THURSDAY, MAY 10, 1894.

THE STUDY OF ANIMAL VARIATION.

Materials for the Study of Variation, treated with especial regard to Discontinuity in the Origin of Species. By W. Bateson, M.A., Fellow of St. John's College, Cambridge. (London: Macmillan and Co., 1894.)

MR. BATESON is to be congratulated on the completion of the first part of his work on variation in animals, which treats of variation in the number and position of organs forming parts of linear or symmetrical series. Variation in the structure of such organs is only partially dealt with, a fuller account being promised in a future volume. The present work may be divided into two parts—one purely descriptive, the other critical. These require separate notice.

It is impossible in a short space to give a proper account of the information brought together in the purely descriptive part of the book. Mr. Bateson has carefully examined many of the principal European collections, both public and private, and has in other ways collected a great store of original matter, which alone would make a respectable volume. In addition, he has compiled a series of abstracts, containing the essentials of a large number of records made by others. The labour expended upon the work of compilation alone may be gathered from the list of authors referred to, which contains some six hundred names.

The facts recorded are grouped in numbered paragraphs, on a system which makes reference to individual cases easy; the descriptions are for the most part admirable, and they are supplemented, where necessary, by adequate woodcuts. Full references are given, either to the actual specimens described, where such reference is possible, or to the source from which descriptions are quoted.

The first twelve chapters deal with organs forming parts of linear series—such as the ribs, vertebræ, or teeth of vertebrates, and similarly "repeated" structures in other animals. In these chapters special attention is drawn to cases of variation, such as the assumption by a cervical vertebra of the characters proper to a dorsal vertebra; and many remarkable examples of analogous phenomena are given. For variation of this kind, in which an organ in one region of the body is "made like" a serially homologous organ in an adjacent region, the convenient word "homœosis" is proposed. Another interesting group of cases is described as showing that the number of specialised organs in a series may be altered by a process of actual division, such as that by which an increase in the number of eyes is effected in Planarians.

The thirteenth and fourteenth chapters deal with increase and reduction in the number of digits in the vertebrate limb, and should be read in connection with the twentieth and twenty-first chapters, where closely analogous phenomena, leading to duplication of arthropod appendages, are described. The wonderful relations of symmetry, which are shown to hold in so many cases between the "normal" and the "extra" limbs, have been shortly described by Mr. Bateson on previous occasions; but the fuller statement here given forms perhaps the most interesting portion of the book.

The remaining chapters deal with variations in radial series, such as those formed by many organs of cœlenterates and echinoderms, and with cases which involve the doubling of structures normally single, or the fusion in the middle line of organs which are normally bilateral and paired.

Such in bare outline is the subject-matter of the descriptive portion of the book. No quotation of isolated passages is attempted, because no such proceeding could give an adequate idea of its importance. The whole work must be carefully read by every serious student; and there can be no question of its great and permanent value, as a contribution to our knowledge of a particular class of variations, and as a stimulus to further work in a department of knowledge which is too much neglected. It is to be hoped that Mr. Bateson will not rest content with his already great achievement, but will proceed with his promised second volume, which will be eagerly looked for by those who read the first.

If the criticism and enunciation of opinions had been performed with the same care as the collection of facts, the commentary which runs through the book would have gained in value, and several inaccuracies, due partly to want of acquaintance with the history of the subject, would have been avoided. The only contention which can here be noticed is that alluded to on the title-page, namely that variation frequently proceeds in such a way that changes in an organ occur only by steps of definite and measurable magnitude; and that discontinuous variation of this kind is necessary for the evolution of new species. In Mr. Bateson's words:

"The first question which the study of variation may be expected to answer, relates to the origin of that discontinuity of which species is the objective expression. Such discontinuity is not in the environment; may it not, then, be in the living thing itself?"

The statement that discontinuity is not in the environment, is justified as follows:

"Here then we meet with the difficulty that diverse environments often shade into each other insensibly, and form a continuous series, whereas the specific forms of life which are subject to them on the whole form a discontinuous series. . . . Temperature, altitude, depth of water, salinity, in fact most of the elements which make up the physical environment are continuous in their gradations," and so on.

Here the reference is only to the physical conditions which form a part of the environment affecting animals. That these physical conditions do often form a "continuous series" is no doubt true, although it is also true that in a large number of cases they do not. But Darwin, Wallace, and the greater number of subsequent writers on the doctrine of natural selection, agree in believing that the most important part of the environment against which a species has to contend consists of other living things. This view is dismissed in the following short foot-note:

"It may be objected that to any organism the other organisms coexisting with it are as serious a factor of the environment as the strictly physical components; and that inasmuch as these coexisting organisms are discontinuous species, the element of discontinuity may thus be introduced. This is true, but it does not help in the attempt to find the cause of the original discontinuity of the coexisting organisms."

Now since the deposition of the earliest palæozoic rocks animals have demonstrably been surrounded by such "discontinuous" organic environment. The statement, that the environmental conditions "form a continuous series," is therefore untrue of all animals known to us. If it has ever been true, we cannot know. The question, whether the first living things which appeared upon the earth were alike or not, is as unprofitable as speculation about the beginning or the ending of any part of the order of things from which our experience is derived must always be.

These preliminary arguments in favour of Mr. Bateson's main contention therefore fail, when applied to any part of the process of evolution of which we can know anything. It remains to consider what experimental evidence is brought forward to prove that variation is in fact "discontinuous" in any living animals.

No definition of what exactly is meant by "discontinuous" variation is given; and the conception adopted is difficult to grasp, since such domestic animals as the bull-dog are taken as examples of its occurrence. It will therefore be necessary to examine the treatment of some special group of variations: and for this purpose the chapters on teeth may be selected. The question is propounded—

"What is the least size in which a given tooth can be present in a species which sometimes has it and sometimes is without it?"

And it is remarked that—

"Considered in the absence of evidence it might be supposed that any tooth could be reduced to the smallest limits which are histologically conceivable; that a few cells might take on the character of dental tissue. . . . Indeed, on the hypothesis that variation is continuous, this would be expected."

Mr. Bateson considers his evidence sufficient to show that "the least size of a tooth is different for different teeth and for different animals," and that therefore variation in the teeth of those animals discussed by him proceeds by integral steps, the magnitude of the step differing in different cases. But he gives no evidence that he has ever looked for teeth reduced to the smallest histological limits. The greater number of observations recorded are made upon dry skulls, in which such rudiments could not be demonstrated. It is notorious that in many animals, such as marsupials and whales, a whole set of teeth exists, which can only be demonstrated by careful histological examination of the entire jaw, at a definite period in the life of these animals. But more important is the fact that teeth have been demonstrated, consisting of a few cells, which have hardly progressed beyond the histological condition of an enamel germ, in several of the animals used by Mr. Bateson to support his view. These cases (*Cercopithecus*, probably *Cynocephalus*, the dog, and the cat) have been completely overlooked by Mr. Bateson; and similar cases in other vertebrates have been recorded. The condition, which is said to be necessary on the hypothesis that variation is continuous, does, therefore, in fact occur; and the contention as to the least possible size of particular teeth fails.

In somewhat similar cases Mr. Bateson lays stress upon the rarity of slight variations, as compared with more considerable abnormalities. In the present case, few persons have examined the jaws of mammals in such a way as to enable them to find the smaller abnormalities and there is no evidence worth discussion which shows whether they are rare or not. The only case in which large numbers of jaws have been examined by proper methods is that of man, in whom a fourth molar tooth is commonly present as a mere uncalcified rudiment, the cases in which this extra tooth becomes calcified and breaks through the jaw being rarer in proportion to the perfection of the extra tooth. A similar continuous series of variations is presented by the "wisdom tooth," which is most commonly a fairly perfect molar, cases of imperfection being rarer in proportion as the tooth approximates to the condition of "a few cells," hardly "taking on the character of dental tissue" at all, which it does occasionally assume. It is therefore curious, that in commencing his attempt to exhibit the discontinuity of dental variation, Mr. Bateson should dismiss the case of human teeth with the remark—

"I do not know that among these human variations are included phenomena different in kind from those seen in other groups, except perhaps certain cases of teeth united together."

Surely variation which proceeds by integral steps of the magnitude of a tooth may justly be held to differ in kind from variation which proceeds by indefinitely small gradations?

In the case of teeth, and in many other cases discussed, the method employed is not adapted to a determination of the least possible magnitude of variations. For in judging evidence based largely upon museum preparations and on printed records, it must always be remembered that there is a tendency among curators and others to regard a slight abnormality as not worth bottle, spirit, and a place on the shelf, or to think it too trivial for printed record. Anyone who has tried to obtain specimens for a museum knows that many persons will take pains to present a rare or striking specimen, who cannot be induced to send quite common things.

The only way in which the question can be settled for a given variation seems to be by taking large numbers of animals, in which the variation is known to occur, at random, and making a careful examination and record of each. Mr. Bateson's chapter on teeth, like all his chapters, is of great interest, and will doubtless serve to throw important light on many things. But a careful histological account of the jaws of five hundred dogs would have done more to show the least possible size of a tooth in dogs than all the information so painfully collected. And so in many other cases.

W. F. R. WELDON.

ALPINE GEOLOGY.

Ein Geologischer Querschnitt durch die Ost Alpen.—A Geological Transverse Section through the Eastern Alps.—By A. Rothpletz. (Stuttgart: Schweizerbart, 1894.)

THE title of this work at once announces its importance in Alpine geology. Most of us, young and old, are familiar with the section through the Eastern Alps which we owe to the veteran Austrian geologist Hofrath von Hauer. Since 1857, this section, from Passau to Duino, has held its place alone in atlas and text-book. In recent years, Swiss sections, more especially Heim's, have been placed side by side with it, but they only embrace the northern flanks and a part of the central chain. Dr. Rothpletz has given us in this volume the second complete section through the Alpine chain. He has laid the line of section farther east than von Hauer's, beginning at the Bavarian plain in the north, and traversing the Karwendel Mountains in the Bavarian Highlands and North Tyrol, the Tuxer and Zillerthal Mountains, east of the Brenner Pass, the Seisser Alpe, Schlern Rosengarten and the Predazzo district in South Tyrol, and the Sette Communi in the Italian Highlands.

The section, which is printed with colours, extends over a surface area 140 miles in breadth, and has the advantage of being drawn to true scale, vertical and horizontal (1 : 75000). So accustomed are we to exaggerated heights in Alpine sections, that this true-scale section conveys an impression of rather unimposing mountains and broad valleys. The eye misses also the familiar dotted lines connecting detached parts of the same geological strata, and helping one to a general appreciation of the author's conception of the whole section.

The absence of any such lines is almost a key-note to the character of the work. In the text, the author declares his opinion that (purposes of explanation, of course, excepted) geological sections should represent so far as possible only what has been actually observed, and should not suggest, by means of dotted lines or continued bands of colour, what may be, after all, only imaginary structural relations of the strata. The author's position in this respect is made very clear in the chapter on the "Glarner Double-Fold."

The bulk of the text is devoted to descriptive, stratigraphical, and tectonic details of the various districts surveyed by the author along the line of section, and is illustrated very fully by sketch-maps and sections. Rigid faithfulness of observation is a marked feature throughout. The same care and precision which may be traced in field-methods, has also been bestowed on the literary workmanship of the book. The treatment of the Bavarian Highlands is quite delightful. The drawings display so unmistakably the dependence of the main physical features on the strata, and the contrasts of landscape which tectonic disturbance has frequently produced. We read with equal interest of the synclinal fold in which the Walchen lake and the Jachenau valley now find themselves, and of the transverse faults which divert the Loisach and the Isar rivers out of their easterly course into a northern. On the other hand, in the case of the bend of the Inn Valley at Wörgl, it was a pre-Alpine oligocene basin whose soft strata guided the river northward to the Bavarian plain. Again, the author's powers of exposition are seen to advantage when he demonstrates the important fault-line between the Mesozoic limestones north of the Inn and the old crystalline rocks of the central massif. He proves also beyond dispute the geological independence of the Tuxer and Zillerthal groups north and south of the Inner Pfitsch valley. But we confess to a feeling of disappointment that although the section passes through these groups, it has been able to do so little to clear away the difficulties of the Central Alps. Several important questions are discussed without advancing us far—for example, the age of the granite intrusion north of Brixen, the significance of the serpentine rocks in the "Tarnthal Köpfe," the constant occurrence of a rocklike "sernifit" at the unconformable succession of Permian and Mesozoic strata on the old Palæozoic and crystalline floor.

Strict adherence to the truths observed in nature, while in itself laudable, seems somewhat to cramp boldness and freedom of thought, and we are landed in a mist of possibilities hovering over a conjectured Triassic period of mountain movement in the Central Alps, which may just as well have been post-Neocomian for all that is proved to the contrary. A similar uncertainty envelops the age-relationships of the overthrust at Tristkogl (Karwendel Mountains), whose special misfortune it is to be directed to the south, whereas the overfolding and overthrusting elsewhere in the Northern Limestone Alps are northward. It seems just possible that the overthrusts in this district are not all told?

One of the most striking chapters in the first part of the book is that on the origin of the Schlern Dolomite in South Tyrol.

Part iii. leaves no doubt as to the author's conception of the form of Alpine structure elucidated by his complete section. In his ideas he differs considerably from the recognised tenets either of Suess or of the Swiss school represented by Heim. Dr. Rothpletz puts the actual areal contraction due to late tertiary folding in the Alps at a much lower figure than Heim did. He emphasises the importance of vertical faults and the great part played by previous Alpine movements in determining the occurrence of overthrusting and overfolding during Pliocene pressure. He finds Suess' theory of the causes of mountain-movement insufficient, and suggests that if the earth's cooling resulted in radial expansion instead of radial contraction, as Suess assumed, a quite as likely explanation could be given of the actual facts observed in crust-movements.

Even if we cannot accept the dicta as final, we must welcome the thoroughly scientific spirit in which the author analyses the various doctrines, and shows what part or parts are doctrines of faith only, and what of the remaining are, in his experience, tenable or untenable. His own opinions are fixed upon most points, but he never seeks to impose *opinions* on his reader; *facts* alone are taught; and there is no more thirsty soil for facts than Alpine geology.

MARIA M. OGILVIE.

OUR BOOK SHELF.

The Natural History of Plants, from the German of Prof. Anton Kerner von Marilaun. By Prof. F. W. Oliver, M.A., D.Sc. (London, Glasgow, and Dublin: Blackie and Son, 1894.)

THE high praise we gave to Prof Kerner's *Pflanzenleben* when it appeared, makes it almost unnecessary for us to say much about the English edition now in course of publication, and which will be completed in sixteen monthly parts. The German work was said in these columns to be "the best account of the vegetable kingdom for general readers which has yet been produced." This judgment can also be applied to the translation which Prof. Oliver has made, with the assistance of Miss Marian Busk and Miss Mary Ewart. In translating a work, some of the brilliancy of the original is necessarily lost. It is difficult, however, to find awkward expressions in the pages before us; in fact, very few of the idiomatic phrases of the original work have survived. And the translation is scientifically accurate, as well as entertaining and instructive. Lovers of nature will find every page of the book interesting, and the serious student of botany will derive great advantage from its perusal. The illustrations are beautiful, and, what is more necessary, true to nature. The complete work contains about one thousand engravings on wood, and sixteen plates in colours. Botanical science will benefit by the issue of Prof. Oliver's edition of a splendid book.

Notes on some of the more Common Diseases in Queensland in relation to Atmospheric Conditions, 1887–91. By David Hardie, M.D., Hon. Physician Hospital for Sick Children, Lady Bowen Maternity Hospital, Brisbane. (Brisbane: Beal, 1893.)

THE author of this work has a most important aim in view, viz. to establish the connection between the weather and the prevalent diseases in Queensland, and expresses a hope that, in time, he will be able, if furnished with a forecast of the weather, to predict with certainty the diseases likely to predominate during the various seasons of the year, and thus to lay the foundations of a practical system of preventive medicine.

The conclusions are so interesting that some of his leading results may be briefly given.

The annual death-rate of Queensland per 1000 population is 15·11, varying from 13·38 in August and September (spring) to 16·28 in November to March (summer and early autumn), as contrasted with the death-rate of Great Britain for the year, 18·8.

The yearly mortality is lowest in West Southern Queensland (Darling Downs and Warrego), where it is only 8·92 per 1000, a little over half of the average Queensland rate, and the highest is 34·70 in West Northern Queensland (Normanton), a tropical region at the extreme north of the colony. This would point to the great advantage of altitude, combined with dryness, as seen on the Darling Downs, over marine influence and moisture to be found in the sea-coast districts. When we come to different classes of disease, we find that diphtheria, though specially prevalent in April, May, June, and July (winter), is endemic to some extent in all seasons, and causes a mortality of 2·15 per cent. Whatever contributes towards cold and dampness of the air during the autumn and winter, causes an increase in the death-rate from diphtheria, and, according to Dr. Hardie, the neighbourhood of swamps and marshes have considerable influence in this respect. Whooping-cough, on the other hand, attains its maximum during the warm and moist months of the year, and its close connection with a medium temperature for all seasons of the year with high relative humidity, is considered to support the assumption of its germ origin.

Phthisis is common along the eastern portion of the colony from Cooktown to Brisbane, reaching a maximum of 12·86 per cent. in the Rockhampton district; but this high mortality is partly to be attributed to the large Polynesian population, employed on sugar plantations in these districts, who are specially liable to phthisis. In the western and northern districts, however, it is much less prevalent, and the average percentage of deaths from all causes gives to phthisis 8·75. The months when the mortality is greatest are July, August, and September, and there seems to be no special connection between atmospheric moisture and phthisis mortality, but a low temperature in summer and autumn is favourable to a low phthisis mortality. Acute respiratory diseases, such as pneumonia, pleurisy, and bronchitis, are observed all over the colony, and vary in mortality in different parts; the highest on the coast and the lowest inland, the months of highest mortality being June, July, August, and September; the maximum is reached during and immediately after the colder period of the year.

The book, with its copious and valuable tables, is an honest attempt to deal with a very difficult problem, and thoroughly merits success, and if the author will only persevere in his researches, still more important results may follow.

LETTERS TO THE EDITOR.

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Panmixia.

I AM much obliged to Prof. Weldon for having so promptly answered my request, and hope that his example will be followed by any naturalists who may have any other grounds for questioning the doctrine of Panmixia. Meanwhile, however, there are two or three points touching which I should like to be sure that we correctly understand each other.

(1) Hitherto all naturalists who have written upon the subject have agreed, that "*the survival-mean must (on cessation of selection) fall to the birth-mean.*" And, in now questioning this view, Prof. Weldon appears to contemplate the difference between birth- and survival-means of only the first generation, which would be very unfair. Again, I do not follow Prof Weldon's meaning in what he says with regard to another point. Assuredly "every statistician knows" that selection can maintain the "mean height of a regiment" at 67½ inches, by enrolling only those individuals who are either "more than 66 and less than 69 inches high." But this would be *artificial* (*i.e.* intentional) selection. The "cases" to which he alludes, where *natural* selection could destroy individuals nearest the mean line, while favouring those which lie at greater distances *both above and below* this line, must be very exceptional.

