THURSDAY, AUGUST 7, 1890.

THE HISTORY OF BOTANY.

History of Botany (1530–1860.) By Julius von Sachs. Authorized Translation by H. E. F. Garnsey, M.A. Revised by Prof. I. Bayley Balfour, F.R.S. (Oxford: Clarendon Press, 1890.)

AFTER fifteen years' interval, this admirable book has made its appearance in English. The translation does justice to the original, and to say this is very high praise, for the "History of Botany" is perhaps the most generally interesting, and the most finished in style, of all Prof. Sachs's works.

There have been scarcely any alterations in this edition, which still represents the state of the author's mind in 1875. To quote his words, in his preface to the present translation:—

> "I came to the conclusion that my book itself may be regarded as a historical fact, and that the kindly and indulgent reader may even be glad to know what one, who has lived wholly in the science, and taken an interest in everything in it, old and new, thought from fifteen to eighteen years ago of the then reigning theories, representing as he did the view of the majority of his fellow botanists."

The paragraph which follows must, we think, in fairness be quoted, though this is done with some regret:—

> "However, these remarks relate only to two famous writers on the subjects with which this history is concerned. If the work had been brought to a close with the year 1850 instead of 1860, I should hardly have found it necessary to give them so prominent a position in it. Their names are Charles Darwin and Karl Nägeli. I would desire that whoever reads what I have written on Charles Darwin in the present work should consider that it contains a large infusion of youthful enthusiasm still remaining from the year 1859, when the 'Origin of Species' delivered us from the unlucky dogma of constancy. Darwin's later writings have not inspired me with the like feeling. So it has been with regard to Nägeli. He, like Hugo von Mohl, was one of the first among German botanists who introduced into the study that strict method of thought which had long prevailed in physics, chemistry, and astronomy; but the researches of the last ten or twelve years have unfortunately shown that Nägeli's method has been applied to facts which, as facts, were inaccurately observed. Darwin collected innumerable facts from the literature in support of an idea; Nägeli applied his strict logic to observations which were in part untrustworthy. The services which each of these men rendered to the science are still acknowledged; but my estimate of their importance for its advance would differ materially at the present moment from that contained in my 'History of Botany.' At the same time, I rejoice in being able to say that I may sometimes have overrated the merits of distinguished men, but have never knowingly underestimated them."

We are sorry that these words have been written. The position of Darwin in biology needs no defence, even when the assailant is Prof. Sachs. With regard to Nägeli, the case is different: but, although recent investigation has re-opened some of the questions which he appeared to have decided, we feel that here also the critic's first thoughts were best, and that the estimate of 1875 is, in the main, more just as well as more generous than that of 1889.

A very brief sketch of the contents of the work, which will already be familiar to so many botanical readers, must suffice. The first of the three books into which the whole work is divided is occupied with the history of morphology and classification, from 1530 to 1860. The early efforts at classification by the German and Dutch botanists of the sixteenth century are first discussed, and it is shown that they were already guided by the perception of natural affinity—an idea which, as the author says, "is not the discovery of any single botanist, but is a product, and to some extent an incidental product, of the practice of describing plants." But for a time these necessarily feeble attempts at a natural arrangement had to give way to artificial systems based on *a priori* principles of classification. Of this tendency Cesalpino is the first great representative, and the author shows how great was the influence of this remarkable man on the succeeding period of systematic botany. It was Cesalpino who first founded a classification mainly on the organs of fructification.

The period inaugurated by Cesalpino culminates in Linnæus.

> "Linnæus," says Prof. Sachs, "in whose works the profound impression which he had received from Cesalpino is everywhere to be traced, retained all that was important in his predecessor's views, but perceived at the same time what no one before him had perceived, that the method pursued by Cesalpino, Morison, Ray, Tournefort, and Bachmann, could never do justice to those natural affinities which it was their object to discover; and that in this way only an artificial though very serviceable arrangement could be attained, while the exhibition of natural affinities must be sought by other means" (p. 81).

The author does full justice to the unrivalled excellence of Linnæus as a descriptive botanist, and further points out that his fragment of a natural system was much the most truly natural proposed up to the middle of the eighteenth century. Linnæus's famous sentence, "It is not the characters which make the genus, but the genus which makes the characters," shows, indeed, a remarkable insight into the meaning of natural affinity.

The development of the natural system by the two Jussieus, Pyrame de Candolle, Robert Brown, and other illustrious systematists is next traced. In the concluding chapter of the first book there is a fine sketch of the splendid work of Hofmeister in establishing the relations between Cryptogams and Phanerogams, and of his position relative to the theory of descent. The author says (p. 202):—

> "When Darwin's theory was given to the world eight years after Hofmeister's investigations, the relations of affinity between the great divisions of the vegetable kingdom were so well established and so patent, that the theory of descent had only to accept what genetic morphology had actually brought to view."

The subject of the second book is the history of vegetable anatomy. An admirable account is given of the work of the great founders of the anatomy of plants in the seventeenth century, Malpighi and Grew, who remained the leading authorities in this branch of science for 130 years. In speaking of the period of barrenness which followed this brilliant beginning, the author is especially severe on our own country. "In England," he says (p. 246), "the new light was extinguished with Hooke and Grew, and has so remained, we may almost say, to the present day." We may hope that, if this passage had been written fifteen years later, Prof. Sachs would have found some reason to modify his judgment.

The following chapters deal with the revival of vegetable anatomy and histology in the present century. Due justice is done alike to the patient investigations of von Mohl and to the brilliant method of the erratic Schleiden, while, as will be gathered from what has been said above, the many-sided activity of Nägeli receives in the text fully adequate recognition.

The third book is on the history of vegetable physiology, and this is of special interest from the fact that the author is himself the leading physiological botanist of our time. The first chapter is concerned with the history of the sexual theory. The chief credit for the discovery of sexuality in plants is given to Camerarius of Tübingen, who in 1694 published the first experimental researches on the necessity of fertilization for the ripening of the seed; though in special cases, as those of the fig and the date-palm, the fact of sexuality had been known even to Theophrastus and Pliny. The author justly points out that Linnæus, though his system called general attention to the existence of male and female organs in the flower, had little or nothing to do with the discovery of their functions.

The following passage discusses the relation of Kaspar Friedrich Wolff to the old theory of "evolution," according to which all the parts of the mature organism pre-exist in little in the embryo.

> "Wolff conceived of the act of fertilization as simply another form of nutrition. Relying on the observation, which is only partly true, that starved plants are the first to bloom, he regards the formation of flowers generally as the expression of feeble nutrition (*vegetatio languescens*). On the other hand, the formation of fruit in the flower was due to the fact that the pistil found more perfect nourishment in the pollen. In this, Wolff was going back to an idea which had received some support from Aristotle, and is the most barren that can be imagined, for it appears to be utterly incapable of giving any explanation of the phenomena connected with sexuality, and especially of accounting for the results of hybridization. Wolff may have rejected the theory of evolution on such grounds as these, but he failed to perceive what it is which is essential and peculiar in the sexual act" (p. 405).

This passage appears of special importance, for theories akin to those of Wolff have reappeared even in our own day.

The investigations of Koelreuter on hybridization, and those of Sprengel on cross-fertilization, the full significance of which was first shown by Darwin 60 or 70 years later, mark the closing years of the eighteenth century. But, in spite of all that had been done, there were still some botanists who, on more or less feeble grounds, expressed doubts as to the sexuality even of the Phanerogams, and it was the work of Gärtner, towards the middle of the present century, which "once more confirmed the existence of sexuality in plants, and in such a manner that it could never again be disputed."

The concluding sections of this chapter give the remarkable history of the discovery of the details of fertilization in the flowering plants, and sketch the rise and progress of our knowledge of corresponding processes among the Cryptogams. These are subjects on which an immense amount of good work has been done in more recent years, and some future historian will have much to add to Prof. Sachs's brilliant summary.

The nutrition of plants forms the subject of the next chapter of the "History." The ideas of the ancients are first considered, and then the gradual rise of the modern doctrine of assimilation is traced from its first beginnings in the discoveries of Malpighi and Hales, of whom the former showed that the green leaves are the organs which prepare the food, while Hales proved that a large part of this food is taken up in a gaseous form. It would be useless to attempt to summarize this interesting story. Probably no piece of scientific history has ever been better told, and few, if any, are better worth the telling. Prof. Sachs is here, above all, on his own ground, and we are conscious that we are reading the words of a great master. It is scarcely necessary to add that here, also, more recent research has been extremely active, and modern investigations on such questions as the source of the nitrogen in plants, and the course of the ascending sap, will probably do much to modify the views expressed in this work.

The concluding chapter is on the movements of plants, and here, once more, the historian is treating of phenomena of which he is himself among the greatest investigators.

The translators and the Clarendon Press deserve the warmest thanks of English readers, whether botanical or not, for bringing before them a scientific history distinguished at once by its clearness, its fairness, and the author's unrivalled mastery of his subject.

D. H. S.

A TEXT-BOOK OF PHYSIOLOGICAL AND PATHOLOGICAL CHEMISTRY.

Text-book of Physiological and Pathological Chemistry, in Twenty-one Lectures for Physicians and Students. By Dr. G. Bunge, Professor of Physiological Chemistry at Bâle. Translated from the second German edition by the late L. C. Wooldridge, M.D., and completed for the press by his Wife. (London: Kegan Paul, 1890.)

THE appearance of Bunge's text-book in its English dress reminds us keenly of the loss which physiology has sustained by the death of the translator. It is some consolation to be able to temper this regret with the satisfaction that so interesting and instructive a work was made available to English students by one so capable as the late Dr. Wooldridge. He wisely contented himself with translating the original without those annotations or additions which are often supplied, and which, while they may be of intrinsic merit, frequently destroy the individuality of the original. Criticism of Dr. Wooldridge's share in the English version thus resolves itself into asking how he has done his work as a translator, and the answer is: "Admirably." While the original text is closely followed and accurately rendered, the result is, unlike some translations, such pleasant reading that the student will scarcely realize that it is a translation. But in justice to the author it must be said that this is also partly due to the simple style and language of the original, and to the lecture-form of its arrangement.

The aim of the author has been to deal with such portions of the subject as are "ripe for a connected account," omitting "all disconnected facts and mere descriptive matter" and all descriptions of analytical methods; to provide such references to the literature of the subject as shall more particularly suffice to put the student on the track of the remainder; and thus as a whole to tell the reader what is most certainly known, and to enable him to pursue further any points in which he is specially interested. In all this the author has been very successful, and particularly with respect to the references to original memoirs, which are quoted judiciously and comprehensively. The work is divided into twenty-one lectures. Of these the first propounds the author's views as to the "aims and prospects of modern physiological research," and consists of a somewhat remarkable protest against the modern tendency to regard cell-activity as the expression and outcome of chemical, physical, and mechanical processes. It is indeed a distinct return to the vitalistic views of the past, and urges the existence of some psychological factor of activity, based on the belief that "for the moment it is not apparent how any further progress of importance can be made with the help of chemistry, physics, and anatomy only;" and concludes by saying that "what these sciences fail to achieve will stand out more prominently, and thus the mechanical theories of the present will assuredly carry us eventually to the vitalism of the future." There are probably few physiologists who will agree with this view. Most will rather hold with Heidenhain (*Pflüger's Archiv*, xliii., Suppl.-Hft. p. 63) that, granted the existence of the psychological factor, still it must produce its recognizable effects by purely chemical, physical, and mechanical means, and accept those views of the "activity" of a cell which stand out so clearly in his masterly work on secretion.

The second and third lectures treat of the chemical elements which constitute living organisms, their circulation through the vegetable and animal kingdoms, the principle of the conservation of energy as applied to living things, and, finally, the correlation of plants and animals. The next three lectures deal with the organic food-stuffs and foods, their composition, importance, and function, in connection with nutrition. In these a clear and comprehensive account is given of the various endeavours which have been made to determine the molecular weight of proteids. The sections on the *rôle* of gelatin and cellulose, and of a vegetarian diet in general, are most instructive, and there is a very full statement of the physiology of the organic compounds of iron, leading up to the author's views as to the mode of action of iron-salts in the treatment of chlorosis. Lecture VII., on the inorganic food-stuffs, contains an interesting and valuable account of various salts, more particularly those of sodium and potassium, in their relationship to nutrition; and Lecture VIII. concludes this part of the subject by treating of subsidiary articles of diet, such as tea, coffee, alcohol, bouillon, &c. Digestion and the absorption of digestive products form the subject-matter of Lectures IX.-XII. In these the well-selected and copious references to the literature of the subject will be found to be by no means the least valuable part. Lecture XIII., on the chemistry of blood and lymph, will probably disappoint those who turn to it for an account of the clotting of blood. Perhaps Prof. Bunge thinks the subject not yet "ripe for a connected account," and this is, perhaps, to a large extent true. Still, he would have done well to treat it from a general point of view, rather than almost entirely with regard to the part played by the leucocytes: some account at least of the work of Hammarsten and the translator seems called for in connection with this part of the subject. The gases of the blood, and their relation to the processes of external and internal respiration, are dealt with in the next two lectures. These call for no special remark apart from saying that the fact that the oxidations of the body take place in the tissues might have been more decisively brought out. Existing views as to the condition of CO_2 in the blood are clearly stated. Lecture XVI. gives an admirable exposition of recent work and existing views as to the seat and mode of formation of the nitrogenous products of metabolism, followed, in natural sequence, by a chapter on the functions of the kidneys and chemistry of urine. Hepatic metabolism is the subject of Lecture XVIII. In this the questions which arise with regard to its glycogenic activity are scarcely so clearly put forward as might be expected. On the other hand, the older and current views on fat-formation are well explained in Lecture XX. The remaining lectures (XIX. and XXI.) deal with the source of muscular energy and diabetes respectively.

It is well for those English students who cannot read the original that this interesting and instructive work by Prof. Bunge has, in this well-turned version, been made accessible to them. We cannot conclude better than by hoping it may attain the recognition and approval in this country which it so fully deserves from every point of view, and which it appears to have already secured in the original, judging by the speedy issue of the second edition, of which the copy here reviewed is a translation.

THE ADVANCEMENT OF SCIENCE.

The Advancement of Science: Occasional Essays and Addresses. By E. Ray Lankester, M.A., LL.D., F.R.S. (London: Macmillan and Co., 1890.)

UNDER this title, Prof. Ray Lankester has republished a number of essays, which have appeared at intervals during the last eighteen years. All of them are of more or less permanent interest, and we are glad to have them presented to us in the convenient form of a well-printed octavo volume. While some of the essays are somewhat too technical for the general reader, the majority are of great and very general interest, well worthy of being read and thought over by all.

These essays are nine in number. The last treats of the history and scope of zoology, and is reprinted from the last edition of the "Encyclopædia Britannica"; it forms a most excellent treatise on the subject, and fairly though briefly sketches the history of zoology from the seventeenth century to the present day. In the second essay the relations that should exist between the State and biology are considered, and there can be little doubt but that as a result of this address to the Biological Section of the British Association at Southport, followed by the fifth essay, which gives an outline of the scientific results of the International Fisheries Exhibition, held in London in the same year (1883), we are in great measure indebted for the valuable help given by our Government towards the establishment of the Laboratory at Plymouth belonging to the Marine Biological Association of the United Kingdom.

The third and sixth essays, on Pasteur and hydrophobia—or rabies, as we would prefer to call this formidable disease—and on centenarism, are full of interest, and while in the former the author has to content himself with a narration of the chief results of Pasteur's invaluable labours, in the latter we find an account of a subject which has been critically worked out by himself.

Three of the essays relate to the subject of Darwinism, and possibly will be found the most interesting in the volume. The first is on the subject of "Degeneration, a chapter in Darwinism," and was delivered as one of the evening lectures at the British Association meeting at Sheffield, in 1879. In it Prof. Lankester calls attention to the fact that degeneration, or the simplification of the general structure of an animal, may be due to the ancestors of that animal having taken to one of two habits of life, either the parasitic or the immobile. Other new habits of life appear also to be such as to lead to degeneration. Let us suppose, for example, a race of animals fitted and accustomed to catch their food, and having a variety of organs to help them in this chase; suppose such animals suddenly to acquire the power of feeding on the carbonic acid dissolved in the water around them, just as green plants have. This would lead to degeneration; for they would soon cease to hunt their food, and would bask in the sunlight, taking food in by the whole surface, as plants do by their leaves. Another possible cause of degeneration appears to be the indirect one of minute size. And so, as is well shown, this hypothesis of degeneration enables very numerous cases of animal structure to be accounted for. The second of this set, forming the seventh of the collected series, is on parthenogenesis, and in it we find the fascinating accounts given to us by the painstaking zeal of von Siebold of the habits and manners of the little wasps belonging to the genus Polistes—a story both wonderful and romantic. The third of these, the eighth of the whole set, treats of Haeckel's theory of heredity, in which the transmission of acquired characters by heredity is discussed, but this phase of belief Prof. Lankester will no longer insist upon, and he points out that Weismann's essays on this question should be carefully studied by naturalists.

The last essay to be alluded to is the fourth, on examinations. The author claims that but few have had a wider or a more continuous experience in examinations than he has had. On this somewhat vexed question he has a good deal that is to the point to say, showing that the use of examinations in schools and Universities is different from their use as a test of fitness for entrance into a profession, or a post in the Home or Indian Civil Service, or as a means of deciding a question of relative merit.

We feel sure that as each of these essays originated in a desire to promote the interests of science, so the author, in collecting the present series, will be found to have had the same aim in view.

OUR BOOK SHELF.

Agenda du Chimiste. Par MM. Salet, Girard, et Pabst. (Paris: Hachette and Co., 1890.)

IN this volume will be found a most complete and exhaustive compilation of facts and numerical tables of use to the chemist. The first edition was published in 1877 by M. Wurtz, and in subsequent editions the work has been thoroughly brought up to date. It is now published annually as a chemical year-book, the publication of each year containing a few special articles called for by the events of the past twelve months. This year the following are among the special articles contributed: "The Progress of the Industry of Colouring Matters," "Review of the Exhibition of 1889" as regards matters of chemical interest, and "Views of the International Chemical Congress concerning Nomenclature." The numerical data included in the book are most full, and ought to be of great service in the reduction of observations. The collection of them represents an immense amount of labour, and the accompanying descriptions of experimental methods are very clear and concise. A most useful portion of the work is that in which all the known physical constants of the elements and numerous compounds are given. Special care appears to have been taken in collecting the published thermo-chemical data, with the result that the chapter upon this subject is one of the most valuable in the book. The tables for use in quantitative analysis, and especially those referring to commercial methods, will doubtless be fully appreciated for the saving of time and arithmetical labour which their use will effect. It is, moreover, of no mean advantage that all formulæ are given according to the ordinary nomenclature, and not according to the old notation still retained by many French chemists. The volume is small and handy in spite of its five hundred pages, and cannot fail to be of service in the laboratory. A. E. T.

