THURSDAY, MARCH 19, 1891.

WOOD'S HOLL BIOLOGICAL LECTURES.

Biological Lectures delivered at the Marine Biological Laboratory of Wood's Holl, in the Summer Session of 1890. Pp. v. + 250. Illustrated. (Boston, 1891.)

As we are informed in the preface, the ten addresses and lectures contained in this little volume were, with two exceptions, delivered at the Marine Biological Laboratory of Wood's Holl, as a continuation of a previous course. The volume is issued under the editorship of Prof. C. O. Whitman, the Director of the Laboratory, but each article appears to have been seen through the press by its respective author, as we notice some differences in the degree to which phonetic spelling has been adopted.

It is stated that one object of these lectures—which deal with some of the higher problems of biology, and among them those with a more or less pronounced metaphysical leaning—was to bring specialists into mutual communication and helpful relations with one another, and, at the same time, to make their work and thought intelligible and useful to beginners. It strikes us, however, that some of these lectures are altogether over the heads of most beginners; this being, indeed, practically acknowledged in a later paragraph of the preface. And, considering the generally practical tendency of the American mind, it also strikes us as somewhat remarkable that in scientific circles there should be such a marked tendency towards speculations which, to put it mildly, contain such a large amount of theory based on such an extremely small modicum of fact.

The lectures cover a very wide expanse of ground—from the Gastræa theory to Weismann on the origin of death, and from the relationship of the sea-spiders to ocean temperatures and currents. All of them are, indeed, practically reviews of the current opinions on the topics with which they respectively deal; and to review them critically would therefore be practically reviewing a review. Moreover, since each lecture or article is the work of a specialist on a more or less highly abstruse subject, it would require a committee of specialists on the subjects dealt with to venture on such a critical review. We shall, therefore, content ourselves with giving the titles of the various lectures, and calling attention to those points which appear to us to be of more than average interest. We heartily commend the volume as a first-rate *résumé* of the current opinions regarding several interesting biological problems, and we shall look forward to seeing it followed next year by a second volume containing the lectures which, we presume, are to be delivered during the coming summer.

The first lecture, on "Specialization and Organization," is by the editor; who first shows how specialization is met with in nature, and then mentions how necessary it is for the right study of nature. While, however, admitting the necessity of specialization in natural studies, the author urges the importance of organization and co-operation. Co-operation, indeed, is considered to be fairly well up to the requirements of the times, owing to the number and efficiency of our scientific journals; but the need for organization is strongly urged, and the author waxes eloquent on the advantages which would be derived from the establishment and endowment of a national biological station in the United States.

In the course of this lecture, Prof. Whitman refers to the recent Japanese experiments on *Hydra*. These tend to show that this creature is more specialized than was originally supposed to be the case; Trembley having omitted to notice that when turned inside out the Hydra again turns itself back to its normal condition, so that the functions of its inner and outer surfaces are not interchangeable, as was first supposed.

The second lecture is likewise by Prof. Whitman, and has for title "The Naturalist's Occupation." The chief object of the first part of this lecture appears to be to inculcate the importance of discovering what are really fundamental characters in animals, and of using these, and these only, in classification, which the lecturer urges should be based on genealogical lines. The Vertebrate affinities of the Ascidians are strongly insisted upon, the author even going so far as to say "that we are compelled to place them in the same great family;"—an opinion with which many zoologists will be by no means disposed to agree, even if they admit an affinity between the two groups. The second part of this lecture takes the special problem presented by the Vertebrate head—as to whether or no it is composed of a number of segments serially homologous with those of the body. After reviewing the original theory of the segmentation of the whole head, it is stated that embryology has now clearly shown that, although wide of the mark in its entirety, the theory rests on a fundamental basis of truth, there being conclusive evidence to show that at least the hinder part of the head is segmented, and the problem to decide being how far forwards this segmentation extends, or how many segments the head contains. The difficulty of determining which is head and which vertebral column in embryos and the Lancelet, is then referred to; this being followed by a discussion of the view that the fore-brain alone represents the original ancestral brain, while the mid- and hind-brain are formed from modified trunk-segments pressed into the service of the head. That this view is true, so far as concerns the mid- and hind-brain, is regarded as practically established; and the conflict is accordingly now restricted to the origin of the fore-brain. Here we are limited to three hypotheses: either the fore-brain is the inherited, unsegmented, ancestral brain of the Invertebrates; or it is an entirely new formation; or it represents a number of fused trunk-segments, among which the ancestral brain is unrecognizable. The third hypothesis is the one which appears to be steadily advancing in favour, but, as the author rightly observes, we are here mainly or entirely dealing with conjectures, and have at present no solid ground to work from. In regard to the genesis of the eyes of Vertebrates, it is stated that all the evidence points to their derivation from paired segmental sense-organs; and from this point of view the author seems inclined to support the doubt which Leydig has thrown on the pineal eye, since it is argued that the existence of such an organ would imply the coalescence of at least one pair of eyes, and we have no example of any analogous fusion in the Vertebrata.

The third lecture dealing with "Some Problems of

Annelid Morphology," is by Prof. E. B. Wilson. In this lecture the chief points dealt with relate to the larval Annelid form known as the *Trochophore*, and the subjects of metamerism and apical growth. The circumstance that the Trochophore corresponds only to the head of the adult Annelid, the trunk of the latter being formed as a bud growing from the lower end of the larva and then segmenting, and thus being a typical instance of apical growth, suggests that the trunk is a linear colony of sexual individuals budded off from the asexual head. And if this be so, then all metameric animals must be regarded as colonial organisms, comparable, so far as regards individuality, with a polyp-colony. Some zoologists have actually accepted this view, while others have propounded alternative hypotheses, none of which the author regards as satisfactory explanations of metamerism; and he concludes by stating that we are not at present in a position to offer any adequate intepretation of this difficult problem. With regard to the nature of the Trochophore, opinion is still undecided; but the author's own inclination is to regard it as a secondary production, and thus of no importance in genealogical questions: a similar view is expressed by Prof. T. H. Morgan in the seventh lecture.

We shall venture to pass over Prof. J. P. McMurrich's admirable summary of the Gastræa theory and its successors, in the fourth lecture, and pass on to the fifth and sixth of the series. These are on kindred subjects, the former being by Mr. E. G. Gardiner, and bearing the title of "Weismann and Maupas on the Origin of Death;" while the latter is by Prof. H. F. Osborn, of Princeton College, and is entitled "Evolution and Heredity." Prof. Osborn has given a more recent epitome of his views of the latter question in a paper read before the American Society of Naturalists at Boston, on December 31, 1890, entitled "Are Acquired Variations Inherited?"

Weismann's theory of the origin of death, our readers need scarcely be reminded, is based on his observations on the Protozoa, in which he finds that since one of these creatures reproduces itself by dividing into halves, each of which starts again as a young animal, there is normally no decay or death, but rather a potential immortality. In the Metazoa, on the other hand, each individual passes through a period of youth, adult life, and old age, finally ending its existence in death. And since the Metazoa have been derived from the Protozoa, it is argued that death is something acquired with the development of the one from the other. It is further urged that when death had once made its appearance it was received as a distinct advantage by the animal world, or nature. And we have further the well-known "continuity of the germ-plasm" theory, in which it is considered that the germ-cells of the Metazoa are those which have inherited the Protozoan immortality; the so-called somatic cells being, so to speak, an addendum to the germ-plasm which have lost their potential immortality; this loss being perhaps due to what is termed the neglect of natural selection, owing to the somatic cells (the body) being merely the protective case to the germ-plasm.

Mr. Gardiner states that since Prof. Weismann started this remarkable theory much opposing evidence has been brought against it; one of his strongest opponents being M. Maupas, of Algiers, who enters the lists by declaring that death and decay do occur normally among the Protozoa, and thus, if right, cutting away the very ground from Weismann's feet. This leads Mr. Gardiner to conclude that we are not at present entitled to regard the Protozoa as potentially immortal, nor to claim that the somatic cells of the Metazoa have no representatives in the Protozoa. It is added, "Nevertheless, it is too soon to declare that the idea that death is an adaptation is altogether erroneous"—a sentence that to our thinking tends to convey the idea that the lecturer considers that a theory based on false premises may yet probably be true.

Other objections to the theory are then considered; among which it will suffice to mention the one founded upon the various ages to which different animals attain; but the arguments employed appear somewhat difficult to follow, and not to advance the question much in one way or another.

Prof. Osborn's lecture is intimately connected with the preceding one, treating of the question whether the neo-Darwinism of Weismann or the neo-Lamarckism of Eimer best explains the phenomena of heredity, in which the chief *crux* is whether acquired characters are or are not transmissible. The neo-Lamarckians maintain that special and local variations in function and structure induced by environment and habit in the life of a parent tend to reappear in some degree in the offspring. On the other hand, Weismann and the neo-Darwinists contend that special individual variations are not transmitted from parent to offspring.[1] The lecturer states that a complete theory of heredity must account for the repetition phenomena (including reversions); for the non-repetition phenomena, or the appearance of new characters; and for the phenomena of physical transmission, which can be only worked out by the embryologist.

In regard to Weismann's views it should be observed that especial importance is attached to the "continuity of the germ-plasm," to which we have already alluded, and still more so to his doctrine of the isolation of the germ-cells from all influences which affect the body, or somatic cells; so that the former cannot be reached by slight acquired variations. Inheritance being the unbroken transmission of racial and ancestral characters by subdivision of the germ-plasm, only changes which affect the body as a whole can be added to the characteristics of the germ-plasm.

Prof. Osborn expresses his own opinion to the effect "that upon the side of evolution, or non-repetition in inheritance, the neo-Lamarckians have much the best of it; while upon the side of repetition and embryology, their opponents are strongest." But he significantly adds that in analyzing the arguments of some of the neo-Lamarckians he finds nearly as much against as in favour of the principle for which they contend. Finally, the necessity of moderation in the discussion is strongly urged, the present spirit displayed by the two sides not being conducive to the settlement of these apparently irreconcilable opinions. "I claim," the lecturer continues, "if the neo-Lamarckians can demonstrate by palæontological or other evidence that acquired characters are

[1] See Prof. Lankester's article in NATURE of March 6, 1890, p. 415.

inherited, it rests with the embryologists to furnish a theory of physical transmission. On the other hand, the embryologists may show conclusively that such inheritance is impossible."

At the end of his address on acquired characters, Prof. Osborn observes that "it follows as an unprejudiced conclusion from our present evidence that upon Weismann's principle [the non-transmission of special individual variations] we can explain inheritance but not evolution, while with Lamarck's principle [the transmission of acquired variations], and Darwin's selection-principle, we can explain evolution, but not, at present, inheritance. Disprove Lamarck's principle, and we must assume there is some third factor in evolution of which we are now ignorant."

A totally different subject is taken up in the seventh lecture, by Prof. T. H. Morgan, on the relationships of the sea-spiders. In this it is considered that the Pycnogonida, as the group to which these curious creatures belong has been termed, come nearest to the Arachnida, both as regards many peculiarities of the adult and also of the various stages of development. After discussing the morphology of the peculiar "Pantopod" larva of these forms, and its relations to the "Trochophore" of the Annelids, and the "Nauplius" of the Crustaceans, the lecturer has some observations upon the initial differences in the development of various groups of animals which are of sufficient general interest to be quoted at length. He observes:—

"If we remember that during the time in which the groups of Annelids and Crustacea have been evolved, the larval forms themselves have been acted upon in an increased degree, there seems every reason to believe that the young may have been much more acted upon and suffered far greater changes. On the other hand, when we see in such a group as the Vertebrates that in the higher forms the young have been removed to a large extent from the action of surrounding conditions—as, for instance, by being enclosed within a shell as in the Sauropsida, or retained within the uterus in Mammals—then can we understand why the young resemble each other more closely than do the adults, for the obvious reason that the adults have had to adapt themselves to more numerous external conditions, while the embryo has remained fixed. Indeed, this may be pushed a step further, it seems to me, and explains why such young retain the characteristics of lower forms, while the adults have lost such structures. This may be due to the young having been removed to a greater extent than the adults from a process of active selection. Hence, in such a group, when we say that the ontogeny tends to repeat the phylogeny, we mean that the embryos have retained more of the ancestral features than have the adults. But in such groups as the ones we are discussing—Annelids, Crustacea, &c.—we ought to expect, if what I have said be true, the reverse of what we find in such a group as the higher Vertebrates; viz. that the young forms diverge far apart, and the adults come nearer together."

Our remarks on the foregoing lectures have extended to so much greater length than we had at first intended, that space allows little more than the quotation of the headings of the three remaining ones; although neither fails in attracting as much interest as their predecessors. The eighth lecture is by Prof. S. Watase on "Caryokinesis"—a term which it may be well to explain is derived from *κάρυον*, a walnut, and is applied to a peculiar kind of nuclear cell-division, as met with in Cephalopods and Echinoderms. In the ninth lecture Prof. Howard Ayers gives us an elaborate dissertation on "The Ear of Man, its past, present, and future." The lecturer concludes that the ear of the higher Vertebrates is derived by the invagination of part of a system of canal sense-organs, like the lateral line of fishes; and he ventures to predict certain lines of modification which he considers will probably arise in the human ear with the course of time. The volume concludes with an article by Prof. W. Libbey, of Princeton College, on "Ocean Temperatures and Currents."

We have already expressed our high opinion of the volume before us as containing valuable *résumés* by specialists on many moot biological topics; but we cannot conclude without recording the pleasure and interest to which its perusal has given rise in ourselves, or without heartily commending it to the attention of our readers.

R. L.

PHYSICAL GEOGRAPHY FOR SCIENCE STUDENTS.

Elementary Physical and Astronomical Geography. Specially designed for Pupil-Teachers, Students in Training, and Science Students. By R. A. Gregory. With Original Illustrations. (London: Joseph Hughes and Co., 1891.)

THE general conception and arrangement of this volume are very good, and the same may be said of the detailed treatment of most of the subjects discussed. There is, however, great inequality in the author's work. The portions dealing with mensuration and astronomy are for the most part excellent, though there is sometimes a want of appreciation of the difficulties felt by beginners, and a consequent deficiency in the explanations given. The geological and meteorological parts are less thoroughly treated, while the chapter devoted to plants and animals is so imperfect that it had better have been altogether omitted. In case the work goes to a second edition, it will be as well to call attention to a few of the points where some alteration or further elucidation would be advisable.

At p. 21 the fact that the earth has been circumnavigated is given as a proof that it is not flat. But this is no proof at all; for if the earth were a flat disk, with the North Pole as its centre and the equator midway between the pole and the southern circumference—as represented by "Parallax" and other flat-earth men—circumnavigation might be performed exactly as at present.

The fact that degrees of latitude are longer near the poles than near the equator, although the polar axis is shorter than the equatorial, is often a stumbling-block to learners. It is, in fact, one of the paradoxes, and some half-century ago Mr. Von Gumpach published a pamphlet, and wrote letters to Prof. Airy and to the Astronomical Society, demonstrating, as he thought, that the earth is elongated at the poles—is, in fact, a prolate spheroid, because, the degrees being measurably longer at the poles, shows, he maintained, that the radius is there longer. Mr. Gregory takes no notice of this difficulty, which is a very real one, and requires some explanation in a text-book for learners.

At p. 107 we have the causes which lead to the difference between solar and mean time accurately stated, and a table given of the equation of time at different periods of the year. But though the causes are stated, they are not explained, and many students will fail to see, without explanation, why the apparent motion of the sun is greatest at the solstices, and least at the equinoxes.

At p. 133, the cause of dawn and twilight is well explained, but the fact that the length of day itself, as measured by the time the sun is visible above the horizon, is also increased by refraction, is not referred to.

In the chapter on the atmosphere we have a statement which, though founded on a fact of physics, is erroneous in the form in which it is given. It is stated that "When water vapour is transformed into the liquid state, a certain amount of heat is liberated, and therefore rainfall must have a considerable effect in warming the air, and conveying heat to higher latitudes." This is quite correct, but the author goes on to say that "one gallon of rain gives out latent heat suffici ent to melt 75 pounds of ice, or to melt 45 pounds of cast-iron, and every inch of rainfall is capable of melting a layer of ice upwards of 8 inches thick spread over the ground." Here, supposing the figures to be correct as regards the amount of heat given out during condensation, yet the latter part of the statement as it stands is incorrect, and very misleading. The heat of condensation is transferred to the cold current which causes the condensation high up above the surface of the earth, and does not remain in the rain, which therefore is not "capable of melting" any definite quantity of ice, since its temperature may be very little above the freezing-point when it reaches the earth. Water in freezing also parts with its latent heat of liquefaction, which goes to reduce the extreme cold of the air currents which froze the aqueous vapour, but no one would say that the snow brought this heat to the earth. The two cases are exactly parallel.

As illustrations of the ignorance or carelessness exhibited in the chapter on plants and animals, a few extracts will suffice. The characteristic plants of the Arctic zone are said to be "rhododendrons, lichens, and azaleas," while the vegetation of the torrid zone is thus described: "the palms, bananas, and other trees are densely packed, climbing vines entwine around their branches, and interlace with enormous tree-ferns and grasses." No doubt all these plants are found in the torrid zone, but the manner in which they are grouped shows that the description was not written by a person conversant with tropical vegetation. The lists of plants and animals given as characteristic of the different regions are always inadequate, and sometimes ludicrously erroneous: as when "plants yielding spices," and "bamboos," are given among the characteristic flora, and "numerous insects" among the fauna, of South America; "olives," "tobacco," "oranges," and "vines" as characteristic of the African flora; "hares" among the Australian fauna, the American "custard-apples" among the Oriental flora, while "grasses" appear as characteristic plants of Australia alone.

Among the less important errata for correction in a future edition are the following: at p. 50, in describing the noonday shadow at different seasons, the words "increase" and "decrease" are misplaced; at p. 170, "the Friendly Islands" are given as examples of conical isolated volcanic peaks; at p. 174, "the late Professor Darwin" is referred to; at p. 250 the dark heat-rays are said to be *reflected* from the earth's surface, instead of *radiated*, while, a few lines above, "radiation" is omitted as an important means of heating the atmosphere; and, at p. 280, the table gives "classes" instead of "genera" of Mammalia and birds.

In pointing out some deficiencies of Mr. Gregory's work, it must be remembered that many of the observations will apply equally to other works, some of them of considerable reputation; while the present volume is by no means without special merits. The chapter on the rotation of the earth and consequent phenomena is exceedingly good, as is the following one on its revolution. The account of eclipses and of the tides is also good, and well calculated to render these phenomena intelligible to learners. The chapters on the atmosphere and its movements are also clear and instructive, as are those on oceans and ocean currents. The numerous illustrations, though rather coarse, are clear, and elucidate some of the more difficult problems discussed; and if the author will carefully revise his work for another edition, it may be rendered a very useful and trustworthy guide to students and teachers.

OUR BOOK SHELF.

Whence comes Man; from "Nature" or from "God"? By Arthur John Bell. (London: Wm. Isbister, 1888.)