(2) As regards the second cause of degeneration under Panmixia (viz. atavism), Prof. Weldon says merely that it is "not demonstrated by any statistics." This is true enough. But the same has to be said of natural selection. Whether in the building up of a structure by natural selection, or in the subsequent breaking down of a structure by atavism on the withdrawal of selection, the statistical method is equally unavailable for testing either theory: in both cases the most effective variations (*i.e.* deviations from the mean) at any given time are those which are most *numerous*, and therefore most *minute*. Hence, in both cases the best "demonstration" of the theory which can be offered is that which is yielded by the parallel facts in our domesticated animals.

(3) The only objection which is urged by Prof. Weldon against the last of the three causes which I mentioned (*i.e.* irregularities of heredity when uncontrolled by selection) is one which tells against the theory of Panmixia only because it does so against that of Natural Selection. As I understand, the argument is, "Natural selection is in most cases an imperfect agent in the adjustment of organisms": *ergo*, the cessation of

selection will not, in most cases, make much difference in the maintenance of such adjustment. Obviously this ground of objection to the theory of the cessation of selection opens up a much larger question than can here be dealt with, viz. the adjusting or eliminating value of the presence of selection. But if Prof. Weldon will read what I wrote last year in the *Contemporary Review*, during the Spencer-Weismann controversy, he will find that in this matter I am quite on the side of Mr. Bateson and himself. It has always been my endeavour to argue that the ultra-Darwinian school of Wallace and Weismann are pushing deductive speculation much too far in maintaining "The All-Sufficiency of Natural Selection." I shall never believe —any more than Darwin believed—that what I have called "selection value" is unlimited. But this is not incompatible with the belief that *in whatever degree* natural selection may have been instrumental in the construction of an adjustment, *in some degree* must its subsequent cessation tend to the degeneration of this adjustment, especially where complicity as distinguished from size is concerned, as stated in my last letter.

Summing up his objections to the doctrine of Panmixia, Prof. Weldon says they are two: "First, it is based on the assumption that selection, when acting on a species, must of necessity change the mean character of the species—an assumption incompatible with the maintenance of a species in a constant condition." This refers to the paragraph of his letter which, as already stated, I do not understand. The doctrine of Panmixia, as far as we are now concerned with it, does not refer to "species," but to specific characters, *i.e.* structures, organs, instincts, &c. Again, the doctrine, even with regard to specific characters, makes no "assumption" touching the *presence* of "selection acting on a species"—least of all that such *presence* will not maintain the species in a constant condition. On the contrary, the very essence of the doctrine is, that it is the *presence* of selection which *maintains* the constancy of a species (or specific character), and *therefore* that it is the *cessation* of selection which *upsets* the constancy by withdrawal of the maintaining influence. Hence, I do not understand Prof. Weldon's first objection. His second is, "that in the only case which has been experimentally investigated, the condition said to result from a condition of Panmixia does not, in fact, occur." This one case, he explains, is:—"Mr. Galton has shown that civilised Englishmen are themselves in a condition of Panmixia, at least with respect to several characters, especially stature and the colour of the eyes. Now the mean stature of Englishmen is known to be increasing, and there is no evidence of the disappearance of coloured eyes." But, as regards stature, it can scarcely be maintained that there is not *some* cause at work to account for the increase; yet, unless this is maintained, the case is clearly irrelevant. Again, the colour of the eyes of our mixed population cannot have had more than thirty or forty generations wherein to be affected by Panmixia, and therefore the most ardent supporters of this doctrine would scarcely expect any result to be yet appreciable in the case of so pronounced a racial character. Surely a better "case" is the one which I have already given in the most ancient and the most rapidly-breeding of our domesticated animals. It was the facts observed in this "case" which first suggested to me the doctrine of Panmixia, and so led me to question the inherited effects of disuse. Similarly, a year later, Mr. Galton, in his "Theory of Heredity" (which anticipated by about ten years all the fundamental parts of Weismann's), wrote of Panmixia thus:—"A special cause may be assigned for the effects of disuse in causing hereditary atrophy of the disused parts. It has already been shown that all exceptionally developed organs tend to deteriorate; consequently those that are not protected by heredity will dwindle. The level of muscular efficiency in the wing of a strongly-flying bird is like the level of water in the leaky vessel of a Danaid, only secured to the race by constant effort, so to speak; let the effort be relaxed ever so little, and the level immediately falls. . . . That this is a universal tendency among races in a state of nature, is proved by the fact that existing races are only kept at their present level by the severe action of selection." GEORGE J. ROMANES.

Oxford, May 5.

P.S.—I gladly accept the verbal correction in Prof. Weldon's third paragraph.

Physiological Psychology and Psychophysics.

OWING to my bookseller's habit of forwarding NATURE in monthly batches, I have only just seen the remarks appended to my letter in the issue of March 15. I think that the terminological question is sufficiently important to warrant a reply to these.

(1) I do not, of course, "subsume" psychophysics to physiological psychology. The latter, I stated, is both wider and narrower than experimental psychology; and wider, because it includes the consideration of certain ("the most important") psychological problems—not of *all* such problems. (For this view of physiological psychology, *cf.* Wundt, "Physiological Psychology," fourth edition, I. p. 9.)

(2) Fechner, "the coiner of the word," defines psychophysics as "eine exacte Lehre von den functionellen oder Abhangigkeitsbeziehungen zwischen Körper und Seele, allgemeiner zwischen Körperlichen und geistigen, physischer und psychischer Welt." (*cf.* "Psychophysik," second edition, I. p. 8.) What my critic says on this head is, therefore, incorrect.

(3) In the most widespread and important school of experimental psychology existing to-day—that of Wundt—there *is* agreement upon definitions. And even if my critic's remarks were true, it would not follow that a number of wrongs made a right.

(4) I might, in my last letter, have adverted upon the term, *psycho-physiological.* I did not understand what it exactly meant. In NATURE of March 29, Prof. Ll. Morgan defines it (p. 504) as the equivalent of Fechner's internal psychophysics. (*op. cit.* p. 10). In this sense it is not wanted; the phrases "external" and "internal psychophysics" are in use. (It might, however, be used to signify that part of physiology which has a conscious correlate.)

(5) My critic triumphantly adduces "reaction-times" as a subject treated of in the University College course. That course, *i.e.* deals with *one* conscious element, and with *one* type (action) of *one* of the two modes of conscious combination (association: fusion is left out of account). Prof. Münsterberg (Preface to *Psychological Laboratory of Haward University*) speaks of "the error, which is so prevalent, that experimental psychology is confined to the study of sensations and simple reaction-times."

(6) I am sorry that Dr. Hill's name should have been mentioned. I should not think of offering any opinion upon his work. I know no more of it than do the other readers of NATURE. If he sees these remarks, I hope he will believe that my original criticism was meant to be quite impersonal.

(7) "By far the larger part of the really fruitful work [in psycho-physiology]" says my critic, ". . . . has been done in the investigation of the senses." If he means by psycho-physiology what Prof. Ll. Morgan does, I must disagree with him. To substantiate either view would need an article. As he writes not as a working psychophysicist (else he would have been acquainted with Fechner's *Psychophysik*), I think that the *onus probandi* lies with him.

(8) As to Prof. Ll. Morgan's paper on "the scope of psycho-physiology [= internal psycophysics]," I must plead guilty to finding the writer's eclecticism somewhat unintelligible, and his whole treatment a little general and superficial.

(9) A very interesting minor question is that of the relation of Wundt's physiology psychology to Fechner's internal psychophysics. (*cf.* Külpe, *Arch. f. Geschichte du Philosophie*, 1892, pp. 183-4.)

Cornell University, April 16. E. B. TITCHENER.

IT seems hardly profitable to carry on a discussion with Dr. Titchener at intervals of more than a month. I readily confess that through an error of memory, for it is a good while since I read the "Elemente der Psychophysik," I misrepresented Fechner's use of the term psychophysics. The fact, however, that he recognised an "outer psychophysics," and the further fact that, as he shows ("Elemente," i. p. 11), nearly the whole of his inquiry has to do with establishing the relation of external stimuli to psychic phenomena, show that the error I fell into was not altogether unnatural. Are not the inquiries of Weber, Fechner, and their successors still brought under the head of psychophysics by those who reject Fechner's peculiar "psycho-physical" interpretation of the results? And do not nine students out of ten, who are not themselves "working psycho-physicists," associate the term "psychophysics" with these important lines of inquiry? If so, I would contend that there is room for a reconsideration of the terminology of the subject. The retention of Fechner's "outer psychophysics" seems confusing if, as I understand Dr. Titchener to say, "psychophysics' has properly to do with the correlation between psychic processes and intra-organic and (I suppose) more especially, central nervous processes. With respect to the term "psycho-physiological," used by Dr. Hill in his syllabus, it may at least be said that it avoids the ambiguity of "psychophysics," as coined and defined by Fechner, while it is the direct descendant of the term "mental physiology," which is well fixed in British scientific literature.

I have only to repeat that Dr. Titchener's attempt to distinguish between the domains of physiological psychology and psychophysics seems to me far from adequate. It has about it, to my irreverent eyes, something of Wundt's own oracular obscurity.

In calling atttention to reaction-time experiments, I did not refer to *simple* reaction-times. I thought the various lines of experiment in which the processes of hesitation and selection, and so forth, are elucidated by measuring the intervals between sensory stimulus and muscular reaction might be brought under the head of reaction-time experiments. But not being in the privileged circle of working "psycho-physicists," I daresay I erred here too.

This little discussion will not be in vain if it wake up Dr. Titchener, or some other working psycho-physicist, to the obscurities that hang over their new field of research for the outside student. It seems to me that we want careful definitions of the respective scopes of the several departments of research which are either psychological or which bear directly on psychology, more especially experimental psychology as a whole, psychophysics, and physiological psychology. Neither Wundt's nor Dr. Titchener's definitions satisfy some of us who, on this benighted island, provokingly placed between two luminous continents, are doing our best to catch some of the rays which they are shooting forth in such abundance.

THE WRITER OF THE NOTE.

Some Oriental Beliefs about Bees and Wasps.

SINCE Baron Osten Sacken's letter appeared in NATURE (vol. xlix. p. 198), I have been taking an occasional survey in my small library of Oriental literature, to inform him of passages referring to the Bugonia-superstition. So far as I could find, the people of the far East seem not to have possessed any belief about oxen-born bees; however, *a propos* of this matter, I have come across several legends relating to some Hymenoptera, which I may group as follows:—

(1) *Fossores Story.*—Of all the insect stories of the far East, this may claim very high antiquity; it was first celebrated, more than two thousand years ago, by a verse in the Confucianist "Book of Poems," and is, to this date, preserved by a well-known metonymy "Ming-ling" (that is, the caterpillar), meaning the Foster-Child. This story, according to Yáng Hiung, a Chinese philosopher (53 B.C.–18 A.D.), was that "the Fossores, having no females, capture infant caterpillars from mulberry-trees, and address them a spell 'Mimic me, mimic me," whereby they are turned into the young Fossores." Indeed, the Japanese name of the Fossores is Jiga (that is, "Mimic-me"). Against this Teóu Hung-King, a Tauist sage (452-536 A.D.), has argued that these insects have had offsprings of their own, but used to deposit the eggs on bodies of other insects to provide them with food in future.

(2) *One-legged Wasp.*—In Li Shi-Chin's work, cited above, we read:—"This production of Ling-nán, resembling a wasp, small and black, has one leg united with the root of a tree; it can move but cannot escape." Also a One-legged Ant is mentioned. I would suggest that these insects were infested by the forms of Cordyceps, as is instanced in the stories of La Guêpe Vegétále.

(3) *Fungus-born Wasp.*—Twan Ching-Shih's "Miscellanies," book xvii., contains the following note:—"A poisonous and noctilucous Fungus of Ling-nán is, after rain, metamorphosed into a large black wasp with serrate mandibles more than three-tenths of an inch long. At night it tries to enter the ears and nostrils of a man, and hurts him in his heart."

(4) *Production of Amber from Bees.*—In the same work, book xi., is the following quotation from the "Record of Southern Savages":—"The Bees-with-Broken-Waists exist in the sands of Ning-chau, and come out when banks fall down; the natives make amber by applying fire to them." Obviously this erroneous inference was drawn from the presence in amber of some hymenopterous remains.

(5) *Diptera mistaken for Hymenoptera.*—Sie Tsái-Kang, in his "Miscellanies of Five Phenomena" (Japanese edition, 1661, book ix., p. 43), narrates thus:—"In Chang-sha I saw honey-bees all without stings, so that, when trifled with upon the palms, they were quite harmless: having no difference from flies, that was strange!" No doubt he has seen some Eristalis, as is indicated by Baron Osten Sacken.

(6) *Horse-hair Wasp.*—Tazan Kan, a Japanese literatus (1748–1827), writes on this subject in his "Rambling Notes" (Tokio, 1890, p. 22):—"About 1817 a half-rotten trunk of *Celtis sinensis* gave birth to wasps, whose tails they could not withdraw from the tree, thus causing many to die. Having the tails cut with scissors the survivors gladly departed. One winter a man bought a heap of fuel comprising a half-rotten oak abounding with the similar wasps, several of which were strung on a horse-hair in the same manner as a rosary, there being altogether several dozens of such hairs. The author's informant took home a hair passing through three or four wasps, and folded it in paper; afterwards the hair became divided, and the insects bit through the paper: the informant's suggestion was—'probably these wasps had been transformed from horse-hairs tangled round the rotten wood.'" Several times I have seen in Japan this so-called "Babi-bō" (the Horse-hair Wasp), still an object of popular amazement: it is nothing but an ichneumon-fly, *Bracon penetrator*, whose ovipositor of unusual length has been the principal cause of such a superstition.

KUMAGUSU MINAKATA.

15, Blithfield Street, Kensington, W., April 30.

P.S.—In my letter on the "Constellations of the Far East" (NATURE, vol. xlviii. p. 542), I gave from Twan Ching-Shih's "Miscellanies" portions of the list of the objects of Indian fancy as to the resemblances of the constellations. Last March, my reverend friend, Atchârya Dharmanâga, then in Paris, kindly sent me an extract from Roshin Sennin's Lecture on the Constellations, recorded in Mahâsannipâta Sûtra. After comparison, I find that both quite agree except for a few variations, so that that Chinese author of "Miscellanies" seems to me to have extracted his list from the above-mentioned Indian authority.

K. M.

The Mass of the Earth.

I HAVE no intention of reopening a discussion on the advisability and necessity of carefully separating in our minds those two notions, the *weight* of a body and its *mass*, which to me (and to a great many others) are now so completely distinct. The subject has already been treated of in these pages. Hence, in reply to the letter signed "K." I shall be very brief. The *mass* of a body is simply the quantity of matter which it contains; its *weight* is the force with which the earth pulls the body towards the centre of the earth; this force varies slightly at different points on the earth's surface, varies very much both when the body is removed outwards from the earth or inwards towards its centre, and would be nothing at all at the centre; the *weight* would be practically nothing if the body were removed a few millions of miles away from the earth. But through all these changes, and through all mere changes of *place*, the *mass* of the body is perfectly unaltered. *Weight* is a mere contingent property of *mass*, logically and physically distinct from it; a more contingent property than *shape*; for, while a body must have some shape, it need have no weight. The terms *weight of the earth*, *weight of Jupiter*, *weight of the sun*, &c., are absolutely ridiculous. The mass of the earth is acted upon by no force whatsoever except the attraction of the sun and the disturbing attractions of the moon and the planets. The earth attracts itself with no force; it has no weight. So much for positive statement.

The notion that the earth has weight—the inability or neglect to distinguish the necessary property of constant *mass* from the contingent property of *weight*—has given rise to many absurdities. "If everything on the earth has weight, the whole earth has weight," is a fallacy of composition worthy of a mediæval dialectician. That the earth is a very *heavy* body (with, of course, an inveterate tendency to fall "down"), has supplied us with the assumption that it is supported on the back of an elephant, these two exceedingly *heavy* bodies being together supported on the back of a tortoise, and so on. To compare the *weight* of Jupiter with that of the sun, "imagine a gigantic balance with equal arms and equal pans; let the sun be placed in one pan; then, *in order to preserve the balance*, more than 1000 Jupiters must be placed in the other pan."

I need not multiply examples of this world-wide fallacy. We have the two words *mass* and *weight*; let us keep them distinct, and thereby help towards an understanding of the nature of an absolute unit of force and other physical entities.

"K." says "the earth's weight, or mass, is $6 \cdot 14 \times 10^{21}$ tons. What is unmeaning or unscientific in this clear, intelligible, and accurate statement?" Answer, the identification of *weight* with *mass*. He is mistaken in supposing that Prof. Poynting's book has a double title. It is simply "The Mean Density of the Earth." The determination of the "constant of gravitation" is a deduction, and is, of course, so treated by Prof. Poynting.

THE REVIEWER.

Icebergs and Weather.

WITH reference to the notice in NATURE of May 3 (page 15), of a letter by Mr. Russell on icebergs and their relation to weather and temperature, I should like to give you a personal experience of my own with an iceberg in mid-Atlantic, when on board the steamship *India*, on its voyage from New York to Newcastle-on-Tyne, in June last year.

Our recorded temperatures of 43° F. and 45° F. of one day fell to 34° F. in water and in air on the next day. On reporting this to the chief officer, an extra look-out was kept, and the vessel put on half-speed, as the weather was foggy, and icebergs were likely to account for the sudden fall of temperature. Twenty minutes afterwards an iceberg was sighted, which showed a length of 1200 feet, and a height of 200 feet above water.

Leeds, May 7. A. SYDNEY D. ATKINSON.

Early Arrival of Birds.

I WAS at Sellack, Ross, Herefordshire, on March 22 and following days. Chiff-chaffs had arrived on the 22nd; cuckoos were heard on the following day. The willow-warbler and garden-warbler followed.

In quest of food, birds follow the path of least resistance. Thus their migrations, in the economy of nature, depend not simply on food, power of flight, distances, temperatures, &c., but on the associated extent of systems of wind.

May 4. W. CLEMENT LEY.

THE EFFECT OF EXTERNAL CONDITIONS UPON DEVELOPMENT.[1]

THERE is now ample justification for the belief that evolution is not due merely to internal causes, though we are as yet by no means quite clear as to the manner in which external influences have formed and transformed organisms. There is still a conflict between rival theories, and important points, though often apparently clear, are in reality not so.

It is often assumed, without sufficient proof, that a particular variation of an organism is the direct consequence of some external influence, simply because some causal connection exists between the two; but such an assumption is based upon a totally false idea as to the interconnection of the phenomena. In many cases this will be readily granted; take, for instance, that of the leaves of *Mimosa*, which close when they are touched. The actual cause of the movement is here due to the peculiar constitution of the plant, and not to the touch. The geotropism of plants, again, is not the direct effect of gravity, but is due to a special power of adaptation possessed by the plant. In reference to the histological adaptation of animal tissues, let us take as an example the structure of the lattice-work in spongy bones. Roux has shown that this is due to processes of selection and for a struggle for existence between the various parts of the body. Prof. Weismann speaks of this process as "intra-selection," and attempts to show that its effects are not inherited, as assumed by Roux, but that heredity only concerns those potentialities from which structures are developed by intra-selection. He believes that the potentialities have not arisen through the struggle between the parts of an organism, but through that between individuals; not by intra-selection, but by the ordinary process of natural selection. The *causa efficiens* of this histological adaptation is not. therefore, the tension or pressure which acts on the bones, but the adaptive material upon which such forces operate. The theory of intra-selection thereby loses nothing of its value, but on the contrary, is admitted to be of the greatest importance in maintaining the "co-adaptation" of parts during the metamorphosis of species.

