THURSDAY, MARCH 17, 1892.

MAN IN NATURE.

L'Homme dans la Nature. Par Paul Topinard. (Paris: Baillière et Cie., 1891.)

IT is with much pleasure that we announce the appearance of a new work from the pen of Dr. Paul Topinard, formerly General Secretary of the Anthropological Society of Paris, and Professor in the École d'Anthropologie. Dr. Topinard is well known in this country as one of the most eminent, if not the most eminent, physical anthropologist in France at the present day, and it is with much regret that anthropologists here have observed the shameful way he has been treated by a faction of the Anthropological Society of Paris, who have done their best to diminish his usefulness and retard the advancement of true anthropological science in Paris. Notwithstanding the troubles and anxieties he has gone through—to which we would not have alluded here had they not been public property, and discussed in French scientific journals—it is gratifying to find that Dr. Topinard has pursued the even tenor of his way, and been able to enrich anthropological science by another of his valuable works, forming the seventy-third volume of the "International Scientific Series."

The work under review is divided into twenty-two chapters. The first contains, in addition to a statement of the scope of the book, a short but interesting history of the development of anthropological science. In the second chapter, its nature, its proper limits, and its relation to biology, ethnology, psychology, and sociology, are pointed out. The term "anthropology" is restricted to the study of man as an animal and a member of a group in the zoological series, in conformity with the acceptation in which it is used by Blumenbach, Broca, Quatrefages, and others. In the third chapter, the various subjects of study included in anthropology, as above restricted, are set forth, and the general principles of zoology on which the distribution of animals in groups of different values rests, the choice of characters on which they are founded, and the differences between race, species, family, and order are indicated.

The methods employed in anthropological research are considered in the fifth chapter. These may be briefly stated as descriptive and anthropometric. As descriptive terms are liable to vary very considerably according to the ideas of different observers, their value has hitherto been much less definite than characters based upon measurement. For some time past the author has been endeavouring to elaborate a system of observation which will render descriptive characters more uniform and trustworthy than heretofore. Having had some practical experience of the plan advocated by Dr. Topinard, we may state that we have been favourably impressed with it, not only for producing more uniform results, but also for saving labour to the observer, facilitating the analyses of observations, and for classifying the latter according to their type.

The character of the hair is dealt with in the sixth chapter. The hair is shown to furnish us with characters which are of importance not only in distinguishing races, but also in comparing man with the animals most nearly allied to him. The author shows that, in respect to their hair, the negro races differ most from the monkeys, while the white races most nearly resemble them, the yellow races being intermediate. In the straight-haired races the hair corresponding to the long coarse outer fur of mammals displaces the woolly hair—the homologue of the woolly under-fur; while in the woolly-haired races the reverse takes place. These characters of the hair would have formed an impassable gulf between the yellow and negro races had not some intermediate forms fortunately been left. The abundant generalization of fur as *lanugo* in the fœtus would go to prove that man descended from a furred progenitor.

The value of statistical maps is discussed in the seventh chapter, and as an example of their use in tracing race-characters, the distribution of the blonde and dark types in France has been taken and illustrated. It is shown, however, that the combination of the descriptive and anthropometric methods gives the most trustworthy results in determining the natural types of man. The latter method is discussed in chapter viii., and the use of indices, projections, seriations, graphic curves, averages, &c., are also explained.

The ninth chapter deals with measurements of the skeleton and the living body, and how these should be made by travellers and others. The directions given for the measurement of the long bones by ascertaining their maximum length is undoubtedly the best method. The proportions which these bones bear to the height of the skeleton (the latter being taken as 100) he gives as follows: humerus 20·0, radius 14·3, femur 27·3, tibia 22·1. To get the stature of the subject when alive, he adds 35 mm. to the height of the skeleton obtained from the detached bones by the above formula. The proportions just mentioned correspond very closely to those of the second series of French skeletons given in the "Éléments d'Anthropologie," and also with Rollet's important observations. Our own observations on subjects which have been carefully measured before dissection, go to support those of Rollet, and to show that the formulæ above given indicate the actual stature as nearly as possible, without adding 35 mm., as Dr. Topinard does. This is probably due to difference in mounting the skeletons—a fertile source of error. We are of the opinion that where it is not possible to get measurements of subjects before dissection in sufficient numbers to establish the formula for the race, it is more trustworthy to do so from the mean stature of the living, than from the height of articulated skeletons. The directions given for measuring the living body and tabulating its descriptive characters, are specially arranged for the purpose of encouraging travellers to undertake anthropometric observations. A model schedule of observations is given, arranged so that the various questions may be answered by a figure or a measurement, which should prove valuable to travellers. The system of measuring the body aimed at in it is to obtain the relative proportions of the several parts, rather than absolute anatomical dimensions; in other words, the author's idea is that it is better that travellers who are not anthropologists, and consequently not trained to anthropological research, should confine their attention to obtaining such measurements as will enable the canon of proportion of the several parts of the body to be made out, and compared in different races to that which obtains in Europeans, rather than attempt to do work which requires previous anatomical knowledge. The measurements are divided into two classes, according to their importance, but all of them are such as any intelligent traveller, with a little practice, should be able to make with accuracy.

Chapters x. and xi. are devoted to the study of the skull and the head of the living subject. A list of the chief measurements of the skull which the author recommends is given. These are to our mind much more satisfactory than any previous list he has produced, and will, we feel certain, be hailed with pleasure by anthropologists in this country, as being quite in accord with their ideas of what the essential measurements of the skull are. This list also agrees more nearly with the measurements used by our fellow-workers in Germany and Austria; indeed, we might even go so far as to say that when an international system of measuring the skull s arrived at (a time which we hope is not far distant), the difference between it and that now propounded by Dr. Topinard will be slight. The most important measurements of the living head, he considers, are those of the nose, and next to them the diameters of the head. As an example of what may be learned from a single craniometric character when systematically studied, the cephalic index is selected, and he alludes with satisfaction to the international agreement recently arrived at regarding the measurements from which it is obtained and its divisions, and shows that it is a character of the first importance for distinguishing types of races.

Having thus prepared the way, he next discusses the connection between man and the animals which approach nearest to him, the distance which separates them, and the relative place which man occupies amongst them. This naturally leads him to the consideration of the characters and descriptive morphology of the Primates. As the subject is a very wide one, he has restricted his investigation to a comparative study of the brain and the skeleton. Chapters xiii. and xiv. are occupied with the evolution of the brain in the vertebrate series, the form and volume of that organ, and the arrangement of its convolutions. The mechanism of the evolutionary transformation of the cranium of the animal into that of man is traced in the fifteenth chapter, and the craniometric characters connected with this transformation are dealt with in the succeeding chapter. The characters of the head, the vertebral column, the thorax and pelvis connected with the quadrupedal and bipedal attitude, are discussed in the seventeenth chapter, and those relating to the attitude of the body and the function of prehension in the eighteenth.

The nineteenth chapter is devoted to the zoometric characters related to the adaptations of the limbs for prehension and locomotion; the muscular and visceral characters connected with attitude are also discussed shortly. Other characters distinctive of man, the anthropoids, and the monkeys, in the vertebræ, the sacrum, mandible, teeth, liver, &c., are pointed out in the twentieth chapter. Retrogressive anomalies, or the accidental appearance in man and other animals of morphological arrangements foreign to the type, but resembling those which occur normally in some other type of animal; rudimentary organs, absolutely useless to man, but which are more or less developed in other animals; and progressive anomalies, are the subject of the twenty-first chapter.

The author concludes his task by devoting the last chapter to a *résumé* of the previous chapters. He classifies the Primates as follows:—

1st Sub-order—Man.		
2nd Sub-order—The Monkeys	1st family,	Anthropoids.
	2nd ,,	Pithecidæ.
	3rd ,,	Cebidæ.
	4th ,,	Arctopithecidæ.
3rd Sub-order—The Lemurs.		

Supposing the distance between the Cebidæ and the Pithecidæ to be 1, that between the latter and the Anthropoids would also be 1, while the distance between the Anthropoids alone, or in conjunction with the other two families, on the one hand, and man on the other, would be represented by 3; the same figure would also represent the distance of the lemurs from the monkeys. We note that Dr. Topinard includes Galeopithecus among the lemurs, although this genus is now accepted by zoologists as belonging to the Insectivora, but this slight error is immaterial.

The important subject of the relationship and descent of man is dealt with more briefly than we could have wished. He shows that man cannot be descended from an Anthropoid, which is essentially a perfected and specialized monkey, and that we must look to a lower source for the origin of the human stem, to one where more generalized conditions obtained; indeed, to a type sufficiently far back in time and low down in the animal series as to be the progenitor of the monkeys and man. The beginning of the Miocene period is pointed out as a very remarkable one in the history of the world, during which many of the initial types of our existing genera were formed, and amongst others the first monkey succeeding the lemurs of the preceding Eocene period. It is during this epoch that we must seek for the stem proper of man, and that common to the monkeys, or to both of these sub-orders. According to Cope, man has descended directly from the lemurs, without passing by way of the monkeys and Anthropoids, the lemurs themselves coming through the Marsupials. Beyond this the genealogy of man is merged with that of the Mammalia, of which the first representatives existed as far back as the Trias.

Regarding the question of the unity or plurality of origin of man the author has not very much to say, and although he has reasoned on the assumption of the monophyletic hypothesis of the human species throughout the work—that is to say, that man has originated from a single stem—he considers that there is not sufficient evidence to show clearly one way or other whether this is the case, or that man has a double stem of origin developed during one epoch, or at two epochs separated from one another by a considerable interval of time. This question has yet to be definitely solved with respect to the monkeys, notwithstanding the fact that some American zoologists have shown that the monkeys of the New World have not the same origin as those of the Old World, which, if substantiated, would support the argument of a double cradle of origin for man—namely, according to the author, one common to Asia and America, the other, situated in some southern continent uniting Africa and Oceania, for the negro.

Whatever the origin of man may have been, the author shows that two periods must be recognized in his history, one before the acquisition of language, which relates to his precursor, and the other after that, during which man properly so-called was constituted.

The author sums up respecting man, the Anthropoids, and the monkeys, by comparing the order of the Primates to a tree. The lemurs are the roots, and give rise to one or several stems. One of these is the stem of the monkeys, a branch of which sends out a bough more elevated than the others—namely, that of the Anthropoids. Another, of which the point of origin or of contact with the former has yet to be discovered, gives off the human branch, which grows up parallel with that of the Anthropoids, but without relation to it, and shoots beyond it.

The book concludes with a few words on the question of whether or not man has attained his perfection. The author's idea is that the volume of the brain cannot increase much more, though it is possible for the anterior lobes to become larger. One thing he thinks is certain, that dolichocephaly will be replaced by universal brachycephaly. Although the brain has probably attained its limits in respect to size, the limits to which the quality of its cells may improve are, as far as can be seen, uncircumscribed, and in this direction man may yet hope to attain to still higher perfection by the development of his intellectual faculties.

From the critical sketch we have given, it will be seen that the book is an important addition to anthropological literature. Not only will it be useful to anthropologists, but also to general readers who desire to obtain an insight into anthropology and to follow what is being done in that science. As an exposition of the subject, we have no hesitation in stating that it is a work of much merit, and worthy of the high reputation of its author. There remains yet the duty to be performed by the publishers, of putting it within the reach of a much wider range of readers than it is accessible to in its present form, by the publication at an early date of the English edition.

J. G. G.

FURNITURE WOODS.

The Art and Craft of Cabinet-making. By D. Denning. (London: Whittaker and Co., 1891.)

THIS neat little volume purports to be "a practical hand-book to the construction of cabinet furniture, the use of tools, formation of joints, hints on designing and setting out work, veneering, &c., together with a review of the development of furniture." It is well planned, and written in a pleasing and simple style, and appears admirably adapted for its purpose in general.

There is one drawback to this book, common to all works of its kind, and that is the meagre information given in the section dealing with the various kinds of woods employed. These woods are mahogany, cedar, oak, walnut, ash, rosewood, birch, beech, satinwood, pine, and a few others. The author argues that the amateur requires no information at all—or practically none—about these woods, but recommends him to trust a respectable dealer. We venture to remark that both the dealer and the amateur stand in need of, and would have been much interested in, a good description of these various woods.

It would at least have been worth mention that the word mahogany, like most trade names of the kind, may refer to very different woods: thus the Cuban or Spanish mahogany (*Swietenia Mahogani*, L.) is a very different wood from the mahogany of India (*Cedrela Toona*, Roxb.), which goes more commonly, perhaps, under the name of cedar (Moulmein cedar), another fallacious appellation, since it has nothing in common with the cedar of the botanist (*Cedrus*), or the pencil cedar of commerce, which is a *Juniper*, while it is closely allied to the "Cuban cedar," also known as Honduras and as Mexican cedar (*Cedrela odorata*, L.). The author is partially alive to this, as his remarks on p. 42 show; but we think he might have put the whole matter in a much clearer light by giving good descriptions of these very different woods. The African mahogany, from Sierra Leone, is a different wood again.

Under *oak*, the author, as elsewhere, begs the whole question by the remark, "Oak, like mahogany, is too well known to require any minute description." Possibly so—it all depends on the meaning of the word "minute"; but we think that such a work as this would be very much more useful if a description of the general distinctive characters of oak were given, and that the reader is entitled to expect such a description. He mentions that several kinds of oak are in the market, but this kind of thing only confuses, instead of helps, the reader.

Then, again, what does the author mean by "rosewood"? The rosewoods of India—*Dalbergia latifolia* (Roxb.) and *Pterocarpus indicus* (Willd.)—are by no means the only timbers which come into this country under that name, and the author might have done much more than merely remark that, "so far as mere appearance is concerned, there is not much difference between the various kinds."

We think that the author has missed an opportunity of compiling what is very much needed in this country, a concise and practical chapter on the distinctive characters of the various cabinet woods, on the lines, for instance, sketched in Marshall Ward's little manual on "Timbers, and some of their Diseases." The expectation that something of the kind might have been attempted is the fairer, since the author, in this very chapter on "furniture woods," goes out of his way to reinstruct amateurs in the use of the multiplication table and superficial measurements, which "may have been forgotten since their school days."

The statement on p. 57 that wood does not contract in length requires modification; and some of the remarks on warping and shrinking would be more intelligible to a reader who understood something of the structure of his woods.

The illustrations are numerous, and, on the whole, good and decidedly useful; and in spite of the omissions we point out, we think the book admirably adapted in many respects for the amateur's shelves.

OUR BOOK SHELF.

L'Électricité dans la Nature. Par Georges Dary. "Bibliothèque Internationale de l'Électricité et de ses Applications." (Paris: Georges Carré, 1892.)

A POPULAR and accurate account of the various forms and ways in which electricity appears in Nature is sure to find a great many readers, for the subject is most interesting. Everyone should become acquainted with at any rate some of the most ordinary electrical phenomena of every-day life, even if he should learn no more than the cause of a flash of lightning and the subsequent peal of thunder. In this volume the author has brought together accounts of many interesting phenomena that have been observed from time to time, with the hypotheses that have been put forward to explain them. The subject is divided into seven parts or chapters. The first relates to the origin, presence, and distribution of electricity in our atmosphere, and also to cosmical electricity, in which the influence of solar spots on atmospheric electricity and the electrical nature of comets and nebulæ are mentioned. There are also descriptions of various conductors and electrometers, the principles of each being brought out clearly. Chapter ii. deals with storms. The author in this part has collected many typical examples which represent various classes of storms. After reference to the formation, height, and constitution of storm-clouds, and the variation and distribution of the storms themselves on the earth's surface, he describes the various forms in which lightning has been observed. Very curious effects, both on men and trees, are recorded to have taken place. An interesting instance here given relates to a flash of lightning that, having struck one tree and travelled down its trunk spiral fashion, suddenly leapt across to another one close by, and went to earth, the spiral curve being continued on this second tree. Chapter iii. contains some useful information relating to lightning-conductors, in which a brief historical summary is given: many kinds that have been or are now in use are described, with accounts of their action, verification, and efficiency. In the next two chapters, hail-storms, waterspouts, tornadoes, and cyclones are dealt with, while earthquakes and auroræ form the subjects of the concluding chapters.

In the above summary of the contents of this volume there is much to which we should have liked to refer, but the reader at any rate will be able to form a general idea of the range of subjects treated of in these four hundred pages.

Besides being interesting, the book will form a useful volume to many readers, for its value is very much increased by the great number of references inserted.

The First Book of Euclid's Elements. By the Rev. J. B. Lock, M.A. (London: Macmillan and Co., 1892.)

NOW that the concession has been made by the University of Cambridge of allowing in all her public examinations any sound proof of the propositions of Euclid provided that their logical sequence remains unaltered, teachers of geometry will have a far freer scope; since they are no longer bound by any hard and fast rule. The present work, by a writer familiar to our readers, will be read with interest, for the arrangement of the text has undergone somewhat of a change from the sequence usually followed. With regard to the order of the propositions, it will be noticed that the theorems are separated from the problems. This seems to be advantageous, for after all there is a fundamental difference between theorems and problems: as the author says, a theorem is a geometrical truth based on fundamental ideas and definitions of geometry, while problems entirely depend on postulates which are practically impossible.

The definitions have also received great attention, and are here thoroughly and clearly explained; in two cases, that of the "straight line" and "angle," the author has thought fit to make a slight divergence from the customary definitions. Accompanying the propositions are numerous exercises, while interpolated occasionally are many worked out examples.

Altogether, the book is one that should be in the hands of teachers, and is worthy of being well tested by them in their classes. W.

The Ilford Manual of Photography. By C. H. Bothamley. (London: The Britannia Works Co., Ltd., 1892.)

THIS manual, which has been compiled at the request of the Britannia Works Company, will be found by our photographic readers to be both well written and useful, containing as it does all information generally needed by amateurs. It is not a complete treatise on the subject, but is intended to aid those who are indulging in the various applications of this art at the present day from a thoroughly practical point of view. The first few chapters are devoted to the description of the apparatus, developing manipulations, faults in negatives, and various printing processes, all of which are well treated; we then come to the method of making enlargements, mounting and framing, lantern slides and transparencies. The concluding chapters are of special interest, consisting of portraiture, copying, photographing of objects in motion, orthochromatic photography, and photography by artificial light.

Preceding the appendix are tables of English and French weights and measures, while the appendix itself contains some formulæ and reprints from one or two photographic journals. Besides an account of the Ilford universal hydroquinone developer, there are papers by Mr. John Howson, on the "Printing Paper of the Future," "Lantern Plates," together with the best methods of cutting up printing-out paper.

A most useful table presents a list of dealers and dark rooms situated all over the world, ranging from modest dealers in Bettws-y-Coed and Leighton-Buzzard to those in South Africa, New Zealand, and Japan. From this table, it can be at once ascertained whether, at a certain place, Ilford plates or paper, chemicals or apparatus, are kept in stock; whether amateurs can receive help or get work done for them, such as printing, mounting, &c.; or whether a dark room is obtainable or not. This list, when thoroughly completed, and other first-class firms included, will be invaluable for tourists on the Continent, while at present it should be used very largely by those travelling in this country. W.

The Advanced Class-Book of Modern Geography. By William Hughes and J. Francon Williams. (London: George Philip and Son, 1892.)

IT is impossible not to have some pity for the unfortunate boys and girls who will have to learn geography from this gigantic "class-book." It consists of more than 800 closely-printed pages, the very appearance of which would suffice to discourage most young students. Geography is one of those subjects in the learning of which very much more depends on the teacher than on the text-book; and a good teacher would have no desire to see so elaborate a work as this in the hands even of "advanced" pupils. The information, so far as we have been able to examine it, is accurate; but it is not, as a rule, presented in a way that would be likely to excite interest or curiosity.

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.*]

Carpenter on Eozoon.

