology are to be congratulated on their staff, and the possession of such admirable text-books, to direct their theoretical and practical studies.

We find a great contrast here with the ordinary treatises on Thermodynamics to which we are accustomed, where the subject is followed up to a great extent for its mathematical interest, and where little appeal is made to the numerical illustrations on a large scale which we see taking place around us; this treatise is written much more in the style of Prof. Cotterill's "Theory of the Steam Engine," where the methods and results of the application of Thermodynamics to engineering are developed.

The book commences with a general theory and formal presentation of Thermodynamics, as employed by the majority of writers (and beyond which they rarely travel), and follows the ordinary notation and treatment, but has the advantage of being illustrated by carefully drawn diagrams of real curves and machines, with collections of instructive numerical exercises taken from real experience; the student can thus test the soundness of his knowledge as he proceeds.

So long as we deal with the Theory of Perfect Gases, the First Law of Thermodynamics will suffice to carry us orward; and now the best illustrations of theory are to be found in the behaviour of compressed air when used as a motor—for instance, in tunnelling machinery, and in the Whitehead torpedo, or in the working of Refrigerating Machines (chapter xxi.), now of such importance in the New Zealand dead meat trade.

The Second Law of Thermodynamics is introduced in chapter iii., as a formal statement of Carnot's principle, and this again as an experimental law. Statements of this law are of various kinds, but the two given here seem to put the matter in as clear a light as possible:—

(1) All reversible engines, working between the same source of heat and refrigerator, have the same efficiency, *i.e.* the efficiency is independent of the working material.

(2) A self-acting machine cannot convey heat from one body to another at a higher temperature.

This is almost equivalent to the convention that, of two bodies, the one to which heat passes by conduction or radiation has the lower temperature.

Sir W. Thomson's definition of an Absolute Scale of Temperature is now deduced from Carnot's principle; and the correspondence of this scale with that given practically by the air thermometer is found to be so close that they may be taken as coincident.

The theoretical advantages of Superheated Steam (chapter viii.) have led inventors to repeated and costly failures in their attempts at its employment, due to a simple humble cause, the consequent destruction of the dirty greasy film of lubricant, which keeps the working parts from cutting and seizing.

It is related that the introduction of the compound principle (chapter xiii.) into marine engines was due to an attempt at the employment of superheated steam, and that the removal of the superheaters revealed the superiority of the compound engine.

The substance employed to do the work in a steam engine is now invariably "Saturated Vapour" (chapter vii.), the worst substance to choose, according to the precepts of pure Thermodynamics.

The Laws of Saturated Vapour are empirical, and deduced from the experiments of Regnault. Here, as throughout the book, the results are expressed in British units of the foot and pound, while the gravitation unit of force is employed, being the force of a pound in latitude 45° at sea-level.

Prof. Rowland's latest determination of the Mechanical Equivalent of Heat is used, namely 427·1, in Metric Units of metre-kilogrammes per calorie at $16\frac{3}{8}$° C., or 778 foot-pounds.

The Laws of the Flow of Fluids, investigated in chapter ix., are applied immediately to the theory of Giffard's beautiful invention, the Injector, in chapter x.

Working diagrams are given of all the principal variations of the application of the Injector, an instrument in which a jet of steam, by reason of its excess of energy and momentum, is capable not only of overcoming an opposing jet of water from the same boiler, but also of carrying with it, in a condensed form, a much larger quantity of water, and thus feeding the boiler. Still more paradoxical, even the exhaust steam of an engine can be made to perform the same office against a pressure several fold greater. The Injector is working to the best advantage when feeding a boiler, as the heat of the steam jet is returned back again! and although the efficiency is small, when compared with a pump, still the Injector has the advantage of working while the engine is at rest.

The same principle is applied occasionally in the Water Injector and the Ejector, where, for instance, a large body of water, in the form of leakage or water ballast, is to be rapidly cleared out. A somewhat similar instrument, although quite different in principle, is that called the Pulsometer, which is really a revival of the Marquis of Worcester's and Savory's Fire Engine, where the pressure of steam acts directly on the surface of the water. To check the great condensation a piston was introduced, and hence our modern steam engine.

Hot Air Engines are described in chapter xi., and here the mathematical theorems for Perfect Gases receive their most beautiful applications, so that formal treatises on Thermodynamics usually treat this part of the subject at length. Our author dismisses it in about eight pages, with a short description of the principal systems, as, unfortunately, all the practical objections against the use of Superheated Steam are intensified tenfold in the Hot Air Engine. Ericsson once fitted a steamer to cross the Atlantic with engines on this principle: they were very cumbrous although the boilers were dispensed with; and the experiment did not lead to further imitation. An exception must be made in favour of the Gas Engine, as the only practical application of the Hot Air Engine; the author works out the theory, and comes to the remarkable conclusion that the efficiency of the Gas Engine Cycle does not depend, as in ordinary Thermodynamics, on the difference of temperatures so much as on the degree of expansion and compression.

The author reaches the real part of his subject in chapter xii., where he discusses the theory of the Actual Steam Engine, as we really find it working, in the mill, mine, and on the railway or steamer.

Here Hirn appears as the great authority on the careful records of what takes place in the actual engine (chapter xvii.).

"The measurement of quantities of heat, especially when it has to be done in an engine at work, is an operation of great difficulty; and it was not till 1862 that it was shown experimentally by Hirn that h, the heat emitted, is really less than H, the heat received by the engine" (Maxwell, "Theory of Heat").

The example of Hirn has been followed up of recent years by careful and long-continued experiments on steamers and pumping engines in regular work, and the results of the most important of these tests receive careful description and analysis, in chapters xv.-xviii.; a preliminary chapter, xiv., giving a detailed account of the best procedure and instruments required in Testing Steam Engines.

The book will be found indispensable, not only by designers of Steam Engines, but also by writers of abstract treatises on Thermodynamics, as restraining their mathematical development within reasonable limits of actuality, and as directing their analytical powers in a useful direction.

A. G. GREENHILL.

BRITISH FLIES.

An Account of British Flies (*Diptera*). By the Hon. M. Cordelia E. Leigh, F.E.S., and F. V. Theobald, B.A., F.E.S. Vol. I., Part I. (London: Elliot Stock, 1891.)

THE reader involuntarily glances back at the title of this work when the first words that meet his eye on the front page are: "One of the branches of science that has advanced with rapid strides during recent years is geology. . . ." To commence with Fossil Diptera, and to enumerate the families (and some of the genera) members of which are found preserved in the earth's strata, before either families or genera have been in the least degree defined, is a somewhat novel way of beginning. When the work is completed, students will find it useful to transfer chapter i. to the end. The second chapter, entitled "Classification of Diptera, with an introductory account of the ancient and modern classification of Insecta," contains much matter of interest to entomologists in general, although it is questionable whether the authors have arranged their material in either the most attractive or the most methodical form. The classification of the Diptera it is intended to follow is that of Verrall, published in 1888, in which the order is divided into two great sections—the Orthorrhapha and the Cyclorrhapha; the Nematocera and Brachycera being included in the former, and the Proboscidea with the Eproboscidea in the latter. The Aphaniptera (now included in Nematocera) form the subject of the third chapter, in the course of which this first part terminates. The structure and metamorphosis of Pulex are discussed at some length, and certain species are described in detail. Some uncertainty seems to exist in the authors' minds as to how many of them are engaged upon the work, for they use both "we" and "I." This calls to the recollection Cruikshank's picture, "In which there is Antagonism of interest yet Mutuality of object."

It is not possible from a perusal of the first thirty-two pages to form a fair idea as to the general character of the work. It may be stated, however, that it appears to be written for those who are already entomologists, a familiarity with entomological science on the part of the reader being assumed by the authors. Considerable trouble has evidently been taken in consulting authorities whose works are accessible only to the few. That there is plenty of room for a good treatise on the British Diptera will readily be admitted, and if the authors should have something new to tell about such genera as Chlorops, Oscinis, Cecidomyia, and Hylemyia, so much the better. Part I. is illustrated by five woodcuts.

OUR BOOK SHELF.

Principles of Agriculture. Edited by R. P. Wright, F.H.A.S. (London: Blackie and Son, 1891.)

THE *raison d'être* of this little volume is to be found in its "tail," where are reproduced the questions set in the Science and Art Department Examinations in the Principles of Agriculture during the last eleven years. The title-page ought to state, but it does not, that this is a revised edition of a book that was published some years ago. This fact is only discoverable from the preface. The original edition was arranged in three parts, whilst the current edition is in four parts. The added part is somewhat of a jumble, inasmuch as it is supplementary of each of the first three parts. The scheme of the book is not apparent from the list of contents, and this omission results in confusion. Whilst, however, the arrangement of the book is bad, the matter is good. In skilful hands, indeed, the material which is here accumulated might have been very attractively presented. At p. 132, a dozen pages are commenced on the pests of the farm, whilst another dozen pages devoted to the same subject begin on p. 180. At p. 71, the reader enters upon 30 pages about manures, and at p. 167 he gets a further dozen pages also upon manures. And so on.

With reference to the fixation of nitrogen by leguminous plants, mention is made of the presence on the roots of these plants of "little bag-like enlargements, or *tubercles* as they are called." It is unfortunate that this effort should be made to associate the pathological term "tubercle" with these structures. The word "nodule" is much preferable, and is not less explanatory.

Despite the fact that the book has been written to enable candidates to "pass an examination," it is as useful and trustworthy a little treatise of the kind as we have seen.

Elementary Trigonometry. By J. M. Dyer and Rev. R. H. Whitcombe. (London: George Bell, 1891.)

THE title of this book is on all fours with the contents. The work is well adapted for school use. The explanations of book-work are clearly expressed, and the text is amply illustrated by a store of exercises. Sufficient ground is covered to meet the wants of average Army pupils.

We have detected errata in the text on pp. 21, 30, 36, 59, 61, 62, 65, 67, 74, 80, 101, 136, 153. The major part of the proof-sheets has been carefully gone over, but occasionally, as we have indicated, the authors have nodded. The printing in places, in our copy, is defective. But these faults only slightly mar a work which treats a hackneyed subject with all the freshness one can look for in an elementary text-book.

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.*]

Opportunity for a Naturalist.

SINCE the completion of "Argentine Ornithology," in which was given an account of the 434 species of birds then known to occur in the Argentine Republic, Mr. Arthur Holland, of the *Estancia Espartilla*, and Mr. J. Graham Kerr, of the Pilcomayo Expedition, have made excellent contributions to the same subject, and have added some 30 species to the Argentine avifauna. But much more remains to be done, and, in continuation of the work, I am now anxious to get a good series of birds from Uruguay, the fauna of which, so far as we know it, does not appear to differ materially from that of its neighbouring Republic. For this purpose I have made arrangements with a friend to take in a naturalist at his *Estancia*, near Minas, about sixty miles from Monte Video, and am looking for a qualified collector to occupy the post. His necessary expenses will be met, but his further remuneration must de end, more or less, on the results obtained. May I ask the aid of NATURE to make known this eligible opportunity for a young naturalist who can make good birds'-skins, and is anxious to pass a few months in a foreign clime? P. L. SCLATER.

3 Hanover Square, London, W.

Warning Colours.

MR. BEDDARD, in his letter published in NATURE of November 26 (p. 78), calls attention to Dr. Eisig's suggestion that those bright colours of animals which have hitherto been regarded as of warning significance are merely the substances which confer the unpleasant taste, and that therefore the older interpretation is unnecessary and in fact erroneous. The writer furthermore implies that Dr. Eisig's views are not alluded to by those who have written upon animal colours, because they have escaped their attention. There is, however, another possible explanation of such neglect, and one which in my own case is certainly the correct one—viz. that the views in question appear to be so inherently improbable that a large body of confirmatory evidence is required before they demand attention. I do not by this mean to suggest that the unpalatable attribute may not possess a bright colour: this is certainly often the case, especially with the secretions expelled by many insects when they are irritated. But it is highly improbable that these facts afford any refutation of the theory of "warning colours"—that is, of the view which regards the bright and conspicuous colouring as an indication (in mimetic forms a false indication) of some unpleasant attribute, whether associated or unassociated with the colour itself. And as regards the bright colours of Lepidopterous *imagines*, such association is, to say the least of it, entirely unproved. It by no means follows that the yellow colouring of the brimstone and other butterflies is disagreeable in flavour because it "is due to a substance formed as a urinary pigment." And the relation of many animal colours to these pigments by no means necessarily implies unpalatability. Again, it would be impossible to regard merely as a coincidence the fact that the substances in question almost invariably produce a conspicuous appearance, and, furthermore, produce it in a variety of ways. Such an appearance is, as is well known, not merely due to the individual colours, but to their mutual arrangement and relationship. It is due, moreover, to a variety of physical principles, for the production of white is very different from the production of the colours which are so often contrasted with it. Conspicuous effects are furthermore often gained without the use of pigment, as in the brilliantly metallic pupæ of *Euplœa core* and of *Mechanitis lysimnia.* Hence the contention that the bright colour of distasteful insects is a mere incident of chemical composition which has been selected on other accounts is so inherently improbable that it would require a large body of evidence to support it.

But perhaps the strongest argument against the view is that it creates such an artificial distinction between inedibility due to mere unpalatability, and that due to other unpleasant attributes. Mr. Beddard would probably admit that the conspicuous colouring of the skunk, the coral snake, and the wasp possesses a true warning significance; and yet he would interpret the black and yellow colouring of the larva of the cinnabar moth or the pupa of the magpie moth (both known to be unpatable) in an entirely different way, and would deny that it possesses a warning meaning.

In addition to these considerations, the undoubted existence of an unpalatable quality not residing in the superficial pigments is quite clear in many brightly coloured insects. The irritating hairs and odoriferous secretions of many Lepidopterous and Hymenopterous larvæ, and the evil-smelling yellow fluids which exude from *Coccinellidæ* and from many conspicuous butterflies are examples.

The recent investigations of the distinguished Russian naturalist Portchinsky (II. "Coloration marquante et taches ocellées, leur origine et leur développement," St. Petersburg, 1890) have, among other things, shown us the distinct manner in which the colours which attend unpalatability are displayed by the insect when it is disturbed. He thus explains some of the cases of "shamming death" which are so often alluded to in works on insects (the other cases being explained by the necessity for concealment). Two examples which he adduces are so interesting, and have so important a bearing on this discussion, that I cannot resist the temptation of reproducing them here, especially as Portchinsky's paper, being written in Russian, is almost unknown in this country. I have, however, been most kindly helped by my friend Mr. Morfill, and now possess a complete translation, which I hope soon to publish. The female of *Spilosoma mendica* possesses black and yellow legs, and, when disturbed, it folds its limbs and drops to the ground, generally falling on its back, so that the contrasted colours are displayed (see Fig. 1). In the closely allied *Spilosoma urticæ* the dorsal surface of the abdomen is black and yellow, and this insect, when irritated, raises its wings and curves the abdomen downwards so that the colour is conspicuous. Furthermore, only its first pair of legs are black and yellow, and these alone are stretched out conspicuously (see Fig. 2). The great differences between the attitudes of these two closely related moths, corresponding to the distribution of startling colours upon them, afford a very strong support to the theory of warning colours. Mr. Beddard might reply that they thus make prominent the unpalatable pigments that the enemies may first

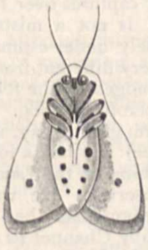


FIG. 1.

FIG. 2.

make trial of them upon a material which will ensure their ultimate rejection. But if the colour has not a meaning as such, there is no reason why this spot should be attacked in preference to any other part of the exposed surface; and the existence of the colour as a covering to the most vital parts seems to indicate that it acts as a warning away rather than in the reverse manner.

The fact that brightly coloured animals are frequently attacked does not seem to me to be a great difficulty. The really important point is whether the enemy remembers the attack, and is assisted in identifying the unpalatable species by its bright colours. Many experiments seem to show that this is so. Certainly Mr. Beddard will not assert that the majority of insect-eating animals fail to know and recognize a wasp without tasting it. Again, the question is really, as Mr. Titchener implies in his interesting communication, one of "comparative palatability"; and there is no doubt that insect eating animals when sufficiently hungry will attack and sometimes devour insects which they would ordinarily reject. Furthermore, an animal which naturally prefers a varied insect food, and which is fed in confinement largely on other substances and partially on a monotonous insect diet, may be expected to be less scrupulous than it would be in the wild state. I may state, however, that the most intelligent insect-eating animals, such as the marmoset, hardly ever make mistakes; their suspicion being at once aroused by any trace of a warning colour.