The organism can, however, also be affected by external influences for which it is not adapted in advance. This is the case as regards the ordinary seasonal dimorphism of butterflies; but even seasonal differences may be produced by adaptation—here a double adaptation—in which the external influences of temperature do not act as the direct causes of change, but only as stimuli, which determine as to which of the two forms of the species shall arise.

In the case of neuters of social insects, the external influence—scanty food—is not, as Herbert Spencer assumes, the true *causa efficiens* which produces the sterility of their caste, but only the stimulus by which the primary constituents (*Anlagen*) of the worker-type are brought into activity. At least three kinds of primary constituents—those of the male, the fertile female, and the worker—must be contained in the eggs of ants, bees, and termites; the nature of the stimulus acting upon the egg determines the kind of primary constituent which shall come into activity. These opinions are confirmed by experiments made on flies, which show that insufficient nourishment supplied to the larva does not in any way affect the development of the ovary. The disappearance of typical organs—such as the ovarian egg-tubes of bees and ants—is thus shown to be a phylogenetic and not an ontogenetic process: it does not depend on mere influences of nutrition, but on variation in the primary constituents of the germ; and thus can only come about in the course of numerous generations. The case of social insects is therefore far from contributing any support to the view that acquired characters are inherited, and that the inheritance of the effects of use or disuse play a part in the transformation of species, as is assumed by Herbert Spencer.

Thus we see that external influences in many cases serve as the impulse which starts the process of development in certain of the primary constituents. The actual cause of these individual dissimilarities is in all cases to be sought in the modification occurring amongst the primary constituents of the body itself; and such purposeful modifications can only have originated by selection. Even when to all appearance external influences have had direct action in causing purposeful modifications, a more careful examination will always show that they have only served to bring some preformed adaptation into activity. This is proved in a specially conclusive manner by the consideration of sterility in the workers of bees and ants: the sterility is not due to poor nourishment, but to natural selection, which has determined the nature of the primary constituents in the ovary. This case is of especial interest, as it has been so much relied on as a support to the Lamarckian principle of the inheritance of acquired qualities. Here, as in all other instances, the Lamarckian hypothesis is untenable; selection has been the only principle on which the development of the organic world has been guided on its course.

[1] Abstract of the Romanes Lecture delivered in the Sheldonian Theatre at Oxford, on May 2, by Prof. August Weismann, Ph.D., D.C.L.

THE PLANET SATURN.

In these days, when the telescope is in more or less common use, and so many have opportunities of observing the heavenly bodies, it is interesting to look back on the past and survey in a general manner the thoughts and ideas of those who in the earlier period of observational astronomy were not so well equipped. To take the case of the great Florentian astronomer, who practically had the whole Cosmos, so to speak, at the end of his telescope, since he was the first who surveyed the objects in the sky with something in addition to the naked eye—one can picture him sweeping with his "optik tube" or small telescope the starry heavens, and suddenly coming across the planet which we have under consideration. Here he had an object which was quite unique, and which, with his small power of magnification, must have puzzled him considerably.

In a letter to the Grand Duke of Tuscany, he refers to Saturn as appearing triple (tergeminus). Later, in a communication to the Austrian Ambassador (November 13, 1610), he makes the interesting statement: "When I observe Saturn with a telescope magnifying more than 30 times, the largest star appears in the middle; of the others, one lies to the west and the other to the east in a line which does not coincide with the direction of the equator, and seems to touch the central star. They appear to me as two servants, who wait upon the aged Saturn, travelling with him and not departing from his side. With a telescope of smaller magnifying power the star appears elongated and of the form of an olive."

Such, then, is the earliest telescopic observation of this planet that we have on record, and it might be interesting to pursue Galileo's inquiries a little further, and follow his state of mind when these "two servants" disappeared, as was the case in his later observations, causing him to look upon his earlier observations as phantoms or illusions.

With us to-day the case is different, and what we see in place of the "two servants" is the beautiful series of rings which girdle the planet in the region of his equator. Huyghens it was who first announced this ring system, and since then observations have shown many details of great interest, both in the ring itself and on the planet's surface.

Many are the objects of inquiry which lead observers to make a study of the appearance of this planet. The ring system and its varied shades, the belts girdling the planet's surface, the dark and light spots on the belts, the period of rotation &c., are only a few that might be mentioned.

Recent oppositions have enabled much work to be done in these lines, and the one just passed (April 11) has, we hope, still more increased our stock of knowledge. About the present time the planet and the brightest star in the constellation of the Virgin (Spica) make a fine pair in the sky. Both are fairly bright objects, and Saturn is known by the more golden hue with which he shines. At the present moment Saturn is retrograding, *i.e.* moving in the westward direction, and his position about the present time is to the north of Spica. The next stationary point in his orbit will be reached on June 21, conjunction occurring on October 23.

With reference to the general brightness which the planet and his ring system exhibit at different times, Dr. G. Müller[1] has recently made some interesting observations. The light-conditions, on account of the rings, are referred to as very complicated. If sufficient observations be considered, he has found that distinct changes in brightness are apparent, depending on the phase of the planet, while much more apparent and naturally greater variations are noticeable, depending on the change of the plane of the ring from the line joining Saturn and the earth. When the rings are broadest, the planet in mean opposition shines a little brighter than Arcturus, and when they are invisible Aldebaran may be taken as their equal in brightness. In referring to some of the larger light changes, such as those which occur at different times with the planets Mars and Jupiter, the proportions here developed do not, we are told, tell us anything. In 1883–85, for instance, the reduced magnitude at opposition (0·85) was a little brighter than that for 1880 (0·90), 1880–83 (0·88), and 1886–88 (0·90). Other magnitudes at opposition, eight in number, have been derived by Sildel: thus in 1852 three values gave the mean opposition magnitude as 1·16 ± 0·07, the remainder (5), made from 1857–58, gave 0·97 ± 0·02. At a later date (1862–65) Zöllner from fourteen observations suggested a magnitude of 0·95; while Müller, in this paper, after a formula of his own, obtains 0·88 as the mean oppositional magnitude.

With regard now to the period of rotation of the planet, Herschel, in 1793, was the first who studied this question, giving its length as 10h. 16m. 0·4s., a value, accurate as he stated, "to much less than two minutes either way."

Since that time several more minute discussions have arisen, from which have resulted different values, among which may be mentioned Prof. Asaph Hall's period of 10h. 14m. 23·8s. ± 2·3s.

The latest important results on this question are due to Mr. Stanley Williams,[1] who has taken every pains for the determination of an accurate value, and to free the results from any possibility of their being influenced by preconceived ideas. With regard to the method of observation employed, and the details of the observations themselves, we must refer the reader to the publication mentioned below, but a brief summary of the results may not be out of place.

The observations were made in 1893, and two kinds of spots were observed: (1) dark spots upon a conspicuous double belt in the northern hemisphere; and (2) bright spots in the equatorial zone.

In the case of the former, the period was obtained from numerous spots, but eleven of them have been used as giving well-ascertained values, a table of these figures showing that they can be arranged into two classes, the means of which are 10h. 14m. 29·07 and 10h. 15m. 0·74s. Between these values there is a difference of over half a minute, a quantity too large, judging from the way sets of observations agree *inter se*, to be due to errors of observation.

With the bright spots a similar result is noticed, only here the difference is not the same. Out of the five series of deduced values, four may be coupled well together giving a mean value of 10h. 12m. 59·36s. The fifth or outstanding value is 13 seconds shorter than this.

These different values for the periods of rotation point out pretty distinctly that the spots that have been observed are by no means fixed relatively to the planet's surface, but are endowed with a proper motion of their own. In the case of the dark spots, the surface material must have rotated over half a minute more quickly in the same latitude upon one side of Saturn than upon the other. Mr. Stanley Williams summarises the results of his discussion in the following words:—

"Between N. Kronometric latitudes 17° and 37° the surface material of Saturn rotated in 1893 at the rate of 10h. 14m. 29·07s. ± 0·27s. between longitudes 45° and 140°, and at the rate of 10h. 15m. 0·74s. ± 0·56s. between longitudes 175° and 340°, whilst between longitudes 340° and 45° there was a region in which the surface material rotated at a rate intermediate between the above values.

"Between N. Kronocentric latitude 6° and about 2° S. latitude, the surface material of Saturn rotated in 1893 at the rate of 10h. 12m. 59·36s. ± 0·27s. between latitudes

[1] "Publikation des Astrophysikalischen Obs. zu Potsdam," 8, No. 30, Stück 4.

[1] *Monthly Notices* of R.A. Society, vol. liv. No. 5, March 1894, p 297.

0° and 140°, whilst between longitudes 140° and 360° the rate of rotation was rather quicker, the average period of rotation here being well represented by . . . . 10h. 12m. 45·8s."

The importance of such results as those stated above will help considerably to unravel the mystery surrounding the circulation of the envelope of this great planet, but, for the observations to be comparable, they must be accurate, systematically made, and extend continuously over moderately long periods. For the years 1891–93, Mr. Stanley Williams points out that the acceleration in the motion of the bright equatorial spots can be clearly deduced from the different periods of rotation.

They are for—

	h.	m.	s.
1891	10	14	21·8
1892	10	13	38·4
1893	10	12	59·4

For the determination of the latitudes of Saturn's belts, the Rev. W. Freeman has recently published a method[1] which should prove useful for observers wishing to measure kronocentric latitudes.

Recent work on Saturn has, however, been done in another direction, Miss Klumpke having undertaken a further investigation of the problem of the figure of a fluid ring or a solid ring covered with liquid, in equilibrium about Saturn. This has been previously treated of by Laplace, and in recent times by M. Tisserand and Mdme. Kowalewski. Miss Klumpke has carried on Mdme. Kowalewski's work, but includes terms of a higher order, showing that the main result is very little altered. The second part of her thesis deals with the hypothetical case in which Saturn's mass is taken as zero: the rings thus will be subject only to the centrifugal force of its motion and mutual attraction of its particles. A first approximation gives the cross section of the ring as a circle, the second becomes an ellipse, and the third cross section is inclined to be egg-shaped, one end being oval. W. J. L.

NOTES.

THE New York Mathematical Society proposes to organise a general session, extending over several days, to be held annually during the summer vacation, at some appropriate place and time. This year the session is to be held in Brooklyn, on August 20, 21, 22, the days immediately preceding the session of the American Association for the Advancement of Science. The Council of the same Society has been considering with great care its present organisation. One of the recommendations made by it is that the name should be changed to the American Mathematical Society.

WE regret to have to announce the death of Mr. Adolph Leipner, Professor of Botany in University College, Bristol. Prof. Leipner occupied the office of honorary secretary of the Bristol Naturalists' Society from its inception in 1862, and was elected President of the Society last year. The loss caused by his death, not only to the Society, and the College he served, but also to all those who are interested in the natural history of the Bristol neighbourhood, is a serious one, for he was a naturalist of wide experience, ever ready to place his stores of knowledge at the disposal of his fellow students.

THE death is announced, at an advanced age, at Marseilles, of M. A. Derbès, one of the pioneers in the study of the life-history of Algæ. His "Recherches sur les zoospores des Algues et les anthéridies des Cryptogames," published in 1847, in conjunction with M. Solier, was a perfect mine of new facts with regard to the reproduction of Cryptogams, and formed the basis of all later observations on the same subject. For many years M. Derbès had been prevented, by the results of an accident, from the pursuits of botany, with the exception of the duties of his professorial chair.

[1] *Monthly Notices*, liv. No. 1, Nov. 1893.

AT the Annual Congress of German Naturalists and Physicians, which will be held at Vienna towards the end of September, there will be an exhibition of objects of interest in natural history and medicine.

THE Government of India are making systematic inquiry into the efficacy of hypodermic injections of strychnine in the treatment of snake-bite. The Punjab Government have at their request forwarded a list of cases so treated in the province during the past year.

AT the congress of the Sanitary Institute, to be held in Liverpool next September, Dr. Klein, F.R.S., will act as President of Section I.—Sanitary Science and Preventive Medicine; Dr. T. Stevenson has accepted the presidency of Section III. —Chemistry, Meteorology, and Geology.

AN earthquake was felt in several districts of South Wales on Wednesday, May 2. At Caerphilly, dwelling-houses were so shaken that light articles of furniture were upset, and crockery-ware fell to the ground. The tremor was also felt at Cardiff, a decided vibration being experienced at about half-past twelve in the day.

THE sum of five thousand rupees has been given by the Maharajah of Bhownagger towards a Pasteur Institute for India. Though the scheme has met with some opposition, the strong committees that have been formed in various parts of India in order to support it, leaves little doubt that the Institute will eventually be established.

THIS year's conversazione of the Society of Arts will take place on Friday evening, June 22, at the Imperial Institute.

THE Institution of Electrical Engineers will hold a conversazione in the galleries of the Royal Institute of Painters in Water Colours, Piccadilly, on the evening of Thursday, May 31.

THE Yorkshire Naturalists' Union will hold a meeting at Sedbergh, for the investigation of the neighbourhood of Dowbiggin, Lune Valley, and Uldale, on Whit-Monday, May 14.

ON Thursday last a public meeting was held in Prince's Hall, Piccadilly, in support of the proposal to erect a memorial to the late Sir Andrew Clark. The Duke of Cambridge took the chair as President of the London Hospital, and the audience contained a large number of persons eminent in all branches of knowledge. Mr. Gladstone testified to his late physician's high character, referring to him as a representative of all that is best and noblest in the medical profession. He concluded by moving:—"That in recognition of the great services rendered to the community by Sir Andrew Clark, Bart., M.D., a memorial be established which shall perpetuate his name and his work." This resolution was carried, and also the following, moved by Canon Wilberforce:—"That steps be taken to raise a sum sufficient for the erection of a block of buildings at the London Hospital, to bear the name of Sir Andrew Clark, which will afford increased facilities for the relief of suffering and the advancement of medical science." Mr. Jonathan Hutchinson, who was one of Sir Andrew Clark's colleagues, made some very appropriate remarks in supporting the first resolution. Medical men did not claim for the deceased physician the discoveries of a Harvey, a Jenner, or a Hunter, he said, but they nevertheless held that he was in the highest and best sense of the word a representative man, to whom it was the duty and the privilege and the interest of the whole community to do honour. Shakespeare had said "one good deed, dying tongueless, slaughters a thousand," and he (Mr. Hutchinson) took that to mean that an injury was inflicted on future ages if a single good deed were allowed to die without suitable commemoration, and in the life of Sir Andrew Clark they had a record which would be a treasure for generations yet to come.

THE arrangements for the sixty-second annual meeting of the British Medical Association, to be held at Bristol on July 31, August 1, 2, and 3, are given in the *British Medical Journal*. The President-elect is Dr. E. Long-Fox. An address in Medicine will be given by Prof. T. G. Stewart, one in Surgery will be delivered by Dr. Greig Smith, and Sir Charles Cameron will discourse on Public Medicine. The Sections and their Presidents are as follows:—(A) Medicine, Dr. Frederick T. Roberts; (B) Surgery, Dr. W. Mitchell Banks; (C) Obstetric Medicine and Gyneæcology, Prof. J. G. Swayne; (D) Public Medicine, Prof. W. H. Corfield; (E) Psychology, Dr. G. F. Blandford; (F) Pathology, Dr. G. Sims Woodhead; (G) Ophthalmology, Dr. F. R. Cross; (H) Laryngology and Otology, Dr. P. McBride; (I) Dermatology, Dr. A. J. Harrison; (J) Diseases of Children, Dr. W. Howship Dickinson. The Annual Museum in connection with the meeting will be arranged in the following sections: Section A.—Food and Drugs, including Prepared Foods, Chemical and Pharmaceutical Preparations, &c. Section B.—Instruments, comprising Medical and Surgical Instruments and Appliances, Electrical Instruments, Microscopes, &c. Section C.—Books, including Diagrams, Charts, &c. Section D.—Sanitary and Ambulance Appliances. All communications on general matters connected with the Museum, and all applications for space, should be addressed to Mr. John Dacre, 14, Eaton-crescent, Clifton, Bristol, before June 20, and a brief description of each exhibit for insertion in the Museum Catalogue must be in the hands of the respective Secretaries before July 1.

AT the meeting of the French Meteorological Society on April 12, M. Renou, President, made some interesting remarks upon thunderstorms. He said that they occurred in some parts of France every day of the year, and during six or seven months in 1892 as many as 328 were counted. He remarked that they were more frequent in Europe than in equatorial regions; at Sumatra, for instance, storms occur during the six months of the south-east monsoon, but thunder is never heard. In France they generally traverse a narrow tract from south-west to north-east, but in the hot regions of the globe, on the contrary, the storms are nearly stationary. They are very exceptional in Peru, occurring only once or twice in a century; there was one in January 1877, but none had occurred previously since 1803.

REPORTS of cuckoos being seen and heard long before the usual date of the arrival of the bird are made every year. Generally the reports cannot be relied upon, but a circumstantial account by Dr. A. J. Fleming, in the *Zoologist* for April, goes to show that he really saw a cuckoo on March 5 of this year. The accuracy of his observations, however, is questioned in the current number of the journal by several naturalists, most of whom assert that March cuckoos do not exist. Mr. J. E. Harting remarks:—"From numerous observations made by competent naturalists in different localities it appears that the usual time of arrival of the cuckoo in this country is between the 20th and 27th April; and the average date of its appearance may be said to be on the 23rd of that month, St. George's Day. In no instance, so far as I am aware, has the bird been heard, or seen (by any competent observer) before the 6th of April. . . . It is surprising how few people are to be trusted, either in the matter of *eyes* or *ears*, in regard to the cuckoo. Many do not know a cuckoo on the wing from a male sparrow-hawk, and others convince themselves that they have heard this bird's notes when they have been listening to a clever imitation by some village bird-nesting boy, or to the still more deceptive notes of a cuckoo-clock in a neighbouring cottage."

LIEUT.-COLONEL SAWYER read a paper to the Royal Geographical Society, at its last meeting, on the Bakhtiari Mountains and Upper Elam, a part of Persia which his surveys in 1890 have made it possible to map correctly. In the practical work of the survey he was assisted by the Indian surveyor Imam Sharif, who subsequently accompanied Mr. Bent's expedition to Hadramaut. The country is practically the continuation of the classical Zagros mountains, and the portion surveyed is a tract 30 miles wide between the bordering mountain ridges, and 300 miles in length from north-west to south-east. In the centre the mountains culminate in the mass of Kuh-i-rang, 12,800 feet above the sea, whence flow the chief rivers of Persia, the Zainderud, the Ab-i-diz and the Karun, the upper courses of which have been mapped correctly for the first time. This central mass not only separates two distinct drainage areas, it divides two entirely different ethnographical regions—the country of the Bakhtiari lies to the east, and Upper Elam to the west. Upper Elam is peculiarly rich in ancient remains, few of which have yet been examined, and many of them reach back to a very high antiquity. The expedition, though primarily for the purpose of surveying, did not fail to take account of more specially scientific matters, and although little was said about the geology of the mountains, their flora seems to have been pretty fully studied, as 350 species of plants were collected, of which seventeen at least were new to science. The people, who have been described from observations on this expedition by Mrs. Bishop, struck Colonel Sawyer as proud and warlike, and although warped in character by their isolated habitat, they yet remain possessed of many fine characteristics, physical and mental.

