THURSDAY, JULY 4, 1889.

CRYPTOGAMIC BOTANY.

A Hand-book of Cryptogamic Botany. By Alfred W. Bennett, M.A., B.Sc., F.L.S., and George Murray, F.L.S. With 378 Illustrations. (London: Longmans, Green, and Co., 1889.)

THE utility of a book presenting in clear outline the present state of our knowledge of the morphology of flowerless plants cannot be doubted. The division of plants into Cryptogams and Phanerogams is, it is true, a non-natural one. As the authors of this hand-book themselves point out, the Vascular Cryptogams are "more nearly allied in many respects to the Phanerogams than to the lower Cryptogams." This fact, however, is not a serious objection to the limitation here adopted. If it is found desirable, for teaching purposes, to treat the vegetable kingdom in two divisions, the line may be as conveniently drawn between the Pteridophyta and the Gymnosperms as anywhere else. The idea of a work dealing with the families of Cryptogamic plants has been familiar to English readers since the publication of Berkeley's famous "Introduction to Cryptogamic Botany" in 1857. Since that date the literature of the subject has accumulated to an incalculable extent. A work of similar scope, written at the present day, demands enormously increased labour in compilation, and offers, perhaps, less room for originality. In the authors' view, the object of the writer of a hand-book is "to gather up and to collate material already existing, winnowing, to the best of his judgment, the wheat from the chaff." The indebtedness of the authors to their predecessors is amply acknowledged, and it may fairly be claimed for the hand-book that the arrangement of the material is to a great extent its own.

The descending order is followed throughout, the Cryptogams being divided into seven groups: Vascular Cryptogams, Muscineæ, Characeæ, Algæ, Fungi, Mycetozoa, and Protophyta. The reappearance of the Characeæ as a main group will be a surprise to those who have become accustomed (and in our opinion rightly) to regard them as green Algæ. The Mycetozoa should probably have been excluded altogether, and the Protophyta, as here limited, form a very heterogeneous collection. On the other hand, we regard the breaking up of the old sub-kingdom, Thallophyta, as a distinct gain. The character on which it was founded is of absolutely no systematic value, and the Algæ and Fungi, at any rate, are groups of sufficient extent and independence to stand by themselves.

The authors have introduced several changes in terminology. The most conspicuous, though not the most important, of these is the adoption of Anglicized terminations for Latin and Greek technical words. This is a matter in which it is hard to draw the line aright; thus we have already become used to "ovule" and "pistil" instead of "ovulum" and "pistillum." But still, as a matter of taste, we think the authors have gone much too far in this direction. They complain of the "awkward and uncouth foreign forms of these words": we should have thought this reproach applied much more strongly to "cœnobe," "sclerote," "nemathece," and "columel." It need hardly be said that the authors themselves have not succeeded in attaining consistency in this matter. Happily, we are still allowed to say "prothallus" and "nucleus," though "nucleolus" has become "nucleole," and "gleba," "glebe."

A more important point is the use of the word *spore*. The authors define a spore as "any cell produced by ordinary processes of vegetation, and not directly by a union of sexual elements, which becomes detached for the purpose of direct vegetative propagation." Thus the word is used in a narrower sense than that of de Bary in his "Fungi," or of Vines in his "Physiology of Plants." The authors' use of the word seems in itself unobjectionable, and is certainly preferable to Sachs's definition; but we do not see that anything is gained by the limitation they propose.

The substitution of "megaspore" for "macrospore" we regard as an improvement, and only hope it may be generally adopted.

For sexual products the authors make use of the termination "sperm" in the place of "spore," thus speaking of zygosperms and oosperms. For the sake of consistency they avoid using words compounded with "sperm" for male cells; thus "antherozoid" is once more substituted for "spermatozoid," while "spermatium" is replaced by "pollinoid." We cannot regard this last name as a happy one, for it suggests a false comparison with pollen-grains. As regards "spermatozoid" and "antherozoid," the reasons on both sides seem very equally balanced, though the former has the great advantage that it emphasizes the homology with the male element in the animal kingdom. The term "spermogonium" is abolished on the additional ground that the "spermogone is a true antherid," a statement which, at least in this general form, is really unwarranted by the facts. The word "reproduction" is limited to "the production of a new individual—that is, to a process of impregnation." This definition seems to us to involve far too much metaphysical hypothesis.

The section on the Vascular Cryptogams is preceded by some useful introductory remarks, in which the homologies with the Phanerogams, and the general course of development, are explained. The statement that in apogamy and apospory "either the oophyte or the sporophyte may be entirely suppressed" is, however, inaccurate; as, in the cases referred to, only the reproductive organs are (wholly or partially) suppressed, and not the generation which bears them.

The Vascular Cryptogams are divided, as in the earlier editions of Sachs's text-book, into a heterosporous and an isosporous series. This arrangement is admittedly provisional, but the difficulty in finding a truly natural arrangement does not seem to us a sufficient reason for adopting one which is manifestly artificial.

The Rhizocarps are the class first described. The only criticism of importance which suggests itself here is that the clear relation of these plants to the isosporous Ferns is insufficiently brought out. The other heterosporous class is the widely different group Selaginellaceæ, including Selaginella and Isoëtes. The following sentence (occurring in the account of the latter genus) is very misleading: "The mode of development of the megaspores presents perhaps the closest analogy to that of the secondary embryo-sacs of Gymnosperms that occurs in any order of Vascular Cryptogams; and the same remark applies to the formation of the microsporanges and pollen-sacs." The structures compared in the latter part of this sentence are homologous; the "secondary embryo-sacs" and megaspores are certainly not so, nor do we see in what sense there is even any "analogy" between them.

The isosporous series is divided into Lycopodiaceæ, Filices, Ophioglossaceæ, and Equisetaceæ. We are glad to see that some account of Treub's classical observations on the sexual generation of Lycopodium is given. In Psilotum the sporangia are described as "plurilocular," and are then said to be "collected into groups of three or four." These two views of the morphology cannot both be true.

The class Filices, as here limited, includes the Marattiaceæ but not the Ophioglossaceæ, an arrangement which we do not think an improvement. At p. 71, the *stem* of Lygodium is wrongly said to be scandent, in contradiction to the correct account given a few pages later on. Slips of this kind are rather frequent in the book, and should be looked to in a future edition.

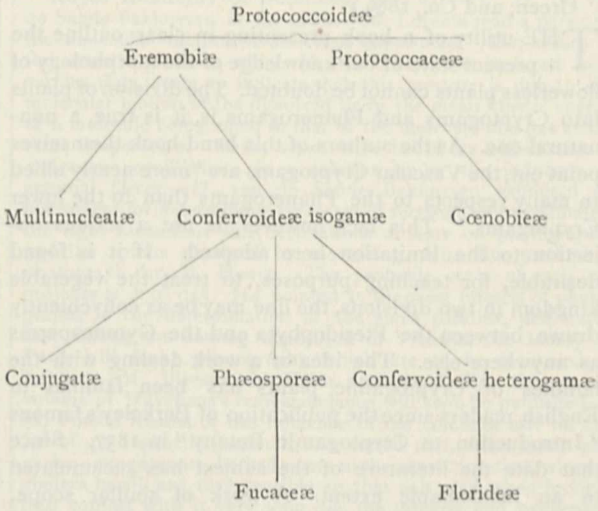
The sections on the Ophioglossaceæ and Equisetaceæ call for no special remark, except that we find Sachs's original description of the division of the spore mother-cells in Equisetum reproduced. The process is a perfectly typical case of cell-division (not "free cell-formation"), and called for no special description here; while Sachs's account, however interesting historically, is now fifteen years out of date.

An interesting notice of fossil Vascular Cryptogams concludes this subdivision. Attention may especially be called to the excellent figure of the stem of Psaronius, which, by the way, is referred to Cyatheaceæ—not, as by Solms-Laubach, to Marattiaceæ. This chapter will be very welcome to English students.

The second subdivision, Muscineæ, is divided as usual into Musci and Hepaticæ. The treatment of these classes, though not very full, is otherwise satisfactory. By an unfortunate mistake, the protonema of the Mosses is twice over described as colourless (pp. 134 and 140). We think that it is undesirable to speak of the archegonia as in any sense "corresponding to the pistil in flowering plants" (p. 141). Expressions of this kind are very likely to mislead the beginner, and the same objection applies to the term "stigmatic cells" for the cells at the apex of the archegonium.

As mentioned above, the Characeæ are separated from the Algæ as a distinct subdivision. The reasons given for this separation do not appear sufficient. Much higher morphological differentiation of the vegetative organs is found among undoubted Algæ, while the great number of Algal characters presented by the Characeæ seem to us to outweigh the points of peculiarity in their reproductive organs. However, the true position of this group is likely long to remain a *vexata quæstio*.

The Algæ themselves are treated at considerable length (120 pages), and the account given is the fullest in any general English work. The authors may be congratulated on being the first in this country to attempt an adequate summary of our knowledge of these plants: as might be expected, however, there are many points which invite criticism. The Algæ are ranged in eight classes: Florideæ, Confervoideæ heterogamæ, Fucaceæ, Phæosporeæ, Conjugatæ, Confervoideæ isogamæ, Multinucleatæ, and Cœnobieæ. The authors' opinion as to the phylogeny of these classes may perhaps be best represented in a tabular form. All Algæ are derived by them from the Protococcoideæ, the latter group being placed among the Protophyta.



On the whole, this arrangement seems to us as good as any which has been proposed, though it is necessarily provisional. We cannot agree that the Dictyotaceæ connect the Fucaceæ with the Phæosporeæ through the Cutleriaceæ. The resemblance between the latter and Dictyotaceæ is a very superficial one. Nor are we satisfied that Coleochæte really marks the transition from oosporous Confervoideæ to Florideæ. Falkenberg has already shown how slender the grounds are for this supposition. We should be disposed to seek the origin of the red seaweeds much lower down in the scale, but in the present state of knowledge it is quite impossible to decide this point.

The Porphyraceæ and Ulvaceæ are included among the Florideæ as degraded forms. The Ulvaceæ, however, have no Floridean characters whatever, and show clear relationships to such Palmelloid forms as Tetraspora, a fact which the authors themselves recognize (p. 418). If, as is possible, Porphyraceæ and Ulvaceæ are really related, the former group will have to be separated again from the Florideæ, with which it has been somewhat too hastily associated on the ground of Berthold's observations.

It is impossible to consider all the authors' classes in detail. As regards the brown Algæ, we do not think it is correct to say that the differentiation of *tissues* in the Laminariaceæ is less strongly developed than in the Fucaceæ, and we entirely decline to believe that the unilocular sporangia of Cladostephus are due to the attacks of parasitic Chytridiaceæ! (p. 250). The Conjugatæ are fully described, but there is some confusion in the use of the words "zygosperm" and "hypnospore" in the order Mesocarpeæ. The class Multinucleatæ corresponds to Siphoneæ in the widest sense. We cannot agree with the authors in regarding Vaucheria as the "culminating genus" of this class. It is inferior to most Siphoneæ in every respect except the heterogamous reproduction.

In the class Cœnobieæ the orders Pandorineæ and Volvocineæ should, we think, have been united, or at least placed in juxtaposition.

A few words on fossil Algæ conclude this section.

The fifth subdivision, Fungi, is very clearly described, in general agreement with de Bary's views. The Fungi are divided into Phycomycetes and Sporocarpeæ, the former including the Oomycetes and Zygomycetes, the latter the Ascomycetes, Uredineæ, and Basidiomycetes. We may note that the Ustilagineæ are included among the Zygomycetes, de Bary's opinion as to the sexuality of the cell-fusions in the former groups being adopted, while Brefeld's opposite view is severely criticized. The phenomena in question are really on the confines of sexuality, and both opinions are tenable. It may be, however, that we have a definite test in the fusion of nuclei, and if Fisch's observations are to be trusted this test goes against the sexuality of the Ustilagineæ.

The account of the Ascomycetes is essentially based on de Bary's work. Due stress is laid on all the cases in which it is possible to find any indication of sexual reproduction. Perhaps it would have been better to point out that there are two sides to the question, and that the views of Brefeld, van Tieghem, and Moeller also have to be taken into account.

The statement that a nucleus has not yet been demonstrated in yeast-cells shows an excess of scepticism. The evidence afforded by Zacharias's observations is as convincing as in the case of any other fungal cells.

The Uredineæ are rather curtly disposed of. In tracing their relationship to the Basidiomycetes, the authors regard the teleutospores and basidiospores as corresponding structures. Several facts may be adduced in support of this view, but on the whole we prefer de Bary's ingenious interpretation, according to which the teleutospores are homologous with the basidia, the promycelium with the sterigmata, and the sporidia with the basidiospores. This comparison rests on very strong evidence, as an inspection of Figs. 130 and 140 in de Bary's "Fungi" will show.

The next subdivision is that of the Mycetozoa. If, as the authors, in agreement with most authorities, state, "we are justified in placing the Mycetozoa outside the limits of the vegetable kingdom" (p. 406), it seems to be time that these organisms were excluded from botanical hand-books.

The last subdivision, Protophyta, is a most heterogeneous group, including the Protococcoideæ, Diatomaceæ, Cyanophyceæ, and Bacteria. In our opinion, the two first-named groups would have been much better placed among the Algæ, and the same probably applies to the Cyanophyceæ, though here there is more room for doubt. This would only leave the Bacteria, and these must eventually accompany the Cyanophyceæ, from which they seem to differ in nothing except the usual absence of chlorophyll. It must be mentioned, however, that the authors themselves fully recognize that the Protophyta are not a natural subdivision.

The work as a whole is a useful summary of Cryptogamic morphology, but there is room for very material emendation. Many inaccuracies occur, some of which have been noticed above, and though these mistakes are no doubt largely due to oversights in revision, they are none the less misleading to the reader. We think also that in some cases, especially as regards the Vascular Cryptogams and the Protophyta, the authors have not made sufficient effort to render their classification as natural a one as possible.

It is especially to be regretted that so few good new figures have been introduced. Nothing adds so much to the freshness and interest of a hand-book as good and original illustrations, while the constant reappearance of familiar text-book figures, however well selected, has become very wearisome, and is unworthy of a living and active science.

We hope that opportunity may be found in a second edition to render this book in every way a satisfactory account of flowerless plants. It should be mentioned that there is a very good and complete index.

D. H. S.

AN ENGLISH RAILWAY.

The Working and Management of an English Railway. By George Findlay, General Manager of the London and North-Western Railway. (London: Whittaker and Co., 1889.)

TO write a good book on the working and management of an English railway is no easy matter. The author of such a work must have an intimate knowledge of his subject, and be in a position to take one of our best English railways for his pattern. It would be quite possible to name more than one railway in this country, the management of which would, if described in a book, serve to illustrate how such a task should not be fulfilled. This, however, does not apply to the present volume. It is a pleasure to read a book on the subject by Mr. Findlay, the General Manager of the London and North-Western Railway, for where would it be possible to find a better managed line than the North-Western Railway?

In a volume of 270 pages, divided into sixteen chapters, the author treats departmentally the various sections of staff and plant necessary for the efficient working and control of this vast system of railways. Chapter ii. deals with the management of the line, and it is evident that this is most thoroughly carried out by means of a system of devolution of responsibility. The author says, "It will thus be seen that the chain of responsibility and supervision is a very complete one, and, in fact, the secret of organizing the management of a great service, such as this, is nothing more than a carefully arranged system of devolution combined with watchful supervision." The staff consists of about 55,000 men, including all grades in the service. It is interesting to note that promotion does not depend on seniority, but solely on merit, the best man for the particular post being chosen; and this no doubt is the only way of getting work well done.

In the chapter on the "Permanent Way" the subject is treated historically. The old cast-iron rails laid on stone blocks are illustrated as a relic of the early days of railways, and the reader is gradually led through the various changes and improvements made therein, until he reaches the standard road of the North-Western of to-day, which is as nearly perfect as it can be made with our present knowledge. Signals and interlocking are efficiently dealt with in chapter v. Then the telegraphs are equally well described in chapter vi. The rolling-stock of a railway forms probably the most important part of the necessary plant, and is affected more than anything else by the gradually increasing speeds and weights of trains. Chapters vii. and viii. deal with this important subject. The standard types of locomotives are very well illustrated, but the descriptions might with advantage have been more detailed. The author discusses the necessity for an increase of power in the passenger locomotives to cope with the increased weight of the principal express trains, and the solution of the problem by the introduction of Mr. Webb's fine compound locomotives. The construction and working of these engines are described clearly enough for the benefit of non-professional readers. It would have been interesting to find an account of the Worsdell compound locomotive, which, although not in use on the North-Western, has many points in its favour. Comparing it with Mr. Webb's engine, many engineers consider it the better engine of the two. Perhaps in a future edition Mr. Findlay might add a description of it, with an illustration.

Carriages and different kinds of rolling-stock are well dealt with, and it is evident that the author has taken the utmost pains to get his information up to date. The locomotive works at Crewe, and the carriage and waggon works at Wolverton, are capitally described. With reference to the automatic vacuum brake, described on p. 120, it is evident that this brake is automatic on the coaches *as well as* on the guards' vans. As this is probably of recent introduction, the North-Western Company are to be congratulated on its adoption in their rolling-stock. The earlier vacuum brake fitted was nothing more than the simple non-automatic vacuum brake as far as the coaches were concerned, and was justly condemned by most railway engineers for that reason.

The remaining chapters deal with the working of the trains, shunting and marshalling of goods trains, the working of goods stations, rates, fares, traffic, &c. With regard to all these matters the writer's statements are clear, concise, and to the point.

It would be of great service to the railway world generally if some of the head officials of the best English railways would follow Mr. Findlay's example, and give us some of their stores of experience. Take, for instance, locomotive engineering: where is the student or apprentice to find a book of recent date on the design, construction, and working of the modern locomotive? Let us hope that the infection will spread on the North-Western staff, and that by and by Mr. F. W. Webb, the able Locomotive Superintendent of that railway, will write a book on the department in which he so greatly excels.

Mr. Findlay's book displays so much knowlege and ability that it well deserves to rank as a standard work on the subject.

N. J. L.

OUR BOOK SHELF.

Zur Geologie der Schweizeralpen. Von Dr. Carl Schmidt. One Plate. (Basel: Benno Schwabe, 1889.)

THIS pamphlet gives a summary of the views entertained at the present time by many leading Swiss geologists as to the geological history of the Alps. So far as we can see, it does not profess to be more than a compilation, or to contain any original work; but as a summary it is as clear and concise as the subject permits. The author, in the first chapter, briefly sketches the history of the principal types of rock which enter into the composition of the Alps; and, as might be expected at the present juncture, lays much stress upon the results of pressure. Some, indeed, may think that the present moment is rather inopportune for such a memoir as this; for the modifications due to pressure, especially in rocks already crystalline, are still the subject of so much controversy among geologists, that it is difficult to know what may be taken for granted; and there is a danger, if the writer be a disciple of the new school, of confusing the results of demonstration and of hypothesis. A quotation (translated) will indicate the author's point of view better than a general statement. After pointing out that two great rock groups exist in the Alps, one consisting of various granitoid rocks, gneisses, and crystalline schists, the other of limestones, sandstones, and other sediments, he proceeds—"In Switzerland the region which intervenes between the two zones is not very broad. The general strike of this intermediate zone is through Coire, Ilanz, the Greina Pass, Scopi, Airolo, Nufenen, the Rhone Valley, to Martigny, and so through the Val Ferret to the Little St. Bernard. The rocks of this intermediate zone are crystalline sediments, the age of which it is difficult to fix with precision. In the Grisons they have lately been claimed by Gümbel as Palæozoic, in the Valais they have been shown by Lory to be Triassic: that the same, from the Greina Pass to the Nufenen, are Jurassic, can be proved by fossils." This, however, begs the whole question. It has yet to be shown that the Swiss geologists have not confused together, as some maintain, two distinct rock groups, owing to their having mistaken (not for the first time) for crystalline schists, deposits which only simulate the latter, because they are locally composed almost entirely of their *débris*.