It is well known that we chiefly owe the theory of warning colours to Mr. A. R. Wallace. My own conviction of its entire validity rests upon the results of a prolonged series of experiments, of which only a part has been published. I believe that I conducted these experiments fairly, that my mind was open, and that I had no personal bias in the matter at all, either in favour of or against the theory. And I can confidently make the same claim on behalf of others who have experimented in the same manner—such as Mr. Jenner Weir, Prof. Weismann, and M. Portchinsky. I may allude especially to the writings of the last-named authority, as they are the most important as well as the most recent contribution to the theory which we owe to Mr. Wallace.

I may also take this opportunity of replying to a very similar objection raised by some reviewers against my book on the "Colours of Animals, their Meaning and Use, &c." They point out that I have not alluded to Eimer's work on the comparison of the wing markings of *Papilionidæ*, and they assume that his paper has, therefore, escaped my attention. But Eimer's paper has no bearing whatever on the value of colour in the struggle for existence, and this is the subject of my book, as anyone can infer from the preface, or even from the title. For this reason I was also compelled to omit reference to what I venture to regard as the far more important work of Weismann on the development of the colours and marking of caterpillars, and of Dixey on the wing-markings of *Vanessidæ* and *Argynnidæ*, as well as a very large proportion of my own work, which is a continuation of that begun by Weismann, and was, in fact, inspired by it.

EDWARD B. POULTON.

Oxford, December 15.

MY friend Prof. Meldola has drawn my attention to a communication by Mr. F. E. Beddard in NATURE of November 26 (p. 78), in which the view is expressed that the brimstone butterfly (*Gonepteryx rhamni*) is rendered protected or unpalatable by the yellow pigment of its wings being due to a substance formed as "a urinary pigment," and that the coloration is "a consequence of the deposition in the integument of bitter pigments."

The following objections may be urged against the view that this coloration, said to be of the nature of a "urinary pigment," affords any protection whatever.

Gonepteryx rhamni itself has its female much paler than the male and of a greenish-white hue, whilst the wings in both sexes are of a leaf-like appearance, which can only be due to the process of natural selection, and can scarcely have been exercised in the direction of "protective resemblance" if the insect was already unpalatable by the "urinative" nature of the yellow pigment of its wings.

Yellow Lepidoptera have certainly no immunity from the attacks of birds; on the contrary, the scanty records we possess of these onslaughts go to prove that the contrary is the case. The late Mr. P. H. Gosse observed one of the greenlets (*Vierosylvia calidris*) to pursue a species of *Terias* in Jamaica ("Birds of Jamaica," p. 194). In Southern India, Mr. E. L. Arnold found the principal victims of the green bee-eaters to be specimens of *Terias hecate* ("On the Indian Hills," vol. i. p. 247–48). Quite recently in the Transvaal I have observed the wagtail, *Motacilla capensis*, to pursue and devour the yellow Lithosiid moth, *Binna madagascariensis*.

But the facts of "mimicry" seem to effectually dispose of the supposition. In South Africa, the yellow black-margined *Papilio cenea* affords by its females the most striking examples to prove the non-protective value of this coloration; for the females respectively mimic those two well-known "protected butterflies," the blackish *Amauris echeria* and the reddish *Danais chrysippus*, whilst, to add to the negative evidence, the yellow male has been seen by Mr. Weale to become the prey of the flycatcher, *Tchitrea cristata*.

On the Amazons, Mr. Bates has long since shown that the yellow and black *Leptalis orise* mimics the markings—even to the colour of the antennæ and the spotting of the abdomen—of the protected or unpalatable *Methona psidii*.

Russell Hill, Purley, Surrey. W. L. DISTANT.

A Difficulty in Weismannism.

IN his communication of November 28 (NATURE, December 3, p. 102), Prof. Hartog asks us to believe that Weismann, in a letter from which he quotes, insists (1) that the Ahnenplasmas are "not completely unchangeable," and (2) that "each Ahnenplasma unit corresponds to an individual of the species itself; and if put under suitable trophic conditions would, singly, reproduce such an individual."

Assuming that thesis II. adequately represents the Freiburg Professor's latest views, and that a few sentences detached from their context are to be depended upon, we must, it seems to me, conclude, with Prof. Hartog, that he has unearthed an inconsistency, and, what is of more importance, shown that the shuffling process is not only unnecessary, but that a new significance must be found for it.

I am, however, still inclined to believe that hypothesis B is the one upon which Weismann has founded his theories of heredity and sexual reproduction. The hypothesis, however, should take account of the variability, slight though it may be, of the Ahnenplasmas. We agree to call the Ahnenplasmas Protozoan, simply because we have no conception of the *kind* and *amount* of the variation they have undergone since they parted company with the unicellular organisms in which they originated. We have no reason, however, to believe that the external causes which led to their variation in unicellular organisms are powerless to affect them now that they are localized in the reproductive cells of multicellular ones.

Prof. Hartog, moreover, while relinquishing the idea of the variability of the offspring of the lioness, endeavours from another point of view to attack Weismannism on the plane of hypothesis B. Is he, too, sceptical as to Weismann's adherence to hypothesis A, or does he simply wish to overwhelm the so-called disciples?

In either case, several objections may be made to his argument. In the *first place*, we object most emphatically to any theory of Weismannism *minus* natural selection. In the *second place*, we believe that Weismann means *permutations*, though he uses the term *combinations*. After a football team has been selected, the men can be arranged in |11 different ways. The arrangements would virtually constitute new teams, and newspapers would speak of them as strong and weak combinations. The combinations of the Ahnenplasmas can be assumed to be of a similar kind. The arrangement almost certainly counts for something. Nevertheless, Prof. Hartog's contention—that the elimination of Ahnenplasmas in the shuffling process would lead to ever-increasing simplicity—demands serious consideration, for duplication lessens the possible number of permutations and combinations. I would point out that we may conceive that the Ahnenplasmas were, in asexual unicellular organisms, either *all the same, all different*, or in intermediate conditions. In any one of these cases we must assume that *m*, the number of individuals, was much greater than *n*, the number of Ahnenplasmas present in every individual. With the evolution of sexuality (all the individuals being different) we should get combinations of, at least, *m* Ahnenplasmas taken *n* at a time. Different permutations of the same combination would be, of course, possible, giving rise to other combinations, using the word in the general sense. We must suppose that natural selection operated upon the variations produced by these first combinations. Natural selection had operated upon the unisexual ancestors of these sexual forms. We can at least conceive that development would follow one of two courses. Along the *first*, combinations in which more than one unit of a kind appeared would, if possible, be prevented. Such might arise, but under the operation of natural selection they would not be allowed to perpetuate themselves. Along the *second*, such combinations might arise and be perpetuated. In either case, it must be assumed that the combinations which survived were such as were best adapted to the varied combinations of external conditions. This may be made clearer by an illustration. In Rugby football, combinations of 15 in which 8 or 9 of the men—the forwards—are all the same would be strong, whereas, if all were different, they would be weak. In Association football, strong combinations could only be made up by selecting different types of players for the different places. I am inclined to believe that both cases are followed by Nature. The one which I have illustrated with reference to Rugby football cannot, however, have been generally followed. It is an adaptation for which the organism has ultimately to pay dearly, and is as dangerous to the development of the *phylum*, as we may suppose parthenogenesis to be to the *species*. Taking the case of plants, I would say that the one course may have been followed along the line of development of the main archegoniate series, the other in the development of such divergent groups as the Ustilagineæ and Gastromycetes. The argument of Prof. Hartog, therefore, while of no avail as directed against Weismannism, is of use in so far as it enables us to better understand *divergence* I am inclined to think that it may serve also to explain the remarkable *persistence* of such forms as Nautilus. It suggests, too, an explanation of the disadvantage of breeding "in and-in." *Finally*, I would remind Prof. Hartog that neither of the disciples of Weismann apparently believes in the non-variability of the Ahnenplasmas. If their beliefs have a substantial foundation, it follows that the number of possible combinations becomes absolutely unthinkable.

I shall be much obliged to Prof. Hartog if he can inform me of any theory of heredity whose foundations are not "more or less mythical." There are, no doubt, many difficulties in Weismannism, before one of which, the theory, having served its time, may come to the ground. I do not think that Prof. Hartog's is one of them. A. H. TROW.

Penarth, Cardiff, December 10.

Destruction of Immature Sea Fish.

IN your number of November 19 (p. 49) you review the Ninth Annual Report of the Scotch Fishery Board. I have not seen the Report, but assume that your reviewer's statements as to its contents are correct. My object in writing is to draw attention to the opinions attributed to Dr. T. Wemyss Fulton as to the destruction of young fish by shrimpers. I may say at once that I am one of the "very many" to whom the "results" are "surprising" as your reviewer remarks. I am an old shrimp-trawler in the Dee and along the Flintshire coast, and I have no hesitation in saying that, as regards the Dee and, I believe, the Mersey and the Lancashire coast as far north as the Ribble, the destruction of young fish is absurdly under-estimated, whether I judge by my own experience or by that of Mr. R. L. Ascroft, of Lytham, with whom I have been in correspondence on the subject since 1889. This gentleman, however, informs me that Dr. Fulton's information was obtained from Morecambe Bay, where smaller trawls are used, and the boats drift with the tide instead of sailing. Dr. Fulton has been informed that in the Solway Firth a single boat in one year captures over 110,000 immature plaice. If the word "year" is not a mistake for "week," either the statement is immensely under-estimated or the conditions in the Solway must be very different from what they are further south. This may be judged by the following extract from a letter written by Mr. Ascroft in 1889. I may say that this gentleman (who is now, I am glad to say, a member of the Lancashire Fishery Committee) has had a long and practical experience in all kinds of sea-fishing on the Lancashire coast, and is a careful and accurate observer. He writes as follows:—"Shrimping destroys more young fish than almost any other agency. I have seen in Formby Channel 10 cwt. of young flukes destroyed, not one the size of half-a-crown, by one boat, and there were sixty boats there that day."

Now, taking the weight of a fluke the size of half-a-crown at $\frac{1}{2}$ oz., a simple calculation will show that each boat captured 35,840 young flukes (a term which includes plaice and dabs) in one day, or 215,040 in a week of six days—nearly twice as many as Dr. Fulton's figures for a year! And elsewhere Mr. Ascroft says: "You may put it as an axiom that 90 per cent. of fish that comes on a boat is destroyed, as when trawling they sail back as they have got their net, and do not commence sorting the take until the net is out again, and they do not, in shallow water, throw the rubbish" (*i.e.* everything except shrimps) "over until they turn out to haul, for fear of getting it into the net again." All of which I may say is borne out by my own experience.

The following is an extract from my diary, written July 10, 1885, when Fishery Committees were not dreamt of. The occasion was an excursion for dredging purposes of the Chester Society of Natural Science, when I took my boat and trawl to meet their steamer at the mouth of the Dee. The Green Buoy marks the bar near Prestatyn, and I let down the trawl in mid-channel (about 5 fathoms) in the hope of getting some natural history specimens:—"Began to trawl just below the Green Buoy. Got a few goodish soles, and an immense number of young soles, which always squeeze their heads through the meshes. (N.B.—Shrimp-trawling at this time of year should only be allowed within a quarter of a mile of the shore, to avoid the immense destruction of fry, which mostly lie further out.) Afterwards got a good haul of shrimps as close in (shore) as we could go." I have a perfect recollection of the occasion, and although the trawl was only down about twenty minutes I was horrified at the number of young soles which were in the net, and most of which had choked themselves. But there were very few shrimps, which mostly lie in very shallow water near the edge of a sand-bank.

As a remedy for this destruction I would suggest that the principal breeding-grounds be ascertained, and trawling on them prohibited at such times as the young fish are there. If the prohibition be evaded, then a steamer-load of very large angular stones, distributed from 100 to 200 yards apart on the selected grounds, would effectually prevent trawling, and at the same time, as they became covered with weed, afford shelter and food to the fish and shrimps. This has been done by Nature in this bay, where large boulders washed out of the drift that here forms the coast-line strew the shore at wide intervals, and render trawling for shrimps impossible, though hand nets can be and are worked.

I trust the importance of the subject will excuse the length of this letter. ALFRED O. WALKER.

Nant y Glyn, Colwyn Bay, December 14.

The Salts in Natural Waters.

THE inquiry of your correspondent "R. B. H.," in NATURE of November 26 (p. 78), may be answered as follows. In the analysis of an ordinary water, after determining the respective amounts of lime, magnesia, (soda), carbonic acid (combined), sulphuric acid, nitric acid, and chlorides (these being the constituents met with usually in such a water), we proceed to combine the acids and bases thus: the carbonic acid is calculated to carbonate of lime; if there be more than sufficient to satisfy all the lime, the remainder is calculated to carbonate of magnesia; if there be too little, however, the remaining lime is combined with sulphuric acid; any remaining sulphuric acid is calculated to sulphate of magnesia, and so on; the order in which the bases and acids are taken being therefore as follows:—

Lime,	Carbonic acid,
Magnesia,	Sulphuric acid,
Soda.	Nitric acid,
	Hydrochloric acid.

Now, although this is the usually accepted and conventional method of returning an analysis, there is no doubt that the assumptions it involves are altogether arbitrary, illegitimate, and unscientific. The *only* scientific method of returning a water analysis is to represent (in parts per 100,000; not in grains per gallon, as the atrocious English system of weights and measures generally compels us to) the constituents *actually found*; as, for instance,

$$CaO ; MgO ; CO_2 ; N_2O_5 ; Cl ; \&c.$$

This is all that an analyst is entitled to say, and this much is certain: when we proceed to combine the constituents, we are dealing in conjecture.

Unfortunately, however, it seems to be a "law of Nature" that those classes of the community who chiefly require the services of analysts are absolutely ignorant of the merest rudiments of chemistry; the consequence is that if any analytical purist endeavours to reform upon the conventionally established procedure, and to return a certificate of analysis in a scientific manner, his clients are up in arms at once, and indignantly demand what he means by sending them such a nonsensical rigmarole.

Thus far, then, we are helpless; but it is *most undesirable* that this conventional procedure should be adhered to whenever it is possible to substitute the scientific (as in an analysis of purely scientific interest).

"R. B. H." asks what salts really exist in solution.

According to Ostwald and others, *no salts at all* if the solution be dilute enough, but only dissociated ions with electrical charges. But whether this theory be correct or not, it is improbable to the last degree that an analysis represents the salts actually present. The indeterminateness of the problem is clearly shown by the fact that from the same solution either sodium chloride and magnesium sulphate, or sodium sulphate and magnesium chloride, may be obtained, according to the method of crystallization adopted. Even supposing that Ostwald's theory be incorrect, and that not ions but salts exist in solution, and that these different results be due to double decomposition occurring in one case, it would be a gigantic assumption that we can definitely show the exact natural distribution in a complicated solution containing eight or ten constituents.

If "R. B. H." wishes to see an account of how acids and bases distribute themselves in a *simple* solution, he may consult Ostwald's "Outlines" (p. 338, &c., English translation), and also the discussion on *avidity* in Lothair Meyer's "Modern Theories of Chemistry" (472–87). F. H. PERRY COSTE.

7 Fowkes Buildings, Great Tower St., E.C., Nov. 28.

I AM much indebted to Mr. Perry Coste for his clear and candid answer to my question. It is exactly the answer which I anticipated. The actual facts established by analysis are too often forced, by the arbitrary assumptions of the analytical chemist, to yield unwarrantable conclusions.

The reason given is, that "the people love to have it so." I had hoped that chemists could give some better grounds for their proceedings. They bring to mind the words of the old prophet: "A wonderful and horrible thing is come to pass in the land; the prophets prophesy falsely," . . . for "my people love to have it so; and what will ye do in the end thereof?" Surely we may henceforth claim, in the interests of truth or (which is the same thing) science, that chemists will give us in every case the actual facts obtained by analysis; and if they proceed further for the sake of the prejudices of the ignorant, they will at least warn them that such further inferences are not trustworthy, and have only a very moderate amount of probability, if they can even lay claim to any probability at all.

I speak feelingly, because I have had occasion to examine a great number of analyses of water from the chalk of the London Basin, telling me, in most cases with a "cocksureness" which has amazed me, what salts, and what amount of them, these waters contained, and these, for purposes of comparison, I have had painfully to reduce back to the real facts from which they were derived.

I am quite prepared to believe that the investigations of Ostwald and others as to solutions show that salts *as such* do not exist in these waters at all, and that the relations of acids and bases in such cases are variable with the physical condition of the water. As an instance which has come under my own notice, it was reported by competent chemists, with reference to water from a deep well in Harrow, in which an unusual quantity of magnesium and sulphuric acid was found, that at 60° F. its hardness was 10°·4 (grs. per gall.); that, mixed with an equal quantity of distilled water, its hardness rose to 24°; while at the temperature of 158° it rose to 26°·5. I suppose that a chemist would hardly attempt to assign with much confidence what exact changes in the relations of the dissolved constituents would produce these and similar results. All the more reason, then, why analysts should limit themselves to statements which they can vouch for by direct observation and the balance.