Why does Man Exist? By Arthur John Bell. (London: Wm. Isbister, 1890.)

IN these volumes Mr. Bell brings an acute and ingenious mind to bear on matters of science and the philosophical questions to which the study of science leads. Unfortunately there are many indications that Mr. Bell has no thorough and adequate first-hand acquaintance with the branches of science concerning which he writes. And when the science is mere scissors-and-paste-work, the conclusions are not likely to have the value which attaches to those of even the humblest practical observer. The first volume deals chiefly with the writings and opinions of Mr. Herbert Spencer, Prof. Huxley, and others, and introduces incidentally some quaint notions concerning matters physical and metaphysical. In the second volume, apart from theological questions, with which it is not our province to deal, the main thesis is that the fundamental cause of evolution is psychological. Every cell of the metazoan body is the seat of an intelligence or life which is capable of being conscious, and is therefore a person; and when a given life multiplies and divides, it gives rise to another life like itself without being diminished. Thus is constituted a *cell-patriarchy*. But a patriarchy involves a patriarch; and the patriarch is found in the parent-cell from which all others are produced. At the first division of the fertilized ovum there is constituted a parent-cell and a child-cell (Mr. Bell does not tell us how to determine which is which). The parent-cell may produce other child-cells, and the child-cell give birth to grandchild-cells, and so on; but throughout all cell-division the patriarch parent-cell remains. When invagination has taken place, we may be sure that the position of the parent-cell is at the mouth of the gastrula, for though it has subordinated its children to itself and to its service, that service has to be paid for. Later, however, we find it safely ensconced in the *lamina terminalis* of the brain, whence it directs the proceedings of its faithful servants and children. For the patriarchal parent-cell is the habitation of the Ego, the I, of the organism, while the child-cells are inhabited by subordinated egos. The protoplasm of a cell is a machine, and the inhabiting Ego its engineer. Reflex action is not merely reflex, but chiefly determined by the purposive action of the cell-ego, and so forth. We need not follow Mr. Bell further. Enough has been said to show the nature of his speculations, and to enable the biological or psychological reader in some measure to decide whether it will repay him to read Mr. Bell's pages for himself. C. LL. M.

Elementary Botany. By J. W. Oliver. (London: Blackie and Son, 1891.)

IN these days, when the pursuit of a "pass" is more keen than that of knowledge, the confession that an elementary text-book has been written for the use of students who are studying in classes under the Science and Art Department, and has been prepared on the lines of the syllabus of the first stage or elementary course, is apt to awake criticism, especially when, as in the present case, ten years' examination papers are printed at the end of it. But this book, though not an ideal elementary text-book, is tolerably free from the vices of cram: its merits are, in short, chiefly negative. What is urgently required at present is an elementary book of positive excellence, written (not compiled, as the present work appears to be) by an author who walks himself near the limits of our present knowledge; under these circumstances, the beginner would receive, from the very first, side lights, whether from terminology or from positive statement, which would prepare him for his more extended study. Such side lights are singularly absent from this work: take, for instance, the andræcium and gynæcium; in connection with these the word sporangium is not mentioned, nor is the word spore, except in the statement (p. 163) that the Cryptogamia "reproduce themselves by spores which contain no embryo": thus the attempt is not made to pave the way for subsequent progress to the study of the homologies in the lower forms. Again, in describing various types of corolla, the old terms such as papilionaceous, hypocrateriform are trolled out (p. 126) with only the minimum of explanation of the romance of insect agency (p. 147); and though function is put in relation to form in treating of the stem, the chapter on leaves is singularly dry, owing to its dealing simply with form and terminology. As regards terms, *fibro*-vascular should not be applied generally to bundles (p. 63), and the term *oospore* may well give place to zygote; while "acropetalous" is, we believe, a new enormity. Many of the figures are old friends: some of the new ones are bad; for instance, Fig. 54, of the wood of pine, which is full of inaccuracies; Fig. 73 (C), in which the cambium in a two-year old shoot is as thick as the phloem; and Fig. 75, in which the bordered pits are entirely omitted; while the difference between spring and autumn wood depends mainly upon filling up the lumen of the tracheids with printer's ink. It must not be concluded from these remarks that the book is worse than others of its class: in some respects, it is above the average, but none the less the field is yet open for a book suitable for beginners, and written by a master hand, which shall deal with the elements of the science, in its modern development, in such a way as to lay a secure foundation for the future progress of the beginner, and leave him with nothing to unlearn.

F. O. B.

Household Hygiene. By Mary Taylor Bissell, M.D. (New York: N. D. C. Hodges, 1890.)

THIS little volume consists of a series of papers which originally appeared as contributions to the Art Interchange Company. They deal with the sanitary regulation of the home. Every chapter is written in excellent style, and considering that much of the matter with which the author is concerned is technical, the manner in which it is presented is exceedingly clear. Dealing as it does with every requirement of the home from a sanitary and scientific point of view, the book contains much information of the utmost value to women of the household. It would be well if the essential conditions for a healthy home, so well laid down by Dr. Mary Bissell, were more carefully remembered, for they would be the means of saving life, now often sacrificed by ignorance of the ordinary laws of health. We recommend the book most cordially to the class of readers for whom it has been written, and we feel sure that everyone upon whom the care of a home devolves would be the better for bearing in mind the lessons which it teaches. H. BROCK.

Lessons in Applied Mechanics. By J. H. Cotterill, F.R.S., and J. H. Slade, R.N. (London: Macmillan and Co., 1891.)

THIS is one of the best little books on the subject that have come under our notice for some time. It is of a thoroughly practical character. Although most of the matter has been selected from the larger work by the first-named author, it is presented here in a more elementary manner, having been for the most part rewritten, with considerable additional illustration.

The work is divided into three parts. In the first part the principle of work is dealt with, and among the more important chapters in it we may mention that on pulleys, belts, and wheel-gears, and that on the direct-acting engine, the latter being thoroughly well examined in detail, with explanatory diagrams. In the second part, which treats of the strength of materials and structures, we have some well-prepared chapters on the bending moments and shearing forces under distributed loads, open beams, lattice girders, and frame-work structures, &c., together with some experimental facts relating to elasticity, strength, and resistance to impact of various materials. The last part consists of the fundamental laws relating to hydraulics, and although rather short, contains a sufficient amount of information for an elementary work.

Altogether, the book is an excellent treatise for students of engineering, and others taking up the subject. The examples, all of which are practical and original, are numerous, and add greatly to its utility.

LETTERS TO THE EDITOR.

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The Flying to Pieces of a Whirling Ring.

IN order not to mislead students, it may be well to point out that the words "parallel to the equator" in my letter on p. 439 (March 12) are emphatic, and that a less paradoxical statement is simply that the tension needed to lay a weightless cable precisely along a parallel of latitude is more than it can stand.

It will be noticed that a horizontal rope must sag either downwards or upwards, according as it is in a liquid lighter or heavier than itself; and that to stretch a thread, even a floating thread, till it is curved no more than the earth, needs too much tension for anything but a quartz fibre to stand; unless the liquid is so delicately adjusted as to buoy the body's apparent weight, without buoying its true weight. The termination of my former letter erred in seeming to suggest (as Prof. Karl Pearson puts it) that the principle of Archimedes fails for centripetal acceleration. OLIVER J. LODGE.

WE may imagine how anxious the practical man would be to test the extraordinary numerical results given by Dr. Lodge for the tension in a steel telegraph cable, due to the whirling effect of the earth's rotation, amounting, according to the formula, to a tension of 30 tons per square inch in latitude 60°, and 120 at the equator.

Dr. Lodge gives the formula $T = \rho v^2$, and at the equator, $v = 1526$ f.s., and now our practical man looks out the density of steel in a table, and finds it given as about 8.

With $\rho = 8$, $v = 1526$, he finds T is very nearly 20 million, so there is a misconception somewhere; however, the result is given in tons, so, dividing by 2240, he finds T is now about 8000, still very far from the result. He next tries dividing by 144, as the result is given in tons per square inch, and now T is about 60—only half the true result.

By this time he remembers that he ought to have taken $\rho = 8 \times 62{\cdot}4$, and T is now about 3700; and as this looks like 32 times the true result, he now thinks of dividing by g, so that, finally, the formula which gives the true result is, not the simple $T = \rho v^2$, but $T = \frac{62{\cdot}4}{2240 \times 144} \frac{\rho v^2}{g}$.

What does the formula $T = \rho v^2$ then mean? With C.G.S. units we may put $\rho = 8$, and $v = 4 \times 10^9 \div (24 \times 60 \times 60)$, and now $T = 10^{10} \times 1{\cdot}7$ *barads*; and as a stress of one pound per square inch is about 70,000 barads, we divide by 70,000 and 2240, and find $T = 110$ tons per square inch.

But with the ordinary British legal units of the foot and the pound weight, the formula $T = \rho v^2$ is meaningless; and a practical man has a just complaint against our vague system of theoretical teaching in dynamics, when he comes across a formula such as $T = \rho v^2$, where it is merely stated that T is the tension, ρ the density, and v the velocity.

It is the interest of those to whom Dynamics is a reality, and not a mere Combinatorial Analysis, that I am encouraged to write this criticism.

Now let us return to the telegraph cable, which, as a girdle round the equator, ought to rise out of the ocean bed, and stand like an arch under a tension of 120 tons per square inch, in consequence of the whirling effect of the earth's rotation.

But, on taking into account the gravity due to the earth's attraction, which is about 289 times the whirling effect, our cable, if it still stood as an arch, would have a pressure of about 32,000 tons per square inch; and now we are confronted with the old Ostrogradsky Paradox. A. G. GREENHILL.

March 16.

PROF. LODGE has invited me to follow up his letter on "The Flying to Pieces of a Whirling Ring" (NATURE, March 12, p. 439), by sending to NATURE a note about the strains and stresses in a whirling disk—a matter which has lately been the subject of some correspondence between us. Before speaking of the disk, however, let me (as an old cable hand) confess that I do not follow the reasoning which leads Prof. Lodge to say that a submerged cable of the average density of sea-water, if lying parallel to the equator, would be subject to a stress of 30 tons per square inch, or more (in latitude less than 60°), in consequence of the earth's rotation. This is in startling disagreement with one's recollection of the behaviour of say a "caya" rope (which satisfies the condition as to density). But surely a cable that is wholly supported by water is as much protected by gravity from flying to pieces as a cable that is partly supported by the mud or rocks of the bottom.

The strains in a revolving disk have been discussed by Mr. Chree (*Quart. Journal of Pure Math.*, No. 89, 1888; see also Proc. Camb. Phil. Soc., vols. xiv. and xv.) and by the late Prof. Grossmann (*Verhandlungen des Vereins zur Beförderung des Gewerbfleisses*, Berlin, 1889, p. 216). Prof. Grossmann—for the reference to whose paper I am indebted to Mr. J. T. Nicolson—treats the case of a disk with a central hole in it, and points out that the hoop tension is greatest just round the hole.

There is, however, an important difference between the values of the hoop tension when there is and when there is not a hole. The difference in question appears to have escaped notice, and my object in writing this note is to point it out.

The case supposed is that of a thin disk, homogeneous, isotropic, and uniformly thick. We have to consider two principal stresses—namely, the radial tension p_1, and the hoop tension p_2.

Let ω be the angular velocity,

ρ the density,

E Young's modulus for the material,

μ Poisson's ratio of lateral to longitudinal strain in a simple stress.

u the radial displacement at any point where the stress is considered, and to which the radius is r.

The equilibrium of any small element of mass under the two radial tensions at its inner and outer surfaces, the two hoop tensions on its front and back faces, and the "centrifugal force," requires that

$$p_1 = \frac{d(p_2 r)}{dr} + \omega^2 \rho r^2 \quad \ldots \ldots \ldots (1)$$

The strain in the direction of the hoop stress p_1 is $\frac{u}{r}$. The radial strain is $\frac{du}{dr}$. Hence

$$\frac{Eu}{r} = p_1 - \mu p_2; \quad \ldots \ldots \ldots (2)$$

and

$$E\frac{du}{ar} = p_2 - \mu p_1 \quad \ldots \ldots \ldots (3)$$

From (2) and (3),

$$p_1 = \frac{E}{1-\mu^2}\left(\frac{u}{r} + \mu\frac{du}{dr}\right) \quad \ldots \ldots (4)$$

$$p_2 = \frac{E}{1-\mu^2}\left(\frac{\mu u}{r} + \frac{du}{dr}\right) \quad \ldots \ldots (5)$$

And, substituting in (1),

$$\frac{rd^2u}{dr^2} + \frac{du}{dr} - \frac{u}{r} + \frac{(1-\mu^2)\,\omega^2\rho r^2}{E} = 0 \quad \ldots (6)$$

From which,

$$\frac{u}{r} = \frac{C}{r^2} + C_1 - \frac{(1-\mu^2)\,\omega^2\rho r}{E} \quad \ldots \ldots (7)$$

In this I have simply followed Grossmann, who goes on to find the stresses in a disk *with a hole* by applying the boundary conditions that $p_2 = 0$ when $r = a_1$, the radius of the disk, and also when $r = a_2$, the radius of the hole. The result is—

$$p_1 = \frac{\omega^2\rho}{8}\left\{(3+\mu)\left(a_1^2 + a_2^2 + \frac{a_1^2 a_2^2}{r^2}\right) - (1+3\mu)r^2\right\};$$

$$p_2 = \frac{\omega^2\rho}{8}\left\{(3+\mu)\left(a_1^2 + a_2^2 - \frac{a_1^2 a_2^2}{r^2} - r^2\right)\right\}.$$

From this it is clear that the maximum hoop tension occurs close to the hole, with the value

$$\text{Max. } p_1 = \frac{\omega^2\rho}{4}\left\{(3+\mu)a_1^2 + (1-\mu)a_2^2\right\};$$

and the maximum radial tension occurs when $r = \sqrt{a_1 a_2}$, with the value

$$\text{Max. } p_2 = \frac{\omega^2\rho}{4}(3+\mu)(a_1 - a_2)^2.$$

In the special case when the hole is very small

$$\text{Max. } p_1 = \text{Max. } p_2 = \frac{\omega^2\rho a_1^2(3+\mu)}{4}.$$

With a given material, the stresses depend simply upon the peripheral velocity.

Next take the case of a disk which has no hole. The boundary conditions are $p_2 = 0$ when $r = a$ (the radius of the disk), and $u = 0$ when $r = 0$. Hence, by equation (7), $C = 0$, and then, by equation (5), $C_1 = \frac{(1-\mu)(3+\mu)\omega^2\rho a^2}{8E}$.

The stresses in the disk without a hole are therefore,

$$p_1 = \frac{\omega^2\rho}{8}\left\{(3+\mu)a^2 - (1+3\mu)r^2\right\};$$

$$p_2 = \frac{\omega^2\rho}{8}(3+\mu)(a^2 - r^2).$$

Each of these is a maximum at the centre, namely,

$$\text{Max. } p_1 = \text{Max. } p_2 = \frac{\omega^2\rho a^2(3+\mu)}{8};$$

and this is just half the value which the intensity of stress reaches when there is a very small hole.

If we take 15 tons per square inch as the greatest safe stress in steel, a thin disk of uniform thickness, with a hole at the centre, may be whirled with a peripheral velocity of about 620 feet per second. If there were no hole, the peripheral speed might be about 870 feet per second.

A plate intended to whirl at a high speed should evidently have its thickness increased in the neighbourhood of the nave.

Cambridge, March 14. J. A. EWING.

I IMAGINE that many experimentalists who have had to employ whirling apparatus running at a dangerously high speed must have come to the same conclusion as Dr. Lodge in finding the limit of safety. When designing the magnetic ring, which the late Dr. Guthrie and I used in investigating the conductivity of liquids, I arrived at the same result—namely, that each material when in the form of a ring has a limiting linear speed depending only on its tenacity and density. The same is true of a portion of a ring held by the ends moving about its centre of curvature, provided that it is so long that its stiffness is not a material factor. It did not, however, occur to me that an Atlantic cable of the same density as sea-water would fly to pieces; and I don't now clearly understand why this should be so, or, if so, why the ocean would in such a case hold together, having the same density as the cable.

Of course in the case of a disk, such as a grindstone, higher speeds are possible, because, to use Dr. Lodge's expression, the outer parts are radially sustained. The investigation of the subject will be found in the reprint of Clerk Maxwell's scientific papers, vol. i. p. 60, where the effect on polarized light of a transparent revolving cylinder is also considered.

C. V. BOYS.

IN his letter on p. 439, Dr. Lodge points out that the tension due to centrifugal forces in a rotating band is independent of the curvature; but the deductions which he draws from this are, I think, mistaken. He argues, in the first place, that a straight band of 30-ton steel moving with a velocity of 800 feet per second in the direction of its length is in a state of very unstable equilibrium, and that the slightest shiver of a "vibration running along it would precipitate a catastrophe."

To be sure, if the band is already stretched to its breaking-strain, the equilibrium is unstable *whether it be in motion or not*. But if the band be not so stretched, and it need not be, there is no instability whatever on account of the motion, and a vibration will travel along a bar of steel advancing with this or any other velocity precisely as if the bar were at rest, and without exciting among its particles any rebellion against the second law of motion.

Further on Dr. Lodge asserts that a cable of the same average density throughout its length as sea-water, and lying across the ocean parallel to the equator in latitude lower than 60°, could not hold together unless of 30-ton steel, and the suggestion to relieve the tension of a telegraph-cable by floating it is pronounced infeasible on account of the centrifugal forces. Surely Dr. Lodge has forgotten that the buoyancy of the sea-water is already itself modified by the centrifugal force, so that such a cable would be in perfect equilibrium. Moreover, and quite apart from this consideration, since the centrifugal force on a body even at the equator is only about $\frac{1}{300}$ of its weight, there remains $\frac{299}{300}$ of the weight which might be relieved by flotation in the manner suggested without encroaching on the remaining $\frac{1}{300}$, which would balance the centrifugal force, and thus free the cable from all tension.

A. M. WORTHINGTON.

Devonport, March 15.

ONE of Prof. Lodge's results would surprise all mathematicians were it correct: unfortunately this is not the case. A submarine cable would have *no* tendency to break if supported by floating matter in the manner described by Prof. Lodge. Every particle of the cable would be under the influence not only of "centrifugal force," but also of gravity, and the upward pressure of the water would just balance the difference of these opposed forces, hence there would be *no tension whatever* in the cable, and it would remain in neutral equilibrium, no matter what the latitude.

Prof. Lodge's other results are well-known to most students of dynamics. His general statement that "an Atlantic cable is only held together by its weight," is merely a particular case of the fact that all bodies, whether cables or otherwise, would fly off or burst away from the earth if gravity did not exist. Had Prof. Lodge realized this fact, he could hardly have made such an obvious mistake with regard to the behaviour of a *supported* cable.

G. H. BRYAN.

Peterhouse, Cambridge, March 14.

Modern Views of Electricity (Volta's Force).