The Philosophy of Clothing. By W. Mattieu Williams. (London: Thomas Laurie, 1890.)

MR. WILLIAMS is a somewhat eccentric writer, and by most people some of the notions set forth in this little book will be regarded as "fads." He is generally able, however, to give a good reason for the opinions he advances, and much of his advice, although opposed to the rules of fashion, is sound and practicable. The subject is one which occupied the close attention of Count Rumford; and of his researches Mr. Williams, as he himself says, has made "free use."

LETTERS TO THE EDITOR.

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

The Zoological Affinities of *Heliopora cerulea*, Bl.

THE remarkable blue coral, *Heliopora cerulea*, of Blainville, represents, I believe, one of those species that, in common with *Stylaster*, *Millepora*, and other allied genera, have been recently relegated to the Hydrozoic subdivision of the Cœlenterata. So far as I remember, however, and without having present access to the most recent literature of this subject, *Heliopora* was thus transferred with reference to the structure of its corallum only, the living animal having been but imperfectly if at all observed.

In the course of my professional investigations of the fisheries of Torres Straits I have on several occasions obtained specimens of *Heliopora*, but had hitherto been unsuccessful in observing the living animal. Last year I obtained this coral on the Warrior Reef near New Guinea, but while apparently living when collected, and kept for days on board ship with the water continually changed, the zooids refused to make their appearance. Through the courtesy of Captain Dawson, R.N., and the officers of H.M.S. *Rambler*, I have this season journeyed north in that ship, and was afforded the opportunity of conducting a series of investigations in the neighbourhood of the Adolphus Islands, off Cape York, close to the scene of the recent *Quetta* wreck, and with relation to which the *Rambler* had been told off to make a careful survey.

At low spring-tide on the reef adjacent to the "Mid-Brother" rock, I came across a luxuriant growth of *Heliopora*, and was fortunate on this occasion to accurately determine the nature of the fabricators of this remarkable coral. The first living manifestations presented, and those visible only with the aid of a pocket lens, were the protrusion of a transparent body and two elongate tentacles from the numerous circular pores with which the corallum is studded. At first sight some near affinity of the animal to the bitentaculate Hydrozoon *Lar sabellarum* of Gosse was suspected. The movements of the zooids during extension and retraction were, however, more active than those which usually obtain among the Cœlenterata, and together with their general aspect and comportment suggested a nearer relation to the Annelida. This last-named section of the Invertebrata was found on a closer examination to represent their actual position in the zoological scale. On splitting one of the smaller flattened branches of the coral perpendicularly and parallel with its wider axis, I found that the entire coronid system was exposed to view. The little annelid fabricators, having an average length of one-fifteenth of an inch, wriggled into the water in every direction, a large number at the same time remaining passively in the tubular chambers which they originally constructed.

The most prominent external characters of the annelid of *Heliopora cerulea* consist of the bitentaculate head and six pairs of lappet-like branchiæ, which originate in segmental pairs on the dorsal surface and commence about the sixth segment posteriorly from the head. Fine isolated or paired setæ are developed in duplicate on the majority of the residual segments, and two brush-like fasciculi of closely adpressed setæ are conspicuous on the dorsal aspect of the penultimate and antipenultimate caudal segments. On my return to Brisbane a few weeks hence, I purpose preparing and remitting a more detailed account, with illustrations, of the organization of *Heliopora*. In the interim it has occurred to me that this brief announcement of its nature may prove of interest to many of your readers, more especially as it may assist in throwing fuller light on the affinities of the many fossil genera that have hitherto been affiliated with this type among the Cœlenterata, but which in common with *Heliopora* should probably find their true position among the more highly organized section of the Tubicolous Annelida.

W. Saville-Kent,
Commissioner of Fisheries, Queensland.

Thursday Island, Torres Straits, June 18.

Chambers's "Hand-book of Astronomy."

As the writer of the article on "Spectroscopic Astronomy" in the above work, I should like to be permitted to comment upon two points wherein your reviewer has, though doubtless inadvertently, scarcely done me justice.

On p. 292 (Nature of July 24) the reviewer says that I have "*selected* certain determinations and arranged them in parallel columns to demonstrate the efficiency of the method adopted." The reference is to the comparison which I gave of the results obtained by Dr. Huggins, Mr. Seabroke, and at Greenwich, for motions of stars in the line of sight. But I made no selection. I took *all* the stars that had been observed at two or more of these Observatories, and gave the mean of *all* the observations of each star. I might further add that I think your reviewer is scarcely fair in his description of the discordances of my observations: expressed in wave-length, the average difference from the mean is but a small fraction of a tenth metre. But this is an unimportant matter compared with the suggestion that I have published a "selected"—that is, a "cooked"—comparison.

Then your reviewer complains that I make no reference to Prof. Vogel's observations of Algol, whilst I give my own "later division" of my observations into groups. I made no reference to Prof. Vogel's observations, because they were not published until some considerable time after the final revise of my article had been passed for press; whilst, so far from my division of my observations into groups being later than Prof. Vogel's work, it was two full years earlier, having been communicated to the Royal Astronomical Society in January, 1888, by the Astronomer-Royal (see *The Observatory*, vol. xi. p. 109). I also gave my results in one of the Gresham Lectures, Easter, 1888.

E. W. Maunder.

Royal Observatory, Greenwich, S.E., August 1.

I regret that my words allowed the interpretation which Mr. Maunder points out, for I had no intention of insinuating that the comparisons were "cooked." What I take exception to is that, according to the values given, γ Cassiopeiæ has a motion in the line of sight of -12, although on February 19, 1887, Mr. Maunder determined it as $-54{\cdot}2$, and eight minutes afterwards as $+60{\cdot}9$; and again, β Pegasi is stated to have a motion in the line of sight of -8, although in November 1881 two determinations, made within ten minutes of each other, differed by nearly 114 miles per second. It would seem, therefore, that in making a tabular statement, even of the mean of such values found by different observers, the magnitude of the probable error should be mentioned; for, as I remarked at the time, "To one unacquainted with instrumental difficulties, the motion of stars in the line of sight would appear to be a quantity that may be determined with some accuracy," whereas this is not the case. I have no intention of questioning Mr. Maunder's skill as an observer, but the fact that the discordances, when expressed in wave-lengths, are very small, only supports my contention that, until more perfect instrumental conditions are possible, many of the values are useless, and their determination an affectation of accuracy.

Mr. Maunder has himself to blame for my want of information with respect to Algol. He gives no reference to the report of the remarks made by the Astronomer-Royal in January 1888, and his own comments, at the meeting of December 1889, upon Prof. Vogel's work, led me to suppose nothing had been done previously.

The Reviewer.

Gregory's Series.

Gregory's series, on which are founded nearly all the methods of obtaining the approximate value of π, is made to depend, in works on trigonometry, on De Moivre's theorem and results flowing from it.

The following does not require the use of $\sqrt{-1}$, but depends only on two things—that the circular measure of an angle and its tangent are practically equal when the angle is indefinitely small, and that $\tan(A - B) = \dfrac{\tan A - \tan B}{1 + \tan A \,.\, \tan B}$.

Let

$$\tan\theta \equiv \tan(a_0 + a_1x + a_2x^2 + \&c.) = x;$$

$$\therefore \tan\{a_0 + a_1(x+h) + a_2(x+h)^2 + \&c.\} = x + h;$$

$$\therefore \tan h \,.\, \{a_1 + 2a_2x + 3a_3x^2 + \&c. + \text{terms involving } h, \text{ say H}\} = \frac{h}{1 + x(x+h)};$$

$$\therefore \frac{\tan h\{a_1 + 2a_2x + 3a_3x^2 + \&c. + H\}}{h(a_1 + 2a_2x + 3a_3x^2 + \&c. + H)} = \frac{1}{\{1 + x(x+h)\} \,.\, (a_1 + 2a_2x + 3a_3x^2 + \&c. + H)}.$$

Let $h = 0$;

$$\therefore 1 = \frac{1}{(1 + x^2) \,.\, (a_1 + 2a_2x + 3a_3x^2 + \&c.)}.$$

Equating coefficients of like powers of x,

$$a_1 = 1,\ a_2 = 0,\ a_3 = -\tfrac{1}{3},\ \&c.;$$

$$\therefore \theta = a_0 + x - \tfrac{1}{3}x^3 + \&c.,$$

where evidently $a_0 = 0$, or a multiple of π.

Taking $\theta = \dfrac{\pi}{4}$,

$$\frac{\pi}{4} = 1 - \tfrac{1}{3} + \tfrac{1}{5} - \tfrac{1}{7} + \&c.$$

R. Chartres.

The Perseid Meteor Shower.

WITH reference to the letter of Mr. Monck in NATURE of July 24 (p. 296), I would remark that his attempted explanation of the displacement in the Perseid radiant point is altogether futile. If your correspondent were better acquainted with the facts in detail, I think he would readily admit this.

My observations in this branch have been effected in the hope that they might prove useful, and I am sorry to see that Mr. Monck has so thoroughly misapprehended them. The shifting radiant of the Perseids is fully proved, and anyone who will take the trouble to watch the sky at the proper season may readily observe the fact for himself. W. F. DENNING.

Bristol, August 2, 1890.

COMPARISON OF THE SPECTRA OF NEBULÆ AND STARS OF GROUPS I. AND II. WITH THOSE OF COMETS AND AURORÆ.

I.

THE first step towards my present views as to the evolutions of the various groups of cosmical bodies was taken when one day I was attempting to trace the origin of the absorption flutings in stars of Vogel's Class III.*a*. So far, no one had endeavoured to trace their origin, all the work having been confined to the absorption *lines*. It is true that both Dr. Huggins and Vogel, as well as others, had published maps of the spectra of these stars, showing the absorption flutings as well as the lines, but the origins of the former were not inquired into.

It was at once perfectly obvious that among the chief absorption flutings were the most prominent of those seen in the spectrum of manganese at the temperature of the oxy-coal-gas flame—a temperature at which only one *line* is visible, while in the sun all the lines of manganese are visible. In order to investigate this further all the flutings seen when the principal metals were exposed to this temperature were mapped, with a view of determining whether any others besides those of manganese were visible in the stellar spectra. Several others, notably one of lead, were found to be present.

Here, then, was proof positive of low temperature: from solar absorption to the absorption of these stars of Class III.*a* we passed from phenomena which we can reproduce at the temperature of the arc to those visible at the temperature of the oxy-coal-gas flame.

It was next found that identical absorption phenomena are seen in comets long before they reach perihelion. This was a striking result, considering the vast difference in the way in which the phenomena of distant and near meteoric groups are necessarily presented to us; and bearing in mind that in the case of comets, however it may arise, there is an action which drives the vapours produced by impacts outward from the swarm in a direction opposite to that of the sun.

It must be a very small comet which, when examined spectroscopically in the usual manner, does not in consequence of the size of the image on the slit enable us to differentiate between the spectra of the nucleus and envelopes. The spectrum of the latter is usually so obvious, and the importance of observing it so great, that the details of the continuous spectrum of the nucleus, however bright it may be, are almost overlooked.

A moment's consideration, however, will show that if the same comet were so far away that its whole image would be reduced to a point on the slit-plate of the instrument, the differentiation of the spectra would be lost; we should have an integrated spectrum in which the brightest edges of the carbon bands, or some of them, would or would not be seen superposed on a continuous spectrum.

But another revelation still more startling was in store for me, when my assistants and myself had exhausted all the flutings then known to us as origins for the so-called dark bands which remained, and found that none would fit, and we seemed at the end of our tether.

My ten years' work on carbon made itself quite unconsciously felt at this juncture. It suddenly flashed upon me that the 517·2, 516·7, 516·6, 516·7, 517·1, &c., recorded by Dunér in his observation of α Orionis as the edge of a dark band, *could be nothing but the edge of the brightest band of carbon*, the bright cometary band *par excellence*, and therefore that these so-called stars not only resemble comets in their absorption flutings, as we now learn, but in their radiation flutings as well; in short, these stars *were* comets, with the difference—a trifling one from my then point of view—that they were not moving round our sun.

This surmise has since been abundantly confirmed. The dark band of Dunér is a *contrast band*—the spectrum looks dark there on account of the extreme brilliancy of the carbon fluting. The other carbon flutings were next sought for and easily found.

These "stars," then, instead of being like our sun, consisted of swarms of meteorites. We have in these bodies a spectrum integrating the *radiation* of carbon and the *absorption* of manganese and lead vapour, as in the case of some comets.

The law of parsimony compels us to ascribe the bright fluting of carbon in these "stars" to the same cause as that at work in comets, where we know it is produced by the vapours between the individual meteorites or repelled from them. Hence we are led to conclude that the absorption phenomena are produced by incandescent vapours surrounding individual meteorites which have been rendered intensely hot by collisions, while the carbon light comes from the interspaces.

I propose in the present paper to give a summary of the evidence of cometary kinship, so to speak, among the other cosmical bodies; and I shall follow this by an historical statement showing how previous observers have suspected the presence of carbon in "stars."

First as to cometary kinship.

The discussion of cometary spectra which I communicated to the Royal Society in November 1888 (Roy. Soc. Proc., vol. xlv. pp. 159–217), contained, among other matters, conclusions which have a special bearing on the relations of their spectra to those of other bodies.

It is obviously desirable to compare this material with the more complete lists of lines which I have now obtained from a very thorough search after all the observations hitherto made of other groups of celestial bodies, since such a comparison—a much more complete one than was possible in the first instance—would strengthen or weaken my hypothesis according as the increased area of observation increased or decreased the number of coincidences in the spectra of the various groups.

The more the coincidences are intensified the greater is the probability that comets, nebulæ, stars with bright lines, stars with mixed flutings, and the aurora have a common origin, independent of the chemical origins which have been assigned to the various lines by laboratory observations.

In the tables which follow, the individual observations are not given, but under each heading all the lines or flutings which have been recorded find place.

I. *Comparison of Comets and Nebulæ.*

We may conveniently begin with a comparison of comets and nebulæ. The Great Comet of 1882 and Comet Wells, when near perihelion, are excluded from the list of cometary lines and flutings, as their temperature was too high for fair comparison with most of the nebulæ and other low temperature phenomena.

In cases where any of these higher temperature lines correspond to lines in the comparison spectrum, however, they have been added to the list of cometary lines, in brackets, as sometimes the phenomena compared may attain a temperature slightly higher than that of comets at mean temperature.

For the nebulæ, all the lines recorded in the visible spectrum by Messrs. Huggins, Vogel, Copeland, Fowler, and Taylor, are given. The list of lines has been considerably extended since my preliminary discussion of the spectra of nebulæ in November 1887. D_3 and a line at 447 have been observed in the spectrum of the nebula in Orion by Copeland, and Mr. Taylor has also recorded D_3 and lines, or remnants of flutings, at 559 and 520. In the nebula in Andromeda, carbon flutings and the lead flutings at 546 have been observed by Mr. Fowler and confirmed by Mr. Taylor; since these observations were made, I find that Vogel (*Bothkamp*, *Beob.*, Heft 1, 1872, p. 57) observed a line at 518, probably carbon 517, in nebulæ numbered in Sir J. Herschel's General Catalogue 4234, 4373, and 4390.

Other nebula lines with which I was not previously acquainted are 479, 509, and 554. All these lines were observed by Vogel in the nebula G.C. 4378 (*Bothkamp*, *Beob.*, Heft 1, 1872, p. 57).

With reference to the appearance of D_3 in nebulæ and bright-line stars, I wrote, in November 1887 (Roy. Soc. Proc., vol. xliii. p. 139):—"It is right that I should here point out that some observers of bright lines in these so-called stars have recorded a line in the yellow which they affirm to be in the position of D_3; while, on the other hand, in my experiments on meteorites, whether in the glow or in the air, I have seen no line occupying this position.

"I trust that some observer with greater optical means will think it worth his time to make a special inquiry on this point. The arguments against this line indicating the spectrum of the so-called helium are absolutely overwhelming. The helium line so far has only been seen in the very hottest part of the sun which we can get at. It is there associated with *b*, and with lines of iron which require the largest coil and the largest jar to bring them out, whereas it is stated to have been observed in stars where the absence of iron lines and of *b* shows that the temperature is very low. Further, no trace of it was seen in Nova Cygni, and it has even been recorded in a spectrum in which C was absent, and once as the edge of a fluting.[1]

"It is even possible that the line in question merely occupies the position of D_3 by reason of the displacement of D by motion of the 'stars' in the line of sight. On this point no information is at hand regarding any reference spectrum employed.

"*If, however, it should eventually be established that the line is really* D_3, *which probably represents a fine form of hydrogen, it can only be suggested that the degree of fineness which is brought about by temperature in the case of the sun, is brought about in the spaces between meteorites by extreme tenuity.*"

The observations of Dr. Copeland (*Monthly Notices R.A.S.*, vol. xlviii. p. 360), have now, I think, established the identity of the yellow line, in the nebula of Orion at all events, with D_3. In a letter to Dr. Copeland, I suggested that the line at 447 was in all probability Lorenzoni's *f* of the chromosphere spectrum, seeing that it was associated both in the nebulæ and chromosphere with hydrogen and D_3. This he believes to be very probable. The line makes its appearance in the chromosphere spectrum about 75 times to 100 appearances of D_3 or the lines of hydrogen.

The association of the line at 447 with D_3 therefore strengthens the view that there is an action in space, away from condensations, whereby matter is reduced to its finest forms.

With regard, then, to the comparison of the spectra of comets and nebulæ the case stands as follows·—

Comets.	Nebulæ.	Probable Origins.	λ of Probable Origins.
—	411	H	4101
431	—	CH	431
—	434	H	434
—	447	?	—
468–474	468–474	C (hot)	468–474
—	479	?	—
483	—	C (cool)	483
486	486·3	H	486
—	495	?	—
500	500	Mg	500
—	509	?	—
517	517	C (hot)	517
519	—	C (cool)	519
521	520	Mg	521
[527]	527	Fe	527
546	546	Pb	546
—	554	?	—
558	559	Mn	558
561	—	C (cool)	561
564	—	C (hot)	564
568	—	Pb, Na	568
—	5872	? (D_3)	—

The table shows that there are many striking similarities between the two spectra, and there is no doubt that many of the lines are identical. The flutings of hot carbon, for example, are common to both, as are also the flutings of magnesium, manganese, and lead. The hydrogen line 486 has only been seen in one comet, namely, Comet III. 1880, by Konkoly ("O'Gyalla Observations," 1881, p. 5.)