My remarks having extended beyond a mere question, I think it best to sign myself in full, ROBERT B. HAYWARD.

Peculiar Eyes.

MR. SHAW'S case is by no means so peculiar as he supposes. I imagine that everyone who has had to do with experimental questions of physiological or psychological optics has found it to be rather the exception than the rule that an investigation of his reagents' eyes has shown their perfect equality—as regards "long" and "short" sight, colour sensitivity, and sensitivity to light. The common preferential use of one eye explains a good deal (cf., *e.g.*, Aubert, "Physiol. d. Netzhaut," p. 18; Schön, *Arch. f. Ophthalmologie*, xx. 2, p. 271). Mr. Shaw may also be colour-blind in one eye; the perception of colour difference alone is no criterion. I find it safest to employ the wool, spectrum, and coloured-card tests in combination.

Animals (with the exception of the very highest) have normally a so restricted binocular vision that they need not be taken into account.

It may be interesting to note that a like difference of sensational capacity exists between the two ears. A tuning-fork held to one ear may, quite normally, drown a tone-sensation which is half a musical tone deeper or higher than that excited by the same fork in the other ear. E. B. TITCHENER.

P.S.—I discovered the very considerable inequality of my own eyes quite accidentally in my sixteenth year.

Alleged Pseudopodes of Diatoms.

WILL you allow me to express my concurrence in your criticism (p. 140) on Mr. Grenfell's paper on the occurrence of pseudopodia in the Diatomaceous genera *Melosira* and *Cyclotella?* I express no doubt on the accuracy of Mr. Grenfell's observations, the knowledge of which I have derived from his paper in the *Quarterly Journal of Microscopical Science*, and from his verbal description at a meeting of the Linnean Society; but I do desire to enter my protest against the use of the term "pseudopodia" for the protoplasmic filaments observed by him. According to the accepted meaning of this term, it is applied to masses of protoplasm which are in organic connection with the protoplasm of the body of the organism, and which are retractile. I understand Mr. Grenfell that he is unable to affirm either of these facts with regard to the structures observed by him; and, until this is done, the application to them of the term "pseudopodia" appears to me to involve a begging of the question at issue, and a needless and regrettable confusion in terminology. ALFRED W. BENNETT.

Intelligence in Birds.

UNDER this head Mr. Wilkins, in your last impression (p. 151), speaks of *Podoces panœri* hiding food in the sand. I have a fox-terrier puppy which was taken from its mother when about seven weeks old, and sent to me. I have no other dogs, nor has he seen any dogs, but he buries bones in the garden with great skill, digging a hole with his fore-paws. He puts in the bone, and carefully pushes it down with his nose, and then covers it with garden soil, which is pushed in with his nose. The work is very carefully and elaborately well done.

I have had, at various times, very many dogs of all kinds and ages, but I never saw so young a puppy bury bones, or any dog do it so well. It is an admirable example of pure heredity.

Norfolk Street December 19. JOE.

A NEW LOCALITY FOR METEORIC IRON, WITH A PRELIMINARY NOTICE OF THE DISCOVERY OF DIAMONDS IN THE IRON.[1]

HISTORICAL *Sketch of the Discovery.*—In the latter part of March 1891 the mining firm of N. B. Booth and Co., of Albuquerque, New Mexico, received a letter from a prospector in Arizona informing them he had found a vein of metallic iron near Cañon Diablo, and sending them at the same time a piece with the request for an assay. Some time in April this piece was examined by a Colorado assayer, who reported "76·8 per cent. of iron, 1·8 per cent. lead, ½ ounce silver, and a trace of gold. From its appearance we should take it to be a furnace product."[1]

This result was naturally not satisfactory to the mining firm, and a mass weighing 40 pounds was broken into several fragments with a trip hammer. One of these was sent to the President of the Santa Fe Railroad, and another to General Williamson, the Land Commissioner of the Atlantic and Pacific Railroad Company, in Chicago.

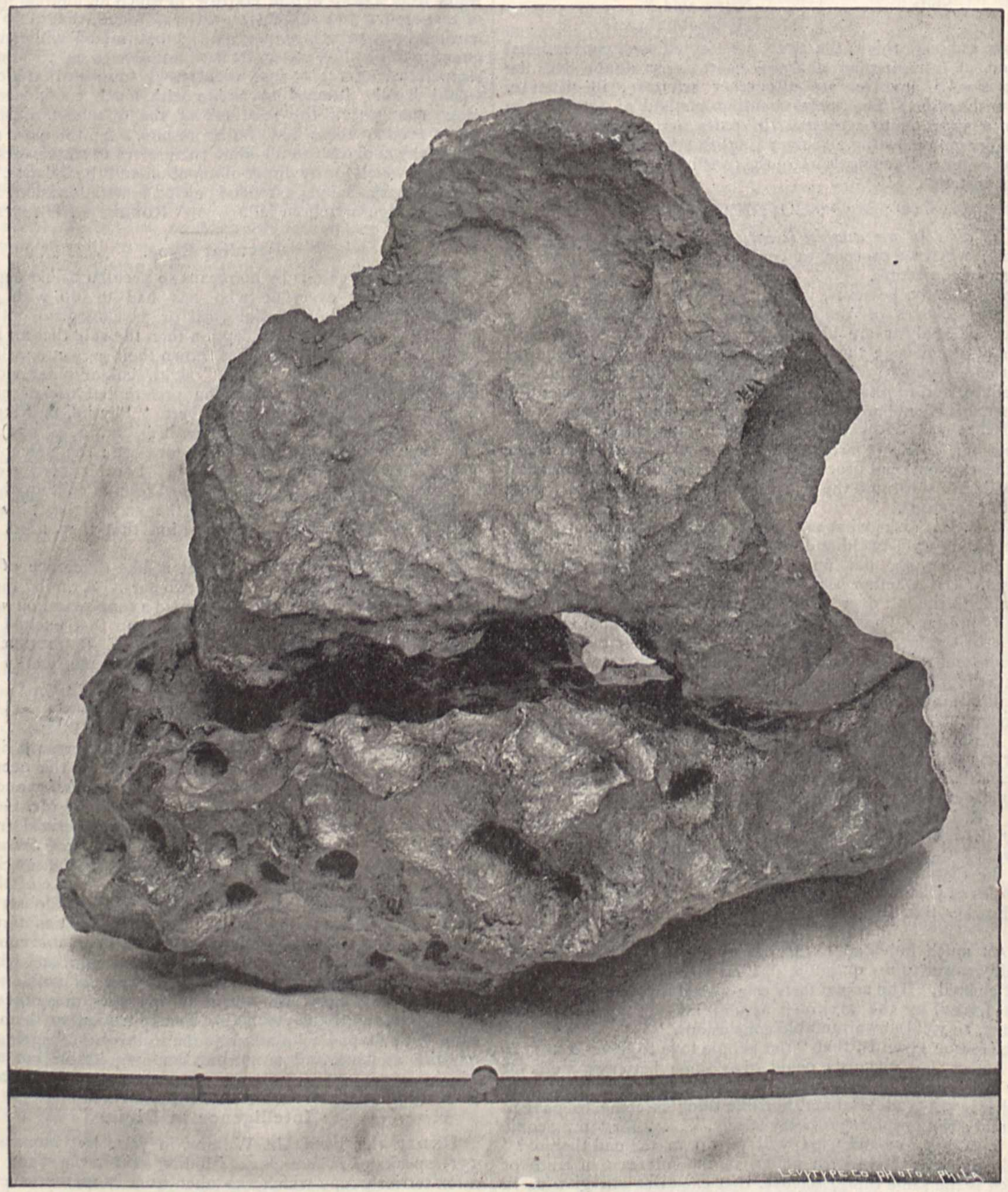


FIG. 1.—General appearance of meteorite.

[1] Read before the American Association for the Advancement of Science, by A. E. Foote, August 20, 1891. From the *American Journal of Science and Arts* for November 1891.

[1] This assay was of such a remarkable character that I took the trouble to stop at the city where it was made, and ask how such extraordinary results were obtained. I was informed that the lead, silver, and gold were probably the results of the materials used in making the assay.

General Williamson consulted me as to the probable value of the so-called mine of "pure metallic iron," stating, on the authority of the prospector, that the vein had been traced for a distance of about two miles, that it was 40 yards wide in places, finally disappearing into a mountain, and that a car-load could be taken from the surface and shipped with but little trouble.

A glance at the peculiar pitted appearance of the surface, and the remarkable crystalline structure of the fractured portion, convinced me that the fragment was part of a meteoric mass, and that the stories of the immense quantity were such as usually accompany the discovery of so-called native iron mines, or even meteoric stones. As soon as possible, in June, I made a visit to the locality, and found that the quantity had, as usual, been greatly exaggerated.

There were some remarkable mineralogical and geological features which, together with the character of the iron itself, would allow of a good deal of self-deception in a man who wanted to sell a mine.

Description of Locality.—Nearly all of the small fragments were found at a point about ten miles south-east from Cañon Diablo, near the base of a nearly circular elevation which is known locally as "Crater Mountain." I believe this is the same as Sunset Knoll, figured on the topographical sheets of the U.S. Geological Survey. This is 185 miles (297·72 kilometres) due north from Tucson, and about 250 miles (402·34 kilometres) west of Albuquerque.

The elevation, according to the Survey, rises 432 feet (131·67 metres) above the plain. Its centre is occupied by a cavity nearly three-quarters of a mile (1·2 kilometres) in diameter, the sides of which are so steep that animals that have descended into it have been unable to escape, and have left their bleached bones at the bottom. The bottom seemed to be from 50 to 100 feet (15·24 to 30·48 metres) below the surrounding plain. The rocks which form the rim of the so-called "crater" are sandstones and limestones, and are uplifted on all sides at an almost uniform angle of from 35° to 40°. A careful search, however, failed to reveal any lava, obsidian or other volcanic products. I am therefore unable to explain the cause of this remarkable geological phenomenon. I also regret that a severe gallop across the plain had put my photographic apparatus out of order, so that the plates I made were of no value.

About two miles (3·22 kilometres) from the point at the base of the "crater" in a nearly south-easterly direction, and almost exactly in a line with the longest dimensions of the area over which the fragments were found, two large masses were discovered within about 80 feet (24·38 metres) of each other. The area over which the small masses were scattered was about one-third of a mile (0·53 kilometre) in length, and 120 feet (36·57 metres) in its widest part. The longer dimension extended north-west and south-east.

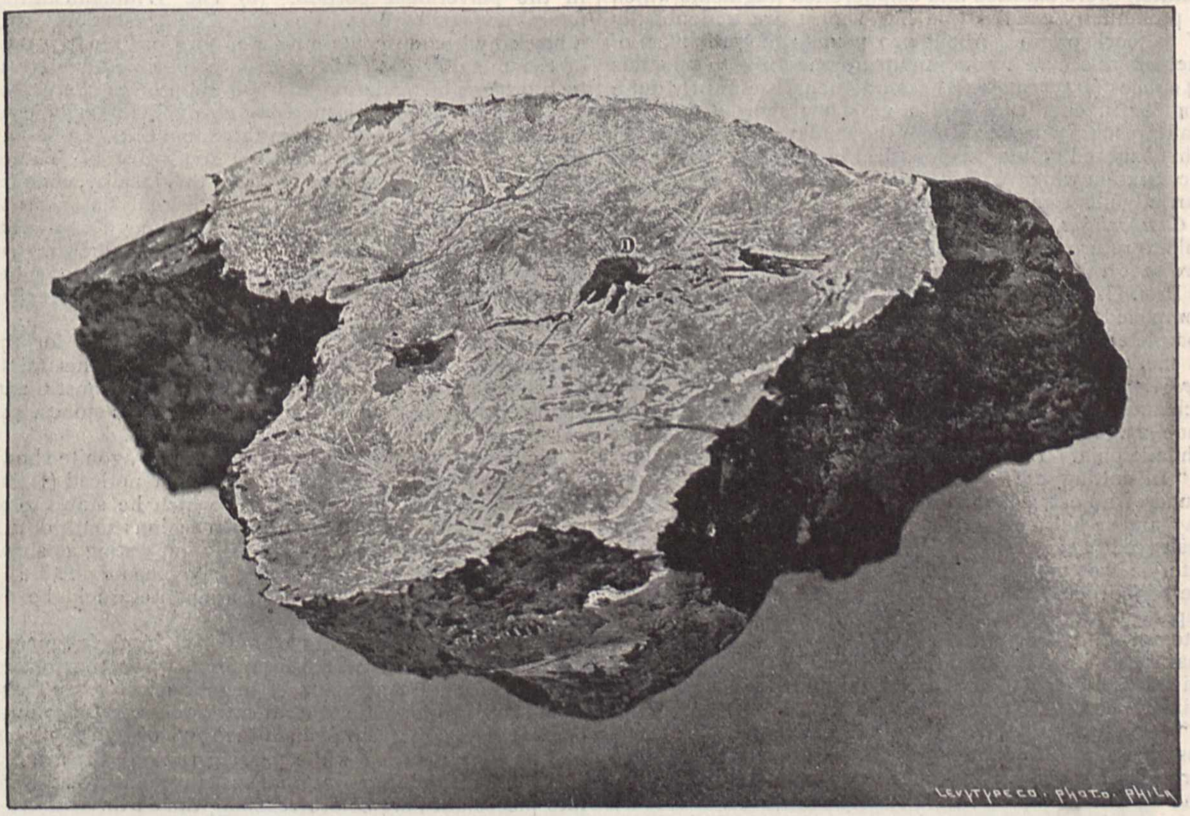


FIG. 2.—Showing the polished surfaces.

Description of the Specimens.—The largest mass discovered weighs 201 pounds (91·171 kilos), and, as the photograph shows (Fig. 1), has a somewhat flattened rectangular shape, showing extraordinarily deep and large pits, three of which pass entirely through the iron. The most remarkable example of such perforation is the Signet Iron from near Tucson, Arizona, now in the National Museum, and figured in Prof. F. W. Clarke's Catalogue.

1 The Signet Iron was discovered about 30 miles (48·28 kilometres) from Tucson. Dr. Geo. H. Horn states that twenty-five years ago he was told by the Spaniards that plenty of iron could be found on a range of hills extending north-west and south-east half-way between Albuquerque and Tucson.

One other large mass was found weighing 154 pounds (69·853 kilos). This is also deeply pitted. A mass weighing approximately 40 pounds (18·144 kilos) was broken in pieces with a trip hammer, and it was in cutting one of the fragments of this mass that diamonds were discovered (Fig. 2).

Besides these masses of considerable size a careful search made by myself with the assistance of five men was rewarded by the discovery of 108 smaller masses. Twenty-three others were also discovered, making a total of 131 small masses, ranging in weight from $\frac{1}{16}$ of an ounce (1·79 grm.) to 6 pounds 10 ounces (3·006 kilos).[1] A brownish-white, slightly botryoidal coating, found on a number of the meteorites, is probably aragonite.

A thorough examination of many miles of the plain proved that the car-load of iron existed only in imagination. Accompanying the pieces found at the base of the "crater" were oxidized and sulphuretted fragments which a preliminary examination has shown are undoubtedly of meteoric origin. About 200 pounds (90·718 kilos) of these were secured, from minute fragments up to 3 pounds 14 ounces (1·757 kilos). These fragments are mostly quite angular in character, and a very few show a greenish stain, resulting probably from the oxidation of the nickel. This oxidized material is identical in appearance with an incrustation which covers some of the iron masses and partially fills some of the pits.

Composition.—After obtaining the meteorite I was unable to return to Philadelphia for some time, and therefore sent a fragment of the 40-pound mass (18·144 kilos) to Prof. G. A. Koenig for examination. Prof. Koenig was compelled to leave town before this examination was completed. I take the following, therefore, from his letters to me, and from an account furnished the daily *Public Ledger* by Dr. E. J. Nolan, Secretary of the Academy of Sciences, of a preliminary notice made by Prof. Koenig, June 23, before the Academy of Natural Sciences of Philadelphia. In this account he says:—

"In cutting the meteoric iron for study it had been found of an extraordinary hardness, the section taking a day and a half, and a number of chisels having been destroyed in the process. When the mass, which on the exterior was not distinguished from other pieces of meteoric iron, was divided, it was found that the cutting apparatus had fortunately gone through a cavity. In the attempt to polish the surface, so as to bring out the characteristic Widmannstättian figures, Dr. Koenig received word that the emery wheel in use had been ruined.

