THURSDAY, APRIL 3, 1890.

TECHNICAL EDUCATION IN THE CODE.

MR. KEKEWICH is to be congratulated on the reception which his Code has hitherto met with. From all sides it has been received with a unanimous chorus of congratulation, tempered only by the difficulty which has been experienced in distinguishing clearly what is new from what is old. Many parts of the Code have in fact been entirely re-cast and re-arranged, and in the absence of the schedule of alterations which it is customary to issue as an appendage to the Code, the compilers of abstracts for the daily papers have this year had a terrible time of it. They have been unable to criticise the alterations without reading the document through, and even this unwonted exercise has not prevented them in more than one case from reproducing as new, old and familiar articles, the order of which has been changed.

But these trials, and the further difficulty of picturing at once the effect on various classes of schools of the action and reaction of numberless modifications, additions, and omissions both small and great, fortunately affect us but little. A great part—some would say the most important part—of the alterations, deal with matters of finance, management, and control, rather than directly with the education given in the schools. And it is with this that we are chiefly concerned in the present article.

So far as regards the changes in curriculum there is no ambiguity. We may fairly congratulate the Government on a solid and unequivocal advance in the right direction. In fact, the framers of the Code have gone a very long way (without the aid of Sir Henry Roscoe's new Bill) to enable elementary school managers to provide technical education, or more strictly to provide the general educational basis on which all specialised technical instruction must be founded.

A few weeks ago, when dealing with the changes in the new Scotch Code, we ventured on two forecasts regarding the coming changes in English elementary schools. The first was that the English Education Office would be unable to maintain its previous *non possumus* attitude on the subject of manual instruction after the Scotch Department had virtually assented to Sir Horace Davey's now famous opinion by including manual training among the grant-earning subjects of the Code. The second was that the policy of the Department would be found to lean (as in Scotland) towards the encouragement and extension of "class subjects," taught throughout the whole school, even at the expense of "specific subjects" which only affect a small minority of picked scholars.

Both these forecasts, as we shall see, have been verified, but this does not by any means exhaust the new provisions by which the range of study, especially of technical and scientific instruction, is extended. We will consider some of the changes in order.

To take first the most striking change, the clause by which manual instruction for the first time is recognized as a part of elementary education will come to many as a surprise. It indicates a change of front on the part of the Department on a matter of interpretation of the Education Acts. Hitherto the authorities at Whitehall have declared that the recognition of manual training without a new Act of Parliament was impossible. They asserted that their hands were tied by statute. That was the position a few months ago. And now no statute has been altered, and manual instruction is in the Code. It may be taught either in or off the school premises, and either by the ordinary teachers of the school or by special instructors, provided "special and appropriate provision approved by the inspector is made for such instruction and the times for giving it are entered on the approved timetable." In a later clause manual instruction is specially recognised as an object to which part of the school funds may be devoted.

Thus the aim of the Bill just drafted by the Technical Association is virtually attained without it. One omission, however, may attract notice. No special grants are provided in aid of manual training. In Scotland, it becomes a "class subject," and is paid for accordingly, but no grant is attached to it in the English Code. We presume, however, that there is nothing to prevent it being paid for as a specific subject under the clauses which provide for grants in aid of any subject "if sanctioned by the Department," provided that "a graduated scheme for teaching it be submitted to, and approved by the inspector."[1]

There is, however, yet another way in which grants for manual instruction may be made, and, reading between the lines of the Code, it looks not unlikely that the Government mean to adopt it. Drawing is already paid for by the Science and Art Department, and in Art. 85 (*b*) of the new Code we find drawing and manual training coupled together. Boys in a school for older scholars must be taught drawing "with or without other manual training." Unless, then, the present confusion of overlapping authorities is to be made worse confounded, it is reasonable to expect that both these subjects will be under the same Department, and we shall look with interest for the inclusion of manual instruction in the next Science and Art Directory. There is this further inducement to the Government to take this course, that payments made by the Science and Art Department fall outside the 17*s.* 6*d.* limit. In any case, two main conditions should be fulfilled in making grants for manual instruction: first, that they should not be given on results of examination; secondly, that they should be dependent on a really effective inspection. The first condition is necessary because no satisfactory scheme of individual examination in such a subject can be devised so as to be a real test of efficiency; the second is necessary to guard the public purse from being depleted to enable small children to construct bad soap-boxes when they ought to be in school.

But if the official recognition of manual instruction (which we assume includes, as in the Technical Instruction Act, "modelling in wood, clay, and other material"), is the most striking victory of the advocates of technical instruction, there are other changes of greater importance from an educational point of view.

The Department has at last screwed itself up to the

[1] Arts. 16 and 101 (*f*).

point of refusing to acknowledge any boys' school as efficient which does not include drawing in its curriculum. This is an enormous advance—how great will be seen if we remember that less than a million out of the five million scholars of our elementary schools are receiving instruction in drawin; at the present time. It is a great advance, also, on the halting proposal of last year, when the requirement was restricted to large schools which aimed at the maximum grant. When a radical change, such as the present one, is proposed, it is only reasonable that the transition stage should be made easy for schools which have to adapt themselves to the new requirements. We make no complaint, therefore, of the year of grace granted before the regulation comes into force, nor even of the power given to the inspector to dispense with it altogether in cases where the "means of teaching drawing cannot be procured." This provision would, indeed, seriously cripple the usefulness of the change if it were intended to be permanent. But clearly it is only meant to obviate temporary hardships in small schools; and we may congratulate ourselves that within a short space of time, every boy (or at least every boy among the working classes) will be receiving instruction in what is stated by all authorities to be the indispensable basis of almost all technical instruction. As a corollary to the change, there is another of less importance, but of value in its way, which makes drawing an alternative to needlework for boys in infant schools.

While thus the manual instruction of boys is provided for, a useful extension is given to the curriculum for girls, by the provision of a grant for laundry work calculated on much the same principle as that for cookery.

Passing to science teaching, the reforms introduced are no less satisfactory. In the first place, science instruction (as well as manual training) is placed on the same footing as cookery as regards facilities for the grouping of schools for central instruction, and attendance at such centres will count as attendance at school.

A still more important change is the extension of the range of class subjects. Under former Codes a single course of elementary science was sketched out meagrely enough in Schedule II., while managers were invited if they pleased to submit alternative courses to the inspector. The result might have been expected. Science teaching gives in any case more trouble than geography, and the additional necessity of framing their own courses of instruction was quite enough to deter managers from taking up the subject. Now, however, while still giving permission to managers to draw up other courses of instruction, the Department gives a lead by suggesting as examples no fewer than eight different courses in various branches of science, which are embodied in a supplement to Schedule II. The subjects thus treated are mechanics, physiology, botany, agriculture, chemistry, sound, light, and heat, electricity and magnetism, and domestic economy; while the model course still retained in the main schedule embodies a scheme of elementary instruction in "nature knowledge" of a more mixed and varied character.

In each of the first two standards the instruction is to consist of thirty object-lessons in common things, designed to lead on to the more specialised instruction in the third and higher standards, the courses for which follow (perhaps somewhat too closely) the syllabus laid down for the corresponding subjects in the schedule of specific subjects. It has, of course, been necessary somewhat to simplify and curtail the schemes of instruction in adapting courses framed for picked pupils to suit the capacity of the whole school. It seems to us that the process of simplification might in some cases be carried still further with advantage. Elementary physics for children should consist of a general view of the properties of matter and the forces which act upon it, rather than a more detailed study of one out of many branches of the subject. This was the line taken up by Michael Faraday in his inimitable lectures to children on the "Physical Forces." This too is the view of the Scotch Department, which has laid down a course of class instruction in "Matter," designed to give general preliminary notions of the whole range of physics. And, we may add, this also is the view taken by the Science and Art Department in framing the alternative course in physics for those who (like the vast majority of elementary school children) are not likely to carry their study of physics to a higher stage.

This, however, is a matter of detail, while the suggestion of alternative courses in science, linked to the instruction of the Kindergarten by graduated object-lessons in the first two standards, is a reform which we cannot praise too highly.

Other changes to be noticed are the inclusion among class subjects of history, and the disappearance of the requirement that English grammar should be compulsory as a class subject.

Turning to the schedule of specific subjects, we find less alteration. Mensuration is separated from Euclid and the alternative course of mechanics disappears. There are a few slight changes in the syllabus of the various subjects. Thus the law of conservation of energy drops out of the course on mechanics, presumably because the idea is thought too hard for young children to grasp. But if it be too difficult for *picked* scholars in the fifth and higher standards, how comes it that in the new Scotch Code this very law appears in the syllabus for the "*class*" subject of "matter" (which we have alluded to above), as part of the course suitable for the whole of Standard IV.? Are Scotch children so very far in advance of English as this difference would seem to imply?

If, however, the fourth schedule presents few changes worthy of note, considerable additions are made to the list of specific subjects for which no special syllabus is suggested, such as book-keeping, shorthand, German, and (in Wales) Welsh. In this way the demand for commercial instruction is met, though how far advantage will be taken of the permission to present scholars in these new subjects remains to be seen. And lastly, payments will be made on account of any other specific subject which the Department may sanction, provided a graduated scheme of instruction be submitted to the inspector.

We have now completed the survey of the purely educational changes of the Code. Henceforth (assuming, as we do, that the provisions of the Code will come into force much in their present form) there can be little complaint on the part of advocates of scientific or technical instruction that its introduction into elementary schools is hindered by the action of the Department. There need be no longer any talk of an educational ladder with its lower rungs wanting. How far managers will take advantage of their powers remain to be seen. The changes which are compulsory, such as that which makes drawing universal for boys' schools, will, of course, take effect widely at once. Those which are merely permissive may be slow in their operation. Meanwhile, those who are in earnest about the introduction of such subjects as manual training into elementary schools could not better occupy the time which intervenes before the new Code comes into force, at the end of August next, than in perfecting a graduated scheme of instruction such as may be confidently recommended to school managers to submit to the Education Department.

We have laid stress in this article on the proposed changes in the elementary school curriculum, because, important as these are, they are likely to be overshadowed in the coming discussions on the Code by other questions which appeal more directly to party politicians. We have thus left ourselves no room to do more than allude to other reforms which will affect as powerfully the educational character of our schools as the widening of the course of study. After all, the main guarantee of efficiency is the quality of the teaching staff. The new Code raises the requirements of the Department as to minimum staff, improves the regulations regarding the examination and training of pupil teachers, and provides for the creation (on a very limited scale it is true) of day Training Colleges attached to the Universities or Higher Local Colleges, as well as for the attendance of day students at the existing Training Colleges. The Code further revises the system under which the Parliamentary grant is paid, and almost entirely abolishes payment on results of individual examination. It gives freedom to teachers to classify their scholars as they please, so that a child may be in three different standards in the three R's, and in two different standards again in the two class subjects. All these and other changes, which demand much more notice than we can give them, make the Minute of the Department which has just seen the light emphatically a "Teachers' Code."

THE CAVE FAUNA OF NORTH AMERICA.

The Cave Fauna of North America, with Remarks on the Anatomy of the Brain and Origin of the Blind Species. By A. S. Packard. Pp. 1-156, with 27 Plates.

THIS important memoir is the first of vol. iv. of the "Memoirs of the National Academy of Sciences," and contains the results of an examination of the Mammoth Caves in Kentucky made during the months of April and May 1874, and of some other caves in Indiana and Virginia which were visited by the author at a later date.

A description of eighteen caves, with notes on their hydrography and geological age, and an account of the fauna of those which are better known, form the first section of the memoir. The caves form the natural drains of the country, all the surface drainage being at once carried down into them through the innumerable "sink-holes" which pierce the thin stratum overlying the Carboniferous Limestone, in which the caves are excavated. The Mammoth Cave is the largest and best known, with its 150 miles of passages and avenues, frequently crossing one another at different levels.

Their geological age is uncertain, but there is very little doubt but that they assumed their present proportions long after the melting of the glacial ice and are coæval with the Niagara river-gorge. And as the caves must have been incapable of supporting life while flooded, their preglacial fauna, if they had one, must have been killed off, and they could not have become ready for their present fauna until comparatively recent times; therefore, they must have been colonized by members of the existing fauna. The mode of colonization is very simple. Tracks of bears, wolves, and smaller animals occur in nearly all those caves which are easily accessible from without, and clinging to the skins of these animals various small Arthropods may have been carried in; other species of insects and Myriopods which naturally lead a subterranean life may voluntarily enter the fissures and sink-holes which abound in this region; others, again, get carried in by the agency of torrents which flow in during certain seasons of the year, as, for instance, the eyed fishes and species of Crustacea which abound in the surface waters.

That cave animals have entered the caves from without is further corroborated by the fact that in the case of very many cave species closely allied outdoor species are found in great numbers in the immediate vicinity of the caves. Also caves situated near one another are populated by a similar fauna, which allows us to classify them in groups closely corresponding to the various zoo-geographical regions of the country.

The author then proceeds to the systematic detailed description of the fauna, a section which constitutes more than one-third of the memoir. As in the case of the fauna of the outside world, the species of Arthropoda form a very large percentage of the total number of cave species; but, however different the groups to which the various species belong may be, they possess the common characteristics of slenderness of body and appendages and of the absence of functional eyes. The systematic description is followed by lists of all the North American and European cave species known at present, showing that the European species are by far the most numerous. It is therefore argued that the European caves have been inhabited for a longer period than the American.

Although the animal kingdom, at any rate as far as certain groups are concerned, is comparatively well represented, vegetable life is almost absent, evidently owing to the dryness and the absence of light; in fact, so far as is known at present, it is only represented by a few Fungi and two or three Moulds. The air must also be comparatively free from the germs of bacteria of putrefaction, as the decay of organic refuse is very slow, and meat hung up in the cave will keep a long time. But though bacteria are absent, their office is performed by larvæ of the blind beetle (*Adelops hirtus*) and of flies.

Cave animals are mostly carnivorous. The blind fish (*Amblyopsis*) lives on Crustacea, and especially on the blind crayfish, which in its turn preys upon living *Cæcidotea*, but how they and other small aquatic Crustaceans maintain an existence is unknown. The Myriopods, which are very common, feed on decayed wood and fungous growths.

However, in all cases, as a rule, food must be very scanty, and "lack of food as well as the absence of light was one of the factors concerned in the diminution of size and in the slenderness of blind cave animals as compared with their lucicolous allies."

The effect of total darkness upon animals is twofold. Firstly, colour is either entirely or partially bleached, and, secondly, the sense of sight is lost. Eyesight may be lost in various ways. Either the optic lobes and nerves may atrophy, while the retina, pigment, and lens remain more or less persistent; or the optic lobes and nerves may persist, while the retina and eye-facets atrophy; or, again, the whole of the optic apparatus may atrophy. Examples of all these cases are given in the important chapter which is devoted to a description of the anatomy of the brain and eyes of certain blind Arthropods, and illustrated by numerous drawings of sections through various regions of the head.

It is argued that this atrophy must be comparatively sudden and wholesale, because no series of individuals has been found with the optic lobes or nerves in different stages of disappearance. Transitional forms have been observed with eyes with a varying number of crystalline lenses, as in the case of *Chthonius*; those individuals which live near the mouth of the cave have better developed eyes than those which live far in. And surely, on further examination, more transitional forms will be discovered, as animals must be continually getting into the caves from the outside; their descendants becoming gradually adapted for cave life, until they finally reach the degree of modification of the present older occupants.

As the sense of sight diminishes, it is compensated by an increase of the delicacy of other senses. The tactile and olfactory senses are rendered more sensitive, the appendages become much more slender, and the blind form is altogether more timid and cautious than its eyed allies, as has been particularly noticed in the blind crayfish.

The last part of this memoir deals with what is of most general interest to the biologist, viz. the bearing of these facts upon the theories of evolution. The author states that here the term "natural selection" expresses the result of a series of causes rather than any one cause in itself. The most important of these causes are: the *change of environment*, from light to partial or total darkness, involving diminution of food, the disuse and loss of certain organs, with compensation as has been mentioned above; *adaptation*, enabling the more plastic forms to survive and perpetuate the stock; *heredity*, which operates to secure the future permanence of the newly originated forms—the longer it acts, the earlier will the inherited characters appear in the development of the animal; and, lastly, *isolation*, which, after adaptation and heredity have established the typical characters, prevents intercrossing with out-door forms, and thus insures the permanence of these characters.

The author adduces facts which seem to prove that the organic adaptations to a life in darkness may have been induced after but a few generations, perhaps one or two only, resulting in the comparatively rapid evolution of cave species. If that be the case, then, there is no reason why they should not be produced artificially, but at present no experiments have been made to prove the mutual convertibility of cave species and their lucicolous allies. If a cave species could be made to revert to an epigean form by keeping it for a number of generations in a gradually increasing amount of light; and if, on the other hand, a lucicolous species could be changed into a cave form by a converse process, the theory of occasional rapid evolution due to sudden changes in the environment would receive its final proof.

Mr. Packard draws attention to the interesting parallel between the life of the abysses of oceans and lakes and that of caves. In both cases vegetable life is almost absent, and a large proportion of the animal forms have become similarly modified with regard to the degeneration of the optic organs and corresponding development of other organs as compensation. But while caves have only been populated comparatively recently, the ocean abysses have had inhabitants for a very much longer time, and consequently these have had time to become much more highly specialized than the inhabitants of caves.

This most valuable contribution terminates with a bibliography containing the titles of previous publications on the subject, and we must not omit to mention that in a separate chapter a list is given of the known non-cavernicolous blind animals. As far as the higher classes are concerned, this list contains about the same number of species as the one of the blind cave-dwelling forms.

R. T. G.

LINEAR DIFFERENTIAL EQUATIONS.

A Treatise on Linear Differential Equations. By Thomas Craig, Ph.D. Vol. I. Equations with Uniform Coefficients. (New York: John Wiley and Sons, 1889.)

TREATISES on this subject have been somewhat numerous of late. We recently noticed in these columns an excellent, but fairly elementary work, "On Ordinary and Partial Differential Equations," by Prof. Woolsey Johnson. The student who wishes to enter on the profitable perusal of the book before us must be well versed in all the ordinary modes of procedure,[1] and then he will find that Dr. Craig is well qualified to lead him through the intricate windings of this difficult branch of mathematics. The advanced student will find the author's analyses of use to him whilst reading the various original memoirs here introduced to him, for the first time, in English. Some may remember that Mr. Forsyth, in his classical treatise, omitted the investigations of Fuchs, the recent researches of Hermite and Halphen contented himself with a slight sketch of Jacobi's method for partial differential equations, and did not at all touch upon the methods of Cauchy, Lie, and Mayer. The consideration of these matters he reserved for a future volume.

The theory of the subject before us, *i.e.* of linear differential equations, almost owes its origin, in Dr. Craig's opinion, to two memoirs by Fuchs, published in vols. lxvi. and lxviii. of *Crelle's Journal* (1866, 1868):—

"Previous to this the only class of linear differential equations for which a general method of integration was known, was the class of equations with constant coefficients, including, of course, Legendre's well-known equation, which is immediately transformable into one with

[1] "The reader is of course supposed to be familiar with the ordinary elementary theory of differential equations" (p. 32).

constant coefficients. After the appearance of Fuchs's second memoir, many mathematicians, particularly in France and Germany, including Fuchs himself, took up the subject, which, though still in its infancy, now possesses a very large literature."

As happens in such cases, these memoirs have to be dug out of journals and publications of learned Societies before the student can be put in possession of results obtained. It is for this labour of research, and then for the arrangement in due sequence of theorems, that the reader has to thank Dr. Craig.[1] Even in the first two chapters, where most of the results are old, the treatment is comparatively new, being founded upon papers by Laguerre (*Comptes rendus*, 1879), and upon memoirs, or works, by Briot and Bouquet and Jordan; reference is also made, in connection with a proof by Jordan, to a paper by Picard (*Bulletin des Sciences Math.*, 1888). Here we may note that the author reserves an account of the investigations of Laguerre, Halphen, and others, from a still higher point of view, to a subsequent volume.