THE May number of the *Scottish Geographical Magazine* contains several interesting papers. One on the geographical unity of the British Empire should strictly be entitled the economic unity, as the bond cannot without much straining be called a geographical one. Herr Victor Dingelstedt gives an account of the isolated valley of the Vièze, at the foot of the Dent du Midi, showing how isolation and the physical conditions of configuration, climate, and vegetation have combined to keep the people in a remarkably backward and primitive state. Mr. Stuart-Glennie gives in very brief abstract an outline of his travels on the border-land between Turkey and Greece in an article on "Dodona, Olympos, and Samothrace," which would have been more valuable if the date of his visit had been indicated. He promises to complete the account of the ethnography of the region in a forthcoming work, under the title of "Ancient Hellas."

THE *Journal* of the Tyneside Geographical Society is rapidly approaching the form of a well-to-do geographical monthly; and although still relying to a considerable extent on articles reprinted from other publications, the May number includes two original lectures, one by Mr. Clements R. Markham, F.R.S., on Peru, and the other by Lord Roberts, on Delhi and its siege in 1857.

THE first part for 1894 of the *Records* of the Geological Survey of India contains a report on the Bhaganwala Coal-field of the Salt Range, by T. D. la Touche. This coal-field was first made known in 1853; reports on it have appeared by Dr. Oldham (1864) and Mr. Wynne (1878). Estimates of its value have recently been published, which Dr. King, the Director of the Survey, believes to be greatly exaggerated; hence the present report. The coal is of Nummulitic age; the seam varies much in thickness, and is irregular in its mode of occurrence. Estimates based on a thickness of from $3\frac{1}{2}$ to 5 feet, and a maximum depth of 2000 feet, give one million tons as the probable quantity available,

THE Director's tri-monthly notes in the same number of the *Records* contain a full account of the gigantic landslip in Garhwal, noticed previously in NATURE (vol. xlix. p. 438). The fall of rock took place last September, and dammed up the valley of the Bihri Ganga by a wall 900 feet high. The water has steadily accumulated behind the dam, and is now daily expected to overflow. The first rush of water will be severe, and probably about 250 feet of the dam will be carried away; after that there may be a permanent lake established with a natural outfall. An investigation of this interesting landslip is in progress by Mr. Holland.

DR. J. W. GREGORY describes the Echinoidea of Cutch in the "Palæontologica Indica" (ser. ix. vol. ii. part 1, 1893). The Echinoids are few and small; they lived probably on sand and in rock-pools in a somewhat rough sea around a series of coral-reefs. More light on the geology of the area may be expected from the large collection of corals. The fauna seems to be homotaxial with the Callovian of Europe.

A RECENT number of the *Comptes Rendus* contains a note by M. E. Bouty on the capacity of a polarised surface of mercury and other substances. By means of the principles of thermodynamics, Lippmann has shown that the polarisation capacity of unit surface of mercury, the surface being kept constant, is equal to $-\frac{d^2A}{de^2}$ where A is the surface tension and e the counter electromotive force of polarisation. M. Lippmann has also calculated the value of this expression for mercury in contact with acidulated water, and found that, at any rate within wide limits, the value is independent of e. On the other hand, the author, as well as Blondlot, finds that in the case of platinum electrodes in different electrolytes, even when the time of charge is evanescent, the capacity increases rapidly with the increase of e. The author has shown that this increase does not take place if we consider that part of the capacity which is capable of producing a discharge current in an external circuit, and that polarisation phenomena are in part irreversible, except for infinitely small values of the electromotive force and the time. The experiments thus show that, if the irreversible parts of the phenomena are omitted, the capacity of platinum is independent of e, just as is the case with mercury. With platinum electrodes and a solution of sodium nitrate the author has found values for the capacity of a square centimetre of the electrode at temperatures between 21° and 25° of from 17·72 microfarads for a solution containing two equivalents of the salt per litre to 9·32 microfarads for a solution of 0·0001 equivalents per litre. In the case of distilled water (platinum electrodes) he finds the capacity per square centimetre to be 8·27 microfarads.

IN a paper read at the Botanical Congress at Genoa, last year, Prof. Saccardo calculates the number of species of plants at present known as 173,706, distributed as follows:—Flowering plants, 105,231; Ferns, 2819; other Vascular Cryptogams, 565; Mosses, 4609; Hepaticæ, 3041; Lichens, 5600; Fungi, 39,603; Algæ, 12,178. Prof. Saccardo thinks it probable that the total number of existing species of Fungi may amount to 250,000, and of all other plants to 135,000.

A SET of simple and instructive heat experiments, illustrating the laws of expansion, radiation, and convection, are described by Dr. V. Dvorák in the *Zeitschrift für Physikalischen Unterricht*. To show the expansion of a solid by heat, a brass wire, 1·5m. long, is suspended horizontally by two clamps. A weight of about 5 gr. is suspended at the middle of the wire, and a small wooden block is placed below it. The expansion produced by passing a lighted match along the wire produces a perceptible lowering of the weight. To prove that emissive power is proportional to the power of absorbing radiant heat, Dr. Dvorák constructs a special thermo-couple, consisting of a disc of thin German silver, to the back of which a thin steel wire is soldered. Both are connected with a delicate galvanometer. When the disc is lamp-blacked and exposed to radiation, a larger deflection is obtained than after the lamp-black has been wiped off. A still larger deflection is obtained when the lamp-black is moistened with olive oil. A comparison of these indications with the deflections obtained from the radiation of a Leslie cube with the corresponding surfaces proves the law referred to. To exhibit the phenomenon of latent heat, a plate of copper foil mounted on a wooden ring is soldered to a German silver wire, and both copper and wire are provided with binding screws, and connected with a galvanometer. By depressing the copper foil into the ring a shallow dish is obtained, in which liquids may be evaporated. The galvanometer shows the loss of heat attending evaporation, the amount of latent heat differing with the liquid used. Convection currents in air and water are, according to Dr. Dvorák, well shown by introducing the liquid or gas between a screen and a point of light. The latter may be produced by placing a screen with a small hole at the principal focus of a lens receiving the sun's rays. A Bunsen burner with a small flame, a glowing match, water containing a wire heated by an electric current, or water cooled by a fragment of ice or salt at the surface, show beautiful ascending or descending clouds on the screen.

WE welcome the May number of "The Country Month by Month," by Mrs. J. A. Owen and Prof. Boulger. What has been said of previous issues applies to this. The book is a chatty and bright companion for country rambles.

PROF. W. R. FISHER wishes us to state that he was not present at the evening meeting of the Essex Field Club at Chingford, reported in our last issue (p. 12).

THE quarterly *Journal* of the Geological Society, No. 198, which has just been published, contains, in addition to the papers read before the Society from November 1893 to February 1894, inclusive, the anniversary address of the President, "On some Recent Work of the Geological Society," part ii. The *Journal* reaches its jubilee this year, and a suitable index to the contents is being prepared to commemorate the occurrence.

MESSRS. HACHETTE AND CO., of Paris, have begun to publish, in parts, a work by M. Maspero, entitled "L'Histoire Ancienne des Peuples de l'Orient." Maspero's book, published under the same title nearly twenty years ago, has become classical. That of which the publication has just been commenced, however, will only be like its predecessor in name and general outline; for the text will be new, and it is to be richly illustrated. It will be divided into three volumes, issued in about 150 separate parts of sixteen pages each. Oriental history will be scientifically treated, and the work will appeal to all who are interested in the discoveries that have been made in Egypt, Assyria, Chaldea, and Asia Minor during the last thirty years.

THE March number of *Modern Medicine and Bacteriological Review* contains a reminiscence of Sir Andrew Clark from the pen of Miss Frances E. Willard. The little article is not only interesting but useful, containing an exact account of general and most simple instructions recommended by Sir Andrew Clark for the maintenance of health, which admit of wide application. Amongst the bacteriological notes, we find a reference to a paper by E. Fraenkel, published in the *Deutsche Medicinische Wochenschrift*, on the treatment of fifty-seven cases of typhoid fever by injection of a sterilised culture of typhoid bacilli. Fraenkel states that the results obtainable by this method of treatment are superior to those of any other method which has been previously employed. The earlier the treatment commences the more successful are the results obtained. The injections are said to be perfectly harmless when made into the muscle, but cause much pain when introduced subcutaneously. Dr. Kellogg contributes an article on the "Relation of Modern Physiological Chemistry to Vegetarianism," which is to be continued. The other longer papers are chiefly of medical interest.

THE explosive halogen compounds of nitrogen form the subject of a memoir contributed to the current number of the *Berichte* by Dr. Seliwanow, of St. Petersburg. Pure trichloride of nitrogen, NCl_3, was prepared for the first time in a state of purity in the year 1888, by Dr. Gattermann, in Prof. Victor Meyer's laboratory at Göttingen. It was shown to be an oily liquid of so unstable a character that strong sunlight, or the light waves emanating from a powerful artificial source such as burning magnesium, instantly provoke its extremely violent explosive decomposition. By working in a dull light, however, Dr. Gattermann succeeded in weighing a quantity of the liquid and analysing it. He showed, moreover, that the crude liquid substance obtained by the action of chlorine on ammonium chloride is a mixture of two or perhaps three different chlorides of nitrogen, and that the pure trichloride is only to be obtained by subjecting this product, after removal of all sal-ammoniac by washing and subsequently draining from water, to the action of a rapid stream of chlorine. Iodide of nitrogen has frequently formed the subject of investigation, and last year Dr. Szuhay, of Buda-Pesth, showed that the substance obtained by adding excess of ammonia to a solution of iodine in potassium iodide consists largely of the compound, NHI_2. The existence of an iodide containing hydrogen had previously been indicated by Dr. Gladstone and M. Bineau, but it appears probable that in presence of excess of iodine, the tri-iodide NI_3 is also produced in large quantity. That halogen compounds of nitrogen containing likewise hydrogen are capable of existence would appear, therefore, to be fully proved by the work of Drs. Gattermann and Szuhay, and the latter chemist actually succeeded in preparing a silver derivative $NAgI_2$, a substance as explosive as the iodide of nitrogen itself. Dr. Seliwanow now brings forward evidence to show that the formation of chloride or iodide of nitrogen by the action of the halogens upon ammonia occurs in two stages, hypochlorous or hypo-iodous acid being first produced. When a dilute instead of a concentrated solution of iodine is employed, no separation of iodide of nitrogen occurs, and the solution is found to contain both ammonium iodide and hypo-iodous acid HOI; the latter is readily detected by means of a reaction with potassium iodide in which iodine is liberated, which Dr. Seliwanow has recently discovered during the course of his work on certain organic derivatives of this acid. Upon increasing the strength of the solution of iodine, iodide of nitrogen at length commences to be deposited, and this is found to occur at the expense of the hypo-iodous acid. Hence iodide of nitrogen appears to be formed directly by the action of ammonia upon the unstable hypo-iodous acid produced in the first stage of the reaction. A similar explanation is also shown to hold with respect to the formation of chloride of nitrogen. It is interesting to observe that Dr. Seliwanow actually proves the existence of hypo-iodous acid in a solution of ammonia, a fact which may perhaps be accounted for by the recent remarkable discovery of Prof. Victor Meyer that this so-called acid is really endowed with basic properties. When it does react with ammonia, the chief product, as above shown, is iodide of nitrogen. The two equations for the formation of the latter are formulated by Dr. Seliwanow as follows:—

$$NH_3 + I_2 + H_2O = NH_4I + HOI.$$
$$NH_3 + 3HOI = NI_3 + 3H_2O.$$

THE additions to the Zoological Society's Gardens during the past week include a De Filippi's Meadow Starling (*Sturnella de filippi*) from La Plata, presented by Sir Harry B. Lumsden, C.B.; two Common Peafowls (*Pavo cristatus*, ♀ ♀) from India, presented by Mr. Richard Hunter; a Chicken Snake (*Coluber quadrivittatus*) from Florida, U.S.A., presented by Master James W. Philips; a Common Boa (*Boa constrictor*), two Tree Boas (*Corallus hortulanus*), a Thick-necked Tree Boa (*Epicrates cenchris*), a Carinated Snake (*Herpetodryas carinatus*) from Trinidad, presented by Messrs. Mole and Urich; a Ring-hals Snake (*Sepedon hæmachates*), two Cape Vipers (*Causus rhombeatus*) from South Africa, presented by Mr. J. E. Matcham; a Jaguar (*Felis onça*, ♂) from South America, two Plumed Ground Doves (*Geopelia plumifera*), two White Storks (*Ciconia alba*), two Vivacious Snakes (*Tachymenis vivax*), two Four-lined Snakes (*Coluber quadrilineatus*), four Green Lizards (*Lacerta viridis*) European, four Dark Green Snakes (*Zamenis atrovirens*), two Glass Snakes (*Pseudopus pallasi*) from Dalmatia, purchased; two Senegal Touracons (*Corythaix persa*) from West Africa, received in exchange; two Barbary Wild Sheep (*Ovis tragelaphus*, ♀ ♀) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE MOON'S APPARENT DIAMETER.—A recent number of *Ciel et Terre* (April 2) contains an article on the moon's angular diameter, by M. P. Stroobant, of which a translation, with copious notes, appears in the *Observatory* for May. The methods employed in the determination of the apparent diameter of our satellite are (1) micrometer measures, (2) meridian passages, (3) heliometer measures, (4) photography, (5) occultation of stars by the moon, (6) eclipses. A comparison of the principal results obtained during the present century by these various methods indicates that occultations give the most accordant values, and M. Stroobant remarks that the method of occultation is the only one in which the apparent diameter of the moon is not augmented by physical or physiological causes. Accurate observations of occultations indicate that the lunar diameter has a value lying between 31′ 5″ and 31′ 6″, but M. Stroobant urges that this approximation is not sufficient. He concludes by saying:—"The application of photography to the determination of the exact instant of disappearance or of reappearance of a star would permit, without doubt, the attainment of great precision, especially when these phenomena occur at the dark limb of the moon, or during eclipses, when a number of small stars can be observed. . . . About every nineteen years the moon passes over the Pleiades in conditions more or less advantageous for observation; this phenomenon will occur next year. Might not the occasion be profitably used in securing a number of photographs at various observations? If these are sufficiently separated from one another, it would be possible to deduce a new value for the parallax of the moon."

GALE'S COMET.—This comet, for which we gave an ephemeris last week, is now very favourably placed for observation in the northern hemisphere. Its track lies from a point near ζ Hydræ (May 7) to near ζ Leonis (May 15). From South Kensington we have received the following report:—"The comet has been clearly visible to the naked eye for some days past, and when viewed with an opera-glass is quite a conspicuous object. Observed with the telescope it appears as a large slightly elongated nebulous mass with a central condensation, but with no obvious tail. The spectrum of the comet was observed by Mr. Fowler on May 7, and was seen to consist of the three carbon bands which have so frequently been recorded in other comets. The bands were found to be coincident with the corresponding bands seen in the spectrum of the blue base of a candle flame, at approximate wave-lengths 4736, 5165, and 5635. There was also a fairly bright continuous spectrum from the nucleus."

DENNING'S COMET.—M. L. Schulhof (*Astr. Nach.* 3227) has computed an elliptic orbit for the comet found by Mr. Denning on March 26, as the parabolic elements previously determined did not satisfy the observations. The period of the comet appears to be 6·745 years. According to the criterion published by M. Tisserand some time ago, the comet is identical with either Grischow's comet (1743 I.) or Blanpain's (1819 IV.), or it may be with both, for the identity of these two objects is admitted by some astronomers. M. Schulhof points out that it is desirable that Denning's comet, which is fading rapidly, should be followed so long as possible with large telescopes. Periodic comets can only throw light upon some obscure points in celestial mechanics and cosmogony when they have been observed during several apparitions. An ephemeris extending to May 15 will be found in NATURE, vol. xlix. p. 586.

STARS HAVING PECULIAR SPECTRA.—In *Astronomische Nachrichten*, No. 3227, Mrs. Fleming gives a list of five faint objects having spectra of Type V., that is, of bright lines, discovered from an examination of photographs of stellar spectra, taken at the Peruvian Station of the Harvard College Observatory, under the direction of Prof. S. J. Bailey. This brings the list of bright-line stars up to sixty. Two new nebulæ have also been found by means of the photographs of their spectra. The positions and descriptions of the objects are stated as follows:—

R.A. 1900. h. m.	Decl. 1900. °	Description.
13 46·5	−66·1	Type V.
15 10·0	−45·17	"
17 11·8	−34·18	"
17 18·2	−43·24	"
17 38·2	−46·3	Gaseous nebula.
18 39·3	−33·27	Type V.
19 10·5	−39·47	Gaseous nebula.

THE IRON AND STEEL INSTITUTE.

ON Wednesday and Thursday of last week, the 2nd and 3rd insts., the annual spring meeting of the Iron and Steel Institute was held at the Institution of Civil Engineers; the President, Mr. E. Windsor Richards, occupied the chair. The following is a list of the papers set down for reading and discussion:—

"On the Physical Influence of certain Elements upon Iron." By Prof. A. O. Arnold.

"On the Capacity and Form of Blast Furnaces." By William Hawdon.

"On Scandinavia as a Source of Iron Ore Supply." By Jeremiah Head.

"On the Walrand Process." By G. J. Snelus.

"On the Results of Heat Treatment on Manganese Steel and their Bearing upon Carbon Steel." By R. A. Hadfield.

"On the Analysis of Steel." By H. K. Bamber.

"On the Application of Electricity as a Motive Power in the Iron and Steel Industries." By D. Selby-Bigge.

"On Methods of Preparing Surfaces of Iron and Steel for Microscopic Examination." By J. E. Stead.

"On the Relations between the Chemical Constitution and Ultimate Strength of Steel." By W. R. Webster.

The last four were taken as read. The usual formal proceedings having been transacted, the Bessemer gold medal for 1894 was presented to Mr. John Gjers, of Middlesborough, in recognition of his great services to the iron and steel industry.

The President then proceeded to deliver his address, which dealt chiefly with the economic side of iron and steel production. This industry appears to be passing through a period of extreme depression, more pronounced even than that of 1885. In the latter year the production of Bessemer steel rails was 706,583 tons. That year was designated at the time as a period of great depression, but in 1893 the production of rails was but 579,386 tons, whilst in 1892 the output was 43,550 tons lower even than in 1893. The price of these rails, which in 1886 was £4 13s. 10d. per ton, fell as low as £3 12s. in 1893. The question arose, the President said, whether this diminished demand was due to any falling off in quality of material, excellency in finished products, or increased cost of manufacture. From careful observations which he had made, Mr. Windsor Richards was convinced that our metallurgists and manufacturers still keep a foremost position. The loss of the continental trade was due solely to protective tariffs, and even the importation of continental rails was to be attributed to the same source, strange as it might seem. The reason for this is that to produce steel economically, it is necessary that it should be made in large quantities; in consequence of the protective tariff the continental manufacturer is freed from foreign competition at home, and can therefore obtain an exorbitant price for his goods. This enabled him to sell in foreign markets, where he had to meet competition, at a lower price than those who had not the same lucrative home market. In fact it was necessary to produce largely, and the surplus quantity could in this way be sold at what would otherwise be a loss. In face of these facts, the President said it was useless to expect relief by resource to labour-saving machinery and other methods of cheapening cost, and it was to be remembered that the foreign manufacturers could take these up as readily as we could. Technical education, he also seemed to think, would be powerless to avail us against the conditions he had pointed out. "Never," said the address, "since the organisation of this Institute (a period it may be mentioned of over twenty-five years) has the metallurgist experienced a more difficult time than the depression we are now passing through. Added to his commereial troubles were constant demands from the workmen for either higher wages or fewer hours of work. We may well anxiously look round to see where markets for our products, and employment for our workmen and capital are to come from." Some English steel makers have been building hopes on the relaxation of the American tariff, but these hopes the President looked on as fallacious, and indeed the United States steel makers have been passing through a period of greater depression than even we ourselves in this country. It is to our colonies, therefore, that Mr. Richards tells us we must look for relief, and he points out the vast field there is for the further development of rails in India, Australia, and Africa. The introduction of steel for rails has not proved an unmixed blessing for the iron and steel manufacturer. The President quoted an instance in which Goliath rails of 105 lbs. per yard had been laid down five years ago on a continental railway, and it was shown that on the basis of the wear already observed during those five years, such rails would last a century. The carbon in the steel was from ·4 to ·5. Rails are being laid down even harder than this, containing from ·6 to ·7 carbon. The extreme hardness obtained in this way entailed, the President said, an unnecessary risk. The address next went on to speak of the uncertainty of phosphorus analyses, and to the desirability of dealing with steel in large masses, in the ingot. He stated that Messrs. John Brown and Co., Sheffield, are having constructed a forging press for steel ingots, which will exert a force of 1000 tons, whilst ingots 6 ft. 9 in. square, and weighing up to 70 tons, are being dealt with by the forging press, the appliance used in handling them having a capacity of 100 tons.