THE scientific world is deeply indebted to Dr. Dallinger for his excellent new edition of Carpenter's invaluable work on the microscope, and among other things for his retaining unchanged the description of *Eozoon canadense*, as a monument of an important research up to a certain date.

Dr. Carpenter devoted much time to the study of Eozoon, and brought to bear on it his great experience of foraminiferal forms, and his wonderful powers of manipulating and unravelling difficult structures. After having spent years in studying microscopic slices of Eozoon and the limestones in which it occurs, I have ever felt new astonishment when I saw the manner in which, by various processes of slicing and etching, and by dexterous management of light, he could bring out the structure of specimens often very imperfect. Not long before Dr. Carpenter's death I had an opportunity to appreciate this in spending a few days with him in studying his more recently acquired specimens, some of them from my own collections, and discussing the new points which they exhibited, and which unhappily he did not live to publish. Some of these new facts, in so far as they related to specimens in our cabinet here, have since that time been noticed in my *résumé* of the question in the Memoirs of the Peter Redpath Museum, 1888; but I hope my friend Prof. Rupert Jones may yet be able to complete Dr. Carpenter's work.

Those who know Dr. Carpenter's powers of investigation will not be surprised that later observers, without his previous preparation and rare insight, and often with only few and imperfect specimens, should have failed to appreciate his results. One is rather surprised that some of them have ventured to state with so great confidence their own negative conclusions in a matter of so much difficulty, and requiring so much knowledge of organic structures in various states of mineralization. For myself, after working for fifty years at the microscopic examination of fossils and organic rocks, I feel more strongly than ever the uncertainties and liabilities to error which beset such inquiries.

As an illustration in the case of Eozoon: since the publication of my memoir of 1888, which I had intended to be final and exhaustive as to the main points, and in so far as I am concerned, I have had occasion to have prepared and to examine about 200 slices of Eozoon from new material: and while most of these have either failed to show the minute structures or have presented nothing new, a few have exhibited certain parts in altogether unexpected perfection, and have shown a prevalence of injection of the canal system by dolomite not previously suspected. Since that publication also, the discoveries of Mr. Matthew in the Laurentian of New Brunswick, and the further study of the singular Cambrian forms of the type of Cryptozoum, have opened up new fields of inquiry.

I think it proper to state, in reference to Dr. Dallinger's footnote on the recent paper of Mr. Gregory, that it must not be inferred from it that Mr. Gregory had access to my specimens from Madoc and Tudor, though he no doubt had excellent material from the collections of the Canadian Geological Survey. It might also be inferred from this note that I have regarded the Madoc and Tudor specimens as "Lower Laurentian." The fact is that I was originally induced in 1865, by the belief of Sir W. E. Logan at that time that these rocks were representatives in a less altered state of the middle part of the Laurentian, to spend some time at Madoc and its vicinity in searching for fossils, but discovered only worm-burrows, spicules, and fragments of Eozoon, which were noticed in the Journals of the Geological Society for 1866. (The more complete specimen from Tudor was found by Vennor in 1866.) On that occasion I satisfied myself fully that the beds are much older than the Cambro-Silurian strata resting on them unconformably; but I felt disposed to regard them as more probably of the age of some parts of the Huronian of Georgian Bay, which I had explored with a similar purpose under Logan's guidance in 1856. As my work was not official, and was palæontological rather than stratigraphical, it did not seem proper to express any dissent from what were at the time the probable conclusions of stratigraphical work; but I was quite prepared to assent to the new views afterwards adopted.

In conclusion, the new material bearing on Eozoon is accumulating so rapidly that I cannot hope to be able to master it in detail, but shall be glad to aid others who may have more time; but I hope to be able, in a work now in preparation, at least to present the facts up to date in a popular form.

J. WILLIAM DAWSON.

McGill College, Montreal, February 3.

The Samoan Hurricane.

REPLYING to the communication in NATURE of December 17, 1891 (p. 161), signed "H. F. B.," relative to my preliminary report on the Samoan hurricane of March 1889 (published in the Proceedings, U.S. Naval Institute, vol. xvii., No. 2, and in the *American Meteorological Journal*, July 1891), I would submit the following statement.

First of all, I wish to acknowledge Mr. Blanford's appreciation of the difficulties involved in the consideration of the subject, owing to the meagreness of the data; and at the same time to express my own appreciation of the fact that he himself has not had access even to such data as we have succeeded in collecting, but only to my necessarily brief discussion thereof, and the conclusions that I have drawn therefrom.

Mr. Blanford's explanation, as I understand it, is as follows: the vortex of the hurricane formed north or north-east (on the equatorial side) of Apia on the afternoon of the 15th, within a "region of disturbance" that had already caused stormy weather throughout the Samoan Islands and a decided barometric depression at Apia. The first effect of this formation was (by adding slightly to the normal evening rise of the barometer) to cause a decided rise of pressure, which, however, decreased again as the vortex slowly approached the harbour, thus causing the second minimum (the afternoon of the 16th), the duration of the storm being explained by the usual slow motion of the newly formed hurricane.

To the above explanation it is necessary to make a correction, I think, owing to the fact that the shifts of wind at the time of and immediately after the first minimum show that the centre of the disturbance then passed to the westward of Apia, and as the wind thereafter remained from north to north-east, the centre (or vortex) evidently remained to the southward and westward. This fact, however, merely introduces a change in the position where the new vortex formed, according to the theory under discussion.

Revising Mr. Blanford's explanation, then, in the light of this correction, it appears that the track of the *depression* is about as I have drawn it, but that a *vortex* formed slightly to the *southward* and *westward* of Apia, thus causing a slight rise of pressure at first, succeeded by a second fall, and the slow motion of this newly formed vortex caused the duration of the northerly gale.

Now, I must here take exception to one of Mr. Blanford's statements, which is as follows (referring to the theories given in my paper): "None of these explanations seem to take account of the circumstances that attend the formation of tropical cyclones, which, as we have elsewhere pointed out, differ in many respects from the storms of the temperate zone." A reference to my paper will show, I think, that I took into consideration the special peculiarity to which Mr. Blanford calls attention, and went so far as to insert a plate in order to illustrate two types of storms—namely, (1) the characteristic tropical hurricane type (where there is a decided vortex, or "centre of aspiration"); and (2) the type where there is a comparatively wide central region surrounded by an annular space where there are steep barometric gradients and correspondingly high wind velocities, but without any decided vortex, properly so called. I said also that "it will be seen that the indications are that the Samoan hurricane (on the 15th and 16th, at least) was of the second type, although during the 17th and 18th it doubtless became more like the first." In a word, I said (both explicitly and by means of the varying strength of the track drawn on the chart) that the depression passed Apia on the afternoon of the 15th, recurved (increasing in intensity and delaying whilst recurving, each of which is to be expected), and then moved off to the southward and eastward. I do not intend to convey the impression that I made any definite statement as to just when or where the vortex formed, nor am I wholly prepared to hazard such a statement even at this late date. I can still only repeat here what I said before, as follows:—

"My idea is, briefly, that the first depression occurred as the storm passed on its westward track, followed by the usual shift of wind to the northward. Along this branch of its trajectory its severity was probably not quite so great as it was later, and the force of its southerly winds was masked by the mountains on the island of Upolu; possibly careful observations of the rapidity of motion and the character of the clouds, or of the state of the sea off the harbour, might have indicated a severe storm, but this does not appear from the evidence at hand, though well worth considering. During its recurve the hurricane probably increased in intensity, the barometric depression at the centre deepening, and thus causing the second depression observed at Apia, which was slightly deeper than the first, although the centre itself was really at a greater distance than on the previous day."

Unfortunately, as stated above, we have no very definite data regarding the severity of the storm before it reached Apia, although I must say that a barometric depression of 0·76 inch below the normal in the tropics is very suggestive of the presence of a fully developed vortex, and it seems more than likely that if the harbour had not been well sheltered to the southward the southerly gales would have been quite as severe as those from the north. The only early data that we have, other than the reports from Apia, are contained in the following very brief report from the American schooner *Equator*, Captain Reid:—

"March 14 (Samoan date), lat. 12° 00'.S., long. 170° 50' W., wind S., S.W., W., N.W.; thick, squally; hove to.

"March 15 (Samoan date), lat. 13° 00' S., long. 170° 40' W., wind N.W.; fierce gale; squalls; heavy sea."

This, although brief, seems to indicate that it was a fully developed hurricane that was approaching Apia, and I am inclined to the opinion that such was actually the case.

Mr. Blanford will surely admit that it is one recognized peculiarity of tropical hurricanes to reduce their speed of translation whilst they are recurving, and he will thus admit also that such recurvature accounts, in the present case, for the duration of the northerly gale at Apia. So far as the sharpness of recurvature is considered, I would say that the report of the *Equator* is sufficiently vague to allow us to draw a curve with a somewhat less sharp recurvature, and I should myself have done so but for the single fact that a hurricane approaching from a more northerly direction must have sent a northerly or north-easterly swell into the harbour that would have been felt to at least such an extent as to be noted in the log of one or more of the vessels there, but I have looked in vain for any such remark, until after the shift of wind on the afternoon of the 15th.

In conclusion, I take pleasure in expressing my obligation to Mr. Blanford for his interesting and able discussion of the subject, which, even with all the data at hand, has still many perplexing features. The principal object of this preliminary publication was to elicit comment and discussion, and it will be very gratifying if other authorities will give us the benefit of their experience and suggestions.

EVERETT HAYDEN.

Hydrographic Office, Washington, D.C., January 4.

THE distinction to which allusion was made in the passage quoted by Mr. Hayden is not, as he seems to have understood, that between tropical and extra-tropical storms when fully formed, but between the circumstances of their respective formation. These are that, in the former, the cyclonic circulation of the winds is preceded by much irregular action, which sometimes extends over a considerable area. Within this area there are local squalls and shifts of wind, with heavy rainfall, but the action is not for some time definitely concentrated and cyclonic. This preliminary stage does not appear to obtain in the storms of the temperate zone, where the deviating effect of the earth's rotation is so much greater than in low latitudes, and indeed, if we accept the views of Werner Siemens and Prof. Hann, the cyclonic circulation is the cause and forerunner of the storm. I cannot think it probable that a vortex, once fully formed, and travelling towards higher latitudes, should recurve so sharply as to produce a fall of the barometer on two successive days (with a rise in the interval) at the same place, simply by twice passing in its vicinity. To effect this, the recurvature must, as I apprehend, describe an angle considerably less than a right angle, and of such I know of no example among tropical cyclones. At the same time, my own view was put forward merely as a suggestion, and in no dogmatic spirit.

H. F. B.

Phoronomy.

ABOUT thirty-five years ago, I had a conversation with the late Dr. Donaldson, a well-known Greek scholar of the time, in which we discussed the appropriateness of the use of the word kinematics, in the sense in which it was then, and is now, employed by writers on mathematical science. Dr. Donaldson's opinion was that it is not the best word which can be employed to represent the science of pure motion, without regard to causation. He said that the word κινέω involved the idea of the cause of motion, and therefore that it ought not to be used when the idea of causation is to be completely set aside. He further gave it as his opinion that the word φορέω is more nearly expressive of the idea of mere going, without any reference to the cause of motion, and therefore that the proper word would be phoronomy, or phoretics.

I was so much impressed by this conversation that, for many years, I headed with the word phoronomy the papers of questions on the subject of pure motion which I was in the habit of preparing for College lectures and private pupils.

I have recently consulted a very eminent Greek scholar, and his opinion is that, on the whole, the word phoronomy is more distinctly expressive of the science of pure motion than the word kinematics. He agreed with Dr. Donaldson that the word kinematics suggests some idea of causation, whereas no such idea is suggested by the word phoronomy.

As a matter of history, the word *cinématique* was introduced by Ampère to represent the purely geometrical science of motion in the abstract, and was anglicized into kinematics by, I think, the late Dr. Whewell.

Sir W. Thomson and Prof. Tait, in the preface to their "Natural Philosophy," adopt the suggestion of Ampère, and employ the word in the same sense. They also employ the word *dynamics* in its true sense as the science which treats of the action of *force*, whether it maintains relative rest, or produces acceleration of relative motion. They further state that these two corresponding divisions of dynamics are conveniently entitled *statics* and *kinetics*.

Here, then, we have two words, kinematics and kinetics, both derived from the same root-word, employed to represent two entirely different sets of ideas; and there is not the same broad line of demarcation between the words themselves as there is between the sets of ideas which they are intended to connote.

Hence it appears to me that the word phoronomy, the law of going, is the most suitable, as it is the most expressive word, to represent the science of pure motion in the abstract.

At the time of my conversation with Dr. Donaldson, we were neither of us aware that the word had been already invented and employed. Some years after, I found that a treatise was published at Amsterdam in 1716, entitled "Phoronomia, sive de viribus et motibus corporum. Autore Jacobo Hermanno, Basil." Hermann, however, uses the word in much the same sense as we now use the word dynamics.

The point has been recalled to my mind by the discovery that the word has been employed in Germany in the sense in which Dr. Donaldson advocated that it should be used.

In the treatise entitled "Allgemeine Mechanik der Punkte und Starren Systeme," by E. Budde, published in 1890 at Berlin, the word phoronomy is adopted, and the author gives his reasons in the following words:—

"Man kann eine Ortsveränderung zunächst rein geometrisch, ohne alle Rücksicht auf ihre Ursachen, betrachten, und das soll in den nächsten Capiteln geschehen. Die Disciplin, welche sich mit dieser Betrachtung befasst, heisst *Phoronomie* oder *Kinematik*. Der Name Kinematic ist seit Resal der gebräuchlichere gewesen; neuerdings aber wird von Reulaux und seinen Schülern die Morphologie der Verknüpfung von Maschinentheilen als 'Kinematik' bezeichnet. Wir wählen deshalb hier den Namen Phoronomie."

With reference to this statement of Budde's, I observe that Ampère, in the "Essaie sur la Philosophie des Sciences," particularly mentions trains of machinery, such, for instance as the works of a watch, as coming under the heading kinematics.

I also find that Grassmann, in the "Ausdehnungslehre von

1844," published at Leipzig in 1878, speaks of "Phorometrie" as representing "die reine Bewegungslehre"; and I see that Möbius uses the adjective in an article on the "Phoronomische Deutung des Taylor'schen Theorems."

A change in scientific nomenclature is by no means an unprecedented occurrence.

For instance, notwithstanding the great authority of Lagrange, the phrase "virtual velocity" has been practically superseded by the phrase "virtual work," and in the year 1876 the word "work" was substituted for "virtual velocities" in the regulations, published in the Cambridge University Calendar, for the Mathematical Tripos.

Another instance is the fact that the phrase *vis viva* has been superseded by *kinetic energy*, as a more convenient term in the expression of the principle of energy.

Further, I notice that Prof. Tait, in lectures on "Recent Advances in Physical Science," gives the suggestion that the time-honoured word force is in all probability destined, as science advances, to be relegated to the limbo of departed nomenclature.

For these various reasons, then, I trust that I shall not be regarded as an iconoclast, if I venture to substitute, for the word *kinematics*, the word *phoronomy*.

W. H. BESANT.

St. John's College, Cambridge, February.

On the Terms "Centrifugal Force" and "Force of Inertia."

THE retention, in the last edition of Mr. Loney's "Elements of Dynamics," 1891, of a paragraph (p. 141) which resuscitates the objections formerly urged by some writers against the use of the term "centrifugal force" seems to call for a protest. It is to be regretted that students of dynamics should find absolutely contradictory statements presented to them respecting the validity of this term. While, however, in one set of text-books we find a perfectly clear definition and consistent use of the phrase "centrifugal force," there does not, on the other hand, appear to be unanimity of ideas amongst the objectors, nor always sufficient clearness in expressing the same.

In the uniform circular motion of a ball rolling on a table against the inner surface of a vertical cylinder, the pressure of the cylinder upon the ball is a centripetal force directed towards the centre of the circle. The contrary pressure of the ball upon the cylinder is the "centrifugal force," which is defined as the reaction to the centripetal in this case, and in every case as the reaction to the normal component of the centrifugal force.

The foregoing definition or usage of the term is adopted without hesitation or apology in the following works, named in order of date:—

Poisson's "Traité de Mécanique," 1833, vol. i. p. 332, or Harte's translation, 1842, p. 256.

Walton's "Mechanical Problems," 1842, pp. 240, 260, 269.

Prof. Niven in "Cambridge Senate-house Problems," 1877, p. 78.

Thomson and Tait's "Natural Philosophy," 1879, p. 221.

Garnett's "Elementary Dynamics," 1875, p. 205, and 1882, p. 255.

Routh's "Rigid Dynamics," Part I., 1882, p. 217, and Part II., 1884, p. 15.

Williamson and Tarleton's "Dynamics," 1889, p. 88.

Objections to the term appear in—

Goodwin's "Course of Mathematics," 1849, p. 275.

Parkinson's "Mechanics," 1863, p. 249.

Blaikie's "Dynamics," 1887, p. 32.

Rankine, "Encyclopædia Britannica," "Mechanics," 9th edition.

Loney's "Statics and Dynamics," 1891, p. 141.

Other authors might have been cited, but I have referred to such as I happen to possess.

Nearly all these objectors evince the same reluctance to giving the name of "force" to the reactionary effect of the body's inertia in the direction of the normal outwards. Yet, if we admit that "to every force there is an equal and opposite reaction," it is not easy to escape from the conclusion that such a reactionary force exists.

Mr. Loney, however, postulates both forces, but adds:—"Centrifugal force is a very misleading term. It seems to imply that the force belongs to the mass instead of being an external force acting on the mass. A somewhat less misleading term is centripetal force. We shall avoid the use of either expression; the student who meets with them will understand that either (*sic*) means the force which must act on a mass to give it the acceleration normal to the curve in which it moves."

These are confusing directions to the student, who must be left in complete bewilderment as to any distinction in meaning between "centripetal" and "centrifugal." "Centrifugal," from its derivation, signifies that the force has a tendency to make the body fly away radially from the centre. And such a tendency there is, and such a motion would result if we could make the centrifugal force last after the centripetal has ceased. But in the objections taken the word "tendency" is regarded as though it implied an actual subsequent motion in the direction of the tendency. A beginner is almost certain to fall into the error of imagining that, when the cord is slipped, the stone from a sling will dart away in a direction intermediate between that of the string and its own previous motion in the circle. But the name "centrifugal" is not answerable for this. The idea is due to the unmistakable pull upon his hand of an outward tending force, to which "centrifugal" merely gives the right name. Clearer conceptions show him that the two forces, the action and the reaction, cease at the same instant when the string is cut, and that there is no initial velocity in either direction.

Uniform circular motion is perfectly unique. In the direction of the force there is no motion, in the direction of the motion there is no force. The real *crux* lies in this conception of a constant acceleration with a perpetual zero velocity in the direction of the acceleration. How, says one, can there be a rate of change when the change itself is zero? But the objection is a metaphysical one, and it may be urged with equal force against the whole doctrine of limiting ratios.

Mr. Loney's statement that the centrifugal reaction is not a force belonging to the mass, but "an external force acting on the mass," requires some elucidation. Dr. Parkinson, in the paragraph referred to, has something similar. He says that the term "centrifugal force" "vaguely conveys an impression, as it were, that the particle of itself resisted curvilinear motion and exerted a force *per se* to move in a rectilinear path, which innate tendency was only overcome by the action of some external force." He also grudgingly recommends the student to use the obnoxious phrase "simply as an equivalent for the moving force in the direction of the normal." Here again "centrifugal" is made to signify a tendency towards the centre! Is not the vagueness complained of imported into the subject in some measure by the writers themselves?