This first instalment discusses principally Fuchs's type of equations, but accounts are given of the researches of Frobenius (chapters iv., viii.), Markoff, Heun, Riemann, and Humbert (chapter vi.), Thomé (chapter ix.), Halphen (chapter xii.), Forsyth's canonical form and associate equations, Brioschi, Lagrange's adjoint equation, Halphen's adjoint quantics and Appell's theorem (chapter xiii.), and Picard (chapter xiv.). An account, due to Jordan, is given of the application of the theory of substitutions to linear differential equations (chapter iii.). Many points are touched lightly here, a fuller development being held in reserve. A prominent feature is the reproduction (chapter vii) of a thesis by M. E. Goursat on equations of the second order satisfied by the hypergeometric series. This consists of two parts. The first part gives an application of Cauchy's theorem, and relations between Kummer's (24) integrals, an application to the complete elliptic integral of the first kind, and Schwarz's results. The second part discusses the transformations of the hypergeometric series, Tannery's theorem, and some other points, the article closing with a collection of 137 transformations due (apparently) to Kummer.

The pages bristle with references to original sources, so that, as we have already indicated, this treatise is an invaluable handy-book to what has been done in this field.

One more word: there is no collection of examples for solution on the Cambridge model, but the work is strictly on the lines of a French or German treatise.

The book itself is very elegantly turned out.

[1] For instance, he obtains certain forms in the same way that Fuchs obtained them, "if for no other reason than that of the desirability of developing the subject in historical order" (p. 64).

THE BACTERIA OF ASIATIC CHOLERA.

The Bacteria of Asiatic Cholera. By E. Klein, M.D (London: Macmillan and Co., 1889.)

SO masterly and complete was the account which Koch gave in 1884 of the comma-bacillus, which he held to be the virus of cholera, that but little, if anything, has been added to our knowledge of its mode of growth, of its reaction to dyes, or of its life-history. As might be expected, the assiduity of many observers, now it has been directed to the subject, has led to the discovery of many other bacilli, which may be described as comma-shaped. But, so far, no bacteriologist, who has had his observations corroborated by other observers, has proved that any of them are indistinguishable in all their physical characters, whether in appearance, in reaction to dyes, or in their mode of growth, &c., from the choleraic bacillus. So far as is known, animals are not susceptible to cholera. If Asiatic cholera could be induced by inoculating with pure cultivations of choleraic comma-bacilli, then beyond a doubt they would be the *vera causa*, or, in other words, the contagium of cholera; but this step in Koch's argument was wanting, probably for the above-named reason, and is likely to remain so; the experimental inoculations of guinea-pigs which have taken place being by no means conclusive.

The present volume is a valuable and most trenchant criticism of every step of Koch's argument, and may be said to contain everything that can at present be said against Koch's theory, of which the author is the most active opponent.

The author commences with an account of the various comma-shaped bacilli which are at present known, and there are well-recognized characteristics which distinguish them from the first form, in all of them, except in those which depend upon solitary observations.

The following is the list of comma-shaped bacilli with the names of their discoverers:—

(1) Koch, in Asiatic cholera; $\frac{1}{2}$ to $\frac{2}{3}$ the length of tubercle bacilli, but thicker and curved. (2) Finkler and Prior, in cholera nostras; but Koch and Frank failed to demonstrate these in typical cases. They are thicker and longer than (1). In 10 per cent. gelatine, the growth is broad and conical, liquefying the gelatine more rapidly. (3) Lewis, in the fluid of the mouth, thicker than (1) Klein only twice has succeeded in growing them; every one else has failed. (4) Miller, in some cases of caries of the teeth, similar to (2). (5) Kuisl, in human fæces similar to (2). (6) Deneke, in stale cheeses. The growth on gelatine is similar, but they will not grow on potatoes. (7) Klein, in some cases of diarrhœa, especially in monkeys. They grow differetly in gelatine, and cause it to smell offensively. (8) Ermengen and others, in the intestines of guinea-pigs, pigs, rabbits, horses, &c., but they will not grow in 10 per cent. gelatine. (9) Lingard, two kinds in a case of noma, the smaller of which is said to have been very similar to the choleraic one. (10) Weibel, various forms in mucus, but their mode of growth is distinct. (11) Gamaleïa, in a fatal fowl disease, which was prevalent at Odessa. He did not distinguish them from (1). (12) Klein, in the intestines of a monkey with diarrhœa. The organisms were smaller, but the growth was similar to (1).

Klein lays great stress upon the difficulty there is in demonstrating the presence of the bacilli in the walls of the intestine in cases of cholera, and thinks that they are not present in the parts which are still alive, but only where the tissue has died; moreover they are absent from the blood.

The bacilli are most readily found in the mucous flakes; and in the presence of fæcal matter they are readily destroyed, which may explain why they are sometimes not easily detected.

The author has done good service in threshing out all the evidence afresh, but the matter remains very much where Koch left it. The detection of the bacilli may enable us more readily to diagnose the earliest cases in an epidemic of cholera; and, as one result of his experiments, we may expect soiled linen to be most efficiently sterilized by drying it; at the same time, until the disease has been reproduced by inoculation with the organism, it cannot be said to be conclusively proved that this is the true virus.

OUR BOOK SHELF.

Manuel de l'Analyse des Vins. Par Ernest Barillot. Pp. xii.-131. (Paris: Gauthier-Villars et Fils, 1889.)

THE student of practical chemistry will find in this book a handy guide to the examination of wines. Works on the same subject are frequently rendered both unwieldy and tiresome by a multiplicity of analytical methods and the introduction of a bulky collection of tables embodying the composition of various classes of wine, a knowledge of which is deemed necessary in forming an opinion of the quality or purity of a particular sample. Here, however, details of this kind are reduced to a minimum. One or two methods, only, of carrying out any estimation are given, and free use is made of such empirical relations between the proportions of the constituents of a wine as seem warranted by the results of previous analyses.

The book consists of two parts and an appendix. Part I. is concerned with the determination of the normal constituents of wines, alcohol, total solids, ash, grape sugar, &c. Part II. deals with adulterations. In its opening sections are placed the indications traceable to the presence of added water, added alcohol, cane sugar dextrine, &c., but the greater bulk of the part is devoted to the detection of foreign colouring matters. The subject of colour reactions is very fully treated, and by the arrangement of the experiments in tabular form their nature and interpretation can be readily appreciated. It seems a pity that in connection with these tests no notice is taken in the text of the absorption spectrum of the colouring agents, as a clue to their identification; in a footnote the author contents himself by merely referring the reader to the works of Vogel and Wurtz for information on this subject. In the appendix is a statement of the chemical constitution of the colouring matters mentioned, followed by an account of some recent work of the author on the detection of added alcohol. His method is based on the effect of the alcohol introduced on the proportion of volatile acid which distils from the wine, and the result is shown to be consistent with the theory of the rate of etherification of organic acids.

The book is intended to be useful for commercial purposes, and for such the analytical processes described are sufficiently accurate. The apparatus employed, as is stated in a footnote, has been constructed by the Société Centrale de Produits Chimiques, and judging from the illustrations, is in some cases, to English eyes at least, a trifle antiquated. The occasional reference to vessels provided with marks, and to which no numerical values are attached, detracts somewhat from the general usefulness of the book, and is unintelligible to a reader who has failed to notice the explanatory footnote.

The graduation of alcoholometers, the maximum amount of alcohol permissible in wines, &c., are of course in accordance with the regulations of the French Excise.

Synoptical Tables of Organic and Inorganic Chemistry Compiled by Clement J. Leaper, F.C.S. (London: George Gill and Sons, 1890.)

THE compiler says in his preface that "the mass of facts presented to the mind of the beginner in chemistry is so large that he often experiences a difficulty in distinguishing the useful from the ornamental, and is apt, consequently, to neglect fundamental principles and reactions for comparatively useless minutiæ. These tables are intended to prevent this error. . . . The experience of many years has convinced the author that the student who honestly commits these tables to memory will lay for himself a solid groundwork for future reading and research." Whatever may be meant therefore by the expression "future reading and research," it appears that the compiler aims no higher than to give a series of unconnected statements which if learned will enable the would-be student to begin his study of chemistry. We do not think this committing to memory will make the study more easy, and should fear that the learner might imagine after his memory exercise that he thereby knew something of chemistry. The separation of "the useful from the ornamental" is always difficult, and it is rare to find two authorities at one in such a matter. It is doubtful, for example, whether any chemist will agree with the compiler when he states as Charles's law that "All gases expand or contract $\frac{1}{273}$ of their volume for each rise or fall of 1° C.," and omits, presumably as ornamental, the limitation of this proportion to the volume of the gas at 0° C.

The British Journal Photographic Almanac, 1890. Edited by J. Traill Taylor. (London: Henry Greenwood and Co., 1890.)

IN this year's volume we find a most interesting collection of notes and articles relating to almost every branch of the subject. Captain Abney contributes an article in which he warns photographers to beware of their principal enemy—dust—and concludes with the best method of exclusion. The Rev. S. J. Perry gives a short summary of the instruments used in celestial photography during the past year, and of the work accomplished, including the wonderful photographs taken by Isaac Roberts of the nebula of Andromeda, nebulæ in the Pleiades, &c. Mention is also made of the success of Mr. Common in rendering still more perfect the reflecting surface of his magnificent five-foot glass mirror. Amongst the other articles we may refer to that on halation by Chapman Jones, hydroquinone by W. B. Bolton, and celluloid films by Colonel J. Waterhouse. An epitome of the year's progress, with notes on passing events, original and selected, is given by the editor, who marks the great advance made in film photography, and also the tendency to diminish the bulk of cameras, as shown by the innumerable hand or detective cameras that have appeared during the last twelve months. Allusion also is made to the new developer, eikonogen, which can, it is believed, develop into full printing density a plate that has been impressed by feeble radiations.

No alteration has been made as regards the general order of the work; there are only slight additions to the tables, formulæ, &c. The specimens of processes which illustrate the volume, especially that of Mrs. Sterling from a negative by Vander Weyde, are very fine.

Four-Figure Mathematical Tables. By J. T. Bottomley, M.A., F.R.S., &c. Second Edition. (London: Macmillan and Co., 1890.)

THIS useful collection of tables has been considerably enlarged and revised since its first appearance. It comprises logarithmic and trigonometrical tables, tables of squares, square roots, and reciprocals, and a collection of useful formulæ and constants. The introduction is sufficiently detailed to make the construction of the table readily understood, assuming a knowledge of the use of logarithms. The book will prove a handy substitute for more bulky volumes in cases where extreme accuracy is not required, such as computations in chemistry and physics.

LETTERS TO THE EDITOR.

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

Panmixia.

MY letter of March 6 commenced with the remark that, without entering into controversy, I proposed to draw attention to the opinions expressed concerning the inheritance of acquired characters by Mr. Darwin. The reasons for my own beliefs on the questions at issue I have given in "The Principles of Biology," § 166, and, with other illustrations, in "The Factors of Organic Evolution." Here it must suffice to say that I have seen no reason to abandon the conclusions there set forth.

Respecting the doctrine of "panmixia," either as enunciated by Prof. Weismann, or as recently presented in modified forms, I will say no more than that I should like to see its adequacy discussed in connection with a specific instance—say the drooping ears of many domesticated animals. "Cats in China, horses in parts of Russia, sheep in Italy and elsewhere, the guinea-pig in Germany, goats and cattle in India, rabbits, pigs, and dogs in all long-civilized countries, have dependent ears."

Here the influence of natural selection is almost wholly excluded; nor can artificial selection be supposed to have operated in most of the cases: save, perhaps, in some pet animals, selection has been carried on to develop other traits. In the cases of most of these creatures, too, artificially fed and often over-fed, it does not seem that individual fates can have been affected by economy of nutrition, either general or special; since there has been no struggle for existence to cause the survival of those in which nutriment was most advantageously distributed. Further, the parts in question are not of such sizes that economy in nutrition of them could sensibly affect the fates of individuals, even had the struggle for existence been going on. Again, it seems that in respect of the ears themselves (though not in respect of their motor muscles) there has been extravagance of nutrition rather than economy of nutrition; since even where selection has been carried on for increasing other traits, the ears have not dwindled but rather increased. Lastly, at the same time that there has been this superfluity of nutrition in the ears themselves, their motor muscles appear to have dwindled either relatively or absolutely—at least relatively, we must suppose, where the weight of the ears has increased, and absolutely where the weight of the ears has not increased.

The question presented by these facts is one in the solution of which the theory of "panmixia" may, I think, be satisfactorily tested; and without expressing any opinion upon the matter myself, I should be glad to see it discussed.

HERBERT SPENCER.

I AM not aware how far Prof. Ray Lankester is disposed to acknowledge his obligations to Prof. Weismann for what I am glad to see he now calls his "anti-Lamarckian" (as distinguished from "pure Darwinian") proclivities. Therefore I do not know how far he professes to be one of "the followers of Prof. Weismann," to whom my previous letter on this subject was addressed. But it seems desirable that I should take some notice of the altogether distinct question which he has now raised—viz. whether, or how far, Prof. Weismann's anti-Lamarckian views were anticipated by Mr. Darwin.

His argument is that Darwin must have been a Lankesterian anti-Lamarckian in disguise; and, more particularly, that "the doctrine of panmixia is recognized and formulated in the last (sixth) edition of the 'Origin of Species' published in 1872."

Taking the most general statement first, Prof. Lankester represents it as not improbable that "when Darwin refers, here and there throughout his works, to a reduced or rudimentary condition of an organ as 'due to disuse,' or 'explained by the effects of disuse,' he does not *necessarily* mean such effects as the Lamarckian second law asserted and assumed (though often he does appear to mean such); but he may mean, and probably had in his mind, the effects of disuse as worked out through panmixia and economy of growth."

Now, here we have a specimen of Prof. Lankester's dialectic at its worst. Truly, with such an interpreter, Darwin "*may*" be made to "mean" anything. First it is represented as seeming "not at all improbable that when Darwin refers" to one principle, "he does not *necessarily* mean" what he says; and then it is concluded that "he may mean, and *probably* had in his mind a totally different principle." Moreover, what is represented as mere references, "here and there throughout his works," are, as all the world knows, one whole and "highly important" (though still subordinate) side of Darwin's system. Yet again, in all passages where the meaning assigned to his term "disuse" is explained, there can be no shadow of ambiguity attaching to it, and everywhere it is alluded to as a principle wholly distinct from the "economy of growth"; while panmixia, as I shall presently prove, is nowhere mentioned at all. This, indeed, is clearly shown even in the passages quoted by Prof. Lankester, and now re-quoted below. For it is there said that, could a certain explanation be found, "then we should be able to understand how an organ which has *become useless* would be rendered, *independently of the effects of disuse*, rudimentary." Obviously, in this context, "the effects of disuse" cannot possibly mean "the effects of disuse as worked out through panmixia and economy of growth": they *can* only mean the direct effects of disuse itself in causing inherited atrophy. And now, lastly, "the effects of disuse" are habitually pointed to by Mr. Darwin in association with the "effects of *increased use*"; and how he can "seem" to have "explained" *these* either by the economy of growth (which he fully recognized), or by panmixia (which he never recognized), I must leave Prof. Lankester to indicate.

It will be observed, from the point last mentioned, that this attempt to read the doctrines of Weismann into the writings of Darwin must equally collapse, whether or not any other human being can be found to follow Prof. Lankester in his commentary on Darwin's "here and there" references to "the effects of disuse": the equally constant and as frequently detailed references to "the effects of the increased use of parts, which I have always maintained to be highly important," are of themselves sufficient to dispose of the Lankesterian gloss. Nevertheless, it remains worth while to see whether there is any shred of evidence in support of the narrower or more particular statement, that the principle of panmixia is to be found "already indicated" in the "Origin of Species." The following are the passages upon which this statement is founded—passages, I may remark, which have certainly neither been "missed" nor "neglected" by me.

(1) "If under changed conditions of life a structure before useful, becomes less useful, its diminution will be favoured, *for it will profit the individual not to have its nutriment wasted in building up a useless structure*. . . . Thus, as I believe, *natural selection* will tend in the long run to reduce any part of the organization as soon as it becomes, through changed habits, superfluous, without by any means causing some other part to be largely developed in a corresponding degree" ("Origin of Species," sixth edition, p. 118).

(2) "Organs, originally formed by the aid of natural selection, when rendered useless, may well be *variable*, for their variations can no longer be checked by natural selection. . . . It is scarcely possible that disuse can go on producing any further effect after the organ has once been rendered functionless. Some additional explanation is here requisite, which I cannot give. If, for instance, it could be proved that every part of the organization tends to vary in a greater degree towards diminution than towards augmentation of size, then we should be able to understand how an organ which has become useless would be rendered, independently of the effects of disuse, rudimentary, and would at last be wholly suppressed; for the variations towards diminished size would no longer be checked by natural selection. *The principle of the economy of growth* explained in a former chapter [cited in quotation No. 1], by which the materials forming any part, if not useful to the possessor, are saved as far as possible, will perhaps come into play in rendering a useless part rudimentary" ("Origin of Species," sixth edition, pp. 401–402).

Can it be that Prof. Lankester has not even yet perceived the significance of "the idea" of panmixia? Such certainly seems to be the case from his use of the above quotations. For the words which I have italicized render it most obvious that the only principle under consideration is the economy of growth or nutrition, *i.e.* the *reversal* of selection: there is no allusion to panmixia, or the *cessation* of selection. In the second passage it is shown that, because "no longer checked by natural selection," useless organs will become *variable;* and hence that *if there were any other cause tending to degeneration* (such as the "impoverished conditions" subsequently suggested), natural selection would *not interfere* with—*i.e.* prevent or "check"—the degenerating process thus induced. But there is no hint that the mere cessation of natural selection must be *itself*, and in *all cases*, a cause of degeneration.

Similarly, at the end of his letter, Prof. Lankester again fails to distinguish between the cessation and the reversal of selection. For, after endeavouring to represent that Mr. Darwin did not understand my "view,"[1] he says, "it is not at all surprising that Mr. Darwin did not recognize any resemblance between it and his own statement, viz. that 'the materials forming any part, if not useful to the possessor, are *saved* as far as possible,' thus 'rendering a useless part rudimentary.'" Not surprising, indeed. But it is surprising that Prof. Lankester, even at this time of day, should thus appear incapable of clearly distinguishing between natural selection as *withdrawn* and as *reversed*. For this is the whole point, and the only point so far as "the doctrine of panmixia" is concerned. It is a matter of familiar knowledge that Mr. Darwin at all times and through all his works laid considerable stress upon the "economy of growth," (or, more generally, reversed selection); but, most emphatically, this is *not*, as Prof. Lankester now says it is, "the essence of the anti-Lamarckian view of the effects of disuse." The essence of this view is, and can only be, the *cessation* of selection, as Prof. Weismann has clearly perceived.[2]

In order that there shall be no doubt upon this point, I must here explain the importance of the *cessation* of selection, as distinguished from the *reversal* of selection, in regard to "the essence of the anti-Lamarckian view"—even though in so doing "I feel it rather a severe burden when I am called upon to expound the merest commonplaces of the subject under discussion."