I THINK that the difficulty which Mr. Burbury expresses on p. 439 (March 12), under the above heading, probably rests on a misapprehension. He says: "When zinc is isolated, a negative charge is on it, and therefore at an outside point there is a positive slope of potential upwards from the zinc." My statement, on the contrary, is that a piece of zinc immersed in an oxidizing medium possesses no charge so long as it is isolated, but experiences a lowering of potential by reason of the chemical tendencies of its surface-film—*i.e.* the contiguity of a number of negatively charged oxygen atoms. In this film, indeed, there is an electrical double-layer, consisting of equal opposite charges, the negative facing the zinc, the positive facing outwards; but there is no charge such as will produce the slightest effect at an external point. Contact with copper of course changes all this; displacing negative electricity from zinc to copper across the junction, from copper to zinc through the air. It is this displacement which affects all external points; and it is this which electroscopic experiments have displayed. There is nothing whatever to be detected in the neighbourhood of a piece of isolated zinc, unless the surface-film itself be explored. The range of its effect is sharply bounded by the thickness of its infinitesimal air-film, on which the whole of the molecular strain is thrown: much as is expressed by Mr. Chattock in the latter half of his letter on p. 367.

If Mr. Burbury does not object to contemplate a piece of isolated zinc surrounded on all sides by straining oxygen atoms, each negatively charged, he can have no difficulty in realizing its depression of potential; nor can he fail to appreciate the momentary transfer of electricity, accompanying the sudden approach of the crowd of oxygen atoms, which occurs as soon as a way of escape for negative electricity is opened by the sweeping away of some of them by copper.

OLIVER J. LODGE.

Ratio of Centimetre to Inch.

PROF. BOYS' letter on p. 439 (March 12), reminds me that I have never seen stated a very simple approximate relation between centimetres and inches, viz. 33 to 13, which is correct to one part in 1700.

OLIVER J. LODGE.

Potassium Salts in Sea-Water.

A CORRESPONDENT in NATURE of January 1 (p. 199), in asking why it is that the water of the ocean contains such a large proportion of sodium and so little, comparatively, of potassium salts, raises one of the most instructive inquiries in the whole range of mineral physiology. The waters which flow into the sea convey the soluble salts derived from the land, and these often include a considerable proportion of potassium. The sources of these salts are two-fold: (1) the sub-aërial decay of crystalline rocks, which give up their alkalies as carbonates; (2) saline solutions and solid salts which have come from evaporated seas or lake basins, and have thus been withheld or abstracted from the ocean's waters. In the latter case they are fossil sea-waters, as in many saline springs from the older sediments. These waters show that the proportion of potassium salts was then not greater but less than at present. Of the alkaline salts of the St. Lawrence River estimated as chlorides, the potassium equalled, by my analysis, 16 per cent., and the Ottawa 32 per cent., the remainder being, of course, sodium chloride. In the numerous saline and alkaline springs which rise from the Palæozoic strata throughout the great valley drained by these rivers the proportion of potassium chloride is seldom over 2 or 3 per cent. of the alkaline salts, and often less, while in the waters of the modern ocean it is found to be not far from 3 per cent.

There are, then, two questions before us: (1) Why do saline springs and ordinary potable spring-waters contain so small a proportion of potash salts? and (2) What prevents their accumulation in the waters of the ocean? The evaporation of sea-water in limited basins gives at first pure sodium chloride, and it is only in the mother-liquor that the potassium salts are found, and, as in the Stasfurth beds, are deposited above the rock-salt. The researches of various chemists have long since shown that surface waters, in filtering through the soil, give up potash, ammonia, silica, and phosphates, retaining, however, lime, magnesia, and soda—a beautiful provision by which the earth retains the elements necessary for the life of plants, while the filtered water thereby becomes purified and fit for ordinary uses. A process not unlike this goes on in the sea. It is well known to chemists that the ashes of sea-weeds abound in potassium salts, and contain, in most cases, from 15 to 25 per cent. of potassium oxide; so that kelp is valuable, not only as a source of iodine, but of potash, and the fertilizing effects of sea-weed, due to the presence of the alkali, are recognized. The marine plants thus select from the sea-water the potash, and by their subsequent decay in the ooze of the bottom, as long since pointed out by Forchhammer, restore this element again to the insoluble sediments. These plants, at the same time, take from the sea-water the minute quantity of iodine which it contains, and also the dissolved metals, as silver, copper, and gold, traces of which are found in their ashes, and, as I have long since shown, are thus agents in the production of metalliferous strata, and, finally, of mineral veins.

Thus, while the soil removes from the surface waters the elements necessary for the nutrition of land plants, the marine vegetation itself performs more directly a similar function in the waters of the ocean, and in the accumulating sediments restores it to the solid earth the alkaline element. In the sea, as on the land, the great process of terrestrial circulation goes on unceasingly. The reader who wishes may see the whole matter discussed at greater length in the author's "Chemical and Geological Essays," pp. 95, 96, 135; and again, in a lecture on "Metalliferous Deposits," *ibid.*, pp. 220–236.

New York. T. STERRY HUNT.

Bright Crosses in the Sky seen from Mountain Tops.

ON March 21, 1881, I made an ascent of the Grossneuediger (3673 m.), and observed at midday from the summit a curious phenomenon. Yesterday, March 8, 1891, I made an ascent of the Patscherkofl (2214 m.), and observed, also at midday, the same phenomenon from the summit. As I have never seen it at any other time from any mountain top, it may be considered rare, and as it was in both cases observed in March, with fine weather, south wind, and relatively high temperature, it may be more or less restricted to that time of the year, when Alpine ascents are rarely made.

The phenomenon is a combination of two rings of light, one of which has its centre in the line connecting the observer with the sun, and appears to have the same dimensions as the large ring sometimes observed round the moon and the sun at low levels. The other ring has its centre in the observer and passes through the zenith and the sun. Both rings are pale, the latter paler than the former, and not visible near the northern horizon. Where these two rings cross each other, they are much brighter than elsewhere; and so it appears at first sight, if one does not look carefully, as if there were merely two bright crosses, one below and one above the sun, the arms of both crosses being vertical and horizontal.

This observation may be interesting to meteorologists, and seems to be but rarely made. R. v. LENDENFELD.

Innsbruck, March 9.

Iridescent Clouds.

A BRILLIANT iridescent zenith arc was seen to-day at 3.15 to 3.30 p.m., without either the primary or secondary solar halos which usually accompany it. A faint "mock sun" was, however, observed in the west at 4 p.m.

A remarkable and distinct display of iridescent cirro-stratus was seen on March 4. The cloud-layer, which appeared to be very high, and was apparently advancing from the west-northwest, was evenly tinted in a striking manner. The hues were most strongly marked in the foremost band, whilst in the far perspective the cloud was still faintly illumined with iridescent colours; these were especially beautiful at 5.45 p.m. Low scud cumulus from the west partially obscured the view at 6 p.m. It is not unusual to see iridescent colouring in clouds, chiefly when a low bank of cloud reflects the light on the higher cirrus. It is also occasionally seen fringing the edges of cirro-cumulus when they are in an azimuth near that of the sun.

York Road, Driffield, March 11. J. LOVEL.

Frozen Fish.

MR. MCLACHLAN'S opinion that fish suffer comparatively little injury when inclosed for lengthened periods in solid ice is fully borne out by an occurrence here in the year 1873. In the early part of the month of July a boy was seriously ill in one of the large boarding-houses; ice-bags had to be applied to his head; the ice was procured from the ice-house, which had been filled in the previous December from a pond in the neighbourhood. On pouring off the water from one of the bags after it had been used, a small fish was seen swimming merrily about. My informant (the master of the house in which the boy lay ill) tells me "the fish was very small, and so transparent that a large portion of its internal organization was clearly visible"; he thinks it was minnow, but is doubtful as to the accuracy of the opinion. At all events we have here a well-authenticated case of a fish surviving inclosure in solid ice for a period of between six and seven months.

I may perhaps mention the effect of the recent winter on the Unionidæ of this neighbourhood. They lie dead in shoals round the margins of several large ponds I have examined, particularly in those which are very shallow round the edges. The dead are far more numerous than I have ever seen in previous years. Two *A. cygnæus* which I exposed in an open vessel to the entire severity of the frost were killed, and both their shells split from dorsal to ventral surface on one side. On the other hand, Unionidæ, even when out of their shells, can be frozen and thawed for two successive nights at least without injury; neither do the contained Glochidia suffer in any way. This last point I chanced to discover last year accidentally, by one of my dissecting dishes containing a living Unio getting frozen solid on two successive nights. OSWALD H. LATTER.

Charterhouse, Godalming, March 15.

MR. MCLACHLAN asks (1) Whether fish necessarily die when enclosed for lengthened periods in solid ice?

To this I can definitely reply that there were many small carp (3 inches to 9 inches), and innumerable sticklebacks, embedded in the ice in a pond here, within a week of the commencement of the long frost (beginning about December 8), and that when pieces of ice containing them were broken up (as was done at that time) and the fish put into water, they showed no signs of life.

(2) Whether this winter caused any important mortality?

Up to the frost, the pond swarmed with sticklebacks, and contained hundreds of small carp (3 inches to 6 inches), and, probably, two or three dozen of the same fine fish from 6 inches to 20 inches long. Since the frost, there has been no sign of fish-life in the pond.

The pond is about 70 feet by 50 feet, and at the time of being frozen had a depth of 2 feet of water, and (in places) over 1 foot of soft mud. Unfortunately, the ice was not kept broken.

Of course, it is possible that the old carp may be still alive at the bottom of the pond or in the mud, but we should have seen the smaller carp and sticklebacks if there had been any still alive. JAMES TURLE.

North Finchley, March 14.

Eskimo Art Work.

AMONG the many objects of interest seen in a brief journey to the cryolite mines in the Arsuk fjord, Greenland, last September, none was more attractive than a collection of what may be called Eskimo works of art, belonging to Assistant-Superintendent Edwards, of the mine. Three of the specimens were photographed with a tourist's camera. Although the photograph was not a very good one, it shows a degree of skill in sculpture that would probably surprise those familiar only with those specimens found in such museums as the Washington and the Berlin.

In the collection, besides candlesticks and cigar-holders, were a number of ash-receivers, anchors, paper-weights, &c. They were all made of green stone (weight stone, the Danes call it), of the variety used in making the Eskimo lamps. Of course, every article was made with the intention of selling it to the Danish rulers; the Eskimos, so far as I could learn, never using their artistic skill for decorating their own homes, although such articles as weapons, toggles for dog harnesses, &c., are often fashioned with an eye for beauty, as well as utility.

Files, purchased of the Danes, were about the only tools used by the Eskimo artists, although the form of the object to be made was first rudely blocked out with a pointed piece of iron used as a chisel.

Some of the objects had a jewellery finish, as founders in bronze would say; while others (and the more beautiful) showed plainly the marks of the file. The art centre—if one may call it so—of Greenland is Godthaab, where Heinrich J. Rink lived when Inspector of South Greenland, although one or two men at Fredrickshaab have, by their skill, made reputations among the whites along the coast. JOHN R. SPEARS.

New York, February 26.

THE ZOOLOGICAL STATION AT NAPLES.

IN NATURE of February 26 (p. 392) a friend of the Zoological Station of Naples has raised his voice to correct one or two misconceptions which, as he thinks, have been the cause of the difficulties, experienced at the last meeting of the British Association in Leeds, in obtaining the renewal of the vote for the occupation by British naturalists of a table in the Zoological Station. While thanking him, I should wish to be allowed to add some remarks to the arguments used in that article.

If opposition to the continuance of the table was really based on the ground that the Zoological Station is in the main an educational institution, nothing would be easier than to show that this is a fundamental error. In fact, the whole conception of the Naples Zoological Station was to found an institution meant *exclusively* for research, and this conception has been carried out in every way. Not only more than six hundred naturalists of various nations have worked for months and years in the laboratories of the Station; not only from six to ten assistants have been occupied with research all these years through; but the Zoological Station has sent ever-increasing numbers of well-preserved marine animals to almost all the greater and many smaller European and other laboratories for pure ends of research. By all this the Zoological Station has almost revolutionized the conditions of biological research; it may yet be the cause of greater changes through the arrangements that it is just now finishing to enable physiologists to carry out experimental and chemical studies on marine animals and plants.

One educational exception is, perhaps, worth recording. The Zoological Station has admitted during the last ten years naval officers and physicians from Italy, Germany, Russia, and Spain, for the purpose of instructing them in the art of collecting and preserving marine organisms on their voyages through the oceans, and I am glad and proud to say that the collections brought home by the Italian corvette *Vittor Pisani* have earned not only well-deserved fame for Captain Chierchia, but have proved to be really the solution of the problem how to add numberless treasures of the oceans to the stock of inland laboratories for research, and to do this by the simple expenditure of a few thousand francs. The example set by the Italian naval authorities has been followed by the Russian Navy, after a visit to Naples by the present Minister of the Navy at St. Petersburg, Admiral Tchichatchoff; and splendid collections from the Pacific and the Indian Oceans, made by the naval physician, Mr. Isnaeff, have been added to the stores of the Moscow and St. Petersburg collections. I still hope that other navies may follow in this line, and I am sure that naval officers and physicians on board as well as naturalists at home would be greatly satisfied if the Italian and Russian examples became more generally imitated.

I am pleased at this opportunity of calling attention to the only case where the Zoological Station made use of special instruction as the most effective way to promote research. All the six hundred naturalists who have worked during eighteen years in the Zoological Station have done so relying only on their previously acquired education in Universities at home and abroad, and if even they went away from Naples better instructed than they came, it is simply because no one is more fitted to profit by example than he who already understands.

Let me now treat of the second objection, of which the author of the article speaks, regarding the "policy of continuing to support an already thriving institution for an indefinite period."

It is obviously more difficult for me to discuss this objection, and especially so after the author of the said article has once more most distinctly called the Zoological Station at Naples "Dr. Dohrn's Station." The author is right in calling me the founder, director, and proprietor of the Station, but I wish most distinctly to point out that my proprietorship involves only a burden and responsibility, and no advantage whatever of a material kind. I am a creditor to the Zoological Station, like other creditors, but with the clear distinction that my material liability is unlimited towards the other creditors, and my moral liability limited to that *imponderabile* called the public opinion of the whole scientific, and a great part of the unscientific, public, which takes an interest in or contributes to the maintenance of the Zoological Station.

But this same unlimited liability may excuse me if I take the liberty to state unrestrictedly the necessities and the conditions under which I hoped to succeed in an enterprise which, when I began it, was considered fantastical, almost Utopian, by many, perhaps by most, of my fellow-workers in biology. I meant from the very beginning to create an international institution, and I counted upon the loyal and lasting co-operation of all those, in whatever country, who understand the extraordinary importance of seaside studies, and who know from experience how difficult progress in biology had become from want of appropriate laboratories near the sea. I hoped, further, to enlist as supporters of the Naples Station all those naturalists who, with me, put the general interests of biology higher than the personal predilection for this or that branch of biological pursuit, and who could help me in securing the material support of Governments and learned bodies for the new institution, which was created under considerable difficulties, and for which I had undertaken to act as a responsible manager. To find myself without that co-operation could alone make me regret the labour and loss that I so incurred.

The author of the article calls the Zoological Station "an essentially German institution," and seems to believe France justified, "in view of national prejudice and having zoological stations of her own," in not having subscribed for one or more tables in the Naples Station. In fact I am German both by birth and culture, and shall remain so to the end of my days, and so are the greater part of my assistants, who have staked like me their existence on the prosperity and efficiency of the Zoological Station of Naples. But the very name of Naples indicates that one might quite as well call it an essentially Italian institution, and the more so as among my assistants there are several Italians of no less importance and service to the Zoological Station than my compatriots, and as Italy like Germany has behaved most generously in supporting the Station.

But I think the time has come when one must raise one's voice most distinctly against the narrowing limits of national prejudice, which nowadays has grown to almost overwhelming and even pernicious importance in many provinces of material and—I am sorry to say—also moral and intellectual existence. Science at any rate ought to be exempt from that morbid exclusiveness which refuses to act in rational community regardless of political or ethnographical boundaries. When I left my country to found the Zoological Station at Naples, I acted simply in the interest of science. I would certainly have preferred to found the Zoological Station in Germany if Germany had offered the same scientific advantages as Naples; or I would have gone to the North Cape or Ireland, if I had been convinced that biology were best served by building a station there instead of in Naples. My choice fell on Naples because I was and am still convinced that no place in the world combines so many advantages for biology as Naples, and no other place would so readily induce others to follow the lead which—it was, perhaps, presumptuous in a young man of thirty years of age—I, with the daring of just these thirty years, believed myself capable of taking, and even entitled to take.

As for France not following the example of almost all the other European nations, allow me to state that Claude Bernard, the great physiologist, asked the Minister of Public Instruction, M. Bardoux, to rent four tables for French naturalists at Naples; and if this has not been achieved, there comes in a greater obstacle than national prejudice—the untimely death of the great physiologist. I believe, indeed I know, that even now a view is predominant among some of the highest authorities of the French biological school, that France ought to be represented at Naples, and it is regretted in some quarters that "national prejudice" is allowed to triumph over those higher aims of the French mind, to which science, as we all know, owes such splendid manifestations and such grand achievements.

I do not know whether it is a better position to plead for the abstinence of France in view of the several French zoological stations; but as Austria has not ceased to rent tables at Naples though in possession of a national station at Trieste, so France might have found it well worth the outlay of an annual £100 to have a share in the maintenance and profitable use of the largest and the only international biological laboratory existing.

If it be alleged that the Naples Station is now a thriving institution, and not any more in need of being supported, as in the case of the table rented by the British Association, I am glad that the author of the article in NATURE gives the account of the receipts and expenditure of the Naples Station, and finishes with the statement, that "the Station would be carried on at a considerable annual loss were it not for the magnificent subsidy of £2000 a year, granted to its support by the German Empire, which just covers the deficiency." I think this statement answers more than fully the question of the desirability of the "international" support of the Naples Station. If it were true that the Station was essentially a German institution, the German Empire would certainly not ask for the support of any other State or foreign Association, but would receive foreign naturalists as guests in a laboratory maintained for the benefit of its own subjects. But the scientific and international importance of the Naples Station is so unrestrictedly recognized at Berlin, that whilst there is a movement on foot to create in Heligoland a "Prussian" biological station for home interests, I am distinctly told that this will in no way interfere with the generous subsidy given by the German Government to the Naples Station.

I believe myself to have been the first to suggest the formation of a net of zoological stations round the globe, and have been either actively or morally helpful in the formation of most of those now existing. If I have not carried out an old plan to assist personally in the creation of a Zoological Station at Sydney, which I considered, and consider still, of the highest importance to science, it was in deference to the remonstrances of my late friend Prof. F. M. Balfour, who insisted even more than myself upon the supreme necessity of a powerful central establishment of the kind, and opposed, even for a time against my own opinion, the plan for the foundation of a British biological station, on the ground that it was too early, and would so interfere with the thorough development and maintenance of the Naples Station.