The first paper read was by Mr. G. J. Snelus, and was on the Walrand-Legénisel process for steel castings. This process consists of adding to the metal in the converter at the end of the ordinary blow a definite quantity of melted ferro-silicon, then making the after-blow, turning down when the extra silicon has been burned out, and adding the ordinary final additions of ferro-manganese, &c., as circumstances required. The advantages of this process are that firstly an ordinary Bessemer pig can be used with 2 to 3 per cent. silicon, thus insuring a steel perfectly free from carbon; secondly, the combustion of the added silicon produces such a large amount of heat at the right time, and so rapidly that the metal becomes very fluid; the third advantage claimed is that as the silicon burns to a solid, it leaves the metal perfectly free from gas, and the steel is sound and free from gas cavities; fourth, that in consequence of the metal being so fluid and already free from oxide of iron, the ferro-manganese or other substances added, such as aluminium, are more effective and remain in the final steel. Another advantage secured by this process is that in consequence of the fluidity of the metal much more time and facility is given for casting operations. The author gave detailed descriptions of experiments he had seen made with this process, and quoted figures in support of his contentions. The system of casting is, however, confessedly expensive, and it would seem to be more especially suitable for those engineering works where it is desirable to have a steel foundry attached, and in which the demand would naturally not be so continuous as in the case of an establishment devoted entirely to the production of steel castings. It may be stated that the price of steel as it stands in the ladle is given as 4s. 6d. per cwt., whilst the cost of a complete installation of moderate size would be about £3500. In calculating the cost of the steel in the ladle, the author appears to leave out the fixed expenses. It is doubtless a tempting thing to the managers of engineering workshops to have their own steel foundry, especially as it is often difficult to obtain castings with promptness and punctuality, the advantage of producing all parts required at home, and thus having control of delivery, is apparent. It is very easy, however, to carry this principle too far. The time of a works manager is limited, and without the master's eye there is likely to be much leakage in a department. Manufacturing establishments may be too self-contained, and there are many unfortunate instances of works producing everything required, excepting dividends.

A short discussion followed the reading of this paper, those who spoke being altogether favourable to the process. Unfortunately the large number of papers that were on the list made the President fearful that the whole programme would not be carried through in the two days, and he therefore closed this first discussion very abruptly. Had he not done so we believe that the discussion would not have been throughout of so flattering a description.

The next paper read was a contribution by Mr. Jeremiah Head, entitled "Scandinavia as a Source of Iron Ore Supply." Mr. Head has recently made a tour through Norway and Sweden, going to the extreme north of the Scandinavian Peninsula, and in his paper he discussed the iron-producing capabilities of these countries, of which he appears to take a somewhat sanguine view. He pointed out that in the case of export duty being placed on iron ore by the Spanish Government, the steel makers of this country might be put in an awkward position, depending as they did so largely on Bilbao ore. Some of the experienced steel makers present, however, by no means agreed with Mr. Head in his estimate of the value of Scandinavian ore. It would appear that until a railway is constructed to the Norwegian coast, which, unlike the Baltic, is free from ice at all times of the year, there is not much prospect of a continuous supply of ore being obtained from northern Scandinavia. The objection that for half a year there is almost continual night in this district, was, Mr. Head said, an imaginary one, the fact being that the Scandinavians carry on their business all through the year without trouble. The brilliant moonlight, the Northern Lights, and the twilight that exists, aided by the reflection from the snow-covered country, enables work to be transacted. Mr. Head's paper contained a great deal of useful information on the subject, analyses of the ores being given, and figures as to the cost, &c.

On the second day of the meeting the proceedings were opened with a paper by Mr. William Hawdon, on "The Capacity and Form of Blast Furnaces." The author commenced with some interesting figures on the increase in capacity of blast furnaces; in Cleveland during recent years the content has risen from about 6,000 cubic feet to as much as 30,000 cubic feet, with the result of increased economy and larger output. In discussing the proportions of furnaces, he pointed out that the crucible or well of the furnace, that is the part immediately above the hearth, has its diameter governed by two considerations: if it be too large, a pillar of perfectly cold material may be formed in the centre of the mass of ore, fuel, &c., contained in the furnace; whilst if the diameter were too small, there would not be sufficient space to give the required volume for combustion in order to obtain a given output. The melting zone above the crucible must also be designed so as to allow an easy penetration of the blast through the materials. When air is blown into a furnace it has to be expanded by the expenditure of heat, but if air be introduced at a high temperature and already in an expanded state, a more rapid combustion is obtained with a saving of fuel in the furnace. In the case of cold blast being used, intensity of combustion does not spread over a large space, and therefore a smaller well suffices. High temperature of blast requires a larger area in the neighbourhood of the tuyeres, through which it is admitted. It is necessary that the furnace materials should come down from the upper reaches thoroughly heated and reduced, and in as level a manner as possible over the entire area. In order to obtain capacity and to support the material, and also to prevent too dense packing near the tuyeres at the zone of fusion, the blast furnace is made with boshes; that is to say, the interior space enlarges suddenly, the walls taking a slope of 60° to 80°, the angle of repose for dry materials being about 45°. But when the minerals become plastic, the angle of the bosh requires to be more steep. Above the slope of the boshes there is the maximum diameter of the barrel of the furnace. When, owing to the relative sizes of the wall and the barrel the bosh occupies a large vertical space, thus retiring a long way back, the materials at the sides are too far removed from the ascending current of gas, and will come down in a perfectly raw state. The author gave an amusing example of this error in furnace construction. In one case, after a few months' working it was discovered that some wooden sleepers that were originally placed in to light up the furnace, had not been consumed, and were in fact found only charred on the surface, resting near the top of the bosh of the furnace. In order to get over the somewhat conflicting conditions we have here referred to, Mr. Hawdon and his friend Mr. Howson had designed a furnace of comparatively narrow dimensions, but enlarged at the upper part, thus giving, as it were, a second bosh. In this way in the higher region where the charge is in a dry and porous state and not subject to extreme pressure, capacity is obtained, whilst the direct weight upon the lower portions of the materials is reduced. With a furnace of this nature, which has been in work some short time at the Newport Iron Works, the author obtained in smelting hematite a fuel economy of 15 cwts. of fixed carbon per ton of iron, the weekly output being 932 tons, the ore being 50 per cent. One great advantage in the use of this form of furnace would appear to be regularity of the product, freedom being obtained from that uncertain recurrence of white iron which is so often a trouble to the blast furnace manager. It should be remembered that the furnace had not been in work for any considerable time, and new furnaces nearly always work better than when they have been in blast a few years. On the whole, however, it would seem that Mr. Hawdon has made out a very good case for his new form of furnace, and indeed the promises are so good that doubtless many more will be erected on these lines.

The remaining paper read at the meeting was by Prof. J. O. Arnold, of Sheffield, and was entitled "The Physical Influence of Elements on Iron." We approach this paper with despair. In the first place, it was one of extreme length and is full of facts from cover to cover. In the second place, the discussion which followed its reading was of such a nature that many of the speeches which had been prepared beforehand, and were read by their authors, were really of the nature of papers in themselves. Indeed one speaker, Mr. Hadfield, of Sheffield, had prepared a paper of some length which had been called forth by Mr. Arnold's monograph, and extracts from this were read by the author during the discussion. M. Osmond, Prof. Roberts-Austen, Sir Lothian Bell, and Mr. Gowland had also prepared what in effect were separate monographs on the subject; whilst Mr. Stead, of Middlesborough, spoke at considerable length. In addition to these there were several other speakers. We could not abstract Prof. Arnold's paper in anything approaching the space we have at our disposal here, important and interesting as the subject is; and even could we do so, it would be hardly fair to those who took opposite views to him, as we cannot reproduce their arguments. Under these circumstances we must content ourselves with giving the very briefest idea of the subject, referring our readers to the *Transactions* of the Institute for full information. It will be remembered that at a meeting of the Institute of Mechanical Engineers, Prof. Arnold made a very strong attack upon the report presented by Prof. Roberts-Austen as chairman of the Alloys Research Committee of that Institution. Prof. Roberts-Austen in his report adopted the theories brought forward by M. Osmond in regard to the critical points, or evolutions of heat during the cooling of mild steel, from a temperature of 1000° C. These critical points were:—firstly, the slight evolution of heat at 850° C. This point is known as Ar_3. Secondly, a faint disengagement of heat at about 750° C., the point Ar_2, the third point Ar_1, is at about 650°; the latter is almost absent in very mild steel, but becomes highly accentuated in steels high in carbon, and was therefore due to a combination of iron and carbon to form the definite carbide Fe_3C. M. Osmond maintained, what Prof. Arnold designated the "startling theory," that the point Ar_3 marked the vital change of the passage into ordinary soft iron of an allotropic modification of iron (existing at temperatures above the critical point) of adamantine hardness. This allotropic form M. Osmond named β iron to distinguish it from α or soft iron. He further stated that the hardness conferred upon tool steel when plunged at a good red heat into cold water was due, not to carbon, but to the presence of β iron, rendered stable at low temperatures on being suddenly chilled in the presence of carbon, the last-named element, as such, possessing a comparatively insignificant hardening influence. M. Osmond also said that an investigation made on a series of alloys had verified Prof. Roberts-Austen's law that the influence of elements on iron is in accordance with the periodic law. These, briefly, are the points on which Prof. Arnold joined issue; and in order to support his contention, he has made a vast number of experiments which he claims, if we understood him correctly, entirely upset the theories of M. Osmond and Prof. Roberts-Austen.

The paper by Mr. Hadfield, to which we have referred, is entitled "The Results of Heat Treatment on Manganese Steel and their Bearing upon Carbon Steel." Mr. Hadfield's connection with that remarkable alloy of iron known as manganese steel is well known, and the great difficulty with which it is magnetised renders it especially interesting in connection with this subject. During the discussion Mr. Hadfield showed that manganese steel may be made magnetic; in fact he produced a bar which was distinctly affected by the magnet at one end, whilst at the other end there were no magnetic properties. We must, however, refer our readers to the *Transactions* for the many interesting details contained in this paper. The meeting terminated with the usual votes of thanks.

The summer meeting this year will be held in Belgium, commencing on Monday, the 20th August, when members will assemble in Brussels. The meeting will extend until the following Friday, so as to give members an opportunity to travel home on the Saturday.

THE ROYAL SOCIETY'S CONVERSAZIONE.

THE first (or gentlemen's) soirée of the Royal Society took place on the evening of May 2, in the Society's rooms at Burlington House. There were numerous exhibits, and it will be seen from the following summary that most branches of science contributed evidences of progress.

Prof. Hunter Stewart and Mr. Henry Cunynghame exhibited apparatus for micro-photography.

Experiments in persistence of vision were shown by Mr. Eric S. Bruce.

Mr. J. Theodore Bent exhibited antiquities and anthropological objects from the Hadramoot, Southern Arabia.

Two models of the South Lodge Camp, Rushmore Park, Wiltshire, an entrenchment of the Bronze age, before and after excavation, with the relics therefrom, were shown by General Pitt-Rivers; and also two models of the Handley Hill entrenchment before and after excavation, on the same scale as the South Lodge Camp, with the relics therefrom.

New Dicynodont reptiles from South Africa were exhibited by Prof. H. G. Seeley; and a skull of Deuterosaurus.

Mr. Richard Kerr showed an ovate palæolithic implement and two molar teeth of *Rhinoceros tichorhinus*, found by him in brick-earth at St. John's-road, Radnor Park, Folkestone, in August 1893.

Chemistry was represented by Dr. J. H. Gladstone's exhibit of early specimens of partly soluble cotton xyloidin, and of Austrian gun-cotton for military purposes. In 1847 the exhibitor prepared xyloidin from starch and from cotton. His specimens have all spontaneously decomposed, except those shown, which are mixtures of the soluble cotton xyloidin and ordinary gun-cotton.

Some maps and plans which accompany the Report on Nile Reservoirs, recently published by the Egyptian Government, were exhibited by Prof. J. Norman Lockyer.

Mr. J. Wimshurst exhibited models showing an improved method of communication between shore stations and light-ships, or other like purposes.

Mr. R. E. Crompton showed an electrically heated altar and electrically heated soldering bits for soldering and brazing; and a potentiometer, to measure electromotive forces, from ·0001 to 1500 volts, correctly to 1–2000; and Sir David Salomons showed some new phenomena in "vacuum tubes."

Mr. Owen Glynne Jones exhibited his absolute and relative viscosimeters.

Prof. Roberts-Austen's exhibit comprised an ink-recording pyrometer, consisting of a thermo-junction of platinum and platinum iridium attached to a dead-beat galvanometer, and a series of pyrometric curves obtained by photographic recorders in different iron works, and showing the temperature of the hot blast used in smelting iron.

Mr. A. E. Tutton exhibited an instrument of precision for producing monochromatic light of any desired wave-length, and an instrument for grinding section-plates and prisms of crystals of artificial preparations accurately in the desired directions. (Both these instruments are described in NATURE, vol. xlix. p. 377.)

Dr. Karl Grossmann and Mr. J. Lomas exhibited crystals of ice (hexagonal hopper) and photographs.

Dr. Karl Grossmann showed some specimens of Obsidian from Iceland. The specimens were brought by the exhibitor from the Hrafntinnuhryggur in Iceland (N.E.) The large specimen showed *conchoidal fracture*, evidently produced on falling from a cliff. The smaller specimen shows *flow structure*.

A twin-elliptic pendulum and pendulum figures were exhibited by Mr. Joseph Goold; and a glass model, showing a method of transmitting force by spheres or discs, by Mr. Killingworth Hedges.

An exhibit which attracted much attention was M. Moissan's electric furnace, and specimens of chemical elements obtained by means of it: vanadium, chromium, molybdenum, tungsten, uranium. The furnace consists of a parallelopiped of limestone, having a cavity of similar shape cut in it. This cavity holds a small crucible, composed of a mixture of carbon and magnesia. The electrodes are made of hard carbon, and pass through holes cut on either side of the furnace, meeting within the cavity. For the purpose of certain experiments a carbon tube was fixed in the furnace at right angles to the electrodes, and so arranged as to be 10 mm. below the arc, and about the same distance from the bottom of the cavity. This tube contains the material to be heated, and by inclining it at an angle of about 30° the furnace may be made to work continuously, the material being introduced at one end of the tube and drawn off at the other. A temperature of about 3500° C. is produced. The metals are reduced by heating a mixture of their oxides with finely divided carbon, and for this purpose a current of about 600 ampères and 60 volts is employed. M. Moissan has not only succeeded in reducing the most refractory metals, but has fused and volatilised both lime and magnesia. Nearly all the metals, including iron, manganese, and copper, have also been vapourised, whilst by fusing iron with an excess of carbon, and then quickly cooling the vessel containing the solution of carbon in molten iron by suddenly plunging it into cold water, or better into a bath of molten lead, he has been successful in producing small colourless crystals of carbon, identical in their properties with natural diamonds.

A new harmonic analyser was exhibited by Prof. Henrici. This analyser differs from that shown last year by an improved integrating apparatus. The maker, Herr G. Coradi, of Zürich, has introduced a glass-sphere, whereby all *slipping* has been avoided, and greater compactness has been obtained. The instrument exhibited gives only one term (two coefficients) in Fourier's expansion at a time, but on going six times over the curve to be analysed as many terms can be obtained. There is no difficulty in introducing more integrators in the same instrument, and one has been made which gives five terms on going once over the curve, and ten in going twice over it.

Callendar and Griffiths' long distance direct-reading electrical thermometers and pyrometers were shown by Mr. E. H. Griffiths; and a torsional ergometer or work-measuring machine, used in connection with a mechanical integrator and as an electrical governor, by the Rev. F. J. Smith.

Mr. Henry Wilde showed his magnetarium for reproducing the phenomena of terrestrial magnetism and the secular changes in its horizontal and vertical components, and a magnetometer for showing the influence of temperature on the magnetisation of iron and other magnetic substances.

Polyphase electric currents were illustrated by Prof. Silvanus P. Thompson, with models and experiments.

The Marine Biological Association contributed living pelagic larvæ, &c., from Plymouth, examples of the echinoderm fauna of Plymouth, and a hybrid between brill and turbot.

Mr. Henry A. Fleuss showed a mechanical pump for the rapid production of very high vacua, and vacuum tubes exhausted by it; and Mr. H. N. Dickson his charts and sections showing the temperature of the water in the northern and western parts of the North Sea and the Faroe-Shetland Channel at all depths, August 1893.

Dr. H. R. Mill and Mr. Edward Heawood exhibited bathymetrical maps of Windermere, Ullswater, Coniston Water, Derwentwater and Bassenthwaite, Buttermere and Crummock, Ennerdale Water, Wastwater and Haweswater. Contour lines at each 25 feet of depth beneath the surface were shown, and the configuration of the basins was thus for the first time accurately delineated.

Prof. J. Norman Lockyer exhibited photographs of stellar spectra taken with a 6-inch objective prism of 45°, and photographs of the great sun-spot of February 1894, taken at Dehra Dun.

Living larvæ influenced by the colours of their surroundings were exhibited by Prof. E. B. Poulton; and microscopic slides illustrating the behaviour of the nucleus during spore formation in the hepaticæ, by Prof. J. B. Farmer.

Photographs of diffraction and allied phenomena were exhibited by Mr. W. B. Croft. The photographs showed Newton's rings, reflected and transmitted; Grimaldi's fringes; Fresnel's interference from a bi-prism; Arago's shifting of bands towards the denser medium; Talbot's bands. The shadows of needles, wire gauze, perforated zinc, a screen with circular holes, opaque circular screens with Arago's bright centre. A comparison of the diffraction of Fresnel with that of Fraunhofer and Schwerd; the diffracting object consists of groups of small circles of light. Uniaxal and biaxal crystals; conical refraction.

Specimens demonstrating some phenomena of chemiotaxis in inflammation were exhibited by Mr. W. B. Hardy and Dr. A. A. Kanthack.