As stated in my previous letter, "the principal evidence on which Mr. Darwin relied to prove the inheritance of acquired characters was that which he derived from the apparently inherited effects of use and disuse—especially as regards the bones *of our domesticated animals*." Now, the reason why our domesticated animals appeared to furnish the most unequivocal proof of the inherited effects of disuse (and so, likewise, of the inherited effects of use, as explained in my last letter) was this. In the case of all species in a state of nature, it is, as Darwin observed, impossible to eliminate the effects of natural selection (acting through the economy of growth, or otherwise) from those of disuse, supposing disuse to be a cause of degeneration in species as it is in individuals. Therefore, in order to estimate what, if any, is the proportional part that is played in degeneration by the inherited effects of disuse, it is necessary to find cases where disuse, if it ever acts at all, must be acting *alone*. Such cases Mr. Darwin took to be furnished by our domesticated animals, seeing that they are so largely protected from the struggle for existence on the one hand, while, "on the other hand, with highly-fed domesticated animals, there seems to be no economy of growth, nor any tendency to the elimination of superfluous details." Having found in such cases material for ascertaining the effects apparently caused by disuse *alone*, Darwin concluded that he was able to estimate the degree in which these effects occurred elsewhere, or generally; even though in all wild species they must usually be more or less associated with the effects of reversed selection. Therefore it was that he chose domesticated animals for all his weighings and measurings of comparatively disused parts—with the result of appearing to obtain good evidence of a high degree of reduction as due to the inherited effects of disuse alone. But it did not occur to him that the amount of reduction thus proved might be equally well explained, not indeed by the *reversal* of selection (as in wild species), but by the *cessation* of selection, or panmixia. And it is just because the cessation of selection thus applies with even more certainty to the case of domesticated animals, than does the reversal of selection to the case of wild animals, that the former principle is of such unique importance to "the essence of the anti-Lamarckian view": by its means, *and by its means alone*, can the apparent evidence of the inherited effects of disuse be overthrown.

Therefore, by seeking to assimilate the distinct principles of selection as withdrawn and selection as reversed, Prof. Lankester is performing but a sorry service to his anti-Lamarckian cause. Weismann may well cry, "Save me from my friends," when he finds them thus playing into the hands of his opponents. For on all the logical bearings of his principle of panmixia, Weismann has perfectly clear and accurate views; and although he was not accurate in representing the relations which obtain between this principle and that of reversed selection, such is but a small error compared with Lankester's identification of the two principles—with the necessary result of again bringing into court the whole body of direct evidence on which Darwin relied in his apparent proof of Lamarck's "second law."

We shall now, perhaps, be able to understand what Prof. Lankester means when he says: "The idea [of panmixia] occurred to me also shortly after the passages above quoted from Mr. Darwin were published." If this is the case, "the idea" in question must have "occurred" to Prof. Lankester before he had reached his teens, seeing that one of "the passages" in question is not confined to "the last edition of the 'Origin of Species,'" but runs through them all. Allowing this to pass, however, what I have now to remark is, that if the idea which occurred to Prof. Lankester "shortly after the publication of that work" (1872) was, as he alleges, the idea of panmixia, it becomes a most unaccountable fact that in his laborious essay on "Degeneration" (1880) there is no hint of, or even the most distant allusion to, this idea. Yet, in the presence of this idea, "Hamlet" without the Prince of Denmark would be a highly finished work compared with an essay on "Degeneration" without any mention of panmixia. Therefore, here again, I can only understand that Prof. Lankester has not even yet assimilated "the idea in question." He confounds this idea with that of the economy of growth: he fails to perceive the very "essence" of the idea, in the all-important distinction between selection as withdrawn and selection as reversed. Without question, his essay on "Degeneration" proves a familiar acquaintance with the doctrine that "the materials forming any part, if not useful to the possessor, are saved as far as possible"; but, most emphatically, this is *not* "the idea of panmixia," while it *is* the idea that is definitely "formulated" scores and scores of times through all the editions of Mr. Darwin's works—an "idea," therefore, which must necessarily have "occurred" to every reader of those works since the time when Prof. Lankester was at school.

As this letter has already run to an inordinate length, I will relegate to a footnote my discussion of the merely personal criticisms which Prof. Lankester has passed upon my former communication.[1] GEORGE J. ROMANES.

London, March 28.

[1] There is something comical to me in this endeavour, in view of all the conversations and correspondence which I had with Mr. Darwin upon the cessation of selection. Moreover, I do not in the least agree with Prof. Lankester where he says that my "view, as it appears in Mr. Darwin's words ('Variation,' &c., vol. ii. p. 309), is certainly *not* the same as that which Mr. Romanes has expounded in NATURE of March 13, 1890." That my "view" is not *fully* given, Mr. Darwin himself affirms; but, "as far as it can be given in a few words," it is given as correctly as I could wish.

[2] It appears to me that Prof. Lankester cannot have read Prof. Weismann's exposition of "the doctrine of panmixia." For, not only does he make this otherwise unaccountable (and, in relation to his "anti-Lamarckian view," suicidal) blunder of seeking to unite, if not virtually to identify, the principles of panmixia and economy of growth; but he alludes to Weismann as having "stated briefly" the former principle. "Stated briefly" it certainly is in "the translated essays"; but this is only because it is set out at length in one of the untranslated essays, which is entirely devoted to expounding the matter ("Ueber den Rückschritt in der Natur"). And this reminds me that in his review of Mr. Wallace's "Darwinism" there is a passage which similarly indicates that Prof. Lankester has either not read, or has strangely forgotten, another of Weismann's unpublished essays. Therefore, seeing how ready he is, on account of a precisely similar omission, to jump upon Mr. Herbert Spencer—whose recent and protracted illness is notorious—one can scarcely refrain from asking in his own words, "Will not Mr. Spencer and *others* who are interested in these matters read Weismann's essays?"

[1] Prof. Lankester says:—"As soon as the matter had taken root in his mind, Mr. Romanes published in NATURE, March 12, April 7, and July 2, 1874, an exposition of the importance of the principle of cessation of selection as a commentary upon a letter by Mr. Darwin himself (NATURE, vol. viii. pp. 432, 505), in which Mr. Darwin had suggested that, with organisms subjected to unfavourable conditions, all the parts would tend towards reduction. Mr. Darwin, with his usual kindly manner towards the suggestions of a young writer, gives, at p. 309 of vol. ii. of 'Animals and Plants under Domestication,' Mr. Romanes's view, 'as far as it can be given in a few words.'" Now, as it is only a few days ago that I myself directed Prof.

The Spectrum of Subchloride of Copper.

It is noticed in Nature (vol. xli. p. 383), as the substance of a paper read to the Academy of Sciences in Paris, on the 10th ult., by M. G. Salet, on the blue flame of common salt, and on the spectroscopic reaction of copper-chloride, that the strongest lines of the former flame, in the indigo and blue, are due to copper-chloride, and coincide with bands given in M. Lecoq de Boisbaudran's "Spectres Lumineux."

Copper and chlorine appear, from the easy formation of copper-subchloride, to have a very unstable affinity for each other; and the readiness with which copper itself seems to volatilize, as shown by Mr. John Parry, in his spectroscopic experiments for the Ebbw Vale Steel-making Company in Wales, on the detection of impurities in iron and steel, by the free and wide diffusion of its vapours compared with those of other metals to a distance from a blowpipe flame, would perhaps tend to promote dissociation and to the production of subchloride from chloride of copper, at least in the presence of reducing-gases, in a flame.

There is a considerable general resemblance in respect of place and brightness between the groups of lines belonging to chlorine, and those belonging to copper-chloride, as those two spectra are represented in M. Lecoq de Boisbaudran's work. But the two spectra are of course very far from showing any precise coincidences with each other. My attention was drawn some time ago (in July 1878, Nature, vol. xviii. p. 300) to a set of line bands of this same description, in very near correspondence, apparently with the chief lines of the copper-chloride spectrum, which presented itself in a violet-blue flame seen very frequently in ordinary fires when they have been fed with almost any kind of household dust and rubbish. But the remarkably neat triplet of line-pairs—green, blue, and indigo—in this blue fire-flame's spectrum could only be recognized as very indistinctly matched by those chief lines of the spectrum of copper-chloride, as those are produced, for instance, by introducing into a Bunsen-flame a piece of copper-foil well wetted with hydrochloric acid; and no counterpart at all to them, any more than to the ordinary chloride of copper spectrum, could be traced in the well known blue fire-flame of common salt, in whose spectrum, when pure, as well as in that of the equally familiar blue fire-flame (when pure also) of carbonic oxide, I do not remember to have ever detected any lines or bands of greatest brightness so obviously discernible and distinct as to admit of measurements.

In the case of a copper-melting furnace, round the loose junction of whose lid small escaping bodies of blue flame, on one of the days on which I analyzed them, showed the well-defined triplet spectrum very neatly, it was afterwards mentioned to me (when that observation had been noted at the above place in Nature), that pieces of ships' old copper-sheathings were sometimes put into the copper-melting pot; and just as the use of logs of broken-up ship-timber (as was also stated at that place in Nature) explained a gorgeous blaze of this flame's fine blue colour in a London house-fire very satisfactorily, so foreign importations by salt into waste-materials from seaworn ships, might by such a practice's occurrence as this in the melting furnace, account very well for the presence of chlorine along with copper in the furnace efflagrations which showed the neat and easily recognized line-spectrum on one of the days of my spectroscopic examinations of them, very plainly. Neglected scraps of brass and copper become, however, so soon contaminated with chlorine in nearly all situations, that it suffices, in general, to throw any rusty piece of them, such as an old, dirty piece of thin brass or copper wire, among the glowing coals of a bright fire, to produce this peculiar-spectrumed blue flame in the hottest crevices of the fuel.

The nature of this flame, since it differs very materially, by the simplicity of its spectrum, from the ordinary one of chloride of copper, although in the strong point of line-positions there is a partial feature of similitude in the spectra of the two flames by which they agree very nearly with each other, remained a mystery to me for several years; but about four years ago I chanced by good fortune to hit upon a compound, in some experiments on subchloride of copper, which yielded in a flame, at least a successful imitation, if not, as seems most probable, the really natural and perfectly exact reproduction of it. Copper subchloride is easily obtained by evaporating hydrochloric acid to dryness in an open dish on an excess of wire clippings or other small fragments of metallic copper. It is a dark greenish-brown powder, which easily deliquesces, and by absorbing oxygen, if exposed to the air, is soon converted into the green chloride of copper. For the spectroscopic purpose it should be dissolved when first formed, and dry, in about its own weight of hot glycerine, and the solution be allowed to cool in a well-corked bottle. This pasty solution inflames, when heated on a wire, and burns with the peculiar-spectrumed violet-blue flame which is observable in common fires when contaminations of copper by chlorine are introduced among the fuel, in its hottest parts. Although these contaminations in the state of exposure to common air probably all consist of ordinary chloride of copper, yet among the interstices of the fire, by the presence of hot fuel and great abundance of carbonic oxide, they doubtless undergo reduction to subchloride, and, in place of the many-lined and banded green-flaming spectrum of ordinary copper-chloride, the far simpler and symmetrically grouped one of three line-pairs—green, blue, and indigo—belonging to subchloride of copper vapour presents itself in the fine blue tint which the fire's flames assume, one may suppose, by chloride's reduction to subchloride, and by the infinitesimal admixture in them of this latter foreign substance. The varieties of tint, from blue below to green above, which a Bunsen-flame exhibits when chloride of copper is introduced into it, are probably due to the same chemical conversion, in dependence on the reducing or oxidizing constitution of the flame in its inner and outer layers, which most purely exhibit the two different colorations.

To produce the subchloride of copper spectrum very purely, the thinnest possible smear of its pasty solution in glycerine, on one side of a narrow strip of paper, suffices very amply, since its colouring effect upon the flame, when the strip is rolled up into a spill and lighted, is very powerful. Chlorate of potash powder, kneaded up with the glycerine solution, burns also self-supportingly with the characteristic rich blue colour, but the spectrum in this case, and also when the paper stain of the glycerine solution is left long exposed to air upon the strip

Lankester's attention to this passage, and as it appears evident that he has not referred to my original letters in Nature, I conclude that he does not know how completely I there recorded my obligation to the article by Darwin which really first did engender the doctrine of panmixia. But, be this as it may, the following is what I wrote:—

"In a former communication I promised to advance what seemed to me a probable cause—additional to those already known—of the reduction of useless structures. As before stated, it was suggested to me by the penetrating theory proposed by Mr. Darwin, to which, indeed, it is but a supplement" (1874).

Again, in 1887, while anticipating and greatly extending Prof. Lankester's present criticism touching Mr. Spencer's attitude with respect to panmixia, I said:—

"The leading idea in Mr. Darwin's suggestion was that impoverished conditions of life would accentuate the principle of economy of nutrition, and so assist in the reduction of useless structures by free intercrossing. Now, in this idea, that of the cessation of selection was really implied; but neither in his own article, nor in a subsequent letter by Mr. George Darwin on the same subject (Nature, October 16, 1873), was it exhibited as an independent principle. It was inarticulately wrapped up with the much less significant principle of impoverished nutrition."

The simple history of the matter, therefore, is as follows. Even up to the time of publishing his article in Nature, Mr. Darwin had not perceived the principle of panmixia as an "independent principle"—any more than Dr. Dohrn perceived it in 1875, or Prof. Lankester perceived it in 1880,—which must act in all cases of degeneration, whether with or without the co-operation of reversed selection in the economy of growth, "impoverished conditions," &c. Therefore, in the sixth edition of the "Origin of Species," after having explained the phenomena of degeneration by the inherited effects of disuse, combined with the economy of growth, he proceeds to give very good reasons for concluding that "some additional explanation is here requisite which I cannot give"; and he suggests that, "*if it could be proved* that every part of the organization tends to vary in a greater degree towards diminution than towards augmentation of size, *then we should be able to understand* how an organ which has become useless would be rendered, *independently of the effects of disuse*, rudimentary," &c. But although he thus saw the "explanation" that was "requisite," he said he was unable to give it; therefore at that time he could not have seen that the cessation of selection was exactly the explanation of which he was in search—to wit, a principle which must always make every unused part of the organization tend to degenerate. Later on, however, it occurred to him that "impoverished conditions," *combined with intercrossing*, might lead to this result. But, although he thus came to such close quarters with the idea of panmixia that he immediately suggested it to me on reading his exposition, the idea was still entangled with that of "impoverished conditions" being required in order to *starve* the degenerating parts. Therefore, the only hand that I had in the matter was to liberate the all-important principle of panmixia from the toils of this entanglement, and thus to show that it must necessarily act in the case of *all* unused structures, with the result of destroying the evidence of "the effects of disuse."

Such is a simple history of the facts; and my only object in previously alluding to the part which I had played in the matter was not that of claiming priority touching so very obvious an "idea," but in order to show how it was that Mr. Darwin, through all the editions of the "Origin of Species," continued to attribute important weight to a line of evidence in favour of the inherited effects of disuse, which the doctrine of panmixia, *and the doctrine of panmixia alone*, has entirely destroyed.

of paper, is apt to lose its purity and acquire confusing lines and bands of ordinary copper-chloride, by oxidation, which the preparation then undergoes spontaneously, before igniting it. For pyrotechnists, therefore, it seems scarcely probable that the subchloride of copper, with its pure cerulean flame, will ever be of any very useful value. But as a parallel example of a coloured-fire composition, it may be mentioned here, that powdered Val-Traversite (a bituminous limestone found near Neuchatel, in Switzerland), on account of its prodigious natural richness in bitumen, when mixed with sufficient chlorate of potash, also burns self-supportingly, with a fine orange-red flame in which the familiar spectrum of calcic oxide is, of course, most vivid. Were hot asphalt, pitch, or bitumen, instead of hot glycerine, used to dissolve or to "masticate" the dry subchloride of copper when it is freshly made, a copper-chlorinated mass would be produced which would probably be capable of resisting atmospheric action, and whose mixture with chlorate of potash would, like the similar Val-Traversite mixture, probably also not suffer by keeping and exposure, and would furnish a source of blue flame and of the significantly simple spectrum of subchloride of copper, not less vividly true and fixed in their distinctness, than the orange-red light and strongly pronounced calcic-oxide spectrum of the other combination of chlorate of potash with a bitumen-containing substance.

As regards the blue salt-flame, whose spectrum in its purity shows no conspicuous lines, or bands of greatest brightness, it can hardly be doubted that the element chlorine, from the positions of its own principal line groups, contributes mainly to produce the blue coloration, at a temperature, in the fire, which is not high enough to dissociate the sodic chloride and liberate sodium vapour, with its tell-tale yellow line, from its chemical union. In the green flame of chloride of copper the colouring groups of lines show a more detailed resemblance than this to the chief colorific lines in the elementary chlorine spectrum,[1] while in copper subchloride's "bluest of blue" flames, the wide green light-bands of copper chloride fade out, leaving the colorific light concentrated almost entirely in three close pairs, or in six bright lines, which, if they do not coincide in place with, are at least not far distant in position from, three chief line-pairs in the ordinary spectrum of chloride of copper. There is much in these resemblances which betokens some kind of continuity of connection with the primary features of the chlorine spectrum itself; the evidences of which, although thus displayed by copper and chlorine in the spectroscope, may perhaps be sensibly regarded as having some near relation of analogy to the appearance of variable chemical combining power under the influence of light, between silver and chlorine, presented in photography. But there is also, undoubtedly, a very marked distinction between the "spectroscopic reactions" of these two different copper chlorides; and, similarly, there are in the apparently mutable photochemical affinity between silver and chlorine in photography, two fairly stable delimitations of its range, in the "subchloride" (or as it has been termed by Mr. Clement Lea, the "photochloride") of silver, and in ordinary silver-chloride. Further discriminations of the copper-chloride spectra in intermediate forms which they seem to comprise transitionally between the two definite ones of the chloride and subchloride, would perhaps extend and strengthen this analogy, and may not impossibly help, at some future time, to explain and illustrate it, if there is any real soundness in it, more fully and completely.

[1] A very suggestive example of a substance's detection by recognition of its spectrum was described, with a drawing of the recorded spectra, by Mr. A. Percy Smith, in a short notice of a series of observations on the spectra of chlorides, and on the blue flame of common salt, in the *Chemical News*, vol. 39, p. 141 (1879). An examination of the flame-spectra of several different chlorides, enabled the author of that notice to recognize a common similarity among them all to the spark- or flame-spectrum of hydrochloric acid gas. This gas showed a belt of green line-bands which agreed in their main positions with the green portion of a long array of band-pairs shown with much constancy by several different alkaline and earthy chlorides, and especially by ammonium chloride, and by mercurous chloride (or calomel, where the agreement was also verified by a direct comparison), in a Bunsen flame; but no line-counterparts to the equally bright, blue-lined portion of the same constant spectral striation were observable in the hydrochloric acid spectrum.

From the easy conversion of chlorides into the corresponding oxides in an air-gas flame, when the flame is not kept artificially saturated with hydrochloric acid gas, we might pretty certainly assume that in the flame's ordinary condition, the heated chlorides would always disengage sufficient chlorine to produce by combination with hydrogen in the coal-gas of the flame, traces of the stable product, hydrochloric acid gas, among the gases of the flame's combustion; and the different chlorides would thus, by suppositions which may not perhaps be unlikely and inadmissible, all supply the flame alike with the substantial factor needed, for the appearance of the green line portion of the constant spectrum.

At the same time new carbon-compounds would be formed by dehydration of the flame's gaseous hydrocarbons, to furnish hydrogen to the liberated chlorine, and some constant carbon-gases then, of not yet known descriptions, might be conjectured just as comprehensibly and fitly, to be concurrently productive in the constant chloride-rank's illumination, of the blue-line portion of its bands, of which no spectral counterparts could be detected in the hydrochloric acid spectrum.