And I think I ought not to conclude without once more respectfully and gratefully recording the splendid gifts of some British naturalists, headed by the late Mr. Darwin, to the Zoological Station, which, in a dangerous moment, went far to protect my, at that time, still isolated and not generally recognized efforts from falling short of the end in view. May these two names be suffered to test the high and purely scientific character of the Naples Station, and may this reference to them help to maintain the ties which, from the beginning, have been established between it and the British biologists.

ANTON DOHRN.

THE HIGH-PRESSURE AREA OF NOVEMBER 1889 IN CENTRAL EUROPE, WITH REMARKS ON HIGH-PRESSURE AREAS IN GENERAL.

UNDER this heading Dr. Hann, of Vienna, has recently had a memoir published,[1] in which he gives in detail and discusses the meteorological conditions and circumstances in the high-pressure area which remained nearly stationary over the Alps and the circumjacent territory in November 1889, during fourteen days. On November 6 there was high pressure over the Atlantic Ocean, France, and the southern part of England. On the morning of the 11th the centre lay over the North Sea, and on the 12th it was transferred to Central Europe, and nearly the whole of Europe was comprised within the high-pressure area, which continued until the 25th. During this time there was low pressure over the extreme north-west, north, and north-east of Europe, but no distinct storm-centre up to and even beyond the 60th parallel of latitude. The centre of high pressure, 780 mm. reduced to sea-level, lay over the eastern part of the Alps. The wind, as shown by the chart, seemed to blow gently out from this centre, and at the same time to turn toward the right, indicating an anticyclonic motion. The charts also show that the region of high barometric pressure corresponded with that of low temperature, the latter, however, without any reduction to sea-level.

After reducing the pressure and temperature observations of twelve high-level stations of the Alps and adjacent territory, with altitudes ranging from 1400 to 3100 m., to the level of 2500 m. of altitude, the centre of high pressure is found to correspond, at that level, with that at the earth's surface, and the temperatures, with little variation between stations, to be a little below that of incipient freezing.

The temperature on the earth's surface first sank under the influence of the high pressure below the normal. Before this, a temperature prevailed which was considerably above the normal, which first sank to the mean on the 11th and 12th, as the centre of high pressure was first transferred to Central Europe.

The dryness of the air at the mountain stations in the centre of the region of high pressure was extraordinary during the whole time from the 12th to the beginning of the stormy west winds on the 25th, and the daily mean of the relative humidity from the 19th to the 23rd ranged from 17 per cent. on the Wendelstein (1730 m.) to 49 per cent. on the Schneeberg near Vienna (1460 m.), while on the low lands with lower temperatures the air was nearly or quite saturated with aqueous vapour. In the higher strata of the air, therefore, during the high pressure, and especially during the latter part of it, there was very great dryness, while near the earth's surface the reverse was the case.

By comparing the observations of the lower stations above 500 m. and over, from the 19th to the 23rd, with the higher ones, it was found that through a range of 2050 m. there was an increase of $0^{\circ}\cdot8$ in the daily mean; but for the lower intervals of altitude, the increase of temperature with altitude, for an average range of 680 m., was $7^{\circ}\cdot1$. This indicates that the air was very cold near the earth's surface only, and that in ascending it rapidly became abnormally warm, and remained so up to the level of the upper stations, and, we have reason to think, much higher. This warm and dry air came not from the south, since, at a few high stations, as Sonnblick, Schneeberg in Tyrol, and Obir, the prevailing winds were northerly. It was a real *foehn*, with its characteristics of great warmth and dryness, arising from the gradual descent of air in the interior of the high-pressure area.

The departures of barometric pressure from the normal

[1] "Denkschriften der mathematisch-naturwissenschaftlichen Classe der kaiserlichen Akademie der Wissenschaften," Band lvii.

of thirty years (1851–80) in the valley stations from the 19th to the 23rd, ranged from plus 15·2 mm. to 17·9 mm., and so there was but little difference between the several stations. For the high stations the range was from 13·1 mm. to 15·2 mm.; and so here, likewise, the variations between stations was small, and the departures differed but little on the average from those at the low stations. They were somewhat in proportion to the whole pressures at the high and low stations. This was a consequence of the almost uniform temperature from the highest of the lower stations up to the highest.

While the temperature departures on the earth's surface from the normal were about $-3°$, they amounted at the high stations to $+8°$. The region of positive departures had a much greater vertical extent than that of the negative ones, which perhaps, on the mean, had not over 300 to 500 m. of depth. The mean temperature, therefore, in the central region of high pressure up to 3100 m. above sea-level, was warmed certainly about 6° over the normal, and as the air on the Sonnblick was 8° too warm, it is reasonable to suppose that, up to a great altitude, the air of the central region of high pressure must have had an unusually high temperature.

The limits between the warmer upper air strata and the under ones, which were relatively very cold and moist, seemed to be sharply drawn. It was principally made visible by the upper limits of the fog-formation. As this did not remain strictly at the same level, but oscillated a little vertically, places situated at about the same altitude were sometimes above and sometimes below this limit, and so were subject to very sudden and great changes of temperature. Places at all times above this limit had very dry air, and, during the day, continual sunshine, while the low stations were mostly in a very cold and moist air, with little or no sunshine. The upper warm strata floated over the cold under ones without mixing with or disturbing them.

Tables of the meteorological conditions for most of the high stations during the time of the high pressure are given, all of which show that there was an increase of temperature with increase of pressure, and that the highest temperature occurred generally soon after the time of highest pressure. A few tables for other times and places are given, which indicate the same thing. In fact, this matter has been so frequently and so ably worked up by Dr. Hann, and the results are now so well known and understood, that further researches in this direction seem almost superfluous.

The same memoir likewise gives the results of a similar investigation of a low-pressure area which lay over Central Europe, and centrally over the east side of the Alps, on October 1, 1890. The isobar of 755 mm. at this time inclosed the west part of the Mediterranean Sea and the northern part of Adria. The lowest pressures were at Vienna, Budapest, Prague, Bozen, and Riva, varying but little from 752 mm. at these places. The winds were gentle, and all Europe had rainy weather, and so the typical weather of a low-pressure area. On the mountain tops of the eastern Alps moderate and variable winds prevailed. The mountain tops were enveloped in clouds, and snow fell there; in the valleys, on both the north and the south sides of the Alps, there was rain.

The temperature departures of October 1 from the normal of three years of observations were negative at all the stations. By forming groups of stations of nearly the same altitudes, the following results were obtained:—

Altitudes (metres)	410	850	1700	2160	3100
Departures	−2°·7	−3°·3	−5°·5	−4°·8	−3°·8
Mean temperatures	8·6	6·0	0·8	−1·7	−6·5

The average departure of the whole air-column, therefore, was about $-4°$.

In a comparison of these temperatures with those from November 19 to 23, in order to not over-estimate the temperature in the high-pressure area the morning temperatures only of the latter were used, and thus the following results were obtained:—

Altitudes (hectometres)	5	10	15	20	25	30	35
Minimum— Oct. 1 ..	7°·9	5°·1	2°·3	−0°·6	−3°·4	−6°·2	−9°·1
Maximum— Nov. 19–23	− 2·7	+6·3	4·4	2·5	0·6	−1·3	−3·2
Max.–Min.	−10·6	+1·2	2·1	3·1	4·0	4·9	5·9

From this showing Dr. Hann concludes that at least from an altitude of 1000 m. up to 3500 m. the air in the high-pressure area was much warmer than that of the low-pressure area of October 1, although the latter was more than 1·5 months earlier.

He then estimates the mean temperatures of the air-columns up to an altitude of 3 kilometres, and obtains the following results:—

In the minimum of Oct. 1 ... ... $-0°·6$ C.
In the maximum of Nov. 19–23 ... $+1°·6$ C.

This seems to be a very fair presentation of the matter, and really, as he thinks, an under-estimate of the difference in the two cases.

A result obtained from the discussion of the temperatures on the Sonnblick, for both high and low pressures, is here referred to, which is that cyclones of the summer half-year cause a great cooling of the air up to at least 3000 metres, and that the mean temperature of the whole air-column in a summer cyclone, from the earth's surface even up to and above 5000 metres, is lower than that in an anticyclone.

Some of the deductions from the results of the memoirs by the author, on account of his prominence as a meteorologist, require here some special notice. With reference to these results he says, "So much at least is established, that an inquiry into the cause of the cyclonal and anticyclonal motions of the air must take into account the fact *that up to at least* 4 *or* 5 *km. the mean temperature of the air in the centre of an anticyclone may be higher* (perhaps, indeed, always higher) *than that in the centre of a cyclone.*"

He further says, in view of this: "Herewith fall the views which have sought the cause of these motions in the difference of specific gravity of the air in a cyclone in comparison with that of an anticyclone; in the impulse to which the air in a cyclone is said to be subject."

This seems to be said with reference to Espy's condensation theory, according to which the ascending air in a cyclone is warmer from the latent heat given out in the condensation of the aqueous vapour, and consequently lighter than the surrounding air. The argument, of which the conclusion merely is given, seems to be somewhat as follows. The temperature in the cyclone of October 1, 1889, was several degrees lower than the three years' normal, and also a little lower even than that of the anticyclone from November 19 to 23, more than a month and a half later. Also the temperatures of summer cyclones over the Alps, and a few other places, have been found to be less than the average temperature or normal. Therefore the temperature in a cyclone is less, and the specific gravity greater, than in the surrounding air at the time of the occurrence of a cyclone.

With regard to taking into account that the temperature in an anticyclone, in the sense in which it is used in the memoir, may be greater than that of a cyclone up to a considerable altitude, the writer does not see how that has anything to do with the cause of the motions of a cyclone, except, perhaps, in a few rare and special cases. During the fourteen days of the anticyclone over the Alps, even if the temperature within had been very much greater, he does not see why Espy's conditions of a cyclone could not have existed in America, on the Atlantic Ocean, or even in the north of Europe. These conditions, as is well known, are that the upper part of the atmosphere shall be cooled down below the normal, so as to give rise to a greater vertical gradient than usual, the gradient required being less for air entirely saturated, or nearly so, than for drier air. Whatever may have been the cause of the anticyclone, that of the cyclone was of course different; and in the case of the supposed cyclone in the north of Europe at the same time, there might have been a little overlapping and mingling of cause and effect on the adjacent sides, but neither cyclone nor anticyclone would have interfered materially with the other; and the complete conditions of a cyclone not being interfered with on all the other sides, these would have controlled the cyclone. Even the conditions of a small cyclone can exist in a large area of pressure considerably above the normal pressure, since it is only necessary that the vertical distribution of temperature be such that the ascending air, by its law of cooling, is kept a little warmer than the surrounding air. It is admitted that, in the case of a high-pressure area being formed and maintained for a considerable time, until the descending currents have caused an abnormal heating in the air within at some distance above the earth's surface, these conditions cannot be fulfilled.

Of course, if there was a ring of high pressure formed all around a cyclone, and maintained for some time, the *oehn* effect of the descending current would soon destroy it. A broad ring of this sort with a very moderate increase of pressure is always formed around the cyclone, but the ascending current of the interior of the cyclone comprises so small an area in comparison with the broad area all around when the air very gently settles back again towards the earth, that the *foehn* effect of the latter may be regarded as being almost insensible. So far as it goes, however, it tends to work the destruction of the cyclone, as well as the ascending currents do, by gradually decreasing the vertical temperature gradient in conveying heat from the lower to the upper strata of the atmosphere. Cyclones, in their nature, are not perpetual; for they are the means by which the atmosphere in an unstable state, which is necessary to give rise to a cyclone, is restored again to the stable state, and when this is brought about the cyclone must necessarily cease.

In the case of the anticyclone, so called, over the Alps, and all similar cases, the air comes in from a great distance on all sides, and descends over a comparatively small area towards the earth in the interior. Here the circumstances are reversed, and the *foehn* effect is very great, while the cooling effect all around, from the very slow ascent of air to compensate the downward current, is extremely small.

It is assumed by Dr. Hann that the high-pressure area over the Alps was an anticyclone, but he does not explain how an anticyclone gyration around the centre of this area could be originated, and maintained for so long a period. With an anticyclonal motion the air is pressed from all sides towards the centre by the force arising from the earth's rotation, and as the gyratory velocity below, at the earth's surface, must be diminished by friction, and this force made less there than that above, of course the descending air in the interior escapes there. If this anticyclonal gyration could be explained, it would furnish an explanation of everything else in the production of the *foehn* effect within. So far as it concerns our preceding arguments it is no matter whether it was an anticyclone or not, and want of space forbids the entering into a discussion of that subject here.

As in the case of anticyclones, so called, so it is with that of normals. They have nothing to do with the conditions of a cyclone, and the comparison of the temperature of the cyclone of October 1 with the three years' normal is not pertinent in an argument against the condensation theory of cyclones. As has been stated, this theory requires simply that the temperature of the interior of a cyclone shall be greater than that of the adjacent surrounding air at the time, without regard to what a three years' normal or any other normal may be. The three years' normal may be in error 1° or 2°, and the observed temperature may have been 5° below a true normal, and yet 5° or more above the prevailing temperature at a distance all around at the time. It is well known that the temperature departures from the normals are often very large and of long continuance. The excess of temperature in a cyclone above the surrounding temperature need not be large. If a column of air were 1° (1°·8 F.) warmer, from the earth's surface to the top, than the surrounding air, it would give rise to an ascending current at an altitude of three miles of about thirty-five miles per hour, not considering friction. Of course considerable allowance must be made for this, but, making ample allowance, there is still a velocity left much greater than that which usually exists in the ascending currents of cyclones.

It is true that the products of condensation, falling from high and cold altitudes, cool the earth's surface and the lower part of the atmosphere in summer cyclones. But this is an effect of the cyclone, and does not enter into the conditions which give rise to it. It affects surface temperatures mostly, and these are the temperatures observed in the Alps. The power of the cyclone is mostly above, in the cloud region, and surface temperatures have little or no effect. The air below in the cyclone may be colder than the surrounding air, yet, as soon as the gyration is started above, by which the pressure is decreased in the interior of the cyclone and increased around it, this lower colder air is brought into circulation by the difference of pressure thus produced, and the action upon it of that above by means of friction. But, as has been said, cyclones are not perpetual, and, so far as the cooling of the body of the air is concerned, it is one amongst others of the causes which retard and gradually destroy the motions of the cyclone after they have been once started. The principal part of the observed cooling in summer cyclones is, no doubt, due to the exclusion of the solar heat by the clouds, but this is compensated by its absorption in the upper part of the cloud.

Dr. Hann seems to have strangely overlooked the fact that cyclones do not occur in a normal, but an abnormal, state of the atmosphere; that is, when the upper strata are so cooled down as to give an unusually great vertical temperature gradient, and so an unstable state of the atmosphere. This has been illustrated in great detail by the writer elsewhere,[1] both in the case of dry and moist air. Since, then, cyclones can occur only when the upper strata are abnormally cold, and the temperature in the interior of cyclones, as we have seen, is but little above that of the surrounding adjacent air, the average temperature at high stations of a large number of cyclones must necessarily fall below the general average of temperature of the month, or season of the year, in which they occur, and in any special cases could hardly be expected to come up to this average. Not only in no special cases, therefore, but also in no cases of averages, can any argument be based upon the comparisons of cyclone temperatures at high stations with normals.

Another quotation which we make from Dr. Hann's memoir reads as follows:—"Ferrel's views with regard to the nature of anticyclones, as still maintained in his latest work, 'A Popular Treatise on the Winds' (London, 1889), are likewise in manifest opposition to the facts. A stationary anticyclone during 14 days over the whole of Central Europe, as in November 1889, and so many others of longer continuance (December 1879, January, February, 1882, &c.), cannot still be regarded as satellites or dependencies of cyclones, and support the

[1] "A Popular Treatise on the Winds, &c.," pp. 36 and 232.

position, 'The duration of the area of high pressure depends upon that of the cyclone' (p. 343). Rather the reverse is the case—the cyclones depend upon the anticyclones; these last rule the general character of the weather, and control the directions of the former. The anticyclones, also, do not depend directly upon the lower temperature, as Ferrel thinks (p. 344); the anticyclone of November 1889 disproves this in a striking manner, as well as the more general position, that the cause of the anticyclone is the increased density of the air through the diminution of its temperature."

In the first of these references to the writer's work, the anticyclone and ring of high pressure which accompanies the cyclone are spoken of. These are clearly deduced from theory, and always observed where not obscured by other irregularities. Many references could be given to show this,[1] and it is not, therefore, "in manifest opposition to the facts." It is stated that this ring of high pressure is often superimposed upon so many other irregularities that it is broken up into areas of high pressure, and a complete ring of high pressure is not observed. With regard to these it is said that their duration depends upon that of the cyclone which has caused them. Now, what argument can be drawn from the area of high pressure over the Alps, or any similar ones, against this reasoning the writer is entirely unable to see.

In forty-four cases of ridges, or areas of high barometer with an area of low barometer between them, passing over the United States, the late Prof. Loomis found the average distance from the centre of low barometer to that of the areas of high barometer preceding and following to be about 1000 miles. Now it is very reasonable to suppose—in fact, absolutely certain—that these ridges of high pressure are caused by the centrifugal forces of the gyrations of the cyclones, which drive the air from their centres and cause low pressures there, and at the same time give rise to the ridges of high pressure between them. Yet Dr. Hann would have us believe that these areas of high pressure do not depend upon the cyclones, but rather the reverse—the cyclones between are caused by the areas of high pressure.

Again, in this reference it is stated that "the anticyclones do not depend directly upon a lower temperature." In the high-pressure areas which immediately follow the great winter cyclones of the northern part of the United States and of British America, the writer took the position that the high pressure is not wholly due to dynamic causes, but in a great measure also to the lower temperature of the air on the north-west side of the cyclone, both from being brought down from a higher and colder latitude by the cyclonic gyration, and also from the increased radiation of heat into space, on account of the great clearness and dryness of this air in comparison with that of the cyclone which had just passed over. But Dr. Hann says this has nothing to do with the matter, and also that the increased high pressure in no way depends upon the cyclone; but he does not inform us what the cause of it is. A sudden change of 30° F. is no unusual change immediately after the passage of a cyclone, and if this extended to the top of the atmosphere, and without any diminution of the height of the atmosphere, this would cause an increase of nearly two inches in the barometric pressure. But it is not to be supposed that this great difference of temperature extends to the top of the atmosphere, and besides we must make some allowance for a little settling down and contraction of the height of the column, or rather disk, of cold air. But still it is very reasonable to suppose that the pressure is very much increased from the lowering of its temperature.