But, putting aside theoretical and controversial matters, the author's summary is generally clear. It would, we think, have been more useful if he had condensed somewhat the general discussion in the opening chapter, and dwelt more fully in the others on the many interesting questions of local physiography which are opened up by a study of the Alps. To this objection, however, an exception must be made in the case of the nagelflue, where Dr. Schmidt's remarks are very interesting. In certain of these great masses of conglomerate, pebbles of crystalline rocks are fairly common. These, he states, whether granites, gneisses, or crystalline schists, show no indications of the dynamic metamorphism which is exhibited by similar rocks in the adjoining chain of the Alps. From this observation, if confirmed by further research, it would result that the "schistosity," or secondary foliation, which is so marked a feature in most parts of the Alps, has resulted not from the post-Eocene but from the post-Miocene set of movements.

Die Entstehung der Arten durch räumliche Sonderung. Gesammelte Aufsätze von Moriz Wagner. Herausgegeben von Dr. med. Moriz Wagner. (Basel: Schwabe, 1889.)

OF later years, zoological literature has been significantly full of contributions, advocating now addition to, now subtraction from, the theory of "evolution by means of natural selection" formulated by Darwin—contributions, the chief characteristics of which are that they are mutually destructive, that they are, comparatively speaking, unsupported by any serious array of observed facts, and that none of them meet with more than a few adherents. We do not believe that truth is appreciably advanced by ingenious speculation of that nature; it is certainly in so far retarded that the energy thus expended would have been better applied in placing Darwin's conclusions under severe and prolonged tests, such as those attempted in Fritz Müller's "Für Darwin," and in Weismann's study of the markings of Lepidopteran larvæ. As Semper pointed out long ago (and too much insistence cannot be laid upon it), the present need is not for fine-spun theory—we have theories galore—but for the judicious compilation of facts selected where the leverage will tell, facts which shall either upset or confirm—it matters not which—the theory of natural selection.

The book before us must be classed among the speculative works just mentioned; and the gist of the author's views may be gathered from the following paragraph, written in 1880 (p. 401):—"Every permanent new form (species or variety) commences with the isolation of individual emigrants, separated for a prolonged period from the habitat of some parent species which is in the stage of variability. The active factors in the process are: (1) adaptation of the immigrant colonists to the external conditions of the new habitat (nutrition, climate, soil-composition, competition); and (2) the impression and development of the individual characteristics of the first colonists upon and in their posterity by reason of the breeding between near kin. This formative process ceases as soon as, owing to rapid multiplication, the levelling and compensating effects of intercrossing make themselves felt, resulting in and maintaining that uniformity which characterizes every good species and permanent variety." Wagner's hypothesis exalts the importance of geographical isolation at the expense of natural selection, and thus approximates, both at starting-point and conclusion, to Mr. Gulick's recent theory of "divergent evolution through cumulative segregation" (Journ. Linnean Soc., vol. xx. p. 189), though in detail the respective courses taken by the two writers are by no means identical.[1]

Consisting of a reprint of articles published between 1868-86, mainly in *Kosmos*, *Das Ausland*, and the *Allgemeine Zeitung*, the matter of the book has been long before the public, and its conclusions have been attacked from time to time by Haeckel, Weismann, Oscar Schmidt, and others; a translation of the first, and perhaps the most important article, has appeared in London (Stanford, 1873): criticism of the theory in this place is therefore unnecessary. The present reprint is edited by Wagner's nephew and namesake, in accordance with a wish expressed some time before his death in 1887, and contains, besides the articles previously published, a biographical sketch by Dr. von Scherzer, and editorial introductions; while the last 127 pages are devoted to an attempt of the editor to build certain recent discoveries, such as those of the *Challenger*, into the original structure. It is hardly necessary to say that, being a close-printed German octavo of 667 pages, the book possesses no index.

Sylvan Folk. By John Watson. (London: T. Fisher Unwin, 1889.)

MR. WATSON expresses much contempt for what he calls "the dry bones of science." We are not sure that we quite understand what he means by this expression, but it evidently does not imply that he dislikes results obtained by careful and exact observation. In the present little volume he gives ample proof that he often brings himself face to face with Nature, and that he knows how to interpret many of the innumerable signs and symbols which are readily misunderstood, or altogether overlooked, by less careful inquirers. Mr. Watson is especially happy in his notes upon the ways of birds; but he has also interesting chapters on mice, voles, and shrews, on red deer, fallow, and roe, on British seals, on British fur-bearers, and on "Nature by night." There is not much that is absolutely new in any of the information he has brought together; but his descriptions are so fresh—they suggest so vividly the idea of happy hours spent among attractive scenes in the open air—that they will give genuine pleasure to everyone who reads them. The book will be especially interesting to young readers, who will be glad to learn that it depends very much upon themselves, according to Mr. Watson, whether they shall be on terms of intimacy with the wildest woodland creatures. Mr. Watson thinks that the power of attracting wild creatures was once a much more common possession than it is now.

A Practical Guide to the Climates and Weather of India, Ceylon, and Burmah, &c. By Henry F. Blanford, F.R.S. Pp. 369. (London: Macmillan and Co., 1889.)

THE appearance of this book is very opportune. The Indian Meteorological Office has been in existence for some twenty years, and inasmuch as the region over which its operations extend comprises a very considerable area of the earth's surface, representing climatological conditions of the most varied character, a general *résumé* of the information as to these conditions is one of the most important contributions to climatology that could be made.

Mr. Blanford has well fulfilled his task. He says his work "is not addressed to meteorologists and physicists, ... but more particularly to agriculturalists, medical officers, engineers, pilots and other seafaring men, and to those others of the general public to whom the weather and the climates of India and of its seas are practical and not scientific objects of interest."

The book is divided into two parts: (1) the elements of climate and weather; (2) the climates and weather in relation to health and industry.

The former is naturally the more technical, while the latter appeals to the general public, as it gives a detailed description of the climates of the principal and most frequented hill stations, as well as of the plains, under which latter general head the different provinces or districts receive each a separate notice.

One section is specially devoted to the storms of the Indian Seas. In their discussion Mr. Blanford is a pronounced adherent of the spiral in-draft theory in contradistinction to the old circular theory and the well-known "eight-point rule."

About the most valuable chapter is the last, which is mainly occupied with rainfall and evaporation. The questions relating to these are of paramount importance for the bare existence of millions of the population. Such a famine as that of 1834 in the Doab was sufficient to induce the authorities of the day to construct the Ganges Canal, the greatest work of the kind in the world, and one which has in a great measure corrected the injurious effects of irregularity in the rainfall.

The appendixes give the tabular results of the instrumental records, which are required to substantiate the general statements contained in the previous pages.

The work is a most creditable production, and it will long remain the standard authority on any question bearing on the climate of the Indian Peninsula.

The Unrivalled Atlas. Enlarged Edition (18th). (London and Edinburgh: W. and A. K. Johnston, 1889.)

A NEW and enlarged edition of this atlas has just been published. The forty maps which it contains are well engraved and especially full of information concerning railway communication, whilst the fact that the index contains 20,000 names of places, with their latitude and longitude, testifies to its completeness. An extension of the atlas has been made by the addition of two classical maps, with an index to them, two physical maps of the British Isles and Europe, and two astronomical plates, each being accompanied with descriptive letterpress. A misleading paragraph occurs in the explanation of tidal action. We read: "The moon exerts a much greater influence on the production of tides than the sun; for, though its mass is excessively small in proportion, it is four hundred times nearer the earth." The inference that a beginner would draw from such a paragraph would be that the predominant effect of the moon in causing tides was simply due to its proximity to the earth, whereas the fact cannot be too strongly insisted upon, that it is the differential attraction of the sun and moon upon the earth's surface and centre that causes tidal action. The atlas is complete and trustworthy.

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.*]

Coral Reefs.

I BELIEVE all the questions asked by Captain W. Usborne Moore in NATURE of June 27 (p. 203) have been already answered in my papers on this subject. However, as I have examined the reefs at Kandavu, Matuku, Ovalau, and other places at the Fiji Islands, I may attempt to answer his questions with reference to these special cases.

According to Mr. Darwin's view, an island equal in extent to the lagoon of an atoll has sunk from view and the circular reef that has grown up marks approximately the position of the ancient coast line. In the case of a barrier reef, land equal in area to the lagoon channels has been submerged, the barrier reef here, too, marking the position of the ancient coast line. The opponents of Mr. Darwin's theory hold that there is no sufficient evidence that any such land once occupied the site of the lagoons and lagoon channels. I believe they might go further, and hold that, had such extensive submergence taken place, these lagoons and reefs would necessarily present features other than those by which they are everywhere characterized in our seas at the present time.

We hold that all the phenomena presented by these coral reefs and islands can be better explained on other principles without calling in subsidence; that slow subsidence or elevation or rest would only modify in a minor way the general features of a reef; that any one of the three kinds of reefs may be formed indifferently in a rising, sinking, or stationary area. In a sinking area the lagoons would probably be relatively deeper, the reefs narrower, and the islands on them small or absent; in a rising area the lagoons would be relatively shallower, the reefs broader, and the islands more numerous or united into a continuous band of land.

In surveying the coasts of any volcanic islands situated in the ocean where there are no coral reefs, Captain Moore must know that these are sometimes surrounded by banks extending much further seawards in one direction than in another. These banks are doubtless formed of the loose materials of the island, which are spread out by wave action. The position of these banks depends on the nature of the rocks in the different parts of the islands; in the case of Graham Island, the whole island was spread out, forming a submerged bank. In addition to the nature of the materials, the depth over the banks depends also on the extent and depth of the surrounding seas. The 20, 50, or 100 fathom line follows sometimes the shape of the coast within, sometimes differs widely from the shape of the coast, just the same as barrier reefs do.

The Fijis are, in my opinion, such a volcanic group, where the shallower waters have now become the home of myriads of lime-secreting organisms. These have built up the wholly submerged banks into atolls, and the banks around islands into barrier reefs, the depth and distance from land most favourable for vigorous coral growth being determined by a variety of local circumstances. When the reef reaches the surface, it spreads seawards. There is, of course, plenty of living coral in the lagoons, but this becomes less and less as the reef becomes more continuous and less oceanic water is admitted. The growth of corals is always, however, much less vigorous and much less rapid in lagoons than on the seaward faces of reefs. The position of the opening in the reef is determined by local conditions, such as the mud from rivers in the case of barrier reefs. I have elsewhere fully explained my view as to the removal of dead coral, and even coral heads and islands, from the lagoons, through solution of the lime by sea-water.

I cannot have seen as much of Kandavu as Captain Moore, but what I did see in no way led me to believe that it was a sinking island;[1] indeed it was here that, not being able to apply Mr. Darwin's theory in explanation of the phenomena of the Kandavu reefs, I commenced to doubt it altogether. The coast around Cape Washington (the western end) appears to be particularly unfavourable for the formation of extensive reefs, because of the high land (2750 feet) and deep water, hence we have only narrow fringing reefs or no reefs. At the north end the extensive banks surrounding the islets are, on the other hand, specially favourable for coral growth, owing to the almost complete absence of rivers and of detritus from the land; here we have what might be called an atoll, except for the presence of the small rocks, which, after reaching the surface, has extended seawards.

If Captain Moore thinks the rock, Solo, was once an island about four miles in diameter and of considerable height, he must explain how in sinking it has left a lagoon around the remaining rock with an average depth of not more that 12 fathoms. He must also explain the nature of the bank extending west from Ono, where there is no continuous barrier.

It is in every way desirable that practical surveyors like Captain Moore should take an interest in these theoretical views, and their observations will be none the less welcome and appreciated whatever side they may take in the controversy. The more observations accumulate the more does it seem to me probable that there never was a barrier reef or atoll formed after the manner required by Mr. Darwin's theory. If Profs. Dana, Bonney, Huxley, and Judd, would mention any one barrier reef and any one atoll that they believe undoubtedly to have been formed in accordance with the subsidence theory, then I think it might be possible within the next few years to undertake a thorough examination of these with the view of testing the rival theories.

Challenger Office, Edinburgh. JOHN MURRAY.

I HAVE been immersed in examinations, and away from London for a few days, so that I did not see Mr. Guppy's letter in NATURE of June 20 (p. 173) till it was too late to reply. I will now only ask space for a few last words. To me the matter does not appear to have "resolved itself into an affair of outposts." The position which I was led to take up from the study of the recent literature on coral reefs, and which I had hoped that I had made clear in my last letter, is this—that till Mr. Guppy can produce cases of *growing reefs* at depths well exceeding 25 fathoms, isolated instances of the occurrence, at such depths, of living corals which are among the reef-builders do not really help him; and that till he can do this he is only supporting hypothesis by hypothesis. For example, I have not seldom, in the Alps, gathered phanerogamous plants, flourishing and in full bloom, at elevations of eleven or even twelve thousand feet above the sea; but I should not direct anyone to this mountain zone who desired to pick a posy of Alpine flowers.

Into the remainder of Mr. Guppy's letter it would be waste of time to enter, or to continue this controversy, for two reasons: one, that evidently Mr. Guppy and myself differ widely as to the nature of hypothesis, and the difference between assumption and proof; the other, that I trust, before another number of NATURE appears, to be among the Alps (with strict orders against forwarding letters or papers), so that any return shots will fail to reach me, until, in this rapidly-moving world, they have passed into the region of ancient history.

June 28. T. G. BONNEY.

BEFORE replying to Captain Moore, I should bear witness to the searching character of his questions, which quite makes up for the fact that during his valuable services of many years abroad he has been, fortunately perhaps, beyond the reach of recent coral reef literature.

However, I would first point out that when he speaks of the second edition of Mr. Darwin's "Coral Reefs," which was published fifteen years ago (1874), as given to the world "after forty years of deep research into various problems of Nature," he surely forgets that after the first edition of 1842 the author had no further acquaintance with coral reefs. Captain Moore also omits to reflect that during the interval between the two editions Dana and Jukes were the only two principal observers that stood up

[1] See "Narrative of the *Challenger* Expedition," vol. i. p. 507.

for Darwin, whilst against them were ranged L. Agassiz, Le Conte, Hunt, Semper, and Rein. One might with equal justice claim for an eminent engineer that a bridge constructed by him as a young man in 1842 and repaired by him when an elderly man in 1874, represented the accumulated experience of a long professional career, notwithstanding that he had never since erected another bridge, and had devoted all his time to works of a very different character, and in spite of the fact that, whilst only two engineers of reputation had in the interval pronounced the bridge to be safe, five others of equal eminence had advised that, owing to its rickety condition, it should be pulled down. If it was a question of personal safety, most people would trust to the opinion of the many.

I will first clear the way by laying stress on the circumstance that neither A. Agassiz nor Murray doubt the reality of subsidence. They only contend that the characteristic form of atolls and barrier reefs is not dependent on it, and that such reefs would be produced by their natural mode of growth alone.

Now, with regard to Fiji, Captain Moore is perhaps not aware that in the writings of Murray, Semper, Geikie, and others, in these columns and elsewhere, the association of the three classes of reefs in the same group, as in Fiji, has often been referred to as opposed to the theory of subsidence. It was the occurrence of all reefs in the Fijis and in the Pelew Group, and the ascertained existence of upraised reefs in the last locality, that amongst other reasons led Murray and Semper to find some different explanation than that of subsidence. Dana saw the difficulty, and tried to avoid it by asserting that small fringing reefs are "often evidence of subsidence, even a greater subsidence than is implied by barrier reefs" (*Amer. Journ. Science and Arts*, 3rd series, vol. viii., p. 316). Semper, however, pointed out that this conclusion at once destroyed the value of the testimony afforded by coral reefs of the movements of the earth's crust, since, if all kinds of reefs can be formed during subsidence, the character of a reef cannot guide us in determining the existence of subsidence or upheaval ("Animal Life," p. 233). Darwin also saw the difficulty of the Pelew Group, where living atolls, barrier reefs, and fringing reefs were associated with upraised ancient reefs; and thus it came about that, when writing to Semper in 1879, he generously observed that, although he still adhered to the theory of subsidence, such cases as that of the Pelew Islands, if of at all frequent occurrence, would make his conclusions of very little value. "Future observers," he went on to say, "must decide between us," &c. (Semper's "Animal Life," p. 456).

I contend that in 1889 Mr. Darwin's condition has already been realized, since we have in the interval proved that in respect to the occurrence in the same locality of all three classes of coral reefs and their association with ancient upraised reefs, the Fijis, the Tongan Group, and the Solomon Islands are but reproductions of the Pelew Group. It will therefore be seen that Captain Moore is in error when he believes that Darwin would not have regarded simultaneous up and down movements in the same group as inconsistent with his theory. Darwin's admission to Prof. Semper in the case of the Pelews shows plainly enough what he would have thought of the Fijis. This difficulty of imagining simultaneous up and down movements in the same group was also perceived by the present Director-General of the Geological Survey, when he remarked some years ago in these columns that "such an association of upheaval with an assumed general subsidence requires, on the subsidence theory, a cumbrous and entirely hypothetical series of upward and downward movements" (NATURE, vol. xxix. p. 107; Proc. Roy. Phys. Soc., viii.).

It is somewhat remarkable that long as the Fiji Group has been known, it is only of recent years that anything has been published concerning its upraised coral reefs and its other evidences of considerable upheaval. Darwin, as late as his edition of 1874, knew nothing of the extensively upraised reefs, and he still coloured the group in his map as in an area of subsidence. Strangely enough, amongst the many errors perpetuated in the edition published in the present year of Mr. Darwin's work, is that relating to this group. In an additional footnote (p. 215) we find no reference to the ancient coral reefs upraised some hundreds of feet in the Lau or Windward Group, which are referred to in the Hydrographic publication of 1882 concerning Fiji, and which were described to me by Lieut. Malan some years ago. We find no reference in this edition of 1889 to the conclusion of Mr. Brady that the Suva soapstone, as indicated by its Foraminifera, was formed in depths of from 150 to 200 fathoms in post-Tertiary times, thus implying an upheaval of from 900 to 1200 feet (Geol. Soc., November 9, 1887). Curious as it may appear, Dana, after his lengthened stay in the group, found proofs of an elevation in the larger islands of only 5 or 6 feet, and was inclined to negative it altogether in the case of the eastern islands ("Corals and Coral Islands," 1872, pp. 342, 346).

Captain Moore refers to the "many evidences of upheaval" in Fiji, and instances the occurrence of shells and coral "at great heights." What evidence has he of subsidence? He points to the form of the reef in various islands, and thus assumes the very question at issue. However, I will leave to Mr. Murray to explain how these reefs attained their characteristic form without the assumed movement of subsidence, of which in fact we can find no direct proof.

H. B. GUPPY.

As Dr. Guppy asks for information with regard to the corals found living at the greater depths round islands in the Indian and Pacific Oceans the following may be of interest.

About two years ago Capt. Wharton, F.R.S., called attention to the Tizard Bank, and last year both it and the Macclesfield Banks were examined by H.M.S. *Rambler*.