Other flutings and lines again are special to comets and others to nebulæ. Thus, there are practically no indications of hydrogen in comets, although the hydrogen lines are amongst the brightest in nebulæ. Again, the lines 447, 479, 495, 509, and 554 are seen in nebulæ, but not in comets. On the other hand, the cool carbon flutings and the fluting at 568 are seen in comets, but not in nebulæ. Most of these apparent discrepancies are explained by a consideration of the differences in the conditions of comets and nebulæ. It must be remembered that in the case of comets there is an action which repels the vapours produced by collisions, and the vapours first affected will, of course, be those which are least dense. Hydrogen will thus be repelled from the comets, whilst the denser vapours of magnesium and carbon remain. There is then a good reason why hydrogen lines should *not* be seen in cometary spectra. As there can be no such repulsion in the sparse swarms which constitute nebulæ, hydrogen lines are seen in them.

Two other lines special to nebulæ are 5872 and 447, to which reference has already been made. The evidence tends to show that D_3 and *f* are finer vapours than hydrogen, and hence there is even greater reason for the absence of these lines from cometary spectra, even were the temperature higher, than for the absence of the lines of hydrogen.

The line at 527 is probably the iron line E; this was seen in the hotter comets, namely, Comet Wells and the Great Comet of 1882, so that there is no discordance with regard to the appearance of this line. The other lines special to nebulæ are 479, 495, 509, and 554; but as no origins for these have yet been determined, it is not possible to explain their absence from cometary spectra. It is not improbable that 554 is an error in measurement for the manganese fluting at 558, the latter having been recorded by Mr. Taylor in the nebula of Orion.

The apparent absence of the cool carbon flutings from nebulæ is in all probability due to insufficient observations, as indicated by the discussion of comets. The lowest

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[1] ". . . . The spectrum is very bright: two strong bands are seen in the red, then the D line, followed by a bright line (D_3) as the edge of a band. . . . ." (Konkoly, "Neuer Stern bei χ Orionis," *Astr. Nachr.*, No. 2712).

temperature (magnesium) and the hot carbon stages of comets are both represented in nebulæ, and the intermediate cool carbon stage is therefore not likely to be entirely absent.

The absence of the hot carbon fluting at 564 from the spectra of nebulæ may possiby be due to two causes. It is much fainter than either 517 or 468–474, and may have escaped notice on that account; or, as in the nebula in Andromeda, it may be masked in the same way as in comets.

It is suggested that the ordinary nebulæ are not hot enough to give the line or fluting at 568, but it appears when the swarms become more condensed—that is, in bright-line stars. The absence of 568 is therefore probably due to the low temperature of nebulæ.

II. *Comparison of Comets and Auroræ.*

If we exclude the exceptional cases of Comet Wells and the Great Comet of 1882, the number of lines and flutings recorded in comets is small, and therefore only the most general list of auroral lines must be taken for comparison. It would be unfair, for example, to take the long list of lines given by Gyllenskiöld. The lines stated are taken from the table which I gave in a note in January 1888 (Roy. Soc. Proc., vol. xliii. p. 321) which has since been slightly rearranged before taking the means.

Comets.	Auroræ.	Probable Origins.	λ of Probable Origins.
—	411	H	4101
[426]	426	?	?
431	431	CH	431
—	435	H	434
468–474	474–478	C (hot)	468–474
483	482	C (cool)	483
486	486	H	486
500	500	Mg	5006
517	517	C (hot)	517
519	519	C (cool)	519
521	522	Mg	521
—	531	?	—
—	535	Tl	535
—	539	Mn	540
546	545	Pb	546
558	558	Mn	558
561	—	C (cool)	561
564	—	C (hot)	564
568	—	Pb, Na	568
—	606	?	—
[615]	620	Fe	615
—	630	?	—

Here, again, it will be seen, that there are many striking coincidences. The hydrocarbon fluting at 431 and the hot and cool carbon flutings at 468–474, 483, 517, and 519 are common to both. The flutings of magnesium 500 and 521 and the flutings of lead and manganese at 546 and 558 are also common. The iron fluting at 615 is not seen in comets at ordinary temperatures, but since it was recorded in the Great Comet of 1882, it has been added, in brackets, to the list of cometary flutings. The line at 426, which was seen in Comet Wells, has also been added. It will be noted also that there are apparent discrepancies; some lines appearing only in comets and others only in auroræ. The explanation of the absence of hydrogen lines from comets which has already been given applies equally in this case. As there is no repulsion in the aurora similar to that exercised upon comets by the sun, there is no reason for the absence of hydrogen. In the aurora the hydrogen lines may also be produced partly from aqueous vapour. The citron carbon flutings 561 and 564 have not been recorded in the aurora, although they are often seen in comets; their apparent absence from the aurora is probably because they fall in the brightest part of the continuous spectrum, and are consequently masked.

The lines special to auroræ are 531, 535, 539, 606, and 630.

III. *Comparison between Comets and Bright-line Stars.*

In the Bakerian Lecture for 1888 I gave a complete discussion of the spectra of bright-line stars, as far as the observations then went, and the conclusion arrived at was that they are nothing more than swarms of meteorites a little more condensed than those which we know as nebulæ. The main argument in favour of this conclusion was the presence of the bright fluting of carbon which extends from 468 to 474. This, standing out bright beyond their short continuous spectrum, gives rise to an apparent absorption-band in the blue. The varying measurements made by different observers may possibly have thrown a little doubt upon the conclusion that the bright band was due to carbon, but recent observations at Kensington have placed this beyond doubt. Direct comparisons of the spectrum of all the three stars in Cygnus with the flame of a spirit lamp have been made by Mr. Fowler, and these showed an absolute coincidence of the bright band in the stars with the blue band of carbon seen in the flame. It was found quite easy to get the narrow spectrum of the star superposed upon the broader spectrum of the flame, so that both could be observed simultaneously.

Other evidence of carbon flutings was shown by slight rises in Vogel's light-curves near 517 and 564. These, however, could not be as well seen as the band in the blue, because they fall on the bright continuous spectrum from the meteorites. In the stars in Cygnus, Mr. Fowler detected brightenings near 517, and perfect coincidences were found with the fluting at 517 in the spirit-lamp flame. In this case both 517 and 468–474 were simultaneously seen to be coincident with flame-bands.

Measurements were made of the brightenings in the spectrum of γ Cassiopeiæ by Mr. Fowler on September 18, and these were also found to be coincident with the carbon flutings 517 and 468–474; the citron fluting at 564 was not seen. It may be remarked that C, F, and D_3 were seen very bright.

The conclusions drawn from my suggestions as to the presence of carbon, as well as hydrogen, in bright-line stars, are therefore strengthened.

In the following table, all the lines and flutings recorded in bright-line stars, with the exception of γ Cassiopeiæ, are given. The lines recorded by Sherman in γ Cassiopeiæ have not yet been confirmed.

Comets.	Bright-line Stars.	Probable Origins.	λ of Probable Origins.
—	4101	H	4101
431	—	CH	431
—	434	H	434
468–474	468–474	C (hot)	468–474
483	—	C (cool)	483
486	486	H	486
500	—	Mg	500
—	507	? Cd	508
517	517	C (hot)	517
519	—	C (cool)	519
521	—	Mg	521
[527]	527	Fe	527
—	540	Mn	540
546	—	Pb	546
558	558	Mn	558
561	—	C (cool)	561
564	564	(C hot)	564
568	568	Pb, Na	568
[579]	579	Fe	579
—	5872	? (D_3)	—
[589 (D)]	589	Na (D)	5889, 5895
—	635	?	—

The coincidences here are between the flutings of hot carbon, manganese 558, and lead or sodium 568. D has only been seen bright in one of the stars (γ Argûs), which is probably one of the hottest; since D was seen bright in two of the hottest comets, I have inserted it in the list of cometary lines and flutings, and [527] and [579] are added for the same reason.

Although nine lines or flutings are common to comets and bright-line stars, six occur in comets which do not appear in bright-line stars, and five in bright-line stars which do not appear in comets.

The apparent absence of hydrogen from comets has already been referred to, as well as the absence of D_3. The cool carbon flutings are not seen in the bright-line stars because the temperature is too high, and the line at 500 is absent for the same reason; 521 is probably also absent because of the higher temperature. The lead fluting at 546 may be masked by continuous spectrum in the bright-line stars; at all events, it appears as an absorption-band when the swarms further condense. Besides the hydrogen and D_3 lines, the lines 507, 540, and 635 appear in bright-line stars, but not in comets.

IV. *Comparison of Comets and Stars of the Mixed Fluting Group.*

In the Bakerian Lecture I also gave evidence to show that stars of Group II. (Vogel's Class III.*a*) are of a cometary character, and a little more condensed than the bright-line stars. The ground on which this conclusion was arrived at was the probable presence of bright carbon flutings, in addition to the metallic absorptions. Observations of α Herculis and Mira Ceti by Mr. Fowler at Kensington and by myself at Westgate-on-Sea have fully confirmed this view. The rapid increase of brilliancy of the flutings of Mira at its maximum in 1888 left little doubt in my mind that they were due to carbon, and Mr. Fowler's comparisons showed perfect coincidences with the carbon flutings, with the dispersion of two prisms of 60°.

Some of the origins which I suggested for the dark bands have also been tested by direct comparisons. Dunér's bands 4 and 5 were found to be coincident with the manganese and lead flutings at 558 and 546 respectively, and band 3 was found to be coincident with the manganese fluting about 586.

Mr. Maunder observed the spectrum of α Orionis on December 16, 1887, and made comparisons with the spectra of carbon, sodium, and manganese, as given by a Bunsen flame. He states the results as follows ("Greenwich Observations," 1887, p. 22):—"The carbon band at 5164 was coincident (within the limits of observation with this dispersion) with the bright space towards the blue of Band VI. (Dunér's band 7), and the sodium lines were clearly represented by two dark lines near the middle of Band II. (Dunér's band 3), but the two manganese bands observed, not only did not coincide with any great band of the spectrum, but were very far distant from any of them. There were, indeed, faint lines about the neighbourhood of either manganese band, but the entire spectrum is full of such lines, and no fluting, nor anything corresponding to one, could be detected near the place of these two bands. A third manganese band was very close to Band II. (Dunér's band 3) of the stellar spectrum." On the other hand, Vogel measured the position of the sharp edge of a fluting in α Orionis as 559·1, and Dunér's measures for the same vary from 557·5 to 559·3, none of which can be described as "very far distant" from the manganese fluting near 558. Mr. Maunder's observation can only be explained by assuming that the band in question is variable. This might be produced by variations in the intensity of the carbon flutings; the manganese fluting falls on the carbon fluting near 564, and, according to their relative intensities, the manganese fluting will be visible or will be masked by the carbon. According to Gore, the star was at a minimum in December 1887.

The fluting near 586 corresponds to Dunér's band 2, for which Dunér measures wave-lengths varying from 585·4 to 586·1. It apparently escaped Mr. Maunder's notice, at the time he made his observations, that no reference was made in my paper of November 1887 to any band in the star spectra which fell near the third fluting of manganese near 535. The first two flutings, near 558 and 586, fell so near to two of the dark bands in the spectra of the stars of Group II. that there was strong ground for believing them to be due to manganese. This has since been abundantly confirmed by Mr. Fowler's direct comparisons of the manganese flutings with the spectra of several stars of the group.

Under the heading of "Dunér's Bands" I give the mean wave-lengths measured by Dunér for the dark bands, and the limits of the bright spaces which are due to carbon.

The figures first given refer to the sharp edges of the flutings; the other figures indicate approximately where the flutings fade away.

This comparison shows that there is a very close relation between comets and Group II. independent of the probable origins suggested. Bright carbon flutings, the manganese fluting at 558, the lead fluting at 546, the iron fluting at 615, and the magnesium fluting 521, are common.

Comets.	Dunér's Bands.		Probable Origins.	λ of Probable Origin.
—	461–451	Bright space	C_B	460–451
—	461–473	(10) Dark space	—	—
468–474	472–476	Bright space	C (hot)	468–474
—	476–486	(9) Dark space	—	—
483	—	—	C (cool)	483
—	495–486	? Bright fluting	?	—
500	495–502	(8) Dark fluting	Mg	500
517	516–502	Bright fluting	C (hot)	517
519	—	—	C (cool)	519
521	516–522	(7) Dark fluting	Mg	521
—	524–527	(6) Dark fluting	Ba (2)	526
546	544–551	(5) Dark fluting	Pb	546
558	559–564	(4) Dark fluting	Mn (1)	558
561	—	—	C (cool)	561
564	—	—	C (hot)	564
—	585–594	(3) Dark fluting	Mn (2)	586
[615]	616–630	(2) Dark fluting	Fe	615
—	647–668	(1) Dark fluting	?	—

The cool carbon flutings are seen in comets, but not in stars of Group II. the reason being that the temperature is too high. The hot carbon fluting at 564 is in all probability present in stars of Group II., but is always masked, in some cases by continuous spectrum, and in others by the absorption fluting of manganese, which is nearly coincident with it.

The line, or probably fluting, at 495 has not yet been recorded in comets, but its association with the fluting at 500 in Nova Cygni indicates that its apparent absence is entirely due to incomplete observations.

The second fluting of manganese, near 586, though one of the most prominent in stars of Group II., has not been observed in cometary spectra, probably because there is not sufficient continuous spectrum from the sparse meteoritic background of the comet to produce the absorption of more than the first fluting of manganese.

Dunér's band 1, 647 to 668, has not yet had an origin assigned to it. J. NORMAN LOCKYER.

(*To be continued.*)

ON THE STUDY OF EARTHQUAKES IN GREAT BRITAIN.[1]

THERE can be little doubt that the more important contributions to our knowledge of earthquakes must be made in countries like Switzerland, Italy, and Japan; countries where earthquakes are frequently occurring, where, occasionally, they are so disastrous as to arrest universal attention, and where, at the same time, there are many skilled observers aided by a sympathetic and intelligent public. In England, as every strong shock shows, there is no lack of observers. But our earthquakes that are strong enough to attract general notice within the disturbed area are few and far between. If we exclude special districts, like Comrie and the Durham coast, we shall probably be well within the mark in stating the average number recorded as less than one a month.

The number of earthquakes that occur in Great Britain must, however, be far greater than this. From various causes, many shocks that are felt are never placed on record. Others, again, that might be felt, must certainly pass unnoticed, for, wherever seismic studies are newly organized, it is found that people become educated in detecting earthquake-shocks. But, however skilful observers may become, there must always be a large number of shocks that never could be felt, either from the small amplitude or the long period of their vibrations. Even in Tokio, where they talk about earthquakes as we in England talk about the weather, "the majority of shocks pass unfelt by people, while seismographs register them sufficiently to allow measurements" (S. Sekiya, Japan Seism. Soc. Trans., vol. x. p. 59).

There is every reason to conclude, then, that, with the aid of simple time-recording seismoscopes, the earthquakes of Great Britain would be found sufficiently numerous to repay a more careful and systematic study. That we shall have to be content, as a rule, with observing shocks that would elsewhere be considered slight is evident of course; but in their very feebleness we possess advantages which are not afforded by severer shocks of other lands. Not only are the phenomena much less complex; but, not being unnerved by danger, the observer is able to concentrate his attention on them more calmly and completely. Still more important is the fact, and in this lies their greatest value, that, the smaller the area disturbed, the more nearly can the position of the epicentrum be determined. If, as is frequently the case, the shock be felt only within a small circular area, we cannot be far wrong in regarding the centre of that area as the approximate site of the epicentrum. And thus we easily obtain the solution of what, in a great earthquake, is one of the most difficult and important of the problems to be attacked.

Methods of Study in Great Britain.—Owing to the feebleness of our shocks, and their comparative rarity in a given district, the methods of study employed by us must clearly be different from, and inferior to, those adopted with such signal success in Italy and Japan. We can hardly expect, for instance, that costly recording instruments will be widely used in this country; for, even in Italy, as Prof. de Rossi points out (*Bull. del Vulc. ital.*, anno iv., 1877, p. 5), it has been found better to have a large number of observatories containing cheap and simple instruments, than a few equipped with seismographs more perfect and refined.

If, on the one hand, then, our methods of earthquake study are limited by the nature of the shocks we experience, on the other we possess advantages, apart from those already mentioned, that are more or less wanting in regions where the phenomenon attains a more destructive and interesting development. For instance, most parts of England at any rate are so densely populated that we are able, almost wherever a shock occurs, to procure a large number of observations of very considerable value. And again, in the ease and accuracy with which we can regulate our clocks in the neighbourhood of every large town, we have an aid in our work which is as valuable as it is rare in foreign countries. These two facts in particular I mention here because they form the foundation of our two most promising methods of investigation.

Looking at earthquakes chiefly from a geological point of view—that is, regarding them as mere incidents, but at the same time delicate indices, of the progress of terrestrial evolution—the prime object of our inquiries is in every case to determine the position of the epicentrum, and, if possible, that of the seismic focus. For this purpose, we have three methods at our disposal, depending severally upon observations of the direction, intensity, and time of occurrence, of the shock in different parts of the disturbed area.

The first method is interesting historically from its having been used by Mallet in the earliest scientific study of an earthquake. But modern seismologists have with good reason generally discarded it; and, in any case, it could hardly be employed with success in this country.

The method of intensities is far more trustworthy, and is attended with good results whenever the observations are sufficiently numerous and made at places that are fairly evenly distributed over the disturbed area. With the aid of such a scale as that drawn up by MM. de Rossi and Forel, the intensity of the shock at any point may be roughly estimated. Then, drawing lines including all places where the intensity is at or above a certain degree of the scale, we obtain a series of lines of equal intensity (isoseismal lines), which, closing in towards the epicentrum, enable its position to be approximately determined. For the slighter shocks that we experience, it would be difficult to over-estimate the value of this method, the only one that in certain cases can be applied.

The last of the three methods, I think I may say, is still upon its trial; and if, so far, it has not yielded all the results that are to be expected from it, I believe the reason is that it has not yet been attempted in a country where the conditions are so favourable for its application as they are in many parts of England. What we require for the purpose is, not a network of time-recording instruments extending over the whole country, so much as a moderate number suitably placed and regularly observed in specially selected districts. If, by means of these instruments or otherwise, the times of a shock can be ascertained with accuracy at five or more places, these, under certain conditions, are theoretically sufficient to determine the position of the epicentrum, the depth of the seismic forces, the velocity of the earth-wave, and, consequently, the time of occurrence at the focus. And it should be noticed that time-recorders in Great Britain are practically free from the objection which attends them in Japan and other regions where earthquakes frequently last for one or several minutes. For, in such cases, the character of the shock varies so greatly throughout the disturbed area, that it need not, and probably will not, be one and the same vibration which is registered in different places, and considerable errors may thus be introduced.[1] If, then, we remember that our earthquake shocks seldom last for more than a few seconds at most, and that, in many parts of England and some parts of Scotland, it should be possible to ascertain the time of occurrence correctly to within a small fraction of a minute, I think there can be little doubt that, for all but the slightest shocks, a most fruitful method of earthquake

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[1] A Paper by Charles Davison, M.A., King Edward's High School, Birmingham; read before the Birmingham Philosophical Society on February 5, 1890. A few passages added since the paper was read are enclosed in brackets.