Of course, if this high-pressure area were a stationary one of 14 days' continuance, there would be a large body of heated air formed in its interior, commencing a little above the earth's surface and extending up to a considerable height, which would change the initial temperature conditions and tend to diminish the downward pressure, and finally to bring it to the normal pressure. This is what took place in the anticyclone, so called, over the Alps. It will be remembered that the high temperature and all the *foehn* characteristics took place from November 19 to 23, during the last part of the 14 days' continuance, and just before its sudden breaking up, which was caused by this increase of temperature within it. The vertical circulation being once established, of course it was continued by the momentum beyond the point where the temperature was equal to that of the surroundings, just as the pendulum does not stop when it has reached its lowest point, but continues on, even to the distance whence it started, if there are no frictional or other resistances. The same is true in the ascending air in a cyclone. It continues, by virtue of its momentum, not until its temperature is reduced to that of the surroundings merely, but to a still lower temperature, and until the diminished temperature and increased density gradually destroy the ascending velocity, and cause the air to be deflected laterally on all sides above. This is another reason why the observed temperature in a cyclone at high altitudes may sometimes be found to be below the normal temperature, for it may even be lower than the surrounding temperature, which, it has been shown, must itself be lower than the normal or average.

It is well known that the writer does not hold that areas of high pressure are anticyclones, and that his anticyclones surround and accompany the cyclones. It is, therefore, not logical to make observations upon one thing, as the long-continued high pressure on the Alps, and then to deduce arguments from them against something entirely different. And this is especially the case here, where it is not shown that the thing observed is an anticyclone in any sense. The writer's view is that high-pressure areas of long duration in winter depend mostly upon the diminished temperature, though dynamic causes may come in at first in the origin of the smaller ones, such as that over the Alps, already so often referred to. The areas of highest pressure and of the greatest extent are found over Europe-Asia, and only in the winter season. Of 81 cases investigated by Loomis,[1] 74 were found to have a barometric pressure of 31 inches and above, and their average extent to be 3800 miles from north to south, and about 4900 miles from west to east, the limits being determined by the isobar of 30 inches. It is unreasonable to consider these as anticyclones, and that the high pressures are due to dynamic causes. It is not only impossible to give any explanation of a supposed anticyclonal motion around so large an area, but the high pressure is very satisfactorily explained by the diminished temperature. In the northern and eastern part of Asia the normal January temperature is 45° (81° F.) below freezing; and this, from what has already been shown, is amply sufficient to give an increase of barometric pressure of more than an inch, making all the due allowances. Of course, in this high-pressure area there is a very gradual settling down of the air, and consequent increase of temperature in the lower strata of the atmosphere, a little above the earth's surface, but this is not sufficient to overcome the effect of the powerful radiation from and through the clear air during the long and clear winter nights of these high latitudes.

In the conclusion of the memoir Dr. Hann ascribes the origin of cyclones to the general circulation of the atmosphere, and considers them as being simply subordinate parts of this circulation, and independent of any local causes. It is somewhat different from the old hypothesis which prevailed before Espy's time, according to which

[1] "Popular Treatise on the Winds, &c.," pp. 269–270.

[1] Memoirs of the National Academy of Sciences, vol. iv. Part 2, p. 39.

all kinds of whirlwind phenomena originate in the meeting of counter currents of the air, and then continue often for days and weeks increasing in power and dimensions, without the apparent expenditure of any energy. He also thinks that the barometric maximums and minimums arise mostly from the descending and ascending motions of the air in the vertical circulation of the atmosphere. He says:—"There can scarcely be any longer a doubt that the pressure relations in the barometric maximums and minimums are generally explained for the most part by these kinds of motion. The forces which we really see in the atmospheric circulation in the higher latitudes, especially in winter, arise from the warmth of the tropics; that is, from the difference of temperature between the polar regions and the equatorial zone. The cyclones and anticyclones are merely subordinate parts of the general atmospheric circulation. The masses of air set in motion polewards by the upper gradients are resolved in part into great whirls, the principal progressive motion of which is controlled by the prevailing west component of the former. The influence of the inequalities of the earth's surface, the different heating and cooling of the land and ocean, and the bringing in of aqueous vapour and its condensation, come thus into account as matters of secondary importance."

Of course, nothing more can be claimed for most of this than hypothesis, but a mere hypothesis is not to be despised if it is a reasonable one. Let us now consider whether these hypotheses are of this character; and first, the hypothesis which ascribes the origin of cyclones to the general atmospheric circulation. The poleward velocities of the upper parts of the atmosphere are very small. The cirrus clouds of these regions, when not agitated by abnormal disturbances, have an apparently very gentle motion toward the east with scarcely any perceptible poleward motion. The velocity of this motion, especially in winter, may, at a maximum, amount to as much as three or four miles per hour, and that of the downward descent in the higher latitudes to a few inches per minute. Such motions are supposed to throw the atmosphere into a great whirl, several hundred miles in diameter, say in the north-west part of the United States. This whirl passes eastward, over the lakes and on to Newfoundland, with destructive and increasing violence all along its course, then passes into the Atlantic Ocean, and often into Europe, causing shipwrecks and other disasters on the way. And all this forms an incipient whirl produced by the gentle currents of the general atmospheric circulation described above. No attempt is made to show how a couple of forces could be formed from those which cause the general circulation, so as to give rise to a whirl, for this can originate in such a couple only, nor to explain where the energy comes from which keeps such a whirl in motion. It is true, Dr. Hann says the latent energy made free in condensation comes in as a matter of secondary consideration, which may either accelerate or retard, but he leaves it to be inferred that, in either case, the whirl would continue without it.

Again, a whirl originates in low latitudes in the Atlantic Ocean, over near Africa, where there are no poleward gradients; it progresses westwardly, and increases in diameter and violence; the partially condensed vapour rushes out from its top like the smoke and heated air from the flue of a great furnace, or the crater of a volcano, giving rise to a hazy cloud seen at a great distance towards the east; the wave of high pressure which precedes and surrounds such meteors is observed, while yet the air is very little disturbed at the place of observation; it arrives at the Windward Islands and then at Cuba, destroying factories and sugar plantations on the way; it passes over to Florida, and along the whole eastern coast of the United States, increasing in dimensions and violence as it goes, causing great injury to plantations and commerce; it finally ends in a great gyrating storm covering the most of the northern part of the North Atlantic Ocean. All this, this new hypothesis, now brought forward by Dr. Hann, requires us to believe originated in, and is a part of, the very gentle motions of the general circulation of the atmosphere, without any inherent power upon which the astonishing mechanical effects depend.

In order to have a gyration in the air, there must be a couple of forces to cause it, but no such couple can in any way arise from the general motions of the atmosphere, or the forces which give rise to them. And if such a gyration could arise in this way, it would have to be followed up by this couple to overcome the friction, and to continue the gyration. The general circulation of the atmosphere being once established as the result of certain forces, these same forces, according to sound principles of mechanical philosophy, can no more give rise to whirls, or any kind of local disturbances of these general motions, than the force of the sun's attraction, from which results the elliptic orbit of a planet, can cause perturbations in the motion in that orbit. Local effects must have local causes; and a cyclone can no more exist in the atmosphere, independent of any local cause, but dependent only upon the forces which give rise to the general circulation, than the motions of the satellites of Jupiter can be independent of the attraction of that planet, and depend simply upon the attraction of the sun.

As the old hypothesis required the whirl, once produced, to be followed up by counter-currents, so the new requires it to be followed up by a couple of forces, to keep it going. Scarcely anything, therefore, could be better or more appropriately said with regard to the new hypothesis, and in favour of the condensation theory of cyclones, than what Dr. Hann (*Zeitsch. für Meteorologie*, B. x., p. 82), has himself said with regard to the old hypothesis, namely:—

"No one can deny that whirls may arise from the meeting of currents at an angle, but it may be difficult, indeed impossible, to explain in this way the exclusive occurrence of gyrations from right to left in the northern, and the contrary in the southern, hemisphere. It is still more difficult to explain by this theory the fact that the whirl, once formed, progresses hundreds of miles; and meanwhile it continually draws into its motion new masses of air, overcomes a great amount of frictional resistance along the whole path, and produces powerful mechanical effects. Without a constant supply of energy it would be physically impossible—it would be the perpetual motion. To assume a continual new meeting of winds throughout in the requisite direction along the whole course of diversified wind regions, must appear to everyone, however, as a mere play of the imagination.

"The more recent theory of the origination of cyclonic motion through the deflecting force of the earth's rotation upon the air flowing in toward the minimum pressure entirely clears up the unexceptionable gyration of it from right to left in the northern hemisphere, and the contrary in the southern hemisphere. It also especially satisfies us in that it points out the source from which new energy always comes to the gyration when once originated. This comes from the fact that every large cyclone is accompanied by a rich condensation of aqueous vapour. The latent heat set free in condensation causes an accelerated ascension of the air in the cyclone, and so continually produces an underflow of the air from all sides. We now see that the gyration can advance forward, indeed that it must do so, if it continues to exist. The force which is necessary for overcoming the frictional resistance, for the drawing in of the hereto quiet air, for its powerful mechanical effects—this already is laid up in store in the air along the path over which it shall pass. It is still latent, but it is freed in being drawn in. When the cyclone does not find enough of aqueous vapour in the air, and at the same time has to overcome great frictional resistance, then must it soon come to a stand.

Since this more recent theory, in this way, gives us a conclusive physical reason of one of the most important phenomena coming into consideration in storms, I have allowed myself to designate it as the 'physical' theory."

Let us now consider the hypothesis that the maximum and minimum barometric pressures depend upon descending and ascending currents. Dr. Hann does not seem to confide wholly in the anticyclone hypothesis of areas of high pressure, though he still calls them anticyclones. He therefore devised another hypothesis to account for them—namely, that they result from an increase of pressure by the downward current which he supposes exists in these areas, and so regards them as the cause, at least mostly, and not as the effect of the high pressures. He still adheres to the old hypothesis that the zones of high pressure a little beyond the tropics in both hemispheres are caused by the crowding of the upper poleward currents into intermeridional spaces gradually becoming narrower toward the poles, and so by their being deflected down towards the earth's surface, although these high-pressure zones have long since been satisfactorily accounted for, without any mere hypothesis, upon true mechanical principles. Starting out from this hypothesis, he says that even beyond these zones there must be local obstructions and a damming up of the air in places in the higher latitudes, and a consequent deflecting of the currents down toward the earth's surface (*Zeitsch. für Meteorologie*, B. xiv., p. 39). This seems to be what is meant by the forms of motion, in the quotation above, to which is ascribed mostly the temperature relations in the barometric maximums and minimums. It does not appear, however, how the minimums of pressure can be explained by this hypothesis, for both ascending and descending currents require an increase of pressure at the bottom, where there are no lateral differences of temperature and density.

The preceding hypothesis, unlike many others, can readily be tested by means of the well-known formula showing the relation between pressure and velocity, which is based upon true and undisputed principles of mechanics. If there were a perpendicular wall around the globe on the 35th parallel of latitude, extending up to the top of the atmosphere, so that any poleward motion would have to be entirely stopped, and we suppose the upper half of the atmosphere between it and the equator to have a poleward motion toward this wall of 10 miles per hour, and that the whole is stopped, turned downward, and deflected back on the lower half of the atmosphere, the greatest increase of barometric pressure, according to the formula, which could arise from this, would be less than 0·004 of an inch. But a very small part only of the air in these high-pressure zones is stopped and turned downward, and the rest passes on to higher latitudes, so that the real effect must be very much less than this. But the observed excess of pressure in these zones is about 0·3 of an inch on the average. Hence the hypothesis could not account for the one-seventy-fifth part of it if all the kinetic energy were there converted into pressure; but considering the very small part which is so changed, it scarcely accounts for the one-thousandth part.

With regard to high-pressure areas being caused by descending currents, it would require a downward velocity of more than 170 miles per hour to cause an increase of 1 inch in the barometric pressure. The same effect would be produced by a horizontal current of that velocity if the kinetic energy were all converted into pressure by a total stoppage of the current; but where the velocity is only slightly hindered by a damming up through obstructions, the velocity would have to be many times more. Hence the hypothesis is entirely inadequate to cause even any measurable increase of barometric pressure.

WM. FERREL.

Martinsburg, W. Va., U.S.A.

THE FLORA OF THE REVILLAGIGEDO ISLANDS.

THE somewhat peculiar flora of Lower California, as revealed by comparatively recent American explorations, aroused the curiosity of botanists concerning the probable composition of the vegetation of the Revillagigedo group of islands, situated between 18° and 19° N. lat., off the west coast of Mexico. During the spring of 1889, the United States Fish Commission steamer *Albatross* visited the two principal islands, Socorro and Clarion; and Dr. G. Vasey and Mr. J. N. Rose have just published the results of their investigations of a collection of dried plants made in these islands by Mr. C. H. Townsend, the ornithologist of the expedition. A less interesting flora could hardly be imagined, if this be a fair sample of it; but on this point the report in question affords no information whatever. Considering the distance of the islands from the nearest points of the continent, and the size of the principal island, a flora possessing some peculiarities might have been expected, and possibly the few dried plants brought away by Mr. Townsend by no means represent the flora, either as to quantity or as to quality.

Socorro is described as the largest of the group, about twenty-four miles long by nine broad, with elevations up to 2000 feet; and the position is given as 18° 43′ 14″ latitude and 110° 54′ 13″ longitude, being about 260 miles south of Cape San Lucas, Lower California, and nearly the same distance from the nearest point of the Mexican coast. Clarion, a much smaller island, in nearly the same latitude, lies about 4° to the west.

"The total number of species found on the two islands was twenty-six; eighteen are from Socorro, and twelve from Clarion Island, four of which they have in common." The sentence quoted is preceded by the statement that the flora of these islands is doubtless tropical and similar to that of Mexico; a statement that is a little ambiguous, because, although these islands are situated within the north tropic, the plants collected are mostly characteristic of warm temperate and sub-tropical regions rather than of the tropics. In this apparently poor flora, for there is no mention of the existence of any other plants besides those enumerated, are such widely-dispersed plants as *Portulaca pilosa*, *Waltheria americana*, *Tribulus cistoides*, *Dodonæa viscosa*, *Sophora tomentosa*, *Elytraria tridentata*, and *Lantana involucrata*. Ten of the others are undetermined species of common genera, and may be common species; three are described as new, one of which had been previously collected in Lower California—*Cardiospermum Palmeri*, *Perityle socorroensis*, and *Teucrium townsendii*. The Mexican *Aristolochia brevipes*, and the widely spread tropical American parasite *Phoradendron rubrum*, are also recorded as doubtful identifications, the material being too scanty to admit of certainty. This is all the information one can extract from the report; perhaps a more detailed account of the islands and their natural history may appear in some other publication connected with the expedition.

W. BOTTING HEMSLEY.

ON LOCAL MAGNETIC DISTURBANCE OF THE COMPASS IN NORTH-WEST AUSTRALIA.

AS the subject of how far the compasses of a ship, when near land, are affected by local magnetic disturbance has hitherto been more frequently one of controversy rather than a study of facts, it seems important that full publicity should be given to well-authenticated observations.

In September 1885, on board H.M. surveying-vessel *Meda*, when passing Bezout Island near Cossack, North-West Australia, a steady deflection of her compass of 30° was observed, whilst the ship was running over half a mile in a north-north-west direction and in a depth of eight fathoms of water. This remarkable result has since been exceeded by observations made in H.M. surveying-vessel *Penguin* on November 6, 1890.

On this occasion, the *Penguin*, being 2 miles N. 79° E. from Bezout Island, a deflection of 22° was observed in her compass. The ship was immediately anchored, and some hours of the next day were spent in drifting backwards and forwards near that position, and the following results were obtained.

On Bezout Island the absolute values of the variation and dip were normal, the dip being 50° 1′·7 S. But at a position N. 79½° E., distant 2·14 miles from that on Bezout Island, the observed dip on board was 83° S., with a very small deflection of the compass. This may be considered the central point of the disturbing force. At 900 feet to the westward of this the dip was normal, and it decreased rapidly as the centre was quitted in any direction. At about 100 feet south of the centre of disturbance, the compass was deflected 55°. This was the largest deflection observed, but the compass was disturbed over an area of about a square mile. The general depth of water in this area was nine fathoms, and the quality of the bottom quartz sand.

The observations of the magnetic elements at Cossack and the neighbourhood showed little or no disturbance from local magnetic effects. It is therefore evident from the remarkable results now related, which have been derived from observations of undoubted accuracy, that the deflections of the compass in the *Meda* and *Penguin* were due to magnetic minerals at the bottom of the sea adjacent to the ship.

All well-authenticated observations point to a similar source, when deflections of the compass are caused by local magnetic forces external to a ship navigating near a coast. E. W. CREAK.

NOTES.

A GENERAL meeting of the International Congress of Hygiene and Demography was held on Monday. The Prince of Wales, who presided, announced that the Queen had consented to act as patron of the Congress. In the report of the Organizing Committee, which was read by Sir Douglas Galton, it was stated that the opening meeting would take place at St. James's Hall on August 10, under the presidency of the Prince of Wales, and the sectional meetings would be held on the five following days in the rooms of the learned Societies at Burlington House. A special committee had been formed to call attention to the Congress in India, and special representations and invitations had been forwarded to foreign Governments (through the Foreign Office), the colonies (through the Agents General), and also to county, municipal, and local councils throughout the country. The Corporation of London had voted £2000 for an entertainment to the Congress, to be given in the Guildhall, and there could be no doubt that much hospitality would be offered to the Congress during its session. The organization of the Congress involved a very large expenditure, first for making the Congress known throughout the world and making the necessary arrangements for its sections, and secondly for the publication of the transactions. It was estimated that between £5000 and £6000 would be necessary, and for this sum an appeal would be made. Sir Spencer Wells read a report from the Reception Committee, Dr. Corfield a report from the Foreign Committee, and Mr. Digby a report from the Indian Committee. It appeared that the Governments of France, Italy, and Switzerland, and several of the colonies, had already accepted invitations to send delegates to the Congress. The reply of the Indian Government had not yet been received, but Lord Cross was fully alive to the importance of India being represented at the gathering. In replying to a vote of thanks for presiding, the Prince of Wales said he had little doubt the seventh Congress would be even more useful than any of its predecessors.

MR. EDWARD BARTLETT, lately Curator of the Maidstone Museum (son of Mr. A. D. Bartlett, Superintendent of the Zoological Society's Gardens), has been appointed by Rajah Brooke to be Curator of the new Museum at Sarawak. Mr. Bartlett will shortly leave England for Sarawak to take up his new duties. The series to be placed in the Museum at Sarawak will be entirely confined to Borneo objects, which Mr. Bartlett will make every exertion to render as complete as possible. He will have the assistance of some excellent native collectors, by whose aid he hopes to make many discoveries in the still imperfectly known fauna of Borneo.

WE regret to have to record the death of Captain Daniel Pender, R.N., Assistant Hydrographer to the Admiralty. He died on the 12th inst., at the age of 58. Captain Pender had a remarkably extensive knowledge of the Pacific, where he served on various ships for a good many years.

DR. J. ERIC ERICHSEN was one of the speakers at the meeting held at the Mansion House on February 23, for raising a fund in aid of the extension of University and King's Colleges. His speech has now been printed separately, and ought to do excellent service to the movement on behalf of which it was delivered. Dr. Erichsen has much to say as to the good work accomplished by both institutions, and pleads eloquently for the support needed to enable them to comply adequately with the intellectual conditions of the present age.