But whether the interesting figure and description given by Mr. A. Percy Smith in the above paper, of his long series of experiments, may or may not admit of such a simple spectro-chemical interpretation, the conflicts of contending chemical affinities of which the spectroscopic recognition of hydrochloric acid in flames fed with different chlorides furnishes such a wonderful example, give weight and value to the notes of the discovery recorded by Mr. A. Percy Smith, in a new wide field of the spectroscope's utility, which are of much deeper interest than any single theory to account only for this particular recognition and discovery itself.

Mr. A. Percy Smith's own capitally based, and clearly proved deductions from his numerous experiments, were accordingly, in prospect of their further prosecution, expressed thus, quite generally:—that the blue flame of common salt in a hot fire owes its coloration to reactions either exactly or very nearly similar to those which produce resemblance of a nearly constant spectral type in different chloride flames, to that of hydrochloric acid; and that, again, among the partly undetermined, and perhaps to some extent variable reactions which produce the similarity, there appear to be some which disturb and modify the ordinary appearance of the hydrochloric acid spectrum, and which would appear to superadd to it a series of blue line-bands which, as it is presented in a flame, or electrically in vacuum tubes, the spectrum of pure hydrochloric acid gas alone does not usually exhibit.

The example of fluoride of calcium is a curious one in spectrum analysis, where sprinkling fluor-spar dust in a Bunsen-flame produces, in addition to the normal calcic oxide spectrum of one orange-red and one green band, a second bright and narrow green one at a distance from the first about equal to that of the red band from it. There are no other distinguishable bands. But if the pair of normal ones is really due to calcium-oxide vapour produced by decomposition in the flame, it is not very easy to conjecture to what other product of decomposition the additional, sharply defined and brilliant, solitary green band can be ascribed. The spectrum of hydrofluosilicic acid gas presents a very gorgeous band-array of violet-blue lines, whose lustrous group is probably indicative of near neighbourhood in place to some bright line concentration in the spectrum of fluorine itself; but if so, the collection of its colorific strength in the single additional green line of the fluor-spar spectrum, seems to imply a freedom from uniformity in fluorine's power of imparting spectral coloration to its compounds, just opposite to the sensible continuity and kinship of spectral clusterings, above described, which the presence of chlorine appears to impose upon its compounds by common resemblances discernible in the blue light-ascendencies of the fire-flames of common salt, chloride and subchloride of copper, when they are spectroscopically analyzed.

A. S. HERSCHEL.

Observatory House, Slough, March 3.

Brush-Turkeys on the Smaller Islands North of Celebes.

THE reviewer of Dr. Hickson's book, "A Naturalist in North Celebes" (March 20, p. 458), believes that the brush-turkey or moleo, *Megacephalon maleo*, has never been recorded as occurring in the smaller islands north of Celebes. I beg to remark that in the year 1879 I recorded this species from Siao, and in the year 1884 from Great Sangi, on both of which islands, besides, occurs a *Megapodius* peculiar to them, viz. *M. sanghirensis*, Schlegel, representing there *M. gilberti*, Gray, from Celebes (see the *Ibis*, 1879, p. 139; *Ibis*, 1884, pp. 6 and 53, &c.). Perhaps Mr. Guillemard did not comprise Siao and Great Sangi under the head of "smaller islands," but Dr. Hickson himself (p. 95) records *two* brush-turkeys from the smaller island of Tagulandang, a larger and a smaller one, and these must be *Megacephalon maleo* and a *Megapodius*. Tagulandang is situated between Celebes and Siao, and much nearer to the latter island. From the volcano islet of Ruang, opposite and within about a mile from Tagulandang, he only records (p. 41) one brush-turkey, and this, of course, may be either the *Megacephalon* or a *Megapodius*, if both do not occur, as appears rather probable. When I visited Ruang in 1871 after the heavy eruption in March of that year (see NATURE, vol. iv. p. 286), nearly the whole of its forest was destroyed and burnt down, and I do not believe that a living brush-turkey then remained on the islet; but it has since been repeopled from its near neighbour, Tagulandang, where both species occur, and therefore, if the one could reach Ruang, the other may have reached it too. This is of no consequence at all. Dr. Hickson's following remark as to brush-turkeys on Tagulandang (p. 95), "The larger bird is perhaps the *Megapodius sanghirensis* of Schlegel, a brush turkey, which is bigger than the *Megacephalon*, and extends over the Sangir Islands," contains a mistake, as *M. sanghirensis* is much smaller than

Megacephalon maleo. The reviewer corrects, by the way, my calling the Celebean whimbrel *Numenius phæopus*, saying that it is probably *N. uropygialis*, but these two names are synonymical. cf. for instance, Salvadori, *Orn. Pap.*, iii., 332, 1882, *sub N. variegatus*. As to its nesting on small trees "small brushes" were intended to be implied (see Legge, "Birds of Ceylon," 1880, p. 913). A. B. MEYER.

Royal Zoological Museum, Dresden, March 22.

Crystals of Lime.

IT was pointed out to me by Mr. W. J. Pope, of the City and Guilds of London Institute, that a lime cylinder which had been used in the lantern during a lecture had become distinctly crystalline where affected by the oxyhydrogen flame.

Examined under the microscope by polarized light, the crystals are seen to be well-defined cubes with striated faces. When immersed in water they break up and give rise to minute doubly refracting plates of rhombic outline, behaving in this respect like ordinary lime; the cubic crystals, however, are less rapidly affected by exposure either to air or water than is amorphous lime.

Lime is commonly stated to be infusible at the temperature of the oxyhydrogen blow-pipe; and the only crystals previously recorded, so far as I know, are those obtained by Brügelmann, by fusing calcium nitrate (*Annalen der Physik und Chemie*, ii. p. 466, iv. p. 277, 1877–78). It seems, therefore, worthy of notice that they are possibly always formed upon the surface of the lime cylinders by the action of the oxyhydrogen flame.

The crystals resemble in all respects those described by Brügelmann. The jet used on the present occasion was an ordinary blow-through jet. H. A. MIERS.

Foreign Substances attached to Crabs.

I AM glad to see that Mr. Garstang agrees with me in regarding the presence of the Ascidians on *Hyas* as accidental.

I had no intention of decrying the value of Mr. Garstang's experiments with Ascidians, but his rule might, perhaps, be limited to those members of the group to which it can be proved to apply. Under natural conditions it apparently fails to apply to *P. corrugata* and *M. arenosa*. As to the latter, Prof. McIntosh assures me that he has frequently found it in the stomach of the cod and haddock.

The appreciation of the cod for *A. mesembryanthemum* is, I think, sufficiently proved by the fact that the latter is one of the most successful cod-baits used here.

ERNEST W. L. HOLT.

St. Andrews Marine Laboratory, March 29.

Wimshurst Machine and Hertz's Vibrator.

IT may interest those who wish to repeat Hertz's experiments on electro-magnetic radiation to know that many of these can be done very well by using a small Wimshurst machine in place of the usual induction coil and battery. The vibrator and resonator which we used were like those described in NATURE (vol. xxxix. p. 548), and the Wimshurst machine had two 12-inch plates (giving at most with the jars on a 4-inch spark). The wires from the vibrator, instead of being connected with an induction coil, were connected with the two outer coatings of the jars of the machine. The machine spark-gap and the vibrator spark-gap should be so adjusted that when a spark occurs at the former one also occurs at the latter. With the apparatus described we got good results when the spark-gaps were 38 mm. and 3 mm. respectively. The outer coatings of the jars are only connected together by the wood of the machine, but it is sometimes an advantage to put a few inches of damp string between the balls of the vibrator.

This combination is obviously a modification, adapted to work a Hertz vibrator, of one of Dr. Lodge's well-known Leyden jar arrangements.

No doubt many persons have connected the vibrator directly with the terminals of the machine, but this arrangement does not work nearly so well. T. A. GARRETT.
W. LUCAS.

THE INSTITUTION OF NAVAL ARCHITECTS.

THE annual meeting of the Institution of Naval Architects was held under the presidency of Lord Ravensworth, on Wednesday, Thursday, and Friday of last week. There was a fair list of papers on the programme, although at one time, shortly before the meeting, it was feared that there would be a sad lack of contributions from members. At the last minute, however, one or two papers came in, and the list, although perhaps below the average in the importance of the memoirs, was of passable interest.

The following is a consecutive enumeration of the business that was transacted at the meeting:—

Wednesday, March 26th: morning sitting—Annual Report of the Council, and other routine business; Address by the President. Paper read and discussed—Notes on the recent naval manœuvres, by Mr. W. H. White, F.R.S., Director of Naval Construction.

Thursday, March 27th: morning sitting—The Maritime Conference, by Rear-Admiral P. H. Colomb; strength of ships, with special reference to distribution of shearing stress over transverse section, by Prof. P. Jenkins; steatite as a pigment for anti-corrosive paints, by Mr. F. C. Goodall. Evening sitting—On the evaporative efficiency of boilers, by Mr. C. E. Stromeyer; on the application of a system of combined steam and hydraulic machinery to the loading, discharging, and steering of steam-ships, by Mr. A. B. Brown; the revolving engine applied on ship-board, by Mr. Arthur Rigg.

Friday, March 28th: morning sitting—On leak stopping in steel ships, by Captain C. C. Penrose Fitzgerald, R.N.; on the variation of stresses on vessels at sea due to wave motion, by Mr. T. C. Read; spontaneous combustion in coal ships, by Prof. Vivian Lewes. Evening sitting—Experiments with life-boat models, by Mr. J. Corbett; on the screw propeller, by Mr. James Howden.

The annual dinner was held on the evening of Wednesday.

Out of the above list of a dozen papers there were fewer than usual of scientific interest, and, indeed, in one or two instances they were not either distinguished by practical interest. Mr. White's paper, which formed the *pièce de resistance* of the meeting, was of military rather than scientific importance, and was chiefly notable from the number of admirals that took part in the discussion; indeed, the whole naval contingent of the Board of Admiralty was present to hear the paper read. Admiral Colomb's paper on the recent Washington Maritime Conference was practically reduced to a consideration of the rule of the road at sea. The general opinion of the authorities assembled appeared to be that the present rule of the road is very well as it stands, with the exception that the "holding-on ship" should not be required, or even allowed, to slacken her speed. This seems in conformity with common-sense. If two ships are converging towards a point, say at right angles to each other, and one shifts her helm to go under the other's stern, if the second, or holding-on ship, slacken speed, the probability will be that the giving-way ship will crash into the other's broadside or cross her bows; in the latter case, there is probability that the holding-on ship will give the other her stem. What is most wanted when danger of collision arises, is certainty on each vessel as to what the other may be going to do. If the holding-on ship never slacken speed—is not allowed to slacken speed—then the other vessel knows exactly what course to take; as the law stands, the quartermaster, or officer in charge, is never quite sure until the last minute, especially at night, whether the other ship considers there is danger of collision or not, and, therefore, whether she will slacken or keep to full speed. We anticipate the proposed alteration, if put in force, will greatly lessen the list of collisions.

The memoir contributed by Prof. Jenkins on the strength of ships was decidedly the most important contribution to naval science of this year's meeting. The paper will open up to the majority of those practically engaged in the design of ships a new field of research, the investigation of which will enable them to solve some problems which have hitherto been without explanation. That is, speaking generally—for the influence of longitudinal bending moment on shearing stress has before been investigated by naval architects; notably by Mr. W. H. White, the Director of Naval Construction, and Mr. W. John. This, however, was many years ago, and in connection with wooden ships with no longitudinal connection between the planking except that supplied by dowells, the friction of the edges, and the "anchor-stock" shape of the pieces. It will be evident, therefore, that previous investigations must have been of a qualitative, rather than of a quantitative, form; and the world of naval architecture is much indebted to the occupant of the John Elder Chair at Glasgow for putting the problem on a practical quantitative basis.

The paper contributed by Mr. C. E. Stromeyer had a most attractive title, "The Evaporative Efficiency of Boilers"; and a good many of the working marine engineer members of the Institution, who were acquainted with the thorough manner in which the author follows up all his work, had assembled to hear the paper read, and take part in the discussion. We are afraid it must have been somewhat of a disappointment to several of these gentlemen when they turned over the leaves of the paper as it was placed in their hands, and found that the matter was rather of a suggestive than of a conclusive character. There is so much business to be crowded into the three days' annual meeting of this Institution that it is necessary the papers should be read with despatch; and we quite sympathize with the engineer whose daily task is of an administrative rather than a contemplative nature, when he is asked to assimilate at a galloping pace two or three pages of mathematical formulæ of by no means an every-day character.

Mr. Stromeyer confined himself chiefly to a consideration of the relative distribution of efficiency in the tubes. He points out that the distribution is governed partly by the temperatures in the combustion-chamber and smoke box, and partly by the resistance of gas in the tubes, and this again depends upon the velocity and temperature of the gas, and on the loss of heat experienced by it. Mr. Longridge has found that the coefficient of transmission of heat through boiler-tubes or combustion-chamber plates is eleven calories of heat per square foot per hour for every degree F. of difference between the gas and the water: 0·091 is the reciprocal value, and is the resistance offered to the flow of heat under the above condition. This resistance is offered when heat passes from one medium to another, as, for instance, from the gas to the metal, from the metal to the boiler scale, or to the water, and it also includes the resistance offered by the metal to the scale. For iron and boiler scale the resistances are 0·00202 and 0·207 per inch thickness; so that a clean $\frac{1}{2}$-inch plate would offer 0·001 resistance; or, if covered with scale one-tenth inch thick, the resistance would be $0·001 + 0·021 = 0·022$.

Arguing from these facts the author concludes that the chief resistance, about 80 per cent., is encountered at the surfaces; and he doubts whether the change of medium from iron to scale, and to water, influences the values very much. The chief difficulty in transmitting heat from the gas to the tubes is want of circulation, or admixture of gas in the tubes. He speaks favourably of draught retarders, corrugated tubes, and ribbed-tubes for the purpose.

Mr. Stromeyer next refers to the experiments of Haverez (see *Ann. du Génie Civil*, 1874), by whom it was shown that more heat is absorbed in the fire-box with flaming material than with flameless coke. It is well known that a luminous flame radiates more heat than one which is non-luminous; and it is for this reason that the latter may not be used in the Siemens-Martin furnace. For reasons given, Mr. Stromeyer would prefer that, in the formulæ used by Mr. Longridge for heating boiler tubes, the coefficient of resistance $\frac{1}{m}$ should be somewhat increased; say from 0·091 to 0·1. This the author works out in detail. We have stripped Mr. Stromeyer's arguments of their mathematical aspect, as, however interesting the matter may be, we have not space to do it justice. We must refer those of our readers who are sufficiently interested in the subject to the Transactions of the Institution.

Mr. Macfarlane Gray, of the Board of Trade, was the chief speaker in the discussion which followed. He said he could not pretend at one reading to follow the author in all his reasoning. Mr. Fothergill, who is the superintending engineer to a north country line of steamers, gave the meeting the benefit of his practical knowledge upon the subject. Mr. Fothergill is well qualified to speak on the question of the evaporative efficiency of marine boilers, as he has made an especial study of the matter in the actual working of vessels in connection with his well-known researches on the subject of forced draught on ship-board.

Mr. Brown's paper was one of unusual interest to the members of the Institution. In it he described the most recent development of that beautiful system by which he has so vastly improved the loading and discharging of cargo on steam-ships, and the steering of vessels. The paper was illustrated by several diagrams without the aid of which it would be impossible to make clear the details of the very ingenious methods by which the author has applied his combined steam and hydraulic practice to the purposes named. Briefly stated, it may be said that, in place of the usual deck winches, there is placed at every hatch a derrick, having mounted upon it the hydraulic cylinder which supplies the motive power to lift the goods. The steering motor is placed directly on the quadrant of the tiller, and is actuated from the bridge by means of what the author describes as a telemotor. The transmission of the controlling force which governs the steering motor is through hydraulic pipes; a vast improvement on the rattling chains and rods now in common use. In fact the great virtue of Mr. Brown's system is its quiet working.

Mr. A. Rigg's revolving engine is an ingenious device, perhaps better suited to water than steam. It was fully described in Section G at the last Birmingham meeting of the British Association.

"Leak Stopping in Steel Ships" was the somewhat misleading title of a rather weak paper by Captain Fitzgerald. The only point the author suggested was that war-ships should be outside sheathed with wood in order that there might be some attachment to which leak stoppers could be affixed. The contention that the swelling of wood by moisture that takes place, or used to take place, when a shot cut through the side of an old man-of-war is quite beside the mark, as we suppose no one proposes to make the wood sheathing of a modern steel steamer as thick as the sides of our old wooden walls. Three or four inches of elm would do very little swelling when pierced by a modern projectile of any considerable size.

Mr. T. C. Read's paper on the variation of stresses at sea is another of those contributions which are the despair of the practical naval architect, not over-given to abstruse science, who attends the meetings of his Institution, hoping to take part in the discussions. We are quite at one with the speaker, Mr. Alexander Taylor, who proposed that a rule should be passed compelling contributors to send in their papers sufficiently early for them to be printed and distributed to members before the meetings. The executive say it cannot be done, but it would be worth trying for a time.

Prof. Lewes's paper on the ignition of coal cargoes was quite a new departure in the practice of the Institution. When the members assembled they found an array of bottles, flasks, and chemical apparatus, that was not a little puzzling to those not in the secret, and must have reminded many of the dear old Polytechnic days and Prof. Pepper. However, the lecture, and the experiments by which it was illustrated, were of a thoroughly sound and practical nature. The question of spontaneous ignition of coal cargoes is one for the ship-owner rather than the ship-builder; excepting that ship-builders have to replace the vessels which are destroyed by reason of such spontaneous ignition. The lecturer illustrated the influence of carbon in producing heating by the power it possesses of attracting and condensing gases upon its surface. The action of the bituminous constituents of the coal in spontaneous ignition was next dealt with, and the author then proceeded to point out the important part the action of iron disulphide, pyrites, or coal-brasses played in promoting spontaneous ignition. The remedy Prof. Lewes advises for the evils of spontaneous ignition are: firstly, non-ventilation of holds, so that oxygen may not be admitted to carry on the chemical processes by which heat is generated; secondly, by placing thermometers, suitably protected, in the mass of coal, so that, by electrical communication, warning may be given when the temperature rises to a dangerous point; and, thirdly, by placing flasks of liquid carbonic anhydride in the coals, the flasks to be sealed by an alloy with a low melting-point. This would be fused when the dangerous temperature was reached, and the carbonic acid, in expanding to its gaseous state, would cool the mass of coal to a safe temperature.

At the last sitting of the meeting, Mr. Corbett's paper on lifeboat models raised a lively controversy. The Royal National Lifeboat Institution had brought Mr. G. L. Watson all the way from Glasgow to meet the bold innovator who proposed to abolish their cherished self-righting boats. Of course, who is right remained an open question, as it always does when the properties of lifeboats are concerned.

Mr. Howden's paper on the screw propeller was of great length, containing no less than twenty-four pages without the appendix. Mr. Howden, like many other people, has a theory of his own on the screw propeller, which is opposed to that of all other authorities on the subject; for he believes that Rankine, Froude, Cotterill, and others, have based their conclusions on erroneous premises. It will be evident that we cannot enter into this vast subject at the end of a notice such as this, but we may briefly record our opinion that the older authorities were right.

On the whole, the meeting passed off very well. The attendance was good, and Mr. Holmes, the secretary, had made his arrangements so that the business proceeded without a hitch, as, indeed, is invariably the case at this well-managed institution.

BOURDON'S PRESSURE GAUGE.