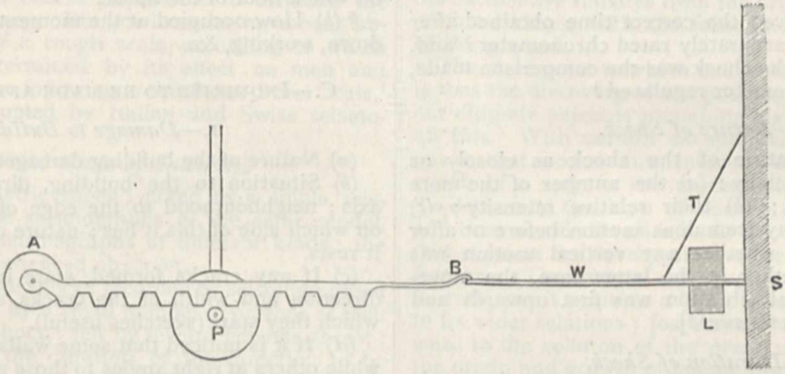
[1] E. Knipping and H. M. Paul, Japan Seism. Soc. Trans., vol. vi. p. 37; also J. A. Ewing, vol. iii. pp. 63–64, and J. Milne, vol. iv. pp. 100–101.

study in this country would be a system for securing accurate time-records whenever a shock is felt.

Seismoscopes.—A large number of simple and inexpensive seismoscopes have been devised and used for recording the time of occurrence of an earthquake shock; but it is difficult to find one that in all respects is thoroughly satisfactory.[1] To be so, they should fulfil the following conditions:—They should be inexpensive, simple in their construction, easy to arrange, and require little attention when once erected. They should record the occurrence of shocks and tremors with equal facility in whatever direction they may arrive; and they should be equally sensible in recording a feeble shock. It is very desirable also that they should be of similar construction, at any rate in a given district, if not throughout the whole country, so that observations from different places may be rightly comparable.

Again, in countries where earthquakes are frequent, and where the shocks may succeed one another at short intervals, it is important that the record should be made without stopping the clock. In Great Britain, however, our catalogues show that, except at Comrie, it is not usual for sensible earthquakes to follow one another rapidly, and it is therefore worth while considering whether, on account of their much greater cheapness and simplicity, it might not be well to avail ourselves in this country of clock-stopping apparatus. Such instruments, are, of course, defective in that, until re-set, they are incapable of recording a second earthquake. But they possess a compensating advantage in the accuracy with which it is possible to time the occurrence of a shock.

Clock-stopping Apparatus.—As it is possible to make with ease, and at little cost, an extremely delicate apparatus for stopping a clock at the moment of a shock, I quote the following description of one devised by Prof. J. Milne (Japan Seism. Soc. Trans., vol. iii. pp. 61–62):—"P is the pendulum of a clock with a small piece of wire standing out at right angles to its face. . . . This wire, as the pendulum swings, passes beneath a series of teeth cut in a strip of wood lightly hinged at A and terminating at the other end, B, with a piece of stiff wire. . . . If such a contrivance is allowed to fall, the teeth catch on the projecting pin of the pendulum, and it may arrest it at any portion of its swing." The arrangement which, at the time of an earthquake, allows the toothed lever to fall "consists of a piece of stiff wire, W, on which, near to one end, is a small cylinder of lead, L. The short end of this wire is pointed, and rests on a pivot-hole made in the head of a drawing-pin pressed into the side of the clock-case, S. To prevent this wire from falling, it is held up by a small silk thread, T, fastened to a second drawing-pin. As suspended, it is very unstable, and, instead of remaining at right angles to the clock-case, it swings round against it. When, however, the wire, B, rests on the end of W, it retains its position, as shown in the figure."



This instrument is so sensitive that it is difficult even to shut the clock-case without stopping the clock. The reason of this appears to be "that, if the clock-case receives a small displacement at right angles to W, the weight remains steady by its inertia, whilst the long arm of W in contact with B multiplies the initial motion" approximately in the proportion of the length of the long to that of the short arm of W.

It would appear that a displacement parallel to the wire W would not give this multiplication; but, practically, Mr. Milne observes, "it seems impossible to give a motion in that direction to which the apparatus does not seem to be just as sensible as to a motion in any other direction. The only other motion which does not result in stopping the clocks appears to be a *very slow* easy swing;" and thus the instrument will probably be incapable of recording the occurrence of the dying-out vibrations of a very distant shock.

The instrument may be placed in a cellar or out-house, or out of doors under the cover of a close-fitting box. A strong stake should be driven into the ground, to the depth of two or three feet, the floor, if any, being removed for a few inches round the stake to prevent the instrument being disturbed by the vibrations of the house. The clock-case should then be screwed firmly to the stake.

If several of these instruments are erected in a district, they should be placed at distances of not less than 5 to 10 miles apart.[1] The sites selected should, if possible, be free from the vibrations of passing carts and trains. If two or three of these record the occurrence of a shock at very nearly the same instant, it may be inferred that the disturbance is not accidental in its origin, and the inference will be strengthened if several instruments closely agree in their indications. But a record from one alone must obviously be regarded as doubtful, if all the others were at the time in good working order.

Suggestions for the Observation of Earthquakes (without the use of special instruments).—Lists of questions for aid in the study of earthquakes have been drawn up by Prof. Heim and Prof. Milne.[2] The following questions are founded partly on these lists, but chiefly on the accounts of earthquakes in different places, and especially in this country. It is hardly necessary to insist that all notes should be written down on the spot, or as soon after the shock as possible; but it may be useful to remark that it is often just as important to note when a given phenomenon is *not* observed as to describe it fully

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[1] A Committee of the British Association is at present considering the form of seismoscope most suitable for use in this country.

[1] If the clock be carefully rated, it should be possible to obtain the time of a shock correct to a tenth of a minute. The velocity of earth-waves is subject to wide variations, even in traversing the same rocks; but, taking it at 1000 feet per second, it follows that the earth-wave will pass over more than a mile in one-tenth of a minute. A good deal more than a mile, then, should separate every pair of stations where seismoscopes are placed.

[2] A. Heim, "Die Erdbeben und deren Beobachtung"; *Arch. des Sc. phys. et nat.*, 3me pèr. t. iii. pp. 286–7; Fouqué, "Les Tremblements de Terre," pp. 133–4; Japan Seism. Soc. Trans., vol. i. part ii. pp. 3–4.

when it is observed. This applies particularly to the mere fact of the perception of the shock, as a knowledge of the places where it is just not felt is of service in enabling us to determine the boundary of the disturbed area.

The questions are arranged in the following sections: A, for places where the shock is felt; B, for those where it is not felt; and C, inquiries to be made after the shock. In each case, the questions to which it is most important that answers should be given are marked with an asterisk.

A.—FOR PLACES WHERE THE EARTHQUAKE IS FELT.

1.—*Place of Observation.*

* (*a*) Its name and position.

(*b*) Nature and form of the surrounding ground, especially with reference to its geological structure and the neighbourhood of mountains, rivers, cliffs, &c.

2.—*Situation of Observer.*

* (*a*) Whether indoors or in the open air; if indoors, on which floor of the house.

(*b*) If indoors, the direction of the street or of the longer axis of the house (if detached).

* (*c*) How occupied at the moment of the shock—lying down, working, &c.

3.—*Time of Occurrence.*

* (*a*) Time at which the shock was felt; if possible to a tenth of a minute.

* (*b*) Is the time given the correct time obtained after comparison with an accurately rated chronometer? and, if so, how long after the shock was the comparison made, and how is the chronometer regulated?

4.—*Nature of Shock.*

* Describe the nature of the shock as closely as possible, stating especially: (*a*) the number of the more prominent vibrations; (*b*) their relative intensity; (*c*) whether there was any tremulous motion before or after the vibrations; (*d*) whether any vertical motion was perceptible; [(*e*) whether, in the latter case, the movement in the principal vibration was first upwards and then downwards, or *vice versâ*].

5.—*Duration of Shock.*

* Total duration, exclusive of that of the sound phenomena (stating whether estimated, or determined by watch).

6.—*Intensity of Shock.*

* Was the shock strong enough (*a*) to make windows, doors, fire-irons, or crockery, &c., rattle; (*b*) to cause the chair or bed on which you were resting to be perceptibly raised or moved; (*c*) to make chandeliers, pictures, &c., swing, or to stop clocks; (*d*) to overthrow ornaments, vases, &c., or cause plaster to fall from the ceiling; (*e*) to throw down chimneys or make cracks in the walls of buildings?

7.—*Direction of Shock.*

(*a*) Direction of the principal shock or shocks.

(*b*) Means by which the direction was ascertained.

(*c*) Was any change of direction perceptible during the earthquake?

8.—*Sound-Phenomena.*

* (*a*) If any rumbling sound was heard at the time of the shock, what did it resemble?

[* (*b*) Did the sound end abruptly, or die away gradually?

* (*c*) Did the sound become deeper or higher towards the end?]

* (*d*) Did it precede, accompany, or follow the shock? (Times useful, especially the intervals between the beginning of the shock and of the sound, and between the ending of the same; stating whether estimated, or determined by a watch.)

* (*e*) Duration (given by 5*a* and 8*b*, if not determined separately).

9.—*Effect on the Water of Ponds, &c.*

Were any movements observed in the water of ponds, rivers, lakes, or the sea, at, or shortly after, the time of the shock; if so, of what kind?

Accessory Shocks.

* Were there any slight shocks preceding or following the principal shock or shocks? If so, a list of these, with the place of observation, time of occurrence, and answers to any of the above questions, would be of great value.

B.—FOR PLACES WHERE THE EARTHQUAKE IS NOT FELT.[1]

1.—*Place of Observation.*

* (*a*) Its name and position.

* (*b*) Nature and form of the surrounding ground, especially with reference to its geological structure and the neighbourhood of mountains, rivers, cliffs, &c.

2.—*Situation of Observer.*

* (*a*) Whether indoors or in the open air; if indoors, on which floor of the house.

* (*b*) How occupied at the moment of the shock—lying down, working, &c.

C.—INQUIRIES TO BE MADE AFTER THE SHOCK.

1.—*Damage to Buildings.*

(*a*) Nature of the building damaged.

(*b*) Situation to the building, direction of its longer axis; neighbourhood to the edge of a cliff or bank, and on which side of this it lies; nature of the rock on which it rests.

(*c*) If any cracks formed, state in which walls; the direction and width of the cracks, and the points from which they start (sketches useful).

(*d*) If it is noticed that some walls are much damaged, while others at right angles to those are but little affected, what are the directions of these walls?

2.—*Rotation of Objects.*

* (*a*) If objects, such as chimneys, grave-stones, gate-pillars, &c., have been rotated on their bases during the shock, describe the initial and final positions of the objects (sketches useful); or state the direction and amount of the rotation (looking down on the object from above, is the direction the same as that in which the hands of a watch rotate, or opposite to that direction?).

(*b*) Is there any evidence of rotation in bodies with a circular base?

3.—*Effects on the Ground, Springs, &c.*

(*a*) Were any fissures or cracks formed in the ground? If so, state their length, width, depth, and direction, the nature of the ground in which they occur, and their relation to neighbouring cliffs, banks, &c.

(*b*) Was the height, quantity, or temperature of the water in springs affected by the shock?

4.—*Observations in Mines.*

If the earthquake was felt in a mining district, inquiries should be made as to the nature of the shock and of the sound-phenomena when observed by men in the mines; the depth of the workings in such cases, &c.

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[1] [The value of observations under this heading would be greatly increased if they are the result of numerous inquiries made in a district.]

5.—*Records of Self-registering Instruments.*

* An examination should be made of the records of self-registering instruments within or near the disturbed area—particularly of recording barometers, magnetic and tidal apparatus—with a view to determine the effects of the shock on these instruments, and also to ascertain by their means the exact time of occurrence.

While answers to any of the above questions would be useful in the study of an earthquake, especial pains should, if possible, be taken to determine accurately the time at which the principal shock occurs. Immediately it is felt, the time should be noted to the nearest second, and written down at once, a few seconds (to be ascertained by trial) being allowed for taking out the watch and reading off the time. As soon afterwards as possible, the watch used should be compared with an accurately regulated clock. But if this cannot be done, if the record cannot be relied on as correct to within a small fraction of a minute, a less close approximation cannot as a rule possess much value. The chief use of such a record is then to determine the epoch of the shock; and, in a matter of this kind, when two consecutive shocks in a given district may be separated by an interval of several years, a question of a few minutes, more or less, is of very little moment.

Next in importance to time-observations are those on the intensity of a shock. Without the aid of delicate instruments it is of course impossible to estimate the intensity with accuracy. But good results have been obtained by the use of a rough scale, according to which the intensity is determined by its effect on men and their dwellings. The following is the Rossi-Forel scale,[1] which is widely adopted by Italian and Swiss seismologists:—

Rossi-Forel Scale of Intensity.

I. Micro-seismometric shock: noted by a single seismograph, or by some seismographs of the same model, but not by several seismographs of different kinds; the shock felt by an experienced observer.

II. Extremely feeble shock: recorded by seismographs of different kinds; felt by a small number of persons at rest.

III. Very feeble shock: felt by several persons at rest; strong enough for the duration or the direction to be appreciable.

IV. Feeble shock: felt by persons in motion; disturbance of movable objects, doors, windows; cracking of ceilings.

V. Shock of moderate intensity: felt generally by everyone; disturbance of furniture and beds, ringing of some bells.

VI. Fairly strong shock: general awakening of those asleep; general ringing of bells, oscillation of chandeliers, stopping of clocks; visible disturbance of trees and shrubs; some startled persons leave their dwellings.

VII. Strong shock: overthrow of movable objects; fall of plaster; ringing of church bells; general panic, without damage to buildings.

VIII. Very strong shock: fall of chimneys, cracks in the walls of buildings.

IX. Extremely strong shock: partial or total destruction of some buildings.

X. Shock of extreme intensity: great disasters, ruins; disturbance of strata; fissures in the earth's crust; rock-falls from mountains.

Results to be expected.—It may be useful, in conclusion, to point out some of the results we may expect to obtain from a systematic study of earthquakes in this country.

The mere indication of the occurrence of a shock felt at a given place on a given day is of service in the compilation of earthquake statistics, and will tend to give completeness to our seismic record. With the help of such a record we can study the laws of the periodicity and geographical distribution of earthquakes. The time is past for drawing up chronological tables of shocks felt over the whole earth; but the importance of making our records complete for a definite area of study is becoming more and more evident.

The accurate determination of the time of occurrence in different places is of the very highest importance. Such observations, if sufficiently numerous, will help us in investigating the position of the area which constitutes the epicentrum; the way in which the vibrations are propagated outwards from the epicentrum; the velocity of the earth-wave, and the laws according to which the velocity varies with the distance from the origin. A knowledge of the time will also determine the question of the coincidence of shocks in distant areas, separated by a region in which the shock is not felt at all, and of other phenomena which may seem to be more or less intimately connected with the earthquake.

By a study of the intensity in the different parts of the disturbed area, we are enabled to draw one or more isoseismal lines with a fair approach to accuracy. From the form of these lines we can ascertain the approximate position of the epicentrum; and, from the relative distances between consecutive pairs of such lines, we can determine the way in which the intensity decreases as the earth-wave radiates from the origin, and the relations of this decrease with the form and geological structure of the ground.

The chief point to which our researches at present tend is thus the discovery of the position of seismic foci. But our ultimate object is something higher than and beyond all this. With certain exceptions, the slightest earthquake that occurs must indicate the site and mark the epoch of a step in the process of terrestrial evolution. To determine the laws of seismic distribution in space and time would therefore be to discover, in part, the laws that regulate the development of the earth's great surface-features. The study of earthquakes is fascinating enough in itself, but it acquires a loftier significance when viewed in its wider relations; for through it we may press forward to the solution of the great problem of geology—the origin and growth of mountain-chains.

THE HORNED DINOSAURS OF THE UNITED STATES.

IN vol. xxviii. of NATURE (pp. 439 and 515), an account was given by Prof. Moseley of the magnificent skeletons of Iguanodons now mounted in the Brussels Museum of Natural History, which were at that time regarded as among the most remarkable of that extinct group of giant reptiles commonly known as Dinosaurs. Since that date, however, we have been gradually—thanks to the indefatigable labours of the transatlantic palæontologists—acquiring a fuller knowledge of the representatives of this curious group, of which the remains are preserved in such fine condition in the Secondary rocks of the United States Within the last few years, from the writings of Profs. Marsh and Cope—and more especially the excellent figures by which those of the former are illustrated—we have acquired so much information as to the form and structure of the gigantic Jurassic species belonging to the Sauropodous sub-order of the Dinosaurs—such as *Brontosaurus*—that we have begun to regard these extinct creatures as old friends (or should we rather say enemies?) and to flatter ourselves that our knowledge of the whole class is well nigh complete.

Recent discoveries in the topmost Cretaceous or Laramie deposits of North America have, however, brought to light the existence of a group of Dinosaurs, hitherto only very imperfectly known, which are remarkable, not only on

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[1] *Arch. des Sc. phys. et nat.*, 3me pèr. t. xi. pp. 148–149; Fouqué, "Les Tremblements de Terre," p. 22 (footnote); *Bull. del Vulc. ital.*, anno iv. (1877), pp. 39–40.

account of their gigantic dimensions, but as being the most bizarre and uncouth-looking forms which palæontology has yet brought to our notice. These are the so-called horned Dinosaurs of the Laramie, in regard to which several important memoirs have been published both by Prof. Cope and Prof. Marsh. There is, however, unfortunately some difference of opinion between these two eminent palæontologists as to the comparatively trivial point of the proper nomenclature to be applied to the various genera; and we must not be supposed to prejudge this question if we adopt the names employed by Prof. Marsh, to whom we are indebted for our illustrations.

As their name implies, one of the most striking features in the organization of these uncouth monsters is the presence of large horn-cores on the skull, as shown in Fig. 1.