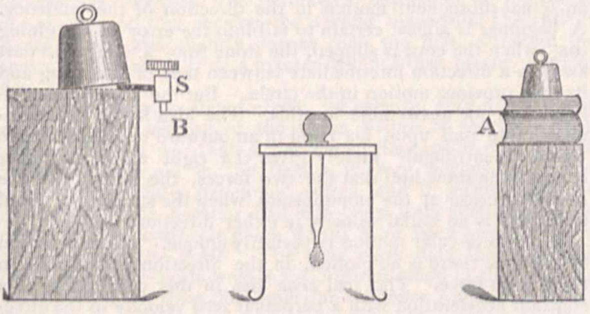
Whatever names are employed, the facts are these. The force towards the centre communicates to the body an acceleration in that direction, which acceleration gives rise (we know not how, but we say by the law of inertia) to a force equal and opposite to the force which produced the acceleration. This reaction always appears to emanate from the mass of the moving body, and it has therefore been called "the force of inertia" of the body. Although this view has been combated by Poisson and others, some of the latest authorities are reasserting it. Thus in Thomson and Tait, 1867 and 1879, we find in Article 216: "Matter has an innate power of resisting external influences. . . . This the inertia of matter, &c." Again, in Sir Robert Ball's "Experimental Mechanics," 1888, p. 252: "When any agent acts to set a body in motion or to modify its motion in any way, the body reacts on the agent, and this force has been called the kinetic reaction."

I cannot see any objection to designating this reaction "the force of inertia." It is a provisional term, which will serve our purpose until the nature of force is better understood. Poisson's argument against it, derived from our experience of friction, appears to me invalid, and his illustration irrelevant, because the law of resistance is not the same as in the case of inertia. If it had happened that the law governing friction was that the resistance to motion was directly proportional to the acceleration, then if a body were moving with constant velocity upon a rough plane there would be no resistance from friction. The smallest acceleration of velocity would give rise to a correspondingly small amount of friction, a double acceleration would double the resistance from friction, and so on, precisely as with the resistance from inertia. GEORGE S. CARR.

A Lecture Experiment in Surface Tension.

HOPING it may be of interest to some of your readers, I venture to send you the following description of a simple experiment on surface tension. A drop of water hanging from the end of a vertical glass tube is regulated in size until it is just on the point of falling away from the tube. On dipping the end of a penholder in ether, and bringing the wet end within a few millimetres from the drop, the latter promptly falls. The drops may be produced and adjusted in the following manner:—

A piece of glass tube, about 8 cms. long and ·8 cms. in diameter, is made into a pipette with an orifice about ·15 cms. diameter, and fitted with a small india-rubber ball at the other end. The tube is then passed through a hole in a piece of wood large enough to rest on the top of a common tripod stand, as shown in the figure. A strip of wood, or a paper-



knife, about 30 cms. long, is placed with its centre resting on the top of the ball, one end (A) being held between the two uppermost of a pile of books, and the other end (B) passing under a screw (S). The ball is squeezed, a beaker of water brought under the end of the pipette, and the tube allowed to fill by the expansion of the rubber. By working the screw S, a drop is formed at the end of the tube, and since one complete turn of the screw I use only lowers the end of the rod (B) $\frac{1}{25}$ inch, it is possible to adjust the drop with great nicety. As the ether is brought up, the absorption of its vapour diminishes the surface tension over a small area of the drop of water, and currents, made visible by suspended dust, *appear* to pass from the interior towards the weakened spot. Bringing the ether still nearer, the drop often becomes much agitated, and finally, when the distance is reduced to about 4 mm., it falls away from the tube.

E. D. FRIDLANDER.

Mason College, Birmingham, February 29.

The Orientation of Ancient Monuments.

THE deeply interesting results obtained by Mr. Norman Lockyer with regard to the orientation of Egyptian temples, and by Mr. F. C. Penrose with regard to the Greek, tempt me to call attention to an extract from the *Century Magazine*, May 1883, from an article by Frank Cushing, describing a visit to the Zuñis, a typical tribe of the Pueblo Indians of New Mexico, in 1879:—

"Each morning just at dawn, the Sun priest, followed by the master priest of the Bow, went along the eastern trail to the ruined city of Ma-tsa-ki by the river side, where, awaited at a distance by his companion, he slowly approached a square open tower, and seated himself just inside upon a rude ancient stone chair, and before a pillar sculptured with the face of the sun—the sacred hand—the morning star and the new moon. There he awaited, with prayer and sacred song, the rising of the sun. Not many such pilgrimages are made ere the 'suns look at each other,' and the shadows of the solar monolith, the monument of Thunder Mountain, and the pillar of the Gardens of Zuñi lie along the same trail; then the priest blesses, thanks, and exhorts his father, while the warrior guardian responds as he cuts the last notch in his pine-wood calendar, and both hasten back to call from the house-tops the glad tidings of the return of spring. Nor may the Sun priest err in his watch of Time's flight; for many are the houses in Zuñi with scores on their walls or ancient plates embedded therein, while opposite a convenient window or small port-hole lets in the light of the rising sun, which shines but two mornings of the 365 on the same place. Wonderfully reliable and ingenious are these rude systems of orientation, by which the religion, the labours, and even the pastimes of the Zuñis are regulated."

In like manner, we read in Prescott that at Quito the festival of the Sun-god was held when "he sat upon the pillar," *i.e.* a pillar cast no shadow.

Might not this Zuñi ceremonial be a description of one enacted at some primitive astronomical temple such as Stonehenge? and do we not gain the best insight into the minds of those who in the Archaic period first orientated—the Babylonian and Assyrian ziggurats, the pyramids and temples of Egypt, the temples of Greece, and our own Stonehenge—by seeing how a people in a similar stage of intellectual and scientific development are acting to-day? FRED. F. GRENSTED.

Merchant Taylors' School, Crosby.

The Nickel Heat Engine.

MR. KARAMATE, in a letter which appears in your issue of March 3 (p. 416), alludes to a new form of heat engine described by me in your issue of January 28 (p. 294). He states that my device is similar to one by Prof. Stefan. I wish to point out that the two heat engines are quite different in design. The engine of Prof. Stefan has a step by step action; that described by me is under certain conditions absolutely continuous in its action. The contrast is shown by Mr. Karamate's description of Prof. Stefan's engine; he writes:—"Nickel plates were fixed on a wheel, like that of a water-mill, and a magnet was placed before it. By heating a nickel plate before the magnet, it was repulsed by the magnet, and a succeeding plate was attracted, so that the wheel commenced to rotate." From this it seems clear that the action of the machine must be step by step, since the different pieces of nickel must come successively under the influence of the magnet and the flame. In my disk form of engine, the action, when the disk has a *certain thickness*, is continuous. Mr. Croft has recently shown that when the disk is not so thick as mine was the disk starts in one direction, then stops, and sets off in the opposite direction. It will also be noticed that the distribution of the magnetic field due to two poles is entirely different from that in the machine of Prof. Stefan. Mr. Karamate writes: "By heating a nickel plate before the magnet, it was repulsed by the magnet." I hardly see how this *repulsion* takes place. Faraday, writing about the behaviour of nickel, states ("Experimental Researches," vol. iii., 2346): "Upon being heated the nickel soon became indifferent to ordinary magnets, but, however high the temperature, still it pointed to and was attracted by the electro-magnet." Surely the action of the engine is due to one piece of nickel becoming partly non-magnetic owing to a rise of temperature, thus upsetting equilibrium, and allowing the next piece of nickel to approach the magnet and consequently the flame.

I may add that E. Berliner in 1885, and Edison in 1887, patented magnetic heat machines on the step by step principle.

FREDERICK J. SMITH.

Trinity College, Oxford, March 4.

The Limpet's Power of Adhesion.

I WOULD like to call the attention of your readers to the results of some experiments, which, I think, are original, referring to the power of adhesion of the common limpet (*Patella vulgata*).

The experiments were carried out as follows, with the assistance of my friend J. Sinel, of the Biological Laboratory here. The shell of the limpet *in situ* was perforated, so as to allow of the attachment of a delicate spring balance, by means of which a gradually increasing strain could be put upon the animal, in a direction normal to the foot. The pull was increased until the animal became detached, and the final weight and the greatest and least diameters of both foot and shell noted.

The following figures are compiled from a series of twenty consecutive experiments, and are typical of other similar series. The individuals operated on were not chosen in any way.

Averages of Twenty Consecutive Experiments.

Area of shell, in square inches	1·07	
Pressure per square inch of shell	22·5	pounds.
Area of foot, in square inches	·45	
Pressure per square inch of foot...	54·3	pounds.
Gross weight	23·9	,,
Maximum pressure per square inch of shell area	31·3	,,
Minimum ,, ,, ,, ,,	13·9	,,
Maximum ,, ,, ,, foot area	71·1	,,
Minimum ,, ,, ,, ,,	37·1	,,
Maximum gross weight	32	,,
Minimum ,, ,, ...	12	,,

The areas in the above table are calculated on the assumption that both shell and foot are ellipses, which is fairly near the truth. I need hardly point out that the old theory of atmospheric pressure will not account for these figures, and in the case of *Patella* the mucus is too thin to have much cohesion. Perhaps some of your readers can throw light on the subject.

29 St. James's Street, Jersey. PERCY A. AUBIN.

On the Variation of Latitude.

IN connection with the interesting investigations of Dr. Chandler on the variation of latitude, permit me to suggest the desirability of calling attention to Maxwell's remarks on the same subject at the conclusion of his paper "On a Dynamical Top," Ed. Phil. Trans., xxi., Part iv. p. 568 (read April 1857). Maxwell, taking the value of $\frac{C}{A}$ as 1·00309, as deduced from the amount of precession (Pontécoulant, ii. 268), finds the time of revolution of the pole of the earth about the true pole to be about 325·6 solar days. Dr. Chandler's period of 427 days would on the same principles correspond to the value $\frac{C}{A} = 1·00235$.

ROBT. B. HAYWARD.

Fairlight, Harrow, February 28.

ORNITHOLOGY OF THE SANDWICH ISLES.

THE departure for Honolulu of Mr. Robert C. L. Perkins (as already announced in these pages), the gentleman sent out by the Joint Committee appointed by the Royal Society and by the British Association to investigate the zoology of the Sandwich Islands, may render the present moment opportune for noticing what has already been done towards obtaining a knowledge of their presumably expiring Fauna, though I can only lay claim to acquaintance with a single Class of it—the Birds. Yet students of that important branch of Biology which is known by the clumsy and not strictly accurate title of the "Geographical Distribution of Animals," will bear in mind that the first successful attempt to grapple with and elucidate it was based upon this very Class; and, furthermore, that by far the most comprehensive work on the subject has proceeded from the pen of a most proficient ornithologist, while nobody can doubt that but for his intimate knowledge of Ornithology many of his results would have been inconclusive if not unattainable. Into the reason why the most vagrant Class in creation was thus so serviceable to Mr. Sclater and to Mr. Wallace, there is no need here to enter. It is perhaps enough to state that indubitable fact to warrant the publication of the present remarks having reference to a small portion only of the Hawaiian Fauna; and, if they should furnish an indication of what may be proved when the rest comes to be better known, it will so much the more redound to the credit of Ornithology; while, should further acquaintance with other Classes contradict the inferences to be drawn from the Birds, the suspicion, to call it by no stronger name, at times expressed, that what is a good Law of Nature for one set of animals will not hold for another, may be justified.

Moreover, within the last six months has been published a very remarkable dissertation, "On the Structure of certain Hawaiian Birds, with reference to their Systematic Position," contributed by Dr. Gadow to Mr. Scott B. Wilson's beautifully illustrated "Birds of the Sandwich Islands." From its title, this essay might be supposed to interest only the taxonomer or the ornithotomist; and the zoologist of wider views might leave it unheeded as having a scope too limited for his purposes. The very contrary is the truth, and those who will follow the author's deductions to their logical end will perceive that his "Remarks" disclose a state of things which is not only subversive of the generally-received opinion as to the nature and affinities of the avifauna of the Sandwich Islands, but is fraught with evidence of a kind hardly hitherto suspected in regard to the origin and derivation of the animal population of that group. Hints to this effect may, indeed, be gathered from Mr. Wallace's works, and especially from his "Island Life"; and that, with the few facts at his disposal, he was able to give them, is proof of the depth of his perception; but henceforth he will be able to speak boldly, and drop every uncertain phrase.

The dearth of facts with which Mr. Wallace had to contend, even in 1880, shows that the Sandwich Isles have not been fortunate in their Natural Historians, though perhaps no worse off in this respect than many another group "lying in dark purple spheres of sea." Discovered in 1778 by Cook, during the last of his celebrated voyages, his ships communicated with one of the more western islands—Atooi, as its name sounded to him and his companions, but since, and doubtless more correctly, written, Kaui. The admiration of the visitors was excited by the cloaks and helmets of the natives, beautifully bedecked with feathers, the more or less moth-eaten remains of which may yet be seen in many a Museum; and the scarlet birds which furnished the most brilliant adornment of these ingenious works of art were duly mentioned by Cook in his journal as published. After less than a fortnight's stay, in the course of which the existence of five islands was made out, his ships stood off to the northward to prosecute their voyage of discovery. Towards the end of the year they returned, and Cook, having had experience of the hospitable treatment of the islanders, designed to make his winter-quarters in the Sandwich Isles, as he had named them, after the then First Lord of the Admiralty; but, keeping more to windward, the first land he made was the most eastern of the group, one that he had not even seen on his first visit. This was the historic Owhyhee—nowadays written Hawaii—which, being the largest of them, and that which produced the warrior-king and statesman who eventually subdued all the rest, has given its official name to the Archipelago.

Though Owhyhee was sighted on November 29, Cook's course along its eastern and southern coast was so deliberate that it was not until January 17, 1779, that he found a safe anchorage, and that in Kealeakakua Bay, on its western side. What passed there during the next three weeks need not be here recorded, but those who know how to read his narrative and the accounts since divulged from native sources will admit that it throws an important and yet most lurid light on the history of superstition. To the unprejudiced it must be doubtful whether even now the whole truth is or ever can be known. The ships sailed on February 4; but in making her way to the northward, the *Resolution* sprung her mainmast, and within a week returned to her old anchorage. Three days later occurred the terrible tragedy which deprived the world of one of its greatest seamen. A week after Cook's death, the ships sailed to the westward, touching at some of the intermediate islands—Mowee (Mauai), Lanai, and Morotai (Molokai), making once more for Atooi (Kauai) and Oneehow (Niihau), the last famous for its yams. Then, on March 15, they bore away again to the northward, and did not return.

Now, the object of giving here these details is to show that the natural history specimens obtained by Cook's ships were procured only on the islands of Hawaii, Kauai, and Niihau. This is the more needful because the first descriptions of any of the birds of the Sandwich Isles were given, with two exceptions, by Latham in his "General Synopsis of Birds," published in 1781–85, and most of the specimens so described no longer exist. Some were in the British Museum or the collection of Sir Joseph Banks, afterwards transferred thereto; the rest were in the Leverian Museum. Of the former, as is well known, not one remains; but fortunately, at the breaking up of the last in 1806, a few were bought by the then Lord Stanley, who (dying in 1851, as thirteenth Earl of Derby, and President of the Zoological Society) bequeathed his collection to the borough of Liverpool, and there, thanks to the care that has been taken of them, they still exist in fair condition. A few more were bought for the private collection of the then Emperor of Austria, and are still carefully preserved in the Museum of Vienna. Of several of the species it is not known that any other specimens were brought to Europe until some three years ago. On both of Cook's previous voyages qualified naturalists had been sent; but, as is known, the arrangements for publishing their discoveries were so imperfect that little credit followed to anyone concerned. On this, his third and last voyage, there was no expert, though Mr. William Ellis, who in an irregularly published narrative, calls himself "Assistant Surgeon to both vessels," was somewhat of a draughtsman, and made a series of sketches, which, becoming the property of Banks, subsequently passed to the British Museum. The commoner species of Sandwich-Island birds are generally recognizable, but others are so unhappily limned that even the word caricature (which always implies some likeness) seems too strong to apply to them. Nevertheless, Mr. G. R. Gray adventured to determine all of them.

More than a quarter of a century passed before any further progress was made in the knowledge of the zoology of the Sandwich Isles, though they were visited by numerous ships, and in 1794 were ceded to Britain under Vancouver. In 1814 an attempt was made to seize them for Russia; and Kotzebue, whose voyage has so much scientific interest, was there in 1816–17, but the accomplished naturalists, Chamisso and Eschscholtz, who were with him, took little heed of the fauna of the islands.[1] The year 1822 saw the arrival of the more celebrated William Ellis, whose missionary labours throughout the Pacific and in Madagascar are so widely known. The Sandwich Isles had by that time fallen under the sway of the conquering Kamehameha I., whose son and successor, desirous of seeing European civilization, arrived in England in 1824 with his wife—both to die of measles within a few weeks. The British Government determined to send their remains for interment in Honolulu, by that time become the capital of the islands, and accordingly H.M.S. *Blonde*, commanded by George Anson seventh Lord Byron (first cousin and successor to the poet), was commissioned to convey the dismal freight. The duty was performed, and the islands again were ceded to the British Crown, but again declined. On board the *Blonde* sailed as chaplain Mr. Rowland Bloxam, together with his brother Andrew, who was somewhat of a naturalist, and it was intended that the published account of her voyage should contain a proper appendix on the natural history of the islands. An "Appendix" there indeed is, but one utterly unworthy of its reputed author, for the book was edited by a lady (as I have been informed) who had nothing but a few of his notes to guide her, and though assisted, as it is stated, by "the gentlemen connected with that department in the British Museum," is a disgrace to all concerned, since, so far from advancing the knowledge of the subject, it introduced so much confusion as to mislead many subsequent writers.[2] Some years later another great opportunity was missed, and this time by the American traveller Townsend, who, after crossing the Rocky Mountains to the Columbia River, sailed, in company with Nuttall, the well-known naturalist, for the Sandwich Islands, where they arrived in January 1836, and stayed nearly three months, visiting Oahu and Kauai. Returning at the end of the year, Townsend found the Prussian naturalist Deppe at Honolulu, and with him passed some time in the pursuit of natural history, visiting most of the windward islands before he left in March 1837. Among the specimens obtained by Deppe for the Berlin Museum were some of two species for which Lichtenstein rightly established a new genus—the singular form *Hemignathus*—and, as it has since proved, both these species were new, though he had, not unnaturally, identified one of them with a species described by Latham. Of Townsend's collection, a considerable part was given to the Academy of Natural Sciences at Philadelphia,[1] where it still remains; but he sent several specimens to Audubon, at that time, I believe, in Edinburgh, and he parted with them to Carfrae, a dealer there, who sold them to the late Sir William Jardine, at the dispersal of whose collection I was so fortunate as to secure them—some of them bearing Townsend's label—for the Museum of this University. If Townsend had but published a list of his captures, he would indeed have rendered a very good service; but of course the value of island-forms, to say nothing of the fact that many of them were threatened with extirpation by colonization and civilization, had not then been appreciated, if even entertained, by naturalists. In the year of Townsend's departure, the French frigate *Vénus*, in the course of her troublous career under Du Petit-Thouars, arrived in the Sandwich Islands, with two naturalists, Léclancher and Néboux, on board; and some years later the atlas of plates illustrating the zoology of her voyage appeared; but the text was deferred for a long while, and, indeed, was not completed till 1856. Herein was figured and described, though not for the first time, a third species of the curious *Hemignathus*. In the meanwhile the celebrated expedition of Commodore Wilkes took place, and he, with some of his ships, wintered there. In the course of their six months' stay, the naturalists attached, Pickering and Peale, seem to have made large collections; but nearly all was lost in the shipwreck of the *Peacock*, one of the ships of the squadron. By 1848, Peale had completed his report on the specimens of Mammals and Birds collected, and it was printed off. A few copies only had been distributed, when the rest were destroyed by fire. It was by no means a bad performance; and I cannot understand why the late Mr. Cassin made so many changes in it when he, ten years later, brought out a new edition of it. Some of them (I speak only of those relating to the Sandwich Island fauna) were certainly not improvements. However, a distinctly forward step was made by the Peale-Cassin labours, and since few can obtain access to the original work, I may mention that Dr. Hartlaub considerately published an abstract of it,[2] just as two years later he did[3] of the French "Voyage au Pôle Sud," wherein, having sorted out the different species observed by various voyagers on the several Pacific groups, he gave a useful list of those found on each, and thus he assigned to the Sandwich Isles *thirty* species of birds, marking two of them as doubtful. One of them is now known to be rightly included, but the other must be struck out, as well as, for one reason or another, four more—leaving a total of *twenty-five*, only *sixteen* of which are Land-birds and only *fourteen Passeres*.