"On examination, he then found that the exposed cavities contained diamonds which cut through polished corundum as easily as a knife will cut through gypsum. The diamonds exposed were small, black, and, of course, of but little commercial value, but mineralogically they are of the greatest interest, the presence of such in meteorites having been unknown until 1887, when two Russian mineralogists discovered traces of diamond in a meteoric mixture of olivine and bronzite. Granules of amorphous carbon were also found in the cavity, and a small quantity of this treated with acid had revealed a minute white diamond of one-half a millimetre, or about 1/50 of an inch in diameter. In manipulation, unfortunately, this specimen was lost, but others will doubtless be obtained in the course of investigation. The minerals troilite and daubreelite were also found in the cavities. The proportion of nickel in the general mass is 3 per cent., and the speaker was not as yet able to account for the extraordinary hardness apart from the presence of the diamonds in the cavities."

[1] October 18.—During September I received three additional large masses weighing respectively 632, 506, and 145 pounds (or 286·678, 229·516 and 65·771 kilos). The two latter were each perforated with three holes. A number of smaller masses up to 7 pounds (3·175 kilos) were discovered by digging. The three large masses and one of 23 pounds (10·432 kilos) were covered with grass and earth.—A. E. F.

Prof. Koenig in a letter to me gives the following points as definitely known:—

"(1) *Diamonds*, black and white, established by hardness and indifference to chemical agents. (2) *Carbon* in the form of a pulverulent iron carbide occurring in the same cavity with the diamonds. The precise nature of this carbide, whether containing hydrogen and nitrogen, is not ascertained, except in so far that after extracting all iron by nitro-hydrochloric acid the black residue goes into solution with deep brown colour upon treating it with potassium or sodium hydrate. From this solution acids do not precipitate anything. (3) *Sulphur* is not contained in the tough malleable portion of the meteorite, but in the pulverulent portion. (4) *Phosphorus* is contained in the latter, and not in the former. (5) *Nickel* and *Cobalt* in the proportion of 2 : 1 are contained in both parts nearly equally. (7) *Silicon* is only present in the pulverulent portion. (8) The Widmannstättian figures are not regular. (9) The iron is associated with a black hydroxide containing Fe, Ni, Co, P, in the ratio of the metallic part, and therefore presumably derived by a process of oxidation and hydration of the latter."

Conclusions.—As this meteoric iron contains only 3 per cent. of nickel, while that from the Santa Catarina Mountains, 30 miles (48·28 kilometres) south-east of Tucson and 215 miles (346 kilometres) from this locality, contains from 8 to 9 per cent., according to the analysis of Brush and Smith, they are quite distinct, although somewhat alike in external appearance. They also somewhat resemble the Glorietta meteoric irons from about 300 miles (482·8 kilometres) to the east-north-east, in New Mexico. These contain 11·15 per cent. of nickel.

The most interesting feature is the discovery for the first time of diamonds in meteoric iron.[1] This might have been predicted from the fact that all the constituents of meteoric iron have been found in meteoric stones, and *vice versâ*, although in different proportions.

The incrustation of what is probably aragonite shown by some of the masses has rarely been noticed (I find two records by J. Lawrence Smith which he states to be unique, and both of these were from regions south of this one). The incrustation is especially interesting as showing that the meteoric irons must have been embedded a long time, as the formation of aragonite would be exceedingly slow in this dry climate.

The remarkable quantity of oxidized black fragmental material that was found at those points where the greatest number of small fragments of meteoric iron were found would seem to indicate that an extraordinarily large mass of probably 500 or 600 pounds (226·796 or 272·165 kilos) had become oxidized while passing through the air, and was so weakened in its internal structure that it had burst into pieces not long before reaching the earth.

THE SEVERE GALE OF NOVEMBER 11.

THE storm which traversed England on November 11 was one of the most severe of recent years. It resulted in considerable loss of life and property at sea on our coasts, and did a large amount of damage on land.

The weather over England at the commencement of the month was dry and fine, and the conditions were those known as anticyclonic, the barometer on November 5 having exceeded 30·7 inches over a great part of the United Kingdom. On November 8, the type of weather became cyclonic, and disturbances were skirting close to our coasts from off the Atlantic, south-westerly gales being experienced in the Hebrides and in the west of Ireland;

[1] Attention may be called to the discovery by Haidinger (1846) of cubic crystals of a graphitic carbon in the Arva meteoric iron, and also of somewhat similar crystals from the Youngdegin (West Australia) iron, described by Fletcher (1887) under the name of cliftonite. Both have been regarded as pseudomorphs after diamond.

whilst on the following day unsettled weather spread to other parts of the United Kingdom, and rain was heavy and persistent over the south of England.

The daily weather report issued by the Meteorological Office at 8 o'clock on the morning of the 10th showed that the winds were westerly and south-westerly over the whole of the British Islands under the influence of a storm area situated off the north-west of Scotland, the readings at our extreme northern stations being 29·1 inches; but a fresh fall of the barometer was already in progress at Valentia, and the wind had there backed to south-south-west. The report added: "The new depression which is approaching our western coasts is at present too far away to enable us to judge of its size or depth." The telegrams received by the Meteorological Office at 2 o'clock indicated the approach of a serious disturbance; the barometer was Islands, and gales were blowing in most parts of the country. The cyclonic circulation of the winds was complete in our islands, the direction being northerly in Ireland, westerly and south-westerly over the Channel and the south of England, southerly on our east coasts, and easterly in Scotland and the northern portion of the Kingdom. The barometer gradients were very steep in the English Channel, as well as in the south-western and south-eastern districts; and at Scilly force 11 of Beaufort's notation was reported from the north-west. At many of the English stations the fall of the barometer since 6 o'clock the previous evening exceeded 0·9 inch, and at Hurst Castle it amounted to an inch, whilst in several places in the south and west the rainfall exceeded an inch in the preceding 24 hours. The gale continued to rage during the day, and at 2 o'clock in the afternoon the

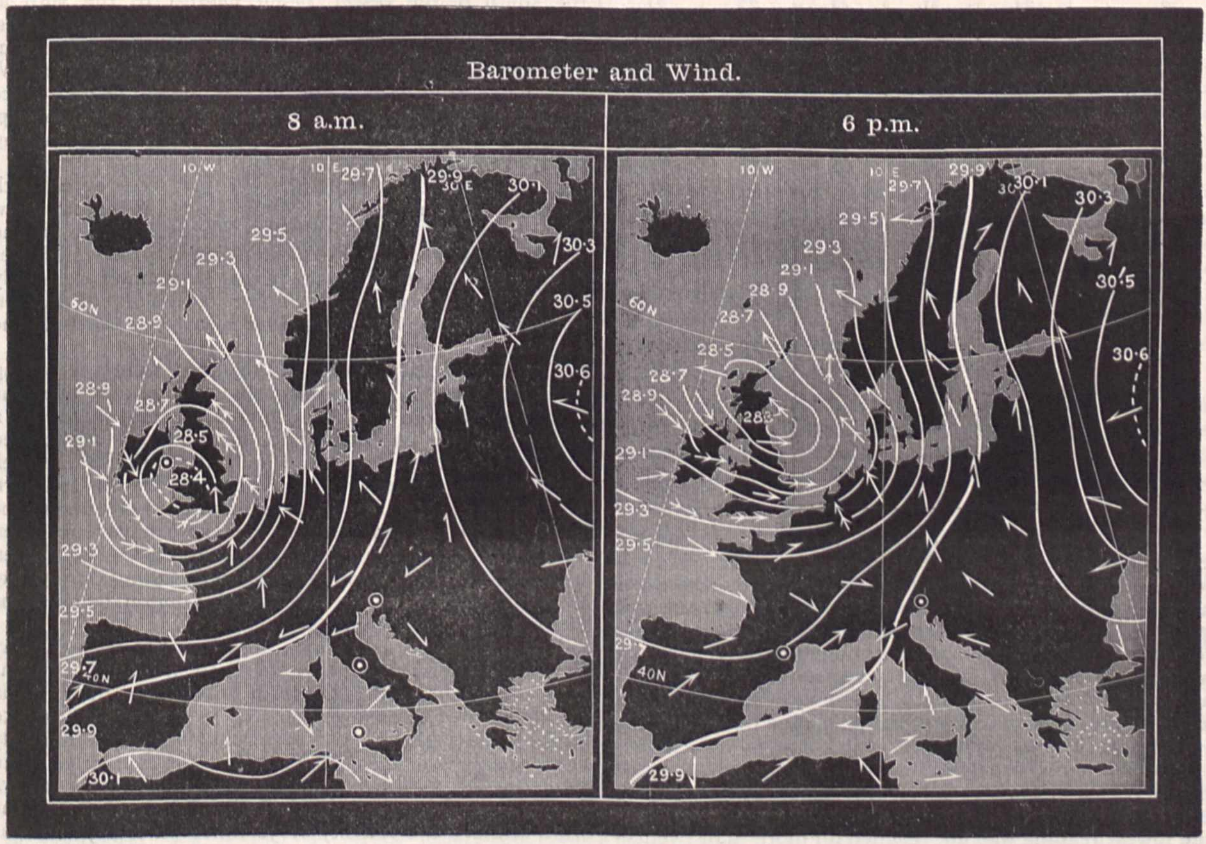


DIAGRAM TO ILLUSTRATE THE SEVERE GALE OF WEDNESDAY, NOVEMBER 11, 1891.

The barometer is expressed by isobars, the pressure corresponding to each line being given in inches and tenths. The winds are shown by arrows which are drawn flying with the wind. ⊙ = a calm; ⟶ = a light or moderate wind; ⟶ = a fresh or strong breeze; ⋙ = a gale.

falling rapidly at the south-western stations, and the fall had now extended even to London, and the wind had backed over the whole Kingdom. The evening reports indicated a still further advance of the storm area towards our islands, and the trend of the isobars over the south-western portion of the Kingdom showed that the centre of the disturbance was not far distant to the south-westward, whilst moderate south-easterly gales were blowing at the entrance of the English Channel.

The conditions on the morning of the 11th are pictured in the diagram for 8 o'clock, obtained from the weekly weather report of the Meteorological Office, and from this it will be seen that the storm area was central over Pembrokeshire, the lowest reading being 28·36 inches at St. Ann's Head, whilst the mercury was below 29 inches over the entire area of the British force of the wind at Dungeness was reported as 12 of Beaufort's notation, which is the extreme limit of the scale, and is equivalent to a hurricane, the lowest barometer reported to the Meteorological Office at this time being 28·34 inches at Shields. At 6 o'clock on the 11th, the central area of the storm had passed to the eastward of our coasts, as shown by the diagram for that hour, the core or heart of the storm not being far distant from Shields, where the barometer was standing at 28·31 inches, which is apparently the lowest reading recorded in the British Islands during the gale. Strong westerly and north-westerly gales were still blowing over the greater part of the United Kingdom, and the succeeding night was very boisterous, although the gale had everywhere subsided before 8 o'clock on the following morning.

The Meteorological Council have very kindly permitted the use of the Observatory records and other documents in their possession, which are more in detail than the eye observations made at the telegraphic reporting stations which furnish data for the daily weather reports.

The following table shows the hourly velocity of the wind as obtained from the anemometrical records. All velocities of 35 miles and upwards are given, and when so strong a wind is recorded at any Observatory, the velocity is given at the other Observatories, although less than 35 miles an hour.

Velocity of the Wind by Anemographs.

Time.	Valentia.	Falmouth.	Holyhead.	Kew.	Aberdeen.
November 11.					
1 a.m.	28	28	41	25	16
2 ,,	34	31	41	27	21
3 ,,	37	24	38	26	20
4 ,,	**41**	23	33	20	27
5 ,,	39	21	31	28	34
6 ,,	38	21	20	34	35
7 ,,	34	25	13	31	37
8 ,,	32	50	2	31	40
9 ,,	34	**62**	17	32	44
10 ,,	33	56	36	35	**47**
11 ,,	28	51	57	30	43
Noon.	33	43	**59**	25	44
1 p.m.	35	43	58	32	41
2 ,,	34	34	52	**45**	39
3 ,,	34	30	43	43	39
4 ,,	32	33	48	35	36
5 ,,	24	20	48	29	30
6 ,,	17	24	42	29	21
7 ,,	17	20	46	29	22
8 ,,	20	18	39	26	17
9 ,,	15	16	46	21	11
10 ,,	18	12	42	17	13
11 ,,	27	11	40	12	22
Midnight.	20	16	36	15	23

From the table it will be seen that the gale did not continue over the United Kingdom for more than twenty-four hours, and at Falmouth and Holyhead, where the highest velocities were obtained, the wind only exceeded 50 miles an hour—a fresh gale of Beaufort's notation—for four hours; whilst the maximum hourly velocity at any observatory was 62 miles, registered at Falmouth at 9 o'clock in the morning. These velocities, although a fair index of the severity of the gale, give no idea of the violence of the gusts or squalls.

The photographic registrations of the barometer show that at Valentia the first fall for the gale set in at 1 a.m., 10th, when the mercury was standing at 29·5 inches, and the lowest reading was not reached for more than twenty-four hours later, the minimum being 28·78 inches at 2.10 a.m., 11th. The fall at Valentia only exceeded the rate of 0·05 inch per hour for two hours, and the subsequent rise there was not very brisk; the wind force, however, at Valentia throughout the storm did not exceed a moderate gale. At Falmouth, the barometer commenced to fall at 8 a.m., 10th, and by 1 a.m., 11th, the mercury had decreased an inch, whilst the lowest reading was 28·37 inches at 5 a.m., 11th. The subsequent rise was very slight at first, but after 8 a.m., 11th, it amounted to 0·15 inch per hour. At Kew the first fall of the barometer is shown at 11 a.m., 10th, just ten hours subsequent to Valentia; and the lowest reading was 28·47 inches at 11·5 a.m., 11th, only nine hours later than Valencia. The fall did not amount to 0·1 inch per hour, but the subsequent rise was 0·15 inch per hour from 1 to 3 p.m. The wind did not veer till after 1 p.m., and then only to west-south-west from south-south-west. The hourly velocity of the wind at Kew evidently affords but little illustration of the violence of the gale, since the maximum velocity was only 45 miles, which occurred at 2 p.m.; whilst at Greenwich the pressure anemometer registered 31·5 lbs. on the square foot at 2·35 p.m. At Fort William the barometer commenced to fall at 11.30 a.m., 10th, and the lowest reading was 28·48 inches at 3.53 p.m., 11th. At Aberdeen the fall of the barometer set in at 7.45 p.m., 10th, and the minimum was 28·38 inches at 9 p.m., 11th; whilst here the wind changed suddenly from south-east by east to west by north at 10 15 p.m., 11th.

The ship *Khyber*, Captain W. Peterkin, keeping a log for the Meteorological Office, felt the first influence of the cyclonic weather system at midnight, 9th, in lat. 49° 30′ N., and long. 13° W., about 300 miles to the west of Land's End, when a moderate south-west wind was blowing, and the barometer stood at 29·64 inches. The wind afterwards changed through south, south-east, east, and north-east, and the centre of the disturbance passed to the south of the vessel, being nearest to the ship at about 10 p.m., 10th, when the barometer was 28·71 inches, and the wind was blowing a fresh gale from north-north-east, the ship being in lat. 49°40′ N., and long. 12° 20′ W. This vessel shows that the wind did not attain gale force until after the centre had passed to the east of the ship, but with a rising barometer she experienced a very strong northerly gale.

The observations from the *Khyber*, considered with those obtained from stations in the United Kingdom, show that the storm system travelled across the area of the British Islands at the rate of about 34 English miles per hour; but the rate of progress was slackening decidedly after it had passed over the centre of England, and on reaching the North Sea it passed away very slowly to the northward.

The exceptional features of the storm were the strong gales experienced in the English Channel and over the southern portion of the Kingdom, accompanied by a terrific sea, the latter being doubtless greatly aggravated owing to the heavy westerly wind setting up the Channel, also the low barometer which occurred in the southern part of the country. In the neighbourhood of London the barometer fell to 28·47 inches, and there have only been seven years since 1811 in which the reading has fallen lower, the absolutely lowest corrected reading during the last eighty years in the vicinity of London being 28·02 inches on January 29, 1814.