THE Council of the Society of Arts will consider, early in May next, the award of the Albert Medal, and invite members of the Society to forward to the Secretary, on or before April 18, the names of such men of high distinction as they may think worthy of the honour. The medal was struck to reward "distinguished merit for promoting arts, manufactures, or commerce."

THE Shaen Memorial Wing of the Bedford College for Ladies (York Place, Baker Street), with the new science lecture-rooms and laboratories, to which we have already referred, will be formally opened by the Empress Frederick on Tuesday, March 24, at 4 p.m.

THE second International Ornithological Congress is to be held in Budapest at Whitsuntide. The opening ceremony and exhibition is on Sunday, May 17, and the concluding session on May 20, after which various excursions, beginning on the 21st, will be made. It is intended to divide the Congress into seven Sections, viz.: (1) Systematy; (2) Biology; (3) Anatomy; (4) Avigeography; (5) Oology; (6) Migration; (7) Economical Ornithology. The President of the General Committee is Count Bethlen, the Minister of Agriculture, and the President of the Scientific Committee, Dr. Otto Herman. The official head-quarters will be the National Museum, Budapest.

AN Electrical Exhibition was opened by the Lord Mayor on Monday in the St. Pancras Vestry Hall. The electric light is to be supplied to the parish by the Vestry, and the object of the present exhibition is to familiarize the inhabitants with the various conditions of electric illumination.

AMONG the deaths registered in the Zoological Society's Gardens last week, was that of a European Crane (*Grus cinerea*), which was acquired by purchase on May 13, 1848, and had thus lived nearly forty-three years in the Gardens. This is not quite so good a case as that of the Black Parrot (*Coracopsis vasa*), which died in 1884, after having lived fifty-four years in the Regent's Park.

THE Geologists' Association has made arrangements for an excursion to the Isle of Wight at Easter. Head-quarters for the first two days will be at the Totland Bay Hotel, and for the remainder of the excursion at the Blackgang Hotel.

THE National Congress of French Geographical Societies will hold its twelfth session this year at Rochefort.

THE Société Botanique de France has decided to hold an extraordinary meeting at Collioure, a small town in the Pyrénées Orientales Department, from May 16 to 24. Many excursions will be made. The region is interesting to botanists, and has been rather neglected.

IN the *Kew Bulletin* for March there is a valuable list of the orchids which flowered at Kew in 1890. It enumerates 766 species and varieties, and is published to afford data as to the time and duration of the flowering period of orchids cultivated in England. In the Kew collection no attempt is made, by the cultivation of a large number of examples, to give prominence to the most showy-flowered. On the other hand, as the *Bulletin* explains, every effort is made to obtain and cultivate even small and unattractive kinds of scientific interest, such as the ordinary collector would consider beneath his notice. In the limited space available for orchids as comprehensive a collection of species as possible is aimed at. Consequently, whilst there is never a great display of orchid flowers at Kew, at no time of the year is the collection wanting in flower interest. Thus, whilst the highest number of species flowered in any one month was 125 in May, the lowest was 85 in January. The average for each month was a fraction over one hundred. In 1811 the number of species in cultivation at Kew was only 37. There are now 1342 species, comprised in 158 genera. These figures do not include 174 varieties, and over 100 undetermined plants. The collection is kept up by means of exchange, and a small outlay, about £20 annually, for plants which can be obtained only by purchase.

THE same number of the *Kew Bulletin* contains an official correspondence on the experimental cultivation of Egyptian cotton on the Gold Coast, and a note on "Dammar from New Caledonia."

IN the second of the Cantor Lectures on Photographic Chemistry, delivered on Monday, the 16th, at the Society of Arts, Prof. Meldola called attention to the importance of the principle of associating other substances with the compound undergoing photo-chemical decomposition so as to increase the sensitiveness of the latter. As an illustration of this principle the lecturer alluded to a discovery which has recently been made, and which was practically demonstrated by the discoverer, Mr. F. H. Varley, at the conclusion of the lecture. By associating iron salts with suitable sensitizers, it has been found possible to prepare films quite as sensitive as any of the modern gelatine emulsions, and containing no trace of any silver compound. The advantage of such films, from an economical point of view, is obviously very great, and a new departure in the applications of photography to scientific and other purposes is likely to originate with the exaltation of the sensitiveness of iron salts.

AT the meeting of the Academy of Natural Sciences, Philadelphia, on January 27, Prof. Heilprin exhibited a specimen of *Porites astræoides* from the Caletta Reef, harbour of Vera Cruz, Mexico, which gave some interesting data regarding the rate of growth of coral structures. The specimen in question was received through Captain J. Powell, Chief of Construction of Piers of the Mexican Railway, and is said by that gentleman to have been removed from an anchor which was cast in the autumn of 1885 and drawn in November 1890. The extreme period of growth is thus somewhat over five years, but naturally it is impossible to state how soon after the casting of the anchor attachment of the polyp was made. The coral is a mammillated sheet or crust measuring four inches in longest diameter, and somewhat less than three inches on the shorter diameter. The general thickness of the basal mass is not over $\frac{1}{8}$–$\frac{1}{4}$ inch, although through involution and secondary crustage knobs of considerable prominence have been added to the surface. Assuming the basal growth as the index of actual development, then the annual accretion would be (if we allow full five years for the process) scarcely $\frac{1}{20}$ of an inch. Observations recently made on other species of corals have yielded somewhat similar results.

M. AMÉDÉE BERTHOULE has published a useful little work on the lakes of Auvergne. The subject is considered especially from the pisciculturist's point of view, but some very good photographs of scenery are given, and the author touches on various geological topics.

AT Mont-Dol, in Brittany, already well known to geologists and palæontologists, the remains of about a hundred elephants have been discovered, gathered on a small surface of about 1900 square metres. All the bones are broken, and it is thought that the animals must have been eaten by prehistoric men.

THE French Society of Physiological Psychology has appointed a Committee to investigate cases in which persons suppose themselves to have seen or heard some one who was not really present. M. Sully Prudhomme, member of the Academy of Sciences, will act as President.

THE Quarterly Reports of the Palestine Exploration Fund contain monthly meteorological observations taken at Sarona, Syria, beginning with July 1888, the results of which are deduced by Mr. James Glaisher. The statement for January last contains a comparison of the atmospheric pressure in Palestine and in England for the ten years ending 1889. Mr. Glaisher shows that the mean monthly readings at Sarona are highest in the winter months, but very seldom so high as 30 inches; the lowes are in the summer months, but none so low as 29·6 inches, so that the mean monthly atmospheric pressure is very uniform. The highest absolute reading was 30·285 inches, and the lowest 29·442. Both these readings have occurred several times in the month of January.

THE injury from hail in Würtemberg during the sixty years 1828-87 has been recently investigated by Herr Bühler (*Met Zeits.*). The yearly average of days with hail is 13, and about 0·92 per cent. of the cultivated land was affected, damage being done to the extent of about £120,000. July had most hail (34 days), June coming next (with 30·1 days). There is no evidence of increase of hail in the course of decades. The Black Forest district seems to have specially suffered. The author makes out 17 paths of the hailstorms. One very often frequented is that on the Danube, from Scheer to Ulm (70 km. long and 15 broad). All the paths seem connected with the configuration of the ground, and limited in many cases by quite low heights. Slopes with a western exposure are more in danger than those with an eastern; and plains suffer much less than hilly ground The frequently affirmed influence of forest on hail-fall is not distinctly proved by the Würtemberg data. Herr Hellmann has made a further study of the figures, and finds that in Würtemberg, as in the Rhone Department and in Carinthia, the chief maximum falls in the second half of July. A secondary one, nearly as high, occurs in June 20–24; this holds also for Carinthia, while in the Rhone Department this maximum is earlier, in the first half of June.

THERE seems to be some room for improvement in the methods adopted at the institutes which teachers in Indiana are compelled to attend. This year, the study at these establishments is botany, and the work is laid out for each month. According to the *American Naturalist*, the teachers will have to study the dog-tooth violet in November; in December they will be searching their gardens for flowering tulips, and scanning the orchards for the blossoms of the apple and peach; and in January the flower and fruit of the strawberry will form the subjects of discussion.

The Government of New Caledonia proposes to establish a Museum at Noumea, and has appealed for support to members of the Civil Service, native chiefs, persons who are known to occupy themselves with scientific inquiries, and colonists generally. It is hoped that the authorities may be able to form important collections, not only of natural products, but of objects interesting to anthropologists.

Messrs. Macmillan and Co. are issuing a new and thoroughly revised edition of "A Treatise on Chemistry," by Sir H. E. Roscoe and C. Schorlemmer. In the part recently issued (Part III. of Vol. III.) the authors deal with the chemistry of the hydrocarbons and their derivatives. The whole of the subject-matter has been revised, but they draw attention especially to the renewed discussion of the constitution of benzene, and to the researches of Nietzki and his co-workers on the higher substitution-products of benzene, which have explained the constitution of the remarkable substances derived from the explosive compound of potassium and carbonic oxide.

A new edition of the "Year-book of New South Wales" has been issued. It contains much well-arranged information as to the history and resources of that colony.

Messrs. George Bell and Sons have published "The School Calendar, and Hand-book of Examinations and Open Scholarships, 1891." This is the fifth year of issue, and the present volume will certainly not be less welcome than its predecessors. In a preface Mr. F. Storr sums up the educational events of 1890.

Mr. L. Upcott Gill has published an excellent "Book of Aquaria," which will be most welcome to many students of the ways of aquatic creatures. It consists of two parts, one dealing with fresh-water aquaria, by the Rev. G. C. Bateman, the other with marine aquaria, by Reginald A. R. Bennett.

Messrs. D. C. Heath and Co., of Boston, are publishing a series of "Guides for Science-Teaching." We have received the eighth volume, which treats of "Insecta." It is by A. Hyatt and J. M. Arms, and contains a series of replies to questions which have arisen in the minds of the authors while teaching.

We learn from the *Journal of Botany* that the series of British plants exhibited in the Botanical Gallery of the Natural History Museum has recently been extended by the addition of a series of British mosses, consisting of 576 species arranged in 129 genera. The arrangement is that adopted by Hobkirk in the second edition of his "Synopsis," and the descriptions have been taken from that work. The Museum has also acquired the extensive herbarium of the late M. Triana, containing upwards of 8000 plants, as well as a large collection from the province of Atacama, Chili, made by MM. Borchers and Philippi.

The same journal informs us that, at the request of the Irish Land Commissioners, Mr. W. Carruthers, F.R.S., is preparing a plain account of the potato disease, with illustrations drawn by Mr. W. G. Smith, which will be reproduced in chromolithography as a wall-diagram for schools and farm-houses. A reproduction of Bauer's water-colour drawings of the germination of wheat, in the form of six wall-diagrams for educational purposes, is also being prepared, under the direction of Mr. Carruthers, for publication by the Royal Agricultural Society, at a sufficiently low price to bring them within the reach of the poorest schools.

Silicon bromoform, $SiHBr_3$, has been obtained in the pure state by M. Besson, and an account of its mode of preparation and more important properties is given by him in the current number of the *Comptes rendus*. It is well known that hydrobromic acid exerts an action upon crystalline silicon of a somewhat similar nature to the action of hydrochloric acid, which forms a mixture of silicon chloroform and tetrachloride; but hitherto, M. Besson states, the products obtained have never been separated. Buff and Wöhler, the discoverers of silicon chloroform, $SiHCl_3$, long ago obtained a colourless fuming liquid by the action of hydrobromic acid upon silicon, resembling the corresponding chloroform in properties, but which was certainly contaminated with other products, especially silicon tetrabromide. M. Besson has isolated the compound by repeated fractional distillation of the product obtained by passing a stream of dry hydrobromic acid gas over crystals of silicon heated to a temperature just below redness. The main bulk of the liquid product consisted of silicon tetrabromide, boiling at 153°, but 5 per cent. distilled constantly at about 110°, and gave numbers, on analysis, closely agreeing with the formula $SiHBr_3$. Pure silicon bromoform is a colourless liquid, most difficult to work with. In the first place, it fumes exceedingly strongly at the first contact with air, and in a few minutes spontaneously inflames. Again, the vapour forms highly explosive mixtures with air which occasionally suddenly detonate with great violence. It is only possible, of course, to distil it in an atmosphere of an inert gas, when it boils at 110°. It still remains liquid at temperatures as low as −60°. Water at once decomposes it, and with solutions of alkalies the decomposition is very violent. Strong potash liberates twice as much hydrogen as is contained in the compound, the reaction proceeding as follows:—

$$SiHBr_3 + 5KOH = K_2SiO_3 + 3KBr + 2H_2O + H_2.$$

Dry ammonia gas also reacts in a very lively manner with silicon bromoform, and if the reaction is not moderated it is accompanied by incandescence. The white product appears to consist of a definite compound mixed with more or less of its products of decomposition. Phosphoretted hydrogen is without action under ordinary circumstances, but when compressed to 25 atmospheres in a Cailletet apparatus in contact with a few drops of silicon bromoform at the ordinary temperature, a white solid body is formed, which persists for some time after the pressure is removed, and which loses phosphoretted hydrogen in a stream of carbon dioxide. Silicon chloroform, when in contact with compressed PH_3 under like circumstances, forms an analogous substance in definite isolated crystals, which rapidly grow as long as the pressure is maintained.

The additions to the Zoological Society's Gardens during the past week include a Passerine Parrakeet (*Psittacula passerina*) from South America, presented by Miss Edith B. Burrell; a Markhoor (*Capra megaceros* ♂) from North-East India; a Bennett's Wallaby (*Halmaturus bennetti* ♀) from Tasmania; an Indian White Crane (*Grus leucogeranos*) from India, deposited; a Striped Hyæna (*Hyæna striata* ♂) from North Africa; a Maguari Stork (*Dissura maguari*); a Brazilian Teal (*Querquedula brasiliensis* ♂) from South America, purchased; an Indian Muntjac (*Cervulus muntjac* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

The "Capture Theory" of Comets.—A memoir by M. O. Callandreau, on the capture theory of periodic comets, has recently been published (*Annales de l'Observatoire de Paris*, vol. xx.). It is generally known that the periodic comets are distributed in groups which depend in some manner on the major planets. Jupiter's family of comets is at least fifteen in number, and all the members of it have direct motion, orbits only slightly inclined to the orbit of Jupiter, and aphelion points near

Jupiter's aphelion; what is more—one of the two points where each of them intersects the plane of Jupiter's orbit is generally very near to the trajectory of this planet. The theory which best explains such distribution is that which regards the comets of which the groups are composed as having come under the perturbing influence of the major planet to which they are respectively related. If a comet arrives from interstellar space into the solar system with a sensible parabolic velocity, and passes near a major planet, the velocity will be either diminished or increased. In the former case, the parabolic orbit would be transformed into an elliptical one, and the comet would be, as it were, incorporated into our system—captured by the planet. If, on the other hand, the velocity is accelerated, the orbit becomes hyperbolic, and the comet moves away from our system, never to return. The results of a research on this subject were given by M. Tisserand a few months ago (*Bulletin Astronomique*, July 1889, and NATURE, vol. xlii. p. 31).

M. Callandreau has at present only investigated the strong perturbations which a comet experiences when passing in the neighbourhood of a major planet—that is, a particular case of the problem of three bodies. He has considered the perturbations when a comet approaches very near to the disturbing body, and examined the difficulties connected with the capture theory. The theory that periodic comets are "ejects" from the major planets is mathematically discussed, and shown to be an improbable one. But it is not sufficient to show that periodic comets may be produced by capture; it is necessary to explain why the hyperbolic comets which the capture operation ought to engender escape observation. M. Callandreau proves that such comets are not seen either because their perihelion distance is very great, or because they only pass perihelion once, and then move to infinity on the hyperbolic orbit. Many other conditions are treated, and similarly interesting results obtained. An accurate knowledge of the formation of comets is of great importance in cosmogony. Such a discussion as the one before us is a decided advance in the matter, the demonstrations being in accordance with M. Callandreau's established reputation.

ANNUAIRE DE L'OBSERVATOIRE DE BRUXELLES.—This interesting *Annuaire* for 1891 has just been received. It is composed of ephemerides containing astronomical data for the ensuing year, statistical, geographical, and meteorological information, and articles on various scientific subjects. The mean positions of the principal stars, with the right ascension for every tenth day, occultations of stars by the moon, and eclipses of Jupiter's satellites are tabulated, as in previous years. Tables are also given of physical units and constants, and a detailed note on absolute measures, on the definition of different electrical units, and on their expression in absolute units. Another section contains a large amount of physiographical information. Dr. Folie contributes an article on diurnal variations in the height of the Pole; M. Spée, one on solar activity in 1890; and M. Lancaster gives an extended account of the climate of Belgium in the same year. An important article on the similarity between maps of the earth and other planets is from the pen of M. W. Prinz. Elements of the planets, and of some of the asteroids discovered in 1890, are also given. The obituary notices refer to the late MM. Montigny, Fievez, and Pirmez.

NEW ASTEROIDS.—Prof. Millosevich discovered the 307th asteroid on March 1, and M. Charlois the 308th on March 5.

THE LONDON-PARIS TELEPHONE.

LONDON and Paris are now connected by means of a telephone, and the completion of so great an enterprise deserves to be specially noted. The scheme was originally proposed by the French Government. It was at once taken into favourable consideration in England, and, when Mr. W. H. Preece had proved that it was practicable, it was adopted by the Postmaster-General.

The following details are taken from the *Times*, which printed on Tuesday a full account of what had been done in the matter. The scheme involved the construction of a trunk telephone line between the two cities, with a telephone cable across the Straits of Dover, the first ever made for the open sea. It was decided to have two separate circuits, so that if one should fail at any time, the other might be in use. The route for the English land line was chosen by Mr. Edward Graves, the Engineer-in-Chief to the Post Office, who has taken a keen personal interest in the whole work. It runs along the South-Eastern Railway to a point near Sidcup, and thence by road and rail through Swanley, Maidstone, and Ashford to the cable-house on the beach at St. Margaret's Bay, between Dover and Deal. The building, which began in September last, was continued throughout the severe frost, except when it snowed too hard to see, and the work was completed by the first week in March. The wire is of copper, the best material for the purpose, and weighs 400 pounds to the mile. The connection between the last pole on the chalk cliff at St. Margaret's Bay and the cable hut on the beach is effected by lengths of the cable core inclosed in an iron pipe and buried in a trench down the face of the cliff. The whole line is 85 miles long, and its excellence is proved not only by the electrical tests, but by the wonderfully clear and loud speaking through it between the cable-hut and the General Post Office. The voice of the speaker in London can be recognized at the hut, and the ticking of a watch distinctly heard.

The French land line follows the direct route of the Chemin de Fer du Nord, through Montdidier and Calais to the cable-house at Sangatte, between Calais and Boulogne. It is similar in construction to the English line, except that only one circuit is run at present, and the copper wire weighs about 600 pounds a mile. Its length is about 204 miles, and the speaking with the D'Arsonval apparatus employed in France is also excellent.