Hitherto, no list of the birds of the Sandwich Isles had been published, so that Dr. Hartlaub's met a great want, though it had, of course, been possible, since 1814, for anyone to pick out for himself the species assigned to that group from the general list compiled by Tiedemann

[1] The same negative results attended his second visit in 1824-25.

[2] I have reason to believe that Mr. Bloxam's original notes are still in existence, though hitherto they have not been accessible to me. It is possible that they would remove uncertainty on several points.

[1] In mentioning these facts, I desire to record my deep gratitude to the authorities of both these Museums—Berlin and Philadelphia—for their obliging readiness in allowing me to have these valuable specimens, one of them unique, for examination.

[2] *Archiv für Naturgeschichte*, 1852, Heft. i. pp. 93–138.

[3] *Journal für Ornithologie*, 1854, pp. 160–171.

("Anatomie und Naturgeschichte der Vögel," ii. pp. 426–436), and, in like manner, since 1859, from Mr. G. R. Gray's useful "Catalogue of the Birds of the Tropical Islands of the Pacific Ocean," printed by order of the Trustees of the British Museum, but the former was obsolete, and the latter, as we now know, very erroneous.[1] Mr. Gray's references show him to have been as usual a model of accuracy, but his judgment as an ornithologist was frequently at fault. It was, therefore, with great pleasure that, some time in the winter of 1870–71, I received a copy of a "Synopsis of the Birds hitherto described from the Hawaiian Islands," which had been communicated in February 1869 to the Boston Society of Natural History, by Mr. Dole, a resident in those islands, and had been published in the Society's Proceedings (xii. pp. 294–309); and Mr. Sclater, who I knew had long taken an interest in the ornithology of the group, lost no time in noticing this very important publication (*Ibis*, 1871, pp. 356–362), adding thereto some valuable observations.[2] This list has naturally proved a serviceable foundation for future work. Forty-eight species were included, the author stating that this number "probably comprises but little more than half the avifauna of the group." That the list should be free from error was not to be expected, and a revised version of it, published in the "Hawaiian Almanack and Annual for 1879" (pp. 41–58), corrected some of the mistakes; but it was an honest piece of work, doing credit to its compiler. In the meanwhile, however, the historic voyage of H.M.S. *Challenger* had commenced, and one of the places at which she was to call was the Sandwich Islands. Of course the main object of her voyage was the exploration of the depths of the sea. Nevertheless, the terrestrial zoology of the countries visited, though forming a very subordinate part of the original plan, was not to be wholly neglected—nor was it in this case, for, during the three weeks she stayed in Hawaiian waters (July 27 to August 19, 1875), her officers availed themselves to some extent of the opportunity of studying the ornithology of the islands, though it does not appear that they had received any special instruction in regard to our imperfect knowledge of it. Here, then, was another great chance lost; for had those who drew up the directions for the scientific members of the Expedition taken the trouble to acquaint themselves with the particular points on which investigation was needed, so as to indicate the lines on which further research was desirable, no doubt some one of the *Challenger's* staff would have supplied, even in the short time of her stay, some of the missing facts, or at least would have thrown some light on the subject. As it was, the collection was reported as "small" (24 bird-skins and no specimen in spirit), and "containing nothing absolutely new except a single species of *Anas*," afterwards named *A. wyvilliana*[3] (Proc. Zool. Soc., 1878, p. 350). Something more, however, may yet be expected. The late Prof. Moseley, in his "Notes of a Naturalist on the *Challenger*," states (p. 514) that the last excursion on shore of his colleague, Von Willemös Suhm, was at Hilo in Hawaii, with a native guide, "in pursuit of the interesting endemic birds," and that "almost the last notes he wrote were some on the Sandwich Islands, relating especially to the birds." These notes do not seem to have been placed at the disposal of the ornithologists who described the specimens obtained by the *Challenger*; but they can hardly have been destroyed.

Having myself felt a good deal of interest in the avifauna of the Sandwich Isles—which, like that of many other islands throughout the world, was, as I had learnt, threatened with extirpation, chiefly in consequence of the destruction of the forests—I could not fail to be disappointed at the meagre results obtained by our people on this celebrated cruise, when it would have been so easy for them to have done better had their attention been duly called, and I cast about in several directions to find some suitable person to visit the islands with the view of investigating their ornithology in a thorough way. My young friend Mr. Scott Barchard Wilson (son of the well-known Mr. George Wilson, F.R.S.)—of whose taste for natural history I was well assured by his residence in my own College, by his journey to Portugal with Dr. Gadow, and by his subsequent sojourn in Switzerland (*Ibis*, 1887, pp. 130–150)—willingly took up the enterprise, and left Liverpool on February 24, 1887, for Honolulu, where he arrived on April 8, having on his way paid a visit to Washington to confer with Dr. Stejneger, whose name had already appeared in connexion with the birds of the Sandwich Isles. Mr. Wilson stayed in the islands until towards the close of the following year. He brought back such a collection as had never before been made there; but, rich as it was in some respects, defects became apparent as it was gradually worked out, and some of these defects are so grave that, until they are remedied, no complete list of the avifauna can be formed. I am deeply sorry that he has not been able to return; for, with his knowledge of what is wanted, it would be more easy for him to fill up the *lacunæ* than anyone else; and I long hoped that he would pay a second visit with this object. However, he has done a great deal more than anybody before him: he has ascertained the precise localities of nearly all the birds hitherto known, and added to them not inconsiderably—*fourteen* new species or local forms of *Passeres*, *two* of which required generic acknowledgment—all, it needs not to say, being peculiar to the islands, and mostly to one particular island only. It can scarcely fail to be interesting that the distribution in the group of the different genera and local forms of *Passeres* should be shown, and this is best done by the accompanying table.[1]

But Mr. Wilson was not content, as so many collectors in foreign countries are, with preserving only the skins of the birds he procured. He was careful to obtain specimens in spirit of all the important existing types; and these, having been properly subjected to examination by Dr. Gadow, have led to some remarkable results—the most remarkable that have been as yet made known in regard to the birds of the Sandwich Islands, and perhaps the most remarkable of those published during the past year in regard to Ornithology at large. They are contained in the dissertation I have already mentioned as being contributed by Dr. Gadow to Mr. Wilson's work. Most of the land-birds of the Sandwich Islands had been at one time thought to belong to the *Meliphagidæ*, or Honey-suckers—a Family very characteristic of the Australian Region, and known to be very polymorphic. It was thought to be still more so; and the surmise had been acted upon, so that some Finch looking birds, *Psittacirostra* and *Loxioides* had been supposed to be Honey-suckers in disguise, and

[1] Many of its worst errors are doubtless due to the loss, before mentioned, of the type specimens, which had been suffered by the Museum long before Mr. G. R. Gray was connected with it. Latham, in 1821, had already lamented their decay. It is almost needless to add that such a loss is not now, nor for many years has been, possible.

[2] Mr. Sclater was pleased to remark that this "memoir" had "escaped" my notice as editor of *The Ibis*. Herein he was in error. It certainly did not come to my knowledge while I was discharging that duty, and I doubt whether any copy reached England until after I had laid down my office.

[3] I do not venture to doubt the distinctness of this species, which had before been mentioned as *A. boscas*, var.; but its describer might have shown more clearly wherein it differs from the well-known American *A. obscura*, which seems its nearest ally.

[1] I have no desire to overlook the services of Mr. Valdemar Knudsen, of Kauai, who sent thence to the United States National Museum several collections, the most important of which was described by Dr. Stejneger in the Proceedings of that institution for 1887 (pp. 75–102), the year of Mr. Wilson's arrival in the islands. The Doctor's paper is of the exhaustive character to which one is accustomed in all his productions, and has been of considerable use in working out Mr. Wilson's collections, while these have enabled the latter to correct several mistakes—under the circumstances quite pardonable—made by the former, who subsequently described in the same Proceedings (xii. pp. 377–386) another collection from the same quarter.

classed accordingly. Dr. Gadow has shown that this supposition is wholly erroneous, and that these last, together with another form, *Chloridops*—one of Mr. Wilson's discoveries—are true *Fringillidæ;* while out of the whole Hawaiian avifauna, only two genera can be referred to the *Meliphagidæ*—namely, *Acrulocercus* (*Moho* of some writers) and *Chætoptila*, the last being presumably extinct. All the other forms which had been accounted Meliphagine, present a peculiar structure of tongue, forbidding that alliance, or any affinity to the *Prionopidæ*, *Dicæidæ*, or *Nectariniidæ*, but revealing a distinct relationship to the *Cœrebidæ*—now known as a Family characteristic of the Neotropical Region! Hereby a beam of light is thrown on the origin and derivation of the ornithic population of the Sandwich Islands. The distinct inference is that the first stock of their existing avifauna was received from America, in days when the range of the *Cœrebidæ* extended further to the northward than it does at present, and that certain cognates or ancestors of the present *Cœrebidæ* colonized the islands, there differentiating into the modern *Drepanididæ*. The importance of this inference on views which are held as to the Geographical Distribution of Birds in North America is a subject into which there is no need here to enter, for that would be a subject foreign to my present remarks; but I doubt not it will receive due attention from American ornithologists, whom it most nearly concerns. That these colonists from what I would venture to term a "Columbian" fauna—since it cannot be literally called a Neotropical one, and is certainly not "Nearctic"—were the earliest settlers which have left descendants one can hardly doubt, for they have existed in the Sandwich Islands long enough to undergo a great amount of change. Subsequently there has been a small infusion of blood from the "Australian Region." I say subsequently, because Dr. Gadow has shown that this immigration has undergone comparatively little modification. We have (or had) the two Meliphagine genera, *Acrulocercus* and *Chætoptila*—the latter, indeed, beyond anatomical examination, but showing no very great external deviation from well-known Australian types; while the former undoubtedly retains the normal Meliphagine tongue. To these may be added *Chasiempis*, a well-marked genus; but, without question, very nearly allied to the genus *Rhipidura*, so widely spread over the Australian Region, and found also in New Zealand. Thus three genera constitute, so far as I am able to see, the "Australian" element in the avifauna of the Sandwich Islands—and what are they among so many others?[1] More recently than this Australian infusion, has supervened an influx of Holarctic types, and especially of the *Fringillidæ*. Whether these have arrived from America or Asia, I do not pretend to say; but the long chain of islets running to the westward—one of which produces a remarkable form (*Telespiza cantans*), the knowledge of which we also owe to Mr. Wilson (*Ibis*, 1890, pp. 339–341, Plate ix.)—suggests the possibility of an Asiatic origin, a possibility confirmed by the consideration that his fine *Chloridops kona* may be the magnified descendant of the long-known *Chloris kawarahiba*, which has already an enterprising relative, *C. kittlitzi* (*Ibis*, 1890, p. 101), established in the Bonin Islands. Still later must have been the appearance on the scene of members of the genera *Corvus* and *Buteo*, both of which are, so far as is yet known, confined to Hawaii, the most eastern of the islands, and therefore suggest an emigration from the Nearctic area. These have been settled long enough to assume recognizable specific characters; but an apparently more modern colonist exists in *Asio accipitrinus*, the common Short-eared Owl of Asia, Europe, and North America, which extends its range over many islands in the Pacific Ocean, so far at least as the Galapagos, and has found a permanent home in the Sandwich Isles, breeding there, as it would seem, regularly—as it once did in England, and would again, if permitted by the gamekeepers. More than this, there is an indication that the tendency to colonization from the Holarctic region still continues. Within an hour or two of his leaving the islands, there was sent to Mr. Wilson a freshly-killed example of *Circus hudsonius*—the American Hen-Harrier—a species which he had already ascertained to have before occurred in the group; but, not being recognized by Judge Dole, it had been endowed with a new name, and figures in his second list as *Accipiter hawaii*. The existence in considerable numbers of a Californian species of *Carpodacus* is thought, and no doubt rightly, by Mr. Wilson to be

Table showing the Distribution of Birds of the Order PASSERES *in the Sandwich Islands.*

	Niihau.	Kauai.	Oahu.	Molokai.	Lanai.	Kahoolawe.	Maui.	Hawaii.
CORVIDÆ.								
Corvus tropicus								*
FRINGILLIDÆ—								
Psittacirostra psittacea		*		*	*			*
Loxioides bailleui								*
Chloridops kona								*
"*Fringilla*" *anna*								?
DREPANIDIDÆ—								
Loxops coccinea								*
,, *flammea*				*				
Chrysomitridops cæruleirostris ...		*						
Oreomyza bairdi		*						
Himatione stejnegeri		*						
,, *parva*		*						
,, *chloris*			*					
,, *maculata*			*					
,, *chloridoides*					*			
,, *montana*					*			
,, *kalaana*				*				
,, *virens*								*
,, *mana*								*
,, *sanguinea*...	*	*	*	*	*		*	*
,, *dolii*							*	
Vestiaria coccinea	*	*	*	*	*		*	*
Drepanis pacifica								†
Hemignathus procerus		*						
,, *lichtensteini*			*					
,, *obscurus*								*
,, *hanapepe*		*						
,, *lucidus*			*					
,, *olivaceus*								*
MELIPHAGIDÆ—								
Acrulocercus braccatus		*						
,, *apicalis*			?					
,, *nobilis*							*	*
Chætoptila angustipluma								†
TURDIDÆ (?)—								
Phæornis myiadestina		*						
,, *lanaiensis*				?	*			
,, *obscura*								*
MUSCICAPIDÆ—								
Chasiempis (quædem species non determinatæ)	*	*	*	*	*		*	*

All the species above-named are peculiar to the group, *i.e.* not found elsewhere. A * indicates that the species inhabits the island whose name heads the column. A † shows that the species is believed to be extinct.

[1] In connexion herewith may be noticed the absence of Parrots, Kingfishers, and Doves—all Families that are very characteristic of an "Australian" Fauna.

due to human agency, and accordingly I do not attach any importance to the fact; but there is one very puzzling species, of which only a few specimens seem to have been preserved, that needs particular attention. This was described by Judge Dole under the name of "*Fringilla anna*," but, of course, is no true *Fringilla*. Mr. Wilson brought home but a single specimen, which he owed to the kindness of the Hon. C. R. Bishop, it having been formerly in the Mills Collection; and, I believe, will establish for it a new genus, *Ciridops*—so named because its bright coloration recalls the well-known *Emberiza ciris* of Linnæus, the Painted Bunting of authors, or "Nonpareil" of bird-dealers. It is supposed to be now extinct, but it was a truly native species; it probably belongs to the fauna which I have above called "Columbian" (for want of a better name); but I cannot suppose it to have been so early a settler as the *Drepanididæ*, since it has changed so little. On the genus *Chasiempis* I would offer only one remark, and that is a word of caution to those who would, on the evidence of from a couple to half-a-dozen of specimens, or perhaps even on the evidence of a badly-coloured plate, attempt to break it up into definable "species." There remains of land-birds the genus *Phæornis*, which earlier systematists were inclined to put among the Fly-catchers (*Muscicapidæ*). The examples in spirit, placed by Mr. Wilson at Dr. Gadow's disposal, have enabled the latter to set aside that view, and to show that, of all the Families to which this genus has been supposed to be allied, "it differs least from the *Turdidæ*," and he would regard it "as a generalized or rather primitive Thrush."[1]

Of the water-birds I do not now propose to speak. Though possessing very many points of special interest to the ornithologist, so far as I understand them they throw no particular light on the general questions I have attempted to consider; and I would conclude this sketch of the Ornithology of the Sandwich Isles by referring to the unhappy fate of one of the most beautiful of their birds—the "Mamo," as I am told it was latterly called—*Drepanis pacifica*, one of the rarest species in collections, and apparently wholly extinct. Until Mr. Wilson brought the specimen which he has kindly given to this University, there seems not to have been one in the British Islands since the dispersal of the Leverian Museum, when two were bought by the Austrian agent, and are now at Vienna. How many other specimens may exist in the world I do not know, but the number can hardly exceed half-a-dozen. The bird was destroyed for the sake of its rich yellow feathers, used in former days to decorate the state robes of the chiefs, and according to all accounts a glorious sight one of those robes when in all its freshness must have been. As the species became scarce, recourse was had to the yellow tufts of *Acrulocercus nobilis*, which in depth of colour are very inferior; and when the *Drepanis* had ceased to exist, the name "O-o," which it seems to have borne in Cook's days, was transferred to the surviving species, according to a practice of which I have observed several instances in other nations. The general similarity of coloration in *Acrulocercus* and *Drepanis* is, indeed, obvious, and Dr. Gadow is inclined to consider the latter to have been the imitating form. If so, its mimicry has proved its destruction; but it clearly could not have foreseen that fashion should ordain its acquired yellow and black feathers to become desirable commodities among the human race, and it would be well to suspend judgment on this point. It had most likely a very limited range, which would, of course, hasten its end; and its two most conspicuous relatives, the scarlet *Vestiaria coccinea*, and the crimson *Himatione sanguinea*, though in equal request for their gaudy plumage, still exist, inhabiting (as will be seen by the table) all the islands that have been examined. How to account for the disappearance of *Chætoptila angustipluma* is beyond my power. It has no attractive colouring, and yet is declared to be extinct. The specimen given to us by Mr. Wilson is, I believe, the only one ever brought to Europe, and there seems to be but one (the type) in any American Museum. In mentioning the former I must acknowledge gratefully the generosity of Mr. Wilson, who promised a complete set of his bird-skins to the Museum of his old University on the completion of his work, a promise that he will doubtless perform.

Finally, I would point out that the conclusions established by Dr. Gadow's researches seem to coincide very much with those arrived at by Dr. David Sharp and Mr. Blackburn from their investigation of a small collection of Hawaiian *Coleoptera* (Trans. Roy. Dublin Society, series 2, vol. iii. Part 6). The entomological captures of Mr. Perkins are therefore awaited with considerable interest; and still more valuable, perhaps, may be his conchological collections, for it seems doubtful at present whether the Mollusks of the Sandwich Isles can be brought into line with their Birds and their Beetles. There is every chance of this question, among many others of importance, being solved if Mr. Perkins is enabled to prolong his stay for sufficient time; but that depends upon the financial support he may receive at home from the two learned bodies which I have mentioned, and from the Hawaiian Government and influential residents in the Islands.

ALFRED NEWTON.

Cambridge, 13 February, 1892.

[1] A minute comparison with the New-Zealand *Turnagra*, if that be still in the land of the living, would be desirable.

PROF. BUNSEN AND THE CHEMICAL SOCIETY.

IT was announced at the last meeting of the Chemical Society that it was proposed to present the following address to Prof. Bunsen, who has now been fifty years a Foreign Member of the Society; and the wish was expressed that, among those who sign it, all who have been his pupils should, as far as possible, be included. Fellows of the Society who have been pupils of Prof. Bunsen are requested to communicate with the Senior Secretary before March 19, in order that they may receive a form for signature.

To Privy Councillor Prof. Bunsen, Fellow of the Royal Society.

YOUR EXCELLENCY,—Fifty years have elapsed since the Chemical Society of London honoured itself by electing you one of its Foreign Members. Your name, and that of your illustrious fellow-countryman Liebig, are, in fact, first on a list which includes the most distinguished cultivators of chemical science in every civilized country of the world.