Prof. Marshall Ward showed apparatus employed for observing and measuring the growth of bacteria, fungi, and other micro-organisms under different conditions under the microscope. The essential feature is the culture-cell. It has a quartz floor, and is capable of holding large quantities of water, and thus while letting the light-rays pass does not rapidly vary in temperature. By the side of the culture-cell containing the hanging-drop in which is the organism under observation, is an exactly similar cell, but with a small thermometer in it, the blackened bulb of which is in the cell, and gives the temperature *inside* the latter. The rest of the apparatus consists in the measuring eye-piece; the screens of coloured glass, various liquids, &c., for growth in different kinds of light; and a warm chamber in which the whole microscope can be enclosed and kept at known temperature.

A demonstration of the trails of Oscillatoriæ formed the exhibit of Mr. J. G. Grenfell.

Prof. E. Waymouth Reid exhibited microscopic specimens illustrative of the process of secretion in the skin of the eel. The chief point of general interest in the process is the peculiar manner in which the surface of the skin is cast off when the animal is stimulated.

Prof. G. B. Howes exhibited eggs and young of *Ceratodus Forsteri*, and a male of *Lepidosiren paradoxa*.

Mr. E. J. Allen showed nerve elements from the ganglia of lobster embryos; and Dr. D. Sharp a collection of white ants (*Termitidæ*).

A specimen and drawing of the South American mud-fish, *Lepidosiren paradoxa*, was exhibited by Prof. E. Ray Lankester. (See NATURE, vol. xlix. p. 555.)

Dr. Alexander Muirhead exhibited a new form of Lord Kelvin's siphon recorder, Muirhead's artificial cable, and Muirhead's automatic curb transmitter. Lord Kelvin's siphon recorder and Muirhead's automatic curb transmitter were shown in operation in connection with an artificial cable of the same capacity and conductor resistance as the Atlantic cable, which is to be laid next July by the Anglo-American Telegraph Company. Capacity of artificial cable 800 microfarads; resistance of conductor 3350 B.A. units.

Demonstrations by means of the electric lantern took place during the evening, Dr. D. H. Scott showing photographs from sections in Dr. W. C. Williamson's collection, illustrating the microscopic structure of fossil plants from the coal-measures. The lantern was also used by Prof. E. B. Poulton, who exhibited illustrations of recent work upon the influence of environment upon the colours of certain Lepidopterous larvæ. Variously coloured twigs and shoots, such as occur in nature, have been shown to influence the appearance of many twig-like larvæ in such a manner as to conceal them. During the summer of 1893 certain larvæ of two species (*Gastropacha quercifolia* and *Otontoptera bidentata*) were surrounded, during their growth, with lichen-covered twigs. Larvæ thus treated developed lichen-like marks upon the body.

THE RELATIVE SENSITIVITY OF MEN AND WOMEN AT THE NAPE OF THE NECK (BY WEBER'S TEST).

THE difference in the sensitivity of the two sexes has been discussed often and from various points of view, but still, as it would seem, upon insufficient data. More observations being wanted, I submit the following, partly for such value as they have in themselves, partly to show an easy method of observation which others may pursue with advantage, and partly as a good illustration of the method of percentiles, or centiles.

The test employed is one of a familiar kind, made with the points of a pair of compasses, and usually associated with the name of Weber. If one person becomes just conscious of the doubleness of the pricks when the distance between the points is a, and another person does so when the interval is b, then the ratio of a to b may fairly be taken to express the relative obtuseness of the two persons, so far as concerns the form of sensitivity tested, and the inverse ratio of b to a to represent its relative delicacy. The particular test used was one that has three especial merits: it requires no minuteness of measurement, no uncovering, and the person tested is unable to see the operation. It consists in pressing the points of the compasses against the nape of the neck and across the line of the spine, while the experimentee sits with his or her head bowed forward. The just-perceptible interval at the nape of the neck averages as much as half an inch or thereabouts, while its variation in different persons is large. Consequently there is no need for extreme delicacy of measurement, neither does the varying thickness of cuticle caused by various degrees of usage, interfere materially with the results, as it does when like experiments are made, as is usual, on the finger-tips. The varying delicacy of perception due to differing amounts of practice is here entirely eliminated, because all persons are equally unpractised, no one occupying himself or herself in attempts to discriminate between two simultaneous pressures on the nape of his or her neck, while everybody has life-long practice in discriminating roughnesses, though in various and unascertainable degrees, with his or her finger-tips. There are parts of the body, such as the back, which are still less discriminative than the nape of the neck, but there is no other equally suitable part that is so get-at-able, in respect to the the ordinary dress of man or woman. Lastly, the attitude of the person who is being tested, entirely precludes him from watching the operator, and guessing from the hands or movements of the latter, whether he is applying two points, or only one, at the moment when he asks what is felt. The observations were all made by Sergeant Randall, who superintends my laboratory; he employed the two points of a Flower's craniometer, which was handy for use, as it was wanted to make other measurements of the same persons. The observations were carried on for some months, until a sufficient number had accumulated to justify discussion. Stature was included among them, but, failing on examination to trace any notable relation between stature and the just-perceptible interval on the nape of the neck, I have disregarded stature altogether in the following summary, and age too, so far that the person tested was often not fully grown.

The observations made on males and females, respectively, are summarised in the first and third lines of Table I. Their sums, reckoned in each case from the beginning of the series, are entered in lines 2 and 4, while the percentages of those sums are given in lines 3 and 6, but solely for the purpose of graphic projection in the form of dots, in Fig. 1. Those dots are joined by straight lines, forming traces for the males and females respectively. The lengths of the ordinates to the traces, which are drawn at the 10th, 20th, &c. divisions of the base, are the 10th, 20th, &c. percentiles, or centiles; or, in still briefer language, the 1st, 2nd, &c. "deciles." Their values, obtained by simple interpolation from the entries in lines

1 and 2 of Table I., are entered in Fig. 1. Thus 13·8 millimetres is the just-perceptible interval of the median man, and 11·8 that of the median female. In one sense, but only in an imperfect one, the relative sensitivity of the two sexes is given by these figures as being about 7 to 6. Much more has, however, to be specified before the relation can be adequately expressed, because it is obvious from the diagram, that what is true for persons having medium sensitivity, is not true for those having high, and still less for those having low, sensitivity. We are, however, able to specify what is wanted very compendiously, because both of the traces conform fairly well to the law of frequency of error, at least between the limits of the 1st and the 9th decile. In the case of males, the median is taken at 3·50 millimetres, and the q (= half the difference between the two quartiles, a value which is identical with that of the ± *probable error* of a single observation) at 3·25; in females, the corresponding values are 12·00 and 3·70. A somewhat nearer approximation to the observed values could be obtained by closer attention to the second decimals, but such minuteness is uncalled for in a case like this. It will be seen from columns B, C, and b, c, in Table II., that the sums of the observed and calculated deciles closely accord, and that the differences between the several pairs of them, headed B—C and b—c, are as nearly alike as we have a right to expect. The calculated deciles, and the curves drawn through them, in Fig. 2, may therefore be accepted as a just rendering of what is more roughly indicated by the observations in Table I. and Fig. 1. In the following remarks reference will be made almost exclusively to the calculated values, but the results can and will usually be checked by reference to the observed ones, with which they tally sufficiently well.

TABLE I.

Summary of Observations of 932 Males and 377 Females, showing the number in whom a just-perceptible feeling of doubleness was given by the pressure of two points across the nape of the neck, and separated by the various intervals, as below.

—	5 and under.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.	20.	21.	22.	23.	24.	25 and above.	Total observed.
	Length of the just-perceptible interval in millimetres																					
Number of Males . . .	32	19	23	41	49	84	56	58	46	71	116	67	65	44	32	35	26	23	10	6	29	932
Sums from beginning . .	32	51	74	115	164	248	304	362	408	479	595	662	727	771	803	838	864	887	897	903	932	
Reduced to per-cents. . .	3	5	8	12	18	27	33	39	44	51	64	71	78	82	87	90*	93	95	96	97	100	
Number of Females . .	23	15	23	23	32	30	24	23	19	27	28	23	15	5	16	18	7	7	5	4	10	377
Sums from beginning . .	23	38	61	84	116	146	170	193	212	239	267	290	305	310	326	344	351	358	362	367	377	
Reduced to per-cents. . .	6	10	16	22	31	39	45	51	56	63	71	77	81	82	87	91*	93	95	96	98	100	

* These figures are protracted in both cases as 90·5, inasmuch as the accordance of the two preceding and of the four subsequent entries make the correction reasonable as well as convenient.

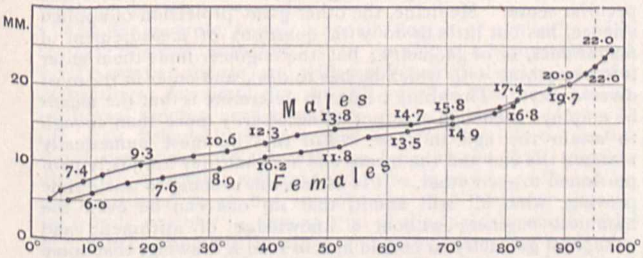

FIG. 1.—Traces and deciles from observations. The dots refer to the observed values as given in the 3rd and 6th lines of the table. They are connected by straight lines. The figures are the values of the corresponding deciles—that is, of the ordinates to the traces erected at each successive tenth part of the base.

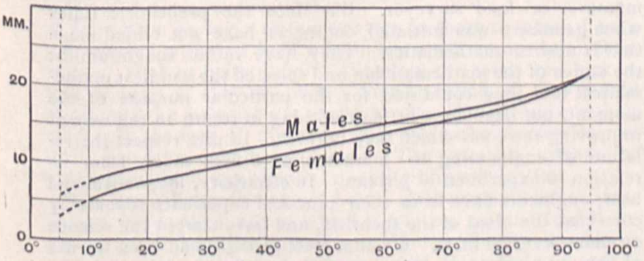

FIG. 2.—Deciles and curves by calculation.
Data for males: median = 13·50, q = 3·25 mm.
Data for females: median = 12·00, q = 3·70 mm.

TABLE II.

Grade or rank in a series of 100.	Males. Rank in a series of 932.	Deciles. By observation.	Deciles. By calculation from median = 13·50, quartile = 3·25.	Differences between observation and calculation.	Females. Rank in a series of 377.	Deciles. By observation.	Deciles. By calculation from median = 12·00, quartile = 3·70.	Differences between observation and calculation.
	A.	B.	C.	B—C.	a.	b.	c.	b—c.
°		m.m.	m.			m.m.	m.	
10	93	7·4	7·3	+0·1	38	6·0	5·0	+1·0
20	186	9·3	9·4	−0·1	75	7·6	7·4	+0·2
30	280	10·6	10·9	−0·3	113	8·9	9·1	−0·2
40	373	12·3	12·2	+0·1	151	10·2	10·6	−0·4
50	466	13·8	13·5	+0·3	189	11·8	12 0	−0·2
60	559	14·7	14·8	−0·1	226	13·5	13·4	+0·1
70	652	15·8	16·1	−0·3	264	14·9	14·9	0·0
80	746	17·4	17·6	−0·2	302	16·8	16·7	+0·1
90	839	20·0	19·7	+0·3	339	19·7	19·0	−0·7
Sums	—	121·3	121·5	—	—	109·4	108·1	—

The average ratio between the sensitivities of the females and males is the same as that between the sums (or means) of columns C and c in Table II., namely as 125·5 to 108·1; or to speak more modestly, as no trust can be reposed on the minute precision of such values as these, the average delicacy of female discrimination between the two points is to that of the male, in a ratio that lies somewhere between 7 to 6 and 8 to 7, or thereabouts. It will be recollected that the former ratio was that between the median female and the median male.

The *variability* of the discriminative power appears from the observations to be distinctly higher among females than among males. Measuring it in the usual way, by the half difference between the two quartiles, which is the same thing as the *probable error* of a single observation, or else by any multiple of this, as by the *mean* error, we find that the variability among females is to that among males as 3·70 to 3·25, say as 8 to 7. It is in consequence of this that so large a difference is shown between the relative sensitivity of the two sexes, at the right and left extremes of their respective curves in Fig. 2. We find from Table II. that the value of C—*c* at the 1st decile is 2·3 millimetres, and at the 9th decile it is only 0·7, the differences between the intermediate pairs decreasing regularly. The regularity of the decrease is not apparent in the actual observations, as shown in Fig. 1, nor in Table I., still there is nothing in what we see there that is incompatible with Fig. 2, while the fact of the difference between the right ends of the traces being much less than that between the left, is conspicuous.

Is it, however, a physiological fact that women are more variable than males in respect to discriminative touch, or are the observations affected by any extraneous cause of variability? I think that the recorded variability may in a very small part be accounted for by the fact that women vary much more than men in the exercise of sustained attention. Carelessness would affect the results in the same direction as diminished sensitivity. Thus suppose one part of a large number of persons who were all really alike in sensitivity, to be very careless, and the remainder to be scrupulously careful; the minds of the careless would be apt to wander; they would then fail to notice the first just-perceptible sense of doubleness, and would appear, in consequence, to be more obtuse than the careful ones. Though the range of variability was in reality *nil*, the existence of carelessness would introduce variability into the records. Some women are religiously painstaking, as much so as any men; but the frivolity of numerous girls, and their incapacity of, or unwillingness to give, serious attention, is certainly more marked than among men of similar ages. Women may, however, be really more variable than men in respect to sensitivity, because they seem more variable in a few other respects, such as in stature and obesity. Many more very tall girls are to be seen now-a-days among the upper classes than formerly, but the run of the statures among men has not altered quite so much. The multitude of extraordinarily obese women who used to frequent Vichy for the cure of fatness, were wonderful to behold; but they are no longer to be seen in their former abundance, as the fashion of treatment has changed within recent years. Again, it appears that women vary much more widely than men in respect of their morality; to which assertion I would quote Tennyson as a corroborative witness, who writes as follows, in Merlin's soliloquy on the character of Vivien:—

> "For men at most differ as heaven and earth,
> But women best and worst as heaven and hell."

Since Fig 2 is true to scale, it is easy to utilise it for ascertaining the class-place of any man or woman in respect to the form of sensitivity now in question. The whole process would be as follows:—Take a pair of compasses, and find with them by experiment the just-perceptible interval across on the nape of the neck of the person tested; then apply the compasses, to Fig. 2, keeping one (the lower) of its points always on the base line of the Fig., and holding the compasses so that the line joining its points shall be perpendicular to that base line. Slide the lower point of the compasses along the base line until the upper point touches the male or female trace, as the case may be; then read off the grade at which the lower point stands on the base line. Suppose it to be 35°; we thereby learn that 35 per cent. of the same sex have more sensitivity than the person tested, and that 65 per cent. have less. Similarly for any other value.

It would, I think, be well worth the while of an inquirer to repeat these tests, to revise my results, and to pursue the subject much further. If any one should feel disposed to do so, I would suggest that he should make his measurements with the cheap form of bow compasses, in common use by carpenters. The legs are connected not by a joint, but by a spring which tends to separate them, and they are brought together to any desired interval by turning a screw with the finger and thumb, which overcomes the spring. The interval between the points could easily be measured on a separate scale; all the more easily, if there were a slight depression at the zero point of the scale, in which one leg might be securely rested.

FRANCIS GALTON.

THE RELATION OF MATHEMATICS TO ENGINEERING.[1]

MATHEMATICS has been described in this room as a good servant but a bad master. It will be my duty this evening to prove by suitable illustration the first half of the proposition, and to show the service mathematics has rendered and can render to engineers and engineering.

In our charter the Institution of Civil Engineers is defined as "A society for the general advancement of Mechanical Science, and more particularly for promoting the acquisition of that species of knowledge which constitutes the profession of a Civil Engineer, being the art of directing the great sources of power in nature for the use and convenience of man, as the means of production and of traffic in states both for external and internal trade, as applied in the construction of roads, bridges, aqueducts, canals, river navigation and docks, for internal intercourse and exchange, and in the construction of ports, harbours, moles, breakwaters and lighthouses, and in the art of navigation by artificial power for the purposes of commerce, and in the construction and adaptation of machinery, and in the drainage of cities and towns." No better definition can, I think, be found for our profession than that it is the art of directing the great sources of power in nature for the use and convenience of man. It covers all that the widest view of our work can include, and it excludes those applied sciences, such as medicine, which deal with organised beings. Mathematics has to deal with all questions into which measurement of relative magnitude enters, with all questions of position in space, and of accurate determination of shape. Engineering is a mathematical science in a peculiar sense. Medicine, the other great profession of applied science, has but little to do with questions of measurement of magnitudes, or of geometry; but the engineer finds them enter into everything with which he has to deal, and enter in the most diverse ways. The thing he has to determine is that the means he employs is enough and not unnecessarily more than enough to attain the end in view. For this he must numerically measure the end and the means and see that they are justly proportioned to each other. It is useless this evening to waste time proving, what all will admit, that no one can be even the humblest engineer without a knowledge of arithmetic and enough of geometry to enable him to read a drawing, that some trigonometry, some rational mechanics and a knowledge of projections, is a very useful part of the mental equipment of a draughtsman. It is hardly necessary to call attention to the great economy in the labour of calculations effected by the use of logarithms, a mathematical instrument for which we are indebted to Napier. We may with more profit examine what use the higher mathematics can be to the practical engineer, and what has been done in the past for engineering by its aid.

Judging from etymology, mathematics must have been begun by engineers; for surely geometry is the work of the earth measurer or land surveyor. But since the prehistoric times when geometry was initiated, engineers have not added much that is new to mathematics. They have rather sought amidst the stores of the mathematician and selected the handiest mathematical tool they could find for the particular purpose of the moment, but have done little or nothing in return in the way of improving the tools which they borrow. In this respect the relation of engineering to mathematics differs much from its relation to experimental physics. In electricity, magnetism and heat, engineers have from their practical experience repeatedly corrected the ideas of the theorists, and have started the science on more accurate lines. If our subject to-night had been the use of the practical applied science of engineering in promoting the development of pure mathematics, we should speedily find that there was hardly any material for discussion. The account being all on one side, let us see to what the debt of the engineer to the mathematician amounts.

There is no department of practical engineering in which the

[1] The "James Forrest" Lecture, delivered at the Institution of Civil Engineers, by Dr. John Hopkinson, F.R.S., on May 3.

application of mathematics is more familiar than in that which relates to the calculation of the strength and rigidity of structures of various kinds. It is impossible to take up any book dealing with the subject without finding that it is crammed either with mathematical formulæ, or with geometrical figures. The question is not whether mathematics is necessary to an adequate comprehension of the subject, but whether analytical or purely geometrical methods are more convenient. Of course one might occupy many lectures in discussing the practical application of mathematics to the question of bridge building, roofs, guns, shafting, and the like. Our object must be to illustrate by various examples rather than to attempt anything like a complete discussion.

Consider the case of a long strut, so long that its transverse dimensions can be regarded as insignificant in comparison with its length. Whilst the strut remains perfectly symmetrical about its middle line, its strength will depend only upon the resistance of the material to crushing. Everyone knows that this would be an inadequate conclusion; we have to consider another element, namely, its stability, that is, we must examine what will happen to the strut if from any cause it is displaced somewhat from the direct line between its extremities. A mathematical discussion of the question results in a differential equation of the second order with one independent variable. Upon consideration of this we are enabled to see that if the thrust upon the strut be less than a certain critical value, a slightly bent strut will tend to return to its straight condition; but that if the thrust upon the strut be greater than this critical value the displacement will tend to increase, and the strut will give way. Further, that the critical value will depend upon whether one or both of the two extremities of the strut are held free, or whether they are rigidly attached by flanges or otherwise, so that the direction of the axis of the strut at this point must remain unaltered. Again, we infer that if the ends are held rigidly fixed, the length of the strut may be twice as great for a given critical value of the thrust as if the two ends are free to turn. We can also infer what the critical value will be for struts of various lengths and of varying cross sections. This critical value depends not upon the resistance of the material to crushing, but upon its rigidity.