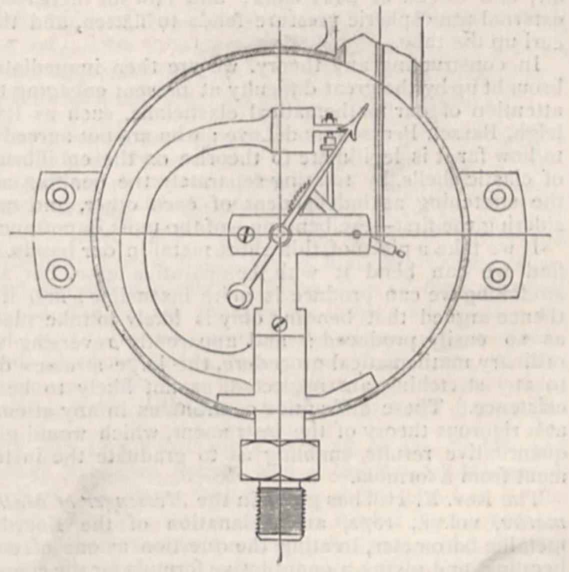
MR. WORTHINGTON'S letter to NATURE, January 30 (p. 296), on the theory of this instrument, has excited some criticism and disagreement of opinion; so it is proposed to examine here how far it is possible to construct a theory which shall be quantitative, in addition to giving a general explanation of the action.

The instrument is in very extensive use, hardly a steam-boiler being in existence which is not provided with one; and the simplicity and strength of the construction are such that it does not easily get out of repair, while it can be made to register either the highest pressure of the hydraulic press, or to record in the form of a barometer the minute fluctuations of atmospheric pressure.

The principle of the instrument was discovered by accident, and the account of this had best be given in the inventor's own words, taken from the paper read by him before the Institution of Civil Engineers, printed in the Proceedings I.C.E., vol. xi., p. 14, 1851:—

"The author had occasion to construct a worm-pipe for a still, by bending a cylindrical tube into a spiral or helical form. The workman performed the operation awkwardly, and partially flattened a considerable portion of the tube. In order to restore its form, one end was closed and the other was connected with a force-pump, by which water was forced into the tube; as the flattened portion of the tube resumed its cylindrical form, it was observed that the spiral uncoiled itself to a certain extent, and it was immediately perceived that this action might be applied to the construction of a pressure gauge."

To construct, then, a Bourdon gauge to register high pressures (*vide* figure, representing a gauge fitted to an indicator, not shown) a steel tube bored out of the solid bar to the requisite thickness for strength is taken, and purposely flattened, and then bent round into the arc of a circle so that the longer axis of a cross-section stands at right angles to the plane of the circle: one end of the



tube is screwed to a pipe which communicates with the liquid whose pressure is to be measured, while the other end is closed and joined by levers and racks to a shaft and a pointer, which traverses a dial on a box in which the curved tube is enclosed.

As the pressure in the tube is increased, the circular axis uncoils into a larger circle of smaller curvature, and the corresponding indications of the pointer on the dial are marked; and thus the instrument is graduated empirically by reference to some standard pressure gauge. As the pressure is again diminished, the elasticity of the tube brings it back to its original form, and the pointer retraverses the dial.

Lord Rayleigh gives an elementary explanation of the action of Bourdon's gauge in the Proc. Royal Society, No. 274, December 13, 1888; treating the movement of the walls of the tube as one of pure bending, he says:—

"In this instrument there is a tube whose axis lies along an arc of a circle and whose section is elliptical, the longer axis of the ellipse being perpendicular to the general plane of the tube. If we now consider the curvature at points which lie upon the axial section, we learn from Gauss's theorem (that in the bending without stretching of an inextensible surface, the product of the principal radii of curvature of the surface at any point remains constant) that a diminished curvature along the axis will be accompanied by a nearer approach to a circular section, and reciprocally. Since a circular form has the largest area for a given perimeter, internal pressure tends to diminish the eccentricity of the elliptic section, and with it the general curvature of the tube. Thus, if one end be fixed, a pointer connected with the free end may be made to indicate the internal pressure." Lord Rayleigh adds, "It appears, however, that the bending of a curved tube of elliptical action cannot be pure (*i.e.* unaccompanied by stretching), since the parts of the walls which lie furthest from the circular axis are necessarily stretched. The difficulty thus arising may be obviated by replacing the two halves of the ellipse, which lie on either side of the major axis, by two symmetrical curves which meet on the major axis at a *finite angle*."

In fact some Bourdon gauges, notably those required for low pressures only, and requiring great sensibility but not much strength, are constructed in this manner, and the difficulty of manufacture is thereby considerably reduced. Barometers are constructed in this way, and give good results; the tube is partially exhausted of air, and closed at both ends; and now an increase of external atmospheric pressure tends to flatten, and thus curl up the tube.

In constructing any theory, we are then immediately brought up by the great difficulty at present engaging the attention of our mathematical elasticians, such as Rayleigh, Basset, Pearson, and Love; who are not agreed as to how far it is legitimate to theorize on the equilibrium of elastic shells, by treating separately the bending and the stretching as independent of each other, and considering the first—the bending—of the most importance.

If we take a piece of thin sheet metal in our hands, we find we can bend it with comparative ease, but any stretching we can produce is quite insensible; and it is thence argued that bending only is likely to take place, as so easily produced; and apparently reversing the ordinary mathematical procedure, the large stresses due to any stretching are neglected, as not likely to be in existence. These difficulties confront us in any attempt at a rigorous theory of the instrument, which would give quantitative results, enabling us to graduate the instrument from a formula.

The Rev. E. Hill has given in the *Messenger of Mathematics*, vol. i., 1872, an explanation of the Bourdon metallic barometer, treating the question as one of pure bending, and giving a quantitative formula for the change of curvature a of the total curvature θ in terms of the change x in the semi-minor axis b, viz. $a/\theta = x/b$. But the determination of x/b for a given change of pressure is as yet an intractable mathematical problem, even for the simplification of supposing the tube a straight elliptic cylinder.

When we attempt to determine mathematically the pure bending produced in an elliptic cylinder by an increase of internal pressure and consequent tendency of the cross-section to the circular form, we are baffled by the analytical difficulties of determining the change in the length of the axes of the section, subject to the condition of keeping the perimeter unchanged in length, this length being expressed by a complete elliptic integral of the second kind, of which the modulus is the eccentricity of the ellipse. This problem was mentioned by Sir W. Thomson at the British Association in 1888; but we have not yet seen any development of it published by him.

Mr. Worthington, on the other hand, treats the question from the point of view of pure stretching; and now, with rectangular cross-section of the tube, as he supposes, a thrust in the inner wall due to the internal pressure will cause this wall to contract, while the pull in the outer wall will cause this wall to elongate; and thus an increase of internal pressure would cause the tube to curl up, the opposite effect to what happens when the bending effect due to the outward bulging of the flat walls is considered the leading phenomenon.

Even with a circular cross-section the stretching hypothesis would prove that the tube curls up under internal pressure; but this effect would be so small as to be imperceptible, because of the enormously greater stresses required for stretching than for bending in a thin tube; and this is found to be practically the case, inasmuch as the circular cross-section of the tube destroys all indications; and further, that the indications of the tube are reversed in direction when the axes of the elliptical cross-section are interchanged so that the minor axis is perpendicular to the plane of the circular axis of the tube.

The action of Bourdon's gauge is a differential effect; the bending of the surface changes the curvature one way, and the stretching produced by the same pressure the other way; but the bending effect is so much greater than that of stretching, that the latter may be left out of account.

In Gunnery we have, in a similar manner, two antagonistic causes producing a tendency for an elongated rifled projectile to deviate from a vertical plane of motion. If fired from a gun rifled with a right-handed screw, the vortex set up in the air by the spinning of the projectile causes differences of pressure, tending to deviate the projectile to the left, and this effect is sometimes very noticeable with golf or tennis balls; but, in addition, the forces set up by the tendency of the projectile to fly with its axis in the tangent of the trajectory urge the projectile to the right, and these latter forces are found to preponderate in practice.

A mathematician might be tempted to apply to the problem of Bourdon's gauge the formulas on the equilibrium of elastic plates and their change of curvature, anticlastic and synclastic, which are given in Thomson and Tait's "Natural Philosophy" (§§ 711–720), but these formulas apply only to a plate originally plane; and, besides, the applied pressures of the liquid complicate the analysis of the question to an extent which has not yet been overcome by elasticians.

The final conclusion would thus appear to be, that any quantitative formula cannot be hoped for yet, for a long time; but that Lord Rayleigh's reasoning, quoted above, gives a clear and concise descriptive explanation of the action.

The analogous practical problem of the resistance of flues to collapse still stands in need of a rational theory, when the supporting influence of the ends or of collapse rings is taken into account. When this question has received satisfactory treatment at the hands of theorists, we may hope to pass on to the far more difficult quantitative theory of Bourdon's gauge.

A. G. GREENHILL.

NOTES.

THE half-yearly general meeting of the Scottish Meteorological Society was held in the hall of the Royal Scottish Society of Arts, Edinburgh, on Monday afternoon. The following papers were read:—Influenza and weather, with special reference to the recent epidemic, by Sir Arthur Mitchell and Dr. Buchan; the temperature of the high and low-level Observatories of Ben Nevis, by T. Omond, Superintendent; thunderstorms at the Ben Nevis Observatory, by R. C. Mossmann. In the last Report presented by the Council, reference was made to a proposed systematic observation of the numbers of dust-particles in the atmosphere with the instrument recently invented by Mr. John Aitken, and an opinion was expressed that, for many reasons, Ben Nevis Observatory was the place where such observations could be most satisfactorily conducted. From the Report presented on Monday, we learn that a grant of £50 has been obtained from the Government Research Fund for commencing this novel and important investigation. Two instruments, constructed by Mr. White, of Glasgow, under the direction of Mr. Aitken, have been obtained—one to be placed permanently within the Observatory itself, and the other, a portable instrument, for outdoor observation. Both instruments are now at the Observatory, and the regular work of observation has begun. The Report also states that the delay in completing the buildings of the low-level Observatory at Fort William turned out to be more serious than was contemplated. This has arisen from various causes, chiefly from the great drought in the West Highlands last summer rendering it necessary that the ships conveying the stones for the building from Elgin be sent round the north and west coast instead of through the Caledonian Canal, which for the time was closed for through traffic; and also from the wet, broken weather of the past winter. In about three weeks the Observatory will be completed, and immediately thereafter the Meteorological Council will erect the self-registering instruments which were originally at Armagh, and otherwise supply a complete outfit of instruments for a first-class Meteorological Observatory. An additional observer has been engaged, and the staff of the two Observatories now consists of Mr. Omond, superintendent, and three assistants. By arrangement with the Post Office, direct communication will be opened between the two Observatories. The regular work of recording the continuous observations will be begun in May. The Directors of the Ben Nevis Observatory will thus soon be in a position to put scientific men in possession of two sets of hourly observations of the completest description, one at the top and the other at the foot of the mountain. With these observations, the changes of the conditions of the weather may be followed hour by hour; particularly those great changes, so vital and essential to the advancement of our knowledge of storms, which take place in the lowermost stratum of the atmosphere between the two Observatories. It is within this aërial stratum, of a vertical height of 4406 feet, that the gradual development of many weather changes from hour to hour may be satisfactorily investigated.

THE Chemical Society held its first anniversary dinner at the Hôtel Métropole on Thursday evening last. Among those present were the Presidents of the Royal Society, the Institute of Civil Engineers, the Society of Chemical Industry, the Institute of Chemistry, the Pharmaceutical and the Physical Societies, Sir F. Abel, Sir Henry Roscoe, Sir F. Bramwell, Mr. Thiselton-Dyer, Prof. J. Dewar, Dr. J. H. Gladstone, and Mr. W. Crookes. Dr. W. J. Russell, the President, in proposing posperity to the Chemical Society, sketched briefly the history of its rise and development. Sir Frederick Abel gave the toast of "Kindred Societies and Institutions," referring to the far-reaching character of the science of chemistry. There was not, he said, a single society or institution which was not dependent upon chemists for, at any rate, some amount of the usefulness which it exercised. The Royal Society was the great parent of them all; and the Royal Institution demanded special homage on account of the splendid discoveries made under its auspices, so many of which were specially interesting to chemists. Sir G. Stokes, in response, said that though specialism had been gaining ground very widely of late years, and though each branch of science had its own particular exponents enrolled in their own association, yet the old society, with which he had the honour to be closely connected, was not altogether effete. He thought that chemistry had as much need of cognate societies as any other branch of scientific research. Sir Lowthian Bell also replied. Prof. M. Foster, secretary to the Royal Society, proposed "The Visitors," and the toast was responded to by Sir F. Bramwell and by Mr. Thiselton-Dyer. The health of the chairman was proposed by Sir H. Roscoe.

ON Friday evening last the learned societies of Newcastle held their second annual gathering at the Durham College of Science. Among the societies represented were the following: the Durham College of Science, Engineering Students' Club, Foremen Engineers and Draughtsmen, Geographical Society, Institute of Mining and Mechanical Engineers, Literary and Philosophical Society, Medical Society, Microscopical Society, Natural History Society, N.E.C. Institution of Engineers and Shipbuilders, Pharmaceutical Association, Photographic Association, Society of Antiquaries, and Society of Chemical Industry. The *Newcastle Daily Journal* says that the professors of the Durham College of Science "worked hard for the success of the gathering," and that "the exhibits which they explained in the chemical, physical, geographical, botanical, and other departments in the building, afforded a vast amount of pleasure."

BY permission of the trustees of the British Museum, the *conversazione* of the Society of Arts will be held this year at the Natural History Museum, South Kensington.

MR. WRAGGE, Government Meteorologist, Queensland, has been dangerously ill with fever caught some time since in his tours of inspection. He has now gone to the Darling Downs to recruit his heath, which has been seriously undermined.

THE following lectures on scientific subjects will probably be delivered at the Friday evening meetings at the Royal Institution after Easter:—Friday, April 18, Sir Frederick Bramwell, F.R.S., welding by electricity; Friday, April 25, Sir John Lubbock, Bart., M.P., F.R.S., the shapes of leaves and cotyledons; Friday, May 9, Mr. R. Brudenell Carter, colour-vision and colour-blindness; Friday, May 16, Prof. Raphael Meldola, F.R.S., the photographic image; Friday, May 23, Prof. A. C. Haddon, manners and customs of the Torres Straits islanders; Friday, May 30, A. A. Common, F.R.S., astronomical telescopes; Friday, June 6, Prof. W. Boyd Dawkins, F.R.S., the search for coal in the South of England.

AT the twenty-first annual meeting of the Norfolk and Norwich Naturalists' Society, held at the Norwich Museum on March 25, Mr. Henry Seebohm was elected president for the ensuing year. The treasurer's report showed that the financial condition of the Society was very satisfactory, and that during the past year there had been an increase of several members. The retiring president, Dr. Taylor, after briefly reviewing the work of the Society during the past year, delivered an address on "Microbes."

THE London Geological Field Class, under the direction of Prof. H. G. Seeley, F.R.S., has made arrangements for a number of excursions, in which many students might find it pleasant and profitable to take part. One set of excursions is specially arranged for the practical study of geography. Others are planned for the illustration of the geological structure of the London district.

A VIOLENT earthquake shock was felt at Trieste on March 26 at 20 minutes past 9 p.m.

AT the last meeting of the Scientific Committee of the Royal Horticultural Society, Mr. Morris alluded to the peculiar vegetation of St. Helena, now confined, for the most part, to a small area in the central and higher part of the island. Many of the trees formerly native to the island are now all but, or quite, extinct. Among them is a species of Trochetia, or Melhania. The trunks of this tree are embedded in the cliffs of the island, and are dug out by the inhabitants for the sake of manufacturing ornaments. The following quotation from Melliss's exhaustive work on St. Helena refers to this plant:—"The Native Ebony of St. Helena.—This plant I believe to be now extinct. It formerly grew on the outer portions of the island, near the coast, at altitudes of 2 to 4, where the weather-beaten stems are still found deeply embedded in the surface-soil. The last plant I saw was a small one growing in the garden at Oakbank, about twenty-five years ago, but it is not there now, and I have searched the whole island over for another, but in vain. The leaves were dark green, and the flowers white; the wood is very hard, heavy, black in colour, and extremely brittle. It is still collected and turned into ornaments, which are much prized on account of its rarity. That this tree once formed a considerable portion of the vegetation clothing the island on those parts that are now quite barren, is strongly evidenced by the many references to it in the local records. Pl. 29. It is the *Dombeya erythroxylon* of Andr., *Bot. Repos.*, vi., t. 389, not of Willdenow." It is interesting to know that the plant is still in existence under cultivation at Kew (and perhaps elsewhere), under the name of *Dombeya erythroxylon.* At the present time the plant, which was obtained from the gardens at Herrhausen, is in flower at Kew. Mr. McLachlan called attention to the interesting remark on the rare plants of St. Helena, contained in Mr. Wollaston's book on the Coleoptera of the Atlantic islands.

CAPT. DELPORTE, Professor of Topography, Astronomy, and Geodesy, at the Military School of Brussels, has just started for the River Congo, for the purpose of making geodetic researches.

THE Geographical Society of Berlin has presented the sum of 1000 marks (£50), to Dr. Hettner for a journey of research in the southern provinces of Brazil.

SOME prehistoric German tombs were recently excavated on the road leading from Apolda to Jena. About 20 skeletons were found (two being without skulls), and a number of ornaments and weapons.

IN the course of some excavations lately made at Ludwigshafen, on the Rhine, the tibia and two teeth of a mammoth, and the jaw of a stag, were found. The skeleton of another "antediluvian" animal was discovered in the limestone near Oberhildesheim. The researches are being continued.

THE *Zoologist* for 1884 announced a proposed supplement to Thompson's "Natural History of Ireland," and contributions of information were invited from persons interested in the subject. A considerable amount of fresh material has been accumulated, but as it relates chiefly to birds, it is now intended that the supplement shall deal only with ornithology. The new work will be published by Messrs. Gurney and Jackson, and an appeal for additional facts has been issued to students who may be able and willing to supply notes. Anyone who is in a position to respond to this appeal is requested to communicate with Mr. R. J. Usher, Cappah, Lismore, Ireland.

MR. ELLIOT STOCK has issued the seventh edition of "Days and Hours in a Garden," by E. V. B. The volume is prettily printed and bound, and lovers of the country will find much to interest them in the writer's bright and pleasant descriptions.

THE Royal University of Ireland has issued its Calendar for the year 1890, and a supplement consisting of the examination papers of 1889.

THE first edition of the life of the Rev. J. G. Wood, by his son, the Rev. Theodore Wood, has been already exhausted; and a second edition is about to be issued.

A FACT noted by Mr. T. H. Hall in the new number of the *Entomologist's Monthly Magazine* indicates the extraordinary variety of conditions in which beetles may thrive. The men employed in breaking up an old disused gasometer at Home Park Mills, King's Langley, spoke to him of some "very curious beetles," which were living in the rusty water at the bottom of the hole left when the iron casing had been removed. Both the water and mud were strongly impregnated with gas. The beetles proved to be of the *D. marginalis* species, and were there in some numbers. Many were carried away when the water was pumped off, but Mr. Hall secured specimens from the mud and shallow water left. He says:—"They carry with them a strong odour of gas, even after two or thr... esh-water baths, and the grooves in the elytra of the females are filled with a ferruginous mud which is difficult to remove. In other respects they appear to be quite normal in form and colour. I think this old gas-holder must have been their home for a long period of beetle life, judging from the time of year when they were found, a fortnight ago, and from the number of *both* sexes seen. The water was partly enclosed and quite stagnant, being unconnected with any other water. Were they there by choice? If not, why did they not emigrate? Most likely they came there by chance, as they are plentiful in the canal not far away, and lacking the inclination to depart, 'made themselves at home.' Had the water been disagreeable to them, we may presume they would not have done so; they were quite active when disturbed."