Our Society remembers with gratitude that you enriched the first volume of its Transactions by communicating to it the results of your ever-memorable investigation of cacodyl and its compounds. That you should have sent to us, in the first and most critical year of our existence, a memoir which the chemical world will ever regard as one of the classics of our science, is a significant proof of the beneficent interest with which you regarded our efforts to foster the growth of chemical learning in this country.

Your masterly investigations, in collaboration with our Fellow, Sir Lyon Playfair, on the gases evolved from iron furnaces, made by methods which you were the first to bring to perfection, greatly extended our knowledge of the theory of the smelting of iron. By the permanent benefit thus conferred on one of the most important of our industries, you have largely augmented our national wealth.

The half-century during which you have been associated with our Society has been fruitful in great discoveries and important inventions. It has witnessed the birth of new elements, the creation of new analytical methods, and an extraordinary development in the instrumental resources of our laboratories. Chemists will never forget that it is to your unwearied assiduity and single-minded devotion that science owes some of the most momentous of these discoveries, and some of the most valuable of these inventions. Your investigations will ever be regarded as models of the highest type of scientific research, and the memoirs in which you have embodied them shed an imperishable lustre on our literature. Your methods of analysis are among the most common of our manipulative operations, and the very furniture and instruments of our laboratories are an ever-present testimony to the obligations under which experimental chemistry will always remain to you.

Many of our members are proud to be numbered among your pupils, and those among them who have become teachers, have, we trust, caught and transmitted something, not only of the method, but also of the spirit, in which they themselves were taught. They have an abiding memory of your kindliness, of your constant and unselfish devotion to their interests, and of the generous sympathy and ready help which you extended to their efforts to enlarge the boundaries of our science.

We, the undersigned Fellows of the Chemical Society of London, now beg to offer you our heartfelt felicitations on the occasion of your Jubilee as a member of our body. It is our fervent hope that you may be able, for many years to come, to enjoy in health and happiness the leisure and repose which you have so justly and so honourably earned.

NOTES.

THE course taken by the Government with regard to a Teaching University for London has met with general approval. The proposed Charter, if it had been accepted, would have done almost irreparable injury to the cause of higher education in the capital. Now we have got rid of it, and the way is clear for new and more carefully considered schemes. The Royal Commission to which the question is to be referred will be free, if it chooses, to examine the question whether, after all, the institution needed by London might not be most readily and most effectually obtained by the development of the existing University. It may be hoped that on this and all other aspects of the question full evidence will be taken. What is wanted is that any recommendations which may be made by the Commission shall be based on extensive and accurate information as to the organization and the proper functions of Universities. On this subject some very crude notions are still current in England.

THE sixth annnal Photographic Conference in connection with the Camera Club will be held in the theatre of the Society of Arts, on Tuesday and Wednesday, March 22 and 23, under the presidency of Captain W. de W. Abney, F.R.S. All photographers are invited to take part in the Conference.

THE Botanical Society of France will hold an extraordinary meeting at Biskra, Algeria, during the first half of April.

A ROYAL COMMISSION has been appointed to consider the question of the water supply of London. Its task will be to inquire whether the present sources of supply are adequate in quantity and quality, and, if inadequate, whether such supply as may be required can be obtained within the watersheds of the Thames and the Lea, or must be obtained elsewhere. Among the members of the Commission are Sir Archibald Geikie and Prof. James Dewar.

A NUMBER of gentlemen, representing institutions interested in science-teaching, recently applied to the Vice-President of the Committee of Council on Education for permission to wait upon him with reference to the changes indicated in the circular issued by the Department of Science and Art, dated November 12, 1891. In answer to a question put by Mr. Schwann, Sir W. Hart Dyke stated in the House of Commons, on Tuesday evening, that he had informed the deputation that he did not think any good purpose would be served by his receiving them. He was, however, willing to reconsider the matter. Sir William expressed his belief that the changes announced in the circular would greatly stimulate more advanced scientific instruction. The minute had been well received in the better schools.

A NEW "Jahrbuch der Chemie" is to be issued by the German publisher, H. Bechhold, Frankfort. It will be edited by Prof. R. Meyer, who has secured the co-operation of many eminent men of science. The intention is that the progress of pure and applied chemistry shall be recorded every year in a connected series of articles.

DR. M. C. COOKE announces that, with the next number of *Grevillea*, his connection with it as editor and proprietor will come to a close, and it will rest with others whether the journal is to be continued. *Grevillea* is "A Quarterly Record of Cryptogamic Botany and its Literature." With the next number it will have completed its twentieth volume.

DR. B. ARTHUR WHITELEGGE will begin at the Royal Institution, on Thursday, March 24, a course of three lectures on epidemic waves. In the first lecture he will deal with cyclic waves; in the second (March 31), with superadded waves; in the third (April 7), with pandemic waves.

THE fifth ordinary meeting of the Egypt Exploration Fund was held last Friday, Sir John Fowler presiding. The balance-sheet was one of the most satisfactory ever presented to the members. The Fund, it was pointed out, was now used in two classes of operations—surveying and excavating; and both "seemed likely to be very successful." In the course of a brief discussion on the report, reference was made to "the generous contributions which had been handed to the Fund from America, in comparison with those received from England."

THE half-yearly general meeting of the Scottish Meteorological Society was held at Edinburgh on Monday, March 7. The following was the business:—(1) Report from the Council of the Society; (2) changes in the temperature of Scotland since 1764, by Dr. Buchan; (3) on the squall of February 1, 1892, by R. C. Mossman; (4) sunshine values at the Ben Nevis Observatory, by R. C. Mossman.

MR. E. J. STONE, F.R.S., and Mr. A. G. Vernon Harcourt, F.R.S., have been elected members of the Athenæum Club, under the rule which provides for "the annual introduction of a certain number of persons of distinguished eminence in science, literature, or the arts, or for public services."

THE fifteenth Convention of the National Electric Light Association, lately held at Buffalo, U.S., seems to have been unusually successful. This is attributed by the American journal *Electricity* to the fact that at previous meetings members attended "not for business but for pleasure." The counter-attractions were so varied and fascinating that it seems to have been difficult "to obtain a creditable attendance on the reading of the papers and the transaction of business." "The plan of doing away with these outside attractions," says *Electricity*, "was therefore an experiment, but so successful did it prove in this instance, that this last Convention will go down in history as 'the business Convention,' unless the increased success of succeeding meetings shall make it necessary to give this one the more specific title of 'the *first* business Convention.'"

THE Natural History Society of Buda-Pesth is stated to number 7800 members. A special botanical meeting of the Society will in future be held monthly, under the presidency of Prof. Juranyi.

THE section of vegetable pathology of the botanical division, in the U.S. Department of Agriculture, was recently made a separate division by Act of Congress. The authorities of the new division decided to begin a fresh series of publications; and they have taken the first step towards the fulfilment of their purpose by the publication of an important *Bulletin*, by Dr. E. F. Smith, presenting "additional evidence on the communicability of peach yellows and peach rosettes."

JAPAN has no fewer than 700 earthquake-observing stations scattered over the Empire, and the Tokio correspondent of the *Times* is of opinion that they are all needed. He points out that not only are the Japanese shaken up by fully 500 earthquakes every year—some of them more or less destructive—but at intervals there comes a great disaster, amounting, as in the earthquake of October 28, 1891, to a national calamity. Japanese annals record twenty-nine such disasters during the last 1200 years.

A SEVERE earthquake shock, lasting twelve seconds, was felt at Napa, California, on March 13, at 8.30 a.m. The direction of the vibration was from north to south.

A CORRESPONDENT at Leon writes to us of an earthquake which was felt in Nicaragua on February 6. He speaks of a connected series of longitudinally oscillating progressive seismic waves, which lasted about ninety-two seconds. They were parallel with, and near, the cones and masses of volcanic ejecta which extend, with some interruptions, between the volcanic groups in the States of Salvador and Costa Rica. The earthquake began at 10.10 p.m.

PROF. HELLMANN, of Berlin, to whom we are indebted for many painstaking investigations into the origin of meteorological instruments and observations, has contributed to the *Zeitschrift für Luftschiffahrt* for January an article on the first balloon voyage made for scientific purposes. The works on the subject of ballooning, of which there are many, state that the first was by Robertson and Lhoëst in 1803, and the next in the following year, by Biot and Gay-Lussac. But this is not correct; the honour undoubtedly belongs to Dr. John Jeffries, of Boston (Mass.), who had for some years lived in this country. In 1786 he published a book (60 quarto pages and 2 plates), entitled, "A Narrative of the Two Aërial Voyages of Dr. Jeffries with Mons. Blanchard; with Meteorological Observations and Remarks." The first voyage was on November 30, 1784, from London to Dartford (Kent), and the second, on January 7, 1785, across the English Channel. A paper containing the results was read before the Royal Society in January 1786. The barometer taken was made by Jones, of Holborn, and read to 18 inches. The heights reached in the two voyages were about 9200 feet and 4500 feet respectively. The latter height was obtained trigonometrically by an officer at Calais, while the balloon lay stationary over the mid-Channel.

THE *Record of Technical and Secondary Education*, published monthly, can scarcely fail to be of service to all who interest themselves in educational progress. The number for March includes, besides editorial notes, accounts of County Council schemes and reports, scholarship schemes, recent progress in various districts, agricultural college for the south-eastern counties, and the financial management of the technical instruction fund. There are also instructive "miscellanea."

A WRITER in the *Mediterranean Naturalist* for March notes that no attention has hitherto been given to the fact that certain species of birds prefer certain trees. He says:—"It is a remarkable fact that, notwithstanding the voluminous literature that has been written on birds and their habits, no writer has noticed the preference that certain species of birds give to certain trees. Jays and rooks are found in the greatest number in oak-trees; finches, in lime-trees; and blackcaps among laurels. The nightingale is always found in the greatest numbers in nut-groves, while the thrush evinces a decided preference for the birch and ash. The beech is the favourite tree of the woodpecker; and the numerous families of tits are generally found in the greatest abundance among the blackthorn."

MR. W. M. GOLDTHWAITE, New York, is publishing a new monthly magazine called *Minerals*. The second number has been sent to us. It contains many short papers, in which interesting facts relating to various classes of minerals are presented in a bright and popular style.

MESSRS. J. AND A. CHURCHILL have issued a fifth edition of Dr. A. Tucker Wise's "Alpine Winter in its Medical Aspects." The work has been condensed and rewritten in many places.

THE U.S. Commissioner of Education has issued his Report for the year 1888–89, and, if it cannot be described as light reading, it is certainly a most instructive and useful work. It consists of two large volumes, and includes a number of chapters in which education in the United States is compared with that of England, France, Germany, and other countries. A full account is also given of normal schools, manual training schools, courses of study, &c. The second volume consists of "detailed statistics of educational systems and institutions, with comments and discussions."

THE peculiar milk-ferment known as "kefyr" or "kephir" has been supposed to be peculiar to the Caucasus and other parts of Eastern Europe and Western Asia. Mr. C. L. Mix has found a yeast apparently identical with it in use in Canada and the United States. It occurs in the form of small granules of a dirty brown colour, which retain their vitality for a long period, and consist of a small proportion of yeast-cells embedded in zooglœa-like masses of rod-shaped bacteria. The yeast-cells increase by budding, and no formation of spores has been detected in them. They do not invert cane-sugar like ordinary beer-yeast, but they cause alcoholic fermentation in milk-sugar or lactose and in dextrine, not in cane-sugar or saccharose. The bacteria are short cylindrical rods with homogeneous protoplasm, developing under cultivation into leptothrix-like filaments in which spores are formed. They appear to take no part in the fermentation, remaining almost entirely embedded in the zooglœa-masses during the process.

HERR W. BELAJEFF has communicated to the *Berichte* of the German Botanical Society a paper on the "Pollen-tube of Gymnosperms," which, if his observations—made on *Taxus* and *Juniperus*—are confirmed with regard to other members of the class, will greatly modify the accepted view as to the morphology of the different parts of the pollen-grain. Hitherto, the two or three small cells in the pollen-grain have been regarded as a survival of the male prothallium of the microspore. Belajeff shows that this cannot be the case, as they are cut off in succession from the large cell. Moreover, he states that it is not, as is usually supposed, the nucleus of the large cell which fertilizes the oosphere in the archegone, but the nucleus of one of the small cells. When the pollen-tube begins to develop, one of these small cells becomes detached and wanders down the tube. Its membrane becomes absorbed; its nucleus overtakes that of the large cell and divides into two; and it is one of these two daughter-nuclei of the wandering small cell, together with the protoplasm which surrounds it, that fuses with the nucleus of the oosphere in the archegone. The other small cell becomes entirely disorganized.

IT sometimes happens that peat bogs swell and burst, giving out a stream of dark mud. Herr Klinge has made a study of this rare phenomenon (*Bot. Jahrb.*), of which he has found only nine instances, in Europe, between 1745 and 1883 (seven of these being in Ireland). Heavy rains generally occur before the phenomenon, and detonations and earth vibrations precede and accompany it. The muddy stream which issues, of various fluidity, rolls along lumps of peat, and moves now more quickly, now more slowly. After the outbreak, the mud quickly hardens, and the bog sinks at the place it appeared, forming a funnel-shaped pool. The bogs considered by Herr Klinge have been almost all on high ground, not in valleys. He rejects the idea that the effects are due to excessive absorption of water by the bog. The peat layers, which often vary much in consistency, have each a certain power of imbibition, and the water absorbed does not exceed this limit. Excessive rain affects chiefly the upper layer not yet turned into peat and the cover of live vegetation, which get saturated like a sponge, after which the water collects in pools, and runs off in streams. The theory of gas explosions is also rejected; and the author considers the real cause to lie in land-slips, collapses, &c., of ground under the bog, permitting water or liquid mud to enter. This breaks up the bog mechanically, mixes with it and fluidifies it, and an outburst at the surface is the result. The limestone formations in Ireland, with their large caverns and masses of water, are naturally subject to those collapses, which, with the vibrations they induce, are more frequent in wet years. The heavy rains preceding the bog eruptions are thus to be regarded as only an indirect cause of these. Herr Klinge supposes that similar eruptions occurred in past geological periods, *e.g.* the Carboniferous, in some cases where fossil tree-stems are found in upright position.

THE geographical position of Mount St. Elias is of considerable popular interest in connection with the boundaries of Alaska. Mr. Israel C. Russell refers to the subject in a report published in the new number of the American *National Geographic Magazine*. In the convention between Great Britain and Russia, wherein the boundaries of Alaska are supposed to be defined, it is stated that the boundary, beginning at the south, after leaving Portland Channel, shall follow the summit of the mountains situated parallel to the coast as far as the 141st meridian, and from there northward the said meridian shall be the boundary to the Arctic Ocean. Whenever the summit of the mountains between Portland Channel and the 141st meridian "shall prove to be at the distance of more than ten marine leagues from the ocean, the limit between the British possessions and the line of coast which is to belong to Russia, above mentioned, shall be formed by a line parallel to the windings of the coast, and which shall never exceed the distance of ten marine leagues therefrom." As Mount St. Elias is approximately in longitude 140° 55′ 30″ west from Greenwich, it is therefore only 4′ 30″ of longitude, or 2½ statute miles, east of the boundary of the main portion of Alaska. Its distance from the nearest point on the coast is 33 statute miles. There is no coast range in South-Eastern Alaska parallel with the coast within the limits specified by the treaty, and the boundary must therefore be considered as a line parallel with the coast, and ten marine leagues, or 34½ statute miles, inland. The mountain is thus 1½ miles south of the boundary, and within the territory of the United States. Its position is so near the junction of the boundary separating South-Eastern Alaska from the North-West Territory with the 141st meridian, that it is practically a corner monument of the American national domain.

A NEW and very simple mode of synthesizing tartaric acid has been discovered by M. Genvresse, and is described by him in the current number of the *Comptes rendus*. It will doubtless be remembered that, some years ago, Dr. Perkin and Mr. Duppa prepared tartaric acid artificially by treating di-brom-succinic acid with hydrated oxide of silver, and this operation became the final stage of a complete synthesis from the elementary constituents, when, a short time afterwards, Prof. Maxwell Simpson succeeded in preparing succinic acid by the action of caustic potash upon the di-cyanide of ethylene. M. Genvresse now shows that tartaric acid may be directly synthesized by the action of nascent hydrogen upon glyoxylic acid, CHO—COOH, the curious compound, half aldehyde, half acid, derived from glycol, and hence directly from ethylene. If we double the formula of this acid, and add two atoms of hydrogen, we arrive at tartaric acid, COOH—CHOH—CHOH—COOH, and this is found to be capable of realization by reacting upon glyoxylic acid with nascent hydrogen liberated in its midst by the action of acetic acid upon zinc dust. A mixture of glyoxylic and acetic acids, the latter diluted with an equal weight of water, in the proportion of one molecule of glyoxylic to three molecules of acetic acid, was treated in small quantities at a time with zinc dust, at first at the ordinary temperature, and finally over the water-bath. The liquid was then filtered, and the zinc in solution removed by means of potassium carbonate. The clear liquid was then mixed with calcium chloride solution, and after removal of any calcium carbonate precipitate, a white crystalline precipitate commenced to separate. This precipitate was found to yield all the reactions of a tartrate, such as silvering glass when gently warmed with ammonia and silver nitrate. Its analysis gave numbers indicating the formula $C_4H_4CaO_6 + 4H_2O$, which is the composition of ordinary tartrate of lime. By treating this salt with the calculated quantity of sulphuric acid diluted with twenty times its volume of water, filtering off the precipitated calcium sulphate and evaporating the filtrate over oil of vitriol, the acid itself was obtained in large crystals. It is interesting to find that the tartaric acid obtained by this mode of synthesis is the optically inactive variety known as racemic acid, there being apparently equal numbers of molecules of both the dextro and lævo varieties produced. The crystals consequently do not show hemihedral faces; the angles observed corresponded with those observed by Provostaye and by Rammelsberg in the case of racemic acid. It may be remarked that, as the product of the synthesis of Dr. Perkin and Mr. Duppa, a mixture of racemic acid with the truly inactive tartaric acid, in which neutralization within the molecules themselves occurs, was obtained. This new synthesis of tartaric acid from glyoxylic acid would appear to throw some light upon the natural formation of tartaric acid. For, remembering the close relationship between glyoxylic and oxalic acids, which latter we know to be one most readily formed in vegetable tissues, and the reducing agencies which appear to be connected with chlorophyll, we have all the means at hand to account, in view of the work of M. Genvresse, for the natural synthesis of tartaric acid.

THE additions to the Zoological Society's Gardens during the past week include a Common Squirrel (*Sciurus vulgaris*), British, presented by Mrs. Crick; a Merlin (*Falco æsalon*), European, presented by Mr. T. A. Cotton; a Blue Titmouse (*Parus cæruleus*), British, presented by Captain Salvin; two Blossom-headed Parrakeets (*Palæornis cyanocephalus*) from India, presented by La Comtesse Cottrell; a Green Monkey (*Cercopithecus callitrichus* ♂) from West Africa, deposited; a —— Hawk (*Asturina* sp. inc.) from South America, purchased; four Yellow-bellied Liothrix (*Liothrix luteus*) from India, received in exchange; sixteen Puff Adders (*Vipera arietans*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

SOLAR INVESTIGATIONS.—*Astronomy and Astro-Physics* for February contains a short note by Prof. Hale, to the effect that photographs have been taken at Kenwood Observatory, showing the H and K lines reversed in regions widely distributed over the sun's disk. These regions closely resemble faculæ in appearance.

At the meeting of the Paris Academy on March 7, Prof. Tacchini communicated a paper on the distribution in latitude of the solar phenomena observed at the Royal Observatory of the Roman College during the second half of last year. Prominences have been more frequent in the northern hemisphere than in the southern, although in the preceding half-year, and in 1889 and 1890, they were more frequently observed in the southern solar hemisphere. The zones of maximum frequency occurred between latitudes 40° and 60°. Faculæ also have been most numerous north of the equator, and the zone of maximum frequency of these phenomena appears to be between latitudes 10° and 30°. Spots have been most abundant north of the equator, with a maximum frequency in the same zones as faculæ.