The further investigation of the material sent home has shown that the large number of eighteen genera with forty species were found living in depths from 20–44 fathoms outside the reefs, and these species differed with but few exceptions from those in the shallow water.

The following ten genera were found at a greater depth than 30 fathoms—Stylophora, Astræa, Pavonia, Cycloseris, Leptoseris, Stephanaria, Psammocora, Montipora, Alveopora, and Rhodaræa, besides seven small scarcely reef-building genera.

The total number of species collected was 142, so that nearly one-third were represented in over 20 fathoms.

P. W. BASSETT-SMITH, R.N.

Hibernation of Martins in the Argentine Republic.

PROF. CARLO SPEGAZZINI, an Italian botanist, and quite a trustworthy observer, living at La Plata, the new town in the Argentine Republic, writes from there the following account to my friend the Marquis Giacomo Doria of Genoa:—

"The bird known here by the name of *Golondrina*, and which I think is *Progne domestica*,[1] is subject to hibernation. Last year, while the zinc roof of a small house was being taken up in the month of August, just in the middle of our winter, I found underneath about a hundred martins, all accumulated one over the other and lethargic, but in good health, so that, exposed to the sun, they awoke and flew away very briskly. This year, again, having seen some holes on a *barrancha*, a steep bank over the Plata, I began to dig at them, hoping to find some bats; but there I found several hundreds of the martins of the same kind as above mentioned, clustered and in a state of lethargy. Is such a thing known to naturalists?"

TOMMASO SALVADORI.

Zoological Museum, Turin, June 18.

Atmospheric Electricity.

THE interesting accounts of certain electrical phenomena of the atmosphere in NATURE of May 16, 23, and 30, lead me to state that it is a common experience of surveying parties, especially on the high peaks and slopes in the western part of this country to undergo these peculiar electrical sensations. In general these may be described as tingling or pricking sensations, accompanied with hissing or crackling sounds, especially marked if a finger be presented to any metallic object near by. But further than this it has been noticed that whenever a flash of lightning occurs there is a sudden cessation of the distressing electrical effects. The explanation of this is, we think, found in some experiments made at the top of the Washington Monument (elevation 500 feet) during thunderstorms. With a "water dropper collecter," Mascart insulators and quadrant electrometer, we measured the difference of the electrical potential of the air and the ground. The electrometer needle becomes very active with the approach of the "thunder-heads," and after considerable oscillation begins to move steadily in one direction as if subjected to a steadily increasing "pull," and then suddenly, when a potential difference of several thousand volts may be indicated, there is an "instant" drop to zero, and apparent rebound in the other direction, not due to the torsion of the

[1] Or more likely *Progne chalybæa*, Gm.—T. S.

suspending wire-fibre. The drop to zero is simultaneous with each flash of lightning. So certain is this relation that we can time the lightning flashes without seeing them. After the flash the needle begins to again move in one direction, repeating its previous behaviour, so that our electrometer measurements seem to prove that every flash of lightning relieves the state of stress of the air, which we may compare with the glass in a Leyden jar, the cloud and ground being the respective coatings.

We may also get at the same result by noticing the effects of the electrification of the dust, smoke, water, and other matter in the air. Whenever our "collector" was "grounded," the fine stream of water issuing from it preserved a certain even rounded form, breaking into drops some four inches away from the place of exit. Removing the "ground" connection, and the stream being now under the influence of the thunder-clouds, the steadily increasing electrification shows itself in the stream's twisting and splitting into innumerable threads and spray; but with each flash of lightning the distortion instantly ceases, and the stream has its normal character, only to be again distorted.

For the benefit of those wishing to photograph lightning I suggest this as a cheap and easy mode of getting warning when to expose. A small tin case with a nozzle giving a fine thread of water or any form of the "burning match" device, well insulated from the ground, and at some elevation, will indicate by changes in the character of the stream or smoke, the approximate degree of the electrification of the air.

New York, U.S.A., June 20. ALEXANDER MCADIE.

Upper Wind Currents over the Equator in the Atlantic Ocean.

REFERRING to the remarks of the Hon. Ralph Abercromby on the above in NATURE of May 30 (p. 101), I would ask for the longitude and latitude for the two crossings of the doldrums, to enable one properly to follow, and eventually work out, the facts. For if our famous meteorologist, on the outer journey, passed within one hundred miles of the West Coast of Africa, the great chain of desert lands, extending many hundreds of miles through Asia to the Sahara in the main weather thoroughfare, would, by its influence, very much contract the width of the calm belt, and otherwise draw the doldrum much north of the line to what would be found more to the westward, where, as it neared the American coast, the breadth of the doldrum belt would very greatly increase; so that, unless the doldrum was crossed at the same longitude, the varying atmospheric conditions should not be put down solely to the sun, or difference of December and May seasons.

Perhaps it is from want of the longitude that I am unable to understand "how low clouds from the south-east drove over north-east trades up to 15° north." All else is exceedingly satisfactory with the law of winds, on the supposition that the return crossing of the doldrum belt took place some hundreds of miles further west, and about half that distance farther south, than on the out journey.

If I may be allowed to digress a little, I would refer to the splendid travels by your correspondent, as published last Christmas under the title of "Seas and Skies in many Latitudes," which in some respects, I think, may be compared with the celebrated *Challenger* Expedition. But, singularly enough, while I am able to follow and accept all the author's research and information, I find myself diametrically opposed to his conclusions. Thus, to quote from p. 428, we have: "Hence we see the proof of the assertion that the trades and monsoons do not meet and force one another to rise, and flow back poleward, but that the two winds coalesce and form one great eastern current over the doldrum." To my mental capacity all the proof is the other way about. If the trades of both hemispheres do not ascend over the calm belt, what other escape or vent is there for them? I am aware that the Meteorological Section of the Krakatão Committee of the Royal Society also favour the idea of a great easterly current ever going west at a certain altitude over the doldrums, but so far as I can find out they do not tell us whence it cometh nor whither or how it goeth. In its circuit going west, we naturally look for its return from the east, and with the constant arrival of fresh winds *viâ* the trades of both hemispheres, it must accumulate if it does not flow back poleward quite as fast as it arrives, for we cannot entertain annihilation of atmosphere any more than of matter. If some other way of escape could be found, we should still have to face the question, Whence comes the supply to the "trades if not from the poles, and whence our prevailing south-westerly winds if not from over the doldrums"? E. FOULGER.

Liverpool, June 18.

P.S.—I do not call in question "the great easterly current over the doldrum," but rather consider its discovery as extremely interesting, it being just what a small school of meteorologists would expect; and it now appears to be left for them to supply a theory for the cause of the direction and also of the motive power of such an atmospheric passage, and possibly for that of the Krakatão dust. E. F.

Patches of Prismatic Light.

I AM curious to know if any of your readers observed the following phenomenon in the sky, and could give any information as to its nature.

When driving with two friends on Saturday evening, June 22, between 6.30 and 7 p.m., in the neighbourhood of Glatton, near Peterborough, we observed on either side of the sun (the sky being almost cloudless) two patches of prismatic light; they appeared to be of nearly the same size as the apparent disk of the sun, and distant from it a hand's span measured from little finger to thumb at arm's length. At the time there were a few light clouds about, but the prismatic patches were not projected on them, as the clouds passed occasionally in front of them, the patches meanwhile shining through the thinnest parts, and reappearing when the clouds had passed, clearly standing out against the sky. There was no appearance of a continuous arch, as in a rainbow, and, unlike a rainbow, the patches were on the same side of the sky as the sun. I may add that the phenomenon was seen by all three of us, and for half an hour after we first noticed it. C. S. SCOTT.

Glatton Hall, Peterborough, June 28.

A Chimpanzee's Humour.

IN a recent lecture Mr. Romanes is reported as having strongly denied the existence of even a trace of any feeling of the ludicrous in the renowned chimpanzee "Sally." It may be worth while to record a small fact observed by me lately, tending, I think, to favour an opposite view.

Being alone with a friend in Sally's house, we tried to get her to obey the commands usually given by the keeper. The animal came to the bars of the cage to look at us, and, adopting the keeper's usual formula, I said, "Give me two straws, Sally." At first she appeared to take no notice, although she had been eyeing us rather eagerly before. I repeated the request with no further result; but on a second or third repetition she suddenly took up a *large bundle* of straw from the floor and thrust it through the bars at us, and then sat down with her back to us. Our request was perhaps unreasonable, seeing that we had no choice morsels of banana with which to reward her. She did not, however, seem ill-tempered at our presumption, and the next instant was as lively as ever. It seems to me that her action on this occasion certainly came very near to an expression of humour. Rather sarcastic humour perhaps it was, but she certainly appeared to take pleasure in the spectacle of something incongruous, and this surely lies at the base of all sense of the ludicrous. HAROLD PICTON.

July 1.

PROF. HUXLEY AND M. PASTEUR ON HYDROPHOBIA.

ON Monday afternoon the meeting called by the Lord Mayor to hear statements from men of science with regard to the recent increase of rabies in this country, and the efficiency of the treatment discovered by M. Pasteur for the prevention of hydrophobia, was held at the Mansion House. Much excellent work was done. Several letters were read from those who were unable to attend. Among these letters was the following from Prof. Huxley:—

"*Monte Generaso, Switzerland, June* 25, 1889.

"MY LORD MAYOR,—I greatly regret my inability to be present at the meeting which is to be held, under your

Lordship's auspices, in reference to M. Pasteur and his Institute. The unremitting labours of that eminent Frenchman during the last half-century have yielded rich harvests of new truths, and are models of exact and refined research. As such they deserve, and have received, all the honours which those who are the best judges of their purely scientific merits are able to bestow. But it so happens that these subtle and patient searchings out of the ways of the infinitely little—of that swarming life where the creature that measures one-thousandth part of an inch is a giant—have also yielded results of supreme practical importance. The path of M. Pasteur's investigations is strewed with gifts of vast monetary value to the silk trader, the brewer, and the wine merchant. And this being so, it might well be a proper and a graceful act on the part of the representatives of trade and commerce in its greatest centre to make some public recognition of M. Pasteur's services, even if there were nothing further to be said about them. But there is much more to be said. M. Pasteur's direct and indirect contributions to our knowledge of the causes of diseased states, and of the means of preventing their occurrence, are not measurable by money values, but by those of healthy life and diminished suffering to men. Medicine, surgery, and hygiene have all been powerfully affected by M. Pasteur's work, which has culminated in his method of treating hydrophobia. I cannot conceive that any competently-instructed person can consider M. Pasteur's labours in this direction without arriving at the conclusion that, if any man has earned the praise and honour of his fellows, he has. I find it no less difficult to imagine that our wealthy country should be other than ashamed to continue to allow its citizens to profit by the treatment freely given at the Institute without contributing to its support. Opposition to the proposals which your Lordship sanctions would be equally inconceivable if it arose out of nothing but the facts of the case thus presented. But the opposition which, as I see from the English papers, is threatened has really for the most part nothing on earth to do either with M. Pasteur's merits or with the efficacy of his method of treating hydrophobia. It proceeds partly from the fanatics of *laissez faire*, who think it better to rot and die than to be kept whole and lively by State interference, partly from the blind opponents of properly-conducted physiological experimentation, who prefer that men should suffer rather than rabbits or dogs, and partly from those who for other but not less powerful motives hate everything which contributes to prove the value of strictly scientific methods of inquiry in all those questions which affect the welfare of society. I sincerely trust that the good sense of the meeting over which your Lordship will preside will preserve it from being influenced by these unworthy antagonisms, and that the just and benevolent enterprise you have undertaken may have a happy issue.

"I am, my Lord Mayor, your obedient servant,

"THOMAS H. HUXLEY.

"The Right Hon. the Lord Mayor,
Mansion House, E.C."

The following letter from M. Pasteur, dated Paris, the 27th ult., was read by Sir H. Roscoe :—

"Dear Colleague and Friend,—I am obliged by your sending me a copy of the letter of invitation issued by the Lord Mayor for the meeting on July 1. Its perusal has given me great pleasure. The questions relating to the prophylactic treatment for hydrophobia in persons who have been bitten and the steps which ought to be taken to stamp out the disease are discussed in a manner both exact and judicious. Seeing that hydrophobia has existed in England for a long time, and that medical science has failed to ward off the occurrence even of the premonitory symptoms, it is clear that the prophylactic method of treating this malady which I have discovered ought to be adopted in the case of every person bitten by a rabid animal. The treatment required by this method is painless during the whole of its course and not disagreeable. In the early days of the application of this method contradictions such as invariably take place with every new discovery were found to occur, and especially for the reason that it is not every bite by a rabid animal which gives rise to a fatal outburst of hydrophobia. Hence prejudiced people may pretend that all the successful cases of treatment were cases in which the natural contagion of the disease had not taken effect. This specious reasoning has gradually lost its force with the continually increasing number of persons treated. To-day, and speaking solely for the one anti-rabic laboratory of Paris, this total number exceeds 7000 ; or exactly, up to the 31st of May, 1889, 6950. Of these the total number of deaths was only seventy-one. It is only by palpable and wilful misrepresentation that a number differing from the above, and differing by more than double, has been published by those who are systematic enemies of the method. In short, the general mortality applicable to the whole of the operations is 1 per cent., and if we subtract from the total number of deaths those of persons in whom the symptoms of hydrophobia appeared a few days after the treatment—that is to say, cases in which hydrophobia had burst out (often owing to delay in arrival) before the curative process was completed—the general mortality is reduced to 0·68 per cent. But let us for the present only consider the facts relating to the English subjects whom we have treated in Paris. Up to May 31, 1889, their total number was 214. Of these there have been five unsuccessful cases after completion of the treatment, and two more during treatment, or a total mortality of 3·2 per cent., or more properly 2·3 per cent. But the method of treatment has been continually undergoing improvement, so that in 1888 and 1889, on a total of sixty-four English persons bitten by mad dogs and treated in Paris, not a single case has succumbed, although amongst these sixty-four there were ten individuals bitten on the head and fifty-four bitten on the limbs, often to a very serious extent. I have already said that the Lord Mayor in his invitation has treated the subject in a judicious manner, from the double point of view of prophylaxis after the bite and of the extinction of the disease by administrative measures. It is also my own profound conviction that a rigorous observance of simple police regulations would altogether stamp out hydrophobia in a country like the British Isles. Why am I so confident of this? Because, in spite of an old-fashioned and widespread prejudice, to which even science has sometimes given a mistaken countenance, rabies is never spontaneous. It is caused, without a single exception, by the bite of an animal affected with the malady. It is needless to say that in the beginning there must have been a first case of hydrophobia. This is certain ; but to try to solve this problem is to raise uselessly the question of the origin of life itself. It is sufficient for me here, in order to prove the truth of my assertion, to remind you that neither in Norway, nor in Sweden, nor in Australia, does rabies exist ; and yet nothing would be easier than to introduce this terrible disease into those countries by importing a few mad dogs. Let England, which has exterminated its wolves, make a vigorous effort, and it will easily succeed in extirpating rabies. If firmly resolved to do so, your country may secure this great benefit in a few years ; but, until that has been accomplished, and in the present state of science, it is absolutely necessary that all persons bitten by mad dogs should be compelled to undergo the anti-rabic treatment. Such, it seems, is a summary of the statement of the case by the Lord Mayor. The Pasteur Institute is profoundly touched by the movement in support of the meeting. The interest which His Royal Highness the Prince of Wales has evinced in the proposed manifestation is of itself enough to secure its success. Allow me, my dear colleague, to express my feelings of affectionate devotion."

AN INDEX TO SCIENCE.

I HAVE lately received the "Sach Register" of the *Berichte der Deutschen Chemischen Gesellschaft*, 1868–87, in three volumes, indexing the twenty years of publication (thirty-six volumes). The work is admirably done, and is of inestimable value to the student of science generally. German scientific men and scientific Societies are far in advance of the English in the art of making the results of scientific research readily accessible. Witness the admirable "Bibliotheca Zoologica," by Engelmann and Carus, and still continued by Taschenberg; the "Sach Registers" to Liebig's *Annalen der Chemie*, 1832–83, to Poggendorff's (now Wiedemann's) *Annalen der Physik und Chemie*, 1824–87, and to the *Journal für Praktische Chemie*, 1833–87; and the "Repertorium Commentationum a Societatibus litterariis editarum," by Reuss, in sixteen quarto volumes, which last valuable production covers the whole ground down to the end of the last century.

The fact is very suggestive with regard to the English neglect of the scientific knowledge, experiment, and discovery locked up in the long series of English scientific journals to be found in our public libraries. The journals have usually, but not always, a short index at the end of each volume, obliging the student to occasionally spend days and weeks in searching through the series for what has been written on a subject he is investigating. The volumes accumulate on the shelves, and the experiments and investigations are repeated again and again.

The Royal Society, with a lively sense of the necessity for a remedy to the existing chaos, some twenty years ago commenced, and continues at intervals the issue of "A Catalogue of Scientific Papers contained in the Transactions of Societies, Journals, and other Periodical Works from the Year 1800." It is an author-list, and does not at all meet the requirements of the case. It necessitates a knowledge, by the inquirer, of the names of all the men who are likely to have written on the subject of inquiry. Life is not long enough for this. Librarians are occasionally of some assistance in the matter, but they often fail. What occurred lately has occurred to me often: I submitted to a chemist of some note the records of experiments published in 1820, which would have saved him many months of investigation of the same subject if a reference to the previous work had been accessible by index or by personal knowledge. It is the experience of all men of science that days may be spent in obtaining a reference to what may be read in five minutes, usefully or uselessly.

The Royal Society Catalogue originated from a suggestion of the late Joseph Henry, the Director, for thirty years, of the Smithsonian Institution (Washington) for the Increase and Diffusion of Knowledge, who said ("Smithsonian Miscellaneous Collections," vol. xxi. p. 295):—

"One of the most important means for facilitating the use of libraries (particularly with reference to science) is well-digested indexes of subjects, not merely referring to volumes of books, but to memoirs, papers, and parts of scientific transactions and systematic works. I know of no richer gift which could be bestowed upon the science of our own day than the provision of these. Everyone who is desirous to enlarge the bounds of human knowledge should, in justice to himself as well as to the public, be acquainted with what has been previously done in the same line."

Henry afterwards communicated with the British Association on the subject (in 1855). The Association appointed a Committee (Mr. Cayley, Mr. Grant, and Prof. Stokes), who reported:—

"The Committee are desirous of expressing their sense of the great importance and increasing need of such a Catalogue. . . . The Catalogue should not be restricted to memoirs in Transactions of Societies, but should comprise, also, memoirs in the Proceedings of Societies, in mathematical and scientific journals, &c. . . . The Catalogue should begin from the year 1800. There should be a Catalogue according to the names of authors, and *also a Catalogue according to subjects*."

The Committee succeeded in interesting the Royal Society of London in the undertaking, and that body ultimately assumed the direction of the work.