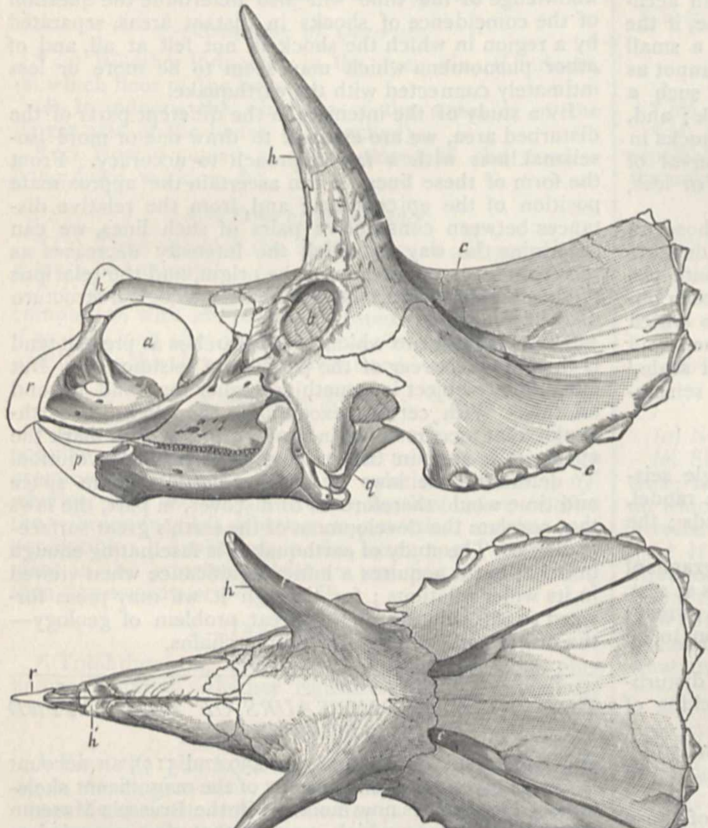

FIG. 1.—Left lateral and superior aspects of the skull of *Triceratops flabellatus*; from the Cretaceous of the United States. 1/20 nat. size. *a*, nostril; *b*, eye; *c*, supratemporal fossa; *e*, epoccipital bone; *h*, frontal, and *h'*, nasal horn-core; *p*, predentary bone; *q*, quadrate bone; *r*, rostral bone. (After Marsh.)

These horn-cores are so like those of the oxen that some detached specimens found lying on the surface of the ground were actually described as belonging to an extinct bison.

The type of skull of which we give a figure belongs to the best known genus, for which Prof. Marsh proposes the name of *Triceratops*. It is remarkable not only for its gigantic size—the length of the figured specimen, which is said to indicate an immature individual, being about six feet—but also for its peculiar armature and structure. An imperfect skull of another species exceeds these dimensions, huge as they are, and is estimated when entire to have had a length of over eight feet. No other known animals, except whales, have a skull making any approach to these dimensions; that of the huge *Brontosaurus* being very small in comparison with the bulk of its owner. The skull of *Triceratops* is remarkable for its wedge-like form when viewed from above, and carries a pair of large horn-cores immediately over the eyes, and a short and single core above the nose. During life it may be inferred with a high degree of probability that these cores were sheathed with horn, like those of oxen, and that they proved equally effective weapons of defence. Equally remarkable is the huge flange-like expansion of the posterior region of the skull, evidently necessary for the attachment of muscles sufficiently powerful to support such a ponderous structure; and it is also peculiar for the presence of an *epoccipital* bone (*e*), which is quite unknown in all other animals. The structure of the teeth is somewhat similar to that obtaining in *Iguanodon*, but each tooth has two distinct roots. As in the latter, the extremity of the lower jaw is devoid of teeth, and likewise has a separate predentary bone at its extremity. The upper jaw is, however, quite peculiar in having a distinct toothless *rostral* bone at the extremity of the premaxillæ. It would thus seem probable that the mouth of these reptiles formed a kind of beak sheathed in horn like that of a tortoise. In young individuals the nasal horn-core is a separate ossification, but in the adult it becomes firmly anchylosed to the underlying bones; so that in this respect we have a precise analogy with the horn-cores of the giraffe. The brain of the creature is very minute—relatively smaller, indeed, than in any known vertebrate; this, however, might have been expected from the size of the brain in other Dinosaurs, since, in the same groups, large animals always have relatively smaller brains than their smaller allies.

Besides mentioning that the limb-bones resemble those of the armed Dinosaurs known as *Stegosaurus*, the only other portion of the skeleton to which we shall allude is the pelvis, of which a representation is given in Fig. 2. In this portion of the skeleton the haunch-bone or ilium (*il*) is remarkable for its great extension both in front of and behind the cavity, or acetabulum (*a*), for the head of the thigh-bone; and also for its horizontal or roof-like expansion, which is in marked contrast to the vertical plate-like form which is assumed by this bone in most other members of the order. With one important exception, the general contour of the pubis and ischium also comes nearest to that found in *Stegosaurus*; this being especially shown in the relation of the former bone to the ilium, and in the shape of the plate which it gives off to form the inner wall of the acetabulum. The remarkable exception is, however, that whereas in *Stegosaurus*, *Iguanodon*, and all other allied forms the pubis gives off a long backwardly projecting process running parallel with the ischium, in the present form there is no trace of any such process.

Mainly from the absence of this postacetabular process of the pubis, Prof. Marsh is disposed to regard the horned Dinosaurs as constituting a distinct primary group of the order; equivalent to those generally known as Sauropoda, Theropoda, and Ornithopoda. The resemblance in the structure of the limb-bones, and in a less degree that of the pelvis, to the loricated forms known as *Stegosaurus*, together with the nature of the dentition, render it, however, far more probable that we should regard these strange reptiles as peculiarly modified forms referable to the sub-order Ornithopoda—the group which includes *Iguanodon* and *Stegosaurus*. In the course of his description Prof. Marsh remarks that, from the relatively large size of the humerus, the horned Dinosaurs were evidently quadrupedal; and since the presence of a postacetabular process to the pubis is evidently (as exemplified in birds and *Iguanodon*) in some way connected with the bipedal progression, it may be a fair inference that, owing to the resumption of a quadrupedal progression in the forms under consideration, this process has been lost. We may note that the pubis of *Triceratops* seems undoubtedly to correspond with the pre-acetabular portion of the pubis of *Stegosaurus*, and not with the pubis of *Megalosaurus*, which represents the postacetabular portion of the latter.

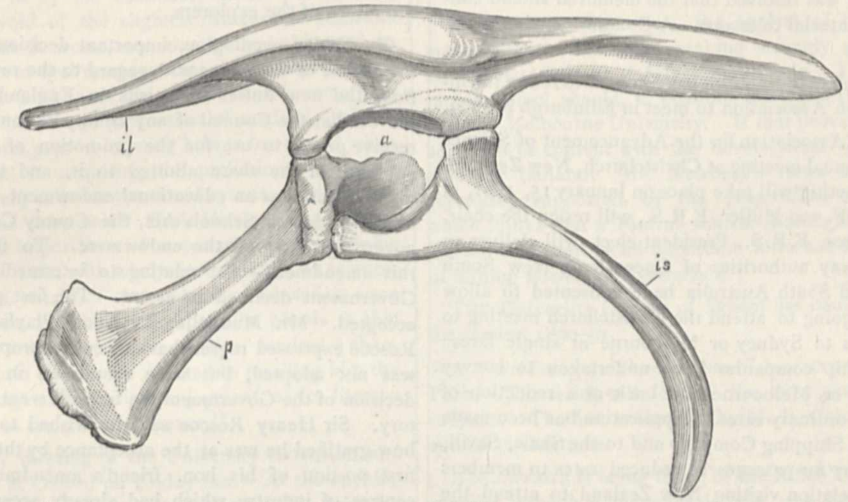

FIG. 2.—Left lateral aspect of the pelvis of *Triceratops flabellatus*. $\frac{1}{12}$ nat. size. *a*, acetabulum; *il*, ilium; *p*, pubis; *is*, ischium. (After Marsh.)

The nature of the dentition clearly shows that the horned Dinosaurs of the Laramie were of herbivorous habits, and as it seems impossible that any carnivorous Dinosaurs could have successfully waged war against such giants, we may fairly regard them as the lords of the plain in the distant Cretaceous epoch.

In conclusion we may venture to express the hope that future "finds" will enable the palæontologists of the United States to give us ere long a complete restoration of the skeleton of these mighty denizens of a long-past epoch.

R. L.

THE MEETING OF THE BRITISH ASSOCIATION AT LEEDS.

ON September 3 the sixtieth meeting of the British Association will be opened at Leeds by the President-elect, Sir F. A. Abel, F.R.S. The address, the lectures to the Association, and that to the operative classes will be delivered in the Coliseum, in which upwards of 3400 persons can be well and easily accommodated. By the courtesy of the Mayor and Corporation, the Victoria Hall will be used as the reception-room, and other rooms in the Town Hall will be provided for the various offices. Excellent Section rooms within short distances of the Town Hall have been secured by the kindness of various public and private bodies.

A guide-book, giving an account of the geology, history, places of interest, and manufactures of Leeds and the district, has been prepared, and a list of lodgings and hotel-accommodation has been drawn up. Various facilities are offered by the railway companies.

His Worship the Mayor of Leeds proposes to invite the members and associates to a reception and *conversazione* in the Municipal Art Gallery; a *soirée* will be given by the Executive Committee; and an afternoon reception at the Yorkshire College.

From the facility of access due to its central position in the railway system, from the number and variety of its industries, and from the beauty and interest of the country by which it is surrounded, Leeds offers exceptional advantages to visitors, of which many eminent members and foreign men of science have already expressed their intention of availing themselves.

Members interested in applied science and manufactures will be able by the courtesy of employers of labour to acquaint themselves with most of the modern processes by which the wealth of England is being augmented. They will be able to follow the smelting and working of iron until it is converted from clay-ironstone and hematite into tools, engines, pumps, textile machinery, and, in short, into everything which can be made of iron or mild steel. They can inspect the modern improvements in the old industry of Leeds by which wool, shoddy, and mungo are converted or reconverted into woollens or worsteds, and subsequently into clothes. Tanning, boot and shoe making, brewing, and the manufacture of sanitary, fire-resisting, and artistic earthenware employ a large number of hands; while among minor industries may be noted the manufacture of sulphuric acid and other chemicals, of bottles, of paper, of soap, of matches, and of soda-water.

Those interested in geology or scenery will find on the coast and in the diversified strata exposed, much that will instruct and interest them; while, to the historian, the architect, and the archæologist, the minsters, the cathedrals, the abbeys, the churches, the castles, the Roman remains, and the historic houses will furnish many objects worthy of attention.

Excursions may be taken or will be organized, in many cases by invitation, to most of the following places:—Add Church, Kirkstall Abbey, Temple Newsam, Farnley Hall, Harewood House, Boston Spa, Low Moor Iron-works, Pontefract Castle, the Ruskin Museum, Walton Hall and Wakefield, Aldborough, Beverley Minster, Bolton Abbey and Skipton, Castle Howard, Ingleborough, Harrogate, Hemsley and Rivaulx Abbey, Malham Tarn and Gordale Scar, Richmond, Ripon Cathedral and Fountains Abbey, Settle and the Victoria Cave, Scarborough and the coast, Wensleydale, and York.

NOTES.

A MEETING was held at Stonyhurst College, on Tuesday, to consider a proposal for the establishment of some memorial of the late Father Perry. Sir Edward Watkin, M.P., presided, and he was supported by the Bishops of Salford, Shrewsbury, and Mangalore (India), Sir John Lawson, and a large body of Catholic gentry. It was resolved that the memorial should consist of a 16-inch equatorial telescope. A Committee of scientific men was appointed.

THE Town Council of Edinburgh has resolved to renew the invitation to the British Association to meet in Edinburgh in 1892.

THE Australasian Association for the Advancement of Science will hold its third annual meeting at Christchurch, New Zealand. The first general meeting will take place on January 15, 1891, at 8 p.m., when Baron F. von Müller, F.R.S., will resign the chair, and Sir James Hector, F.R.S., President-elect, will deliver an address. The railway authorities of Queensland, New South Wales, Victoria, and South Australia have consented to allow members who are going to attend the Christchurch meeting to obtain return tickets to Sydney or Melbourne at single fares; and various steamship companies have undertaken to convey members to Sydney or Melbourne and back at a reduction of 20 per cent. on the ordinary rates. Application has been made to the New Zealand Shipping Company and to the Shaw, Savill, and Albion Company for passages at reduced rates to members of the British Association visiting New Zealand to attend the meeting, and it is expected that this will be granted. Information may be obtained from Mr. A. Vaughan Jennings, 27 Chancery Lane, the local secretary in London.

THE International Medical Congress, now at work in Berlin, held its first meeting on Monday in the Renz Circus. The Berlin correspondent of the *Times* says it is calculated that no fewer than 4500 members of the medical profession were present, representing every State and city in Europe. Many also came from North and South America. The French delegates, 34 in number, were received with marked cordiality. The medical profession in England was largely represented, among those present being Sir James Paget, Sir Henry Acland, Sir Joseph Lister, Sir John Banks, Sir William Turner, of Edinburgh, Sir William Stokes, of Dublin, Prof. Grainger Stewart, of Edinburgh, Dr. Dick, Director-General of the Naval Medical Department, Mr. Ernest Hart, representing the British Medical Association, Surgeon J. K. Notter, of Netley, representing the War Office, and Dr. Lauder Brunton. The proceedings began with the opening address of the President, Prof. Virchow, who heartily welcomed to Berlin his *confrères* from all parts of the world. The President was followed by Dr. Lassar, Secretary-General of the Congress, who sketched the general plan of the labours of the Congress, and gave some interesting statistics regarding the representation of the countries taking part in it. After Herr von Gossler, Minister of Public Worship, and Herr von Forckenbeck, Burgomaster of Berlin, had welcomed the members in the name of the State and of the town of Berlin, several of the foreign delegates addressed the Congress. Dr. Hamilton, Surgeon-General of the American Army, was the first speaker. He was followed by Sir James Paget, who, on mounting the tribune, was warmly received. A paper on "The Present Position of Antiseptic Surgery," by Sir Joseph Lister, brought the proceedings to a close. At the end of the plenary sitting, the Congress resolved itself into its various Sections, which met in the halls of the Exhibition buildings in Moabit. The proceedings in the Sections on Monday were, for the most part, confined to the election of the various office-bearers. The serious work began on Tuesday.

ON July 22 Messrs. D. C. Worcester and F. S. Bournes left Minneapolis for the Philippine Islands, where they will spend two years in the study of distribution, the collection of birds and corals, and the prosecution of general zoological and botanical work. The expedition was fitted out at a cost of over $10,000, by Mr. L. F. Menage, of Minneapolis, and the collections made by Messrs. Worcester and Bournes will be deposited in the museums of the Minnesota Academy of Sciences at Minneapolis, where also the work upon the collections will be conducted after the return of the explorers.

ON Friday evening an important decision was arrived at in the House of Commons with regard to the revenue to be derived from the new duties on spirits in England. Mr. A. Acland moved that the Council of any county or county borough should receive power to use for the promotion of technical education any part of the share allotted to it, and that the remainder might be used as an educational endowment within the meaning of the Endowed Schools Act, the County Council acting as the governing body of the endowment. To the second part of this amendment—that relating to intermediate education—the Government declined to assent. The first part, however, they accepted. Mr. Mundella, Sir Lyon Playfair, and Sir Henry Roscoe expressed regret that the entire proposal of Mr. Acland was not adopted, but were unanimous in thinking that the decision of the Government, so far as it went, was most satisfactory. Sir Henry Roscoe said he wished to be allowed to say how gratified he was at the acceptance by the Goverment of the first portion of his hon. friend's amendment. To the great centres of industry which had already accepted the Technical Education Act it would be a matter of very great importance. In small places also, especially in the country, the money would be of the very greatest consequence.

DR. NANSEN's expedition to the North Pole will start in the spring of 1892. Captain Sverdrup, who will take the nautical command, is at present on board a fishing-boat in the Polar seas, practising manœuvring among ice. Dr. Nansen wishes that his crew may consist wholly of Norwegian sailors, but will admit some foreigners among the scientific staff.

Science announces that Prof. R. S. Woodward, who was for many years chief geographer of the U.S. Geological Survey, has been appointed assistant in the U.S. Coast and Geodetic Survey. Before his connection with the Geological Survey, Prof. Woodward was assistant astronomer of the U.S. Transit of Venus Commission. He was chairman of the Section of Mathematics and Astronomy of the American Association for the Advancement of Science in 1889, and is well known for his investigations in mathematics, astronomy, and physics.

WE learn from *Science* that records have been received, at the office of the U.S. Coast and Geodetic Survey, of observations made during the last cruise of the *Pensacola*. The stations include the West Coast of Africa, and some islands in the North and South Atlantic. The work was done by an officer of the survey, Assistant E. D. Preston, aided by members of the ship's company. Gravity and magnetic measures were made at St. Paul de Loanda (Angola), Cape of Good Hope, St. Helena, Ascension, Barbadoes, and Bermuda. In addition, magnetic observations alone were made at the Azores (Fayal), Cape Verde Islands (Porto Grande), Sierra Leone (Freetown), Gold Coast (Elmina), and in Angola at Cabiri. The pendulums used in the gravity work were the ones employed in 1883 in Polynesia, and in 1887 at the summit of Haleakala and other stations in the Hawaiian Islands. The computations are now under way at the office in Washington.

PROF. HUXLEY contributed to the *Times* of Tuesday a valuable letter on medical education—the subject with which Dr. Wade had dealt in his Presidential address to the British Medical Association. In this letter Prof. Huxley urges that the scientific training of medical students, and of those who propose to become medical students, should be much more thorough and exact than it has hitherto generally been. "Those who know what modern medicine is," he says, "are well aware that four years would be but a brief period of study, even if it could be allotted exclusively to the practical branches of the medical science and art. But in the present condition of middle-class education the youth of 17 too commonly enters the medical school, not only devoid of the slightest tincture of scientific knowledge, but, what is worse, so completely habituated to learn only from books or oral teaching that the attempt to learn from things and to get his knowledge at first hand is something new and strange. Thus a large proportion of medical students spend much of their first year in learning how to learn, and when they have done that, in acquiring the preliminary scientific knowledge, with which, under any rational system of education, they would have come provided." Prof. Huxley does not, of course, underrate the importance of a proper literary training for medical students. This, with adequate instruction in science, they might, he thinks, obtain, if our methods of education were improved. The reform for which he especially pleads is that "the time wasted in forcing upon the medical student a sham acquaintance with Latin should be devoted to teaching him the use of his own language and the right enjoyment of its literary wealth, no less than to the study of science."

THE third summer meeting of the University Extension and other students began at Oxford on Friday last. At the opening meeting Prof. Max Müller delivered an address. He defended the method of teaching by means of lectures, but admitted that most lectures were too long, and recommended that they should be limited, as in Germany, to three-quarters of an hour. He also defended the annual gatherings at Oxford against the charge of being mere academical picnics. He showed how well the different classes of lectures had been arranged so as to meet the requirements of different classes of students. He pointed to the large and zealous classes attending these lectures, and to the substantial work done by students who stayed at Oxford for two or three weeks after the public lectures given during the first fortnight were over. Finally, he dwelt on the silent influence which a stay at Oxford must exercise on everyone. "I doubt not," he concluded, "that while teachers and hearers are exploring together in this place the ruins of ancient thought and the labyrinth of modern science, they will feel the silent influence of Oxford, and take to heart the lesson which our University has taught to so many generations of Englishmen, Scotchmen, and Irishmen—respect for what is old and the warmest sympathy for what is new and true."