In the *Comptes rendus* containing Prof. Tacchini's results, occurs also a note by J. Fényi, on a remarkable prominence observed at Kalocsa, on February 19, as the recent large spot-group was passing over the sun's limb.

NEW DOUBLE STAR, 26 AURIGÆ.—In a communication to the *Astronomical Journal*, No. 256, Mr. S. W. Burnham records the discovery that 26 Aurigæ is a close double star, made up of two nearly equal components. His measures of position-angle and distance for 1892·0 are 344°·4 and 0″·15; and of magnitudes, 5·6 and 6·0. The distance very probably never exceeds a quarter of a second, or the duplicity of the star would have been noticed by many observers of the distant companion discovered by Herschel in 1783.

ROTATION OF JUPITER.—Writing in the March number of the *Observatory*, Mr. Denning notes that his observations of one of the chief dark spots in Jupiter's north temperate belt, for the period from August 21 to November 3 (180 rotations), gave the mean period of rotation 9h. 49m. 36·9s. Observations of the red spot, from August 7 to February 2 (432 rotations), indicate a mean period of 9h. 55m. 42·2s. The value obtained during the opposition of 1890 was 9h. 55m. 40·2s., so that the motion of rotation of the red spot would appear to have slackened by two seconds. Since the period of rotation derived from this spot is now 6m. 5s. longer than that given by the dark spots on the north temperate belt, the latter revolves around Jupiter, relatively to the former, in 40½ days.

THE NEW STAR IN AURIGA.—In No. 3078 of the *Astronomische Nachrichten* are recorded three communications relative to this Nova, two of which refer to its position, while the third deals with its spectrum. The observations of the last-named were made at the Astro-physical Observatory in Héreny, Ungarn, by Herrn Eugen and Gothard, previous to February 15. On the 10th and 13th of the same month the following lines were observed:—

Bright lines.	February 10. Wave-lengths in micro-millimetres.	February 13. Wave-lengths in micro-millimetres.
I.	654·2	—
II.	532·2	530·3
III.	513·2	516·8
IV.	501·9	501·9
V.	492·3	492·3
VI.	486·6	486·2
VII.	439·0	—
End of spectrum	412·0	—

In the first series of observations, the authors give suggestions for some of the lines, and for comparison's sake we have added Mr. Lockyer's also, both of which are included in the following table:—

Wave-lengths.	Origin.		Lockyer.
532·2	Chromosphere line (531·7)		— (531·3)
501·9	Nebula-line	(500·5)	Mg (500·6)
486·6	Hydrogen line Hβ		H (486·1)
439·0	" " Hγ		— —

In the second series comparisons were made with Geissler's tubes, and the following measurements were made:—

	Geissler Tubes.	Nova.
IV.	500·6	501·9
V.	492·6	492·3
VI.	486·2	486·2

THE LICK SPECTROSCOPE.—The February number of *Astronomy and Astro Physics* contains an excellent plate, taken from a photograph by Mr. Barnard, of the spectroscope on the great 36-inch refractor of the Lick Observatory. In the description of the instrument it is stated that the spectroscope itself is no less than 130 pounds in weight, while the two brass rods which connect it to the telescope form an extra addition of 75 to 80 pounds. Accompanying the plate, which shows the general arrangement of its parts, is a plan of the instrument which is completely described in the text. Many ingenious ideas have been displayed in the completion of the instrument as regards accessories, such as that of the lighting up of the pointers and production of the comparison-spark. Owing to the great focal length of the telescope, only 1·06 inch of the full aperture of the spectroscope can be used, but when it is dismounted, it rests on a truck, and its full aperture, 1·50 inch, is then available for laboratory work.

A BRIGHT COMET.—A circular from the Royal Observatory, Edinburgh, communicated by Dr. Ralph Copeland, and dated March 11, contains the following information of the appearance of a bright comet:—

Dr. L. Swift discovered a bright comet at Rochester, N.Y., at 16h. 50·1m. local mean time, on March 6, its place then being R.A. 18h. 59m., South Declination 31° 20′. It was moving eastwards.

The exact place of the comet was observed at the Royal Observatory, Cape of Good Hope, on the 8th inst. to be:—

Cape of Good Hope Mean Time	16h. 58m. 36s.
Right Ascension	19h. 2m. 27·8s.
South Declination	30° 2′ 54″

Astronomische Nachrichten, No. 3079, also contains some information with regard to this comet. A telegram from Boston contained the following: "Comet Swift was observed by Barnard, March 8·0399 G.M.T.; R.A. app. = 285° 51′ 20″, Polar Distance = 120° 32′ 53″. Comet is visible to the naked eye."

Another telegram from Capetown read: "Comet was observed March 9·6024 G.M.T.; Right Ascension = 287° 45′ 50″, P.D. app. = 119° 16′ 12″. R.A. March 8 read, 286° 36′ 57″ instead of 285° 36′ 57″."

Prof. H. Kreutz in the same number gives the following elements and ephemeris:—

Elements.

T = 1892 March 26·8545 Berlin M.T.

$$\left.\begin{aligned}\omega &= 9^\circ\ 57'\cdot21\\ \Omega &= 239\ 33\cdot76\\ i &= 33\ 38\cdot44\end{aligned}\right\}\ \text{M. Eq. 1892·0}$$

log. q = 0·02208.

Ephemeris for 12h. Berlin M.T.

1892.	α. h. m. s.	δ. ° ′
March 9	19 10 25	− 29 22·9
13	29 13	25 57·8
17	46 57	22 22·0
21	20 3 41	18 39·0
25	19 26	14 52·6
29	34 17	11 5·9
April 2	20 48 19	− 7 21·9

CALCULATION OF TRAJECTORIES OF ELONGATED PROJECTILES.

AS the correct determination of the law of resistance of the air to the motion of elongated projectiles is a matter of considerable national importance, I hope you will allow me to offer a few concluding remarks explanatory of the present state of the case. Some results of my first systematic experiments were published in 1868, and General Mayevski, after applying a few tests of his own, adopted my results in his "Traité de Balistique Extérieure," 1872. Siacci made use of these results in his ballistic tables (1881). But Mayevski appears to have recently become a disciple of Krupp, from the diagram in NATURE, August 28, 1890 (p. 411), where the dotted line (1) represents roughly the resistance of the air to ogival-headed projectiles given in my "Final Report," 1880; line (2) represents the law of resistance deduced from these results by Major Ingalls, of the United States Artillery, which is similar to the law deduced by me (NATURE, April 29, 1886, p. 605); and line (3) represents the results Mayevski professes to have deduced from Krupp's Meppen experiments. My law of resistance has been very closely followed throughout by Mayevski, as is evident from the diagram above referred to, which is suggestive of a free use of the parallel ruler. The main object of these proceedings seems to have been to persuade the world, and the Americans especially, that Krupp guns are far superior to English guns, regard being had to the initial steadiness imparted by them to their projectiles. But this claim is unworthy of notice so far as it depends upon the Meppen experiments with chronoscopes, the patent defects of some of which were pointed out in NATURE, April 29, 1886 (p. 606). If, however, Government consider this matter worthy of investigation, there are simple practical methods of determining the comparative steadiness of projectiles fired from two or more guns.

At present, my concern is with English guns only, and I wish to point out, as briefly as possible, (1) that my results obtained from English guns are quite correct; (2) that the coefficients of resistance for each round are expressed by such a short unit of time that they are made to appear more irregular than they are in reality, while the variation in their value is just what experiment leads us to expect; and (3) that when my *mean* coefficients are *fairly* used to calculate results of *good* experiments made with *recent* English guns, in *calm* weather, the agreement between calculation and experiment is perfectly satisfactory.

My chronometric arrangements were made with a view to guard against the errors of remaining magnetism, which is the chief source of error in the measurement of extremely short intervals of time by the help of electro-magnetism. All the time-records were made by one electro-magnet, whose galvanic current was interrupted once a second by the swing of a half-seconds clock pendulum; and all the screen records were made by another electro-magnet, whose galvanic current was being rapidly interrupted by a self-acting contact-breaker, till the pull of the lanyard turned off the contact-breaker, and then fired the gun, after which the shot momentarily interrupted the galvanic current as it passed each of ten or more equi-distant screens. Also care was taken to reduce the strength of the galvanic currents, so as to leave each electro-magnet only just sufficient power for the performance of its appointed work. Under these circumstances it may be safely assumed that, if there were any errors arising from remaining magnetism, in either clock or screen records, they would be *constant* in each case, and therefore they would have no injurious effect on the result obtained. The records on a 4-inch cylinder were read off by a vernier to the 1/3000 of its circumference; but as the scale of time was in general only 9 or 10 inches to the second, it may be concluded that the records were read off to the 1/2000 of a second at least.

The accuracy of the time and of the screen records was tested by differencing, when slight adjustments were applied to render the first and second order of differences regular to an *additional place of decimals.* The following is a list of the adjustments so applied in seven successive rounds, 146–152, which are fair samples of those applied in the other rounds (1867–68). They are expressed in decimals of the unit read off by the vernier, or of the 1/2000 of a second. In round 258 an example is given of the correction of an occasional erroneous record at screen 6:—

Round	146.	147.	148.	149.	150.	151.	152.	258.
Screen								
1	+0·3	+0·2	−0·2	0·0	+0·2	0·0	0·0	− 0·1
2	−0·5	−0·1	+0·5	0·0	0·0	+0·2	+0·6	+ 0·1
3	−0·3	−0·2	−0·1	0·0	−0·7	−0·3	−0·1	+ 0·2
4	−0·1	−0·1	−1·0	0·0	+0·1	+0·4	−0·1	− 0·6
5	+0·1	+0·2	−0·2	0·0	+1·4	+0·3	+0·7	− 0·1
6	+0·3	−0·2	+0·3	0·0	+0·3	+0·5	+0·3	+32·9
7	−0·4	+1·6	+0·4	0·0	−0·2	0·0	−0·4	+ 0·6
8	−0·1	+0·7	−0·7	−1·0	−0·1		+0·7	+ 0·2
9	+0·2	0·0	+0·1	0·0	+0·6		−0·4	− 0·1
10	−1·5	−0·4	−0·2	0·0	−0·1		+0·2	− 0·1

Here is conclusive evidence of the perfect trustworthiness of the observations made, such as no other ballistic experiments have afforded to my knowledge. When the readings of the screen records required only such slight adjustments as those above indicated, there could be no reasonable doubt about the perfect accuracy of the experiment, and the round was accordingly adopted as good in all cases, unless there was some known disturbing cause, as when the bronze gun expanded, or where the gas check left the shot, &c.

Although the records are read off only to the 1/2000 of a second, we are able to express the coefficients of resistance with much greater exactness through the employment of a long range (1350 feet) where the only absolute errors in time possible are at the two extremities of the range, and the accuracy of each of these readings is tested by the differencing. Supposing the retarding force of the air, acting upon an ogival-headed projectile moving in the direction of its axis with a velocity v f.s., to be expressed by $-2bv^3$, the values of $2000bw/d^2$ corresponding to all velocities from 900 to 1700 f.s. were found by experiment in 1867–68, where w denotes the weight of the projectile in pounds, and d its diameter in inches. Corresponding to a velocity of 1200 f.s., the mean value of $2000bw/d^2$ was found to be 0·0001089. But to avoid the use of so many decimal places, K was subsequently employed to denote $(1000)^3 2bw/d^2 = (1000)^3 w\Delta^2 t/d^2 l^2 = 108·9$, where $\Delta^2 t$ is the second difference of the times at which the shot passed successive equi-distant screens l feet apart, with a velocity 1200 f.s. more or less, and, in the case of the solid 5-inch shot it equals 0″·00124. From this it appears that for the specified shot the time by which the unit of K is expressed is 0″·0000112 in $\Delta^2 t$. Considering the shortness of this unit of time, it seems very natural that some variations should have been found in the experimental values of K for any specified velocity, derived as they have been from both *hollow* and *solid* projectiles fired with *various charges* from 3, 5, 7, and 9-inch guns. If we turn to actual experiment, it is plain that the coefficients of resistance for any given velocity cannot practically retain a constant value for all rounds. For do not we frequently read that shot are "noisy," or "unsteady" in their flight? There was much unsteadiness in the Jubilee rounds; and Captain May, R.N., in speaking of experiments with *recent* guns, remarked, "the range of 500 yards is selected, because at this range shell which *start unsteadily* will have steadied (that is if they ever do so), &c." It is, therefore, quite natural that exact experiment should afford evidence of this unsteadiness.

It now remains to test the value of my mean results by making use of them to calculate the ranges and times of flight of projectiles for comparison with the results of experiments made with *recent* guns. In 1879 some range tables of the 6·3-inch howitzer were forwarded to me to show that my coefficients for *low* velocities did *not* give satisfactory results. As the muzzle velocities in these tables were 332, 507, 628, 697, 740, and 751 f.s., and the elevations varied from 5° to 40°, the trajectories were much curved, so that my general tables were not applicable in these cases. But when the ranges and times of flight were properly calculated by Bernoulli's method, experiment and calculation were found to agree remarkably well. In the same way numerous German range tables (Krupp guns ?) were calculated for muzzle velocities varying from 380 to 774 f.s., which gave very satisfactory results in general. Although there was no allowance for jump or vertical drift in these calculations for *low* muzzle velocities, the calculated often exceeded slightly the experimental ranges, showing that my resistances were perhaps a little too low. The results of each of these comparisons—32 English and 82 German—will be found in my "Final Report," 1880, pp. 45–47. For specimens of the best and worst results of each kind, see NATURE, April 29, 1886 (p. 606). Now Mayevski proposes to reduce these coefficients of resistance, already rather too low, by 20 per cent. more! (Ingalls, pp. 29, 36).

In consequence of the Krupp scare, the authorities desired to have the accuracy of my results tested by practice on a *long* range, with a *recent* gun, and for this purpose *they selected* the 4-inch B.L. gun. Careful experiments were subsequently carried out with this gun (1887), which showed that my coefficients of resistance were perfectly satisfactory. But there was no real necessity for any special experiments to be made with this gun, as its own range table was afterwards found to be abundantly sufficient for the purpose of testing my results. By calculating trajectories carefully by Bernoulli's method, and then recalculating by the general tables the time of flight over the range already obtained, and also the striking velocity, it is found that the general tables may be used for elevations of the 4-inch gun as high as 15°, or even more, with a muzzle velocity of 1900 f.s. In this way the merest tyro may test my coefficients for his own satisfaction by calculating the times of flight over ranges of two or three miles given in any good range table for a high muzzle velocity. The following are the results of such testing, using the full extent of the range table of the 4-inch B.L. gun, chosen by the authorities. Muzzle velocity, 1900 f.s.; weight of ogival-headed shot (two diameters), 25 pounds :—

Range	2000.	3000.	4000.	5000.	6000.	7500 yds.
Horizontal muzzle vel.	1898·5	1895·0	1888·0	1875·0	1853·5	1796·8 f.s.
Horizontal striking vel.	1124·9	955·4	843·4	751·2	667·6	556·5 f.s.
Exp. time	4″·21	7″·20	10″·49	14″·3	18″·4	26″·2
Calc. time	4·17	7·10	10·48	14·30	18 66	26·43
Difference	−0·04	−0·10	−0·01	0·00	+0·26	+0·23

Here, as before, the calculated time is rather too short for velocities 1900 to 751 f.s. And if we allowed for a slight diminution of the density of the air for the higher elevations, as we ought to do, the calculated would throughout fall very slightly short of the experimental times of flight. Thus it is clear that my coefficients of resistance give perfectly satisfactory results when fairly tested by *recent* guns, chosen by Government, for velocities 330 to 751 f.s., and from 751 to 1900 f.s., or from 330 to 1900 f.s.

In the same way we may use the model range table, carefully prepared for the 12-inch B.L. gun by Captain May, R.N., for a muzzle velocity 1892 f.s., and weight of shot 714 pounds (Proc. R.A. Inst., 1886, p. 356) :—

Range	1000.	2000.	3000.	4000 yards.
Experimental time ...	1″·66	3″·47	5″·44	7″·61
Calculated time ...	1·654	3·457	5·428	7·591
Difference	−0·006	−0·013	−0·012	−0·019

Here, again, the calculated times of flight, being a trifle too short, show that my coefficients of resistance are very slightly too low.

When coefficients tested in this manner give calculated times of flight accurately over ranges gradually increasing up to two or three miles, those coefficients must be correct for all practical purposes, and they will give correctly the striking velocity and time of flight for any other reasonable distance from the gun.

The tables of "Mayevski nach Siacci," printed by Krupp, 1883, may be used to calculate the times of flight of the shot fired from the 4-inch gun as above :—

Range ...	2000.	3000.	4000.	5000.	6000.	7500 yds.
Experimental time	4″·21	7″·20	10″·49	14″·3	18″·4	26″·2
Calculated time ...	4·10	6·91	10·11	13·69	17·67	24·58
Difference ...	−0·11	−0·29	−0·38	−0·61	−0·73	−1·62

From this it is evident that the reduction of my coefficients proposed by Mayevski on the strength of Krupp's experiments is uncalled for.

Again, it has been urged that my resistances ought to be reduced in order to adapt them to *recent* guns, which, it is assumed, impart an increased degree of steadiness to their projectiles. But that assumption requires proof. After most carefully testing the admirable range tables of the 4-inch and 12-inch B.L. guns, I have failed to find any indication whatever of increased steadiness in their projectiles. Besides, Admiral Robert A. E. Scott wrote to the *Morning Post* (November 9, 1889), condemning the system of rifling the 110-ton gun, which he blamed for causing the projectiles to "issue from their guns with a *very unsteady motion*." He then went on to notice the large number of unsteady shot fired from the 9·2-inch gun in 1888. I would also remind my critics that my coefficients of resistance for velocities 1000 to 1700 f.s. were derived from experiments made in 1867–68, while all those for velocities less than 1000 f.s., and greater than 1700 f.s., were derived from experiments in 1878–80, carried out with some of the newest and best guns of the time. As conclusive evidence of the excellence of the 3, 5, and 7-inch guns used in the early experiments, reference may be made to the fact that, from the results of the experiments of 1867–68, I was able to deduce the Newtonian law of resistance for velocities 1350 to 1700 f.s. (Proc. R.A. Inst., 1871); and using the mean of the eight numerical coefficients there given for velocities 1350, 1400, . . . 1700 f.s., the numerical value of k will be found to be 143·9.

In 1879 experiments were made with a *new* Armstrong 6-inch B.L. gun, with velocities 1700 to 2250 f.s. (Reports, &c., Part ii., 48); and again, in 1880, further experiments were carried out with a *new* Armstrong 8-inch B.L. gun, with velocities 2250 to 2800 f.s. (Final Report, 56). Combining these three sets of experiments, Major Ingalls found that the Newtonian law of resistance held good for velocities 1330 to 2800 f.s., where $k = 142·1$ (Ext. Bal., 36). I also deduced the same law for velocities 1300 to 2800 f.s., where $k = 141·5$ (NATURE, 1886, p. 606). And lastly, after a thorough revision of the reduction of every round, I finally adopted the same law for velocities above 1300 f.s., where $k = 141·2$.

Hence it appears that the early experiments of 1867–68 were so accurate that they gave a correct law of resistance for velocities 1350 to 1700 f.s., which has since been found to hold good for velocities 1300 to 2800 f.s.; and they also gave the coefficient $k = 143·9$ (with *studded* shot) sufficiently accurate for all practical purposes up to a velocity 2800 f.s. This is conclusive evidence of the steadiness of the shot in the early experiments, and of the accuracy of the method of reduction of those experiments.