But they have achieved an instalment only of the recommendation of the distinguished Committee at whose suggestion they took action. They have produced an author-list, but the "Catalogue according to subjects" is wanting. All the subjects in the Royal Society's Catalogue should be at once placed under a subject-heading as well as under the author-heading in alphabetical arrangement, as near a concordance as possible by means of cross-references, and should be systematically continued and published annually. What has been done by Mr. Poole, of Chicago, for the great portion of English *general* periodical literature in publishing a subject-list, alphabetically arranged, of the articles contributed to 238 periodicals from 1802–81, and to 141 periodicals in the supplemental volume for 1882–87, should be done for all the scientific publications. The principle of the work is simple, and could be readily carried out. "The main purpose of this (Poole's) work was to meet the average wants of students, literary men, and writers for the press—in other words, to help general scholars, who are many, in preference to the few who give their whole attention to a single topic." From an experience of thirty-two years in libraries, I must say, with all due deference to Mr. Poole, that a subject-index of the scientific journals would be of vastly greater benefit to the community—material benefit, if he pleases. I ought, however, to add my meed of praise to the practical, sensible, and sufficient way in which the work that he attempted has been done.

An attempt at remedying our great literary defect was made by Robert Watt, 1819–24, when he added to the two quarto volumes of an alphabetical list of 40,000 authors in his "Bibliotheca Britannica, or General Index to British and Foreign Literature," an additional two volumes of an index of subjects. This has all the disadvantages of a first attempt. The study of bibliography was little known, and less cultivated, at the time. The book is almost entirely out of date. A great deal more may be said in favour of the combination of authors and subjects in one general catalogue, as successfully achieved by Lieutenant-Colonel Billings in the "Index Catalogue of the Library of the Surgeon-General's Office, United States Army," as far as the letter N, in nine volumes, royal octavo. This is a specimen of cataloguing almost perfect: every article, and every disease, and the complications of every disease, with the various organs, being catalogued and sub-catalogued with their cross-references.

The history of scientific research exhibits a continual tendency towards specialization; and as the sphere of the labourer has become limited each area of research has expanded, so that it has become essentially necessary that every subdivision of knowledge should be digested and arranged. With the co-operation of a few librarians, a subject-catalogue of all scientific literature might be readily undertaken by the Royal Society or the Society of Arts, the publications contained in the British Museum being marked by an asterisk or other sign. Or a system similar to that of the "Smithsonian Catalogue of Scientific and Technical Periodicals (1665–1882)" might be adopted, in which, by means of a check list at the end, it is shown in what American libraries all the periodicals may be found. A good portion of the work is done in such works as have been mentioned: in the indexes to the literature of special subjects in the Smithsonian collections, in the publications of the Harvard University, and in the indexes to the publications of Societies, such as the Chemical, Geological, Zoological, Linnean, Astronomical, Geographical, Engineers, Statistical, the Society of Arts, and the Royal Society, and in the indexes of various periodicals. These should be systematically arranged; the chaos should be organized and classified, to enable the man of science to find out at a glance all that has been published on any branch of his subject, and the work would be of value to the country.

The range of subject-headings should include everything relating to scientific and technical subjects. These would include (taking Comte's classification for convenience in the serial arrangement), Mathematics, Astronomy, Physics, Chemistry, Physiology (or what is commonly understood as Natural History, with the generic term of Physiology or Physio-Philosophy), and Social Physics (including Sociology). These subjects are clear, well defined, and well known to the librarians.

There is nothing so necessary as that scientific thought and method should be embodied, classified, and arranged, preliminary to its organization as a whole. It would quicken the slow process of improvement that has extended over a thousand years. It is wonderful that it should be necessary to say this in the nineteenth century. The need for organization in all departments of science is keenly felt; and the growth of Positivism in these latter days is one of the expressions of that need. Three centuries ago Bacon aimed at the organization of the sciences, holding that the sciences can be advanced only by combining them; that, as natural laws are invariable and uniform, "Physics being the mother of all science," so moral and civil philosophy could not flourish when separated from their roots in natural philosophy.

On national grounds it is necessary that this work should be done, for it is in the interest of the community generally, of the nation, that we should know what we possess. Public libraries and educational institutions are increasing; scientific experiment, discovery, and invention are increasing; and the demand for such a subject-catalogue will increase. Its value to the community would be inestimable. If it is thought advisable that the Royal Society or the Society of Arts should take the work in hand, the work should be subsidized by the Government, for the simple reason that it would be of national value. Scientific research is not so remunerative but that the student may fairly expect facility of access to the labours of those who have gone before. The want of a serviceable key to the vast body of scientific work contained in our literature is daily experienced by cultivators of science. There is a serious necessity that this material should be made more readily accessible for comparison, for verification, and for improvement. Much of it is a monument of shattered hopes, the unsuccessful efforts of poverty and despair; but all of it is suggestive to the earnest student.

No one Society or publisher can take the work in hand without Government support or benefaction. Attempts have been made, such as the attempt of the Royal Society, and that of Agassiz in his "Bibliographia Zoologiæ et Geologiæ," published by the Ray Society, which voluminous work, be it remembered, "was mainly compiled by the Professor for his own private use during the leisure moments of a life of almost incessant scientific research." In whatever hands the work may be placed, there is no doubt that the co-operation of the scientific Societies might be relied upon. With regard to the question of assistance from the Government, the following Minute of the Lords Commissioners of Her Majesty's Treasury, dated November 28, 1864, referring to the Royal Society's Catalogue, is very suggestive:—

"Having regard to the importance of the work with reference to the promotion of scientific knowledge generally, to the high authority of the source from which it comes, and to the labour gratuitously given by members of the Royal Society to its production, my Lords consider themselves justified in having the work printed at the cost of the public, with the understanding that, reserving such a number of copies for presentation as my Lords, in communication with the President of the Royal Society, may hereafter determine, the work shall be sold at such a price as may be calculated will repay the cost of printing.

"Their Lordships, however, desire it to be understood that the work shall go forth to the public under the authority of the Royal Society, by the exertions of whose members this important aid to the study of science has been produced."

J. TAYLOR KAY.

IRIDESCENT CRYSTALS.[1]

THE principal subject of the lecture is the peculiar coloured reflection observed in certain specimens of chlorate of potash. Reflection implies a high degree of discontinuity. In some cases, as in decomposed glass, and probably in opals, the discontinuity is due to the interposition of layers of air; but, as was proved by Stokes, in the case of chlorate crystals the discontinuity is that known as twinning. The seat of the colour is a very thin layer in the interior of the crystal and parallel to its faces.

The following laws were discovered by Stokes:—

(1) If one of the crystalline plates be turned round in its own plane, without alteration of the angle of incidence, the peculiar reflection vanishes twice in a revolution, viz. when the plane of incidence coincides with the plane of symmetry of the crystal. [Shown.]

(2) As the angle of incidence is increased the reflected light becomes brighter and rises in refrangibility. [Shown.]

(3) The colours are not due to absorption, the transmitted light being strictly complementary to the reflected.

(4) The coloured light is not polarized. It is produced indifferently whether the incident light be common light or light polarized in any plane, and is seen whether the reflected light be viewed directly or through a Nicol's prism turned in any way. [Shown.]

(5) The spectrum of the reflected light is frequently found to consist almost entirely of a comparatively narrow band. When the angle of incidence is increased, the band moves in the direction of increasing refrangibility, and at the same time increases rapidly in width. In many cases the reflection appears to be almost total.

In order to project these phenomena a crystal is prepared by cementing a smooth face to a strip of glass, whose sides are not quite parallel. The white reflection from the anterior face of the glass can then be separated from the real subject of the experiment.

A very remarkable feature in the reflected light remains to be noticed. If the angle of incidence be small, and if the incident light be polarized in or perpendicularly to the plane of incidence, the reflected light is polarized in the *opposite* manner. [Shown.]

Similar phenomena, except that the reflection is white, are exhibited by crystals prepared in a manner described by Madan. If the crystal be heated beyond a certain point the peculiar reflection disappears, but returns upon cooling. [Shown.]

In all these cases there can be little doubt that the reflection takes place at twin surfaces, the theory of such reflection (*Phil. Mag.*, Sept. 1888) reproducing with re-

[1] Abstract of the Friday evening lecture delivered by Lord Rayleigh, F.R.S. at the Royal Institution on April 12, 1889.

markable exactness most of the features above described. In order to explain the vigour and purity of the colour reflected in certain crystals, it is necessary to suppose that there are a considerable number of twin surfaces disposed at approximate equal intervals. At each angle of incidence there would be a particular wave-length for which the phases of the several reflections are in agreement. The selection of light of a particular wave-length would thus take place upon the same principle as in diffraction spectra, and might reach a high degree of perfection.

In illustration of this explanation an acoustical analogue is exhibited. The successive twin planes are imitated by parallel and equi-distant disks of muslin (Figs. 1 and 2) stretched upon brass rings and mounted (with the aid of three lazy-tongs arrangements), so that there is but one degree of freedom to move, and that of such a character as to vary the interval between the disks without disturbing their equi-distance and parallelism.

The source of sound is a bird-call, giving a pure tone of high pitch (inaudible), and the percipient is a high-pressure flame issuing from a burner so oriented that the direct waves are without influence upon the flame (see NATURE, xxxviii. 208; Proc. Roy. Inst., January 1888). But the waves reflected from the muslin arrive in the effective direction, and if of sufficient intensity induce flaring. The experiment consists in showing that the action depends upon the distance between the disks. If the distance be such that the waves reflected from the several disks co-operate,[1] the flame flares, but for intermediate adjustments recovers its equilibrium. For full success it is necessary

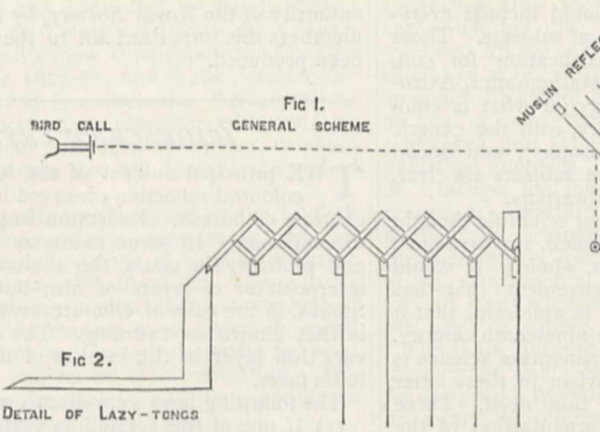


FIG. 1. GENERAL SCHEME

FIG. 2. DETAIL OF LAZY-TONGS

that the reflective power of a single disk be neither too great nor too small. A somewhat open fabric appears suitable.

It was shown by Brewster that certain natural specimens of Iceland spar are traversed by thin twin strata. A convergent beam, reflected at a nearly grazing incidence from the twin planes, depicts upon the screen an arc of light, which is interrupted by a dark spot corresponding to the plane of symmetry. [Shown.] A similar experiment may be made with small rhombs in which twin layers have been developed by mechanical force after the manner of Reusch.

The light reflected from fiery opals has been shown by Crookes to possess in many cases a high degree of purity, rivalling in this respect the reflection from chlorate of potash. The explanation is to be sought in a periodic stratified structure. But the other features differ widely in the two cases. There is here no semicircular evanescence, as the specimen is rotated in azimuth. On the contrary, the coloured light transmitted perpendicularly through a thin plate of opal undergoes no change when the gem is turned round in its own plane. This appears to prove that the alternate states are not related to one another as twin crystals. More probably the alternate strata are of air, as in decomposed glass. The brilliancy of opals is said to be readily affected by atmospheric conditions.

NOTES.

THE thirty-eighth meeting of the American Association for the Advancement of Science will be held at Toronto. On Tuesday, August 27, at noon, a meeting of the Council will be held at the Queen's Hotel, where will be the hotel head-quarters of the Association. On Wednesday, August 28, the first general session will begin at 10 o'clock in the forenoon in the Convocation Hall, University Buildings. After the adjournment of the general session, the several Sections will organize. In the afternoon the Vice-Presidents will give their addresses before their respective Sections, and in the evening there will be a general session, when the retiring President, Major J. W. Powell, will deliver his address. The sessions will continue until the Tuesday evening following, and on Wednesday morning, September 4, a meeting of the Council will be held. Saturday, August 31, will be devoted to excursions. The meeting will close with excursions extending to September 7. The general sessions and the meetings of the Sections will be held in the University Buildings, where also will be the offices of the local committee and of the permanent secretary during the meeting. *Science* states that arrangements have been made for a discussion in Section B on the "Relative Merits of the Dynamometric and Magnetic Methods of obtaining Absolute Measurements of Electric Currents." Prof. Thomas Gray, of the Rose Polytechnic Institute, will open the discussion with a paper on the subject, and he will exhibit one or more of Sir William Thomson's most recent forms of electric balance. Arrangements have been made by the local committee for the proper care and exhibition of instruments and specimens.

EFFORTS are being made by the American Association for the Advancement of Science to form a National Chemical Society, with its head-quarters at Washington. A meeting will be held to consider the matter during the session of the American Association at Toronto.

THE annual general meeting of the Marine Biological Association was held in the rooms of the Royal Society on the 26th ult. In the absence of Prof. Huxley, the chair was taken by Sir E. Bowman, and there were present, among others, Lord

[1] If the reflection were perpendicular, the interval between successive disks would be equal to the half wave-length, or to some multiple of this.

Walsingham, Prof. Flower, Prof. E. Ray Lankester, Admiral Sir Erasmus Ommanney, Mr. Gassiott, and Mr. Crisp. The report of the Council shows that a most satisfactory amount of work has been done at Plymouth since the Laboratory was opened at the end of June last year. Studies on various matters connected with the fishing industry are being carried on under the instructions of the Council, the most important being the study of the life-history of the common sole, by Mr. Cunningham, and an investigation on the sense-organs of fishes, by Mr. Bateson, which it is expected will throw new light on the bait question. Other naturalists, among whom Mr. Weldon may be specially mentioned, have utilized the Laboratory for carrying on independent biological researches, and much valuable work is being done. The Director of the Laboratory (Mr. Bourne) reports that the arrangements at the Laboratory are very satisfactory, and that the arrangements for the circulation of sea-water in the aquarium have worked well during the year. A substantial increase has been made in the library, a complete set of the *Challenger* publications, presented by the Lords Commissioners of the Treasury, being the most noticeable addition to its shelves. With one exception, the officers, Vice-Presidents, and Council, are the same as last year. Mr. Crisp has been compelled by increasing pressure of work to resign the post of Hon. Treasurer, which he has held with so much profit to the Association since its foundation. His place is taken by Mr. E. L. Beckwith, formerly a Prime Warden of the Fish mongers' Company, and Mr. Crisp retires to the Council *vice* Mr. W. Caine, M.P.

The Council of the Library Association has decided to hold the twelfth annual meeting of the Society in London during the second week in September. The Masters of the Bench of Gray's Inn have placed their Hall, for the third time, at the disposal of the Association.

On the night of June 15, about 10 p.m., there was a shock of earthquake, accompanied by a heavy subterranean rumbling, in the villages around Lake Arresö, in Denmark. Windows rattled and furniture oscillated, and in one place people ran out, believing that a powder-mill had exploded. The sound seemed to come from the east.

On June 12, at 11.16 p.m., a brilliant orange-coloured meteor was seen at Copenhagen. It radiated at τ Leonis, moving slowly towards β Virginis, where it burst into many fragments. It was accompanied by a tail, pointed at the end, about 2° in length.

In 1886 the Prince of Monaco, wishing to study the course of the Gulf Stream, threw into it some copper flasks from the *Hirondelle*. Three of these flasks have come ashore on the south coast of Iceland, two near the Ö Mountains, in the Rangárvall district, and the third at Flöj, in the Arnaes district.

It appears that the meteoric stone found in Scania, and acquired by Baron Nordenskiöld for the National Museum at Stockholm (p. 179), fell on April 6, and that its fall was accompanied by a red flash like lightning and a thunder-like detonation. It weighs 11 kilogrammes, and had made a hole 30 centimetres in depth; but, having recoiled, it lay on the level ground at the edge of the hole. The colour is greyish-black, and the fracture greyish-white. From a hasty analysis made by Herr A. Wingårdh, of Helsingborg, the chief mass appears to consist of manganese, in which are yellow and grey particles of metal. The meteorite seems to have been in a red-hot state, being covered with a glazed coating of fused metal half a millimetre in thickness.

Through the efforts of Dr. Filip Trybom, the Swedish Oyster-culture Society is attempting to acclimatize the American oyster, imported from Connecticut, in several places along the coast of the province of Bohus. The young oysters seem to thrive well.

The Norwegian cod-fishery in Finmarken this spring has been above the average, viz. 16,000,000 fish, against 9,000,000 last year. In Lofoten the fishery was a good average one, yielding 20,000,000 fish. Here the shoals congregated off the fishing-bank, in deep water, some five to ten miles from the shore.

On both sides of the Jösen Fjord, on the west coast of Norway, mountains rise perpendicularly to a height of several thousand feet. One morning, some days ago, stones and rocks, some of which are said to have been as large as a house, began to fall on the western side of the fjord. The avalanche continued for over two hours, accompanied by a noise heard 10 miles distant. A black cloud settled over the fjord, the water of which was in terrible commotion for many hours.

At a recent meeting of the Scientific Society of Copenhagen, Prof. Steenstrup gave an account of the results of his examination, last year, of the great mammoth deposit at Predmost, in Moravia. Dr. Wankel and Prof. Maschka, who have devoted much attention to the subject, are of opinion that the mammoths whose remains are found in this district were killed by man, and that their bodies were dragged thither to be eaten. Prof. Steenstrup, on the contrary, holds that the mammoths themselves sought the locality, and that they must have died from want of water, or from some other cause with which man had nothing to do. The splits in the remains are due, he thinks, to the action of water and sand, and afford no support to the notion that the knuckles were cleft for the sake of the marrow. It is certain that some of the bones have been exposed to the action of fire; but Prof. Steenstrup maintains that the traces of fire may be due to the fact that fires were at one time lighted upon them. On some of them, decorative lines have been scratched, but these may have been made long after the mammoth was extinct in Moravia. The lines, according to Prof. Steenstrup, are identical with the ornamentation of pottery of the Neolithic Age.

Mr. Bosworth-Smith, in a report on the Kolar Gold Field, in Southern India, issued by the Madras Government, records some "finds" of old mining implements, old timbering, fragments of bones, an old oil lamp, and broken pieces of earthenware, including a crucible, the remains of ancient mining operations. He expresses astonishment at the fact that the old miners were able to reach depths of 200 or 300 feet through hard rock, with the simple appliances at their command; and he describes the method which he thinks they pursued, sinking pits at short distances from each other, and leaving a "bar" between to prevent falling in.

Herr Richard Andree has issued a new series of his ethnographical parallels and comparisons. Ten years ago the first series appeared, and now, as then, the system pursued is to select a particular topic and then range over the whole literature of ethnology in search of references to the particular subject and collate them, until, finally, an ethnological monograph on the topic in question is produced. This method of work is of course exceedingly laborious, but it has the merit of being exhaustive and effective. Single subjects are thus worked out, and the results published in some scientific periodical; as soon as one is concluded another is taken up, and so on. By and by material for a volume is accumulated, the various subjects are brought up to date, and the public gets a work on ethnology, conceived on a novel plan, and full of interest. Each topic is pursued all over the earth, from country to country, with marvellous industry. The present volume deals with such topics as red hair, albinos, games, masks, marks of property, superstitions connected with the chase, "tree and man," circumcision, drawing amongst primitive peoples, thunderbolts, money for the dead, emotional expressions and gestures, demoniacs and mental disorders, &c.

THE latest number of the Journal of the Asiatic Society of Bengal (vol. lvii. part ii. No. 4) contains, among other papers, some interesting notes, by Prof. J. Wood Mason, on objects from a Neolithic settlement recently discovered by Mr. W. H. P. Driver, at Ranchi, in the Chota Nagpore district. Among the objects described, and represented on plates, are some chisel-edged arrow-heads similar to those which have been found in Egyptian tombs—in several cases still secured by bitumen to the shaft—and on Neolithic sites in different parts of Europe, including the British Isles.