Another example, having a certain degree of similarity with the case of struts, is that of a shaft running at a high number of revolutions per minute, and with a substantial distance between its bearings; for simplicity, we will suppose that there are no additional weights, such as pulleys, upon the shaft. How will the shaft behave itself in regard to centrifugal force as the speed increases? In this case, so long as the shaft remains absolutely straight it will not tend to be in any way affected by the centrifugal force, but suppose the shaft becomes slightly bent, it is obvious to anyone that if the speed be enormously high this bending will increase, and go on increasing until the shaft breaks. In this case also we may use mathematical treatment; we find that the condition of the shaft is expressed by a differential equation of the fourth order, and from consideration of the solution of this equation we can say that if the speed of any particular shaft be less than a certain critical speed, the shaft will tend to straighten itself if it be momentarily bent, but that, on the other hand, if the speed exceeds this critical value, the bending will tend to increase with the probable destruction of the shaft. I do not know that either of these two questions can be properly understood without some knowledge of differential equations.

A problem having a certain analogy to those to which I have just referred is that of hollow cylinders under compression from without, such as boiler tubes. Whether the tubes be thick or thin, so long as they are perfect circular cylinders, they should stand until the material was crushed. But if the tubes are thin, what will happen if the tube from any cause deviate ever so little from the cylindrical form? The solution cannot be obtained without a substantial quantity of mathematics.

The next illustration shows how a mathematical conclusion, correct within the limits to which it applies, may mislead if applied beyond those limits, and how a more thorough mathematical discussion will give a correct result. Considering a case of shafting in torsion it was shown by Coulomb that the stiffness and strength of a shaft having the form of a complete circular cylinder could be readily calculated if the transverse elasticity of the material and its resistance to shearing were known. From the complete symmetry about the axis it is evident that points which lie in a plane perpendicular to the axis before twisting will still be in that plane when the shaft is twisted; it is also clear that the angle through which all points in the same plane move will be the same; hence the problem was as simple as problem could be. But many who had occasion to make use of Coulomb's results gave them an application which was wholly unwarranted. They assumed that they were equally applicable to other cases than complete circular cylinders; they assumed in fact that every point of the material which lay in a plane perpendicular to the axis would remain in that plane when the shaft was twisted, whether the shaft was symmetrical about its axis or not, and they consequently arrived at very erroneous results. That the assumption was erroneous is obvious enough from a consideration of an extreme case. In Fig. 1 is shown in cross-section a hollow cylindrical shaft, which is not complete, but divided by a plane passing through its axis. In this case the shaft when twisted will be as illustrated in the side elevation; two points, A and B, were in one plane perpendicular to the axis when the shaft was free from twist; they cease to be in one plane when the shaft is twisted. St. Venant[1] in 1855 investigated the question of shafts without making incorrect assumptions; he expressed the condition of the material by a partial differential equation of the second order, and gave suitable surface conditions. A general solution of the problem for all forms of shafts has not been obtained, but St. Venant gives a number of solutions for particular forms, and he obtains some general results of interest. In all cases the stiffness of the shaft is less than would be inferred from an erroneous application of Coulomb's theory. Fig. 2 shows diagrammatically the strain in a shaft of triangular section; the full lines indicate that the parts of the shaft which lay in one

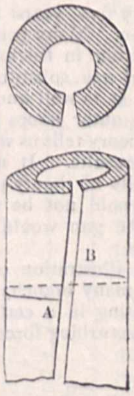

FIG. 1.

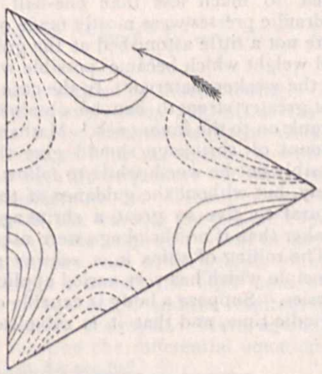
FIG. 2.

plane before twisting when twisted rise above the plane; the dotted lines indicate that they lie behind the plane of the paper. The shearing stress is least at the angles of the triangle, and is greatest at the middles of the sides. At this point then the shaft will begin to break under torsion. The fact is probably well known to men of practical experience, but it is directly contradictory to the conclusion at which one would arrive by a careless use of Coulomb's theory beyond the narrow limits within which it is applicable. The longitudinal ribs which one often sees on old cast-iron shafts are useful enough to give stiffness to the shafts against bending, but are good for very little if torsional stiffness or torsional strength is desired.

Another application of mathematical theory which has been carried somewhat further than the premises warrant is found in the case of girders. It is almost invariably customary to treat a girder as though the sections retain when the girder is bent the form and size which they had before bending. Making this assumption, it is very easy to calculate the strength and stiffness of a girder of any section. Unfortunately, the assumption is untrue; but, fortunately, it is approximately true in the case of most girders with which engineers in practice have to deal. That it is untrue can be readily seen from consideration of a girder of exaggerated form, the section of which is shown in Fig. 3. Any practical man would at once see that the outer parts of the flanges would add little to the strength of the girder, but according to the usual mathematical theory the outer parts of this flange should be as useful as the parts which are nearer to

[1] "Mémoires des Savants Étrangers," 1855; and Thomson and Tait, "Treatise on Natural Philosophy."

the web. This problem St. Venant also deals with in a rigid mathematical manner. Amongst other things, he showed that a girder of rectangular section, such as shown in Fig. 4, would, when bent, take the form shown by the curved lines in the same Fig. The last two examples show how a little knowledge may be a dangerous thing, and how easy it is for anyone who attempts to apply mathematics without adequate mathematical knowledge to be misled.

The theory of thick cylinders under bursting stress from within has many important practical applications to hydraulic presses and to guns. It has been discussed more than once in this room. As usual in considering these cases we are immediately led to differential equations which here are fortunately solved without serious difficulty, and the solution tells us the whole story. We learn that doubling the thickness by no means doubles the strength of the cylinder. And as a converse, that doubling the

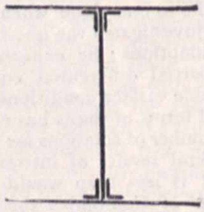

FIG. 3.

strength of the material will permit the thickness to be diminished to much less than one-half. Twenty-five years ago hydraulic presses were mostly made of cast-iron. Many people were not a little astonished at the great reduction in thickness and weight which became possible when steel was substituted for the weaker material. In the case of guns it is well known that greater strength can be obtained if the outer hoops are shrunk on to the inner ones.[1] Mathematical theory tells us what amount of shrinkage should give the best results. It may possibly not be worth while to follow the results of theory precisely, but without the guidance of theory it would not be unnatural to give so great a shrinkage that the gun would be weaker than if no shrinkage were used.

The rolling of ships in a seaway gives an illustration of a principle which has very varied application in many branches of physics. Suppose a body is capable of oscillating in a certain periodic time, and that it is submitted to a disturbing force of

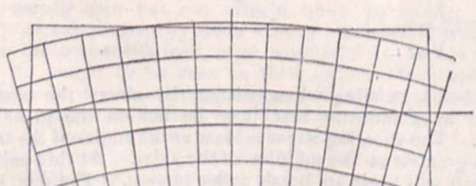

FIG. 4.

given period, the equation of motion easily shows that the resulting disturbance will be great if the two periods are equal or nearly equal. We meet with the principle in acoustics as resonance. If two tuning-forks are tuned to the same pitch, and one is sounded in the neighbourhood of the other, that other will presently be thrown into vibration by the waves transmitted through the air from the first. You may try a similar experiment at any time on any piano. Strike the higher G in the treble, the sound ceases on raising the finger. Now hold down the middle C, and again strike G; the C string at once takes up the note sounded, and can be heard after the exciting string has been silenced by damping. The same fundamental idea is found in the lunar theory in the term in the equation known as the evection, and again in the theory of Jupiter's satellites. The reason why the metals present in the solar atmosphere give black lines in the spectrum by absorption, corresponding in position with the bright lines in the spectrum which the same metals give when incandescent, is again the same. Gas will absorb or take in from the ether waves of the exact period which it is capable of giving to the ether. The general explanation of all these phenomena is easy. Imagine a pendulum, and suppose it experiences a periodic disturbing force, the first impulse of the disturbing force gives the pendulum a slight swing; the effect of the second impulse depends entirely on when it occurs; it may occur so as to neutralise the effect of the first, or it may occur so as to increase it. If the period of the force is the same as the natural period of the pendulum, the effect of the second, third, and later impulses will be added to the effect of the first, and the final disturbance will be great, even though the individual impulses be minute. But the mathematical theory tells us much more than any general explanation can do. It tells us exactly what the character of the effect will be, and its amount if the periods are nearly but not exactly the same. It tells us, too, exactly how friction affects the results. And the beauty of it is that the mathematical theory is much the same in all cases, so that having learned to deal with one case we are enlightened as to a host of others. The oscillating body may be an ironclad, or it may be an atom of hydrogen; the disturbing periodic force may be the waves of the Atlantic, or it may be the waves in the ether occurring five hundred millions of millions of times in a second; it is all one to the mathematician; the treatment is substantially the same.

The question of the speed of ships and the power to propel them is probably more effectually treated by experiment on models, as was done by the late Mr. Froude, than by mathematics alone; but in order to learn from the experiments all they are capable of teaching, a mathematical understanding is needed. Given that we know by experiment all about a given model, that we know what force is needed to propel it at every speed, we want to know from these experiments how a great ship, 100 times as big, but similar in form in every respect, will behave; and here mathematics come in to aid us in making the inference.

The construction of ships at once leads us on to the methods of navigating them. In navigation I should find much material for my purpose, but navigation is not usually included in engineering, but many of the implements of navigation undoubtedly are. The mariners' compass has for ages been the mainstay of the navigator, and a simple enough instrument it was till it was disturbed by the iron of which ships came to be built. The disturbance of the compass by the iron of the ship was first seriously attacked by two senior wranglers, Sir G. Airy and Mr. Archibald Smith. The disturbance may be divided into two parts, the first due to the permanent magnetism of the ship, the second to the temporary magnetism induced by the earth's inductive action on the iron of the ship—the first causes the semicircular, the second the quadrantal, error. One has only to open the "Admiralty Manual of Deviations of the Compass" to see how the mathematics of Archibald Smith have accomplished a proper understanding of the subject. The errors of the compass are dealt with in two ways: they are compensated by soft iron correctors, and by permanent magnets so placed as to have an effect equal and opposite to the effect of the temporary and permanent magnetism of the ship. Or they are dealt with by formulæ of correction which enable the error to be calculated when the course of the ship and the conditions of the earth's magnetism are given, or a combination of the two methods is used. Either method is based on Archibald Smith's theory. It is not possible to leave the subject of the mariners' compass without referring to the great improvements of Lord Kelvin. The improvements relate to every part of the instrument, and I venture to say that none of them could have been made by anyone but a mathematician. In order to get his card steady he knew that its period must be different to any possible period of the waves, or he would have the resonance to which I have just referred coming in, so he gave his card a considerable moment of inertia; but this was managed with a light card so that small needles could be used. If the needles are small the correction by soft iron masses and by permanent magnets is easier and more accurate. Then the bowl of the compass had to be suitably carried so that it would not be unduly disturbed by shock, and provision had to be made for damping by fluid friction the oscillations of the bowl if they occurred. Lastly, a most beautiful method of correcting the compass, without taking a sight, was discovered. In every detailed improvement one can detect that the inventing mind was that of a most able and trained mathematician.

An essential of safe navigation is an efficient system of light-

[1] Lamé, "Leçons sur la Théorie Mathématique de l'Élasticité des corps solides." 14th Leçon.

houses. The optical problem of the lighthouse engineer is to construct apparatus which shall usefully direct all the light produced. The present forms of apparatus are in their leading features due to Fresnel, the able mathematician, who established on an absolutely firm foundation the undulatory theory of light. To properly design an optical apparatus formulæ must be used, and the advantage is great if the designer can with ease manufacture the formulæ he requires.

Submarine telegraphy yields some interesting examples of the application of the higher mathematics. When a cable across the Atlantic was first seriously entertained, the first point to be settled was, how many words a minute could be sent through such a cable. This was the most practical question possible. Upon the answer depended the prospect of the cable paying commercially if successfully laid. The matter was dealt with by Prof. Thomson,[1] of Glasgow, now Lord Kelvin. He showed that the propagation of an electric disturbance in a cable could be expressed by a partial differential equation, and that the solution of this equation under certain conditions applicable to practice could be expressed either by a definite integral or by an infinite series. The values of these were calculated, and hence before an Atlantic cable was laid at all it was known how long it would take a signal to reach the opposite shore, and how much its intensity would be diminished in transmission. Referring to Fig. 5, abscissæ represent time, reckoned from the time of making contact at the sending

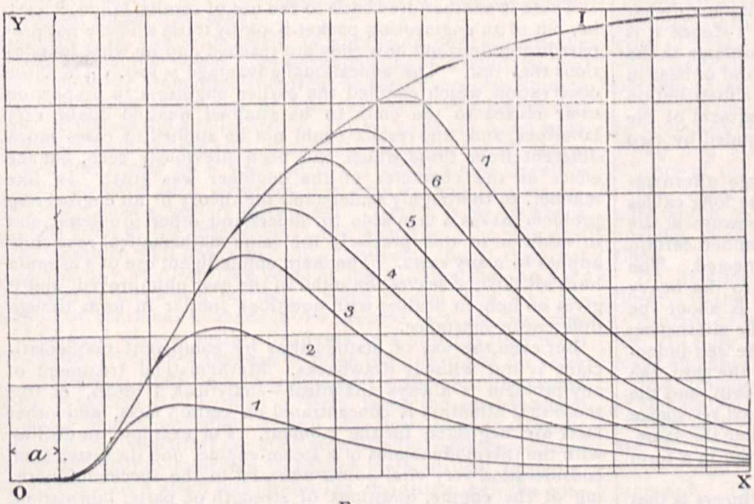

FIG. 5.

end of the cable, ordinates the currents at the receiving ends, curve (1) gives these currents when the contact at the receiving end, after being made, is continuously maintained. It will be observed that for a time a there is hardly any current at the receiving end, that then the current rapidly increases and attains to half its final value after a time equal to about $5a$. Curves (1) . . . . (7) show the currents at the receiving ends when the contact is made at the sending end maintained for times a, $2a$ . . . $7a$ respectively, and then broken. Looking at curve (1) one sees how small is the amount of current and how long it lasts compared with the time during which contact is made. The time a depends on the length and character of the cable; it is equal to $k\,c\,l^2 \log_e \frac{4}{3}/\pi^2$, where k is the resistance per unit length, c the capacity per unit length, and l the length of the cable. The knowledge of what is the commercial value of a cable depends on a knowledge of the value of a, and this cannot be obtained without knowing the differential equation $c\,k\,\frac{dv}{dt} = \frac{d^2v}{dx^2}$,[2] to which I have referred, and its by no means simple solution either as a definite integral or as an infinite series. So far as I know, this piece of higher mathematics cannot be evaded by any mere elementary treatment. The transmission of disturbance in a cable is quite different from the transmission of sound waves in air, which move with constant velocity. If the cable be doubled in length, it takes four times as long for the signal to pass through it instead of just twice as long, as would be the case if it were a proper wave motion. In fact the time of passage between the making of contact at the sending end of the cable and the beginning of the resulting disturbance at the receiving end, varies as the square of the length of the cable. The mathematical theory is exactly the same as that of the transmission of heat in a plate, one surface of which is suddenly exposed to a temperature different to the temperature of the plate. This is constantly occurring in the application of mathematics—one piece of mathematical work serves for many physical problems having apparently little in common. Fourier long ago discussed the heat problem, little dreaming that his analysis would be just what was wanted for ascertaining how fast signals could be sent across the Atlantic by a system of telegraphy which in his days had not even been projected in its simplest form. The same differential equation also gives the theory of the transmission of telephonic messages through cables; but the solution is then easier, and tells us exactly why it is so much more difficult to speak through 100 miles of cable than through 1000 miles of overhead line. As I have just stated, the differential equation of the disturbance in the cable is $c\,k\,\frac{dv}{dt} = \frac{d^2v}{dx^2}$. A musical note of period T spoken into the cable through a telephone is properly represented by $A \sin \frac{2\pi t}{T}$; the disturbance in the cable will be—

$$v = A\,e^{-x\sqrt{kc\pi/T}} \sin\left(\frac{2\pi t}{T} - x\sqrt{kc\pi/T}\right),$$

as may be easily verified by differentiating. This equation tells us everything. It tells us the rate at which the waves diminish with the distance. This rate increases with the resistance, with the capacity and with the frequency. If the capacity is at all considerable the diminution is rapid. The velocity of the waves is not the same for all frequencies, as is the case with waves in air, but varies as the square root of the period, so that if two notes were sounded the high note would arrive after the low notes, and the resultant effect would be entirely destroyed. Here, again, it is difficult to see how the differential equation and its solution can be evaded.

Though the history of the telegraph dates only from a little more than fifty years ago, it is ancient in comparison with the other great applications of electrical science, which have received their development during the last fifteen years. Here again mathematics which are not quite elementary have played their part. In the theory of transformers we find another illustration of the need of knowing how formulæ are obtained if they are to be correctly applied. The early transformers were made with unclosed magnetic circuits; there was an iron core, but the lines of magnetic force passed through air for a considerable part of their path. In this case a complete mathematical theory was not very difficult. But speedily closed magnetic circuits were found to be better, and the relation of magnetic induction and magnetic force became all important. If anyone were to apply mathematical formulæ, which were true for the earlier transformers, to the later ones, his results would be inaccurate. Indeed a wholly different method of attack on the problem was needed, taking account of the facts as they are, and not applying results which were true of older apparatus to cases essentially distinct.[1]

The employment of alternating currents has brought into use, as a necessity for understanding the actually observed phenomena, a great deal of mathematics. Why is the apparent resistance of a conductor greater for an alternating current than for a direct current? And by resistance I do not mean the quasi-resistance due to self-induction.[2] The mathematical electrical theory is ready with an answer; it is ready, too, to tell us how the difference depends upon the frequency of the current and on the size of the conductor. In the case of a cylindrical conductor

[1] "Mathematical and Physical Papers," vol ii. p. 61. Sir W. Thomson.
[2] v is the potential, t the time, and x the distance from the sending end of the cable.