But when those coefficients, which have been found correct by the use of the general tables, are employed to calculate trajectories of elongated shot moving with high velocities, the calculated ranges and times of flight gradually fall more and more below those quantities given in the range tables, as the elevation increases beyond 4° or 5°. These defects are generally only small when the variation in the density of the air is taken into account; but their presence indicates some slight disturbing cause independent of the coefficients of resistance. We can now make use of the exact method of calculating trajectories given by modern analysis, which was first published by J. Bernoulli. But this method applies with strictness only to the motion of a spherical projectile, whose centre of gravity coincides with its centre of figure. Many years ago Count St. Robert remarked: "On doit en conclure que les formules ordinaires de la balistique ne peuvent représenter la trajectoire décrite par les projectiles *allongés*" (Balistique, p. 183). Also Mayevski has published an elaborate paper, "De l'Influence du Mouvement de Rotation sur la Trajectoire des Projectiles oblongs dans l'Air" (Technologie Mil., 1866, pp. 1–150), which, however, leads to no useful result beyond showing that the author recognized the effect of drift on the form of the trajectory. The chief cause of the difficulty is this. For a short time after a steady elongated shot has left a rifled gun, the shot preserves the parallelism of its axis, and in consequence of the action of gravity the point of the shot gradually rises above its trajectory till the resistance of the air causes the axis of the shot to begin to describe a conical surface, with nearly constant vertical angle, about the moving tangent to the trajectory. Consequently, soon after a steady elongated shot leaves the muzzle of a rifled gun, the resistance of the air acting on the inclined under side of the shot, begins to raise the shot bodily, and continues to do so until its axis has made one-fourth of a revolution about the tangent to the trajectory. This vertical drift, near the gun, causes the shot to move in its path as if it had been fired at a slightly increased elevation. Consequently, the observed range and time of flight are each somewhat greater than that due to the elevation at which the gun was laid.

Another difficulty, common, however, to both spherical and elongated shot, is caused by the jump of the gun. In the range tables of the 4 and 12-inch guns above considered, six minutes were allowed for the effect of jump for all elevations. But Major Ingalls remarks that "it varies in value from an angle too small to be appreciable, to one of a degree of arc or even more. . . . It also varies somewhat with the angle of elevation." In one of his examples he supposed the jump to be 22 to 23 minutes. Although the jump and vertical drift are uncertain in amount, they have considerable influence on the range and time of flight, and on this account the calculation of trajectories is a decidedly unsatisfactory method of testing the coefficients of resistance of the air to elongated projectiles.

In the early days of elongated projectiles the vertical drift caused by the "kite-like action" of the shot was duly recognized, but of late this disturbing force has been commonly ignored. For now, when a calculated range is shorter than the experimental range, it is at once assumed that the theoretical resistance is too high. This resistance is forthwith reduced so as to make the calculated agree with the experimental ranges, but seldom is any care taken to compare the time of flight calculated with this reduced resistance with the experimental time. It ought, however, to be remembered that, while a reduction of resistance *increases* the range it *diminishes* the time of flight over a given range. Major Ingalls has given a complete example of this method of correcting my resistance (Problems, &c., p. 151). For elevations of

2°, 4°, 6°, 8°, and 10°,

he found it necessary to reduce my coefficients by

4·5 2·3 5·2 8·1 9.7 per cent.,

in order to obtain the experimental ranges by calculation; and these *reduced* resistances gave the calculated times of flight *too short* by

0″·09 0″·12 0″·13 0″·26 and 0″·42,

which proves clearly that the theoretical resistances had been too much reduced throughout. Also, if the method of correction pursued in the above example was correct, it would follow that the coefficient of resistance is a function of the elevation, which is simply absurd.

On the other hand, suppose we correct the elevation so as to make the calculated agree with the experimental range, which seems to me to be a satisfactory *approximate* correction in such cases. A careful calculation of the trajectory of an ogival-headed shot (two diameters) fired from the 4-inch B.L. gun, at an elevation of 15°, gave a range of 6448 yards (185 yards too short by the range table), and time of flight 20″·46 (1″·10 too short), the density of the air having been supposed to vary with the height of the shot. Now, corresponding to an elevation of 14° 16′, the range table gives a range of 6448 yards, and time of flight 20″·53. If we suppose that the elevation of the gun, 14° 16′, was practically increased by 44′ by jump and vertical drift, we obtain an elevation of 15° for the initial direction of the shot. But, according to calculation, for an elevation of 15° we have found the range 6448 yards exactly, and the time of flight 20″·46, which is only −0″·07 in error, and the calculated horizontal striking velocity is 646 f.s. This, I maintain, is the proper method of correction, because it corrects both *range* and *time* of flight, when there is no wind. Using the general tables and the horizontal muzzle velocity, the calculated time over 6448 yards is found to be 20″·53, and horizontal striking velocity 647 f.s., where $\tau = 0{\cdot}967$, the mean density of the air, as the projectile would rise to a height of 1800 feet.

If the above be a correct view of what takes place, it follows that the axis of the shot during its flight is inclined at such a small angle to the direction of its motion, that the resistance of the air to its forward motion is not sensibly greater than when it moves in the direction of its axis. But small as this angle must be, we find evidence of the marked effect of the lateral action of the air in causing the shot to drift to the right towards the end of the range. It is therefore to be expected that the resistance of the air, acting from below on the shot, soon after it leaves the gun will raise the shot upwards, and cause it to move as if it had been fired at an elevation a little greater than that at which the gun was laid.

The Ordnance Committee fired some ogival-headed projectiles from a 9·2-inch wire gun at high elevations in 1888, professedly to try whether calculations of trajectories at very long ranges are trustworthy. But before experimenting they invited calculators to furnish them with the calculated range and time of flight of a 380-pound elongated shot fired at an elevation of 40° with a muzzle velocity of 2360 f.s. My calculations for an ogival head, struck with a radius of one diameter and a half, gave a range of 19,436 yards, and time of flight 62″·15, which were sent in in March 1888. Allowing 300 yards for jump and vertical drift, I obtained a range of 19,736 yards. In April two rounds were fired with a velocity of 2375 f.s. at an elevation of 40°, which gave ranges of 21,048 and 21,358 yards. My reply to this announcement, by return of post, was that the ranges were about 1500 yards too great. In July the experiment was repeated, which gave ranges of 20,236 and 20,210 yards, being a reduction of near 1000 yards. Two rounds fired at an elevation of 30° gave ranges of 17,500 and 18,344 yards; two at 35° gave ranges of 19,420 and 18,963 yards; and one at 45° gave a range of 21,800 yards. The times of flight have not been published. These great variations in range were due in part to unsteadiness in the motion of the shot, but chiefly to the prevalence of high favourable winds.

In order to test guns or coefficients of resistance in a satisfactory manner, *calm weather* is absolutely necessary. And if the shot is expected to rise to a height of two or three miles, trial balloons ought to be sent up to test the state of the currents in the higher regions. Afterwards, when good mean results of experiments have been obtained, then, and not till then, may these mean results be used to test the results of calculation. Experiments ought not to be made at all unless precautions necessary to secure a correct result can be taken.

I have calculated a complete range table for the 9·2-inch gun for elevations 1° to 45°, according to the original programme of the Ordnance Committee (NATURE, September 13, 1888, p. 468), where the net results of calculation have been given. These ranges will require an addition perhaps of about 2 per cent. for jump, vertical drift, &c., and a further 1 per cent. if the ogival head be struck with a radius of two diameters instead of one and a half, and the increment of time must be ruled by this increment of range.

My sole object having been to obtain the correct law of resistance of the air to the motion of projectiles, I was always ready to consider any proposed correction. But my results, obtained from numerous and most exact experiments, could be changed only on perfectly satisfactory evidence. That evidence I have failed to obtain in any single case, so that my results remain practically the same as they were given in my original reports, 1868–80. I have recently published a revised account of all my experiments, accompanied by newly calculated general tables, for both English and French measures, and other tables required in the calculation of trajectories, according to the results of modern analysis. With these helps I have now thoroughly tested my final results by the use of range tables of the 4-inch, 6·3-inch, and 12-inch guns, with the most gratifying results. And Major McClintock, R.A., has tested my coefficients for small-arm bullets, with very satisfactory results (Proc. R.A. Inst., xii. 569). This evidence of the accuracy of my results is the more valuable because it is derived from Government experiments, made for other purposes, which have manifestly been carried out with great care and ability. Anyone so disposed has the means to re-examine the whole matter for himself. If there be not some error in my calculations, it appears that my results do not admit of any real improvement, and consequently my labours in this matter may be considered to have reached a satisfactory conclusion.

F. BASHFORTH.

FORTHCOMING SCIENTIFIC BOOKS.

THE following is a list of scientific works which will be issued by various publishers in the course of the spring:—

Messrs. Macmillan and Co.:—"Essays on some Controverted Questions," with a Prologue, by Prof. Huxley; "The Beauties of Nature," by Sir John Lubbock, F.R.S., illustrated; "Island Life; or, The Phenomena and Causes of Insular Faunas and Floras," including a revision and attempted solution of the problem of geological climates, by A. R. Wallace, with illustrations and maps, new and cheaper edition; "The Apodidæ," a morphological study, by Henry M. Bernard, illustrated (Nature Series); "Experimental Evolution," by Henry de Varigny; "The Diseases of Modern Life," by B. W. Richardson, F.R.S., new and cheaper edition; "The Geography of the British Colonies"—"Canada," by George M. Dawson, "Australia and New Zealand," by Alexander Sutherland (Macmillan's Geographical Series); "Scientific Papers," by Oliver Heaviside; "The Algebra of Co-Planar Vectors and Trigonometry," by R. B. Hayward, F.R.S., Assistant Master at Harrow; "Key and Students'

Companion to Higher Arithmetic and Elementary Mensuration," by P. Goyen, Inspector of Schools, Dunedin, New Zealand; "Arithmetic for Schools," by Barnard Smith, late Fellow and Bursar of St. Peter's College, Cambridge; carefully revised in accordance with modern methods by W. H. H. Hudson, Professor of Mathematics, King's College, London; "Blowpipe Analysis," by J. Landauer, authorized English edition by J. Taylor and W. E. Kay, of the Owens College, Manchester, new edition, thoroughly revised with the assistance of Prof. Landauer; "Nature's Story Books," I. "Sunshine," by Amy Johnson, illustrated.

The Clarendon Press:—"Mathematical Papers of the late Henry J. S. Smith, Savilian Professor of Geometry in the University of Oxford," with portrait and memoir, two vols.; "Plane Trigonometry without Imaginaries," by R. C. J. Nixon; "A Treatise on Electricity and Magnetism," by J. Clerk Maxwell, new edition; "A Manual of Crystallography," by M. H. N. Story-Maskelyne; "Elementary Mechanics," by A. L. Selby; "Weismann's Lectures on Heredity," Vol. II., edited by E. B. Poulton, F.R.S.; "Epidemic Influenza," by F. A. Dixey.

The Cambridge University Press:—"A Treatise on the Mathematical Theory of Electricity," by A. E. H. Love, Fellow of St. John's College, Cambridge, two vols., Vol. I. in the press; "The Origin of Metallic Currency and Weight Standards," by W. Ridgeway, Professor of Greek, Queen's College, Cork, and late Fellow of Gonville and Caius College; "Solutions of the Examples in 'A Treatise on Elementary Dynamics,'" by S. L. Loney, formerly Fellow of Sidney Sussex College, Cambridge.

Messrs. Longmans and Co.:—"Darwin and after Darwin: an Exposition of the Darwinian Theory, and a Discussion of Post-Darwinian Questions," by George John Romanes, F.R.S., two vols.

Messrs. A. and C. Black:—"Life in Motion; or, Muscle and Nerve," a series of lectures delivered at the Royal Institution, Christmas, 1891, by John Gray McKendrick, F.R.S., illustrated.

Messrs. J. and A. Churchill:—"A Treatise on Hygiene, edited by Thomas Stephenson and Shirley F. Murphy, in two vols., with numerous illustrations, Vol. I. nearly ready; "Chemical Technology; or, Chemistry in its Applications to Arts and Manufactures," edited by Charles E. Groves, F.R.S., and William Thorp (with which is incorporated "Richardson and Watts' Chemical Technology"), Vol. II. "Lighting: Fats and Oils, Candles, Stearine, Gas, Electric Lighting"; "Materia Medica, Pharmacy, Pharmacology, and Therapeutics," by W. Hale White; "The Student's Guide to Diseases of the Nervous System," by J. A. Ormerod, with 66 illustrations; "A Dictionary of Psychological Medicine, giving the Definition, Etymology, and Synonyms of the Terms used in Medical Psychology, with the Symptoms, Pathology, and Treatment of the Recognized Forms of Mental Disorder, together with the Law of Lunacy in Great Britain and Ireland," in two vols., edited by D. Hack Tuke.

Messrs. Whittaker and Co.:—New volumes of the Specialists' Series—"Lightning Conductors and Guards," by Oliver J. Lodge, F.R.S., with numerous illustrations; "The Dynamo," by C. C. Hawkins and F. Wallis, with numerous original diagrams; "A Guide to Electric Lighting," by S. R. Bottone, for householders and amateurs, with 77 illustrations. Whittaker's Manual Instruction Series—"Manual Instruction: Woodwork," by S. Barter, Organizer and Instructor for the London School Board, and to the Joint Committee on Manual Training of the School Board for London, the City and Guilds of London Institute, and the Worshipful Company of Drapers, with over 300 illustrations; "Leather Work, Stamped, Moulded, and Cut, Cuir-Bouillé, Sewn, &c.," by Charles G. Leland, author of "Wood Carving," with numerous illustrations. Whittaker's Library of Popular Science—"Mineralogy," by Dr. F. Hatch, with numerous illustrations; "Chemistry," by T. Bolas, with many illustrations.

Messrs. Sampson Low and Co.:—"Answers to the Questions on Elementary Chemistry, Theoretical and Practical (Ordinary Course), set at the Examinations of the Science and Art Department, South Kensington, 1887 to 1891," by John Mills, two vols., fully illustrated; "Chemistry for Students, consisting of a Series of Lessons based on the Syllabus of the Science and Art Department, and specially designed to facilitate the experimental teaching of Elementary Chemistry in Schools and Evening Classes," by John Mills, numerous illustrations; "Decorative Electricity," by Mrs. J. E. H. Gordon, with a chapter on Fire Risks by J. E. H. Gordon, and numerous illustrations by Herbert Fell, engraved on wood by J. D. Cooper; "Examination of Soils," by W. T. Brannt.

Messrs. George Philip and Son:—"Makers of Modern Thought; or, Five Hundred Years' Struggle (A.D. 1200 to A.D. 1699) between Science, Ignorance, and Superstition," by David Nasmith, in two volumes; "Christopher Columbus," by Clements R. Markham, Vol. VII. of "The World's Great Explorers and Explorations"; "The Development of Africa," by Arthur Silva White, new and cheap edition, revised to date, with fourteen coloured maps; "Philips' General Atlas," entirely new and revised edition, with several additional maps; "Philips' Systematic Atlas," for higher schools and general use, a series of physical and political maps of all the countries of the world, with diagrams and illustrations of astronomy and physical geography, specially drawn by E. G. Ravenstein; "Philips' Atlas of Astronomy," a series of seventy-two plates, with notes and index by Sir Robert Stawell Ball, F.R.S., Royal Astronomer of Ireland; "Tourists' Handy Volume Atlas of Europe," a series of coloured maps, with notes, plans of cities, and complete consulting index, by J. G. Bartholomew.

Messrs. Swan Sonnenschein and Co.:—"Animal Colouration," by Frank Beddard, Prosector to the Zoological Society, with four coloured plates by P. J. Smit, and numerous woodcuts; "Text-book of Embryology: Man and Mammals," by Dr. Oscar Hertwig, of the University of Berlin, translated and edited from the third German edition by Dr. E. L. Mark, Professor of Anatomy in Harvard University, fully illustrated; "Text-book of Embryology: Invertebrates," by Drs. Korschelt and Heider, of the University of Berlin, translated and edited by Dr. E. L. Mark, Professor of Anatomy in Harvard University, and Dr. W. M. Woodworth, Assistant Professor in Harvard University, fully illustrated; "Text-book of Geology," adapted from the work of Dr. Kayser, Professor in the University of Marburg, by Philip Lake, of St. John's College, Cambridge, fully illustrated; "The Geographical Distribution of Disease in England and Wales," by Alfred Haviland, with several coloured maps; "A Treatise on Public Hygiene and its Applications in different European Countries," by Dr. Albert Palmerg, translated, and the English portion edited and revised, by Arthur Newsholme, fully illustrated; "The Photographer's Pocket-book," by Dr. E. Vogel. "Introductory Science Text-Books," additions—introductions to the study of "Zoology," by B. Lindsay, illustrated; "The Amphioxus," by Dr. B. Hatschek, of the University of Vienna, and James Tuckey, of the University of Durham, illustrated; "Geology," by Edward B. Aveling, Fellow of University College, London, illustrated; "Physiological Psychology," by Dr. Th. Ziehen, of the University of Jena, adapted by Dr. Otto Beyer, with twenty-two figures.

Messrs. Crosby Lockwood and Son:—"A Hand-book of Brewing, a Practical Treatise for the use of Brewers and their Pupils," by Herbert Edwards Wright; "A Treatise on Earthy and other Minerals and Mining," by the late D. C. Davies, third edition, revised and very considerably extended by his son, E. H. Davies; "Fuels: Solid, Liquid, and Gaseous, their Analysis and Valuation," for the use of chemists and engineers, by H. J. Phillips, second edition, revised and much enlarged.

SCIENTIFIC SERIALS.

THE most important article in the numbers of the *Journal of Botany* for January and February is one by the late Dr. A. Barclay on rust and mildews in India. He shows that the years in which the grain-crops were deficient have been those in which the climatal conditions were favourable to the growth of parasitic fungi. The chief enemy to wheat in India is *Puccinia rubigo-vera*; and it is an interesting fact that while the æcidioform of this Uredine occurs in Europe on Borraginaceous plants, no æcidium is known in India on any species of the order. Dr. Barclay believes that its life-history has a different course in India from that taken in Europe.—Mr. W. G. Smith reports the progress at present made in the commission which he has received from the Trustees of the British Museum to make a series of water-colour drawings of the whole of the British *Basidiomycetes* for the public gallery of the Department of Botany.—A series of papers is commenced in these numbers on the first records of British flowering plants, by Mr. W. A. Clarke.

The *Bullettino* of the Italian Botanical Society is now published apart from the *Nuovo Giornale*. The first number contains reports of the papers read at the annual meeting, held at Naples in August, and of the regular meetings held since till the end of the year, and of the discussions which followed. Among the more noteworthy papers may be mentioned the following:—On a new carpellary theory, by Signor F. Pasquale, who maintains that the carpel is not derived, as has been generally supposed, from the modification of a single leaf, but from the concrescence of two, or sometimes of three leaves, which unite in the formation and nutrition of the ovules and seeds.—On the floral structure and process of pollination in some species of *Nigella*, by Dr. A. Terracciano.—On the period of formation of the inflorescence within the bud of the vine, by Signor U. Martelli.—On the non-sexual propagation of *Cynomorium coccineum*, by the same writer, who has established its parasitism on *Atriplex nummularia*.—Prof. G. Arcangeli also describes the results of experiments on the cultivation of this plant, which he finds to be parasitic on many hosts.—On earthquakes and vegetation, by Signor A. Goiran. He finds the effects of seismic motions in the earth to be the more rapid germination of seeds, as well as a more rapid growth of the young plant.—Signor E. Tanfani has a paper on the teaching of botany in gymnasia, which he considers to be in a very backward state in Italy.