THE Trustees of the Indian Museum, Calcutta, have issued the first number of a Catalogue, by Prof. J. Wood Mason, of the Mantodea, with descriptions of new genera and species, and an enumeration of the specimens, in the collection of the Museum. This number consists of 48 pages, and is illustrated with 34 woodcuts.

WE have to welcome the first report on Greek climate published in Greek by Prof. Sp. E. Marinos, of Corfu. Some meteorological reports relating to Greece have at times appeared in the Proceedings of the Society Parnassus at Athens, but these have mostly emanated from the late Baron Sina's observatory, directed by Dr. Schmidt, have related to Attica, and have been published in German. The present paper is a short notice of the records of Corfu for 1887 and 1888.

THE Annual Report of the Director of the Mauritius Observatory for the year 1887 shows that the mean temperature of the year was 1°·4 below the average, and that the temperature was below the average in every month, the greatest deviation being 2°·5 in August. No storm passed near the island, which has not been visited by a hurricane since March 1879. During the year 1887 the velocity of thirty miles an hour was reached only once, in June. Rainfall is recorded at seventy-five stations, and Dr. Meldrum states that the comparisons made during the last ten years show conclusively that there is a close connection between the rainfall and the malarial fever on the low lands.

IN his last Meteorological Report for India, Mr. Elliot, referring to sun-spots and weather in India—a subject which has been frequently mentioned in these Reports—says:—"So far as India is concerned, it would appear that it is the period of minimum sun-spots which is associated with the largest and most abnormal variations of meteorological conditions and actions. Thus, exceptionally heavy snow fell in the North-West Himalayas in the winter of 1866, and again in 1876 and 1877. The latter is to some extent described in the Annual Reports on the meteorology of India for these two years. Again, the most striking and disastrous famines of recent years in India have occurred near the period of minimum sun-spots; as, for example, the Orissa famine of 1866, the Behar famine of 1874, and the Madras famine in 1876–77. Similarly, there is a clearly marked tendency for the largest and most intense cyclones to occur shortly before the period of minimum sun-spots; as, for example, the great Calcutta cyclone of 1864, in which 60,000 people were drowned by the storm-wave, and the still larger Backerganj cyclone of 1876, in which 100,000 lives were lost by drowning. As we are now approaching or passing through the same phase of the sun-spot period, it is interesting to inquire whether there are any large abnormal variations common to the present period of minimum sun-spots, and the previous corresponding periods of 1865–66 and 1876–77."

THE Manchester Microscopical Society has published its ninth Annual Report, with a Presidential address, by Prof. Milnes Marshall, on "Inheritance," and a lecture, by Prof. W. Stirling, on "Electrical Phenomena in Animals." The volume also contains many papers and communications by members of the Society.

THE new number of the *Mineralogical Magazine* opens with a valuable paper, by Mr. Fletcher, on crystals of percylite, caracolite, and an oxychloride of lead (daviesite) from Mina Beatriz, Sierra Gorda, Atacama. The number also contains, besides some shorter papers, an article by Prof. Judd, on the processes by which a plagioclase felspar is converted into a scapolite.

THE ninth part of Cassell's "New Popular Educator" has just been published. It contains a good map of the world, showing isothermal lines, and the distribution of races and vegetation.

THE first twelve numbers of *Life Lore*, a monthly magazine of biology, have now been collected in a volume, a copy of of which has been sent to us. The volume contains many brightly-written articles, and should do much to excite the interest of young readers in the more popular aspects of biological science.

PROF. MILTON WHITNEY, Professor of Agriculture and Vice-Director of the Experiment Station of the University of South Carolina, has devised a modification of Six's thermometer for soil temperature. The bulb is 6 inches long, protected by a metallic cylinder perforated with many holes, and is buried in the soil, so that the bulb shall extend from 3 to 9 inches below the surface of the soil—the maximum and minimum scale being of course above the soil, and arranged very much as in the ordinary Six's form. The long bulb allows a good height of scale, while it is narrow enough to respond readily to changes of temperature. In a series of readings the instrument gives exactly the mean of the readings of a 3-, 6-, and 9-inch thermometer of the usual form placed beside it at these respective depths in the soil, besides recording the maximum and minimum temperatures. This length of bulb and depth in the soil was decided on, as it is assumed to be the depth which contains most of the roots of the ordinary cultivated plants. The instrument need only be read once a day, and saves an immense amount of calculation and tabulation attending the tri-daily readings of the 6 to 8 instruments comprising the set of the usual form.

THE Russian Geographical Society is now publishing its Memoirs in parts, each of which contains a separate paper, and is circulated as soon as the paper has been printed. The last parts of the "Memoirs of General Geography" contain the following interesting papers:—"The Agricultural Meteorological Observations in Russia in 1885 and 1886," by Dr. Woeikof, being the observations made at fifty-one different stations in accordance with a scheme issued by the Geographical Society; "On Barometrical Observations at Distant Stations and during Journeys," by R. N. Savelieff; "On the Measures taken in Western Europe for consolidating Shifting Sands, and growing Bushes and Trees upon them," by S. Rauner; and "On the Comparison between Normal Barometers of the Principal Meteorological Stations of Europe," by P. Brounow. It appears that the corrections to be applied to the pressures shown by the barometers of the following stations are as follows (the normal barometer at St. Petersburg being taken for zero):—Berlin, − 0·02 millimetres; Hamburg, − 0·39; Utrecht, − 0·32; Brussels, + 0·23; Paris, + 0·11; Sèvres (Bureau International des Poids et Mesures), + 0·10; Zurich, − 0·06; Vienna, + 0·11.

IN a communication lately made to the Russian Geographical Society, General Annenkoff insisted upon the possibility of the

Transcaspian region being colonized. He pointed out that the girdle of loess which encircles the mountains is quite as productive as the loess in China. The climate is, of course, quite different—not so much on account of the want of rain (the amount of rain at Merv, during the winter months of 1885-86, reached the very high figure of 1654 millimetres), as on account of its absence during the summer months. But the rivers of the region—the Amu, the Tejen, and the Murghab—if their waters were utilized for irrigation, instead of being lost in the sandy deserts, would supply the amount of water necessary for irrigating immense tracts of land. All that is wanted to make of Central Asia a rich oasis of agriculture is human labour and human intelligence. The soil, when irrigated, is not inferior in fertility to the fertile loess fields of China.

THE Inspector-General of Indian Affairs in Canada, in his Report for the past year, urges the continuation of the policy of amalgamating the Canadian Indians as far as possible with the surrounding population, especially by inducing them to adopt agriculture and handicrafts, so that they may acquire a taste for a settled life. The Indians of the Province of Ontario seem to be most successful in reaching this end, and in all cases the tribes show an increase in numbers. There were no serious disturbances during the year, and the only excitement was produced amongst the natives on the Upper Skeena, in British Columbia, owing to the arrest and execution of one of their number for murder. The total number of Indians in the Dominion is given at 124,589; of these, 37,944 are in British Columbia, 26,368 in Manitoba and the North-Western Territory, 17,700 in Ontario, 12,465 in Quebec, 8000 in Athabasca, 7000 in the Mackenzie district, 4016 in Eastern Rupert's Land, 4000 on the Arctic coasts, 2145 in New Scotland, 2038 in the Peace River district, 1594 in New Brunswick, 1000 in the interior of Labrador, and 319 in Prince Edward's Island. 6127 Indian youths and girls attend the schools provided for them, nearly half the pupils belonging to Manitoba and the North-Western Territories; of the 956,000 dollars appropriated for native affairs, the same districts received 876,000 dollars. 21,344 acres of the land set apart for the improvement of Indians were alienated during the year, and 458,283 acres still remain.

THE additions to the Zoological Society's Gardens during the past week include a Black-headed Lemur (*Lemur brunneus* ♂) from Madagascar, presented by Mr. Charles C. Stewart; four Angora Goats (*Capra hircus*, var., ♂ ♀ ♀ ♀) from Barrol, Cape Colony, presented by Messrs. Theophilus Bros.; a Two-spotted Paradoxure (*Nandinia binotata*) from West Africa, presented by Mr. Philip Lemberg; a Ring-necked Parrakeet (*Palæornis torquatus*, yellow var.) from India, presented by Colonel C. Swinhoe; a Common Kestrel (*Tinnunculus alaudarius*), British, presented by Master W. P. Teil; two Goshawks (*Astur palumbarius*), European, deposited; a —— Lemur (*Hapalemur*, sp. inc.) from Madagascar, two Undulated Grass Parrakeets (*Melopsittacus undulatus*) from Australia, purchased; a Thar (*Capra jemlaica*), two Mule Deer (*Cariacus macrotis* ♂ ♂), three American Wild Turkeys (*Meleagris gallo-pavo*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

ASTRONOMICAL SOCIETY OF THE PACIFIC.—The second of the publications of this Society has appeared, containing an address delivered before the Society on March 30, 1889, by the President, Prof. Holden, on "The Work of an Astronomical Society." Prof. Holden sketched the state of astronomical science in England immediately prior to the foundation of the Royal Astronomical Society, and quoted freely from its first paper as showing the spirit which should inspire similar organizations elsewhere. The points Prof. Holden especially put forward as to be attained by the new body were the publication of observations, the guidance of amateur workers, especially those who were skilled photographers, the instruction of learners, the formation of an astronomical library, together with all the advantages resulting from free discussion, and the friendly interchange of ideas. The address was able, straightforward and unpretentious, and concludes with the recommendation, "Whatever we do, let us do thoroughly. Whatever we say, let it be well considered. Let us clearly understand the objects for which we are organized, and let us pursue these with entire confidence." With these principles for its guidance, the new Society will not fail of an honourable and useful career.

A NEW COMET.—A new comet was discovered by Mr. E. E. Barnard (Lick Observatory) on June 23·9499 G.M.T., R.A. 20° 13′ 21″, N.P.D. 51° 9′ 16″; daily motion, R.A. + 1° 6′, N.P.D. − 0° 34′. The comet was only faint.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *JULY* 7–13.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on July 7

Sun rises, 3h. 55m.; souths, 12h. 4m. 40·03.; daily increase of southing, 9·5s.; sets, 20h. 15m.: right asc. on meridian, 7h. 7·3m.; decl. 22° 33′ N. Sidereal Time at Sunset, 15h. 19m.

Moon (Full on July 12, 21h.) rises, 13h. 46m.; souths, 19h. 16m.; sets, 0h. 34m.*: right asc. on meridian, 14h. 19·8m.; decl. 8° 49′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	2 49	10 40	18 31	5 42·3	19 44 N.
Venus.....	1 18	8 51	16 24	3 53·4	16 46 N.
Mars......	3 21	11 41	20 1	6 43·5	23 55 N.
Jupiter....	19 9	23 3	2 57*	18 7·6	23 18 S
Saturn....	6 55	14 24	21 53	9 26·8	16 14 N.
Uranus...	12 33	18 4	23 35	13 7·1	6 28 S.
Neptune..	1 16	9 5	16 54	4 7·6	19 18 N.

* Indicates that the setting is that of the following morning.

July.	h.	
10	8	Venus at greatest elongation from the Sun, 46° west.
11	14	Jupiter in conjunction with and 0° 52′ south of the Moon.
12	10	Mercury at greatest elongation from the Sun, 21° west.
12	—	Partial eclipse of the Moon; first contact with shadow, 19h. 43m.; middle of eclipse, 20h. 54m.; last contact with shadow, 22h. 5m. In England the Moon rises generally before the middle of eclipse.

Saturn, July 7.—Outer major axis of outer ring = 37″·4; outer minor axis of outer ring = 9″·2: southern surface visible.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·5	81 17 N.	July 9,	21 25	*m*
S Ursæ Majoris	12 39·1	61 42 N.	,, 11,		*m*
W Ophiuchi	16 15·4	7 26 S.	,, 8,		*M*
U Ophiuchi	17 10·9	1 20 N.	,, 8,	0 5	*m*
			,, 13,	0 51	*m*
X Sagittarii	17 40·6	27 47 S.	,, 7,	23 0	*M*
Y Sagittarii	18 14·9	18 55 S.	,, 11,	3 0	*m*
β Lyræ	18 46·0	33 14 N.	,, 12,	1 0	*M*
R Aquilæ	19 1·0	8 4 N.	,, 12,		*m*
S Sagittæ	19 51·0	16 20 N.	,, 10,	0 0	*m*
R Sagittæ	20 9·0	16 23 N.	,, 12,		*m*
V Cygni	20 37·7	47 45 N.	,, 10,		*M*
T Vulpeculæ	20 46·8	27 50 N.	,, 13,	2 0	*m*
R Vulpeculæ	20 59·5	23 23 N.	,, 10,		*m*
δ Cephei	22 25·1	57 51 N.	,, 13,	22 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl.	
Near 96 Herculis	270	21 N.	Very slow.
	280	14 S.	Slow.
,, π Cygni	330	36 N.	Swift; red streaks.
,, ι Andromedæ	352	40 N.	Swift.

OPTICAL TORQUE.[1]

I.

SEVENTY-EIGHT years have elapsed since the first discovery, by Arago, of the remarkable chromatic effects produced by slices of quartz crystals upon light, previously polarized, which was caused to traverse them. These effects were shown, one year later, by Biot, to be caused by a peculiar action of the quartz in rotating the plane of polarization; the amount of the rotation being different for lights of different colours. Ever since then, the rotation of the plane of polarization of light has been a topic familiar to physicists. It has stimulated the devotee of research to an endless variety of experiments and suggestive speculations: it has lured on the mathematician to problems which tax his utmost skill: it has afforded to the lecturer an array of beautiful and striking illustrations. Here, in this place, made classical by the researches and expositions of Thomas Young, of Michael Faraday, and of William Spottiswoode, and last, but not least, by the labours of those eminent men whom we rejoice still to number amongst the living—here, I say, on this classic ground, the rotation of the plane of polarization of light is almost a household word, and its phenomena are amongst the most familiar. We know now that not only certain actual crystals, such as quartz, bromate of soda, and cinnabar, rotate the plane of polarization, but that many non-crystalline bodies—liquids, such as turpentine, oil of lemons, solutions of sugar and of various alkaloids, and even certain vapours, such as that of camphor—possess the same property.

In 1845, at the very culminating point of his unique career of research, Faraday opened a new field of inquiry, linking together for the first time the science of optics with that of magnetism, by his discovery that the rotation of the plane of polarization of light could be effected by the application of magnetic forces. This effect he observed first in his peculiar "heavy-glass," when it lay in a powerful magnetic field. Subsequently he found other bodies to possess similar properties: some of these being magnetic liquids, such as solutions of iron, others being diamagnetic. Time will only permit me in passing to refer to the researches of Verdet, and those of Lord Rayleigh and of Mr. Gordon upon the numerical values of the magneto-optic rotation in these substances. H. Becquerel has extended them to gases, and has shown how the magnetism of the earth rotates the plane of polarization of the light which, previously polarized by reflection from the aërial particles which give the sky its "blue," passes earthward through the oxygen of the air.

Other experimenters have dealt with the rotatory effects (whether crystalline, molecular, or magnetic) in relation to lights of different colours, and have studied the dispersion which arises from the greater actual angle of optical torsion which is produced upon waves of short wave-length (violet and blue) than that which is produced under the influence of equal rotatory forces upon the waves of longer wave-length (red and orange). It has also been demonstrated that the plane of polarization of waves of invisible light, whether those of the infra-red, or those of the ultra-violet species, if they have been previously polarized, can be rotated just as can that of waves of visible light.

In 1877, Dr. Kerr, of Glasgow, discovered a point which Faraday had sought for, but fruitlessly—namely, that in the act of reflection at the pole or surface of a magnet, there is a rotation of the plane of polarization of light. This discovery was completed in 1884 by Kundt, of Strasburg, by the further demonstration, also dimly foreseen by Faraday, that a magneto-optic rotation of the plane of polarization is caused by the passage of previously polarized light through a normally magnetized film of iron so thin as to be transparent.

Lastly, in this brief enumeration, we were shown a month ago, by Oliver Lodge, how the magnetic impulses generated by the rapid oscillatory discharges of the Leyden jar can produce corresponding rapid oscillatory rotation in the plane of polarization of the waves of previously polarized light.

You will not have failed to notice the cumbrous phrase which, whether in speaking of the purely optical effects (of quartz, or sugar, or turpentine), or in speaking of the magneto-optic effects of more recent discovery, I have employed to connote a very simple fact. You may have wondered that any lover of simple English speech should indulge in such sesquipedalian words.

Of course, at this period of the nineteenth century it is no longer open to debate that light consists of waves. The plane of polarization of the waves of light is the plane of polarization of the light itself. The rotation of the plane of polarization is the rotation of the polarized waves, and therefore of the polarized light itself. Yet I must draw attention to the fact that in all the array of discoveries which I have enumerated, that which had been observed was the rotation—whether by crystalline, molecular, or magnetic means—not of natural light, but of light which had by some means been previously polarized. It was not known to Arago or to Biot, to Fresnel, to Faraday, nor even to Spottiswoode or to Maxwell, that natural unpolarized light could be rotated. They may have inferred so, but it was not in their time even demonstrable that a beam of circularly-polarized light could be rotated upon itself in the same sense as that in which a beam of plane-polarized light could be rotated.

That light of any and every kind, however polarized or devoid of that which is called polarization, can be, and in fact is, rotated when it passes across a slice of quartz or along a magnetic field, is a wider generalization of more recent date; but one of the reality of which I hope to convince you before the warning finger of the clock puts a period to my discourse.

In order the better to enable this audience to comprehend the ultimate significance of this discovery, I must claim the indulgence of those amongst them who are already familiar with the subject of the polarization of light, whilst I go back to the most simple elementary matters. Having illustrated the fundamental facts about the plane of polarization of light and its twisting, I shall then go on to methods of precisely measuring the amount of optical torsion produced by the various substances under various conditions. And after dealing with the magnetic as well as the crystalline and molecular methods of producing optical torsion in the case of light that has been previously polarized into a given plane, I shall be in a position to speak of the nature of the torque,[1] or twisting force, which in the several cases produces the torsion; and shall finally endeavour to indicate the scope of the researches by which it is now definitely ascertained that the very same optical forces which are capable of impressing a rotation upon light which has been artificially polarized into a definite plane are also capable of impressing a rotation upon natural, non-polarized light.

At the outset, to elucidate to any who may not comprehend the meaning of the term polarization as applied to wave-motion, I will show a simple apparatus, constructed from my designs by Mr. Groves. In this there are two sets of movable beads, fixed upon stems which pass into a box containing a piece of mechanism actuated by means of a handle. These beads, when I turn the handle, oscillate to and fro in definite directions, and, by their successive motions, give rise to progressive waves. One set of beads, tinted red, executes movements in a plane inclined 45° to the right, another set, silvered, simultaneously executes movements at 45° to the left. There are therefore here two waves, the planes of polarization of their movements being at right angles to one another. Their velocity of march is equal; but in this model, as a matter of fact, their phases differ by one-quarter—that is to say, each successive wave of the one set is always a quarter of a wave-length behind the corresponding wave of the other set. [Model exhibited.]