The influence of this storm area had not passed away from our islands before an entirely fresh disturbance was seen to be approaching the Irish coasts, and at Valentia a fresh fall of the barometer was in progress after 7.50 p.m. on the 11th, the barometer having only previously risen to 29·20 inches. The mercury subsequently fell to 28·36 inches at 6.20 p.m., 12th, which is more than 0·4 inches lower than during the gale of the 11th; and the wind attained the velocity of 58 miles an hour, and was above 50 miles an hour for ten hours, from 1 to 10 p.m. At Falmouth the wind attained the hourly velocity of 47 miles at 6 p.m., 12th, and at Holyhead 45 miles at noon, 12th; but at Kew and Aberdeen the wind did not increase beyond a fresh breeze.

The sudden manner in which this second disturbance collapsed, after assuming very threatening proportions, is of considerable interest, in so far as it affords a good illustration of the extreme difficulty experienced at times in the weather forecasting for our islands; the present position of science affording no explanation why the one storm should traverse our islands, and the other prove entirely abortive after reaching the western stations.

CHAS. HARDING.

NOTES.

THE Duke of Devonshire, of whose death every one was sorry to hear, maintained throughout life the interest in science which had been fostered by his studies as an undergraduate at

Cambridge, where he distinguished himself equally in mathematics and in classics. He acted as Chairman of the Royal Commission on Scientific Instruction and the Advancement of Science, whose reports might have marked an era in our national progress if there had been a scientific department of the Government to give effect to them. At Cambridge he did what he could to encourage scientific study by his splendid gift of the Cavendish Laboratory. The Duke was the first President of the Iron and Steel Institute; and the Owens College, Manchester, owed much to the zeal and liberality with which, on every suitable occasion, he sought to promote its interests.

MR. E. RAY LANKESTER, Deputy-Professor of Human and Comparative Anatomy, has been elected to the Linacre Professorship of Human and Comparative Anatomy, Oxford, vacated by the death of Prof. Moseley.

PROF. MARSHALL WARD has been engaged lately in studying the strange compound organism called by villagers the "ginger-beer plant." We print elsewhere an abstract of an interesting paper in which he submitted his results to the Royal Society last week.

AT the annual meeting of the Academy of Medicine of Paris, on the 15th instant, the Alvarenga Prize, which is given annually for the best treatise on some medical subject, was awarded to Dr. Bateman, of Norwich, for his work on aphasia, and to Dr. Legneu, of Paris, for his treatise on renal calculi, these gentlemen being bracketed together *ex æquo*. This prize confers the title of Laureate of the Academy.

THE "Committee of Council on Education" have sanctioned the appointment of Mr. George Brebner as first Marshall Scholar in Biology at the Royal College of Science, London. Mr. Brebner has passed through both the botanical and zoological advanced classes of the Biological Division in the Royal College, and in 1889 obtained the Edward Forbes Medal and Prize awarded to the best student of the year in biology. Mr. Brebner has already been engaged in botanical research, and has published two original papers on structural subjects, in conjunction with Dr. D. H. Scott. He has also assisted Dr. E. Schunck, F.R.S., of Manchester, in his investigations of the chemistry of chlorophyll, and is about to publish a joint paper with him. Mr. Brebner's researches as Marshall Scholar will be carried on in the Huxley Research Laboratory, and will be concerned with questions relating to the histology of plants.

THE Paris Museum of Natural History has been partly reorganized by a recent decree. The financial mangement is changed; and it has been decided that the Professors shall, as a rule, retire from their Professorships at seventy-five years of age. To this rule, however, there are to be exceptions. An exceptional case is that of M. de Quatrefages, who retains his post, although Profs. Fremy and Daubrée will have to retire. The name of "aide-naturaliste" disappears, and that of "assistant" takes its place—a fact which is rather curious, since "assistant," in French, has not the same meaning as in English, or as the corresponding word has in German. The assistants are empowered, under some limitations, to deliver courses of lectures, and their financial position is to be improved.

THE Royal Geographical Society is to be congratulated on the success of its system for the proper spelling of geographical names. When its rules on the subject were drawn up, it was not anticipated that foreign nations would make any change in the form of orthography used in their maps. As a matter of fact, however, considerable changes are being effected. In the circular letter, the principal passages of which we print elsewhere, it is noted as a most satisfactory piece of news that France and Germany have both promulgated systems of orthography for foreign words, which in many details agree with the English system.

AN Italian correspondent of the *Lancet* writes that on December 10 the academic world of Rome entertained at a banquet the Senator Stanislao Cannizzaro, in celebration of the bestowal on him of the Copley Medal by the Royal Society of London. The Accademia dei Lincei (the "Royal Society" of Rome), the Accademia di Medicina, and the Senatus Academicus of the "Sapienza" were fully represented on the occasion. The Chairman was the eminent mathematician and engineer, the Senator Brioschi, who, in a few felicitously chosen sentences, conveyed the sense of pride shared by all Italians at the bestowal on their compatriot of the "blue ribbon" of science. Signor Villari, Minister of Public Instruction, also spoke. The Senator Todaro, the Professor of Anatomy in the University, gave the toast of "The Royal Society of London," which was as cordially received as it was eloquently proposed. Prof. Cannizzaro thereafter delivered an effective speech, in which he showed that it was in the effort to make his prelections clear to successive generations of students that he had trained himself to reach those laws, the co-ordination of which had won for him the recognition of the greatest court of scientific arbitration in the world.

ACCORDING to a despatch from Philadelphia, published in the New York *Sun*, it has been decided that an Expedition shall be sent to Greenland for the relief of Lieutenant Peary and his party. Dr. Keeley, who accompanied Lieutenant Peary on his exploring expedition, but afterwards returned, has said that, unless such an Expedition, fully equipped for an Arctic season, were sent to his assistance, Lieutenant Peary and his companions would never reach the bounds of civilization.

MR. RICHARD BOXALL GRANTHAM, who died lately in his eighty-sixth year, was one of the engineers who helped Brunel in the construction of the Great Western Railway. He made the branch line from Gloucester to Cheltenham. He was an authority on sanitary matters, and in 1869 became Chairman of the Committee appointed by the British Association to inquire into the treatment and utilization of sewage. In 1876 he successfully completed the reclamation of Brading Harbour, in the Isle of Wight. This had been attempted by Sir Hugh Middleton 250 years previously, but his work had afterwards been destroyed by the sea.

DUTCH newspapers announce the temporary nomination of Mr. E. Engelenburg, meteorologist at the Royal Meteorological Institution at Utrecht, as Director of the Observations on land. This directorship had become vacant by the appointment of Dr. M. Snellen to the position of Chief Director of the same Institution, which had been held by the late Prof. Buys Ballot. Mr. Engelenburg accompanied Dr. E. van Rÿckevorsel to Brazil, acted as his assistant during the magnetic survey of that country, 1882-85, and prepared a part of the report on this survey published in 1890 by the Royal Academy of Sciences at Amsterdam. In 1887 he was attached by Prof. Buys Ballot to the Meteorological Institution, and has since been responsible for the yearly report on the thunderstorms observed in the Netherlands, formerly prepared by Dr. Snellen. He has also investigated the quantities of rain in different parts of the Netherlands and in the different months of the year. His results on this subject have lately been published in the Memoirs of the Royal Academy of Sciences at Amsterdam; the accompanying rain-maps give a clear idea of the dependence of the rainfall on the distance from the seashore. He has repeatedly directed his attention to the tides at the coast of the Netherlands; to the variation of the velocity of the tidal currents in the Dutch "*zeegaten*," *i.e.* the entrances to the Dutch roads or harbours (*de Ingenieur*, Nos. 5, 9, and 38, of the year 1889); and to the influence of the wind and atmospheric pressure on the height of the sea-surface (*de Ingenieur*, 1891, No. 39).

THE Annual Meteorological Report for Japan for 1889, recently received in this country, shows that considerable attention is given to the subject of meteorology, and contains the results of the hourly observations or continuous records for Tokio, together with observations taken simultaneously at the top and base of Mount Fuji, the highest mountain in Japan. The observations on the mountain were made at a height of about 12,250 feet, during 38 days from August 1 to September 7, 1889. Twice during this period the anemometer was broken by the force of the storms. The position of this station—an extinct volcano, near the Pacific—renders it very important for the investigation of the meteorology of high regions. On this account it has several times been used for that purpose, but the observations have previously all been confined to a few consecutive days.

ON December 18, at 7.30 a.m., a violent earthquake shock was felt at Corleone, an inland town in the province of Palermo, Sicily. The first shock was followed by a pronounced undulatory movement in the direction of north to south.

MR. G. A. NUSSBAUM, agent in London for the Société Générale des Téléphones, Paris, informs us that he has lately made a complete telephone installation at the Adelphi Hotel, Liverpool. The installation comprises three floors, and on each floor a switch board for seventy directions is fitted, the total number of stations being 210. Visitors are thus enabled to communicate with one another, but it seems somewhat doubtful whether they will all be quite pleased to find this sort of thing in their bedrooms.

AN interesting paper on electricity in relation to mining, by Mr. Ernest Scott, was read before the Institution of Engineers and Shipbuilders in Scotland on November 24, and is now printed in the Institution's Transactions. About fifty mines in the United Kingdom are already supplied with electricity, and the new methods are not unlikely, he thinks, to effect "a small revolution in the mining industry." Mines which have been commercially unworkable owing to their depth, or the great distance of the working face from the pit-head, may now be turned into profitable undertakings. Mr. Scott notes that electric power can claim the following advantages over steam, hydraulic, and compressed air: (1) greater efficiency, and therefore reduced first cost and expenses in working, than other mediums of power transmission over considerable distances—say above half a mile; (2) the greater ease with which the comparatively small copper conductors can be manipulated and kept in order as compared with piping, especially where there are falling roofs or shifting floors; (3) the facility with which machines which require to be moved occasionally—*e.g.* coal-cutters, pumps, &c.—can be advanced along the roadways as the work proceeds, or taken about on bogey carriages from one part of the workings to another.

AT the meeting of the Society of Arts on December 16, General Pitt-Rivers delivered a capital lecture on typological museums, as exemplified by the Pitt-Rivers Museum at Oxford, and his provincial museum at Farnham, Dorset. The lecture is printed in the current number of the Society's Journal. By "typological museums," General Pitt-Rivers means museums in which objects are arranged in a way that brings out the sequence of types. Museums of this kind are, he thinks, best suited for educational purposes; and he urged strongly that many of them should be established. The museum he has formed at Farnham has been greatly appreciated; and he believes that in some respects it is even better than the institution which bears his name at Oxford, because such series as it contains are more fully represented. Among the speakers after the delivery of the lecture, was Dr. E. B. Tylor, who gave a striking instance of the value of the principle on which the objects in the Pitt-Rivers Museum are arranged. It often happened, he said, that a series might be made purely theoretical, by putting in their order a number of specimens which referred to one another more or less distinctly, thus showing where the curve of development had probably passed; but yet important links were often wanting, and the visitor went away possessed with the desire to find those links and present them to the Museum. Only a few weeks ago they thus acquired a much-desired link in the history of stringed instruments. The late Mr. Carl Engel suggested that the strung bow must have been the origin of the whole series of stringed instruments, whether pianoforte, violin, or guitar. This view was proved to be correct when the instruments were arranged in a series, beginning with a strung bow. The difficulty, however, was to get the starting-point—an authentic bow capable of being used both for hunting and twanging. One people who were described as using the bow for this double purpose were the Damaras; it was said that the hunter shot game with his bow during the day, and when he came home sat by the fire and amused himself by twanging the string. Three or four weeks ago Miss Lloyd, who had spent some time in South Africa, sent them one of these bows, and it now stood at the head of the series of stringed instruments.

THE Indian Bureau of the U.S. Government propose to have at the Chicago Exposition an interesting exhibit, which will perhaps occupy two acres. Representatives of all the leading Indian tribes, especially those of a distinctive type, will be shown, together with their habitations, industries, &c. The Navajos will display their skill in blanket-weaving; the Zuñis, who will live in a "hogan," as they call their dwellings, will make pottery; the Piutes are to make water-bottles of rushes. There will be a great collection of relics, weapons, and utensils; and it is intended that competent teachers shall carry on their work in a model Indian school. Visitors will have ample opportunities of comparing the aborigines in their wild state with the civilized or semi-civilized Indians of to-day.

ACCORDING to official returns, lately reviewed by the *Adelaide Observer*, the area of land devoted in South Australia to gardens and orchards has advanced since 1885 from 10,775 acres to 15,362 acres, representing an increase of 50 per cent., this area apparently including that devoted to viticulture. The statistics show that the orange, almond, walnut, chestnut, and olive are largely cultivated. The number of almond trees is given as 134,038, or 27,768 more than last year; olive trees, 59,118, or 11,694 more; and orange trees, 56,341, the latter producing 44,762 cases of fruit, or 3040 more. The increase in the manufacture of olive oil is even more marked. The quantity made is returned at 6838 gallons, as against 1486 in the previous year. Almond trees are stated to have produced 3311 cwt. of nuts, being an increase of 1468 cwt. In 1890 walnut trees numbered 7644, and chestnut trees 1128 The climate and flora of South Australia are also well adapted to the needs of the bee-keeper. According to the rough estimates of the bee-owners, 25,383 hives in the colony last year produced nearly 500 tons of honey, of which 80,793 pounds were exported.

TOWARDS the end of last March the citizens of Sydney were astonished by the sudden discoloration of the water in Port Jackson. In the harbour the water presented in many places the appearance of blood. This remarkable phenomenon, which was soon found to be due to the presence of a minute organism, has been made the subject of a paper, by Mr. Thomas Whitelegge, in the Records of the Australian Museum (vol. i. No. 9). On March 31, Mr. Whitelegge went to Dawe's Point, and got a bottle of water, in which there was a good supply of the organism in question. At first he thought it was a species of the genus Peridiniidæ; but further research convinced him that it was a new species of the closely allied genus, Glenodinium. So far as Mr. Whitelegge is able to judge, fully one-half of the shore fauna must have been destroyed by these small invaders. The bivalves were almost exterminated in those localities where the organism was abundant during the whole of the visitation. Mr. Whitelegge is of opinion that the great destruction of life brought about by an organism apparently so insignificant is of the highest interest from a biological point of view, showing, as it does, how limited is our knowledge of the causes which influence marine food-supplies. This, he points out, is particularly the case in regard to the oyster, which has often mysteriously disappeared from localities where it formerly abounded.

THE U.S. Department of Agriculture has published the fifth number of a series of papers on the North American fauna. The number contains the results of a "biological reconnoissance" of a part of Idaho, which Dr. C. Hart Merriam conducted during August, September, and October 1890; and also descriptions of a new genus and several new species of North American mammals. The new genus (Microdipodops) is a kind of dwarf kangaroo rat from Nevada. Dr. Merriam speaks of this as one of the most remarkable of the many new and interesting mammals discovered in North America during the past few years. Six specimens were collected in Nevada by Mr. Bailey in October and November 1890.

IT is most important that members of the medical and scholastic professions, and the public generally, should have sound ideas on the best means for guarding great educational establishments from the outbreak and spread of preventible infectious and contagious disease. We are glad to note, therefore, that the code of rules on the subject, drawn up by the Medical Officers of Schools Association, has been so much in demand that it has been necessary for Messrs. J. and A. Churchill, the publishers, to issue a third edition. An important note to an appendix in which disinfection is dealt with has been added in this edition. The measures indicated in the appendix have hitherto been regarded as at least serviceable for the attainment of disinfection. Recent experiments, however, seem to show that none of them can be relied upon as absolutely effectual in certain cases.

AT the meeting of the Field Naturalists' Club of Victoria, on October 12, Mr. J. P. Eckert read a paper on "some peculiar changes in the colour of the flower of *Swainsonia procumbens*." When the flower opens, the corolla is lilac, and the first change is noticed in the longitudinal venules of the largest petals, which soon after assume a deep crimson. Then, at two different points of the petals a dark blue is noticed, which gradually extends over the whole surface, the peripheral portion being a little paler in colour. In the central portion the colour varies through all the shades of blue till finally it assumes a rosy tint. Frequently the petals will assume their original colour for some days, and afterwards go through all the gradations of colour once more. Mr. Eckert assigns the phenomena to a meteorological cause, and claims that his theory is supported by experiments with the electric current.

THE New York *Engineering News* says that prehistoric irrigation canals in Arizona are "really worthy of more notice than is usually given them. The Salt and Gila River valleys are intersected by a vast network of these canals, which antedate, at least, the arrival of Coronado in 1552, for he mentions these ruins and the traditions of the Indians regarding a once dense population in this region. Modern engineers cannot improve upon the lines of these canals, nor in the selection of points of diversion from the rivers. The first irrigation canal in this section, the one that has made Phœnix, with its present population of 20,000, simply followed the lines of one of these old canals. Their extent may be appreciated when it is said that in the Salt River valley alone the land covered by these canals once aggregated over 250,000 acres, and the canals themselves, with their laterals, must have exceeded 1000 miles in length. This country is filled with prehistoric ruins, with walls of stone or adobe, and almost every acre contains fragments of pottery, steel ornaments, stone implements, and other remains of a population which can only be estimated in its aggregate."