A CONFERENCE in connection with the University Extension movement was held on Tuesday in the debating hall of the Oxford Union, Mr. Arthur Sedgwick, of Corpus Christi, presiding. There was a large attendance. The subject for discussion was—Is it desirable that local committees should seek to obtain a Treasury grant in aid of the expenses of University Extension teaching? If so, on what conditions is it desirable that the grant should be distributed? The chairman said, speaking as a private individual, and not as a delegate, he most heartily assented to the proposal to ask for State aid for University extension. It seemed to him that there was no test which they could apply in order to see whether an object was worthy of State aid which could not be successfully applied to University extension. In order that the movement might have its proper development it was absolutely necessary that there should be elements of permanence in it. Experience had shown that, at any rate with existing machinery and existing resources, it was extremely difficult to establish this element of permanence. Mr. Macan moved, "That this conference supports the proposal of State aid to University extension, provided that aid could be given without undue State interference." Mr. Mackinder seconded the resolution, which was carried by an overwhelming majority. In the evening a second conference was held in the Examination Schools, the chair being occupied by Mr. J. G. Talbot, M.P. The subjects discussed included University extension teaching in training colleges, village lectures, students' associations, and University extension teaching in connection with free public libraries.

MR. COSMO NEWBERY, the analyst of the Mines Department, Victoria, has been speaking strongly as to the necessity for a central School of Mines in Victoria. He would like that such a school should, if possible, be established in connection with the Melbourne University. If that proved to be impossible, he would be content with the development of the well-known school at Ballarat. Mr. Newbery's views on the subject are vigorously supported by the *Australian Mining Standard*, which thinks that a central school, thoroughly organized, could not fail "to exercise an important influence in the development of mining science in Australasia."

THE University College of Bristol has issued its Calendar for the session 1890-91.

A "*Bibliothèque Darwinienne*" has recently been started in Paris. The series will deal for the most part with sociological subjects. The first volume is by M. P. Combes, and relates to animal civilizations.

ADVANTAGE is being taken of the Eiffel Tower to obtain high pressure through a manometric tube (the height of the tower) containing mercury. M. Cailletet proposes to utilize the enormous pressure—about 400 atmospheres—for his researches on the liquefaction of gases, and interesting results may be looked for.

WE extract from *La Nature* of July 26 the following facts relating to exceptional seasons in past centuries. They have been collected by M. Villard, of Valence, for France especially, and for Europe generally. In 1282 the winter was so mild that cornflowers were sold in Paris in February. New wine was also drunk at Liége on August 24. In 1408 the winter was so severe that nearly all the Paris bridges were carried away by the ice. Ink froze in the pen, although a fire was in the room. [A similar fact is quoted by Dove as occurring at Sebastopol on December 13, 1855.] All the sea between Norway and Denmark was frozen. The summers of 1473 and 1474 were disastrously hot. In the winter of 1544-45 wine was frozen in barrels all over France. It was cut with hatchets and sold by the pound. In 1572-73 nearly all the rivers were frozen. The Rhone was traversed by carriages at various places. In 1585 the winter was very mild; corn was in ear at Easter, but the third week in May was extremely cold.

THE *Annalen der Hydrographie und Maritimen Meteorologie* for July contains an article by Dr. G. Meyer, on the influence of the moon on weather. Although such investigations have hitherto given a negative result, the author thought that with the materials furnished by synoptic charts he might eliminate local influences, and he gives tables extending over a number of years, which seem to show the influence of the moon in lowering the height of the barometer in the months of September to January, at the time of full moon, and in raising it during the first quarter. The Deutsche Seewarte, which communicates the article, points out that a similar result has been independently arrived at by Captain Seemann, one of the assistants of the institution. The same effect or any other is not perceptible in other months.

AN ingenious contrivance has been recently adopted at the Hippodrome in Paris, with a view to producing scenic effects, in the central oval space, without the spectators opposite being seen at the same time. An elliptical screen of fine steel netting is let down in comparative darkness, so as to be about 12 feet in front of the benches. This is painted on the inner side with a representation of the Place du Vieux Marché at Rouen (the piece being *Jeanne d'Arc*), and, as it is strongly illuminated, at a given moment, from the centre, the light outside being low, a spectator at any point has an excellent view of the scene, while seeing nothing of the crowd beyond.

The additions to the Zoological Society's Gardens during the past week include a Malbrouck Monkey (*Cercopithecus cynosurus* ♂) from West Africa, presented by Miss Florence Schuler; an American Black Bear (*Ursus americanus*) from Canada, presented by Mr. John Sands; a Common Otter (*Lutra vulgaris*) from Ross-shire, presented by the Hon. J. S. Gathorne Hardy, M.P., F.Z.S; two Cape Doves (*Œna capensis*) from South Africa, presented by Miss Grace Debenham; two Imperial Eagles (*Aquila imperialis*) from Spain, presented by Mr. Walter Buck; two Smooth Snakes (*Coronella lævis*) from Hampshire, presented by Mr. E. Penton, F.Z.S; a Hairy Armadillo (*Dasypus villosus*) from La Plata, a Greater Sulphur Crested Cockatoo (*Cacatua galerita*) from Australia, deposited; five Common Peafowls (*Pavo cristatus*), six Ring-necked Pheasants (*Phasianus torquatus*), three Gold Pheasants (*Thaumalea picta*), five Silver Pheasants (*Euplocamus nycthemerus*), seven Californian Quails (*Callipepla californica*), a Vulpine Phalanger (*Phalangista vulpina* ♀) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

Objects for the Spectroscope.

Sidereal Time at Greenwich at 10 p.m. on August 7 = 19h. 5m. 29s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4485	—	—	19 12 17	+29 53
(2) G.C. 4499	—	—	19 26 19	+ 9 0
(3) D.M. + 30° 3409 ...	6	Yellowish-red.	19 0 43	+30 34
(4) ε Aquilæ	4	Yellow.	18 54 36	+14 55
(5) λ Aquilæ	3	Yellowish-white.	19 0 24	− 5 3
(6) 222 Schj.	9	Very red.	18 53 30	+14 13
(7) S Herculis	Var.	Reddish-yellow.	16 46 53	+15 8

Remarks.

(1) This cluster is thus described in the General Catalogue: "A globular cluster; bright; large; irregularly round; gradually very much compressed in the middle; easily resolved." Dr. Huggins has observed that the spectrum is continuous, with "a suspicion of unusual brightness in the middle," but he apparently made no attempt to determine the position of the brightness. Such a maximum of light in one part of the spectrum is suggestive of radiation phenomena, though of course it is possible that it may be simply a contrast effect due to the presence of dark lines or bands. In any case trustworthy measures may give some clue to the constitution of the stars of which the cluster consists.

(2) The G.C. description of this object is as follows: "Considerably bright; small; irregularly round; easily resolvable." It is thus apparently an undoubted cluster, and it is therefore very remarkable that Dr. Huggins records: "I believe that the spectrum consists of one bright line." If this be confirmed, the object must evidently be a cluster of "nebulous stars," and resolvability can no longer be a criterion for non-nebulosity."

(3) Dunér describes the spectrum of this star as a feebly-developed one of Group II.; only the bands 2, 3, and 7 being seen. As the complete series of bands has been recorded in stars of much smaller magnitude with the same instruments, it is clear that there are decided specific differences. A more detailed examination, with special reference to the presence or absence of bright lines or flutings and dark lines, is suggested.

(4 and 5) These are stars of the solar type and of Group IV. respectively. The usual observations are required in each case.

(6) The spectrum of this star is one of Group VI. The dark bands are strong, but the blue zone is very feeble. Further details should be looked for.

(7) This variable has a spectrum of Group II., and the approaching maximum of August 15 may be utilized for ascertaining whether, in common with other variables of the same group, bright lines appear at or near maximum. The magnitude at maximum appears to vary between 6·6 and 7·7, whilst that at minimum is about 11·5, the period being about 408 days. The line of hydrogen at G is apparently the most easily seen in this class of objects. The bright flutings of carbon should also be carefully observed as the star fades. A. Fowler.

Catalogue of Red Stars.—No. V. of the Cunningham Memoirs of the Royal Irish Academy contains a new edition of Birmingham's "Catalogue of Red Stars," by the Rev. T. E. Espin. The work undertaken by Mr. Espin is (1) the observation of such stars of Mr. Birmingham's Catalogue as seemed to merit special attention; (2) a search for new red stars; (3) the spectroscopic observation of all stars not previously observed with the spectroscope. This comprehensive programme was commenced about four years ago, and much important work has been done under each of the heads. The original catalogue contained ruddy and orange stars in addition to those having a decided red colour, but these are now given in a separate list.

In some remarks on the spectroscopic observations of the stars in the Catalogue, Mr. Espin brings forward "one of the most striking examples of the disagreements among spectroscopic observers," viz. the difference between the spectrum of 152 Schjellerup as observed by Secchi and Dr. Huggins. The former observer remarked that the dark zones coincided with the carbon flutings given by an alcohol flame. Dr. Huggins made the comparison, and, either from imperfect instrumental conditions or a different comparison spectrum, found there was no such coincidence, although later observations, by Vogel, Dunér, and others, have established Secchi's view.

A useful list is given of stars with bright lines in their spectra discovered up to the date of publication, and no one has worked more assiduously in this direction than Mr. Espin himself. After an admirable and extended account of the discovery and the spectra of these stars the following conclusions are arrived at:—

(1) That in stars of type I.*c* (Group I.) where the hydrogen lines and D_3 are bright, the lines vary, and this variation is not simultaneous.

(2) That in stars with type III.*c* one or more of the hydrogen lines may be brilliant and the others invisible, as in Mira, where γ and δ were conspicuous, but there was no trace of ε and F.

(3) In the cases of R Andromedæ, R Cygni, and S Cassiopeiæ, the extremely brilliant F line was detected *after* the maximum.

(4) In Vogel's type I.*b*, the hydrogen lines may really be faintly bright, and in one of the stars of this class the existence of other bright lines is proved, and they will hence, probably, be found in others.

It should be remarked that the stars of Group II. which have bright lines in their spectra (*e.g.* Mira Ceta) are classified by Mr. Espin as a new type, III.*c*.

The total number of stars contained in the Catalogue is 1472, of which 766 are given in the red star catalogue, 629 in the list of ruddy stars, and 77 in an addendum. Besides these there are 52 "bright-line" stars. Seven new variables were detected by Mr. Espin during the four years of observation, and he concludes that the work of discovering new red stars in the northern heavens is complete as far as magnitude 8·5. Every spectroscopist appreciates this valuable and important Catalogue, and Mr. Espin is to be congratulated on having been able to complete it in so short a time.

Ancient Eclipses.—In the *Astronomical Journal*, No. 220, Mr. John Stockwell continues his discussion of the secular and long-period inequalities in the moon's motion. The following are the dates of the sixteen eclipses that have been investigated, and some particulars referring to them.

No.		Date.	No.		Date.
1.	A.D.	1140 March 20	9.	B.C.	480 April 19
2.	A.D.	1030 August 30	10.	B.C.	546 October 23
3.	A.D.	364 June 16	11.	B.C.	556 May 19
4.	A.D.	360 August 28	12.	B.C.	584 May 28
5.	A.D.	348 August 29	13.	B.C.	602 May 18
6.	B.C.	309 August 15	14.	B.C.	609 September 30
7.	B.C.	423 March 21	15.	B.C.	762 June 15
8.	B.C.	430 August 3	16.	B.C.	1184 August 28

1. This eclipse is mentioned by Halley, by William of Malmesbury, and in the Saxon Chronicle. It is shown that the line of central eclipse passed over Cambridge.

2. This is the eclipse of Stiklastad, and Mr. Stockwell's computations appear to satisfy the account given by Hansen in vol. ii., p. 388, of his "Darlegung."

3. Observed at Alexandria by Theon.

4. An annular eclipse which occurred before sunrise in any part of Mesopotamia, so that it could not have occasioned the phenomenon mentioned by Ammianus Marcellinus (book xx. chap. 3).

5. This eclipse was total in the eastern parts of Mesopotamia at 9h. 50m., and satisfies the phenomenon described by Ammianus.

6. The eclipse encountered by the fleet of Agathocles while on its voyage from Sicily to Africa.

7. The eclipse described by Thucydides as having occurred during the eighth year of the Peloponnesian War.

8. This eclipse is shown to be identical with that described by Thucydides as having occurred during the first year of the Peloponnesian War, when the darkness was so great that some of the stars were visible.

9. The account given by Aristides ("Scholiast," ed. Frommel, p. 222) of the eclipse which took place while Xerxes was on the march from Sardis to Abydos at the beginning of the Persian War is confirmed by the computations.

10. This is shown to explain the disappearance of the sun described by Xenophon ("Anabasis," Book iii.) as having occurred at Larissa.

11. Contrary to the conclusions of Hansen and Prof. Airy, Mr. Stockwell finds that this eclipse does not satisfy Xenophon's account.

12, 13, and 14. Each of these has been supposed to be Thales's eclipse. Mr. Stockwell finds that both 13 and 14 satisfy equally well the astronomical conditions of the problem, but thinks the former is rather the more probable of the two.

15. The record of this eclipse was discovered on the Assyrian tablets in the British Museum, and the computations show that an eclipse happened at Nineveh at two o'clock in the afternoon on the date given.

16. Homer mentions a singular darkness that occurred during one of the great battles of the last year of the Trojan War ("Iliad," Book xvi.). Mr. Stockwell explains the darkness by means of this total solar eclipse.

Many of the conclusions arrived at with respect to the dates of eclipses differ widely from those generally accepted, and are open to much discussion.

COGGIA'S COMET (*b* 1890).—*Edinburgh Circular* No. 9 contains the following elements and ephemeris, computed by Dr. Berberich, of Berlin, from observations made at Marseilles on July 19, and at Kiel on July 21 and 22. Dr. Berberich finds there must be an error in the comet's place deduced at Marseilles on July 18, the date of discovery. He also points out that the orbit closely resembles that of the comet of A.D. 1580.

Elements of Comet Coggia.

$$T = 1890 \text{ July } 7{\cdot}9775 \text{ Berlin Mean Time.}$$

$$\left.\begin{array}{rl} \pi - \Omega &= 84^\circ\ 20'\ 52'' \\ \Omega &= 14\ \ \ 4\ \ 56 \\ i &= 63\ 28\ 17 \end{array}\right\} \text{Mean Eq. } 1890{\cdot}0.$$

$$\log q = 9{\cdot}88007.$$

Ephemeris for Berlin Midnight.

1890.	R.A. h. m. s.	Decl. ° ′	Log Δ.	Log *r*.	Brightness.
Aug. 7	10 43 24	+28 1·0	0·2526	9·9826	0·50
8	10 47 12	27 10·4			
9	10 50 53	26 20·3	0·2601	9·9929	0·46
10	10 54 27	25 30·7			
11	10 57 55	24 41·7	0·2676	0·0033	0·43
12	11 1 16	23 53·2			
13	11 4 31	23 53	0·2752	0·0137	0·39
14	11 7 41	22 18·0			
15	11 10 45	21 31·2	0·2827	0·0240	0·36

THE INSTITUTION OF MECHANICAL ENGINEERS.

THE annual summer meeting of the Institution of Mechanical Engineers was held last week in Sheffield. There could be no more appropriate centre around which either this Institution, or the sister Society, the Iron and Steel Institute, could gather. Sheffield has, however, of late years been somewhat tardy in offering a welcome to visitors. Six years ago it was proposed that the Iron and Steel Institute should hold a session in Hallamshire, but Hallamshire would not open its doors, and the Iron and Steel Institute had to journey to Chester. It is 29 years since the Mechanical Engineers met in Sheffield, and now, when they once more congregate there, they find but a partial welcome. The fact is, the big Sheffield steel makers—the Browns, Firths, Cammells, Jessops, and Vickers—have always pursued an absurd policy of secrecy. There is as much Abracadabra about these Sheffield steel makers as ever was practised by the alchemists of old. One can walk into the other steel works of the country with no more formality than presenting one's card, and see all that is to be seen; but these Sheffield works remain a sealed book. The reason given for this is that "The Foreigner" comes over here and learns too much, imparting no information in return. Unhappily for the cogency of this argument it is just the foreigner that the steel makers must admit. All those firms who do work for foreign Governments must admit foreign Government inspectors. These men come into the works to stay for months or even years. They are experts in the business they are engaged upon. They come and go where they will, ask what questions they will, make analyses, tests, and experiments at will; in short, they obtain a thorough and complete knowledge of everything that goes on. When they return home they would look on two or three hundred a year as an ample income, or a hundred pound note as a handsome consulting fee.

In the face of such facts is it not childish to shut out the necessary engineer, who simply wants to satisfy his scientific curiosity regarding the chief material he uses?

Although the big steel makers had shut their doors on the Sheffield visitors, there were still some things of interest left. Many of the older class of crucible steel makers were willing to explain the whole process of steel production as introduced by Huntsman one hundred years ago, and indeed were able to give practical illustrations of the same. Steel affords as much food for contemplation to the industrial economist as to the physicist and chemist. That the addition of less than one half of one per cent. of carbon should so entirely change the character of the metal is curious enough, although so familiar; but that the making of crucible cast steel should have stood, as it has, through the last century of industrial change and revolution is still more surprising. Watt, Faraday, and Thomson, nay, even Bessemer and Siemens, have lived and laboured without writing a single record on the process. Crucibles are still made by hand, charged by hand, pulled out of the fire by hand, teemed by hand, and in fact the steam-engine is not called into requisition throughout the process. The steel manufacturer makes no chemical analysis to find the grade of his steel. He breaks a piece, and his eye tells him by the fracture the percentage of carbon nearly enough for all practical purposes; *i.e.* as nearly as his neighbour knows, who does the same. And yet if one wants trustworthy steel of the highest grade one has to go to Sheffield for it, and pay the Sheffielder's price. All the science of all the engineers, chemists, and physicists of the last hundred years, allied with the industrial activity engendered by the fierceness of modern competition—even the mingling of science and commercial acumen, as in the persons of Siemens and Bessemer—has failed to unseat the ancient steel trade of Sheffield. No wonder the grimy town remains the stronghold of industrial empiricism, where they fall down and worship with the prophets of the rule of thumb.

But though the crucible steel maker is conservative in his method of working, he proved liberal in showing his work to others, and the members of the Institution had a good opportunity of seeing the way in which the finer kinds of steel they use are produced. The works of Messrs. Seebohm and Dieckstahll, Samuel Osborn and Co., and many others in which crucible steel making is carried on, were open to inspection; but, had not Park Gate come to the rescue, those who were unacquainted with the Bessemer or Siemens processes would have had to go to South Wales, Glasgow, or the north-east, where they could find works open to their inspection quite as well organized as any they missed seeing at Sheffield.