[1] *Proceedings* of Royal Society, February 17, 1887.
[2] Lord Rayleigh, *Phil. Mag.*, vol. xxi. p. 381.

the solution involves a knowledge of Bessel's functions. We learn that if the current has a high frequency, or if the conductor be large, there will be very little current in the centre of the cylinder, and that therefore for any practical purpose the centre of the cylinder might just as well not be there; the current is largely confined to the part of the conductor near to its surface. The currents at different depths in the conductor attain to their maximum values at different times; those near the surface of the cylinder occur before those at some distance from the surface. The mathematical conditions are expressed by the same equation as is used to express the disposition of heat in a cylinder the surface of which is submitted to a periodic variation of temperature. Anyone who had thoroughly mastered the heat problem would be quite prepared to deal with the problem of currents in a conductor. It cannot be too often repeated, any piece of pure mathematics which finds one application to a physical problem is almost sure to find, in exactly the same form, applications to other problems which superficially are absolutely distinct. The differential equation in this case is $kc\frac{dv}{dt}=\left(\frac{d^2v}{dr^2}+\frac{1}{r}\frac{dv}{dr}\right)$, the similarity of physical condition to the problem of linear propagation of heat is close, but the mathematics differ materially owing to the presence of the term $\frac{1}{r}\frac{dv}{dr}$ in the equation. Mathematics deals with the relation of quantities to each other without troubling as to what the physical meaning of the quantities may be. Hence it is that the mathematical treatment of two such problems as the distribution of currents in a cylindrical conductor and of heat in a cylinder is identical, whereas the treatment of the distribution of heat in a cylinder is quite distinct from the treatment of the distribution of heat in a sphere or in a solid bounded by two parallel planes.

A curious phenomenon was observed in the large alternate-current machines at Deptford when connected to the long cables intended to take the current to London. The pressure at the machines when connected to the conductors was, under certain conditions, actually greater than when not so connected. The phenomenon is one of resonance very analogous to the heavy rolling of ships when the natural period of roll is about the same as the period of the waves.[1] The period of the alternating current corresponds to the period of the waves, the self-induction of the machine to the moment of inertia of the ship, the reciprocal of the capacity to the stiffness of the ship, and the electrical resistance of the conductors to the frictional resistance to rolling. The mathematics in the two cases is then the same. The effect was predicted long before it was observed in a form calculated to cause trouble.

A problem which is still agitating electrical engineers is that of running more than one alternate-circuit dynamo machine connected to the same system of mains. Before the matter became one of practical concern, it was considered in this room, and it was shown mathematically that it was possible to run independently-driven alternators in parallel but impossible to run them in series. That is to say, that if two alternators were connected to the same mains they would tend to adjust themselves in relation to each other so that their currents could be added, but that if an attempt were made to couple them, so that their pressures should be added, they would adjust themselves so that their effects would be opposed.[2]

Perhaps of all engineering problems which have received their solution in the last hundred years that of the greatest practical importance is the conversion of the energy of heat into the energy of visible mechanical motion. The science of thermodynamics has advanced along with the practical improvement of the steam-engine. By its aid, particularly by the aid of the so-called second law, we know what is possible of attainment by the engineer under given conditions of temperature. I must not trench on the subject of one of my successors, but I may point out that our knowledge of the second law of thermodynamics was first developed by means of mathematics, and that to-day its neatest expression is by means of partial differential coefficients. The two most notable names in connection with the development of the second law of thermodynamics in harmony with the first are those of Kelvin and Clausius; both dealt with the subject in a mathematical form not comprehensible to those who have not had substantial mathematical training.

Illustrations such as these might be multiplied almost indefinitely. They show that the advancement of the science of engineering has been aided in no inconsiderable measure by the labours of mathematicians directly applying the higher mathematical methods to engineering problems. They show, too, one way in which respect for a formula may be dangerous, one way in which it is true that mathematics may be a bad master. In St. Venant's problems we have an example in which the use of older results of limited application in cases where the assumptions on which they rest are not true will mislead. The examples show the proper remedy; it is a more complete application of mathematical methods. The error is just one which a man will make who has the power to use a formula without a ready understanding of how it is arrived at. A practical man, ignoring mathematical results, might or might not escape the error of supposing that a triangular shaft would break at the angles under torsion; the half-educated mathematician would certainly fall into the snare from which complete mathematical knowledge would deliver him. You can only secure the services of that good servant, mathematics, and escape the tyranny of a bad master by thoroughly mastering the branches of mathematics you use. The mistake caused by the wrong application of mathematical formulæ is only to be cured by a more abundant supply of more powerful mathematics.

There is another drawback to the use of results, taken, it may be, out of an engineering pocket-book by those who are not prepared to understand how they are reached and on what foundations they rest. The educational advantage is lost. The close observation which enabled the earlier engineers to proportion their means to the ends to be attained was no doubt very laborious, and the results could not be applied to cases much different from those which had been previously seen, but the effect on the character of the engineer was great. In like manner, to thoroughly understand the theory of an engineering problem makes a man able to understand other problems, and in addition to this precisely the same mathematical reasoning applies to many cases. The mere unintelligent use of a formula loses all this; it leaves the mind of the user unimproved, and it gives no help in dealing with questions similar in form though different in substance.

But even the use of mathematics by competent mathematicians is not without drawbacks. Mathematical treatment of any problem is always analytical—analytical, I mean, in this sense that attention is concentrated on certain facts, and other facts are neglected for the moment. For example, in dealing with the thermodynamics of a steam-engine, one dismisses from consideration very vital points essential to the successful working of the engine, questions of strength of parts, lubrication, convenience for repairs. But if an engineer is to succeed he must not fail to consider every element necessary to success; he must have a practical instinct which will tell him whether the instrument as a whole will succeed. His mind must not be only analytical, or he will be in danger of solving bits of the problems which his work presents, and of falling into fatal mistakes on points which he has omitted to consider, and which the plainest, intelligent practical man would avoid almost without knowing it.

Again, the powers of the strongest mathematician being limited, there is a constant temptation to fit the facts to suit the mathematics, and to assume that the conclusions will have greater accuracy than the premises from which they are deduced. This is a trouble one meets with in other applications of mathematics to experimental science. In order to make the subject amenable to treatment, one finds, for example, in the science of magnetism, that it is boldly assumed that the magnetisation of magnetisable material is proportional to the magnetising force, and the ratio has a name given to it, and conclusions are drawn from the assumption, but the fact is, no such proportionality exists, and all conclusions resulting from the assumption are so far invalid. Wherever possible, mathematical deductions should be frequently verified by reference to observation or experiment, for the very simple reason that they are only deductions, and the premises from which the deductions are made may be inaccurate or may be incomplete. We must always remember that we cannot get more out of the mathematical mill than we put into it, though we may get it in a form infinitely more useful for our purpose.

Engineers no doubt regard their profession from very different

[1] Institution of Electrical Engineers, November 13, 1884.

[2] Minutes of *Proceedings* Inst. C.E., April 5, 1883; Institution of Electrical Engineers, November 13, 1884.

points of view; some think it a mere means of making money; some regard it as an instrumentality for benefiting the race; whilst others again delight in it as an interest in itself, and delight in it most of all when new knowledge is added to that which we know already. It is just the same with the medical profession; some attend patients for the guineas they receive, some give a very high place to motives of benevolence, whilst others love it as a field where new knowledge may be found and the delight of discovery enjoyed. In regard to the first class of engineers, I have no doubt a little skill in managing a board of directors or impressing a committee of Parliament will be much more useful to the engineer than a great deal of mathematics. Let him manage his board and buy his mathematician, and it is very probable he will make much more money than the mathematician or any other person of skill whom he may employ. But we cannot all of us make money in this way. In the future it is likely that educated men will have to work harder and receive less, and it is a great thing if their work can be made itself a joy, and surely this can best be by a thorough understanding of the reason of all they do by the feeling that they have full competence to form their own judgments without depending much on the authority of others. This can only be in the words of Sir John Herschel by a "sound and sufficient knowledge of mathematics, the great instrument of all exact inquiry, without which no man can ever make such advance in any of the higher departments of science as can entitle him to form an independent opinion on any subject of discussion within their range."

After all, in any department of applied or pure science the highest satisfaction comes from accomplishing that which no one has done before, from disclosing what no one hitherto has known. If a department of the arts or sciences ceases to advance and becomes simply the application in known ways of known principles to obtain known ends, that department has lost its charm till the time comes for a fresh advent of change and development. To effect such advances it is easy to show that mathematics is a most necessary instrument. Here it is no drawback that the mind of the discoverer is too analytical; he may deal at his pleasure with one aspect of a problem, and it does not detract in any way from the value of his solution that he does not touch on incidental matters. Some of you who love the interest of continual advance in our science and practice, may look forward with a shade of sadness to a possible time when all is done or known which can be done or known, and the work of the engineer shall be merely applying principles discovered by his predecessors. In such a state, when the experience of the older generations shall control the practice of to-day, the free use of mathematical methods may be effectually superseded by the application according to rule of mathematical formulæ. But it would be a much less interesting condition than the constant change of to-day, when the practical experience of ten years ago is in many departments rendered worthless by later discoveries. But we need not fear that such a time of petrifaction will come so long as, whilst reverencing the discoverers who have added to our knowledge, we endeavour to replace their methods by better, and expect that those who come after us will, in their time, improve upon ours. Our knowledge must always be limited, but the knowable is limitless. The greater the sphere of our knowledge the greater the surface of contact with our infinite ignorance.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—By a resolution of Congregation, the care of such portions of, as may seem desirable, the Lee collection of anatomical and physiological specimens, which have for many years past been placed in the University Museum under care of the Linacre Professor, may be transferred to the care of other Professors with the sanction of the Trustees of the Lee bequest.

The date of the preliminary examinations in natural science in the Michaelmas term has been fixed for the Monday in the eighth week in Full Term in lieu of the dates hitherto observed.

The resolutions proposed by the committee on the granting of degrees for research were brought before Convocation on Tuesday afternoon. The first resolution, affirming the general principle that it was desirable that such degrees should be established, was agreed to *nemine contradicente*. Some difference of opinion manifested itself in the discussion on the succeeding resolutions. That which suggested that the new degrees should be styled Master of Science and Master of Letters was rejected on a division, with the intention that the titles should be further considered by the committee. The remainder of the resolutions were agreed to, and the drafting of a statute embodying the recommendations of the committee was referred to a committee consisting of the Vice-Chancellor and twelve others.

The annual Boyle Lecture was delivered on Tuesday evening by Prof. A. MacAlister, who chose as his subject, "Some morphological lessons taught by human variations." The lecture, which is held under the auspices of the Junior Scientific Club, was largely attended.

CAMBRIDGE.—The new Engineering Laboratory, just completed under the supervision of Prof. Ewing, will be formally opened on May 15, at 3 p.m., by Lord Kelvin, President of the Royal Society. The Vice-Chancellor will preside, and it is expected that Prof. Kennedy and others will deliver addresses on the occasion. After the opening ceremony Prof. and Mrs. Ewing will be "at home" in the laboratory, in order to give members of the University an opportunity of seeing for themselves the provision that has been made for the scientific study of engineering.

The degree of Doctor of Laws will, on May 10, be conferred upon Dr. Carl Theodor von Inama-Sternegg, honorary Professor of Political Science in the University of Vienna, and President of the KK. Statistical Central-Commission of the Austro-Hungarian Empire. Prof. von Inama-Sternegg was President of the Demographic Section of the International Congress of Hygiene held in London in 1891, but was unable to visit Cambridge with the other members of the Congress for the purpose of receiving the degree.

At the same Congregation the complete degree of M.A. will be conferred on Prof. Ewing's able demonstrators, Mr. W. E. Dalby and Mr. C. G. Lamb, who are already Bachelors of Science of the University of London.

Mr. Oscar Browning, who is an officier d'Académie, will next month represent the University of Cambridge at the festal opening of the new Palais des Facultés of the Académie of Caen.

Dr. Hobson, F.R.S., has been appointed a syndic of the library, in the room of the late Prof. Robertson Smith.

A grant of £20 from the University chest has been made to Mr. H. Yule Oldham, University Lecturer in Geography, for maps and apparatus.

The growth of the botanical department under the direction of Deputy-Prof. F. Darwin, F.R.S., has led the General Board of Studies to recommend that his stipend as reader, and that of Mr. W. Gardiner, F.R.S., as lecturer, should be increased to £150 a year. An additional demonstratorship in botany is also proposed. The Board further recommend that the annual stipend of Mr. S. J. Hickson, as lecturer in Advanced Morphology, should be increased to £100.

The Natural Sciences Tripos, for which there are about 130 candidates, begins on May 23, and will extend to June 12.

THE Convocation of the University of London met on Tuesday. It was expected that a warm discussion would take place on the Gresham scheme, but the expectation was not realised, as the chairman, Mr. E. H. Busk, ruled out of order all motions relating to that subject. One of these resolutions, standing in the name of Mr. Thiselton-Dyer, was—"That Convocation, while reserving its right to represent its views before the proposed Statutory Commission, hereby expresses its general approval of the Report of the Royal Commission." The *Times* reports that, when this and other motions had been ruled out of order, Prof. Silvanus Thompson moved the adjournment of the House. After some discussion an amendment to Prof. Thompson's motion—that the House should adjourn until seven o'clock—was accepted almost unanimously, the common object of all parties being to ascertain the result of the voting for the annual committee, for the election of which the two parties had their separate lists, one list consisting of those who were practically in favour of the Gresham scheme, and the other desiring to leave the whole question in the hands of the joint committee. The former party carried their whole list.

On the adjournment before the declaration of the poll a body of some 230 graduates met in the Graduates'-room of the University to draw up a protest against their having been again prevented from discussing the report of the Gresham Commission. Sir Henry Roscoe presided, and the speakers were Mr.

Thiselton-Dyer, Mr. Anstie, Q.C., Prof. Silvanus Thompson, Dr. Allchin, Sir Philip Magnus, Dr. R. D. Roberts, and Principal Cave. A resolution was unanimously passed in the following terms—"That this meeting of graduates, while reserving its right to represent its views before the proposed Statutory Commission, hereby expresses its general approval of the report of the Royal Commission." A second resolution expressed regret that, for the second time, discussion in Convocation of the report of the Gresham Commission had been prevented. Further, that an account of the proceedings at this meeting of graduates be prepared by the secretaries, and at once be transmitted to the Senate and to the press. A committee of graduates has been formed under the chairmanship of Mr. H. H. Cozens-Hardy, Q.C., M.P., for the purpose of obtaining from the graduates at large an expression of opinion in support of the scheme of reconstruction proposed by the Gresham Commission.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, April 30.—M. Lœwy in the chair.—On the equilibrium of ocean waters, by M. Poincaré. A mathematical paper dealing with the theory of tides.—On the soil and climate of the island of Madagascar from an agricultural standpoint, by M. Grandidier. The author warns intending colonists of the comparative infertility of most of the soils in Madagascar, notwithstanding the apparently vigorous vegetable growth supported thereon, and points out the importance of meteorological conditions, which are peculiarly unfavourable in certain districts.—Digestion without digestive ferments, by M. A. Dastre. Fresh proteids (fibrin, albumin, &c.) undergo the same series of changes when subjected to the prolonged action of 10–15 per cent. solutions of ammonium or sodium chloride (or 1–2 per cent. sodium fluoride), as when acted on by gastric juice.—Observations of Gale's comet made at Nice and at Algiers, by M. Tisserand.—Elliptic elements of Denning's comet, 1894, by M. L. Schulhof.—Observations of the same comet made at Toulouse Observatory, by MM. E. Cosserat and F. Rossard.—A theorem concerning the areas described in the movement of a plane figure, by M. G. Kœnigs. If a finite arc AB of any curvature roll upon any arc of equal length successively on the two sides of this arc, the area swept by the radius IM joining the instantaneous centre to a point M on the arc AB is independent of the form of the arc AB.—On the lines of curvature of *surfaces cerclées*, by M. Lelieuvre.—On the analytical integrals of equations of the form

$$\frac{d^n z}{dy^n} = F(z),\ F(z) = \sum a_{ik} \frac{d^{i+k} z}{dx^i . dy^k},\ i + k < n,$$

by M. Delassus.—A note by M. Bendixon on a theorem by M. Poincaré.—On hysteresis and permanent deformations, by M. P. Duhem.—On a new method of determining critical temperatures by the *critical index*, by M. James Chappuis. The author employs the method of interference fringes for following the variation in the index of refraction of the substance examined. The critical temperature of carbon dioxide determined by this method is 31·40°, a number in substantial agreement with Amagat's determination 31·35°.—On a new method for the determination of the lowering of the freezing point of solutions, by M. A. Ponsot. The temperature is read at which a solution is in equilibrium with a quantity of ice with which it is thoroughly agitated, the exterior radiation being minimised, and the solution is then in part withdrawn and analysed.—On cupric bromide, by M. Paul Sabatier. The anhydrous salt and the form $CuBr_2, 4H_2O$ are described. The green crystals of the latter lose water over sulphuric acid and are converted into black $CuBr_2$.—On an unsaturated natural ketone, by MM. Ph. Barbier and L. Bouveault. This ketone is obtained from crude essence of lemon grass (*Andropogon citratus*). It has the composition $(CH_3)_2 : C : CH . CH_2, CH_2, CO, CH_3$. It has a very agreeable but penetrating odour, and boils at 169–170° under ordinary pressure.—A purely mechanical action suffices for Cliona to bore its tunnels in the valves of oysters, by M. Letellier.—On the glandular system of ants, by M. Charles Janet.—Creation of new varieties by grafting, by M. Lucien Daniel. Hybridisation by grafting is possible for certain herbaceous plants which can be made to acquire new alimentary qualities by grafting them on plants superior to them in this respect, and afterwards sowing seed from the graft. The influence on the graft varies, but is particularly marked among the Cruciferæ.—On the chemical composition of wavellites and turquoises, by M. Adolphe Carnot.—On the microstructure of *mélilite*, by M. L. Gentil.—New researches on association among bacteria. Augmentation of the virulence of certain microbes. Increase of receptivity: A note by M. V. Galtier in which the following conclusions are given:—(1) Microbes, attenuated till they cannot alone produce a mortal malady, become again virulent when two species are introduced into the organism. (2) The two species may multiply side by side, but generally one tends to disappear, and the other becomes again pathogenous. (3) When two species of microbes are found associated, it is sometimes one and sometimes the other which regains its virulence according to the conditions. (4) Association of bacteria is able to be employed in the laboratory to render attenuated microbes again virulent. (5) Not only can the return of certain epidemics be explained by it, but the effects of vaccination with mild virus may be aggravated by this means. (6) The passage of one microbe, conferring immunity against a given malady, may predispose to the attack of another.—Properties of serum from animals protected against the poisons of different species of serpents, by M. A. Calmette.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—The Country Month by Month, May: J. A. Owen and Prof. Boulger (Bliss).—Die Fauna von Göriach: A. Hofmann (Wien).—Die Cephalopoden der Hallstätter Kalke: Dr. E. M. E. von Mojsvár, 2 Band, Text and Atlas (Wien).—La Rectification de l'Alcool: E. Sorel (Paris, Gauthier-Villars).—Biological Lectures and Addresses: Prof. A. Milnes Marshall (Nutt).

PAMPHLETS.—McGill University, Montreal, Engineering and Physics Buildings, Formal Opening, February 24, 1893.—Theophrastus Paracelsus: G. W. A. Kahlbaum (Basel, Schwabe).

SERIALS.—Science Progress, May (Scientific Press, Ltd.).—Bulletin of the New York Mathematical Society, Vol. 3, No. 7 (New York, Macmillan).—Quarterly Journal of the Geological Society, Vol. L., Part 2, No. 198 (Longmans).—Fortnightly Review, May (Chapman and Hall).—Jahrbuch der K. K. Geologischen Reichsanstalt Jahrg. 1893, xliii. Band, 3 and 4 Heft (Wien).—Medical Magazine, May (Southwood).—Bulletin de l'Académie Royale des Sciences, &c., de Belgique (Bruxelles).—Himmel und Erde, May (Berlin).

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