Now, in the case of waves of natural light from all ordinary sources—sun, stars, candles, gas-flames, or electric light—the waves emitted are not found to be polarized. That is to say, their motions are not executed in any particular plane, nor even in any particular path of any kind; they appear to be absolutely heterogeneous at least so far as this, that no vibration of the millions of millions emitted in a second of time is followed by more (on the average) than about 50,000 vibrations of a similar sort, executed along a similar path—the plane of the polarization, if any, changing after the lapse of such an incredibly short time that for most purposes the vibrations in different directions are as inextricably mixed as if they had all been simultaneously jumbled up. Since, then, natural light is non-polarized or miscellaneous, the production of polarized light must be brought about by the employment of polarizing apparatus or agents which will so operate on or affect the mixed waves as to bring their vibrations into one direction—or, what amounts to the same thing, transmit the light whilst destroying or absorbing those

[1] A Discourse delivered at the Royal Institution, May 17, 1889, by Prof. Silvanus P. Thompson.

[1] The convenient term *torque* was first proposed by Prof. James Thomson, of Glasgow, for the older and more cumbrous phrase "moment of couple," or "angular force." Its general acceptance by engineers justifies the extension of the term to optics. As a mechanical torque is that which produces or tends to produce mechanical torsion, so optical torque may be defined as that which produces or tends to produce optical torsion.

parts of the vibrations which are executed *across* the desired line of vibration. So we have *polarizers* consisting of tourmaline slices; oblique bundles of thin glass plates; black-glass reflectors; and Nicol prisms cut from calc-spar. About the two latter I may be permitted a passing word presently. These objects polarize, *i.e.* turn into one plane, the vibrations of light falling upon them. A rough mechanical illustration may here be permitted me. A long india-rubber cord is passed through the open ends of a box provided with vertical partitions. Fig. 1 shows the arrangement. These partitions confine the motion of the cord, and effectually polarize the vibrations which I now impart to the cord by shaking the end of it to and fro. If the partitions are vertical,

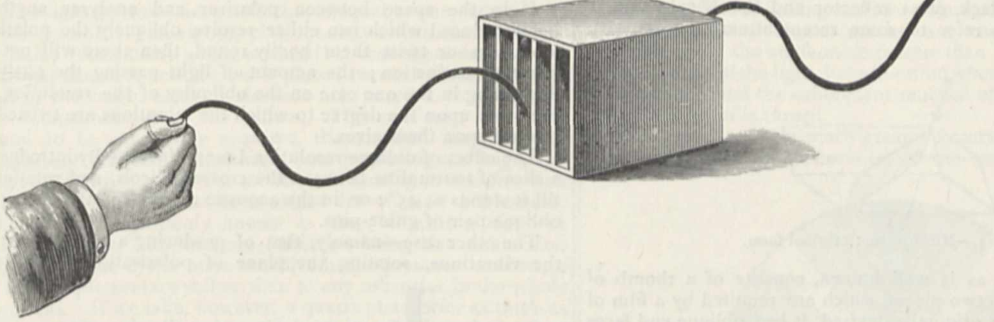


FIG. 1.—Box with partitions to illustrate polarization of vibrations.

the box polarizes, into vertical vibrations only, the miscellaneous vibrations which are sent to it. If rotated until its partitions are horizontal, it polarizes the vibrations into a horizontal position.

Let us now turn to the optical analogue of this experiment. The large Nicol prism which I introduce into the field of the electric-light lantern, polarizes the light, so that the vibrations are executed simply in an up-and-down direction. Your eye will not detect this, the motions being millions of times too rapid. To detect the direction, an analyzer is necessary. For this purpose a second apparatus of the same sort is used, for then, by crossing the positions of the two, the whole of the light is cut off; the second Nicol prism, if set so as to transmit only horizontal vibrations, cutting off the vertical vibrations that are sent through the first prism. So, whilst the first prism serves as

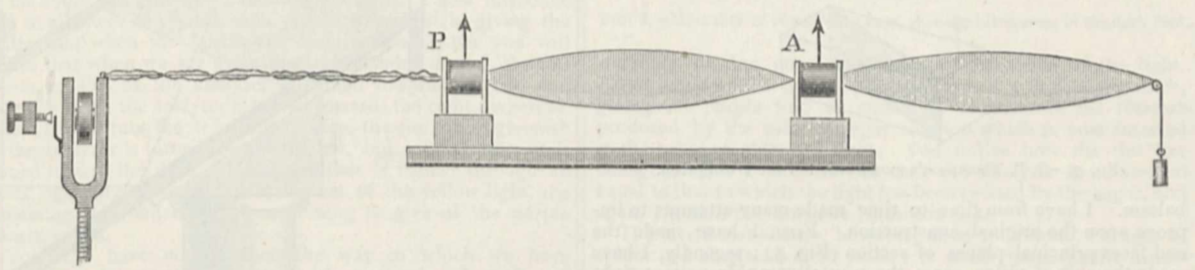


FIG. 2.—Acoustic model illustrating polarization of vibrations. P, the polarizer; A, the analyzer.

a polarizer, the second serves as an analyzer to detect by cutting them off when turned to the proper position, the direction of the polarization which had been previously impressed by the first prism.

Here I may illustrate the action of the analyzer for determining the plane of polarization of the vibrations, by the extinction which it produces when turned to the crossed position. For this purpose I have refined upon the box with partitions, using instead parallel plates of glass mounted in wooden cylinders, whilst for the cord swung by hand I am using Prof. Schwedoff's device, and am producing the vibrations in this silken cord by means of an electrically-driven tuning-fork (Fig. 2). At the first nodal point of the stretched cord a pair of parallel glass plates acts as a polarizer, the cord from that point vibrating in the

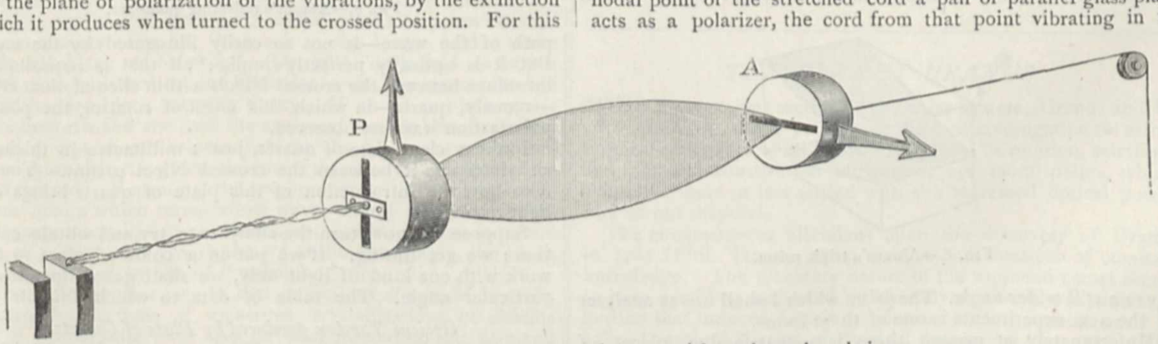


FIG. 3.—Vibrations cut off by turning the analyzer A at right angles to the polarizer P.

plane thus imposed upon it. I can alter this plane at will by rotating the polarizer. This polarizer, P, consisting of a pair of glass plates, is mounted in a cylindrical mount, and is provided with an arrow to indicate their direction. If now at any subsequent node I introduce a second such device, it will act as an analyzer, A. This excellent suggestion is due to M. Macé de Lepinay. In Fig. 2 the polarizer and analyzer are parallel. You see (Fig. 3) how the vibration is extinguished when the positions of analyzer and polarizer are crossed. Half a degree of error in the position of the analyzer produces something less than perfect extinction of the vibrations. Hence it is possible, by this analyzer, to determine the plane of the vibrations to the accuracy of half a degree. I should say that the whole of this model has been constructed by my assistant, Mr. Eustace Thomas.

Now let me show you the optical effect which corresponds to this. Placing a second Nicol prism as analyzer in the path of the polarized waves, I turn it to the position where it cuts off the polarized light. The "*dark field*" so produced by the crossed Nicol prisms corresponds to the motionless cord beyond the crossed analyzer of the acoustic apparatus.

Returning for a moment to two well-known forms of polarizing apparatus, viz. the black glass reflector and the Nicol prism, I may be permitted to refer to some recent attempts to improve upon these devices.

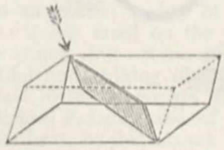


Fig. 4.—Nicol prism: original form.

The Nicol prism, as is well known, consists of a rhomb of Iceland spar cut into two pieces, which are reunited by a film of Canada balsam. As originally devised, it had oblique end faces (Fig. 4), and a comparatively narrow angle (19°) of aperture. These may be noticed in the small example which I here exhibit, which is an original constructed by William Nicol himself. It also has the disadvantage of giving a field in which the directions of the planes of polarization are not strictly parallel to one another throughout its whole extent. Consequently there is never complete extinction of light all over the field at one time. Hartnack and others have attempted to remedy this by giving the prism a different form and using other materials than Canada

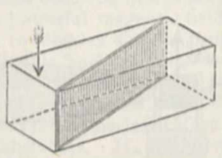


Fig. 5.—S. P. Thompson's modification of the Nicol prism.

balsam. I have from time to time made many attempts to improve upon the original construction. First, I have made the end faces principal planes of section (Fig. 5); secondly, I have made the axis of vision cross the crystallographic axis at right angles, so getting a flatter field, a shorter length, a wider angle, and less loss of light by reflection. Mr. Ahrens, the prism-cutter, on whose able assistance I have relied during the last six or seven years in cutting these prisms, has aided me with his ingenuity in devising a method of cutting up the spar so as to give these advantages with a minimum waste of material. He has further devised a method of putting a polarizing prism together in three instead of two pieces—illustrated in the diagram (Fig. 6)—which

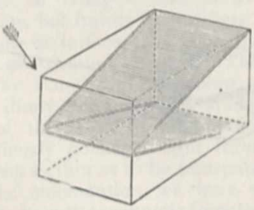


Fig. 6.—Ahrens's triple prism.

gives a still wider angle. The prism which I shall use as analyzer in the next experiments is one of these forms.

Unfortunately at present there is a spar-famine, pieces of Iceland spar of a size and purity suitable for the making of large polarizers such as that I employ being not now procurable at any price. To avoid the excessive cost of large Nicols I have lately got Mr. Ahrens to construct for me a large reflection-polarizer, on the plane of Delezenne, but modified by Mr. Ahrens in detail. In this prism the light is first turned to the proper polarizing angle by a large total-reflection prism of glass, and then reflected back, parallel to its original path, by impinging upon a mirror of black glass covered by a single sheet of the thinnest patent plate-glass to increase the intensity of the light. This form of polarizer, depicted in Fig. 7, is quite equal for projection purposes to a Nicol prism of equal aperture, and is much less costly. This one has $2\frac{1}{2}$ inches clear aperture.

Having so far reviewed the apparatus for polarizing and analyzing, I will return to the apparatus set with its prisms crossed, so that the analyzer completely extinguishes the polarized light emitted from the polarizer.

If in the space between polarizer and analyzer anything be introduced which can either resolve obliquely the polarized vibrations or twist them bodily round, then there will not be complete extinction; the amount of light passing the analyzer depending in the one case on the obliquity of the resolution, in the other upon the degree to which the vibrations are twisted or rotated upon themselves.

The effect of oblique resolution I may illustrate by introducing a slice of tourmaline between the crossed Nicols, and rotating it till it stands at 45°; or, in the acoustic model, by introducing an oblique pair of guide-pins.

The other case—namely, that of producing a bodily twist of the vibrations, rotating the plane of polarization around the

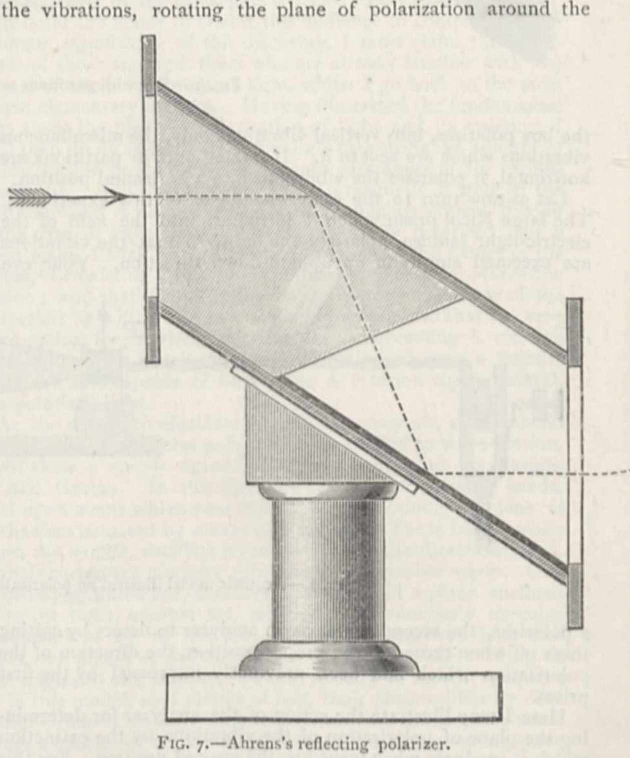


Fig. 7.—Ahrens's reflecting polarizer.

path of the wave—is not so easily illustrated by the model. But it is optically perfectly simple: all that is requisite is to introduce between the crossed Nicols a thin slice of that crystal —namely, quartz—in which this effect of rotating the plane of polarization was first observed.

I take a clear plate of quartz, just 1 millimetre in thickness, and interpose it between the crossed Nicol prisms. You will note how the introduction of this plate of quartz brings *some* light into view.

Suppose we now turn the analyzer to try and obtain extinction: we get tinting. If we put in a coloured glass so as to work with one kind of light only, we shall get extinction at a particular angle. The table of data to which I invite your

Optical Torsion produced by Plate of Quartz.

	1 millimetre. (°)	3·75 millimetres. (°)
Red	19	71·2
Orange	21·5	80·6
Yellow	24	90
Green	29	108·7
Peacock	31	116·2
Blue	35·5	133·1
Violet	42·8	161

attention states this amount for the different colours. If we use a piece of quartz so thick that it rotates any particular tint just 90°, that tint will be cut off by the crossed analyzer, and all others will—in greater or less proportion—be transmitted, so that the resulting tint will be complementary to that cut off. For example, a slice just so thick as to twist yellow waves round 90° must be 3·75 millimetres thick. (I may remark, for the benefit of those who think it easier to express this exact thickness in fractions of a British inch, that the quartz which rotates yellow light 90° must have a thickness equal to one-eighth, plus three-sixteenths of an eighth, plus one sixty-fourth of an eighth of an inch.) When such a quartz is placed between the crossed Nicols, the light shown is yellow; but if placed between parallel Nicols (*i.e.* in the bright field), it shows a rich purplish-violet colour, the complementary of the yellow. This particular tint Biot found to be excessively sensitive, the smallest inaccuracy in adjustment between the prisms at once producing a change, the colour appearing too red or too blue, according to the direction in which the analyzer has been turned out of exact adjustment. This tint is accordingly known as the "transition tint" or "sensitive tint," its accurate definition being due to the fact that the human eye is more sensitive to the presence or absence of the complementary yellow than to any other tint in the whole spectrum. If we take, however, a quartz plate twice as thick as this—namely, 7½ millimetres thick—this will give the yellow light a torsion of 180°. Hence this gives the purple transition tint in the dark field, and is yellow in the bright field. A quartz plate 11¼ millimetres thick gives again a transition tint in the bright field. I shall recur presently to the question of the transition tints of the several orders.

One of the familiar facts in this subject is that there are two kinds of quartz crystals, optically alike in every other respect, differing only in this, that one kind produces a right-handed twist, the other kind a left-handed twist. All the pieces of quartz I have so far employed are right-handed specimens. I now introduce two small slices of crystal, each 3¾ millimetres thick, giving the yellow tint when the Nicols are exactly crossed, but you will notice that when we are using the right-handed crystal, the tint grows reddish as the analyzer is turned towards the left, and greenish when the analyzer is turned towards the right; whereas, when I substitute the left-handed slice, the tint grows greenish as the analyzer is turned toward the left, and reddish when it is turned toward the right. If the analyzer is turned through an exact right-angle, we get an extinction of the yellow light, the remaining blue and red rays combining to give us the purple transition tint.

You will have noticed that the way in which we have (approximately) measured the angle of rotation has been first to set the analyzer to extinction, then to introduce the substance which has the property of rotating the beam, then to turn the analyzer again to extinction, and read off its angle. For, of course, the angle through which the analyzer is turned measures the angle through which the plane of polarization has been turned.

It is possible, however, to show in the lantern something like a more obvious rotation of the light by introducing between the Nicols a crystal star, built up of radial pieces of mica, twenty-four in number (Fig. 8). You see in the bright field a white cross with black sectors at 45°. Or, in the dark field we have a black cross with vertical and horizontal arms, the sectors next to those that are black seeming dusky. If now I put in a quartz plate between the star and the analyzer, you see the cross shift round, and it shows colours, because the blue rays are twisted round more than the green, the green than the yellow, the yellow than the red. Repeating the experiment with the 3·75 millimetre quartz which turns yellow waves round just 90°, we get this gorgeous radiation of colours, and our black cross is turned into a yellow one. With the 7·5 millimetre quartz, the black cross is replaced by one of "transition" tint.

The black crosses seen in certain sections of natural crystals, sphæroliths, sections of stalactites, crystallizations of salicine and of Epsom salts, may also be used instead of the 24-rayed star of mica. But best of all I find to be the beautiful black cross which is seen by polarized light in the prepared crystalline lens taken from the eye of a fish. You notice how, when the fish lens is projected and the quartz introduced, the cross turns round.

This is, however, a rough-and-ready way of displaying the rotation, and it is of vast practical importance that precise methods of measuring the angle of rotation should be available—of vast importance, because in several large industries this optical method is applied as a species of handy analysis. I have named a solution of sugar as being an "active" substance. In the industry of sugar-refining, as in that of brewing, the strength of sugar in the liquids is directly measured by measuring its optical effect. Consequently there has been developed a special instrument, the *polarimeter*, for this express purpose.

I have here examples of several practical forms of polarimeters; there are diagrams of several more upon the walls.

The problem of finding the best polarimeter naturally leads to the inquiry what special means are there for making the observation of the angle more precise than by merely observing the extinction of the light, its restoration when the active substance is interposed and the subsequent renewal of extinction when the analyzing prism is turned.

Biot considered that much greater accuracy could be attained by watching for the restoration of the sensitive tint than by

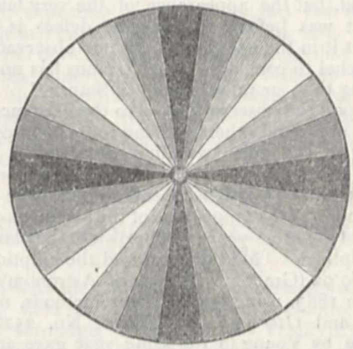


Fig. 8.—Mica disk of twenty-four rays, showing black cross in the dark field.

watching for the mere restoration of extinction of the light. Accordingly we will use the plate of quartz 7·5 millimetres thick, giving the purple tint, to enable us to measure the rotation produced by the tube of sugar solution which is now inserted in the beam of polarized light. You notice how the tint has changed. But I have only to turn the analyzer to an amount equal to that to which the light has been twisted by the sugar, and again I obtain the sensitive transition tint.