There were eight papers down for reading during the meeting, the sittings being held on the 29th and 30th ult. in the large hall of Firth College. The President of the Institution, Mr. Joseph Tomlinson, presided throughout. The papers on the agenda were as follows:—

"On Steel Rails, considered chemically and mechanically," by C. P. Sandberg, London.

"On Recent Improvements in the Mechanical Engineering of Coal Mines," by Emerson Bainbridge, of Sheffield.

"Description of the Park Gate Iron and Steel Works," by C. J. Stoddart.

"Description of the Sheffield Water Works," by Edward M. Eaton, Engineer.

"Description of the Loomis Process for making Gas Fuel," by R. N. Oakman, Jun., of London.

"On Milling Cutters," by George Addy, of Sheffield.

"On some Different Forms of Gas Furnaces," by Bernard Dawson.

"On the Elihu Thomson Electric Welding Process," by W. C. Fish, of London.

The first five papers only were read, the other three being adjourned until the next meeting.

Steel rails first occupied the attention of members, Mr. Sandberg opening the business part of the meeting by reading his paper. The author attributed the well-known greater durability of the first Bessemer rails made in Sheffield, to the hammered blooms and slow running mills of early days. There is no doubt that hardness is a virtue in railway lines, and hardness may be obtained by work; but it can also be obtained chemically. By the latter means, however, other desirable features may be jeopardized. In the tables showing results of tests, given as an appendix to the paper, this point was strongly brought out, the amount of phosphorus being, in the case of some Russian rails, exceedingly high, in fact dangerously so in the opinion of some of our best authorities. In dealing with the question of silicon, the author gave a seasonable reminder as to the different composition required for steel which was to be used in bridge and ship work, and that intended for rails. This point was taken up in the discussion, Messrs. Windsor Richards, Jeremiah Head, and others speaking on the question of mechanical tests. Tensile tests were generally pronounced as undesirable, being costly, and of little or no use; the falling weight test, and a test for hardness, together with such light as might be thrown by chemical analysis, being considered sufficient. It may be pointed out, however, that there is no well-established means of testing for hardness. Mr. Wicksteed spoke to the point when he referred to the desirability of ascertaining the percentage of elongation, although, as will be seen, this is not a sure guide. This question leads up to another which arose in the discussion. Some of the rail-makers present exclaimed against engineers insisting on steel containing a given percentage of certain alloys. The engineers have nothing to do with chemical analysis, the metallurgists say; it is a subject they know little or nothing about, and yet they lay down the law to the steel makers, whose business it is. Let the engineers be satisfied with results, and leave to those who understand the question the means of attaining these results. This is very good logic as far as it goes, but unfortunately it is not easy to make tests which will definitely settle the question of practical use. One speaker very well said that he looked on the Metropolitan Railway as the best testing machine for rails in the world; and so long as engineers find that a given chemical analysis gives a durable and safe steel rail, they will be justified in asking for that analysis as supplementary to mechanical tests. Speaking on the latter question, the author says in the paper: "As for tensile tests, they tell us very little; for soft rails broke at only 33 tons per square inch, instead of 41 tons for the good rails; while the brittle rails gave almost the same tensile strength as the good rails, and even more elongation and contraction." Could the transverse test under a falling ball have been substituted for these slow and costly tensile tests, it would have shown better the merits of safe or brittle rails. It may be mentioned in passing that the hardness machine of Prof. Turner, of Mason's College, Birmingham, to which Mr. Hadfield made reference, appears to promise well as a means of determining the second desirable feature in steel rails. By the tables to which reference has been made, it was shown that 0·24 per cent. of silicon in steel rails gave the best results. This the author considers the most striking feature in the analysis.

Mechanical engineering in coal-mines, as described in the contribution of Mr. Emerson Bainbridge, next occupied the attention of the meeting. We do not propose giving an abstract of this paper in the present notice; it would be like trying to run the River Thames through a 12-inch main. The author ranged over the whole field of mining engineering; the illustrations, which were shown by aid of the magic lantern, being more than a hundred in number. This paper had evidently cost the author much trouble and time in its preparation, and was one eminently fitted for the consideration of the members of the Institution of Mechanical Engineers. Mr. Bainbridge is well known in the north as a mining engineer of ability—a fact which it is well to emphasize, as his paper was received by some members, not themselves acquainted with its subject, in a very ungracious spirit. It is to be hoped that the proposal which he made to withdraw the paper from publication in the Transactions will not be carried out.

The papers of Messrs. Sandberg and Bainbridge were the only two taken on the first day of the meeting, the sitting being adjourned about one o'clock for the members to visit the various works open to their inspection.

On the members reassembling on the next day, Wednesday, the 30th ult., the first paper taken was that of Mr. C. J. Stoddart. The author is the managing director of the Park Gate Iron and Steel Works, and in his contribution he dealt with the new plant for steel making lately erected there. Should it ever be necessary to put these works into the market, the paper would form an excellent auctioneer's catalogue, it reading more like a document of that nature than a memoir to be put before a meeting of a scientific or technical Society. There were, however, a few passages of historic interest which we reproduce. These works, which are near Rotherham, were founded in 1823, and here many of the iron rails used on the first railways were rolled; amongst some of the later ones were those for the Metropolitan Railway, many of which were case-hardened. Here, also, were rolled a large part of the iron plates used in the construction of the *Great Eastern*; whilst armour plates were first rolled here also. The latter were presumably for the *Warrior*, as she was our first armour-clad ship, and they were very different from the compound steel and iron plates now so elaborately prepared, being, it will be remembered, no more than 4½ to 5 inches thick. Park Gate has, however, had to abandon these early methods of iron working, and, advancing with the times, has laid down within the last two years a costly steel plant, the outline particulars of which are duly set forth in Mr. Stoddart's catalogue. We are not, however, disposed to quarrel with the author of the paper for not going more fully into particulars, as he was liberal enough, in his capacity as managing director, to invite the meeting to make an excursion to his works on the day following the reading of his paper. The members were therefore enabled to see for themselves the five blast furnaces, plate and sheet mills, bar mills and their appurtenances, four 25-ton Siemens furnaces, cogging mills, slab rolls, billet mills, and plate mills duly set forth in the author's list of plant. The capacity of the steel works is from seven to eight hundred tons of steel and from four to five hundred tons of plates per week.

Mr. Oakman's paper on the Loomis process of making gas fuel was next brought before the meeting. The apparatus in which Loomis gas is made consists mainly of a generator and steam boiler. The generator is not novel in principle, the air being drawn through the charging door in the top, whilst an exhauster is used to set it in motion. The result is producer gas, which is superheated and then led through the boiler to produce steam, finally passing to the gas-holders. This part of the process is carried on for about five or six minutes, after which the admission of air is suspended, and the steam which has been generated is carried through the incandescent fuel, having been previously superheated in the superheater. The second operation produces, of course, water-gas, which, however, has one great advantage over ordinary water-gas, inasmuch as it possesses a strong and characteristic odour. This proceeds from the hydrocarbons taken up from the fuel, a bituminous coal being used. The apparatus has been applied with success in Sheffield, notably by the big steel house of T. Firth and Sons. A representative of that firm stated during the discussion that a saving of at least 50 per cent. in the cost of fuel in the manufacture of crucible steel was made by using Loomis gas, as compared with the old method of melting by coke—a statement which we have no difficulty in accepting when it is remembered how extremely wasteful is the present usual method of firing.

The discussion which followed the reading of the paper soon fell into the familiar groove which seems to have become stereotyped for use whenever the question of gas fuel comes to the fore. Mr. John Head and Sir Lowthian Bell both spoke. The former naturally soon brought the subject round to the Siemens furnace; upon the merits of which he was speaking

—especially in the matter of cheapness—when he was stopped by the President. The practice Mr. Head follows at meetings of the scientific and technical Societies is not tending to enhance the respect felt for the once honoured name of Siemens. Sir Lowthian Bell said what he said in Paris last year over again. The position he takes up—that no more heat can be got from a pound of fuel than Nature put in it—is perfectly sound, but there is no need to repeat the truism at such great length and so often.

The Sheffield Water Works was the subject of the last paper read at the meeting.

On the whole, it cannot be doubted that the meeting of the Mechanical Engineers at Sheffield was below the average, and badly managed. If Mr. Eaton's paper on the water-works had been taken as read, and Mr. Addy's contribution on milling cutters had been brought forward, the meeting might have done something to redeem its character as a representative assemblage of Mechanical Engineers. The Catalogue of the Park Gate Iron and Steel Works might also have been taken as read. Both the latter and the water-works paper were acceptable as guides to the respective excursions, but that was no reason why members should be required to sit and listen whilst Mr. Bache read through them at a speed which rendered it quite impossible to follow.

We have not space to refer to the visits to works in Sheffield open to visitors, and indeed there was not much of exceptional interest. Exception must be made, however, to a loom for weaving horse-hair cloth, which was seen at the works of Laycock and Sons. The wonderful ingenuity displayed by designers of textile machinery appears here to have reached its culminating point. Horse-hair has several undesirable features from a textile point of view. The filaments are generally no longer than 3 or 4 feet; though exceptional hairs have been known as long as 6 feet, we believe. The thickness differs considerably at each end; the material is very elastic, and it is so hard that it will speedily wear away the hardest steel over which it may be dragged. In order to overcome these difficulties, the designer of this loom, Mr. W. S. Laycock, has introduced a shuttle with jaws that take hold of each hair as it is presented, and a device which is known as the selector. The latter is a hand—for we can call it nothing less—which picks up one hair, and only one, to present to the jaws of the shuttle. It has to let go at the very instant the shuttle takes hold, otherwise the hair would be dragged through its fingers, which would soon be worn away. Sometimes, however, the fingers fail to grasp this single hair; it must be remembered if it were to take two hairs the cloth would be spoiled. It then makes a second try, and, if the second fail, yet a third. Supposing the third attempt also prove unsuccessful, there being no time to make a fourth, the selector promptly stops the weft motion, so that no change takes place whilst the shuttle is making its traverse without a hair to form the weft. Theophrastus Such, after a visit to a textile factory, had a nightmare, in which mechanism usurped the place of humanity, and became the inexorable master of mankind. The conceit is worked out with much skill, and appears quite plausible when viewed in the light of mechanism which not only performs the most delicate operations, but knows when it misses, tries again as long as trying is of avail, and, if it fail at last, takes steps to prevent mischief following.

ON THE ORIGIN OF THE DEEP TROUGHS OF THE OCEANIC DEPRESSION: ARE ANY OF VOLCANIC ORIGIN?[1]

THE consideration of the question with regard to the origin of the ocean's deep troughs requires, as the first step, a general review of oceanic topography; for according to recent bathymetric investigations, the deep troughs are part of the system of topography, and its grander part. We need, for this purpose, an accurate map of the depths and heights through all the great area. Such a map will ultimately be made through the combined services of the Hydrographic Departments of the civilized nations. At the present time the lines of soundings over the oceans, especially over the Pacific and Indian, are few, and only some general conclusions are attainable. It is to be noticed that the system of features of the oceanic area are involved in the more general terrestrial system; but since the former comprises nearly three-fourths of the surface of the sphere, it is not a subordinate part in that system.

With reference to this discussion of the subject I have prepared the accompanying bathymetric map.

I. THE BATHYMETRIC MAP, AND THE GENERAL FEATURES OF THE OCEANIC DEPRESSION DISPLAYED BY IT.

1. *The Map.*—In the preparation of the bathymetric map I have used the recent charts of the Hydrographic Departments of the United States and Great Britain,[1] which contain all depths to date, and the lists of new soundings published in German and other geographical journals. In order that the facts on which the bathymetric lines are based may be before the reader a large part of the depths are given, but in an abbreviated form, 100 fathoms being made the unit: 25 signifying 2500 fathoms or nearly (between 2460 and 2550); 2·3, about 230 fathoms, ·4, about 40 fathoms. Only for some deep points is the depth given in full. The addition of a plus sign (+) signifies no bottom reached by the sounding.[2]

In the plotting of oceanic bathymetric lines from the few lines of soundings that have been made, the doubts which constantly rise have to be settled largely by a reference to the general features of the ocean, and here wide differences in judgment may exist in the use of the same facts; but through the depths stated on the map, the reader has the means of judging for himself. In the case of an island the lines about it may often have their courses determined by those of adjoining groups, or by its own trend; but in very many cases new soundings are needed for a satisfactory conclusion.

Some divergences on the map from other published bathymetric maps require a word of explanation. The northern half of the North Pacific is made, on other deep-sea maps, part of a great 3000-fathom area (between 3000 and 4000) stretching from the long and deep trough near Japan far enough eastward to include the soundings of 3000 fathoms and over in mid-ocean along the 35th parallel. It has seemed more reasonable, in view of present knowledge from soundings, to confine the deep-sea area off Japan to the border-region of the ocean, near the Kurile and Aleutian Islands, and leave the area in mid-ocean to be enlarged as more soundings shall be obtained. Again, in the South Pacific, west of Patagonia, the area of relatively shallow soundings (under 2000 fathoms) extending out from the coast, is on other maps bent southward at its outer western limit so as to include the area of similar soundings on the parallels of 40° and 50°, between 112° and 122° W. The prevailing trends of the ocean are opposed to such a bend, and more soundings are thought to be necessary before adopting it.

It may be added here that in the Antarctic Atlantic, about the parallel of 66½° S. and the meridian of 13½° W., a large area of 3000 and 4000 fathoms has been located. It was based, as I have learned from the Hydrographic Department of the British Admiralty, on a sounding in 1842 by Captain Ross, R.N., in which the lead ran out 4000 fathoms without finding bottom. The sounding was, therefore, made before the means available were "sufficient to insure the accuracy of such deep casts."[3]

2. *The Feature-lines of the Oceanic and Bordering Lands.*—The courses of island-ranges and coast-lines have a bearing on the question relating to the courses of the deep-sea troughs, and

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[1] This paper is accompanied in the *American Journal of Science* from which it is reprinted, by a bathymetric map.

[1] I am indebted to the Hydrographic Departments of Great Britain as well as the United States for copies of these charts.

[2] On the map the bathymetric lines for 1000, 2000, 3000, and 4000 fathoms, besides being distinguished in the usual way by number of dots, have been made to differ in breadth of line, the deeper being made quite heavy in order to exhibit plainly the positions of the areas without the use of colours. The line for 100 fathoms is, as usual, a simple dotted line. As the bathymetric map herewith published is necessarily small, and none of the ordinary maps of the oceans give either deep-sea soundings or a correct idea of the trends of the oceanic ranges of islands, I state here that the charts of the U.S. Hydrographic Department for the Atlantic, Pacific, Indian, and Arctic Oceans may be purchased of dealers in charts in the larger sea-board cities for 50 cents a sheet and less according to size. (There are several large charts to each ocean.) One of the firms selling them in New York City is that of T. S. and J. D. Negus, 140 Water Street. The British Admiralty have published a map of the Pacific with its soundings on a single sheet, and for the Atlantic and Indian Oceans with part of the Pacific, besides charts of the Antarctic and Arctic seas. The occasional Bulletins from the Hydrographic Department and *Petermann's Mitteilungen* contain nearly all the new data issued for the perfecting of such a chart.

[3] The communication received from the Admiralty Office adds that "Some of Ross's soundings up to 2660 fathoms have been proved correct, and hence the sounding in 68° S., referred to, has been retained on our charts until disproved." "Another sounding obtained by Ross in the Atlantic has had strong doubts thrown upon it by a sounding of 3000 fathoms obtained not very far from its position." See the accompanying map, near latitude 14° S.

therefore, by way of introduction, they are here briefly reviewed.[1] The system of trends in feature-lines takes new significance from a bathymetric map, for the courses are no longer mere trends of islands or emerged mountain peaks; they are the trends of the great mountain ranges themselves; and, in the Pacific, these mountain courses are those of half a hemisphere. Some of the deductions from such a map are briefly as follows:—

(1) Over the Pacific area there are *no* prominent north-and-south, or meridional, courses in its ranges, and none over the Atlantic, except the axial range of relatively shallow water in the South Atlantic. And to this statement it may pertinently be added that there are none in the great ranges of Asia and Europe, excepting the Urals; none in North America; none in South America, excepting a part of those on its west side.

(2) The ranges in the Pacific Ocean have a mean trend of not far from north-west-by-west, which is the course very nearly of the longer diameter of the ocean. One *transverse* range crosses the middle South Pacific—the New Zealand—commencing to the south in New Zealand and the islands south of it, with the course N. 35° E., and continuing through the Kermadec Islands and the Tonga group, the latter trending about N. 22° E., and this is the nearest to north and south in the ocean, except toward its western border.

(3) The oceanic ranges are rarely straight, but, instead, change gradually in trend through a large curve or a series of curves. For example, the chain of the central Pacific becomes, to the westward, north-north-west; and the Aleutian range and others off the Asiatic coast make a series of consecutive curves. Curves are the rule rather than the exception. Moreover, the intersections of crossing ranges, curved or not, are in general nearly rectangular.

(4) Approximate parallelisms exist between the distant ranges or feature-lines; as (1) between the trend of the New Zealand range and that of the east coast of North America; and also that of South America (which is continued across the ocean to Scandinavia); also (2) between the trend of the foot of the New Zealand boot with the Louisiade group and New Guinea farther west, and the mean trend of the islands of the central Pacific both south and north of the equator, and also that of the north shore of South America. These are a few examples out of many to be observed on the map.

(5) The relatively shallow-water area which stretches across the North Atlantic from Scandinavia to Greenland—the Scandinavian plateau, as it may well be called—is continued from these high latitude seas south-westward, in the direction of the axis of the North Atlantic (or parallel nearly to the coast of eastern North America and the opposite coast of Africa), and becomes the "Dolphin Shoal."

It may be a correlate fact in the earth's system of features that a Patagonian plateau stretches out from the Patagonia coast, or from high southern latitudes, in the direction of the longer axis of the Pacific, and embraces the Paumotu and other archipelagos beyond.[2]

The above review of the earth's physiognomy, if accompanied by a survey of the map, may suffice for the main purpose here in view: to illustrate the general truths—that system in the feature-lines is a fact; that the system is world-wide in its scope; and —since these feature-lines have been successively developed with the progress of geological history—that the system had its foundation in the beginning of the earth's genesis and was developed to full completion with its growth.

II. FACTS BEARING ON THE ORIGIN OF THE DEEP-SEA TROUGHS.

In treating of this subject, the facts from the vicinity of volcanic lands that favour a volcanic origin are *first* mentioned; *secondly*, those from similar regions that are not favourable to such an origin; *thirdly*, facts from other regions bearing on the question.