THE fifth part of "Bibliotheca Mathematica," edited by G. Eneström (Stockholm), is devoted to a bibliography of the history of the mathematical sciences in the Netherlands, by D. Bierens de Haan, of Leyden. By the conditions of the publication the writer is restricted "aux écrits se rapportant exclusivement ou au moins essentiellement à l'histoire des mathématiques pures." In ten octavo pages the list ranges from 1667 to the present time, and contains sixty entries, the compiler being credited with thirteen of them. There is also a long list of *éloges* on Dutch mathematicians.

WE have received from Mr. Elliot Stock the second volume of *The Field Club*. It is a magazine of general natural history, and cannot fail to give pleasure to readers who are interested in the results of scientific observation. The editor is the Rev. Theodore Wood.

THE new number of the *Economic Journal* (vol. i. No. 4) deals with various questions which are of great scientific interest as well as of urgent public importance. It opens with an introductory lecture on political economy, by Prof. F. Y. Edgeworth, the editor. Then come papers on the alleged differences in the wages of men and women, by Sidney Webb; the coal question, by Forster Brown; the new theory of interest, by W. Smart; the evolution of the Socialist programme in Germany, by Prof. G. Adler; labour troubles in New Zealand, by W. T. Charlewood; and an attempt to estimate the circulation of the rupee, by F. C. Harrison.

THE January number of *Mind*, the first of the new series, will contain articles by Mr. W. E. Johnson on "Symbolic Logic," by Mr. Alexander on the "Idea of Value," by Mr. McTaggart on the "Change of Method in Hegel's Dialectic," and by Prof. Lloyd Morgan on the "Law of Psychogenesis."

THE arrangements for science lectures at the Royal Victoria Hall during January are as follows:—January 12, Sir Herbert C. Perrott (Chief Secretary of the St. John's Ambulance Association), "First Aid to the Injured: its object, origin, and development" (this lecture will be followed by an ambulance class in the Morley Memorial College); 19, Mr. Locke Worthington, "Egypt 3000 years ago." On the 26th, Prof. Reinold will deliver a lecture.

TWO new methods of preparing free solid hydroxylamine, NH_2OH, are described by M. Crismer in the current number of the *Bulletin de la Société Chimique*. It will be remembered that this important substance was isolated a few weeks ago by M. Lobry de Bruyn; an account of the manner in which it was obtained, together with a description of the dangerous properties of the free base, was given in NATURE (p. 20). M. Crismer now publishes two very simple methods of isolating anhydrous hydroxylamine, by the use of a compound of hydroxylamine and zinc chloride, previously described by him (comp. NATURE, vol. xli. p. 401). This interesting compound is a crystalline substance, of the composition $ZnCl_2 . 2NH_2OH$, readily prepared in large quantity by dissolving the hydrochloride o hydroxylamine, NH_2OH . HCl, in alcohol, in a flask provided with an inverted condenser, adding the requisite quantity of zinc oxide, and boiling the liquid until it is quite clear. Upon cooling, crystals of the compound are deposited. These crystals are very permanent in their behaviour towards solvents; they resist organic solvents completely, and are only slightly attacked by water. They are rendered much more unstable by rise of temperature, and explode most violently when an attempt is made to fuse them. If, however, they are carefully warmed up to 120° in a flask connected with a series of U-tubes, they dissociate regularly, a gas being rapidly evolved, which condenses to a liquid, mainly in the first U-tube. This liquid is very rich in hydroxylamine, but owing to the dehydrating action of the zinc chloride, contains small quantities of decomposition products. This destructive action of the zinc chloride may be altogether avoided, however, if another base capable of replacing the hydroxylamine in the compound is present during the distillation. The base which M. Crismer finds most effective is aniline. About ten grams of the zinc salt are added to twenty cubic centimetres of freshly distilled aniline, and the mixture is submitted to distillation under reduced pressure. Under these circumstances a liquid of very high refractive power distils over. In a few minutes this liquid commences to crystallize in large colourless lamellæ, and upon surrounding the receiver with ice-cold water the whole completely crystallizes. These crystals, when washed with a little ether to remove a trace of aniline which is mechanically carried over, are found to correspond to the formula NH_2OH. They are identical in all respects with those described by M. Lobry de Bruyn. They dissolve in all proportions in water, and the solution possesses the ordinary properties of aqueous hydroxylamine. The crystals are very deliquescent, attracting moisture with the utmost avidity. They melt at the temperature of the hand. The compound of zinc chloride and aniline, which remains in the distillation flask, may be obtained from solution in boiling alcohol in minute snow-white crystals.

The second method by which anhydrous hydroxylamine may be prepared consists in passing dry ammonia gas into an emulsion of the zinc compound $ZnCl_2 . 2NH_2OH$ in absolute ether. As soon as the first bubbles of ammonia enter the flask an energetic reaction occurs, the zinc salt swells up rapidly, and eventually the whole of the hydroxylamine is liberated, and is dissolved by the ether. The clear ethereal solution is subsequently decanted, and the ether removed by distillation *in vacuo*, when white crystals of hydroxylamine remain in the vessel in which the distillation is carried out. The only precaution necessary in adopting this mode of preparation is to employ a tolerably large proportion of ether, as hydroxylamine does not dissolve in that liquid to a very large extent. M. Crismer finds it most convenient to perform the experiment in an apparatus so constructed that the extraction by ether of the product of the action of ammonia is continuous.

The additions to the Zoological Society's Gardens during the past week include three Rhesus Monkeys (*Macacus rhesus* ♂ ♂ ♀) from India, presented respectively by Dr. Hewetson, Mr. H. Godfrey, and Mr. W. A. Morgan; a Puma (*Felis concolor*) from Sante Fe, Argentine Republic, presented by Mr. Thos. Bowers; a Musanga Paradoxure (*Paradoxurus musanga*) from the Indian Archipelago, presented by Mr. J. Watson; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Mr. W. Needham; an Azara's Agouti (*Dasyprocta azaræ*) from British Guiana, presented by Mr. R. Scott-Brass; a Northern Mocking Bird (*Mimus polyglottus*) from North America, presented by Major N. Gosselin; two Brown Hyænas (*Hyæna brunnea* ♂ ♀) from South Africa, a Two-toed Sloth (*Cholopus didactylus*) from Demerara, purchased.

OUR ASTRONOMICAL COLUMN.

Capture of Comets by Planets.—During the last two or three years several astronomers have studied the action of planets in changing the orbits of comets which pass near them, and a considerable amount of interest has been aroused in this problem. Prof. H. A. Newton, in the *American Journal of Science* for September and December, establishes a number of propositions relative to the perturbations by planets which lead to the annexation of comets. Some of the results obtained may be expressed as follows:—(1) If a comet passes in front of Jupiter, the kinetic energy of the comet is diminished; if it passes behind the planet, the kinetic energy of the comet is increased. (2) The greatest effect of perturbation of a planet moving in a circular orbit in shortening the periodic time of a comet originally moving in a parabola is obtained if the comet's original orbit actually intersects the planet's orbit at an angle of 45°, and if the comet is due first at the point of intersection, at the instant when the planet's distance therefrom is equal to the planet's distance from the sun multiplied by the ratio of the mass of the planet to the mass of the sun. (3) If in a given period of time 1,000,000,000 comets come in parabolic orbits nearer to the sun than Jupiter, 126 of them will have their orbits changed into ellipses with periodic times less than one-half that of Jupiter; 839 of them will have their orbits changed into ellipses with periodic times less than that of Jupiter; 1701 of them will have their orbits changed into ellipses with periodic times less than one and a half times that of Jupiter; and 2670 of them will have their orbits changed into ellipses with periodic times less than twice that of Jupiter. (4) Of the 839 comets which are reduced to have periodic times less than Jupiter's period, 203 will, after perturbation, have retrograde motions, and 639 will have direct motions. (5) Somewhat more than five times as many of these comets move in direct orbits inclined less than 30° to Jupiter's orbit as move in retrograde orbits inclined less than 30° to Jupiter's orbit. It may therefore be said that comets which are changed by the perturbing action of a planet from parabolic orbits of every possible inclination to the ecliptic into short period ellipses must, as a rule, move in orbits of moderate inclination, and with direct motions.

Law of Limiting Apertures.—The results of some interesting photometric experiments connected with the application of the law of limiting apertures to small object-glasses are given by Dr. E. J. Spitta in *Monthly Notices R.A.S.*, November 1891. The apertures of six object-glasses were reduced to one-half and one-quarter respectively, and the intensity of a point at the focus of each was then photometrically tested. The numbers obtained were in neither case proportional to the square of the linear aperture of the object-glass, and they indicated that the outer zones do not contribute as much to the intensity of the image at the focus as they should do theoretically. Some photometric observations by Dr. Müller, of Potsdam, also show that the brilliancy of the focal image is only very slightly affected by blotting out the outer parts of his object-glass; the observed and computed intensities being very discordant until the diameter had been diminished to about one-half. Dr. Spitta believes that the cause of the difference lies in the aplanatisation of the glasses used.

Connaissance des Temps for 1893, and the extract from the one for 1892, containing information useful for mariners, have just been received from the Bureau des Longitudes. The arrangement appears to be the same as usual, and no comment as to its excellence is needed.

ORTHOGRAPHY OF GEOGRAPHICAL NAMES.

The Council of the Royal Geographical Society have just issued a circular letter, signed by Sir M. E. Grant Duff, the President of the Society, on this important subject. The following are its principal passages:—

In 1885 the Council, impressed with the necessity of endeavouring to reduce the confusion existing in British maps with regard to the spelling of geographical names, in consequence of the variety of systems of orthography used by travellers and others to represent the sound of native place-names in different parts of the world, formally adopted the general principle which had been long used by many, and the recognition of which had been steadily gaining ground, viz. that in writing geographical native names vowels should have their Italian significance, and consonants that which they have in the English language.

This broad principle required elucidation in its details, and a system based upon it was consequently drawn up with the intention of representing the principal syllabic sounds. The object aimed at was to provide a system which should be simple enough for any educated person to master with the minimum of trouble, and which at the same time would afford an approximation to the sound of a place-name such as a native might recognize. No attempt was made to represent the numberless delicate inflexions of sound and tone which belong to every language, often to different dialects of the same language. For it was felt not only that such a task would be impossible, but that an attempt to provide for such niceties would defeat the object.

The adoption by others of the system thus settled has been more general than the Council ventured to hope. The charts and maps issued by the Admiralty and War Office have been, since 1885, compiled and extensively revised in accordance with it. The Foreign and Colonial Offices have accepted it, and the latter has communicated with the colonies, requesting them to carry it out in respect to names of native origin. Even more important, however, than these adhesions is the recent action of the Government of the United States of America, which, after an exhaustive inquiry, has adopted a system in close conformity with that of the Royal Geographical Society, and has directed that the spelling of all names in their vast territories should, in cases where the orthography is at present doubtful, be settled authoritatively by a committee appointed for the purpose. The two great English-speaking nations are thus working in harmony.

The Council, by printing the rules in "Hints to Travellers," and by other means, have endeavoured to insure that all travellers connected with the Society should be made aware of them; but as it is possible that some bodies and persons interested in the question may still be in ignorance of their existence and general acceptance, they feel that the time has come to again publish them as widely as possible, and to take every means in their power to aid the progress of the reform.

To this end, and with a view to still closer uniformity in geographical nomenclature in revisions of editions of published maps, a gigantic task requiring many years to carry out, the Council have decided to take steps to commence tentatively indexes of a few regions, in which the place-names will be recorded in the accepted form.

RULES.

The rules referred to are as follows:—

1. No change is made in the orthography of foreign names in countries which use Roman letters: thus Spanish, Portuguese, Dutch, &c., names will be spelt as by the respective nations.

2. Neither is change made in the spelling of such names in languages which are not written in Roman character as have become by long usage familiar to English readers: thus Calcutta, Cutch, Celebes, Mecca, &c., will be retained in their present form.

3. The true sound of the word as locally pronounced will be taken as the basis of the spelling.

4. An approximation, however, to the sound is alone aimed at. A system which would attempt to represent the more delicate inflexions of sound and accent would be so complicated as only to defeat itself. Those who desire a more accurate pronunciation of the written name must learn it on the spot by a study of local accent and peculiarities.

5. *The broad features of the system* are:—

(*a*) That vowels are pronounced as in Italian and consonants as in English.

(*b*) Every letter is pronounced, and no redundant letters are introduced. When two vowels come together, each one is sounded, though the result, when spoken quickly, is sometimes scarcely to be distinguished from a single sound, as in *ai*, *au*, *ei*.

(*c*) One accent only is used, the acute, to denote the syllable on which stress is laid. This is very important, as the sounds of many names are entirely altered by the misplacement of this "stress."

6. Indian names are accepted as spelt in Hunter's "Gazetteer of India," 1881.

The following amplification of these rules explains their application:—

Letters.	Pronunciation and remarks.	Examples.
a	*ah*, *a* as in *father*	Java, Banána, Somáli, Bari.
e	*eh*, *a* as in *fate*	Tel-el-Kebir, Oléleh, Yezo, Medina, Levúka, Peru.
i	English *e*; *i* as in *ravine*; the sound of *ee* in *beet* ... ... Thus, not *Feejee*, but	Fiji, Hindi.
o	*o* as in *mote*	Tokyo.
u	long *u* as in *flute*; the sound of *oo* in *boot*. *oo* or *ou* should never be employed for this sound ... ... Thus, not *Zooloo*, but	Zulu, Sumatra.
	All vowels are shortened in sound by doubling the following consonant	Yarra, Tanna, Mecca, Jidda, Bonny.[1]
	Doubling of a vowel is only necessary where there is a distinct repetition of the single sound	Nuulúa, Oosima.
ai	English *i* as in *ice*	Shanghai.
au	*ow* as in *how* ... Thus, not *Foochow*, but	Fuchau.
ao	is slightly different from above	Macao.
aw	as in *law*.	
ei	is the sound of the two Italian vowels, but is frequently slurred over, when it is scarcely to be distinguished from *ey* in the English *they*...	Beirút, Beilúl.
b	English *b*.	
c	is always soft, but is so nearly the sound of *s* that it should be seldom used ... ... If *Celébes* were not already recognized it would be written *Selébes*.	Celébes.
ch	is always soft as in *church*	Chingchin.
d	English *d*.	
f	English *f*. *ph* should not be used for the sound of *f* ... ...Thus, not *Haiphong*, but	Haifong, Nafa.
g	is always hard. (Soft *g* is given by *j*)	Galápagos.
h	is always pronounced when inserted.	
hw	as in *what*; better rendered by *hw* than *wh*, or *h* followed by a vowel, thus, *Hwang ho*, not *Whang ho*, or *Hoang ho*	Hwang ho, Ngan hwi.
j	English *j*. *Dj* should never be put for this sound	Japan, Jinchuen.
k	English *k*. It should always be put for the hard *c* ... ... Thus, not *Corea*, but	Korea.
kh	The Oriental guttural	Khan.
gh	is another guttural, as in the Turkish	Dagh, Ghazi.
l, m, n	As in English.	
ng	has two separate sounds, the one hard as in the English word *finger*, the other as in *singer*. As these two sounds are rarely employed in the same locality, no attempt is made to distinguish between them.	
p	As in English.	
ph	As in *loophole*	Chemulpho, Mokpho.
th	stands both for its sound in *thing*, and as in *this*. The former is most common.	Bethlehem.
q	should never be employed; *qu* (in *quiver*) is given as *kw* ... ... When *qu* has the sound of *k* as in *quoit*, it should be given by *k*.	Kwangtung.
r, s, sh, t, v, w, x	As in English.	Sawákin.
y	is always a consonant, as in *yard*, and therefore should never be used as a terminal, *i* or *e* being substituted as the sound may require. Thus, not *Mikindány*, but not *Kwaly*, but	Mikindáni. Kwale.
z	English *z*	Zulu.
zh	The French *j*, or as *s* in *treasure*	Muzhdaha.
	Accents should not generally be used, but where there is a very decided emphatic syllable or stress, which affects the sound of the word, it should be marked by an *acute* accent	Tongatábu, Galápagos, Paláwan, Saráwak.

[1] The *y* is retained as a terminal in this word under Rule 2 above. The word is given as a familiar example of the alteration in sound caused by the second consonant.