ACCORDING to a French journal, the number of foreign students now studying in Paris is about 1000, of whom 729 (107 of them women) are studying medicine, and 182 law. Literature has 66 (including 9 women), science 60, and pharmacy 23. It is remarkable that Russia furnishes the largest contingent of the foreign medical students, viz. 150, America coming next with 139. We find no mention of England. The foreign element is, on the above estimate, about one-tenth of the whole.

THE Punjab Forest Administration Report for 1888–89 was recently published. During the year, nine thousand acres were added to the area of gazetted forests in the Multan district. This area was taken up in pursuance of the policy of establishing irrigated plantations in connection with several new canals constructed in what are known as the "Bar" tracts—that is, the dry upland deserts of the Punjab. The number of forest fires increased during the year, and 17,617 acres were burnt as against 10,324 during 1888. The financial results are satisfactory. The net revenue amounted to Rs. 4,52,846, or nearly half a lakh in excess of the net revenue of the preceding year. The Conservator complains that the Working Plans Branch cannot get on with their work on account of the undermanning of the Department. As a consequence, working plans are only in force over 364 square miles, out of a total of two thousand square miles gazetted and six thousand controlled by the Forest Department. Experiments with exotics were made, but the result was not encouraging. European fruit-trees have been introduced in many places with great success.

THE first Report published by the Marine Fisheries Society of Great Grimsby is a modest record of work done and investigations decided on by an institution which, by employing scientific methods, will probably amass information of great value to the biologist, and improve our fisheries in their commercial aspects. The Society was incorporated in June 1888. It has already established an aquarium and hatchery which is 37 feet by 21 feet, and a small museum and library. The building has a frontage of 50 feet, and is situated at Cleethorpes, facing the Promenade, two miles distant from Grimsby. The tanks are set on concrete walls; they were purchased from the National Fish-Culture Association, and originally formed the aquarium at the Fisheries Exhibition at South Kensington. They form a reservoir storing 4000 gallons of sea-water, from which the water is pumped into a wooden tank 10 feet above the hatchery, holding 1200 gallons. Thus a constant circulation of the water in the tanks is maintained. The water is pumped from the sea at high water, and left to settle some days in a storage reservoir before use; each hatching tank has room for twelve wooden trays, measuring 16 inches by 10 inches, by 9 inches in depth, with a canvas strainer at the bottom to prevent the eggs escaping. The Society aims at recording observations respecting marine life, and the improvement of the fisheries of the United Kingdom, by the artificial propagation of marine fishes and Crustacea, by the pursuit of scientific observations and investigations respecting the natural history, habitat, migration, spawning food, and the effect of weather, temperature, and conditions of the water, currents, tides, light, and darkness upon the fauna of the sea; by the protection of young fish, and the introduction of practical appliances for the capture of mature fish; by endeavouring to ascertain the best methods of transporting fish in a fresh condition, and economically preserving them. By admitting fishermen into the Society, at a nominal subscription, they hope to get numerous observers and collectors from amongst those who spend their life reaping the harvest of the sea.

AT the last meeting of the Société Chimique de Paris a paper by M. Meslans was presented by M. Moissan, announcing the isolation of fluoroform, CHF_3, the fluorine analogue of chloroform, $CHCl_3$. A brief abstract of this preliminary communication will be found in the *Chemiker Zeitung* for March 26. During the course of the work recently published concerning propyl and isopropyl fluorides, M. Meslans had occasion to study the action of silver fluoride upon iodoform. The result of this action was found to vary according to the conditions of experiment, liquid products being obtained under certain conditions, and gaseous products under others. The end result, however, was always the production of a gas, which turns out to be fluoroform. Chloroform, as is well known, is readily attacked by a warm alcoholic solution of potash, potassium chloride and potassium formate being produced: $CHCl_3 + 4KOH = H.COOK + 3KCl + 2H_2O$. It is interesting to learn that fluoroform behaves in precisely the same manner, for the gas is decomposed by either aqueous or alcoholic potash with formation of fluoride and formate of potassium. On being heated to redness in a glass tube fluoroform is also decomposed, with production of gaseous silicon tetrafluoride and a deposit of carbon. The gas is only very slightly absorbed by water, but it dissolves readily in chloroform or alcohol. Fluoroform has also been prepared by substituting chloroform or bromoform for the iodoform used in the first experiments.

AT the same meeting M. Chabrié reported that he also had obtained a gas by heating silver fluoride with chloroform in a sealed tube, which yielded potassium formate with potash, and was evidently identical with the fluoroform described by M. Meslans. The density of the gas was determined, and found to be 2·414. Fluoroform possesses the density 2·43, so there can be no doubt as to the identity of the gas. Although so readily attacked by warm potash, it was found that a cold alcoholic solution of potash was almost incapable of acting upon it.

M. MOISSAN also presented another interesting paper in the names of MM. Guenez and Meslans, describing the isolation of fluoral, $CF_3.CHO$, the analogue of chloral, $CCl_3.CHO$, the tri-chlor derivative of common aldehyde, $CH_3.CHO$, and the hydrate of which has recently become so famous as a drug. Fluoral, like fluoroform, is a gas, and has been obtained by heating silver fluoride with anhydrous chloral. The gas dissolves to only a very slight extent in water, but is absorbed by aqueous or alcoholic potash with formation of formate and fluoride of potassium, thus again resembling its chlorine analogue. To complete the proof of its identity, the density of the gas was determined and found to agree very closely with the calculated density of anhydrous fluoral.

THE additions to the Zoological Society's Gardens during the past week include two Ring-necked Pheasants (*Phasianus torquatus* ♂ ♀), British, presented by H.R.H. the Prince of Wales, K.G.; a Chacma Baboon (*Cynocephalus porcarius* ♀) from South Africa, two Indian Pythons (*Python molurus*) from India, five Common Boas (*Boa constrictor*) from South America deposited; three Red-footed Ground Squirrels (*Xerus erythropus*) from West Africa, two Himalayan Monauls (*Lophophorus impeyanus* ♀ ♀) from the Himalayas, two Diuca Finches (*Diuca grisea*), a Black-chinned Siskin (*Chrysomitris barbata*) two Field Saffron Finches (*Sycalis arvensis*), an Alaudine Finch (*Phrygilus alaudinus*) from Chili, purchased; a Hog Deer (*Cervus porcinus* ♂), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on April 3 = 10h. 48m. 43s.

Name.	Mag.	Colour.	R A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 2343	—	Greenish.	11 8 10	+55 36
(2) 44 Leonis	6	Yellowish-red	10 19 27	+ 9 20
(3) 58 Leonis	4·5	Whitish yellow.	10 54 54	+ 4 13
(4) θ Leonis...	3	White.	11 8 30	+16 2
(5) 145 Schj.	8	Red.	12 19 36	+ 1 23
(6) S Coronæ	Var.	Reddish-yellow.	15 16 55	+31 46

Remarks.

(1) This is the well-known nebula 97 M, near β Ursæ Majoris. In the General Catalogue it is described as "a planetary nebula, very bright, very large, round; at first very gradually, then very suddenly brighter in the middle to a planetary disk; 19·0s. in diameter." Lord Rosse's drawing of the nebula indicates a very complex structure. I examined the nebula recently with Prof. Lockyer's 30-inch reflector at Westgate-on-Sea, but was unable to see all the details shown in Lord Rosse's drawing. The nebula appeared to be a large disk, ill-defined at the edges, and equally illuminated, with the exception of two darker disks situated diametrically opposite to each other, each being about half a radius in diameter. Dr. Huggins observed the spectrum in 1866, and found it to consist of bright lines. The two lines near λλ 500 and 495, and possibly a little continuous spectrum were recorded. On the occasion above referred to I saw the three usual nebula lines and the hydrogen line at G, but was unable to continue the observations on account of clouds. In further observations, additional lines ought to be looked for, and the character of the chief line near λ 500 particularly noted, as in the case of the nebula G.C. 2102, given last week.

(2) A star of Group II. Dunér states that the bands 2-8 are well seen, but that they are not strongly marked. It is important to secure further observations of stars like this, as there may very well be other differences besides the weakening of the bands as compared with those in which the banded spectrum is more fully developed.

(3) This has a fine spectrum of the solar type (Vogel). The usual differential observations are required.

(4) The spectrum of this star is a typical one of Group IV. (Vogel). The hydrogen lines are probably therefore very thick, and the metallic lines very thin, if visible at all. The thicker the hydrogen line the hotter the star, and the higher therefore its place on the "temperature curve."

(5) Vogel and Dunér agree in describing the spectrum of this star as a very fine one of Group VI. The three carbon bands are stated to be visible, but the intensity of the band near λ 564 relatively to the others is not given. This point should therefore receive attention. The secondary bands 4 and 5, and possibly 2 and 3 are visible. It is interesting to note that this star shows considerably more detail than several brighter ones of the same group.

(6) This variable will reach a maximum about April 9. Its period is about 360 days, and the magnitudes at maximum and minimum are 6·1–7·8 and 11·9–12·5 respectively (Gore). The spectrum is a very fine one of Group II., and the great range of variation makes it extremely probably that bright lines will appear at maximum or soon after, as already observed by Mr. Espin in variables with similar spectra. Variations in the intensities of the bright carbon flutings should also be noted.

A. FOWLER.

THE GREAT COMET OF 1882.—The *Bulletin Astronomique* for February 1890 reproduces with some additions a paper presented by M. F. Tisserand to the Academy of Sciences on February 3. It will be remembered that the segmentation of the nucleus of this comet was observed on September 30, 1882—that is, thirteen days after perihelion passage, and that Mr. Common in January 1883 saw five nuclei in a line. From an elaborate investigation into the conditions necessary for the development of these secondary nuclei, M. Tisserand concludes that the cause existed in the comet itself, and was not the result of external influence. The minimum relative variation required for the disaggregation of the nucleus is $\frac{1}{300000}$ of the perihelion velocity. And it is suggested that this variation may be produced by interior actions, collisions, mutual attractions, or explosions, because of an excessive increase of temperature or the rotation of the head.

MELBOURNE STAR CATALOGUE.—In 1874 the First Melbourne General Catalogue of 1227 stars for the epoch 1870 was issued. The Second General Catalogue has just been received, and contains 1211 stars for the epoch 1880, deduced from observations made at the Melbourne Observatory under the direction of Mr. Ellery from 1871·0 to 1884·7. The separate results and the details of the observations from which this Catalogue has been compiled are contained in vols. v., vi., and vii., of the *Melbourne Observations*, and in the present Catalogue explanations are given of the processes used in forming the stars' places and the corrections applied. The whole of the observations were reduced and prepared for publication by Mr. E. J. White, the First Assistant Astronomer.

COMET *a* 1890.—The first comet of this year was discovered just before sunrise on March 19 by Mr. Brooks, of Geneva, U.S. Its exact place was found to be—

Cambridge Mean Time.		R.A.		Decl.
	h. m.	h. m. s.		° ′ ″
21 March ...	16 57·5 ...	21 9 34·07 ...		6 25 30 N.

The daily movement in right ascension is + 16s., and in declination + 25′.

DISCOVERY OF ASTEROIDS.—On March 20, Dr. Palisa, at Vienna, discovered another minor planet, and the telegram announcing his discovery was received at the *Astronomische Nachrichten* office at midday on March 21. This comet is of interest, for, from its rapid movement, viz. – 25′ in R.A. and + 10′ in N.P.D., it appears to be near to the earth.

M. Charlois, of Nice Observatory, discovered a minor planet on March 10, and re-observed it on March 20. This brings the number of asteroids up to 290.

The asteroid (285) discovered by Prof. Luther on February 24 has received the name of Glauke.

SOLAR ACTIVITY IN 1889.—The record of the past year as to solar phenomena presents several noteworthy features. (1) The number of days on which the sun appeared to be free from either spots or faculæ; the days without spots being 211 as compared with 158 in 1888; and the days when neither spots nor faculæ were seen being more than twice as numerous last year as in the year previous. (2) The distinct but temporary revival of spot activity during the months of June, July, August, and September. (3) The appearance of spots in high latitudes; and lastly, the remarkable falling off in chromospheric phenomena, particularly during the last months of the year. It is, therefore, still difficult to be certain whether we have yet reached the actual minimum or no; the revival of the spots during last summer, connected as it was with so remarkable an increase in their mean distance from the equator, seemed to point to the minimum having been passed; but the season of almost perfect quiet which followed it, together with the decrease in the number and size of the prominences, favour the opposite conclusion. The mean daily spotted area for 1889 was less than that for 1888, but only by about one-seventh.

The three most remarkable groups of 1889 were those first seen on June 16, June 29, and August 2 respectively. The first-named was the largest group of the year; it formed and disappeared on the further side of the sun, and was seen during three rotations. The third was also seen during three rotations, but formed and died out in the visible hemisphere. It was the second group as to dimensions, and lay in S. lat. 20°, whilst the spot of June 16 was in S. lat. 6°. The spot of June 29 was only a very small one, and lasted but a couple of days, but was noticeable from its high latitude, 40° S. A fourth group, that first seen on August 9, though not attaining so large a mean area as the spot of June 16, exceeded it on one particular day, August 15.

The following table gives the monthly numbers for spots and faculæ as supplied by Prof. Tacchini in the *Comptes rendus*, vol. cviii. No. 21, vol. cix. No. 4, and vol. cx. No. 5, and may be compared with those given in NATURE for 1889 March 7, and in previous volumes:—

1889.	Proportion of days without spots.	Sun-spots. Relative frequence.	Sun-spots. Relative size.	Sun-spots. Mean daily number of groups.	Faculæ. Relative size.
January ...	1·00 ...	0·00 ...	0·00 ...	0·00 ...	6·00
February ...	0·50 ...	3·26 ...	8·12 ...	0·56 ...	1·56
March ...	0·62 ...	1·69 ...	3·64 ...	0·50 ...	6·81
April ...	0·60 ...	0·65 ...	4·35 ...	0·40 ...	7·25
May ...	0·96 ...	0·04 ...	0·65 ...	0·04 ...	5·30
June ...	0·56 ...	1·97 ...	25·22 ...	0·45 ...	9·63
July ...	0·39 ...	2·75 ...	16·97 ...	0·87 ...	14·35
August ...	0·19 ...	6·97 ...	20·03 ...	1·26 ...	17·77
September ...	0·48 ...	1·18 ...	8·22 ...	0·61 ...	28·48
October ...	0·73 ...	0·64 ...	1·55 ...	0·27 ...	18·18
November ...	1·00 ...	0·00 ...	0·00 ...	0·00 ...	0·62
December ...	0·61 ...	1·68 ...	4·09 ...	0·65 ...	29·55

The table shows that as in 1888 the faculæ did not vary quite in accordance with the spots, September and December being heavy months for the former, their relative area then exceeding that for any month since July 1886. The prominences on the other hand showed a very marked falling off towards the end of the year; February and March, light months for spots and faculæ, being much the most prolific as to the flames. The following are the mean numbers for the prominences resulting from Prof. Tacchini's monthly reports. It must be borne in mind that the difference in the atmospheric conditions of England and Italy renders it impossible to compare Prof. Tacchini's results with those formerly given by the late Rev. S. J. Perry, and which have been incorporated in former annual summaries in NATURE.

	Prominences. Days of observation.	Prominences. Mean daily number.	Prominences. Mean height. ″	Prominences. Mean extent.
1887 ...	214 ...	8·26 ...	45·2 ...	1·7
1888 ...	227 ...	7·94 ...	45·9 ...	1·5
1889 ...	247 ...	3·20 ...	34·7 ...	1·1

The variations in the magnetic elements accorded in their more general features, though not in details, with those of the sunspots, as the following table given by Dr. R. Wolf in the *Comptes rendus*, vol. cx. No. 3, sufficiently shows:—

1889.	Wolf's relative numbers (Zurich). r	Wolf's relative numbers (Zurich). Δr	Variation in magnetic declination (Milan). r	Variation in magnetic declination (Milan). Δr
January ...	1·0 ...	– 12·0 ...	1·75 ...	– 1·28
February ...	7·9 ...	+ 0·9 ...	3·99 ...	+ 0·97
March ...	6·3 ...	+ 0·0 ...	6·17 ...	– 0·94
April ...	4·9 ...	+ 1·0 ...	8·85 ...	+ 0·58
May	2·4 ...	– 8·4 ...	8·19 ...	– 0·29
June	7·0 ...	+ 0·5 ...	8·86 ...	– 0·41
July	8·0 ...	+ 6·1 ...	8·25 ...	– 0·32
August ...	20·6 ...	+ 18·7 ...	8·99 ...	– 0·18
September	6·3 ...	– 1·5 ...	6·84 ...	– 0·47
October ...	0·0 ...	– 2·0 ...	6·10 ...	– 0·22
November	0·0 ...	– 12·9 ...	2·55 ...	+ 0·37
December	5·7 ...	– 4·2 ...	1·96 ...	+ 0·20
Mean ...	5·8 ...	– 0·9 ...	6·04 ...	– 0·17

Dr. Wolf's formula for Milan, $v = 5'·62 + 0·045\ r$, with $r = 5·8$, would give $v = 5'·88$, a much closer accord than for the two preceding years.

THE GLOW OF PHOSPHORUS.[1]

THE word *phosphorus*, originally applied to any substance, solid or liquid, which had the property of shining in the dark, has gradually lost its generic sense, and is nowadays practically restricted, as a designation, to the wax-like inflammable substance which plays such an important part in the composition of an ordinary lucifer match. Phosphorus, indeed, is one of the most remarkable of the many remarkable substances known to the chemist. The curious method of its discovery, the universality of its distribution, its intimate connection with the phenomena of animal and vegetable life, its extraordinary physical properties and chemical activity, its abnormal molecular constitution, the Protean ease of its allotropic transformations—all combine to make up a history which abundantly justifies its old appellation of *phosphorus mirabilis*. Godfrey Hankewitz more than 150 years ago wrote: "This phosphorus is a subject that occupies much the thoughts and fancies of some alchymists who work on microcosmical substances, and out of it they promise themselves golden mountains." Certainly no man of his time made more in the way of gold out of phosphorus than Mr. Hankewitz, for at his little shop in the Strand he enjoyed for many years the monopoly of its sale, guarding his *Arcana* with all the jealousy of a modern manufacturer of the element.

Phosphorus, or, as it was then called, the *noctiluca*, was first seen in this country in 1677. It was shown to Robert Boyle, who had already worked on phosphorescence in general, and who seems to have been specially struck with the remarkable peculiarity of a factitious body which could be made "to shine in the dark without having been befo e illumined by any lucid substance and without being hot as to sense." In these respects the substance differed from all the *phosphori* hitherto known. The conditions which determine its glow were the subject of the earliest observations on phosphorus, and Boyle has left us a minute account of his work on the point. In the first place, he noticed that the substance was only luminous in presence of air. He accurately describes the nature of the light, and noticed that the water in which the phosphorus was partially immersed acquired "a strong and penetrant taste, . . . and relished a little like vitriol." On evaporation it would not "shoot into crystals, . . . but coagulated into a substance like a Gelly, or the Whites of Eggs which would be easily melted by heat." On heating this "Gelly" it gave off "flashes of fire and light," and had a "garlick smell." He also found that the *noctiluca* was soluble in certain oils, and he particularly mentions oil of cloves as a convenient means of showing the luminosity, as it is "rendered more acceptable to the standers-by by its grateful smell." "In Oyl of Mace it did not appear luminous nor in Oyl of Aniseeds." Boyle describes a number of experiments showing how small a quantity of the phosphorus is required to produce a luminous effect. "A grain of the *noctiluca* dissolved in Alkohol of Wine and shaken in Water; it render'd 400,000 times its weight luminous throughout. And at another Tryal I found that it impregnated 500,000 times its weight; which was more than one part of Cochineel could communicate its colour to." "And one thing further observable was that when it had been a long time exposed to the air it emitted strong and odorous Exhalations distinct from the visible Fumes." The strong and odorous exhalations we now know to be ozone.