The *Botanical Gazette* for January contains two interesting original papers:—Herr A. F. Foerste speaks of the relationship of autumn- to spring-blossoming plants, and concludes that late autumn-flowering plants may be divided into two classes—those which have developed from summer-flowering plants by the increase in the number of internodes with their appendages and the gradual retardation of growth, and those which have developed from spring-blossoming plants by the premature development of buds destined to flower during the ensuing spring.—Mr. H. L. Russell discourses on the effect of mechanical movement on the growth of certain plants. The experiments were made chiefly on certain yeast-fungi; and the general results were that the development of filaments was hindered by shaking; but that strong agitation greatly increases the activity of cell-division, while it diminishes the intensity of fermentation. This may be compared with the fact mentioned above relative to the effect of earthquakes on the growth of plants.

The greater part of the number of the *Nuovo Giornale Botanico Italiano* for January is occupied by a paper by Signor A. Jatta, on the Lichens of Italy, accompanied by a very elaborate bibliography.—Signor C. Massolongo has a note on a floral monstrosity in *Jasminum grandiflorum*; and Dr. R. Cobelli a paper on the movements of the flower and fruit of *Erodium gruinum*. These movements belong to three organs—the calyx, the upper portion of the style, and the mericarp—and do not appear to be in any way connected with the pollination of the flower, since the species is apparently self-fertilized, and no pollinating insects were observed at any time upon it.

SOCIETIES AND ACADEMIES.

London.

Physical Society, February 26.—Prof. W. E. Ayrton, F.R.S., Past President, in the chair.—Prof. S. P. Thompson, F.R.S., read a paper on modes of representing electromotive forces and currents in diagrams. The author said he had found it advantageous in some cases to depart from the usual methods of representation, and he now brought the subject before the Society in order to have it discussed and improvements suggested. To indicate the directions of currents in wires seen end-on, Mr. Swinburne had used circles with and without crosses, but no symbol had been suggested for wires not conveying currents. He (Prof. Thompson) thought the plain circle should be used for inactive wires. A circle with a dot in the middle could then be used to indicate that a current was flowing towards the observer, and a circle with a cross in it to represent a wire conveying a current away. These meanings could be recalled by considering the direction indicated by an arrow, the dot showing the tip of the arrow, and the cross the feathers. Some method of distinguishing between E.M.F. and current was required. For this he proposed to use thin-stemmed arrows with feathers for E.M.F.'s, and thick-stemmed ones without tails for currents. In the case of electrical transmission of energy, this convention had the important advantage that where the two arrows had the same direction, energy was being given to the system, and where the arrows were opposite, energy was leaving it. Mr. Maycock, he said, had recently published a simple rule for finding the direction of magnetic force due to a current of known direction in a wire. Grasp the wire with the right hand, the thumb pointing in the direction of the current; the fingers will then encircle the wire in the direction of the magnetic force. Dr. Fleming's well-known rule for induced currents was also a *right-hand* rule, but as it referred to the direction of *currents*, another rule was necessary when considering motors. By making the rule refer to E.M.F.'s, only one rule was required for generators and motors. For alternating currents the author found it convenient to draw polar curves analogous to Zeuner's valve diagrams. Suppose a line OP (Fig. 1), representing the maximum value of an E.M.F. or

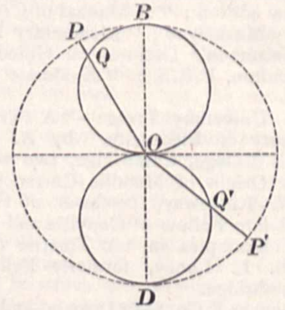


Fig. 1.

current whose magnitude is a sine function of the time, to revolve at uniform velocity about O; the intercepts OQ, OQ', &c., cut off by circles OQB, OQ'D, will represent the magnitudes at the times corresponding to the positions OP and OP'. The effect of lag can also be represented in such diagrams. In cases where the variables are not sine functions, the curves OQB and OQ'D are no longer circles. Polar diagrams representing the E.M.F. and current curves obtained by Prof. Ryan in his transformer experiments were exhibited, and a working diagram, illustrating the changes in three-phase currents, was shown. To show the directions of induced E.M.F.'s in diagrams of dynamos and motors, diagonal shading of the pole-faces was sometimes convenient; the lines over north poles being drawn from left to right downwards in the direction of the middle stroke of the letter N, and those over south poles from left to right upwards. A conductor passing over a north pole from left to right would have an E.M.F. induced in a downward direction, as indicated by the slope of the diagonal lines. This method of representation was used to show the ways of connecting up multipolar drum armatures, the winding being supposed cut along a generating line, unwrapped from the core, and laid out flat in the manner adopted by Fritsche. In connection with armatures, the author said a formula had been published by means of which the nature of a winding consisting of a given number of convolutions, and to be used with a given number of poles, could be predetermined. This, he thought, would be very useful in practice. Mr. Blakesley said the old method of representing alternate current magnitudes by means of the projections of revolving lines, seemed preferable, for it left no ambiguity as to the directions of the quantities. The method of shading the poles also required that the direction in which the diagram was to be viewed should be known before the direction of the E.M.F. could be determined. Mr. Swinburne suggested that the author might use a bow to represent E.M.F., and an arrow for current. He was glad to see that Prof. Thompson recognized the differences in dynamos and motors, and approved of the view that mnemonic rules should refer to E.M.F. and not to current. The diagrams of drum windings would be very useful, and he hoped the author would make the subject clear to ordinary workmen in the next edition of "Dynamo-Electric Machinery." Prof. Perry considered it undesirable to use polar curves for anything but circles. In his opinion it was not sufficiently known that any curve can be split up into a series of sine curves, and each component dealt with separately; the separate results being added together in the end. Mr. Swinburne pointed out that before one could analyze a curve in this way, the curve must be known, and would probably have to be determined experimentally. If means for finding one curve are available, any other required curve could probably be found by the same apparatus; hence there was no need for analysis. Prof. Perry remarked that experiments could not be made on a machine before the machine was built; whereas the E.M.F. curve could be predetermined from its design. By analysis, its current curves, when working under various conditions, could be found. Prof. Ayrton, referring to the mnemonic character of the modes of representation described by Dr. Thompson, suggested that the symbols in the author's book should be more mnemonic. He himself was in the habit of using large letters for currents and small ones for resistances: A and a for the armature, S and s for series, and Z and z for the shunt, currents and resistances, respectively, and σ and ζ for the series and shunt turns. He also found the following "electromotive force" rule very convenient. Draw three rectangular axes, OM, OF, and OE, as shown in Fig. 2. If, then, OF represents the direction of the

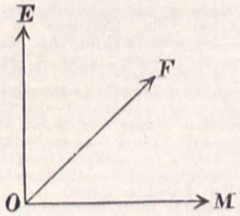


FIG. 2.

force (magnetic), OM that of the motion, then OE shows the direction of the induced E.M.F. Dr. Thompson, in replying, said he thought Mr. Blakesley had misunderstood what had been said, for no ambiguity existed. In describing the windings of armatures, difficulty arose from want of proper names for the various elements, and in his forthcoming work suitable names had been given. To Prof. Ayrton he pointed out that in his book he (Dr. Thompson) had used mnemonic characters, for r_a, r_s, and r_m represented the resistances of armature, shunt, and series magnet coils respectively. The symbol I for current had also been recommended for adoption by the Frankfort Committee. He objected to Greek letters except for specific quantities, such as angles, specific inductive capacities, refractive indices, &c. He appreciated the simplicity of Prof. Ayrton's E.M.F. rule, but thought it would be better to rotate OE and OF through a right angle about OM, thus giving Fig. 3.—A paper on

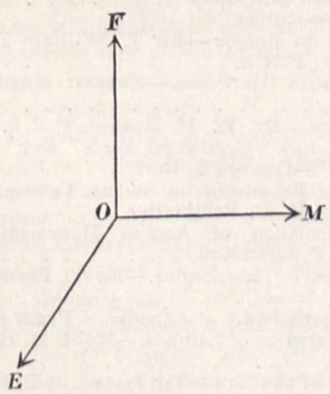


FIG. 3.

the flexure of long pillars under their own weight, by Prof. M. Fitzgerald, was read by Mr. Blakesley. The subject of upright pillars fixed at the base and free at the top is treated mathematically, the differential equation being integrated in two series, involving ascending powers of the variable. Putting L for the ratio of length to diameter, the results, when applied to thin steel tubes and rods, for which Young's modulus is taken as 12,000 tons per square inch, show that the limiting height (in feet) of pillars which can stand without bending is given by $H = \frac{15 \times 10^6}{L^2}$ for tubes; and $H = \frac{7.5 \times 10^6}{L^2}$ for rods. If $L = 100$, the maximum height of a tube is 1500 feet, the diameter being 15 feet. For wires, L may have larger values, and the limiting length of a No. 28 B.W.A. steel wire, is about 10 feet. In the case of pillars whose neutral axes are constrained to be vertical both at top and bottom, the results show that a definite ratio must exist between the bending moments producing the constraints.—A paper on choking coils, by Prof. Perry, F.R.S., and a description of the uses of Rice's choking coils for regulating the brilliancy of incandescent lamps, by Mr. Hammer, were postponed until next meeting.

Linnean Society, March 3.—Prof. Stewart, President, in the chair.—A letter was read from the Home Secretary, conveying the thanks of Her Majesty the Queen for the address of condolence which had been forwarded on behalf of the Society on the death of H.R.H. the Duke of Clarence and Avondale.—The President announced the presentation by Sir Joseph Hooker to the Society of two medallion portraits of Sir James Ross and Dr. John Richardson, whose names are well known in connection with Arctic exploration. The medallions were executed in 1843 by the late Bernhard Smith. A vote of thanks to the donor was passed unanimously.—Mr. Clement Reid exhibited a collection of fossil plants and seeds which he had found associated with the bones of Rhinoceros and other mammals in the neighbourhood of Selsea, and West Wittering. By means of diagrams Mr. Reid showed the exact position of the bed, and described the condition in which the various specimens were deposited.—On behalf of Mr. W. E. Beckwith, of Shrewsbury, Mr. H. Seebohm exhibited a specimen of White's Thrush (*Turdus varius*) which had been shot near Shrewsbury on January 14 last. He pointed out that this species, which inhabits Eastern Asia, belongs to the sub-genus *Oreocincla*, an exclusively Eastern group of ground Thrushes, and is the only one which is Palæarctic and migratory. It does not breed anywhere west of the Yenisei, and its occurrence in Europe is accidental. Mr. Seebohm added that it had been met with twice in France, four times in Italy, three times in Belgium, once or twice in Austria and Prussia, once in Norway, thirteen times in Heligoland (between 1827 and 1884), and about a score of times in the British Islands, including three occurrences in Ireland, and one in the extreme south of Scotland.—On behalf of Mr. A. Craig Christie, the Secretary exhibited some specimens, as was supposed, of *Lycopodium complanatum*, collected in Scotland, on which it was suggested that the plant might be regarded as British. In the opinion, however, of Mr. James Groves, who had carefully examined the specimens, and other botanists present, they were referable to *L. alpinum*. Mr. Groves pointed out the distinctive characters of both. Mr. Carruthers was of opinion that *L. complanatum* had been met with in the south of England, but not within the last ten years. Mr. E. M. Holmes was under the impression he had seen it growing a few years ago near Stroud.—A paper was then read by Mr. A. D. Michael, on variations in the internal anatomy, and especially the genital organs, of the *Gamasinæ*, a typical sub-family of the Acari. In this paper the author gave the results of two years' research, including many hundreds of dissections and serial sections, with lengthy observations of the living creatures. The comparison of variable organs was worked out in numerous species, showing great specific differences. Four of the species were found to be previously undescribed, and for these the names *Hæmogamasus horridus*, *H. nidi*, *Lælaps oribatoides*, and *L. ligoniformis* were proposed.

Royal Microscopical Society, February 17.—Dr. R. Braithwaite, President, in the chair.—Prof. F. Jeffrey Bell said that he had, in accordance with the resolution passed at the last meeting, forwarded a copy of the message of condolence from the Society to the Prince of Wales, to General Sir Dighton Probyn, and he had received the following letter of acknowledgment:—"*Sandringham, Norfolk.*—General Sir Dighton Probyn, Comptroller and Treasurer of the Household, is desired to convey to the members of the 'Royal Microscopical Society' the heartfelt thanks of the Prince and Princess of Wales for the Society's kind resolution, expressing sympathy for their Royal Highnesses in their deep affliction.—*January* 25, 1891."—Mr. Watson exhibited and described a new vertical camera for photomicrography designed upon the same lines as that used by Dr. Van Heurck.—The President then read his annual address, postponed from the last meeting under the special circumstances then mentioned. The subject chosen was the impregnation and modes of reproduction in Ferns and Mosses; diagrams in illustration were exhibited and explained, and specimens were also shown under microscopes.—A cordial vote of thanks, proposed by the Rev. Canon Carr and seconded by Prof. Groves, was given to the President for his valuable address.—Mr. J. J. Vezey moved that the best thanks of the Society be given to its officers, and also to the auditors and scrutineers for their services during the year.—The President declared the motion to be carried by acclamation.—Prof. Bell thanked the Society on behalf of himself and the other officers, at the same time calling attention to the special services rendered by the Treasurer, Mr. Frank Crisp.—The following are the names of the members of the new Council, who met for the first time at this meeting:—President: Dr. R. Braithwaite. Vice-Presidents: Mr. A. W. Bennett, Prof. J. W. Groves, Mr. G. C. Karop, and Mr. A. D. Michael. Treasurer: Mr. Frank Crisp. Secretaries: Prof. F. Jeffrey Bell and the Rev. Dr. W. H. Dallinger. Ordinary members: Dr. Lionel S. Beale, Rev. E. Carr, Mr. James Glaisher, Dr. R. G. Hebb, Mr. E. M. Nelson, Mr. T. H. Powell, Prof. Urban Pritchard, Mr. W. W. Reeves, Prof. C. Stewart, Mr. W. T. Suffolk, Mr. C. Tyler, and Mr. F. H. Ward.

Paris.

Academy of Sciences, March 7.—M. d'Abbadie in the chair.—Fermentation of blood, by MM. Berthelot and G. André. The blood of cattle, defibrinated, was fermented for 130 days in a water-bath at 35°. The paper contains an account of the products of the fermentation. It will be published in greater detail in the *Annales de Chimie et de Physique*.—On the distribution in latitude of solar phenomena observed at the Royal Observatory of the Roman College during the second half of 1891, by M. P. Tacchini. (See Our Astronomical Column.)—Phenomena observed at Kalocsa, on the large group of spots of February 1892, by M. J. Fényi. A prominence, 124" high, was observed in the position 220°–230°, as the recent large spot-group was crossing the limb.—On the impossibility of certain movements, by MM. A. de Saint-Germain and L. Lecornu.—On the movement of a conical pendulum, by M. de Sparre.—On electro-capillary phenomena, by M. Alphonse Berget.—On the co-existence of dielectric power and electrolytic conductivity, by M. E. Bouty. It appears from the experiments described that the dielectric constant only varies slightly under conditions which produce an enormous increase of conductivity. Thus, water and ice have sensibly the same dielectric constant, whilst the conductivity may vary from 1 to 10^5 or 10^6.—On the thermal conductivity of crystalline bodies, by M. Charles Soret.—Rule for finding the number and nature of accidentals of the gamut in a tone and a given mode, by M. Pierre Lefebvre.—On the density of aqueous solutions, by M. Georges Charpy. The author concludes from his results that the variation of the density of a solution, as a function of the concentration, is a complex phenomenon, and cannot be used in studying the state of the dissolved body. There is no reason why the solution at which the maximum density is reached should be regarded as corresponding to a definite hydrate.—Compounds of gaseous ammonia with boron iodide and bromide, by M. A. Besson.—Synthesis of the minerals crocoisite and phœnicite (*phœnicochroïte*), by M. C. Luedeking.—On the value of the primary alcoholic function, by M. de Forcrand.—On the production of quinine di-iodomethoxide from cupreïne, by MM. E. Grimaux and A. Arnaud.—A study of the velocity of decomposition of diazo-compounds by water, by MM. P. Th. Muller and J. Hausser. The law according to which sulphanilic acid is decomposed is expressed by the formula $C = \frac{1}{\theta} \log. \text{nat.} \frac{A}{A - x}$, in which θ is the time, C a constant, A the total nitrogen that can be evolved, and x the amount of nitrogen evolved. C is independent of the concentration.—Action of capryl iodide on trimethylamine in aqueous solution, in equimolecular proportions; formation, when heated, of dimethylcaprylamine; production in the cold of caprylene, by MM. H. and A. Malbot.—New synthesis of tartaric acid, by M. P. Genvresse. (See Notes.)—On the pyloric secretion of the dog, by M. Ch. Contejean.—New rings or intercalary rings of nerve-ducts (*tubes nerveux*), produced by the impregnation of silver, by M. Benjamin Ségall.—On two new species of *Streptothrix*, Cohn, and on the place of this genus in the classification, by MM. C. Sauvageau and M. Radais.—History of the *Garcinia* of the sub-group *Xanthochymus*, by M. J. Vesque.—On the magnetic disturbance and the aurora borealis of March 6, 1892, by M. Th. Moureaux. Disturbances similar to those of February 13–14, but less violent, were registered by the Parc Saint-Maur instruments on March 6–7.—On the magnetic storm of February 13–14, by M. H. Wild. A comparison of the records made by instruments at Pawlowsk with those obtained at Parc Saint-Maur shows that, although the recent magnetic storm commenced at approximately the same time, the variations at the two places were in the opposite directions. Other differences have been observed in the two records.—On the atmospheric, magnetic, and seismic disturbances of February 1892, by M. Ch. V. Zenger. The author has marshalled facts to show that magnetic storms, cyclones, snow-storms, discharges of atmospheric electricity, earthquakes, and volcanic eruptions occur simultaneously.—On three fossil human skeletons found in the Baousse-Roussé grottos, in Italy, by M. Émile Rivière.

BOOKS RECEIVED.

Books.—Phases of Animal Life: R. Lydekker (Longmans).—Meteorological Observations made at the Adelaide Observatory, &c., during the Year 1889 (Adelaide).—The Rationale of Mesmerism: A. P. Sinnett (Kegan Paul).—Zoological Record, 1890 (Gurney and Jackson).—Elements of Economics of Industry: Prof. A. Marshall (Macmillan).—Elementary Mathematical Astronomy: C. W. C. Barlow and G. H. Bryan (Clive).—The Dietetic Value of Bread: J. Goodfellow (Macmillan).—Air and Water: Prof. V. B. Lewes (Methuen).—Le Climat de la Belgique, 1891: A. Lancaster (Bruxelles, Hayez).—Soils and Manures: Dr. J. M. H. Munro (Cassell).—Longmans' School Geography for North America: G. G. Chisholm and C. H. Leete, 2nd edition (Longmans).—Precious Stones and Gems: E. W. Streeter, 5th edition (Bell).—The Oak: Prof. H. M. Ward (Kegan Paul).—The Labrador Coast: Dr. A. S. Packard (Kegan Paul).—Contribution à l'Étude de la Morphologie et du Developpement des Bactériacées: A. Billet (Dulau).—Bateaux et Navires: Marquis de Folin (Paris, Baillière).—Diseases of the Nose: Dr. G. Macdonald, 2nd edition (Watt).—Annals of the Royal Botanic Garden, Calcutta, vol. iii. (Calcutta).—A Short Text-book of Inorganic Chemistry: Dr. H. Kolbe, translated and edited by Dr. T. S. Humpidge, 3rd edition (Longmans).—Laboratory Practice: Dr. J. P. Cooke (Kegan Paul).—Catalogue of the Specimens illustrating the Osteology of Vertebrated Animals recent and extinct, contained in the Museum of the Royal College of Surgeons of England, Part 3, Class Aves: Dr. R. B. Sharpe (Taylor and Francis).

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