The eye is not always, however, alive to minute changes of colour in a single coloured patch; it much more readily distinguishes a minute difference between two tints when both are present at once. Hence Soleil devised the well-known biquartz arrangement, consisting of two pieces of crystal, equal in thickness but possessing opposite rotations. You will notice how the slightest inaccuracy in placing the analyzer causes the two halves of the field to differ in tint. This is especially marked when the tint chosen is the transition purple.

(*To be continued.*)

THE PLANET URANUS.

THAT anomalous section of the solar system, Uranus and its satellites, offers yet a wide field of investigation to astronomical specialists of all kinds. Its figure, its rotation, satellites, and physical constitution altogether, are moot points, which should be more or less settled with the increased optical power now at our disposal.

The circumstances attendant upon the discovery of Uranus in 1781 (Phil. Trans., 1781, p. 492) are matters of common knowledge. The planetary nature of the supposed comet seems to have been first suspected by Maskelyne, and it was this suggestion that induced Lexell to calculate for it a circular orbit in 1781 (Grant's "History of Physical Astronomy," p. 274). The elliptic elements of the planet were first calculated by Laplace in 1782 (*Mém. Acad. des Sciences*, Paris, January 1783).

The first measures of the Uranian diameter were singularly incongruous. Herschel found it in March 1781 to be 2″·53; whilst in the following month he measured it as 4″·52 and 5″·2, (Phil. Trans., 1781, p. 494). A like discordance occurred in the results obtained by other astronomers. Maskelyne fixed the magnitude of the apparent diameter as 3″, whilst Mayer, of

Manheim, estimated it to be as high as 10″ (Grant's "History of Astronomy," p. 275).

The difference between these measures of the diameter of Uranus would seem to be a consequence of the fact that it is always extremely difficult to get the planet clearly defined in the field of the telescope. Herschel himself noted (Phil. Trans., 1798, p. 69), "The Georgian planet is not so well defined as, from the extraordinary distinctness of my present 7-feet telescope, it ought to be. There is a suspicion of some apparatus about the planet."

Herschel several times had the impression that Uranus was surrounded with a ring. One of his observations is contained in the following (Philosophical Transactions, 1798, p. 68):—"My telescope is extremely distinct, and when I adjust it upon a very minute double star, which is not far from the planet, I see a very faint ray, like a ring crossing the planet, over the centre. This appearance is of an equal length on both sides, so that I strongly suspect it to be a ring. . . . I have turned the speculum one quadrant round, but the appearance of the very faint ray continues where it was before, so that the defect is not in the speculum nor is it in the eye-piece." Later observations, however, led Herschel to conclude "that Uranus has no ring in the least resembling that, or rather those, of Saturn."

Following upon the observations as to the existence of a ring round Uranus, are found others relating to its polar compression. The flattening at the poles was first observed by Herschel in February 1794 (Phil. Trans., 1798, p. 69), and announced in the following words: "The planet seems to be a little lengthened out, in the direction of the longer axis of the satellite's orbit;" and again in April of the same year: "The disk of the planet seems to be a little elliptical." Mädler measured the ellipticity in 1843, and found it 1/9·92 (Grant's "History of Astronomy," p. 278). Schiaparelli, in 1883, using two different methods, obtained the results 1/10·98 and 1/10·94 (*Astr. Nach.*, No. 2526). A few measures made by Young in the same year gave an ellipticity 1/14 ("General Astronomy," Young, p. 367). The fact that the ellipticity was in the same plane as the major axis of the satellite's orbit led Herschel to conclude from analogy with Jupiter and Saturn, "That the Georgian planet also has a rotation upon its axis of a considerable degree of velocity" (Phil. Trans., 1798, p. 71). Other observers of the bulging out of the Uranian equator—Schiaparelli, Young, Safarik—agree with Herschel in saying that the plane is coincident with that of the satellite's motion, but the following observations of markings on the surface of Uranus lead to an entirely different conclusion.

Buffham noticed some bright markings on Uranus in 1870–72 (*Monthly Notices*, vol. xxxiii. p. 164), and, from observations of their motion, deduced the time of rotation as twelve hours, but the plane of rotation was *not* coincident with that of the satellite's orbit. This was borne out by observations of dusky bands by Young, in 1883 ("Princetown Observations," 1883); of apparent equatorial belts by the brothers Henry, in 1884 (*Comptes rendus*, t. xcviii. p. 1419); and observations in 1884, at Nice, of a bright spot by Lockyer, Perrotin, and Thollon (*Comptes rendus*, t. xcviii. pp. 717, 967). The plane of rotation, according to these observers, is from 15° to 40° from the trend of the satellite's. Thus the difference between the two sets of observations amounts to nearly half a right angle. Does the error lie in the observation of the belts, or in the measurements of the planet's ellipticity and the satellite's orbit? This is an enigma which yet remains to be solved, and another character of Uranus requiring investigation.

Herschel made the first determination of the mass of Uranus in 1788 (Philosophical Transactions, 1788, p. 369), and found it to be 17·740612 as compared with the earth, or about 1/18,000 that of the sun. Bouvard found a value 1/17,918; Lamont, in 1837, 1/24,605. Lassell's observations of the motion of the satellites gave a value 1/20,897, whilst Struve's observations gave a value 1/26,860. The mass, 1/22,600, found by Newcomb ("Washington Observations," 1873), is probably the most correct, and he estimates that the probable error in the denominator is not more than 100. This mass, revolving round the sun at a mean distance of about 1800 millions of miles, must exert considerable influence upon bodies near it, influence which may often predominate over that of the sun.

But it is the question of Uranian satellites that is so enigmatical. Herschel discovered two on January 11, 1787 (Phil. Trans., 1787, p. 125 *et seq.*), and in 1798 announced the discovery of four more. Regarding the real existence of these four, Herschel remarks (Phil. Trans., 1798, p. 66): "It remains now only to be mentioned that, in such delicate observations as these of the additional satellites, there may possibly arise some doubts with those who are very scrupulous; but as I have been much in the habit of seeing very small and dim objects, *I have not been detained from publishing these observations sooner, on account of the least uncertainty about the existence of these satellites*, but merely because I was in hopes of being able soon to give a better account of them, with regard to their periodical revolutions."

Sir John Herschel observed the two brightest satellites between 1828 and 1832 (Mem. Ast. Soc., vol. viii. p. 1); but "of other satellites," he says, than these, "I have no evidence," although the telescope he was using was precisely similar to that used by his father. A systematic search was made by Lassell for the lost satellites, and a definite announcement of the discovery of two satellites between Uranus and the two brightest was made in 1851 (*Monthly Notices R.A.S.*, xi. 248). He declares, however, that it would have been impossible for Sir William Herschel to have seen these two faint bodies; and although Prof. Holden has attempted to identify the two with two of Herschel's quartett, the balance of evidence is certainly to the contrary, and we are bound to conclude that no one has ever seen the four but Herschel himself.

Herschel announced, in 1798 (Phil. Trans., 1798, p. 48), the retrograde movements of the Uranian satellites in the terse paragraph, "I take this opportunity of announcing that the movement of the Georgian satellites is retrograde." The fact that their orbits were inclined about 80° to the ecliptic plane was discovered in 1788.

Herschel also particularly noticed that the light of the two brightest satellites was subject to considerable fluctuations, and in 1815 (Phil. Trans., 1815, p. 356) he suggests for a cause that given by Newton in the "Principia" to account for the periodical variability of certain stars. His conclusion was:—"The variable brightness of the satellites may be owing to a rotation upon their axes, whereby they alternately present different parts of their surface to our view. These variations may also arise from their having atmospheres that occasionally hide or expose the dark surface of their bodies, as is the case with the sun, Jupiter, and Saturn."

The two inner satellites, Ariel and Umbriel, discovered by Lassell, seem also to fluctuate in brightness, and Newcomb observed in 1875 ("Washington Observations," 1873, p. 43):—"I strongly suspect that Ariel, at least, belongs to that class of satellites of which the brilliancy is variable and dependent on its position in its orbit. The evidence of variability of some kind seems indisputable, as I have repeatedly failed to see it when the circumstances, distance from the planet included, were in every respect favourable, and when Umbriel, though less favourably situated, was visible. On the other hand, there were two occasions, January 28, 1874, and March 25, 1875, when it was surprisingly conspicuous. Unfortunately no systematic record was made of the times when, being near greatest elongation, it was looked for and not seen; but on at least one such occasion its position angle was 180°. An inspection of the observations shows that out of the eight observations only two were made near the southern elongation; while in the two cases where its brightness was most remarkable, the position angles were respectively 348° and 351°."

The time of revolution of Ariel is 2·520378 days at a mean distance of 120,000 miles; its diameter is about 500 miles.

Herschel also observed that these satellites became invisible some distance from the planet's disk (Phil. Trans., 1798, p. 75); thus, on February 22, 1791, the first satellite was lost when 22″ from the planet. This distance was not, however, constant, for on May 2, 1791, the same satellite disappeared at an apparent distance 19″·8. A table is given showing at what distance from the planet the first and second satellites respectively became invisible during a period of seven years. A fact exhibited by this table is that the distance at which the satellites disappeared regularly diminished from 1791 to 1797, until in the latter year the first satellite was traced to 4″·8 from the planet's disk. The reason assigned to account for this phenomenon by Herschel was that the light of the satellites was "put out" by the stronger light of their primary, and regarding this he remarks (Phil. Trans., 1798, p. 78):—"We may avail ourselves of the observations that relate to the distances at which the satellites vanish, to determine their relative brightness. The second satellite generally appears brighter than the first; but as the former is usually lost farther from the planet than the latter we may admit the first satellite to be rather brighter than the second." The diameters of the first and second satellites are about 1000 and 800 miles respectively, hence Herschel's comparative measures were correct.

R. A. GREGORY.

BABYLONIAN ASTRONOMY.[1]

I.

CLASSICAL writers seem to be unanimous in considering the Babylonians as the most ancient astronomers; a close examination shows, however, that the statement emanated from one or two writers, and that all the others merely repeated it without taking the trouble of verifying it. The figures given by various authors as to the period covered by the Babylonian astronomical observations are most extravagant, but the disagreement of the authors proves their inaccuracy. In spite of all discrepancies, one fact comes out clearly—that is, the Semitic origin of the Babylonian astronomy. Belus, the eponymic king of Babylon, considered by the classics as the first ruler or even the colonizer of Babylonia, is called the "inventor of astronomy," and Seneca considered the work of Berosus as a translation of that of Belus.

If we turn now to the native documents—the tablets now in the British Museum—we can class them under two distinct periods, those previous to the Greek rule and those contemporaneous with the Seleucids. Some tablets of the first period give us lists of certain astronomical or atmospheric observations, with the events which took place at the same time; the others are mere reports of the official astronomers stating what they observed, as the occurrence or non-occurrence of an eclipse. A characteristic point is that the observations are in no case dated: the day and the month are, indeed, given, but not the year; and this leaves no doubt as to the real character of these documents.

The Babylonians held the belief that the sky was a reflection of what was going on upon the earth: if, therefore, a certain event took place at the time of the conjunction of two stars, the same event would repeat itself when the same conjunction would take place. There were no predictions, but merely statements of real facts taking place at the same time in the sky and upon the earth, the actual date of which was of no consequence, as the object in view was to establish the supposed connection between what happened in the sky and what happened upon the earth—in short, correlative events. It appears also that the Babylonians admitted the existence of a cosmical year—that is, a period after which the same events were to occur again; this period was one of 360,000 years. The number was obtained by a mere play on figures. The basal number of the Semites was six, as the system used to form their numerals shows; by multiplying it by ten (the number of the fingers) they formed the *soss*, 60; by multiplying again by ten was formed the *ner*, 600; and the square of the former gave 3600, the *sar*, or "multitude." The cosmical year was supposed to be formed of 100 *sari*, or the square of the *ner*; this was probably the number given by Berosus to the antediluvian period, 10 *sari* being attributed to each king. From this number were derived those given by the classics as the period of the Babylonian astronomical observations—720,000 years (or two cosmical years) by Epigenes, 1,440,000 (or four cosmical years) by Simplicius; the 490,000 years given by Berosus, according to Pliny, represent one cosmical year and 130,000 years, elapsed in his opinion, of the actual period. It cannot be doubted, however, that the stars have been observed in Babylonia from a very high antiquity, for we have lists of eclipses for almost every day in the year, and as these eclipses actually took place, they prove a long period of observations; some of the astronomical statements also refer to the pre-Akkadian period—that is, earlier than 7000 B.C. If the Babylonians, in spite of this long period of observations, never arrived at any correct knowledge of the motion of the planets and stars, it is no doubt due to their deficient calendar.

Omen-taking being therefore the only object of the Babylonian astronomers, or rather star-gazers, they distributed the stars and planets under the direction of certain gods, according to the influence attributed to them. This has unfortunately thrown much confusion into their nomenclature, for the name of the god is sometimes taken for that of the star which he is supposed to influence, and the same god influences several; in some cases, also, the same star is sometimes under the influence of one god, sometimes under that of another. Besides this, we have many groups of seven stars: the seven *dibbu* or planets, the seven *masu* or double stars, the seven *zikru* or males, &c. The stars were also divided by regions—the twelve stars of the north, and the twelve stars of the south—and associated in groups of two with certain months; the months themselves were associated with certain regions, and were under the guidance of a god.

In all this we see the rudiments or rather germs of astrology; but, as astrology requires a knowledge of the movements of the planets, the Babylonians never arrived at this point—they merely took omens, and to do so they appear to have proceeded exactly as did the augurs of Rome. They described first in the sky a circle with their rod, divided this circle into eight divisions by lines passing through the centre, and then observed the position of the stars in this imaginary geometrical figure, and what kind of phenomena took place, in order to draw from them their omens. Having made his observation, the operator, or priest, then referred to the lists of omens, copies of which have come to us, to ascertain if the same celestial phenomenon had already been noted, expecting as correlative fact the same terrestrial event which had happened in the previous case.

It may be noticed, before concluding, that all the astronomical omen tablets recovered from Nineveh or Babylon are written in the Semitic language; there are no doubt a great many ideograms, but the phonetic complements, the words spelt phonetically, and the grammatical peculiarities show that the idiom used must be Semitic.

[1] Abstract of the first lecture delivered by Mr. G. Bertin at the British Museum.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

DUBLIN.—At the Summer Commencements of the University of Dublin, held in Trinity College, Dublin, on June 27, Mr. F. W. Burbidge, Curator of the Trinity College Botanical Gardens, received the honorary degree of M.A. The Public Orator, Prof. Palmer, called attention to the benefits conferred on botanical science by Mr. Burbidge, by his travels in Borneo, and by his labours in elucidating the natural history of those classic flowers the Narcissi and the Hellebores. The honorary degree of LL.D. was conferred on Mr. Valentine Ball, F.R.S., the Director of the Science and Art Museum, Dublin, and at one time Professor of Geology and Mineralogy in Trinity College, Dublin, whose works on the geology and mineralogy of India, and researches on the identification of the animals and plants of India which were known to the early Greek authors, merited the eulogium pronounced on them by the Orator.

SOCIETIES AND ACADEMIES.

LONDON.

Linnean Society, June 6.—Mr. Carruthers, F.R.S., President, in the chair.—Dr. John Anderson, Mr. J. G. Baker, Dr. Braithwaite, and Mr. F. Crisp, were nominated Vice-Presidents.—Prof. Martin Duncan exhibited under the microscope some beautifully mounted preparations of the ambulacral tentacles of *Cidaris papillata*, and drew attention to the fact, previously unrecorded, that the tentacles of the abactinal region of the test differ in form and character from those of the actinal region. The latter have a well-developed terminal disk, and are richly spiculated; whereas the former have no disk, but terminate distally in a pointed extremity with very few spiculæ. Mr. W. P. Sladen made some remarks on the significance of this dimorphism with reference to its archaic character, and its relation to the primitive forms of Echinoids and Asteroids.—Mr. Narracott exhibited a singular fasciated growth of *Ranunculus acris*, found at Castlebar Hill, Ealing.—Mr. H. B. Hewetson exhibited under the microscope a parasite of Pallas's Sand Grouse (*Syrrhaptes paradoxus*) taken from a bird shot in Yorkshire, and described as a species of *Argas*. Mr. Harting pointed out that an apparently different parasite from the same species of bird had been recently described by Mr. Pickard Cambridge (*Ann. Mag. Nat. Hist.*, May 1889) under the name *Hæmaphysalis peregrinus*.—Dr. Cogswell showed some examples of Jerusalem Artichoke and Potato, to illustrate the spiral development of the shoots from right to left.—Governor Moloney, of the colony of Lagos, exhibited a large collection of birds and insects from the Gambia, the result of twelve months' collecting in 1884-85. The birds, belonging to 134 species, had been examined and named by Captain Shelley. Amongst the beetles, of which 89 species had been collected, he called attention specially to *Galerita africana* and *Tefflus megelii*, and to the Rhinoceros and Stag-horned Beetles. Of butterflies there were 90 species, amongst which the most noticeable and characteristic were the *Acreas* and the pale-green *Eronia thalassina*, said to be typically Gambian. The moths, of which some 220 species had been brought home, were named by Mr. Herbert Druce, and several had proved to be new or undescribed. A portion of this collection had been exhibited at the Indian and Colonial Exhibition of 1886, but had since been carefully gone over and named, and was now exhibited for the first time in its entirety.—Mr. Herbert Druce alluded to some of the Lepidoptera which are most characteristic of the Gambia region; and Mr. Harting made some remarks upon the birds, pointing out the wide geographical range of some of the species which had been collected.—Mr. Clement Reid exhibited several specimens of fossil plants from a newly-discovered Pleistocene deposit at South Cross, South-elmham, near Harleston.—Mr. D. Morris exhibited specimens of the fruit of *Sideroxylon dulcificum*, the so-called "miraculous berry" of West Africa, belonging to the *Sapotaceæ*. Covered externally with a soft sweet pulp, it imparts to the palate a sensation which renders it possible to partake of sour substances, and even of tartaric acid, lime-juice, and vinegar, and to give them a flavour of absolute sweetness. The fruit of *Thaumatococcus* (*Phrynium Danielli*), possessing similar properties, was also shown; and living plants of both had lately been received at Kew from Lagos through Governor Moloney.—Mr. Thomas Christy exhibited growing plants of *Antiaris toxicaria* (the Upas-tree) and *Strophanthus Kombe*, both of them poisonous, to show the similarity of the foliage.—On behalf of Dr. Buchanan White, a paper was then read by Mr. B. D. Jackson, entitled a "Revision of the British Willows."