The earlier volumes of the Philosophical Transactions contain several papers on the luminosity of phosphorus, and one by Dr. Frederic Slare is noteworthy as giving one of the earliest, if not actually the earliest account of what is one of the most paradoxical phenomena connected with the luminosity of phosphorus, namely its increase on rarefying the air. "It being now generally agreed that the fire and flame [of phosphorus] have their pabulum out of the air, I was willing to try this matter *in vacuo*. To effect this, I placed a considerable lump of this matter (phosphorus) under a glass which I fixed to an engine for exhausting the air; then presently working the engine, I found it grow lighter [*i.e.* more luminous] though a charcoal that was well kindled would be quite extinguished at the first exhaustion; and upon the third or fourth draught which very well exhausted the glass, it much increased its light, and continued so to shine with its increased light for a long time; on re-admitting the air, it returns again to its former dulness." This observation was repeated and its result confirmed by Hawksbee in this country and by Homberg in France, and seems subsequently to have led Berzelius, and after him Marchand, to the conclusion that the luminosity of phosphorus was altogether independent of the air (*i.e.* the oxygen) but was solely due to the volatility of the body. Many facts, however, combine to show that the air (oxygen) is necessary to the phenomenon. Lampadius found that phosphorus would not glow in the Torricellian vacuum; and Lavoisier, in 1777, showed that it would not inflame under the same conditions; and the subsequent experiments of Schrötter, Meissner, and Müller are decisive on the point that the glow is the concomitant of a chemical process dependent upon the presence of oxygen. It is, however, remarkable that phosphorus will not glow in oxygen at the ordinary atmospheric pressure and temperature, but that if the oxygen be rarefied the glow at once begins, but ceases again immediately the oxygen is compressed. Indeed, phosphorus will not glow in compressed air, and the flame of feebly burning phosphorus may be extinguished by suddenly increasing the pressure of the gas. Phosphorus, however, can be made to glow in oxygen at the ordinary pressure or in compressed air if the gases be gently warmed. In the case of oxygen the glow begins at 25° and becomes very bright at 36°. In compressed air the temperature at which the glow is initiated depends upon the tension. If the oxygen be absolutely deprived of moisture the phosphorus refuses to glow under any conditions. This fact, strange as it may seem, is not without analogy; the presence of traces of moisture appears to be necessary for the initiation or continuance of chemical combination in a number of instances.

It was observed by Boyle that a minute quantity of the vapour of a number of essential oils extinguished the glow of phosphorus. The late Prof. Graham confirmed and extended these observations; he showed that relatively small quantities of olefiant gas and of the vapours of ether, naphtha, and oil of turpentine entirely prevented the glow; and subsequent observers have found that many essential oils, such as those of peppermint and lemon and the vapours of camphor and asafœtida, even when present in very small quantity, stop the absorption of oxygen and the slow combustion of phosphorus in air.

It has been established that whenever phosphorus glows in air or in rarefied oxygen, ozone and hydrogen peroxide are formed, but it is not definitely known whether the formation of these substances is the cause or the effect of the chemical process of which the glow is the visible sign. That there is some intimate connection between the luminosity of the phosphorus and the production of these bodies is highly probable. Schönbein, as far back as 1848, sought to demonstrate that the glow depends on the presence of ozone. It is certainly true that many of the substances, such as the essential oils, which prevent the glow of phosphorus, also destroy ozone. At a low temperature, phosphorus produces no ozone in contact with air, neither does it glow. It has been found, in fact, that, with air, ozone is produced in largest quantity at 25°, at which temperature phosphorus glows brightly. On the assumption that the oxidation of the phosphorus consists in the immediate formation of the highest oxide, the production of the ozone and the hydrogen peroxide has been represented by the following equations:—

$$P_2 + 3O_2 = P_2O_5 + O.$$
$$O + O_2 = O_3.$$
$$O + H_2O = H_2O_2.$$

Both these reactions may, of course, go on simultaneously; ozone and hydrogen peroxide are not mutually incompatible; the synthesis of hydrogen peroxide by the direct oxidation of water seems to occur in a number of processes. But such symbolic expressions can at most be only very partial representations of what actually occurs. It is highly probable that the combination which gives rise to the glow only occurs between the *vapour* of phosphorus and the oxygen. Phosphorus is sensibly volatile at ordinary temperatures, and by rarefying the atmosphere in which it is placed its volatilization is increased, which serves to account for the increased glow when the pressure of the gas is diminished. When phosphorus is placed in an atmosphere of hydrogen, nitrogen, or carbonic acid, these gases, when brought into contact with oxygen, become luminous from the oxidation of the vapour of phosphorus diffused through them. The rapidity of volatilization varies with the particular gas; it is greatest in the case of hydrogen, and least in that of carbonic acid. Indeed, a stream of hydrogen gas at ordinary temperatures carries away comparatively large quantities of phosphorus, which may be collected by appropriate solvents. No ozone and no glow is

[1] Lecture delivered on Friday evening, March 14, at the Royal Institution, by Prof. Thorpe, F.R.S.

produced in oxygen gas at ordinary temperatures and pressures, but on warming the oxygen, both the ozone and the glow are formed. On passing ozone into oxygen at temperatures at which phosphorus refuses to glow, the phosphorus at once becomes luminous, oxygen is absorbed, and the characteristic cloud of oxide is produced, and the effect continues so long as the supply of ozone is maintained. A drop of ether at once extinguishes the glow. The ether is in all probability converted into vinyl alcohol with simultaneous formation of hydrogen peroxide by the reaction indicated by Poleck and Thümmel:—

$$\left.\begin{matrix}CH_3CH_2\\CH_3CH_2\end{matrix}\right>O + O_3 = \begin{matrix}CH_2CHOH\\CH_2CHOH\end{matrix} + \left.\begin{matrix}HO\\HO\end{matrix}\right\}.$$

A. W. Wright has shown that formic, acetic, and oxalic acids are also formed by the action of ozonized oxygen on ether.

Phosphorus combines with oxygen in several proportions, and the study of the mode of formation and properties of these oxides is calculated to throw light upon the nature of the chemical process which attends the glow of phosphorus. Certain of these oxides have recently been the subject of a considerable amount of study in the chemical laboratories of the Normal School of Science. When phosphorus is slowly burned in air, there is produced a considerable quantity of a volatile substance, having a characteristic garlic-like smell, which solidifies, when cooled, in beautiful arborescent masses of white crystals. It melts at about 23°, and boils at 173°. In a sealed tube kept in the dark, it may be preserved unchanged, but on exposure to light, and especially to bright sunshine, it rapidly becomes deep red. It slowly absorbs oxygen at the ordinary temperature and pressure, but from the mode in which the solid product of the reaction (P_2O_5) is deposited, it is evident that the union only takes place between the *vapour* of the oxide and the oxygen gas. Under diminished pressure the act of combination is attended with a glow which increases in brilliancy if ozone be present. On compressing the oxygen, the glow ceases. No ozone is formed during the act of oxidation. The degree of rarefaction needed to initiate the glow depends upon the temperature of the oxide—the warmer the oxide the less is the diminution of pressure required. By gradually warming the oxide, the luminosity steadily increases both in area and intensity, until at a certain temperature the mass ignites. The change from glow to actual flame is perfectly regular and gradual, and is unattended with any sudden increase in brilliancy. In this respect the process of oxidation is analogous to the slow and barely visible burning of fire-damp which is sometimes seen to occur in the Davy lamp, or to the slow combustion of ether and other vapours, which has been specially studied by Dr. Perkin. Other instances of what may be called *degraded combustion* are known to chemists. Thrown into warm oxygen, the substance bursts into flame at once and burns brilliantly, and it also takes fire in contact with chlorine. Alcohol also ignites it, and when it is warmed with a solution of potash or with water it evolves spontaneously inflammable phosphoretted hydrogen. In contact with cold water it suffers only a very gradual change, and many days may elapse before even a comparatively small quantity is dissolved. This substance has long been known; it was discovered, in fact, by the French chemist Sage, but its true nature has only now been determined. Its chemical formula is found to be P_4O_6; hence its composition is similar to that of its chemical analogue, arsenious oxide.

The study of the properties of this remarkable substance enables us to gain a clearer insight into the nature of the chemical process attending the glow of phosphorus. When phosphorus is placed in oxygen, or in an atmosphere containing oxygen, under such conditions that it volatilizes, the phosphorus oxidizes, partly into phosphoric oxide and partly into phosphorous oxide. Ozone is formed, possibly by the reaction already indicated, and this reacts upon the residual phosphorus vapour and the phosphorous oxide with the production of the luminous effect to which the element owes its name. The glow itself is nothing but a slowly-burning flame having an extremely low temperature, caused by the chemical union of oxygen with the vapours of phosphorus and phosphorous oxide. By suitable means this glow can be gradually augmented, until it passes by regular gradation into the active vigorous combustion which we ordinarily associate with flame. Many substances, in fact, may be caused to phosphoresce in a similar way. Arsenic, when gently heated, glows in oxygen, and sulphur may also be observed to become luminous in that gas at a temperature of about 200°.

"*BEFORE AND AFTER DARWIN.*"

ON Tuesday, March 25, Prof. G. J. Romanes, F.R.S., concluded his course of between thirty and forty lectures, which, under the above title, he has been delivering at the Royal Institution during the last three years. At the close of the lecture he announced his intention of publishing the whole course in November next, and distributed among the audience printed slips, conveying in the form of twelve propositions the "general conclusions" to which his lectures for the present year have led. The following is a copy of this printed slip:—

(1) "Natural selection has been the main, but not the exclusive means of modification," both as regards species and all the higher taxonomic divisions.

(2) Of the other factors of organic evolution it is not improbable that we are still to a large extent ignorant. Whether, or to what extent, sexual selection and the Lamarckian principles have co-operated, is a matter with which I am not specially concerned; but I think there is abundant evidence to establish the high importance in this connection of amixia, or independent variability,—at all events as regards the evolution of species.

(3) Natural selection is primarily a theory of the cumulative development of adaptations wherever these occur, and therefore is only incidentally, or likewise, a theory of the origin of species in cases where allied species differ from one another in respect of peculiar characters, which are also adaptive characters.

(4) Hence it does not follow from the theory of natural selection that all species—much less all specific characters—must *necessarily* have owed their origin to natural selection, since it cannot be proved deductively from the theory that no "means of modification" other than natural selection is competent to produce such slight degrees of modification as go to constitute diagnostic distinctions between closely-allied species; while, on the other hand, there is an overwhelming mass of evidence to prove the origin of "a large proportional number of specific characters" in causes of modification other than natural selection.

(5) Even if it were true that all species and all specific characters must necessarily owe their origin to natural selection, it would still remain illogical to define the theory of natural selection as indifferently a theory of species or a theory of adaptations; for, even upon this erroneous supposition, specific characters and adaptive characters would remain very far indeed from being conterminous—by far the larger number of adaptations which occur in organic nature being the common property of many species.

(6) In no case can natural selection have been the cause of mutual infertility between allied or any other species.

(7) Without isolation, in the sense of either separate or segregate breeding, organic evolution is in no case possible; and hence, heredity and variability being given, the whole theory of organic evolution may be regarded as a theory of the causes and conditions which have led to isolation, or the mating of similar variations to the exclusion of dissimilar.

(8) Natural selection is one among sundry distinct kinds of isolation, and presents in this relation the following peculiarities: (*a*) the isolation is with reference to superiority of fitness; (*b*) is effected by destruction of the excluded individuals; and (*c*) unless assisted by some other kind of isolation, can only effect monotypic as distinguished from polytypic evolution.

(9) It is a general law of organic evolution that the number of possible directions in which divergence may occur can never be more than equal to the number of cases of efficient isolation; but, excepting natural selection, any one kind of isolation need not necessarily require the co-operation of another kind in order to create an additional case of isolation, or to cause polytypic as distinguished from monotypic evolution.

(10) Where common areas are concerned, the most general and most efficient kind of isolation has been the physiological—and this whether the mutual infertility has been the antecedent or the consequent of morphological changes on the part of the types concerned, and whether or not these changes are of an adaptive character.

(11) This form of isolation—which in regard to *incipient species* I have called physiological selection—may act either alone, or in conjunction with other kinds of isolation on common areas: in the former case its agency is of most importance among plants and the lower classes of animals; in the latter case its importance consists in its greatly intensifying the segregating power of whatever other kind of isolation it may be with which it is associated.

(12) Although physiological selection must in all cases refer primarily to first crosses, its activity as a cause of segregation is intensified in cases where it extends also to second crosses.

SCIENTIFIC SERIALS.

American Journal of Mathematics, vol. xii., No. 3 (Baltimore, March 1890.)—A memoir "Sur les équations aux dérivées partielles de la physique mathématique," by that brilliant mathematician, M. Poincaré, occupies pp. 211–294. Some idea of the writer's aim will be gained from the following passages:—"Quand on envisage les divers problèmes de calcul intégral qui se posent naturellement lorsqu'on veut approfondir les parties les plus différentes de la physique, il est impossible de n'être pas frappé des analogies que tous ces problèmes présentent entre eux." "Cette revue rapide des diverses parties de la physique mathématique nous a convaincus que tous ces problèmes, malgré l'extrême variété des conditions aux limites, et même des équations différentielles, ont, pour ainsi dire, un certain air de famille qu'il est impossible de méconnaître. On doit donc s'attendre à leur trouver un très grand nombre de propriétés communes." The concluding sentence is: "Je pourrai dire alors que les conclusions sont démontrées d'une façon rigoureuse au point de vue physique. Peut être même est-il permis d'espérer que, par une sorte de passage à la limite, on pourra fonder sur ces principes une démonstration rigoureuse même au point de vue analytique."—The remaining article of the number is one on singular solutions of ordinary differential equations, by H. B. Fine (pp. 295–322). Following the lead of Briot and Bouquet, this memoir bases the theory of singular solutions on the differential equation, and avoids all use, direct or indirect, of the notion of the complete primitive.

IN *Bulletin* No. 2 of the Brussels Academy of Science, M. E. Ronkar criticizes a paper by M. J. Liagre, on the mutual impulse of the earth's surface and centre because of interior friction. The paper in question dealt with the interior structure of the earth, and the conclusions drawn have some bearing on diurnal nutation.—In a paper on the venous pulse, M. Léon Fredericq gives his investigations into the form of various pulses—jugular, venous, and carotid; traces the identity of the pulse of the jugular vein and that of the right auricle; and discusses generally the phenomena of circulation and respiration. The same author adds a note on the preservation of oxyhæmoglobin.—M. A. F. Renard has examined phillipsite crystals from the deposits obtained from the centre of the Pacific Ocean. These microscopical crystals were discovered by Mr. Murray, and a brief description of them published by him in conjunction with the author in 1884 (Royal Society of Edinburgh). A more particular description and determination of the character of these zeolites, and the deposits in which they occur, is now given. A plate containing four drawings of the crystals accompanies the paper.—M. G. van der Mensbrugghe, in a paper on the condensation of water-vapour in capillary spaces, reviews the principal facts owing their origin to such condensation, and shows that they are in confirmation of the theory propounded by Sir William Thomson in 1874, in a paper on the equilibrium of vapour at a curved surface of liquid. The experimental verification of the formula there given will form the subject of a second communication.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, February 20.—"Some Stages in the Development of the Brain of *Clupea harengus*." By Ernest W. L. Holt, Marine Laboratory, St. Andrews. Communicated by Prof. McIntosh, F.R.S.

The stages described are (i) newly-hatched or early larval; (ii) early post-larval; (iii) ½ inch long; (iv) ¾ inch long.

The development of the pineal region is treated separately, and in this a fifth stage—1$\frac{1}{12}$ inch long—is introduced.

In the early larval stage the downward flexure of the fore part of the brain is very noticeable. It appears due to the general conformation of the head at this stage. A diverticulum of the 3rd ventricle extends downwards and backwards, its distal extremity underlying the optic commissure. The broad ventral commissure of the infundibulum, noticed by McIntosh and Prince in *Anarrhicas*, is well marked. A commissure shuts off the lumen of the infundibulum from the hind part of the 3rd ventricle immediately in front of the splitting off of the infundibulum. The valvula appears in transverse section as a pair of ridges externally to the tori, before it shuts off the aqueduct of Sylvius. The cerebellar fold is very short.

In the early post-larval stage "an apparent rectification of the cranial axis" has taken place, by the upward rotation of the cerebrum on its posterior end, doubtless owing to the rapid development of the oral and trabecular cartilages, and consequent forward rotation of the mouth. The same causes have also operated so as to withdraw the diverticulum of the 3rd ventricle from its position below the optic commissure. The infundibulum has undergone vertical flattening. The future lobi inferiores are indicated as lateral expansions, behind which the 3rd oculomotor nerves pass outwards from the centre of the ventral surface of the cerebral mass. The infundibulum extends some way back above the notochord as a thin-walled sac. Its walls are little plicated compared with those in some other forms, *e.g.*, *Rhombus*, *Anarrhicas*.

In the ½-inch stage the olfactory lobes appear as bulbous masses projecting from the front end of the cerebrum. A pale median septum appears between the anterior extremities of the lateral optic ventricles, its base resting on the fibrous tract over the hind part of the 3rd ventricle. The tip of the valvula now appears in transverse section before its connection with the cerebral mass can be made out, having thus grown forward. The cerebellum has greatly increased in size; instead of terminating as before on the surface of the brain, it is now continued into a thick fold bent sharply down on the anterior portion; its posterior end passes at once into the thin roof of the 4th ventricle. Two fibrous bands cross over the aqueduct of Sylvius in the substance of the cerebellum; their lateral extremities are fused. The lobi inferiores are better marked than in earlier stages. Longitudinal bands of fibres pass back from the roots of the oculomotor nerves through the medulla oblongata. Groups of large ganglionic cells appear on either side of these bands, and are connected by a fine commissure passing through both bands. At the origin of the 8th auditory nerves, this commissure is replaced by a St. Andrew's cross of fibres, the dorsal limbs of the cross passing to the nerve roots, and the ventral to the ganglionic areas.

In the ¾-inch stage the olfactory lobes are more elongated. The olfactory nerves pass outwards from their anterior extremities. The septum behind the pineal body, after losing its ventral connection with the fibrous tract over the 3rd ventricle, persists for some way back as a cellular leaf-like appendage of the thin median roof of the optic ventricle; a few fibres pass back into this appendage.

Large ganglionic cells appear in the tori semicirculares about the region of the splitting off of the infundibulum.

From behind the region of the auditory nerves a ganglionic area on either side persists backwards through the medulla oblongata.

Pineal Region.

The roof of the thalamencephalon in the early stages is a single layer of large columnar cells passing forward from the front wall of the pineal stalk. It passes into the roof of the cerebrum, the cells diminishing greatly in size. The superior commissure of Osborn is present from the early post-larval stage; it it also present in the larval and post-larval *Zoarces viviparus*, where it is distinctly double. The first signs of the infrapineal recess of Hoffman are seen in the ½-inch stage. It is thus much later in developing than in *Salmo*, and the fold forming its front wall never extends backwards to the same degree as in that form and in *Anarrhicas*. This fold, in the post-larval *Zoarces*, is thickened in its apex, and lodges a fine commissure. As pointed out by Balfour in Elasmobranchs the fold is due to the upward rotation of the cerebrum.