A. *Facts apparently favouring a Volcanic Origin.*

1. The Pacific soundings have made known the existence of two deep-sea depressions, if not a continuous trough, *within forty miles of the Hawaiian Islands;* one situated to the north-east of Oahu, or, north of Molokai, with a depth of 3023 fathoms, or 18,069 feet, and the other east of the east point of Hawaii, 2875 fathoms, or within 750 feet of 18,000 feet. Again, 450 miles north-east of Oahu, there is a trough in the ocean's bottom, over 800 miles long, which runs nearly parallel with the group and has a depth of 3000 to 3540 fathoms; and, as far south, another similar trough of probably greater length has afforded soundings of 3000 to 3100 fathoms. The depths about the more western part of the Hawaiian chain of islands have not yet been ascertained, and hence the limits of the deep areas are not known. Such depths, so close to a line of great volcanic mountains, the loftiest of the mountains not yet extinct, appear as if they might have resulted from a subsidence consequent on the volcanic action.

The subsidence might have taken place (1) either from underminings—which the amount of matter thrown out and now constituting the mountain chain, with its peaks of 20,000 to 30,000 feet above the sea-bottom, shows may be large; or (2) from the gravitational pressure in the earth's crust, about a volcanic region which speculation makes a source of the ascensive force and of the upward rising of the lavas, the subsiding crust following down the liquid surface beneath. In either case the mass of ejected material might be a measure more or less perfectly of the maximum amount of subsidence.

2. In the western part of the North Pacific, at the south end of the volcanic group of the Ladrones off the largest island of the group, Guam, the *Challenger* found a depth of 4475 fathoms, one of the two deepest spots yet known in the Pacific. The situation with reference to the group is like that off the east end of the Hawaiian group.

3. East of Japan and the Kuriles, a region of ranges of volcanoes, there is the longest and deepest trough of the ocean, the length 1800 miles, the depths 4000 to 4650 fathoms; and farther north-east, south of one of the Aleutian Islands, a depth of 4000 fathoms occurs again; and depths of 3100 to 3664 fathoms also still farther east. It is probable that the 4000-line trough continues from the Kurile to this deep spot off the Aleutian volcanic range; and if so, the length of the trough is over 2500 miles. The map is made to suggest its extension still farther eastward; but among the very few soundings made, none below 3664 fathoms have yet been obtained off the more eastern Aleutians.

Other similar facts may be found on the map; and still others may exist which are not now manifest owing to the sinking of oceanic areas and islands. But no cases can be pointed to which are more decisively in favour of volcanic origin.

B. *Facts from the Vicinity of Volcanic Regions apparently not referable to a Volcanic Origin.*

The ocean off the western border of North and South America affords striking examples of the absence of deep troughs from the vicinity of regions eminently volcanic. The South American volcanoes are many and lofty; and still the ocean adjoining is mostly between 2000 and 2700 fathoms in depth; and just south of Valparaiso, it shallows to 1325 fathoms. The only exception yet observed is that of a short trough of 3000 to 3368 fathoms close by the Peruvian shore. It may, however, prove to be a long trough, although certainly stopping short of Valparaiso. The waters, however of the Pacific border of America deepen abruptly compared with those of the Atlantic border; and the significance of this fact deserves consideration.

The facts off Central America are more remarkable than those off the coast to the south. The volcanoes are quite near to the Pacific coast, and still the depths are between 1500 and 2500 fathoms.

The condition is the same off the west coast of North America. Of the two areas of 3000 and over, nearest to the east coast of the North Pacific, one is 600 miles distant in the latitude of San Francisco, and the other is within 10° of the equator and 20° of the coast; both too far away to be a consequence of volcanic action in California, Mexico, or Central America.

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[1] This subject of the system in the earth's feature-lines is presented at length, with a map, in my Expedition Geological Report, pp. 11–23 and 414–424; and also more briefly in the *American Journal of Science*, II. ii. 381, 1846.

[2] As parallelisms may have importance that is not now apparent, I draw attention to one between the Mediterranean Sea that divides Europe from Africa, and the West India (or West Mediterranean) sea that divides North from South America. Both have an *eastern*, *middle*, and *western* deep basin. Their depths (see map) in the East Mediterranenn, are 2170, 2040, and 1585 fathoms; in the West Mediterranean (the three being the Caribbean, the West Caribbean or Cuban, and the Gulf of Mexico), 2804, 3428, and 2080 fathoms. Further, in each Mediterranean Sea, a shallow-water plateau extends from a prominent point on the south side, northward, to islands between the eastern and middle of the deep basins; one from the north-east angle of Tunis to Sicily, the other from the north-east angle of Honduras to Jamaica and Haiti, the two about the same in range of depth of water. And this last parallelism has its parallels through geological history, even to the Quaternary, when the great Mammals made migrations to the islands in each *from the continent to the south.*

In the North Atlantic the European side has its volcanoes, and has had them since the Silurian era, and yet the non-volcanic North American side of the ocean has far the larger areas of deep water and much greater mean depth. The Azores or Western Islands, which are all volcanic, have depths around them of only 1000 to 2000 fathoms, and no local troughs. Iceland, the land of Hecla, is in still shallower waters, with no evidence of local depressions off its shores. The Canaries are volcanic, but no deep trough is near them.

C. *Facts from Regions not Volcanic which are unfavourable to the idea of a Volcanic Origin.*

1. In the North Pacific, near its centre, the area of 3000 or more fathoms about 35° N.; the two similar but smaller areas toward its eastern border; the areas north of the Carolines in the western part of the ocean; the broad equatorial area about the Phœnix group; the area in the South Pacific in 170° W., east of Chatham Island, and another just south of Australia, are all so situated that no reason is apparent for referring them to a volcanic origin. Some of the areas are in the coral island latitudes, and the supposed volcanic basis of coral islands makes a volcanic origin possible, but their probable size and position appears to favour the idea of origin through some more fundamental cause. The area in the South Pacific, east of Chatham Island, is 450 miles distant from the land. The border of southern Australia, abreast of the deep-sea trough, has no known volcano.

2. *In the Atlantic, away from the West Indies.*—The 3000-fathom areas of the North and South Atlantic—that is, the three in the North Atlantic, the two in the South Atlantic, and the two equatorial, one near the coast of Guinea and the other near that of South America—occupy positions that suggest no relation to volcanic conditions. The Cape Verdes, north of the equator, are partly encircled by one of the deep areas, somewhat like the eastern end of the Hawaiian group; but this bathymetric area appears to be too large to owe its origin directly to volcanic work in the group. The coast of Guinea near the 3000-fathom area has nothing volcanic about it, and the opposite coast of South America, near another, is free from volcanoes.

The only facts in the Atlantic that suggest a volcanic origin are the depression of 2445 fathoms within 40 miles of the west side of the volcanic Cape Verde archipelago, and that of 2060 fathoms within 20 miles of Ascension Island; and a connection is possible.

3. *In and near the West Indies.*—The most remarkable of the depths of the Atlantic area are situated in and near the region of the West Indies, as is well illustrated and discussed by Mr. Alexander Agassiz in his instructive work on the "Three Cruises of the *Blake*." The deepest trough of the ocean, 4561 fathoms, occurs within seventy miles of Porto Rico; and yet this island has no great volcanic mountain, though having basaltic rocks. By the north side of the Bahama belt of coral reefs and islands, for 600 miles, as Mr. Agassiz well illustrates, the depth becomes 2700 to 3000 fathoms within twenty miles of the coast-line, and at one point 2990 within twelve miles, a pitch-down of 1 : 3·5; and nothing suggests a volcanic cause for the abrupt descent. Cuba and Hayti are not volcanic, and look as if they were an extension of Florida, so that no grounds exist for assuming that the Bahamas rest on volcanic summits.

One of the strangest of 3000-fathom troughs is that which commences off the south shore of Eastern Cuba, having there a depth of 3000 to 3180 fathoms. It is within 20 miles of this non-volcanic shore, and nearly three times this distance from Jamaica. No sufficient reason appears at present for pronouncing its origin volcanic. It is continued in a west-by-south direction to a point beyond the meridian of 85° W., or over 700 miles, making it a very long trough, and the depths vary from 2700 to 3428 fathoms. The depression extends on into the Gulf of Honduras, carrying a depth of 2000 fathoms far toward its head, and in a small indentation of the coast it stops; for nothing of it appears in the outline of the Pacific coast or the depths off it, and nothing in the range of volcanic mountains on the coast. Against the three deepest parts of the trough there are, *first*, the Grand Cayman Reef, 20 miles north of a spot 3428 fathoms deep; *second*, banks in 13 and 15 fathoms within 15 miles of a depth of 2982 fathoms; and *third*, Swan Island Reef, 15 miles south of a depth of 3010 fathoms; the first of the three indicating a slope to the bottom of 1 : 5, and the last of 1 : 4·4. Why these greatest depths in the trough, so abrupt in depression, should be on one side of shoals or emerged coral reefs, it is not easy to explain; and the more so that the part of the trough south of Cuba has nothing volcanic near by in the adjoining mountain range, and the fact also that the westernmost end of the trough extends on for 175 miles, and there has a depth of 3048 fathoms, with 2000 fathoms either side and no coral reefs.

D. *Arrangement of the Deep-sea Troughs in the two halves of the Oceans, pointing to some other than a Volcanic Origin.*

The *western* half of the Atlantic and Pacific oceans contains much the larger part of the 3000-fathom areas and all the depths over 4000 fathoms. In the North Atlantic the areas of 3000 and over in the western half, or off the United States, are very large; and the bathymetric line of 2500 fathoms extends westward nearly to the 1000-fathom line. This important feature can be appreciated for both oceans from a look at the map without special explanations.

As a partial consequence of this arrangement, the Pacific, viewed as a whole, may be said to have a westward slope in its bottom, or from the South American coast toward Japan. This westward slope of the bottom exists even in the area between New Zealand and Australia—the ocean in this area being shallow for a long distance out on the east side and deepening to 2500–2700 fathoms close to that non-volcanic land, New South Wales, in eastern Australia. In the Atlantic, the slope is in the direction of its north-east–south-west axis, either side of the Dolphin Shoal, but especially the western side, rather than from east to west, it commencing in the Scandinavian plateau and ending in the great depths adjoining the West Indies.

Owing to the system in the Atlantic topography, the Dolphin Shoal—the site of the *Atlantis* of ancient and modern fable—is really an appendage to the eastern continent, that is to Europe, and is shut off by wide abyssal seas from the lands to the west that have been supposed to need its gravel for rock-making.

But the view that the west half of an oceanic basin is always the deepest becomes checked by finding in the Indian Ocean that the only areas that are 3000 fathoms deep or over are in the *eastern* part of the ocean and off the north-west coast of Australia, and near western Java and Sumatra. The greatest depths in its western half or toward Africa, are 2400 to 2600 fathoms.[1]

III. CONCLUSIONS.

1. The facts reviewed lead far away from the idea that volcanic action has been predominant in determining the position of the deep-sea troughs. It has probably occasioned some deep depressions within a score or two of miles of the centre of activity, but beyond this the great depths have probably had some other origin.

2. It is further evident that the deep-sea troughs are not a result of superficial causes of trough-making. Erosion over the ocean's bottom cannot excavate isolated troughs. The coldest water of the ocean stands in the deep holes or troughs instead of running, as the reader of Agassiz's volume has learned.

The superficial operation of weighting the earth's crust with sediment, or with coral or other organic-made limestone, and filling the depressions as fast as made, much appealed to in explanations of subsidence, has not produced the troughs; for filled depressions are not the kind under consideration. Moreover, the areas are out of the reach of continental sediments and too large and deep to come within the range of possibilities of organic sedimentation or accumulation. The existence of the troughs is sufficient proof of this. The deep troughs of the West Indian and adjoining seas are in a region of abundant pelagic and sea-border life, and yet the marvellous depths exist. And the depths of the open oceans are no less without explanation. Those close by the Bahamas, extending down to 16,000 and 18,000 feet, are evidence of great subsidence from some cause; and the coral reefs for some reason have manifestly kept themselves at the surface in spite of it.[2]

3. If superficially acting causes are insufficient, we are led to look deeper, to the sources of the earth's energies, or its interior

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[1] In the Arctic seas, going north from the Scandinavian plateau, the water deepens north of the latitude of Iceland, between Greenland and Spitzbergen, to 2000 fathoms, and farther north to 2650 fathoms, in the latitude nearly of Greenwich; and it is probable that the 2000-fathom area extends over the region of the North Pole. The continents of Europe (with Asia probably) and North America are proved by the shallow soundings over the adjoining Arctic seas and the islands or emerged land, to extend to about 82½° N., which is about 450 miles from the Pole.

[2] The migrations from South America alluded to in note [2] on page 358, proving an elevation of 2000 feet to make it possible, prove also that a large part of the West India seas *afterward* suffered subsidence in the Quaternary. How far the Bahama and Florida region participated in the subsidence is not known. That it did not participate in it has not been proved.

agencies of development, to which the comprehensive system in its structure and physiognomy points. Whatever there is of system in the greater feature-lines, whether marked in troughs or in mountain chains, or island ranges, must come primarily from systematic work within. The work may have been manifested in long lines of flexures or fractures as steps in the process, but the conditions which gave directions to the lines left them subject to local causes of variation, and between the two agencies, the resulting physiognomy has been evolved.

We have from the Pacific area one observation of a volcanic nature bearing on the comprehensiveness of the system of feature lines in the oceans, and although I have already referred to it, I here reproduce the facts for use in this place.

If the ranges of volcanic islands were, in their origin, lines of fissures as a result of comprehensive movements, the lines should continue to be the courses of planes of weakness in the earth's crust. The New Zealand line, including the Kermadec Islands and the Tongan group, has been pointed to as one of these lines, and one of great prominence, since it is the chief north-eastward range of the broad Pacific, and nearly axial to the ocean. The series of volcanoes along the axis of New Zealand is in the same line. It was noticed, at the Tarawera eruption of 1883, that *four or five days after* the outbreak, and three after it had subsided, White Island, in the Bay of Plenty, at the north end of the New Zealand series, became unusually active; and *two months later* there was a violent eruption in the Tonga group, on the Island of Niuafou. The close relation in time of the latter to the New Zealand eruption is referred to by Mr. C. Trotter, in NATURE of December 7, 1886.[1] May it not be that these disturbances were due to a slight shifting or movement along a series of old planes of fractures, taking place successively from south to north; and, hence, that even now changes of level may take place through the same comprehensive cause that determined the existence of the earth's feature lines? Owing to the long distance of the Tonga group from New Zealand an affirmative reply to the question cannot be positively made. But there is probability enough to give great interest to this branch of geological enquiry. JAMES D. DANA.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, July 28.—M. Hermite in the chair.—Aquatic locomotion studied by photo-chronography, by M. Marey. The author has made similar investigations on animal locomotion to those of Mr. Muybridge, but with different apparatus. A single camera, the sensitive plate of which takes the form of an endless band moving past the focus of the lens, has been used in the investigations, and appears to possess many advantages over the multiple camera system. The contractions and dilatations of the body of the medusa, the undulations of the lateral fins of the ray, and the rapid movements of the dorsal fin of the *Hippocampus* (sea-horse), have all been analyzed, and in the zoëtrope the successive photographs appear to have reproduced the motions to perfection.—Observations, orbit, and ephemeris of the comet discovered by M. Coggia (*b* 1890) at Marseilles Observatory, by M. Stephan.—On the observation of the annular eclipse of the sun of June 17, by M. A. de la Baume Pluvinel. A detailed description of the instruments employed by the author for his observations in Canea (Island of Crete) is given. As previously noted (NATURE, July 10), the results give further support to the view that the oxygen absorption bands in the solar spectrum are of telluric origin.—Observations of the minor planet recently discovered by M. Charlois (294), made with the *coudé* equatorial and the Foucault telescope at Algiers Observatory, by MM. Rambaud and Sy. Some observations of position and comparison stars are given.—Observations of Coggia's comet, made with the great equatorial of Bordeaux Observatory, by MM. Picart and Courty.—Observations of the same comet made at Paris Observatory, by Mdlle. D. Klumpke.—On a new method of exposition of the theory of theta functions, and on an elementary theorem relative to hyperelliptic functions of the first dimension, by M. F. Caspary. It is shown that the fifteen hyperelliptic functions of the first dimension are proportional to the fifteen elements of an orthogonal system.—Earthquakes in Madagascar, by M. R. P. Colin, Director of the Antananarivo Observatory. The five earth-tremors observed this year appear to have had an influence on the azimuth error of the transit instrument.—On the water of crystallization of neutral sulphate of alumina; analysis of a natural product, by M. P. Marguerite-Delacharlonny. The analysis of two samples of definitely crystallized natural sulphate of alumina from Bolivia supports the author's previous conclusion that its formula should be written with sixteen instead of eighteen molecules of water of crystallization.—On the optical rotatory power of camphor in solution in various oils, by M. P. Chabot. The author finds that the rotation produced by the solutions is sensibly proportional to their strengths, and that, after allowing for the slight rotation due to the oil, the calculated molecular rotatory power of camphor is practically constant.—On the malonates of lithia and on the malonate of silver, by M. G. Massol. Some experiments on the heats of formation are given.—Researches on the optical dispersion of organic compounds; fatty acids, by MM. Ph. Barbier and L. Roux. The authors have examined the normal fatty acids from formic to pelargonic as well as isobutyric and isovaleric acids, and find that the specific dispersive powers increase with the molecular complexity, and that those of isomeric acids are practically equal, though the normal acids have slightly the higher value.—On the presence of furfural in commercial alcohols, by M. L. Lindet.—Contribution to the study of artificial musk, by M. Albert Baur.—Mode of action of bacterial secretions on the vasomotor nervous system; connection between these phenomena and diapedesis, by MM. A. Charrin and E. Gley.—Does hæmoglobin exist in the blood as a homogeneous substance?, by M. Christian Bohr.—On the identity of structure of the central nervous system of Pelecypoda and other Mollusca, by M. Paul Pelseneer.—On the bathymetric distribution of the deep-sea Brachiopods collected in the *Travailleur* and *Talisman* expeditions, by MM. P. Fischer and D. P. Œhlert.—On the position in the plant of the compounds which produce the sulphuretted essential oils of the Cruciferæ, by M. Léon Guignard.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Theory of Light: T. Preston (Macmillan).—Weather Forecasting of the British Isles: Captain H. Toynbee (Stanford).—Psychology: M. Maher (Longmans).—Geometrical Conics, Part 1: Rev. J. J. Milne and R. F. Davis (Macmillan).—Text-book of Mechanics: T. W. Wright (New York, Van Nostrand).—Sap: Does it rise from the Roots? J. A. Reeves (Kenning).—The History of Federal and State Aid to Higher Education in the United States: Dr. F. W. Blackmar (Washington).—Proceedings of the Department of Superintendence of the National Educational Association at its Meeting in Washington, March 6 to 8, 1889 (Washington).

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[1] *American Journal of Science*, III., xxxiii., 311.