THE ECLIPSE OF JANUARY 1, 1889.[1]

DURING the year 1889 two total eclipses occurred, one on January 1, and the other on December 21. The present report refers to the former, and contains a detailed account of all the work that was undertaken by those who formed the party from the Washington University Observatory. Owing to the proximity of this Observatory to the path of totality, the chance of observing the eclipse was made the most of, but was only made possible, as Prof. Pritchett says, "by the kindness of Government officials and others in lending instruments, and by the liberality of friends of the University in subscribing money for the necessary expenses."

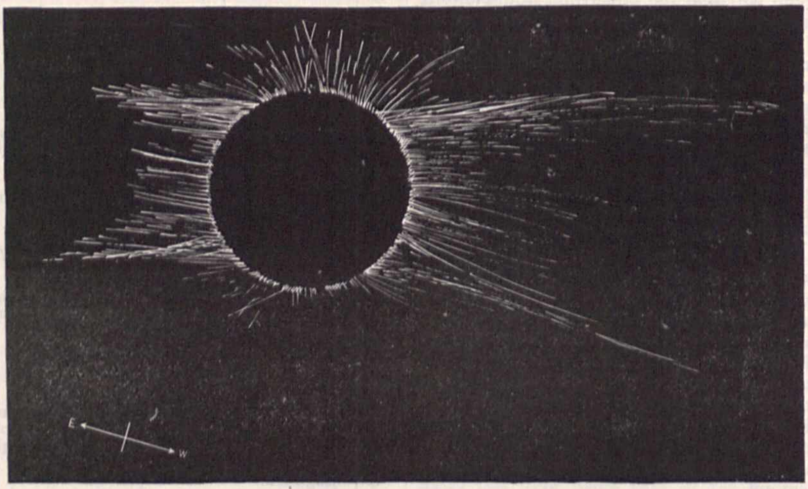


FIG. 1.—Prof. Engler's sketch of corona, 1889.

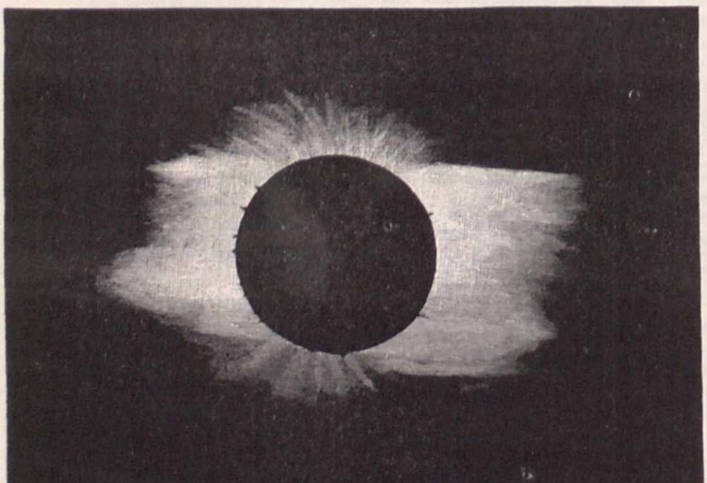


FIG. 2.—Composite photograph of corona, 1889.

The party consisted of Profs. H. S. Pritchett, Director of the Observatory, F. E. Nipher, and E. A. Engler, together with Prof. C. M. Charroppin, of the St. Louis University Observatory, and Prof. Señor Valle, of the National Observatory of Mexico.

The spot fixed upon for observing the eclipse was a small hamlet called Norman, on the north branch of the Southern Pacific Railway, about 130 miles from San Francisco. Situated south was Mount Shasta, and so nearly was it in the meridian that, as Prof. Pritchett says, "its snow-capped cone was used for instrumental adjustment without appreciable error."

The equipment as regards instruments included an equatorial camera, with one of Dallmeyer's patent portrait and group lenses, size No. 8.D., having a clear aperture of 6·0 inches, and an equivalent focal length of 37·9 inches. Owing to the difficulty of keeping the tube light-tight, to prevent the fogging of the plates, an automatic shutter had to be used, the largest obtainable cutting the aperture down to 4·5 inches. Two telescopes were also employed—one being a 4-inch Clark's refractor equatorially mounted, with magnifying powers from 50 to 400; the other a French instrument with an aperture of 3 inches, and an altitude and azimuth mounting.

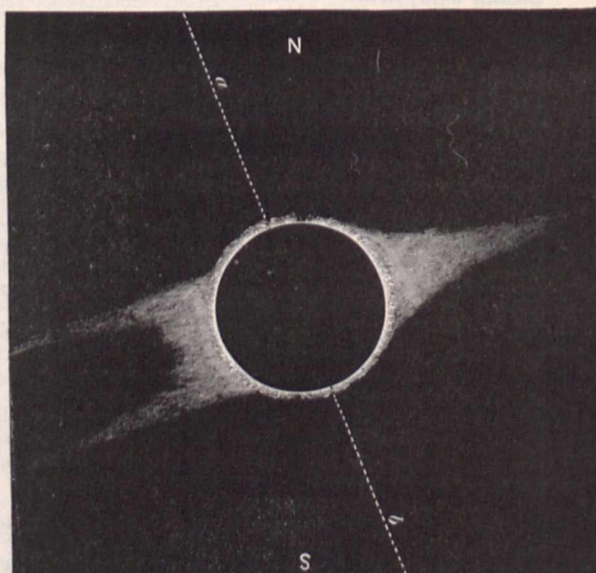


FIG. 3.—Lockyer's sketch of corona, 1878.

Although the first contact was lost through the formation of a heavy bank of clouds, the sky near the sun soon afterwards became clear, and "the seeing was excellent and the image of the sun was sharp and distinct." In all the six negatives

[1] Report of the Washington University Eclipse Party.

obtained the definition was found to be very good, but on account of the difference in the lengths of the exposures given to the several plates, some of them proved to be rather thin. The task of developing them was imposed upon Prof. Charroppin, who gives a brief, interesting account of the process of their development. The observations of the times of contact and the study of the corona were undertaken by Prof. Engler, with the aid of the French instrument. Although only two contacts were recorded (the second and the fourth), he made no attempt at the third, owing to the short space of time at his disposal for observing and sketching the corona. The drawing which he made is produced (together with the other photographs) in this report, and tallies, when compared with them, in nearly all respects, with the exception of the two equatorial streamers on the west side, that appear to extend further westward than those recorded in the photographs.

It is interesting to note in the illustrations the great similarity between the corona observed in this eclipse and that of the year 1878, in which year the sun-spot disturbances were at the minimum. Figs. 1 and 2 represent the corona of the year 1889, the former being a drawing by Prof. Engler, the same as the one previously mentioned, while the latter is the integrated result of the examination of all the photographic plates, and "does not represent the corona as seen by the eye, nor as shown on any one of the negatives, but is a combination of all that could be found in the negatives."

The next illustration (Fig. 3) is a sketch of the corona made by Mr. Lockyer during the eclipse of 1878 (NATURE, vol. xviii. p. 457), and when compared with the above figures fully bears out the idea that at the several periods throughout a sun-spot cycle there corresponds in the corona a like period, which is apparent to us only in the changes of form undergone by the equatorial and polar streamers, and this only at the time of eclipses.

The following brief extracts, which we give in the observers' own words, will show how the descriptions of the appearance of the corona resembled one another in all the main points. With regard to the structure at the north and south poles, Mr. Lockyer says (NATURE, vol. xviii. p. 457):—

"I had a magnificent view of the corona with a power of 50 on my 3¾-inch Cooke, and saw exquisite structure at the north and south points. Curves of contrary flexure started thence, and turned over, and blended with the rest of the corona, which was entirely structureless and cloudlike; the filamentous tracery, which in India I observed till three minutes after totality, had indeed almost gone. Prof. Bass, however, tells me that by confining his attention to the same point for nearly the whole time of totality, the structure came out and seemed to pulsate like an aurora."

Prof. Pritchett's account is almost the same, word for word, as may be seen from the following extract:—

"I was particularly struck," he says, "with the brilliant appearance in the telescope of the filaments at the north and south limbs of the sun. They seemed radial at the poles, but gradually bending over and merging into the equatorial streamers in passing from the pole to the equator. I could not resist the impression that these filaments pulsated."

From the above it will be seen that the appearances at the pole for both years were very similar; and with regard to the equatorial streamers also, their notes show that the characteristic features of each coincided in almost all particulars. Owing to the fact of the minimum spot period occurring at both eclipses, the above results strengthen very considerably the hypothesis connecting the spot cycle with the corona.

In addition to Prof. Engler's drawing mentioned above, Señor Valle also made some eye observations of the coronal streamers. The method he adopted was similar to that employed by Prof. Newton in 1878, and consisted in placing a screen in such a position that during totality the moon and the brighter corona were cut off. The photo-engraving of the drawing shows an extension of the equatorial streamers to about a distance of three solar diameters, while the polar regions were described as of a curved luminous filamentary contraction.

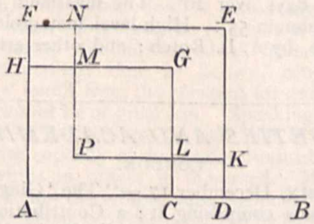
Before concluding, we must not forget to mention the admirable artotype reproductions, at the end of the report, of all the negatives: as they are arranged in the order of the times of exposure, they show well the progressive increase of detail on the outer part of the corona as the exposure was lengthened.

W.

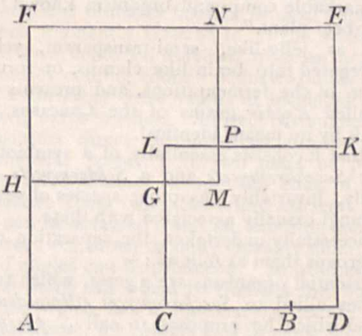
SIMPLE PROOF OF EUCLID II. 9 AND 10.

THE following proof of Euclid II., 9 and 10, believed to be new, due to Miss Hilda Hudson, was communicated to the London Mathematical Society at their last meeting.

AB is bisected in C and divided unequally in D either internally (II. 9) or externally (II. 10). It is required to prove that the sum of the squares on AD and DB is equal to twice the sum of the squares on AC and CD.



II. 9.



II. 10.

On AD, AC, CD, describe the squares ADEF, ACGH, CDKL, all on the same side of AB.

On FH, within the square AE, describe the square FHMN; this is equal to the square on CD.

Let NM, KL, produced if necessary, meet in P.

Then PE is a square equal to the square on AC.

And PG is a square equal to the square on DB.

The sum of the squares on AD, DB is equal to the figures AE and PG, that is to AG, PE, FM, and CK, that is to twice the sum of AG, CK, that is to twice the sum of the squares on AC, CD.

SCIENTIFIC SERIALS.

American Meteorological Journal for November.—Prof. H. A. Hazen gives the results of three rather high balloon voyages in the United States, in which he took part. (1) June 25, 1886, at 7h. 50m. a.m., a dense cloud was entered at 1000 feet, which seemed like a dry fog. The temperature from the earth up to more than half-way through the cloud hardly varied a degree, but after that it rose rapidly. There was a region of marked dampness at 7000 feet. The temperature at starting was 61°·3, and at 9640 feet it had fallen to 8°; time, 9h. 16m. (2) June 11, 1887, at 2h. 34m. p.m., temperature 90°·6. At 15,080 feet, it had fallen to 40°; time, 6h. 18m. p.m. Great dryness was experienced in the upper strata. There were two rather sharply defined layers of dampness, at 7500 feet and at 12,000 feet. (3) August 13, 1887, at 3h. 35m. p.m., temperature 75°·8. At 6940 feet it was 53°·3; time, 6h. 28m. p.m. The relative humidity fell to 8 per cent.—Meteorology at the French Association at Marseilles, by A. L. Rotch. Among the most interesting papers was one by M. Crova, upon the analysis of diffused light. Observations made at Montpellier at the zenith show the blue to be greatest in the early morning, and least about 2 p.m., and then increasing until towards evening. A cloudy sky also shows a considerable amount of blue rays. M. Teisserenc de Bort explained the existence of a vertical barometric gradient, first noticed in mountain observations, but lately measured more exactly on the Eiffel Tower.—The zodiacal light as related to terrestrial temperature observations, by O. T. Sherman.—Features of Hawaiian climate, by C. L. Lyons, in charge of the Weather Service there. The temperature averages for January are 69° to 71°, and in July and August 78° and 79°. The daily range is greater than is generally supposed, averaging 11° for the year, and some days over 20°. The maximum temperature is 89°, and the minimum 55°.—High-level meteorological observatories in France, by A. L. Rotch; and other articles of minor importance.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 17.—"The 'Ginger-beer Plant, and the Organisms composing it: a Contribution to the Study of Fermentation-yeasts and Bacteria." By H. Marshall Ward, Sc.D., F.R.S., F.L.S., Professor of Botany at the Forest School, Royal Indian Engineering College, Cooper's Hill.

The author has been engaged for some time in the investigation of a remarkable compound organism known to villagers as the "ginger-beer plant."

It occurs as jelly-like, semi-transparent, yellowish-white masses, aggregated into brain-like clumps, or forming deposits at the bottom of the fermentations, and presents resemblances to the so-called *Kephir* grains of the Caucasus, with which, however, it is by no means identical.

He finds that it consists essentially of a symbiotic association of a specific *Saccharomycete* and a *Schizomycete*, but, as met with naturally, invariably has other species of yeasts, bacteria, and mould-fungi casually associated with these.

He has successfully undertaken the separation of the various forms, and groups them as follows:—

(1) The essential organisms are a yeast, which turns out to be a new species allied to *Saccharomyces ellipsoideus* (Reess and Hansen), and which he proposes to call *S. pyriformis*; and a bacterium, also new and of a new type, and named by him *Bacterium vermiforme*.

(2) Two other forms were met with in all the specimens (from various parts of the country and from America) examined—*Mycoderma cerevisiæ* (Desm.) and *Bacterium aceti* (Kützing and Zopf).

(3) As foreign intruders, more or less commonly occurring in the various specimens examined, were the following:—

α. A pink or rosy yeast-like form—*Cryptococcus glutinis* (Fresenius)?

β. A small white aërobian top-yeast, with peculiar characters, and not identified with any known form.

γ. The ordinary beer-yeast—*Saccharomyces cerevisiæ* (Meyen and Hansen).

δ. Three, or probably four, unknown yeasts of rare occurrence.

ε. A bacillus which forms spores, and liquefies gelatine with a greenish tinge.

ζ. A large spore-forming bacillus, which also liquefies gelatine.

η and θ. Two—perhaps three—other Schizomycetes not identified.

ι. A large yeast-like form which grows into a mycelium, and turns out to be *Oïdium lactis* (Fresenius).

κ. A common blue mould—*Penicillium glaucum* (Link).

λ. A brown "Torula"-like form, which turns out to be *Dematium pullulans* (De Bary).

μ. One, or perhaps several, species of "Torula" of unknown origin and fates.

Saccharomyces pyriformis (n. sp.) is a remarkably anaërobian bottom-yeast, forming spores, and developing large quantities of carbon dioxide, but forming little alcohol. It has also an aërobian form—veil form of Hansen—in which the rounded cells grow out into club-shaped or pyriform cells, whence the proposed specific name. It inverts cane sugar, and ferments the products; but it is unable to ferment milk sugar. It forms rounded, morula-like, white colonies in gelatine, and the author has separated pure cultures from these. He has also studied the development and germination of the spores, which are formed in 24 to 48 hours at suitable temperatures on porous earthenware blocks. They also develop on gelatine.

The specific Schizomycete (*Bacterium vermiforme*, n. sp.) has been very fully studied by the author. It occurs in the fermentations as rodlets or filaments, curved or straight, encased in a remarkably thick, firm, gelatinous sheath, and is pronouncedly anaërobic, so much so, that the best results are got by cultivating it in carbon dioxide under pressure.

The sheathed filaments are so like worms, that the name proposed for the species is appropriately derived from this character.

It will not grow on gelatine, and separation cultures had to be made in saccharine liquids by the dilution methods.

It grows best in solutions of beet-root, or of cane sugar, with relatively large quantities of nitrogenous organic matter—*e.g.* bouillon, asparagin—and tartaric acid. Good results were obtained with mixtures of Pasteur's solution and bouillon.

The author found that the bacterium into which the filaments subsequently break up can escape from its sheath and become free, in which state it divides rapidly, like ordinary bacteria. Eventually, all the forms—filaments, long rods, short rodlets—break up into cocci. No spores have been observed. These changes are dependent especially on the nutritive medium, but are also affected by the gaseous environment and the temperature. The jelly-like clumps of the so-called "ginger-beer plant" are essentially composed of these sheathed and coiled Schizomycetes, entangling the cells of *Saccharomyces pyriformis*. But the fermentative actions of the Schizomycete on the saccharine medium are different when alone, from those exercised when associated with the yeast, or from those exerted by the latter alone.