Royal Meteorological Society, June 19.—Dr. W. Marcet F.R.S., President, in the chair.—Mr. W. Marriott gave a very graphic and interesting account of the recent thunderstorms which have prevailed over this country. On Sunday, June 2, a thunderstorm passed across the country in a northerly direction from Wiltshire about 5 a.m., and reached Edinburgh by 10.44. It travelled at the rate of about 50 miles an hour. It is possible that this storm travelled still further north, and reached Kirkwall at 3.37 p.m. A severe thunderstorm prevailed over the neighbourhood of the Tweed between 11 a.m. and noon, and was accompanied by hail of very large size, some of the stones being 5 inches in circumference. A very destructive storm occurred over the whole of the north-west of England and south of Scotland during the afternoon; much damage was caused by lightning, and very large hail fell over an extensive area. Some of the hailstones measured 7 inches in circumference and weighed 7 ounces. During the night of the same day a severe thunderstorm prevailed over Norfolk, which was also accompanied by very large hailstones, some of which were 5 to 6 inches in circumference. On Thursday, the 6th, thunderstorms prevailed during the afternoon over the whole of the south-east of England; that which passed over the Metropolis about 9 o'clock was remarkable for the brilliant and continuous display of lightning. During the same night and in the early morning of the following day a very destructive storm prevailed over the Eastern Counties, much damage being done by the lightning in the north-west of Norfolk. Severe hailstorms occurred between 2 and 3 a.m., both at Margate and Ipswich. During the afternoon of the 7th, destructive thunderstorms prevailed over the whole of the Southern Counties, much damage being done by lightning, while at Tunbridge Wells there was a most remarkable hailstorm. One of the hailstones which was weighed was actually half a pound in weight. An interesting collection of over forty photographs of lightning taken during the storm on June 6 was also exhibited to the meeting. In addition to the sinuous, ribbon, and meandering flashes of lightning, several photographs showed knotted, multiple, and dark flashes.—The following papers were also read:—The climate of British North Borneo, by Mr. R. H. Scott, F.R.S.—On the variation of the temperature of the air in England during the period 1849 to 1888, by Mr. W. Ellis.—Atlantic weather and rapid steamship navigation, by Mr. C. Harding.—Meteorological phenomena observed during 1875-87 in the neighbourhood of Chelmsford, by Mr. Henry Corder.—Rainfall in China, and meteorological observations made at Ichang and South Cape in 1888, by Dr. W. Doberck.

Geological Society, June 5.—Prof. J. W. Judd, F.R.S., Vice-President, in the chair.—The following communications were read:—Observations on some undescribed lacustrine deposits at Saint Cross, Southelmham, in Suffolk, by Charles Candler (communicated by Clement Reid). Some remarks were made on this paper by Mr. Clement Reid, Prof. Prestwich, and Mr. Lydekker.—On certain Chelonian remains from the Wealden and Purbeck, by R. Lydekker. In the first part of the paper the author described a portion of the hind lobe of a Chelonian plastron from the Wealden, which was remarkable as showing a median row of epidermal shields. The name of *Archæochelys valdensis* was proposed for the form so represented. The new generic term *Hylæochelys* was also proposed for the Purbeck Chelonian described by Sir R. Owen as *Pleurosternum latiscutatum*, and was also taken to include some other forms from the Wealden. The second section of the paper treated of the affinities of *Pleurosternum*. It was concluded that *Digerrhum*, Cope (as represented by the so-called *Platemys Bullocki*), is identical with *Pleurosternum*, of which there appears to be only one Purbeck species. Evidence was brought forward to show that in the adult of *Pleurosternum* the pubis had a facet for articulation with the xiphiplastral; and it was proposed to refer this genus, together with *Platychelys* and *Baëna*, to a new section termed "Amphichelydia," which was regarded as allied both to the true Cryptodira and to the Pleurodira.—On the relation of the Westleton Beds or Pebbly Sands of Suffolk to those of Norfolk, and on their extension inland; with some observations on the period of the final elevation and denudation of the Weald and of the Thames Valley, by Prof. Joseph Prestwich, F.R.S. The author in this, the first part of his paper, described the Westleton beds of the East Anglian coast. He commenced with a review of the work of previous writers, especially Messrs. Wood and Harmer, and the members of H.M. Geological Survey, including Messrs. H. B. Woodward, Whitaker, and Clement Reid. In discussing this work, particular attention was paid to the Bure Valley beds, which were considered as a local fossiliferous condition of the Pebbly Sands; but the term is not so applicable to these sands as that of the "Westleton and Mundesley Beds," which the author proposed in 1881. The Westleton beds were carefully described, as seen in coast-sections in East Anglia, proceeding from south to north, and the following classification was adopted:—

The Westleton and Mundesley series (The Mundesley section of it).	1. Laminated clays, sand, and shingle with plant-remains and freshwater shells (the Arctic forest-bed of Reid).
	2. Sand and quartzose shingle with marine shells (the *Leda myalis* bed of King and Reid).
	3. Carbonaceous clay and sands with flint-gravel and pebbles of clay, driftwood, land and lacustrine shells and seeds (the Upper freshwater bed of Reid).
The Forest-bed series of Reid (exclusive of No. 3 of above).	4. A greenish clay, sandy and laminated in places, containing abundant mammalian remains, and driftwood, with stumps of trees standing on its surface (the forest- and elephant-bed of authors; the estuarine division, in part, of Reid).
	5. Ferruginous clay, peat, and freshwater remains and gravel (the Lower freshwater bed of Reid).

The Westleton beds were found to rest with discordance on various underlying beds; in places on the Forest series, elsewhere on the Chillesford Clay, whilst occasionally the latter had been partly or entirely eroded before the deposition of the Westleton beds. In the north, where the present series dies out, they come in contact with the so-called Weybourn Crag, which the author supposed to be the equivalent of the Norwich Crag. A similar discordance has been noted between the Westleton beds and the overlying glacial beds, so that the former mark a distinct period, characterized by a definite fauna, and by particular physical conditions. The Westleton beds being marine, and the Mundesley beds estuarine and freshwater, the author proposed to use the double term to indicate the two facies, as has been done in the case of other deposits. But these facies were found to be local, and the most persistent feature of the beds is the presence of a shingle of precisely the same character over a very wide area. By means of this the Westleton beds can be identified far beyond East Anglia, and where there is no fossil evidence, and they throw a considerable light on important physiographical changes. The author described the composition of the shingle, which, unlike the glacial deposits, contained pebbles of southern origin. The paper concluded with a list of fossils, excluding those of the Forest-bed (the stumps of which, the author considered, were frequently in the position of growth). Should the Forest-bed eventually prove to be newer than the Chillesford beds, it was maintained that the former must be included in the Westleton series, and its flora and fauna added to the list, whilst if, on the contrary, the Forest-bed should be proved synchronous with the Chillesford beds it must be relegated to the Crag. The second part of this paper will treat of the extension of these beds into and beyond the Thames Valley, and on some points connected with the physical history of the Weald. The reading of this paper was followed by a discussion, in which Mr. B. H. Woodward, Mr. J. A. Brown, and Mr. Topley took part.

June 19.—Prof. J. W. Judd, F.R.S., Vice-President, in the chair.—The following communications were read:—On tachylyte from Victoria Park, Whiteinch, near Glasgow, by Frank Rutley.—The descent of *Sonninia* and of *Hammatoceras*, by S. S. Buckman.—Notes on the Bagshot Beds and their stratigraphy, by H. G. Lyons.—Description of some new species of Carboniferous Gasteropoda, by Miss Jane Donald; communicated by J. G. Goodchild.—*Cystechinus crassus*, a new species from the Radiolarian marls of Barbados, and the evidence it affords as to the age and origin of those deposits, by J. W. Gregory.—The next meeting of the Society will be held on Wednesday, November 6.

Zoological Society, June 18.—Prof. Flower, F.R.S., President, in the chair.—The Secretary exhibited (on behalf of Mr. J. F. Green) a very fine example of the Common Eel, obtained from a pond in Kent, and measuring upwards of 4 feet in length.—Mr. B. B. Woodward exhibited, and made remarks on, a drawing representing a living example of *Ærope kaffra*, a carnivorous snail from the Cape Colony.—Mr. Woodward also exhibited an example of a fossil shell from the Eocene of the Paris Basin (*Neritina schmideliana*), and a section of it showing the peculiar mode of its growth.—Mr. Eadweard Muybridge, of the University, Pennsylvania, exhibited a series of projections by the oxyhydrogen light, illustrative of the consecutive phases of movements by various quadrupeds while walking, trotting, galloping, &c., and of birds while flying.—A communication was read from Prof. Henry H. Giglioli, containing the description of a supposed new genus and species of Pelagic Gadoids from the Mediterranean, proposed to be called *Eretmophorus kleinenbergi*.—Lieut.-Colonel H. H. Godwin-Austen, F.R.S., read the first of a proposed series of papers descriptive of the land-shells collected in Borneo by Mr. A. Everett, with the descriptions of new species. The present paper treated of the *Cyclostomaceæ*.—Captain G. E. Shelley read a list of birds collected by Mr. H. G. V. Hunter in Masai Land during the months of June, July, and August 1888. The collection (which Mr. Hunter had presented to the British Museum) consisted of examples of ninety-four species, seven of which were described by the author as new to science.—Mr. P. L. Sclater, F.R.S., gave a further description of Hunter's Antelope (*Damalis hunteri*) from specimens obtained by Mr. H. C. V. Hunter on the River Tana, Eastern Africa.—Mr. F. E. Beddard read a paper on the fresh-water and terrestrial Annelids of New Zealand, with preliminary descriptions of new species.—A communication was read from Mr. H. W. Bates, F.R.S., containing descriptions of some new genera and species of Coleopterous insects collected by Mr. Whitehead during his recent visit to Kina Balu. The collection was stated to comprise an unusual proportion of new and remarkable forms.—This meeting closed the session. The next session (1889–90) will begin in November 1889.

Victoria Institute, July 1.—Annual Meeting.—Sir George Stokes, Bart., P.R.S., President, in the chair.—After the reading and adoption of the Report, an address by Prof. Sayce was read by Dr. Wright. It gave a description of what has become known as to the conquests of Amenophis III., the palace and its archives, which have only lately been discovered, and which Prof. Sayce went last winter to investigate on the spot. Of the tablets and inscriptions, he said:—"From them we learn that in the fifteenth century before our era—a century before the Exodus—active literary intercourse was going on throughout the civilized world of Western Asia, between Babylon and Egypt and the smaller States of Palestine, of Syria, of Mesopotamia, and even of Eastern Kappadokia. And this intercourse was carried on by means of the Babylonian language, and the complicated Babylonian script. This implies that, all over the civilized East, there were libraries and schools where the Babylonian language and literature were taught and learned. Babylonian, in fact, was as much the language of diplomacy and cultivated society as French has been in modern times, with the difference that, whereas it does not take long to learn to read French, the cuneiform syllabary required years of hard labour and attention before it could be acquired." A vote of thanks was passed to Prof. Sayce for his address, to Dr. Wright for reading it, and to the President.

Paris.

Academy of Sciences, June 24.—M. Des Cloizeaux, President, in the chair.—On the condition of matter near the critical point, by MM. L. Cailletet and E. Colardeau. The series of experiments here described, and carried out for the purpose of testing the views of Cagniard de Latour, Andrews, Ramsay, and other physicists, tend to show that the critical temperature of a liquefied gas is not that at which the fluid is totally evaporated abruptly within the space containing it, for the liquid state persists beyond this temperature; nor is it the temperature at which a fluid and its saturated vapour have the same density; but it is the temperature at which a fluid and the gaseous atmosphere above it become capable of being mutually dissolved in any proportion, so as to form, when shaken, a homogeneous mixture. This interpretation of the critical point supplies some interesting data on the unbroken continuity of the liquid and gaseous states of matter.—On the heat of formation of the hyponitrites, by M. Berthelot. M. Maquenne's experiments on the hyponitrites, here communicated to the Academy by M. Berthelot, seem to decide the question of the formula of hyponitrous acid, which corresponds with the percentage composition suggested by Divers, but with twice the molecular weight. The complete analysis of the salts of calcium and strontium removes all further doubt on this point.—Restoration of the skeleton of Dinoceras, by M. Albert Gaudry. The specimen here described is that of Marsh's *D. mirabile*, copies of which have been supplied both to the British Museum and to the new gallery of palæontology in the Paris Museum.—On the mastodons found at Tournan, Gers, by M. Albert Gaudry. The numerous remains of mastodons recently found by M. Marty in the Middle Miocene of Tournan, all belong to *M. angustidens*. Amongst them is the most perfect head of any mastodon yet brought to light in Europe. This, with some other important parts, has been secured for the Paris Museum, and affords facilities for determining the specific differences between *M. angustidens* of the Miocene and the *M. americanus* of the Quaternary epoch.—On the occlusion of gases in the electrolysis of the sulphate of copper, by M. A. Soret. Having already shown (*Comptes rendus*, November 5, 1888) that certain relations exist between the quantities of gas occluded in electrolytic copper and the conditions of temperature and acidity of the electrolyte, the author has carried out some further experiments leading to more accurate results, and throwing some light on the action of the electrolysis in this particular case. He finds that the electrolytic copper contains carbon dioxide and hydrogen, the latter of which is in most cases present in largest proportion (five-sixths may be taken as an average), and in all cases when the electrolyte is acid; further, that the brittleness of the deposit of copper is related to the presence of carbon dioxide.—On the compounds of ruthenium and ammonia, by M. A. Joly. The author has already shown that the constitution of the red chlorides of ruthenium was more complex than was supposed by Claus. It results from his further researches that the chloride and all bodies derived from it contain an atom of nitrogen more than was indicated by Claus, and that their formula might be written thus: $Ru . NO . OH . X_2(4NH_3)$.—On the presence of sulphate of soda in the atmosphere, and on the origin of saline dust, by M. P. Marguerite-Delacharlonny. Some facts are here adduced which may give a more general and perhaps a more correct explanation of the presence of the sulphate of soda in the atmosphere than that of M. Parmentier.—Camphor and borneol of rosemary, by M. A. Haller. A new method is described for separating these substances.—Mean altitude of the continents and mean depth of the oceans, by M. A. de Tillo. From the author's minute researches it results that the numerical data hitherto accepted by various authorities require to be modified. He finds the mean elevation of all the continents above sea-level to be 693 metres: northern hemisphere, 713; southern hemisphere, 634; Europe, 317; Asia, 957; Africa, 612; North America, 622; South America, 617; Australia, 240. Mean depths of all the oceans, 3803: Pacific, 4380; Atlantic, 4022; Indian, 3674; northern seas, 3627; southern seas, 3927.

BERLIN.

Physical Society, June 7.—Prof. von Helmholtz, President, in the chair.—Dr. R. von Helmholtz communicated the results of his experiments on the radiating power of flames. The problem which he had set before himself was to determine the relationship between the radiant energy of flames, and the amount of gas consumed for their production. The latter was measured by the fall of the gasometer-globe which contained the gas, the former by means of a bolometer, for each of whose scale-divisions the equivalent value in heat-units had been carefully determined by three different methods. The radiating energy of the flames depended upon a number of conditions which were each severally investigated; as, for instance, the size and shape of the flames, the amount of foreign gases introduced, and the ratio of the amount of oxygen to the amount of gases with which it was mixed. For the purposes of comparative measurements, a moderately high flame was chosen, which produced no smoke and was 6 mm. thick. Luminous flames radiated more energy than non-luminous, and it was proved by an extended series of careful quantitative experiments that the radiating power of the flames was not dependent upon their temperature. From this it follows that Kirchoff's law does not hold good for flames—a result which is, however, quite in accordance with the limitations he put to his law for those cases in which heat is directly converted into radiating energy. In the case of flames it must be borne in mind that chemical affinity comes additionally into play: the speaker entered fully into the influence of this upon the radiation of energy, and endeavoured to make it clear by means of an extremely interesting hypothesis. After this he stated the numerical data which he had obtained for both luminous and non-luminous flames, produced with a series of gases—hydrogen, carbonicoxide, methane, coal-gas, methyl-alcohol, &c. Starting as a basis with Julius's statement that the products of combustion are the only criteria of the amount of radiation, and hence calculating the radiating energy of the flames, he obtained values which corresponded very closely in most cases with those actually observed. Finally he calculated the total useful effect which can be obtained as radiant energy from the gases which are being consumed in the production of the flame. From this he arrived at the interesting result that it is far more economical to use the gases for driving a dynamo which supplies incandescent lamps, and to utilize the energy radiated from the latter, than to burn the gases, and utilize the energy which is radiated out from their non-luminous flames. The communication, as a whole, of which only a short sketch has here been given, contained a large number of very valuable quantitative results.—Dr. Budde spoke on "tautological" contacts in mechanics, and deduced the general conditions under which a close determination of contacts between surfaces and points is unnecessary ("tautological").

Physiological Society, June 14.—Prof. Munk, President, in the chair.—Dr. Openchowski spoke on the researches which he has carried on since 1883 on the movement and innervation of the stomach. The movements were recorded by introducing into the stomach a small bag filled with water and connected with a manometer, the motion of the column of fluid in the latter being recorded graphically. The fundus and first third of the stomach never exhibit any spontaneous peristaltic movements, these being confined to the second and last third, including the pylorus. The centre for the initiation of the motor movements lies in the ganglion cells under the serous coat which extend all over the stomach along the branches of the vagus nerve. The motor and inhibitory centres on which the movements of the cardiac end depend are situated in the brain and spinal cord; the motor centres lie in the corpora quadrigemina and in the spinal cord between the fifth and eighth thoracic vertebræ; the inhibitory centres lie in the corpus striatum, and a few are scattered in the spinal cord. The connection between the cranial centres and the stomach is provided by the vagi; there is no connection between the cranial and spinal centres. The centres for the pyloric end are situated in the same places as those for the cardiac end, but the centres which are inhibitory for the latter are motor for the former, and *vice versâ*. The speaker has studied the act of vomiting very fully. The stomach plays an active part in this act. After paralysis of the stomach, the movements of vomiting may be brought about, but do not lead to an ejection of the stomach's contents. After administering an emetic, such as sulphate of copper, the fundus and first third of the stomach is actively dilated; the pylorus is at the same time relaxed, and the contents of the small intestine are driven into the stomach by the contraction of the intestinal walls, and then, by the strong and progressive contractions of the last two-thirds of the stomach, they are driven on into the first third of the stomach. At this stage the stomach has a pear-like shape, the fundus being unduly distended. The reflex movements of vomiting now occur, and the pressure exerted by the abdominal muscles leads to the emptying of the highly distended fundus. As after paralysis of the stomach, so also after section of the vagi and excision of the stomach and intestines, the movements of vomiting occur when an emetic is administered. No *single* centre for vomiting appears to exist and be actively functional during the act, but there would seem rather to be a combination of co-ordinated centres, whose position has still to be more definitely ascertained.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

U.S. Commission of Fish and Fisheries, 4 vols.: G. Brown Goode.—Inorganic Chemistry: Ira Remsen (Macmillan).—Eclipses and Transits in Future Years: S. J. Johnson (Parker).—Fundamental Problems: Paul Carus (Open Court Publishing Company).—Rendiconti del Circulo Matematico (Palermo).—Avifauna Italica, parte prima: E. H. Giglioli (Monnier, Florence).—Injurious Farm and Fruit Insects of South Africa: E. A. Ormerod (Simpkin).—Gaseous Fuel: B. H. Thwaite (Whittaker).—Electricity: A. Rust (Spon).—American Resorts: B. W. James (Davis).—The Prospector's Hand-book, 4th edition: J. W. Anderson (Lockwood).—*Phormium tenax* as a Fibrous Plant, 2nd edition: edited by Sir J. Hector (Wellington, New Zealand).—Geology in Systematic Notes and Tables, 2nd edition: W. F. Gwinnell (Allman).—Kant's Critical Philosophy, vol ii. The Prolegomena; translated: J. P. Mahaffy and J. H. Bernard (Macmillan).—Physics of the Earth's Crust, 2nd edition: Rev. O. Fisher (Macmillan).—Schriften der Naturforschenden Gesellschaft in Danzig: Neue Folge, Siebenten Bandes, Zweites Heft (Danzig).—Observations made at the Blue Hill Meteorological Observatory, Mass., U.S.A., 1887: A. L. Rotch (Camb., Mass.).—Transactions and Proceedings of the New Zealand Institute, vol. xxi. (Trübner).

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