The connecting cable, which is the joint property of the two Governments, was designed by Mr. Preece, and contains four separate conductors, two for each circuit. It was taken on board Her Majesty's telegraph ship *Monarch*, on Monday, March 2, and the following day, in order to be laid when the weather was favourable. On Tuesday evening, March 3, the *Monarch* left her moorings near the *Warspite* and put to sea. Next morning she arrived off St. Margaret's Bay, and afterwards she steamed across to Sangatte; but for several days there was a nasty swell on the sea and a disagreeable haze. After waiting nearly a week in hopes of better weather, the morning of Monday, March 9, broke fine and clear. The long-expected opportunity seemed to have come, and preparations were made for landing the shore end of the cable into the hut at Sangatte. The two lifeboats were lowered, and a strong platform placed across them to form a raft, on which a length of cable sufficient to reach the shore was quickly coiled by the cable hands. The steam launch took the boat raft with the black coil of the cable in tow, the men paying it by hand as she went along to ground. She cast off and gave place to the men, who, in their white overalls and sea-boots, dragged the cable up the sand, along the trench, and into the cable hut. It was half-past 9 when the lifeboats were launched, and 12 minutes to 11 when the end was landed. No time was lost in returning to the ship, which immediately started paying out towards St. Margaret's Bay. The cable ran smoothly out of the tank, through the iron "crinoline," which keeps it from lashing about with the rolling of the ship, it glided along the guides, took three turns round the huge revolving iron drum, with its friction brake which controls the speed of egress, and passed over the starboard sheave or pulley projecting from the bows, then dived into the sea, just grazing the hull about the water line. Mile after mile was traversed in this way, and all was going on well. As yet there were no signs of an approaching storm. A drizzling rain began to fall, and the breeze freshened, but it was not until towards 3 o'clock, when 10 miles of cable had been paid out, and the *Monarch* was half seas over, that the gale came on, and the water became rough. At length it was decided to anchor until a lull in the storm should reveal the land, if only for a little while. The cable was fastened, and the anchor rattled out soon after 4 o'clock. The snow cleared about 5 o'clock, and it was then discovered that the *Monarch* was lying off St. Margaret's Bay, about a mile from the shore, and eastward of the cable hut. An attempt was made to lift the anchor and pay out all the cable, but the strong tide, aided by the furious wind, had driven the cable foul of the anchor, and after a fruitless attempt to clear, the anchor was slipped with 14 fathoms of chain. It was now a quarter past 8 at night, and very dark, but the *Monarch* paid out the rest of the cable to avoid cutting it, and buoyed the end well off the shore to the east of St. Margaret's Bay, about 20 minutes past 9, then ran for the Downs, where she anchored soon after 10 o'clock. Next morning the weather made further operations impossible. Wednesday was not much better, for, although it brightened up, the glass was still unsettled. The

Monarch was now lying at Dover, where she went to land a visitor and take in stores. Thursday was fine, and after picking up the cable from the buoy, she proceeded to clear it from the lost anchor. The line was coiled four times round the anchor, and could only be released by cutting out the damaged part. This was done, the anchor and chain being recovered, and the end of the cable buoyed. She returned to Dover. On Friday nothing could be done owing to the high wind and sea; but Saturday morning was as quiet as a lamb, the blue sky smiling through fleecy clouds. The *Monarch* was early astir, and although the sea was a little hazy, and a strong easterly breeze blowing, the glass was very steady. The ship had spliced the cable by 20 minutes past 11, and then picked up some 5 miles of cable from the buoy, towards Sangatte, relaying it so as to clear a bight in the Calais-Dover line, arriving off St. Margaret's Bay about 20 minutes past 3 in the afternoon, where she anchored 1000 yards away from the landing-place. A raft was speedily formed with the lifeboats, and the shore end landed in the same way as at Sangatte. It was now getting dusk, but groups of spectators had collected on the beach to watch the operations, and a local photographer, deputed by a London illustrated paper, took a picture of the scene. The end was hauled ashore by the sailors at 10 minutes past 6, and 12 minutes later brought into the cable hut. Lieutenant O'Meara called up St. Martin's-le-Grand and announced the good news. Three cheers were given at the Post Office and in the hut through the land line, and those from London sounded so lustily that the lieutenant declared they had split the drum of his telephone. The end of the cable was then stripped and the sheathing filed off, the rasping of the file being plainly heard in London. The cores were then pared, and the cable connected to a Morse apparatus, by which the hut was put in communication with Sangatte. The French electricians there telegraphed a "hurrah for the telephone," and the work was done.

COCO-NUT BEETLES.

THE destruction of coco-nuts in the Straits Settlements by insects has been so great that of late much attention has been given to the question. Perhaps the most important contribution that has yet been made to our knowledge of these pests is a recent report by Mr. H. N. Ridley, Director of Forests and Gardens at Singapore, on the destruction of coco-nut palms by beetles, which has been printed by the Government and issued from the Colonial Secretary's Office. There are, Mr. Ridley says, two species of beetles which are especially destructive to coco-nut palms. The first is the *Oryctes rhinoceros*, commonly known as the rhinoceros, elephant, or black beetle, and the other the *Rhynchophorus ferrugineus*, known as the red beetle. Two other larger species of Calandra attack some palms at Singapore, but Mr. Ridley has not received any notice of their attacking coco-nuts.

The *Oryctes rhinoceros* belongs to the group of Lamellicornia. The parent beetle usually deposits its eggs in decaying coco-nut trees. The identification of the larvæ is very difficult, for the grubs of all the larger Lamellicorn beetles are very much alike. The larva is white and fleshy, and when full grown is about three inches long; the head is round and hard, and is of a dark chestnut colour. It is covered with short bristles; the legs are about half an inch long; the antennæ are short and hairless, and the jaws thick and strong. The chrysalis has the form of the perfect insect; but the insect is very rarely found in this state. The beetle itself is sometimes two and a half inches long; it is very broad, and is of a dark-brown or black colour, and its chitinous coat is very hard. The head of the male is small, and has a horn, about half an inch long, curved towards the back. The wing-cases do not quite cover the body; they are broad and oblong, and covered over with minute punctures. The legs are strong, and the second joint is armed with teeth, by means of which the beetle cuts its way into the tree. The female is usually much smaller, and is readily distinguishable from the male. The grub is quite harmless, but the perfect insect is most destructive. It always works at night, attacking the base of a leaf-stalk, burrowing into the heart of the cabbage, where, as a rule, it remains all the next day. The attack is generally renewed till the rain finds its way in and rots the palm. The destruction of the tree is hastened by the fact that when once a tree has been attacked it appears to become popular. Besides the coco-nut palm, very many other palms, a list of which is given by Mr. Ridley, are destroyed by this insect, but, so far as is known, it does not attack other trees. The present methods adopted for destroying the *Oryctes rhinoceros* are described and criticized in the report. The usual mode is to search for the beetles in the palms, and spear them with a flexible iron wire. Large fires are also made in the plantations at night, and the beetles, flying towards the light, are beaten into the flames by men and boys with branches of trees. Mr. Ridley does not hope to exterminate the pest, but he thinks that its numbers can be greatly reduced by destroying in all the plantations rubbish and vegetable refuse of all kinds. Dead trees should be burnt, and the law should prevent any planter from allowing any heap of vegetable matter, in which the insects always breed, accumulating, and also from keeping any dead trees on his land. By this simple measure the ravages of the beetle can be minimized, if not quite abolished.

The second species of beetle spoken of in this report is the *Rhynchophorus ferrugineus*, the red beetle, which is, perhaps, even more destructive than the other. In the case of *Oryctes rhinoceros*, it is the perfect insect which is destructive; in the present instance, it is the grub. It attacks the trees at night, and having perforated the base of the leaf-stalk, it pushes the egg deeply into the body of the tree. The grub is white and footless, and tunnels through the soft portion of the palm. Unfortunately the presence of this insect in the tree is not so easily detected as in the former case. The grub is a thick, cylindrical, white larva, without feet or antennæ. The head and jaws are small, and the body curved and wrinkled. The perfect insect is usually about two inches in length. The head is small and usually red; the wing-cases are black, sometimes ornamented with red, and a good deal shorter than the body. The legs are black and long, and have a strong claw at the end of the second joint, and two small ones on the feet. The methods of destruction used by the planters are very similar to those used in the case of the rhinoceros beetle, but on account of the difficulty of tracing the red weevil they are not so efficacious. If the black beetle is much reduced in numbers, the effect will be to reduce the red beetle also very much, for the latter will not then be able to take advantage of the holes which have already been made by the former. In dealing with this beetle also, the report urges the necessity of making the destruction of all vegetable refuse compulsory, particularly in the neighbourhood of the palm plantations.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The General Board of Studies have re-appointed Mr. J. E. Marr, Sec.G.S., Fellow of St. John's College, to the Lectureship in Geology, for five years from Lady Day.

The subject of the Adams Prize Essay of 1893 is "The Methods of Determining the Absolute and Relative Value of Gravitation and the Mean Density of the Earth." Candidates must be graduates of the University. The value of the prize is about £170.

Mr. S. F. Harmer, Fellow of King's College, has been nominated to the use of the University table at the Naples Zoological Station.

The Mechanical Workshops Enquiry Syndicate have issued an important memorandum, setting forth a scheme of practical and theoretical instruction in engineering within the University; and state that a sum of £20,000 will be needed for the establishment and equipment of the necessary laboratories. As the funds are not to be had in Cambridge, they propose to make an appeal for benefactions outside the University.

The Agricultural Education Syndicate have issued a voluminous report, containing a complete plan of education and examination in agricultural science and practice, leading either to the B.A. degree or to a diploma in agriculture. It involves the formation of a Board of Agricultural Studies, and the foundation of readerships or lectureships in agricultural botany, chemistry, physiology, &c. Without the amplest pecuniary assistance, the plan is likely to fall by its own weight, but the Syndicate plainly indicate their expectation that adequate subsidies will be forthcoming from the County Councils and from the Government. The report is signed by the fourteen members of the Syndicate, including the Vice-Chancellor, Lord Hartington, Lord Walsingham, Canon Browne, Profs. Liveing and Foster, and Mr. Albert Pell.

SCIENTIFIC SERIALS.

Studies from the Johns Hopkins University Biological Laboratory, vol. iv., No. 7, October 1890, contains:—Notes on the anatomy of *Sipunculus gouldi*, Pourtales, by E. L. A. Andrews (plates xliv. to xlvii.). The very detailed anatomical account is prefaced by an interesting history of the habits of this little sipunculoid, which is very abundant at Wood's Holl, Mass. Comparisons are instituted between the author's accounts of this species and those by Andreæ on *Sipunculus nudus* and by Shipley on *Phymosoma varians*.—On the relationships of Arthropods, by H. T. Fernald (plates xlviii. to l.). Though in working on the problem of the phylogeny of the Arthropods, much labour has been bestowed on the anatomy of the Crustacea, Arachnids, and on Limulus and Peripatus, next to nothing has been done among the Hexapods and Myriapods. Among the Thysanura, the section containing Campodea, Iapyx, Lepisma, &c., is now well known; but for the anatomy and histology of the Collembola the author knew of only Sommer's article on *Macrotoma plumbea*, he therefore has devoted himself to a patient investigation of *Anurida maritima*, and from this standpoint he considers the existing views of the relationships of Arthropods, which are passed in review and commented upon.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 5.—"Preliminary Notice of a New Form of Excretory Organs in an Oligochætous Annelid." By Frank E. Beddard, M.A., Prosector of the Zoological Society. Communicated by Prof. E. Ray Lankester, M.A., LL.D., F.R.S.

So far as our knowledge of the Oligochæta goes at present, the excretory system appears to consist either of one or more pairs of separate nephridia in each segment, or of a diffuse, irregularly arranged system of tubules with numerous external pores upon each segment, and often with numerous cœlomic funnels in each segment; there may or may not be a connection between the tubes of successive segments. All the aquatic Oligochæta have nephridia of the first kind; a large number of the terrestrial Oligochæta have nephridia of the second kind; there is occasionally in the latter forms a specialization of part of the diffuse nephridial system into a pair of large nephridia; these species connect the two extremes. But in all these worms the nephridia are contained in the cœlom, though some of the connecting branches may be retroperitoneal; the ducts which lead to the exterior may branch in the thickness of the body wall, but there does not seem to be any extensive ramification and anastomosis of the tubes in the muscular layers of the body wall (*Quart. Journ. Micr. Sci.*, vol. xxviii., Pl. xxx., Fig. 1, *n*, and Fig. 2).

I have recently found a remarkably different arrangement of the nephridia in an Annelid belonging to a new genus of Eudrilidæ. This family is chiefly noteworthy on account of the remarkable modifications of the reproductive organs, and the present genus is no exception to the rule in that particular; but it shows a further peculiarity—in the structure of the nephridia; the arrangement of these organs in the clitellar region of the body is unique among Annelids, and is to a certain extent suggestive of the condition of the organs supposed to be nephridia in certain Nematoidea. Throughout the body generally, as in other Eudrilids, the nephridia are paired; in the genital region I was struck, on dissecting the worms, by the apparent absence of nephridia. Sections through the body wall in this region show that the longitudinal and transverse muscular layers are traversed by a system of peculiar canals not at all like nephridia in appearance. These canals are not mere clefts between the muscular fibres, such as Kükenthal has described in his paper "Ueber die Lymphoidzellen der Anneliden" (*Jenaische Zeitschr. f. Naturw.*, vol. xviii., 1885, p. 319); such lymph spaces I have found in a good many Oligochæta, but they never possess a definite wall. On the contrary, the canals which I describe here have a definite darkly-staining wall, with nuclei here and there. They resemble the blood-vessels very closely, and might easily be confounded with them.

These vessels are arranged in a longitudinal and a transverse series with numerous branches and interconnections. The longitudinal muscles are embedded in a nearly homogeneous, transparent, connective tissue, which is of some thickness between the peritoneal epithelium and where the muscular fibres end. It is in the latter tract of tissue that the four principal longitudinal trunks run, corresponding in position to a line connecting the four successive pairs of setæ; there appear to be smaller longitudinal trunks, but the four principal ones run through several segments without a break; these longitudinal trunks are connected with a metamerically repeated system of transverse vessels; these lie between the transverse and longitudinal muscular coats, and appear to run right round the body. They are of considerable calibre, but not so wide as the longitudinal trunks; I could not detect any ciliation anywhere, and their walls are extremely thin. They give off numerous branches, which traverse the body wall in every direction, and form a finer meshwork of tubules; some of the branches run towards the epidermis, and although I could not detect in transverse sections the actual orifices, on account of the fineness of the tubes, I could make out at frequent points a slight modification of the epidermis which seemed to correspond to an external pore.

Upon fragments of the chitinous cuticle being stripped off and examined with a high magnifying power, the orifices were quite plain. They were much smaller than the nephridiopores of *Perichæta*, but not so minute as to be confounded with the pores of the gland cells of the epidermis.

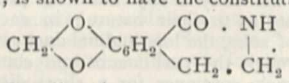
The system of tubes was everywhere accompanied by blood-vessels; but, it is perhaps unnecessary to remark, there was nowhere any connection between these tubes and the capillaries; no coagulated blood was in a single instance found in the excretory tubules.

In spite of their very different appearance, as well as arrangement, from the nephridia of other types, such as *Perichæta*, which possess a diffuse nephridial system, the excretory nature of these tubes seems probable, without any further description. A connection with the body cavity must be proved in order to remove all doubts as to their nature; in each segment, just behind the pair of setæ, the longitudinal duct gives off a branch, which passes through the peritoneum and comes to lie in the cœlom; this branch continues for a short distance, and then abruptly ceases; whether it is furnished with an actual orifice or not I am unable to say. In a few cases, the branch entering the cœlom became connected with a very small coiled nephridial tubule, so small that it was not, as already mentioned, recognizable in dissection.

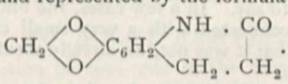
I am inclined to refer the atrophy of the intra-cœlomic part of the nephridia to their having been used up in the formation of the genital ducts. I have recently communicated to this Society a notice of the development of the genital ducts out of nephridia in *Acanthodrilus* ("Roy. Soc. Proc.," vol. xlviii., 1891, p. 452); and that mode of development is possibly general. In any case *the nephridial system of the genital segments of this Eudrilid consists almost entirely of a complex system of tubes, which ramify in the thickness of the body wall, which open by numerous pores on to the exterior, and are connected by a few short tubes with the body cavity.* If the tubes leading to the cœlom became obliterated, and they are very short as it is, the excretory system would consist only of the network in the body walls.

This system of tubes in the skin may perhaps be more comparable to the nephridial network of Cestodes and other flat Worms, than the intracœlomic network of other Oligochæta; its presence, however, in the body walls suggests a comparison with the Nematoidea, which appear to possess at least the remains of a cœlom. In some of these Worms a system of fine tubes connected with the excretory pore permeates the interspaces between the longitudinal muscles. In *Echinorhynchus* the tubes connected with the lemnisci also ramify in the integument, and the lemnisci themselves are processes of the body wall depending into the cœlom.

Chemical Society, February 19.—Dr. W. J. Russell, F.R.S., President, in the chair.—The following papers were read:—The action of reducing agents on αα'-diacetylpentane: synthesis of dimethyldihydroxyheptamethylene, by F. Stanley Kipping and W. H. Perkin, Jun., F.R.S. On reducing αα'-diacetylpentane dissolved in moist ether with sodium, it is converted into a colourless liquid of the composition $C_9H_{18}O_2$. This compound is formed by the addition of two atoms of hydrogen to diacetylpentane, and from its behaviour the authors conclude that it is *dimethyldihydroxyheptamethylene*. The authors also describe the properties and some derivatives of this reduction product.—The osmotic pressures of salts in solution, by R. H. Adie. The osmotic pressures were determined by direct observation by the method of Pfeffer. The results are arranged under the following five headings:—(1) *Boyle's law, applied to solutions*. The results show that the osmotic pressures do not vary directly as the concentration, and when represented graphically give a definite curved form, approaching Ostwald's line of no dissociation at two points, and receding from it elsewhere. Potash alum gives the nearest approach to a straight line of the salts examined. (2) *Charles's law, applied to solutions.* Only one experiment yielded any result, and that was confirmatory of the correctness of applying Charles's law to solutions. (3) *Influence of bases on osmotic pressures.* No definite influence can be attributed to the base, except that the salts of some bases, *e.g.* soda, appear to be more readily dissociated by water than those of others, *e.g.* potash. (4) *Influence of acids on osmotic pressures.* In the case of inorganic acids no definite influence can be detected. In the case of organic acids, an increase in molecular weight of homologous acids increases the osmotic pressure, and a ring nucleus gives a higher osmotic pressure than an open chain. (5) *Avogadro's law, applied to salts in solution.* The author suggests that the differences between the observed results and those calculated on the assumption that Avogadro's law applies to salts in solution, may be due to the existence of simple and complex molecules, as well as to the formation of water compounds.—A direct comparison of the physical constants involved in the determination of molecular weights by Raoult's method, by R. H. Adie. The author shows that the constants of Raoult's method of determining molecular weights, and those ascertained by direct observation of the osmotic pressure do not agree. An attempt is also made to connect, by the same method of Raoult, the osmotic pressure with the gaseous pressure of any substance in solution.—Derivatives of piperonyl, by Frederick M. Perkin. The compound $C_{10}H_9NO_3$, obtained from berberine by the author's brother, is shown to have the constitution



The author also describes the preparation from piperonal of the isomeric compound represented by the formula



—Studies on the constitution of tri-derivatives of naphthalene; No. 9, Andresen's β-naphthylaminedisulphonic acid, by Henry E. Armstrong and W. P. Wynne. The authors have examined the acids obtained by reduction of the products of nitration of naphthalene-1 : 3′-disulphonic acid. The chief product on reduction gives the so-called α-naphthylamine-ε-disulphonic acid [$NH_2 : SO_3H : SO_3H$ = 1′ : 1 : 3′]. Of the other products, one on reduction yields a β-naphthylaminedisulphonic acid [$NH_2 : SO_3H : SO_3H$ = 2 : 4 : 2′] (Andresen's acid). This is the first instance in which the formation of a β-nitro-acid by the nitration of a naphthalenesulphonic acid has been observed.