This was proved by cultivating each separately, and also by cultivations in which, while each organism was submerged in the same fermentable medium, they were separated by permeable porcelain (Chamberland filters), through which neither could pass.

The author has also reconstructed the "ginger-beer plant" by mixing pure cultures of the above two organisms; the Schizomycete entangled the yeast-cells in its gelatinous coils, and the synthesized compound organism behaved as the specimens not analyzed into their constituents. The symbiotic compound organism so closely resembles a lichen, in its morphological aspects, that it may be said to be a ferment-lichen.

Some very curious phenomena in connection with the formation of the gelatinous sheaths and the escape of the bacteria from them were observed in hanging-drop-cultures, and are figured and described by the author. The conditions for the development of the gelatinous sheaths—and therefore of the coherent brain-like masses of the Schizomycete—are a saccharine acid medium and absence of oxygen. The process occurs best in carbon dioxide: it is suppressed in bouillon, and in neutral solutions in hydrogen, though the organism grows in the free, non-sheathed, motile form under these conditions.

The behaviour of pure cultures of the bacteria in as complete a vacuum as could be produced by a good mercury pump, worked daily, and even several times a day, for several weeks, is also noteworthy. The development of the sheaths is apparently indefinitely postponed *in vacuo*, but the organism increased, and each time the pump was set going an appreciable quantity of carbon dioxide was obtained. In vacuum tubes the same gas was evolved, and eventually attained a pressure sufficient to burst some of the tubes. The quantity of carbon dioxide evolved daily by the action of the bacterium alone, however, is small compared with that disengaged when the organism is working in concert with the symbiotic yeast; in the latter case the pressure of the gas became so dangerous that the author had to abandon the use of sealed tubes.

The products of the fermentation due to the Schizomycete have not yet been fully determined in detail; lactic acid, or some allied compound, seems to be the chief result, but there are probably other bodies as well. The author considers that the bacterium removes from the sphere of action substances which, if accumulated, would exhibit the fermentative power of the yeast, hence the advantages of the symbiosis.

The pink yeast-like form proved to be very interesting. It has nothing to do with the "ginger-beer plant" proper, though it was invariably met with as a foreign intruder in the specimens. The author identifies it with a form described by Hansen in 1879 ("Organismer i Öl og Ölurt," Copenhagen, 1879); unfortunately the original is in Danish, but the figures are so good that little doubt is entertained as to the identity. It is also probably the same as Fresenius's *Cryptococcus glutinis* in one of its forms. It is not a Saccharomycete, and does not ferment like a yeast; it is aërobian.

The chief discovery of interest was that in hanging drops the author traced the evolution of this "rose-yeast" into a large complex mycelium, bearing conidia, and so like some of the Basidiomycetes that it may almost certainly be regarded as a degraded or "torula" stage of one of these higher fungi. Full descriptions and figures are given by the author.

The form *Mycoderma cerevisiæ* was thoroughly examined. The author's results confirm what is known as to its aërobian characters. Statements as to its identity with *Oïdium lactis* were not only not confirmed, but the author grew these two forms side by side, and maintains their distinctness. Nor could he obtain spores in this fungus, thus failing to confirm earlier statements to the contrary. He regards it as probable that oil-drops have been mistaken for spores; he also finds that in later stages of fermentation by this organism a strong oily-smelling body is produced.

With regard to *Bacterium aceti*, the author has nothing new to add. A point of some interest was the repeated production of acetic ether, which scented the laboratory, when this Schizomycete was growing in company with the small white aërobian top-yeast referred to under (β). Full details regarding the rest of the organisms, which have nothing to do with the "ginger-beer plant" proper, are given in the original paper.

Physical Society, December 4.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—A paper on a permanent magnetic field was read by Mr. W. Hibbert. The author had noticed the approximate constancy of an "aged" bar magnet, and he obtained still greater constancy by attaching pole pieces to a bar magnet, of such a shape as to give a nearly closed circuit of small "magnetic resistance." The pattern now described consists of a steel rod 1 inch diameter and about 2½ inches long, with a cast-iron disk 4 inches diameter and ⅜ thick fixed at one end; the other end is fitted in a hemispherical iron shell which surrounds the bar and comes flush with the upper surface of the disk. An annular air space less than $\frac{1}{16}$ inch wide is left between the cylindrical surface of the disk and the inside of the shell, and when the bar is magnetized, a strong magnetic field exists in this space. To use this field for producing electro-magnetic impulses, a coil of wire is wound in a shallow groove on a brass tube which can slide axially through the annular space, thus cutting all the lines. The tube is allowed to fall by its own weight, a neat trigger arrangement being provided for effecting its release. The instrument exhibited had 90 turns of wire in the coil, and the total magnetic flux across the air space was about 30,000 C.G.S. lines. A large electro-magnetic impulse is, therefore, obtainable even through resistances as great as 10,000 ohms. Tests of three instruments show that there has been practically no magnetic decay in seven months. The author therefore considers them satisfactory, and is prepared to supply them as magnetic standards. To facilitate calculation, the number of lines will be adjusted to a convenient number, say 20,000 or 25,000. Several uses to which the instruments are well suited are mentioned in the paper, and a simple way of determining permeability by the magnetometer method is described. Mr. Blakesley thought the name given to the instrument was inappropriate, for it really gave a constant impulsive E.M.F. Dr. Sumpner said the constancy of the sensibility of d'Arsonval galvanometers was a measure of the constancy of magnets having nearly closed circuits. Such instruments, in use at the Central Institution, had remained unchanged for several years. Prof. S. P. Thompson admired Mr. Hibbert's instrument, and thought it would be very useful in laboratories. Standard cells, he said, were not always reliable, and condensers were the most unsatisfactory of electrical standards. On the subject of permanency of magnets, he said that Strouhal and Barus found that magnets with nearly closed circuits were most constant, and that, to give the best results, the hardness of the steel should be less the more closed the circuit. Mr. Hookham had also found that by using a nearly closed circuit, and reducing the strong magnetization by about 10 per cent., great constancy could be obtained. Some years ago he (Dr. Thompson) had tried the effect of ill-treatment on magnets, and observed that touching or hitting a magnet with non-magnetic material had little effect, whilst similar treatment with iron or magnets affected them considerably. Suddenly removing the keeper of a magnet tended to increase the magnetism, whilst putting a keeper on suddenly had the reverse effect. Strouhal and Barus had also investigated the temperature coefficient of magnets, and found that this might be reduced by subjecting the magnet to rapid changes of temperature after the first magnetization, and then remagnetizing. Mr. W. Watson inquired what was the percentage fall in strength of Mr. Hibbert's magnets. The bars used in magnetic surveys had been tested frequently, and they lost about 0·5 per cent. in 6 months. The President asked what were the temperature coefficients of the magnets described in the paper? Mr. Evershed, he said, thought it was between 0·01 per cent. and 0·05 per cent. for ordinary magnets. He thought the instrument shown by Mr. Hibbert would be of immense value if the magnet was really permanent. By it ballistic galvanometers could be readily calibrated, and, when combined with a resistance box, it could also be used as a standard for current; for, since the constant of a ballistic galvanometer for quantity can be determined from its constant for current, if the periodic time be known, conversely that for current can be found from the constant for quantity. In some instances this would be of great use. Speaking of the temperature coefficient of condensers, he said that in some cases the specific inductive capacity of dielectrics diminished with rise of temperature whilst in others it increased. Mr. Hibbert, in reply, said he found the temperature coefficient of his magnets to be, roughly, about 0·03 per cent., but he had not investigated the matter very carefully. In making his measurements no correction had been made for the variation of capacity of his condenser with temperature.—Mr. Walter Baily took the chair, and the President communicated a note on rotatory currents. The subject, he said, was probably familiar to most persons present, for it had been frequently referred to in the scientific papers. Alternate currents could be obtained from an ordinary direct current dynamo by making contact with two points in the armature, say by connecting these points to insulated rings on the shaft, and using extra brushes. A direct current motor similarly treated transforms direct currents into alternating currents, or into mechanical power. If two pairs of points in the armature be selected, situated at opposite ends of two perpendicular diameters, then two alternating currents differing in phase by 90° can be obtained; and by choosing suitable points in the armature, two, three, or more currents differing in phase by any desired angles can be produced. In ordinary motors the connections for doing this would be troublesome, but the Ayrton and Perry form, which has a stationary armature, lends itself readily to this purpose, for contact can be made with any part of the armature with great facility. A motor of this kind was exhibited, in which contact was made with four equidistant points on the armature. On connecting opposite points through fine platinum wires, and running the motor slowly, the wires glowed alternately, one being bright whilst the other was dark, and *vice versâ*, thus demonstrating the existence of two currents in quadrature. When the four points on the armature were joined to the four corners of a square of platinum wire, the wires became incandescent in succession, the glow appearing to travel round the square, and suggesting the idea of rotatory currents. A Tesla alternating current motor was also driven by two currents differing in phase by 90°, obtained from the armature of the Ayrton and Perry direct current motor above mentioned. The ease with which currents differing in phase by any amount can be obtained from such a motor led the author to investigate theoretically the case of two circuits connecting opposite ends of two diameters inclined at any angle, α. Calling the currents in these circuits at any instant, A_1 and A_2, he had found that

$$A_1 = 2nE_0 \frac{\sqrt{\left(r_2+\rho\frac{\pi}{2}\right)^2+\rho^2\left(\frac{\pi}{2}-\alpha\right)^2-2\left(r_2+\rho\frac{\pi}{2}\right)\rho\left(\frac{\pi}{2}-\alpha\right)\cos\alpha}}{\left(r_1+\rho\frac{\pi}{2}\right)\left(r_2+\rho\frac{\pi}{2}\right)-\rho^2\left(\frac{\pi}{2}-\alpha\right)^2}$$

multiplied by $\sin(pt+\phi)$;

where n = number of turns on armature per radian,
ρ = resistance of armature per radian,
r_1 = resistance of external circuit in which current A_1 passes,
r_2 = resistance of external circuit in which current A_2 passes,
E_0 = maximum E.M.F. per convolution
p = angular velocity of rotation, and

$$\tan\phi = \frac{\rho\left(\frac{\pi}{2}-\alpha\right)\sin\alpha}{r_2+\rho\frac{\pi}{2}-\rho\left(\frac{\pi}{2}-\alpha\right)\cos\alpha}.$$

A similar expression, in which r_1 is written for r_2, and r_2 for r_1, gives the value of A_2. The phase angle between the currents is given by

$$\tan(\phi+\psi)=\frac{\left(r_1+\rho\frac{\pi}{2}\right)\left(r_2+\rho\frac{\pi}{2}\right)-\rho^2\left(\frac{\pi}{2}-\alpha\right)^2}{\left(r_1+\rho\frac{\pi}{2}\right)\left(r_2+\rho\frac{\pi}{2}\right)+\rho^2\left(\frac{\pi}{2}-\alpha\right)^2-\frac{\rho\left(\frac{\pi}{2}-\alpha\right)\left(r_1+\rho\frac{\pi}{2}\right)}{\cos\alpha}}\cdot\tan\alpha$$

The expression for ϕ shows that the phase of the current in circuit A_1 is independent of the resistance r_1. On the other hand, varying r_2 alters ϕ. It was also pointed out that $\tan(\phi+\psi)$ is generally greater than $\tan\alpha$.—Prof. J. Perry, F.R.S., read a paper on struts and tie-rods laterally loaded. He pointed out that, in the case of struts, a slight want of straightness may considerably reduce the breaking load. Even if a strut be originally straight and the thrust properly distributed, its weight usually produces lateral loading and consequent bending. Similarly, centrifugal force produces lateral loading in connecting rods. For some years the author has given his students practical examples of struts and tie-rods to work out, taking into account the effect of lateral loads. The chief results obtained, together with a general treatment of the whole subject, are embodied in the paper. Where the curves of bending moment and the deflections due to lateral loading can be easily developed by Fourier's series, solutions can readily be found. Simple cases of uniformly loaded struts and tie-bars have been fully worked out, and also the case of locomotive coupling rods. In one problem on the latter subject, a rectangular cross-section was chosen, and the proportions of depth to breadth determined so as to make the rod equally strong in the two directions when running at various given speeds. With cranks 12 inches long, the results show that, at a speed of 390 revolutions per minute, the ratio of depth to breadth must be infinite, so as to give equal strength, so great is the influence of the lateral loading due to centrifugal force, when combined with the thrust. Horizontal tie rods loaded by their own weight have been investigated, and the tensions required to neutralize compression due to bending determined. A steel bar, 1 inch diameter and 48 inches long, was used as a strut, with a thrust of 1500 pounds. The maximum stress, due to bending by its own weight alone, was 810, and on applying the thrust the maximum stress was raised to 23,190, or about 26 times that due to lateral loading alone. More complex cases have also been treated, the results of which are given in the paper.

PARIS.

Academy of Sciences, December 14.—M. Duchartre in the chair.—On the distribution of prime numbers, by M. H. Poincaré.—On the fixation of nitrogen by arable soils, by MM. Arm. Gautier and R. Drouin. The conclusion is drawn that only soils containing organic matter fix the free or ammoniacal nitrogen of the atmosphere, even in the absence of plants, and that the organic matter existing in all arable soil is an indispensable intermediary in this fixation of nitrogen.—On the camphoric and isocamphoric esters, and the constitution of the camphoric acids, by M. C. Friedel.—Remarks on the history of supersaturation, by M. Lecoq de Boisbaudran. The author gives some notes, made by him in 1866, on the subject of supersaturation, which are in agreement with the phenomena of solution observed in recent years.—Observations of Borrelly's asteroid (Marseilles, November 27, 1891), made at Paris with the East Tower equatorial, by Mdlle. Klumpke. Observations for position were made on November 30, December 2 and 5.—On integrals of the second degree in problems of mechanics, by M. R. Liouville.—On a class of *congruences* of lines, by M. A. Petot.—On the actual state of geodetic and topographic works in Russia, by General Venukoff.—A brief note on the maps of Russia, prepared under the direction of General Kowersky.—On circular polarization, by M. E. Carvallo.—On a thermo-electric standard of electromotive force, by M. Henri Bagard. The author has experimented with thermo-electrolytic couples consisting of two liquids, one an amalgam of zinc, containing a known proportion of this metal, and the other a solution of sulphate of zinc. He finds that such a couple is absolutely constant between two given temperatures, its electromotive force between 0° and t° being given by the formula—

$$E_0^t = 0{\cdot}001077t + 0{\cdot}00000090t^2.$$

And it is not necessary to exercise any great precision in the determination of the weight of zinc dissolved in the known weight of mercury to form the amalgam, for the variation of the electromotive force when the couple is at the temperatures 0° and 100° appears to be only 0·0001 volt when the proportion of zinc was varied from 0·00025 to 0·00075 the mass of the mercury.—The three basicities of phosphoric acid, by M. Daniel Berthelot. The basicities have been investigated by the author using a method of determining the electric conductibilities of phosphoric acid solution, and of the same with varying quantities of soda, potash, or ammonia respectively added. The conclusion is drawn that monobasic and bibasic phosphates are stable even in dilute solution, and that the tribasic alkaline phosphates are nearly completely dissociated in dilute solution. Phosphoric acid differs completely from the true tribasic acids as the monobasic and bibasic salts of the latter are partially dissociated by water, and the tribasic salts, on the contrary, are stable in solution.—Salts in solution, sodium sulphate and strontium chloride, by M. A. Etard.—A green solid chromic sulphate, by M. A. Recoura. It has the formula $Cr_2(SO_4)_3,11H_2O$. — Bismuthic acid, by M. G. André.—On the distillation of oil, by M. Pierre Mahler.—A new porcelain, asbestos porcelain, by M. F. Garros.—On the presence of reticulated tissue in the muscular walls of the intestines, by M. de Bruyne.—On the first phases in the development of *Crustacea edriophthalma*, by M. Louis Roule.—On *Gymnorhynchus reptans*, Rud., and its migration, by M. R. Moniez.—On the *rôle* of the foot as a prehensile organ in Hindoos, by M. Felix Regnault. Many travellers have remarked on the ability possessed by most Hindoos of using the foot as well as the hand in work of all descriptions. M. Regnault has made some measurements of the lengths of the feet and toes of a number of natives, and draws some conclusions therefrom as to the adaptation "of the organ to the function."—On the discovery of Tertiary shells in the volcanic tufa of Limburg (Grand Duchy of Baden), by M. Bleicher.—The circulation of winds on the surface of the earth: fundamental principles of the new theory, by M. Duponchel.

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