Physical Society, February 27.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—The following communications were read:—Proof of the generality of certain formulæ published for a special case by Mr. Blakesley; tests of a transformer, by Prof. W. E. Ayrton, F.R.S., and Mr. J. F. Taylor. In 1888 Mr. Blakesley published a number of formulæ relating to the measurement of power, &c., in alternating current circuits by means of electro-dynamometers, one of which had its two coils independent, and placed in different circuits. These formulæ were deduced on certain assumptions, the chief ones being that the currents and magnetizations varied harmonically, and that the magnetic stress in the iron was proportional to the ampere-turns. The present paper shows that the above assumptions are not necessary to the truth of the resulting formulæ. To take the case of a transformer, let alternating current ammeters be placed in the primary and secondary circuits, and a direct-reading "split dynamometer" have a coil in each circuit. Let D_p, D_s, and D_{ps}, be the respective readings of these instruments, then, whatever be the law of variation of the currents,

$$D_p = \sqrt{\frac{1}{T}\int_0^T A_p^2 dt}$$

$$D_s = \sqrt{\frac{1}{T}\int_0^T A_s^2 dt}$$

and

$$D_{ps} = \sqrt{\frac{1}{T}\int_0^T A_p A_s dt},$$

where A_p and A_s are the values of the primary and secondary currents at any instant, and T the time of one complete alternation. If i be the total induction in the core, P and S the numbers of turns of wire on the primary and secondary respectively, V_p and V_s the terminal pressures, p the resistance of the primary coil, and s the resistance of the whole secondary circuit, then the following equations hold at any instant—

$$V_p = pA_p + P\frac{di}{dt}$$

$$\frac{di}{dt} = \frac{s}{S}A_s.$$

Therefore

$$V_p = pA_p + \frac{P}{S}sA_s.$$

Multiplying both sides by A_p, we get

$$A_pV_p = pA_p^2 + \frac{P}{S}sA_pA_s,$$

and integrating from t = 0 to t = T, and taking the mean,

$$\frac{1}{T}\int_0^T A_pV_p dt = \frac{p}{T}\int_0^T A_p^2 dt + \frac{P}{S}\frac{s}{T}\int_0^T A_pA_s dt;$$

or

$$\text{watts in primary} = pD_p^2 + \frac{P}{S}sD_{ps}$$

quite independent of the laws of variation of A and V. The power lost in heating the iron core was also shown to be

$$s\left(\frac{P}{S}D_{ps} - D_p^2\right).$$

Other formulæ, such as that given by Mr. Blakesley, expressing the primary volts in terms of the dynamometer readings, are shown to be true generally. On the subject of transformer magnetizations, the authors state that it is desirable not to speak merely of the ampere-turns, but also of the self-induction, for they have reason to believe that the magnetizing value of an ampere-turn varies. Defining the self-induction of the secondary by the equation

$$L_s = \frac{dN}{dA_s},$$

N being the total flux through the secondary, they show geometrically (assuming harmonic variations) that if L_s diminishes, the efficiency increases, and at the same time the phase angle θ between the primary and secondary currents, and the magnetic lag φ, decrease. On comparing this theoretical deduction with the results of experiment, they find a good agreement, as will be seen from the following table:—

Frequency.	Efficiency. Per cent.	L_s	θ °	φ ° ′
160	96·3	0·0017	113	0 24
160	92	0·0368	169	18 24
160	85·4	0·1241	174·6	45 36

Similar results were obtained in different sets of experiments. It was also noticed that, as the secondary current is increased, the efficiency rises to a maximum and then diminishes, whilst L_s, θ, and φ diminish and then rise again, the current which gives the maximum efficiency coinciding almost exactly with that which gives minimum values to L_s, θ, and φ. The methods employed in making the tests are described in the paper, and the formulæ used in working out the results are there demonstrated. Amongst the facts deducible from the experiments are the following: (1) with constant frequency, the ratios $\frac{V_s}{V_p}$ and $\frac{D_s}{D_p}$ increase as the primary volts rise; (2) with frequency constant and primary volts constant, $\frac{V_s}{V_p}$ decreases, and $\frac{D_s}{D_p}$ increases as the secondary current increases; (3) with constant secondary current, L_s decreases as the primary volts are increased; (4) with small constant secondary currents, the efficiency diminishes as the frequency increases; (5) for large secondary currents the efficiency is approximately independent of the frequency; (6) shunting the secondary with a condenser increases the efficiency. The transformer in which the above tests were made was one of the Mordey type kindly lent by the Brush Electric Engineering Corporation.—

Further contributions to dynamometry, by Mr. T. H. Blakesley. The object of Mr. Blakesley's paper was, in the first place, to show what sort of physical quantities could be advantageously evaluated by using electro-dynamometers of two coils of low resistance in circuits conveying electric currents. The meaning of a dynamometer reading was explained to be the mean value of the product of two currents, either steady or undergoing any periodic variations with sufficient rapidity. In mathematical language such an instrument measured $\frac{1}{T}\int_0^T C_1C_2dt$, where C_1 and C_2 were the instantaneous values of the currents in the two coils, including, of course, the common case where these currents are identical. Any physical quantity whose value was such a product C_1C_2 multiplied into something which was independent of the time, and which, therefore, on integration, came outside the integrator, was well adapted to have its mean value given by such instruments. Power was such a quantity, being merely $(\text{current})^2 \times$ resistance. The square of an E.M.F. was another such quantity, but he did not wish to restrict the method to such evaluations. It follows that any quantity whose instantaneous value can be expressed by terms each quadratic in current, and whose other factor was independent of time, could have its mean value expressed in dynamometer readings. In addition, the particular place and mode of coupling of the dynamometers was indicated by the instantaneous equation, as well as the factor to be applied to each reading. *Thus the equations are made to indicate the practical arrangement to be adopted, and the use to be made of the observations* in each case. Examples were given for the cases of transformers in series and parallel, and special applications of the method were suggested in the measurement of the power employed in such diverse apparatus as voltameters subject to direct or variable currents of any sort, welding machines, parallel generators, tuning-fork circuits, vacuum discharges, and imperfect condensers. For parallel generators the power of each could be separately estimated, and in the case of electric welders, the power employed in the welding circuit was shown to be measurable without introducing any resistance whatever into that circuit. Mr. J. Swinburne said the author's assumption that there is no back or forward E.M.F. in the primary and secondary circuits of transformers except that due to $\frac{di}{dt}$, where i is the total induction in the core, was unwarrantable, for in all real transformers there was a "drop" due to waste field, and this made the split dynamometer method useless. It makes the full load efficiencies too high, and this, he thought, accounted for the extraordinary results obtained by Prof. Ayrton and Mr. Taylor. If a dynamometer be used at all, it should, he said, be used as a wattmeter, the moving coil of one turn being joined in series with a non-inductive resistance and put as a shunt to the primary. The power absorbed by the instrument itself should be then determined, and the power given out by the secondary measured by the same instrument, if the secondary be not non-inductive. Any errors due to self-induction in a wattmeter are, he said, equally present when it is called a split dynamometer, and in addition to this the wattmeter as a split dynamometer precludes the possibility of measuring power. Mr. Mordey said the results obtained by Prof. Ayrton and Mr. Taylor confirmed experiments he had made himself by an entirely different method, for he found that the losses in the iron decreased considerably as the secondary current increased, and this gave increased efficiency. In his experiments he kept the load constant until the transformer attained a steady temperature, and then substituted a direct current for the alternating one, varying its strength until the same steady temperature was maintained. The power thus supplied is a measure of the loss in the transformer under the working condition. A 6 kilowatt transformer tested by this method gave a loss of 110 watts at no load, and at full load 205. Of this 205, 176 was accounted for by the loss in the copper coils, leaving only 29 watts as the iron losses at full load. Figures which he quoted from Prof. Ayrton and Mr. Taylor's paper showed the same general result.—A note on electrostatic wattmeters, by Mr. J. Swinburne, and a paper on Interference with alternating currents, by Prof. W. E. Ayrton, F.R.S., and Dr. Sumpner, were postponed.

Linnean Society, February 19.—Prof. Stewart, President, in the chair.—Mr. Thomas Christy exhibited a number of food nuts utilized by the natives of Northern Queensland, and gave the native names for them. The species, however, had not been determined, since no flowers nor foliage of the trees producing them had been obtained.—On behalf of Mr. A. R. Hunt, the Secretary exhibited a curiosity in the shape of an orange within an orange, and remarked that, although by no means of common occurrence, a similar abnormality had been described and figured by Dr. Perrier (*Bull. Soc. Linn. Normand.*, ix. tab. 2).—Mr. G. C. Druce gave an account of the Dillenian Herbarium at Oxford, prefacing his remarks with some particulars of Dillenius's life and labours, and of the botanists of his day with whom he was in correspondence.—Prof. Stewart exhibited and described a remarkable hermaphrodite trout, explaining, by means of the blackboard, the normal structure of the genital organs in both sexes of the fish, and pointing out in what respects the specimen in question differed.—A paper was then read by Dr. John Lowe, on some points in the life-history and rate of growth in yew-trees, and some excellent photographs and drawings of celebrated yews were shown in illustration of his remarks.

Zoological Society, March 3.—Prof. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of February 1891.—Mr. Sclater exhibited the typical and unique specimen of Macgregor's Bower-bird (*Cnemophilus macgregorii*) from the Queensland Museum, Brisbane, which had been kindly lent to him by the authorities of that institution.—A report was read, drawn up by Mr. A. Thomson, the Society's head keeper, on the insects bred in the insect-house during the past season.—Mr. O. Thomas gave an account of a collection of small Mammalia made by Mr. F. J. Jackson in Eastern Central Africa during his recent expedition through the territories of the British Imperial East African Company. Fifteen species were represented in the collection, of which three appeared to be new to science. These were named *Nyctinomus lobatus*, *Otomys jacksoni*, and *Rhizomys annectens*.—A communication was read from Miss E. M. Sharpe on the Butterflies collected by Mr. F. J. Jackson during the same expedition. Twelve new species were described in this paper, and a general account of the whole collection was promised on a future occasion.—A communication was read from Dr. R. W. Shufeldt, containing observations on the comparative osteology of the Columbidæ of North America.

Mathematical Society, March 12.—Prof. Greenhill, F.R.S., President, in the chair.—Dr. Hirst, F.R.S., drew the attention of the members present to the loss the mathematical world had sustained by the recent death of Madame Sophie Kovalevsky (see NATURE, February 19, p. 375), and gave many personal reminiscences. The President also touched upon some of her mathematical work.—The following papers were communicated:—On cusp-loci, which are enveloped by the tangents at the cusps, by Prof. M. J. M. Hill.—On the partitions of a polygon, by Prof. Cayley, F.R.S.—Some theorems concerning groups of totitives of n, by Prof. Lloyd Tanner.—Mr. Love and Prof. Hill spoke at some length on four theorems, connected with the motion of a liquid ellipsoid under its own attraction, more particularly concerning the surfaces which always contain the same particles.—Dr. Larmor made an impromptu communication on the collision of two spherical bodies with reference to Newton's experiments.

EDINBURGH.

Royal Society, February 16.—A. Forbes Irvine, Vice-President, in the chair.—Prof. Tait read a further note on the virial. In a former communication he deduced from the expression for the virial a general equation connecting the pressure, volume, and temperature of a substance, and also gave numerical values of the constants in the equation which enabled it to roughly represent the isothermals of carbonic acid. In the present note he gives values of the constants which enable the equation to represent these isothermals with great accuracy in the neighbourhood of the critical point.—Dr. Berry Haycraft discussed the adverse criticism of Salkowski and Jolin regarding his process for the estimation of uric acid. He also reviewed the favourable notices of Hermann, Czapek, and Camerer. He showed that the results obtained by the methods of Salkowski and Jolin cannot be accepted in disproof of the utility of his method.—Dr. Haycraft also described a method for the determination of the density of a liquid, when only small quantities of it can be obtained. A drop of the liquid is placed in another liquid of greater density than it, and a liquid of less density is added until the drop will neither float to the surface nor fall to the bottom of the mixture.—Prof. Tait communicated a paper, by Prof. Cargill G. Knott, on the interaction of longitudinal and circular magnetizations in iron and nickel wires. In a former paper on this subject, Prof. Knott described effects which were observed when a constant current of electricity was made to flow along a wire which was subjected to a cyclical variation of longitudinal magnetization. He has since found that some of the results were due to a slight amount of twist which had been given to the wire previously to its magnetization. A twist of not more than a few minutes of arc per centimetre of length causes a profound modification in the magnitude of the average polarity which is developed in the wire by the cyclical process when the constant current is maintained. The effect of the current is to reduce hysteresis.—Prof. Tait read a paper, by Mr. Robert Brodie, on the value of the method of demonstration by superposition.—Dr. Hugh Marshall described the method of formation of, and exhibited a specimen of, potassium persulphate. The unexpected discovery of the stability of a salt of persulphuric acid is of great theoretical importance.—Dr. John Murray communicated a paper on the temperature of the Clyde sea-area. Among other points, he described the action of an in-shore breeze in summer in accumulating the warm surface-water on the lee shore, and the action of an off-shore breeze in blowing away the warm surface-water from the coast, and thus causing the cold water to rise to the surface. This action is reversed in winter. In one case a variation of temperature of a number of degrees was observed within two days from a reversal in the direction of the wind.

Paris.

Academy of Sciences, March 9.—M. Duchartre in the chair.—On some experiments made in 1890 at the Aubois sluice, by M. Anatole de Caligny.—Observation of the asteroid (308) discovered at Nice Observatory on March 5, by M. Charlois. The asteroid is of the 13th magnitude. Its position was R.A. 10h. 1m. 26s., N.P.D. 70° 17′ 50″, at 8h. 46m. 45s. mean time at Nice on March 5.—Observations of the asteroid (307), made at Toulouse Observatory by MM. Baillaud and Cosserat, and of (308), made by M. Andoyer with the great equatorial. Observations for position were made on March 3, 4, 5, 6, and 7.—Observations of (307) made at Paris Observatory with the East Tower equatorial, by Mdlle. Klumpke. Observations for position were made on March 3, 5, and 6.—On the measure of the 52nd parallel in Europe, by M. Vénukoff. In presenting to the Academy "Mémoires de la Section Topographique de l'État-Major Général Russe," vols. xlvi. and xlvii. (St. Petersburg, 1891), M. Vénukoff remarked upon the results obtained from the Russian triangulation, and those obtained in England by Clark. The Russian stations give 68·6412 kilometres as the mean length of a degree of longitude in latitude 52°; the English measures give the value 68·6880. A similar variation is seen if only Russian stations are considered. It appears, therefore, that the terrestrial surface under the 52nd degree of latitude is not that of an ellipsoid of revolution. This conclusion depends, of course, upon the accuracy of the results cited. Measures of the 42nd parallel in the United States lead, however, to a similar conclusion. M. Vénukoff therefore adds that the earth is not a perfect sphere.—On equations of two minimum periodic surfaces possessing the symmetry of an octahedron, by M. A. Schœnflies.—On harmonic spirals, by M. L. Raffy.—On the compatibility of the laws of dispersion and double refraction, by M. G. Carvallo.—Longitudinal and transverse superposed magnetizations, by M. C. Decharme.—On the hydrated manganites of sodium, by M. G. Rousseau. The author treats of the decomposition of potassium permanganate by heat, and of the formation of manganites by heating sodium manganate with sodium chloride.—On the transformation of sodium pyrophosphite into hydrogen sodium phosphite, by M. L. Amat. The solution of sodium pyrophosphite gradually changes according to the following equation:—

$$Na_2H_2P_2O_5 + H_2O = 2(HPO_3NaH).$$

The law according to which the transformation takes place is given by

$$\frac{1}{x} \log \frac{l-\phi_0}{l-\phi} = k \log e,$$

where k is a constant, x is the duration of the experiment in hours, ϕ is volume in c.c.s of standard soda required for neutralization at end of experiment, ϕ_0 = value of ϕ at beginning of experiment, l = limiting value of ϕ when transformation is complete. The experimental values of the constant $k \log e$ vary from 0·000327 to 0·000369. In the presence of very dilute acid (H_2SO_4), $k \log e$ varies from 0·00149 to 0·00122; with stronger acid, and taking x in minutes, $k \log e$ varies from 0·0125 to 0·0059.—On silicobromoform, by M. A. Besson.—A study of the thermochemical properties of some alkaline derivatives of erythrite, by M. de Forcrand.—On some ammoniacal compounds of cyanide of mercury, by M. Raoul Varet.—On the fermentation of starch by the action of the butyric ferment, by M. A. Villiers. The writer claims to have discovered a new carbohydrate, differing from the sugars, and crystallizing from alcohol as

$$(C_6H_{10}O_5)_6 \cdot C_2H_6O \cdot 5H_2O.$$

—The histological changes of the skin during measles, by M. Catrin.—On the existence of "attractive spheres" in vegetable cells, by M. Léon Guignard. Some time ago M. van Beneden gave the name *sphères attractives* to certain small bodies playing an important part in the segmentation of the cells of animals. M. Guignard now announces that vegetable tissues show attractive spheres similar to those found in animal tissues. He thinks the bodies merit the name of *sphères directrices*, since they govern the division of the nucleus, and move without discontinuity from one cell to another during the whole life of the plant.—On the classification and history of *Clusia*, by M. J. Vesque. The histological characteristics of those Guttiferæ known as *Clusia* are described.—On the chalk of Cotentin, the white Meudon chalk, and the Maestricht tufa, by M. A. de Grossouvre.—Skull of a cave-bear, having traces of a blow inflicted by a flint axe, by M. Wanzel.

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