The fibrous tract over the 3rd ventricle in the herring is well marked in the ¾-inch stage. It is seen to consist of fibres passing upwards and inwards from the optic thalami to the middle line above the 3rd ventricle, and then running forward to the stalk of the pineal body. The tract has a double nature, as is readily seen in vertical longitudinal sections of a herring 1$\frac{1}{12}$ inch long. It is seen here to be a backwardly directed fold of the brain roof, continuous ventrally with the back wall of the pineal stalk, and dorsally with the roof of the optic ventricle, the apex of the fold being the posterior commissure. Its length in this form is due to the flattening of the brain, the tract being very short in

Zoarces, where the brain is not flattened. In *Zoarces*, also, from the same cause, the limbs of the fold are less closely applied to each other and much thicker.

The pineal body is roundish and solid in the early larval stage in the herring. It is vertically flattened in the early post-larval stage. In the ½-inch stage it is much larger and contains a lumen; it shows signs of constriction into proximal and distal elements, and the lumen contains a coagulable albuminous fluid, as in *Petromyzon*. In the $1\frac{1}{12}$-inch stage the constriction is still visible, and the walls are generally crenated. The tissues of the pineal wall are now divided into three layers, and are of varying thickness. The cartilage of the tegumen cranii overlies the body at this stage. The constriction of the body appears to be an exaggeration of the crenation of the pineal wall met with in *Salmo*; it has not, probably, the morphological value of the constriction of the body in *Petromyzon*.

March 27.—"On the Stability of a Rotating Spheroid of Perfect Liquid." By G. H. Bryan. Communicated by Prof. G. H. Darwin.

The investigations of Riemann, Basset, and others have proved that Maclaurin's spheroid, when composed of frictionless liquid, ceases to be stable for an "ellipsoidal" type of disturbance when its eccentricity attains the value 0·9528867. The object of the present paper is to discuss the conditions of stability with reference to disturbances of a general type expressible in terms of spheroidal harmonics, with the view of examining whether Riemann's condition is sufficient to ensure stability for displacements other than ellipsoidal.

Taking the criteria of stability determined in a previous communication (Phil. Trans., A., 1889), the author shows by numerical calculation that the form which is critical for an ellipsoidal disturbance is stable for disturbances determined by several of the lower harmonics. These results are then extended by a perfectly general investigation to all other types of displacement.

The conclusion is that Riemann's and Basset's condition of stability is sufficient to ensure the absolute stability of Maclaurin's rotating spheroid for every possible displacement. Also that, unless the liquid is subject to hypothetical constraints, we cannot initially obtain any form other than ellipsoidal from the instability of the spheroidal form. In the case considered of perfect liquid this ellipsoid does not rotate as if rigid, but its principal axes rotate with half the angular velocity of the liquid.

Physical Society, March 7.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—Dr. S. P. Thompson described Bertrand's refractometer, and exhibited the capabilities of the instrument before the Society. Its action depends on total reflection. The refractometer consists of a hemisphere of glass, about 8 mm. diameter, set at the end of a tube, the plane face being outwards and inclined at about 30° with the axis. One side of the convex surface of the hemisphere is illuminated through a piece of ground glass set about perpendicular to the plane face. The hemisphere is viewed through an eye-piece focussed on a scale divided to tenths of millimetres placed within the tube. The instrument is particularly useful for mineralogical specimens and liquids. The procedure in the latter case is to smear a film of the liquid over the plane face of the hemisphere, and by looking through the eye-piece determine the scale reading of the line which separates the light and darker portions of the field. A reference to a calibration table gives the refractive index. In experimenting with solids a thin film of a very dense liquid (supplied with the instrument) is placed between the specimen and the glass, and the procedure is then as above. The refractive index of opaque solids can be determined in this way. In using the instrument for minerals great care must be taken not to scratch the glass. The handiness of the refractometer and its perfect portability (its dimensions being about 5 centimetres long by 2½ cm. diameter) are great recommendations. Mr. Blakesley asked to what accuracy the scale could be read, and whether the sensitiveness of the instrument was at all comparable with that of other methods. Prof. Dunstan inquired if it could be used with volatile liquids. In reply Dr. Thompson said that with non-homogeneous light the scale could be read to 1 division, but with a sodium flame one-tenth of a division could be estimated. For volatile liquids, a drop may be used instead of a film, or the evaporation of a thick film may be retarded by a cover-glass.—Mr. H. Tomlinson's paper, on the Villari critical point in nickel, was postponed.—Prof. Dunstan described an apparatus for distilling mercury in a vacuum, devised by himself and W. Dymond, and showed the working of the arrangement. It consists of a 3 mm. soft glass tube rather more than a metre long, having an oblate spheroidal bulb blown at the upper end. The bulb is placed over a ring burner. At the top of the bulb, a tube of 1·5 mm. diameter is attached, and this passes outside the bulb, and descends close to the larger tube. The part of the smaller or fall tube just below the bulb is enlarged so as to form a condensation chamber, and the lower part serves as a Sprengel tube. A conical reservoir containing the mercury to be distilled is in flexible connection with the lower end of the large tube as in Clark's well-known apparatus. The advantages claimed for the new apparatus are, its relative shortness and portability, the small quantity remaining undistilled, and its non-liability to damage or derangement if left unsupplied with mercury. To ensure satisfactory working a constant pressure of gas is necessary, and this is obtained by inserting a Sugg's dry governor in the supply pipe. During distillation, peculiar green flashes are seen within the condensation chamber, and these are intensified by bringing it near an electric machine in action. The apparatus also serves well to show the character of an electric discharge through mercury vapour, for the mercury in the two tubes may be used as electrodes. Prof. Thompson said he devised a simple form of distilling apparatus some time ago which answered fairly well, and could be made by any amateur glass-worker. It consisted of a double barometer, one leg of which was of small bore, so as to act as a Sprengel tube. The rising part of the bend at the top of the larger tube was expanded and served as the evaporating chamber, below which a burner was placed. The President asked why Clark's apparatus is made so lengthy. In reply to this question Mr. Boys said that as the fall tube goes down within the rising one, the mercury near the top of the latter is heated by the condensing mercury (thus economising gas) and hence condensation does not take place until the vapour has passed a considerable distance down the fall tube.—Prof. S. U. Pickering read a paper on the theory of osmotic pressure and its bearing on the nature of solution. The author said that considerable doubt exists as to the accuracy of the premises on which the theory is based, and if the theory is to be regarded as true and not merely a rough working hypothesis, the following conditions must be fulfilled by weak solutions—(1) The molecular depression of the freezing-point must be independent of the nature of the dissolved substance. (2) Any deviations from (1) must be in the direction indicated by the theory. (3) the depression must be independent of the nature of solvent. (4) The depression must be independent of the amount of solvent (all solutions being weak). (5) The deviations with strong solutions should be in the theoretical direction. (6) They should be regular. Prof. Pickering proceeded to show that experiment, instead of confirming the above statements, disproves them all. As regards (1), without counting abnormally low (half) values, Raoult's results show variations of 60, 40, 30, &c., per cent. in different cases, and the author quoted other values where the variations were 500, 260, 230, &c., per cent. These variations, he considered, were too great to be explained by the fact of the solutions used being 3 or 4 times too strong. Referring to (2), he said that low values are reasonably explained by the polymerization of the dissolved molecules, high values by their dissociation into ions. He then argued that there are no abnormally high values, for the view that such exist, and that they are explainable by dissociation involves the following conclusions: (*a*) that the more stable a substance is, the more easily is it dissociated; (*b*) that solution dissociates molecules which we know can exist undissociated as gases; (*c*) that water must consist of $1\frac{1}{2}H_2O$, and the atomic theory is wrong; (*d*) that energy can be created, and therefore the theory of its conservation is untenable. With respect to (3), it was pointed out that in many instances the same dissolved substance gives the full depression with one solvent and half depression with another. Cases were quoted where the depression produced by the same dissolved body in different solvents showed variations of 36,000, 21,000, and 28,000 per cent. In discussing (4), the author said that even with solutions weaker than that corresponding to a gas, the law is not fulfilled. Taking the case of sulphuric acid (the only one at present fully investigated), the variations amount to 40 per cent., or about 28 times the experimental error. With reference to (5), it was stated that with strong solutions the molecular depression should become smaller, but in every known case (9 were quoted) it becomes larger, the increase in one instance being 3,200 per cent. As regards (6), all experimental data available, especially those relating to sulphuric acid, show that the deviations are neither regular nor always in the same direction. Mr. T. H. Blakesley said he was greatly interested with Prof. Pickering's paper, for some time ago he was induced to make experiments on the volume of salts in solution by reading Joule's papers on that subject. Some of the results confirmed, but others did not agree with, Joule's theory that the molecular volume in solution was a whole number. If this theory was true, then (he said) it would be possible to predetermine the density of solutions, and from the measured density of any known solution we could determine the water of crystallization of the salt from the formula

$$n = \frac{1 - \frac{W}{w}(D-1)}{D}\left(\frac{A}{H_2O} + x\right);$$

where W and w are the masses of the water and salt respectively, D the density of the solution relative to water at the same temperature, A the molecular weight of the dehydrated portion of the salt, x the number of molecules of water, and n the molecular volume of the salt in solution, the two latter being whole numbers.

Chemical Society, March 6.—Dr. W. J. Russell, President in the chair.—The President announced that the senior Secretary would attend the meeting to be held in Berlin on March 11 to celebrate the 25th anniversary of the promulgation of Prof. Kekulé's benzene theory, and would present a congratulatory address from the Society.—The following papers were read:—Some crystalline substances obtained from the fruits of various species of *Citrus*, by Prof. W. A. Tilden, F.R.S., and Mr. C. R. Beck. The authors have examined the solid matters which are deposited from freshly extracted oils of limes, lemons, and bergamot made by hand. The substance, limettin, obtained from oil of limes (*C. limetta*) has the composition $C_{16}H_{14}O_6$, and crystallizes in tufts of needles melting at 121°–132°. It is neither an acid nor a glucoside, is not acted upon by acetic chloride or phenylhydrazine, and yields phloroglucol, and acetic and formic acids on fusion with potash. Essence of lemons yields a substance, $C_{14}H_{14}O_6$, very similar to limettin in appearance, though the crystals are more lustrous and melt at 116°. Bergamot yields a compound which crystallizes in colourless prisms and melts at 270°–271°.—Reduction of α-diketones, by Prof. F. R. Japp, F.R.S., and Dr. F. Klingemann. Benzil, when reduced by boiling with fuming hydriodic acid for a few minutes, gives an excellent yield of deoxybenzoïn. Phenanthraquinone, under like conditions, gives so-called phenanthrone, which, contrary to Lachowicz's view, is not the deoxybenzoïn of phenanthraquinone, but a mono-hydroxyphenanthrene.—Studies on isomeric change, No. IV; halogen derivatives of quinone, by Mr. A. R. Ling. The experiments of Hantzsch and of Nietzki have proved, in opposition to those of Levy, that the "anilic" acids are paradihydroxy-derivatives of quinone, and Hantzsch and Schniter have shown that an isomeric change occurs when paradichloroquinone is brominated, the product being metadichlorometadibromoquinone. The author has investigated the action of bromine on paradichloroquinone and diacetylparadichloroquinol, and the action of chlorine on paradibromoquinone, and has obtained results which confirm Hantzsch and Schniter's conclusion, since all attempts to prepare paradichloroparadibromoquinone, CO<(CBr.CCl)(CCl.CBr)>CO, have been unsuccessful, the product in every case consisting of the isomeric metadichlorometadibromoquinone, CO<(CCl.CBr)(CCl.CBr)>CO.—Note on a phenylic salt of phenylthiocarbamic acid, by Prof. A. E. Dixon.—Contributions to the chemistry of thiocarbamides; interaction of benzyl chloride and of allyl bromide with thiocarbamide, phenyl- and diphenylthiocarbamides, by Mr. E. A. Werner.

Geological Society, March 12.—Mr. J. W. Hulke, F.R.S., Vice-President, in the chair.—The following communications were read:—On a deep channel of drift in the valley of the Cam, Essex, by W. Whitaker. In Scotland and in Northern England long and deep channels filled with drift have been noticed, but not in Southern England. For some years one deep well-section has been known which showed a most unexpected thickness of Glacial drift in the higher part of the valley of the Cam, where that drift occurs mostly on the higher grounds and is of no very great thickness. Lately, further evidence has come to hand, showing that the occurrence in question is not confined to one spot, but extends for some miles. The beds found are for the most part loamy or clayey. At the head of the valley various wells at Quendon and Rickling show irregularities in the thickness of the drift, the chalk coming to or near the surface in some places, whilst it is nearly 100 feet below it sometimes. Further north, at Newport, we have the greatest thickness of drift hitherto recorded in the South of England, and then without reaching the base. At one spot a well reached chalk at 75 feet; whilst about 150 feet off that rock crops out, showing a slope of the chalk-surface of 1 in 2. In the most interesting of all the wells, after boring to the depth of 340 feet, the work was abandoned without reaching the chalk, the drift in this case reaching to a depth of about 140 feet below the level of the sea, though the place is far inland. The chalk crops out about 1000 feet eastward, and at but little lower level, so that there is a fall of about 1 in 3 over a long distance. At and near Wenden the abrupt way in which drift comes on against chalk has been seen in open sections. Two wells have shown a thickness of 210 and 296 feet of drift respectively; and as the chalk comes to the surface, at a level certainly not lower, only 140 yards from the latter, the chalk surface must have a slope of 1 in less than 1½, and this surface must rise again on the other side, as the chalk again crops out. The drift here reaches to a depth of 60 or 70 feet below the sea-level. At Littlebury, in the centre of the village, a boring 218 feet deep has not pierced through the drift, which reaches to 60 feet below the sea-level. As in a well only 60 yards west and slightly higher, the chalk was touched at 6 feet, there must here be a fall of the chalk surface of about 1·2 in 1. Eastward, too, on the other side of the valley, the chalk rises to the surface. The places that have been mentioned range over a distance of 6 miles. How much further the drift-channel may go is not known, neither can we say to what steepness the slope of the underground chalk surface may reach; the slopes given in each case are the lowest possible. The author thinks that the channel has been formed by erosion rather than by disturbance or dissolution of the chalk. After the reading of the paper there was a discussion, in which Dr. Evans, Mr. Clement Reid, Mr. Topley, Mr. J. Allen Brown, Dr. G. J. Hinde, and the author took part.—On the Monian and basal Cambrian rocks of Shropshire, by Prof. J. F. Blake.—On a crocodilian jaw from the Oxford Clay of Peterborough, by R. Lydekker.—On two new species of Labyrinthodonts, by R. Lydekker.

Linnean Society, March 20.—Mr. W. Carruthers, F.R.S., President, in the chair.—After reading the minutes of the last meeting, the following resolution, moved from the chair, was unanimously adopted:—"On the occasion of a gift, from Mr. Crisp, of a handsome oaken table for the meeting-room, the Society desires to record its deep sense of the valuable services rendered by that gentleman, not only as Treasurer, but by numerous acts which are not generally appreciated because they are practically unknown to the Fellows."—Prof. P. Martin Duncan, F.R.S., exhibited several specimens of *Desmophyllum cristagalli* obtained from an electric cable at a depth of 550 fathoms. Though showing great variation in the shape and nature of the wall, the specific characters of the septa were maintained. The core, extending as a thin lamina far beyond the peduncle, had no connection with the septa. A section of *Caryophyllia clavus* showed theca between the septa, and a section of *Lophohelia prolifera* exhibited a true theca extending beyond the septa.—Mr. E. B. Poulton, F.R.S., exhibited some Lepidopterous larvæ showing the variation in colour induced by natural surroundings; and some lizards, in spirit, from the West Indies, showing the pineal eye very distinctly.—In continuation of a former paper on the external morphology of the Lepidopterous pupa, Mr. Poulton gave a detailed and interesting account of the sexual differences observed in the development of the antennæ and wings.—Prof. G. B. Howes read a paper on the intestinal canal of the Ichthyopsida, with especial reference to its arterial supply. He described certain arteries hitherto unrecorded, and some variations he had found in them in the Frog and Salamander. The artery known in the Elasmobranchii as the inferior mesenteric, was shown to belong to the superior mesenteric series. Discussing the morphology of the intestine and its derivates, the author defined the large intestine of the Pisces more precisely than had hitherto been done, and showed that the appendix digitiformis of the Elasmobranchs must be regarded as homologous with the appendix vermiformis of mammals, and that a short cæcum coli is present at any rate in the Batoidei. The anatomical relationships of the appendix digitiformis were described in certain Elasmobranchs for the first time, and some notes were added upon the cæcum and large intestine among Teleosteans.—An interesting paper was then read by Mr. R. A. Grimshaw, on heredity and sex in the honey-bee.

PARIS.

Academy of Sciences, March 24.—M. Hermite in the chair.—M. Mascart presented a note on a direct-reading transmission dynamometer with a photographic registering arrangement, and also one on the Observatory at Tananarivo, setting forth some of the meteorological work to be undertaken in this new Observatory.—M. Berthelot, in a paper on the condensation of carbonic oxide, and on the penetrability of glass by water, says that he has been unable to obtain evidence of the transmission of water through glass under the influence of the silent discharge, and finds that the carbonic oxide is truly condensed into a body which rapidly takes up moisture from the air.—Under agricultural chemistry, M. Th. Schlœsing makes some remarks relative to the subject of M. Berthelot's observations on the reactions between soils and atmospheric ammonia, and discusses the differences of opinion existing between them.—M. L. Ranvier, in microscopical observations of the contraction of living muscular fibres striated and unstriated, has contrived a method by which muscles may be excited whilst being viewed under a microscope, and from comparative observations of muscular elements in repose and contracted, finds that the homogeneous period and the inversion imagined by Merckel does not exist.—On the regulation of the motion of governors by an auxiliary dynamo, by M. A. Ledieu.—On the Cretaceous Echinodermata of Mexico, by M. Cotteau. Descriptions are given of six specimens received from Mexico. The specimens are interesting both from a zoological and geological point of view, since they determine the age of the strata in which they were found.—In studies on the capture theory of periodic comets, M. O. Callandreau extends the elaborate work done by M. Tisserand on the same subject.—On the discovery of a remarkable transcendental function, by M. Fredholm.—On the invariants of a class of equations of the first order, by M. Z. Elliot.—Relation between the volume, the pressure, and the temperature of different vapours, by M. Ch. Antoine.—Comparative study of specific inductive power, and of the conductibility of spaces filled with rarefied air, by M. James Moser. From the study of these properties with spaces containing air in three states of rarefaction—namely, (1) at a pressure of 10 mm. of mercury, (2) at 1 mm. pressure, (3) with an extreme vacuum—the author deduces that while the conductibility varies the specific inductive power remains constant.—Electrolysis of a mixture of two salts in aqueous solution, note by M. L. Boullevigne. Using a mixture of Zn and Cu salts, it is found that the composition of the brass deposited varies rapidly with the intensity of the current employed, contrary to Buff's law. Considering the variation to be due to the chemical action of the sulphate of copper upon the zinc in the alloy deposited, and that the amount of this action is proportional to the time, an expression is found which allows the composition of the alloy obtained with any given intensity to be calculated with a fair degree of accuracy as tested by experimental results. — A new method of preparation of betaïnes, by M. E. Duvillier. The author uses a reaction similar to that by means of which M. Schützenberger obtained the leucines synthetically; an ethereal iodide is caused to act upon the zinc salt of an amide acid in the presence of zinc oxide.—Titration of acetone by the iodoform reaction, by M. G. Arachequesne.—On callose, a new fundamental substance existing in cell membranes, by M. Louis Mangin.—The estimation of fatty matter in milk, by M. Lezé. 100 parts of milk are heated in a flask with a graduated neck till the mixture becomes brown, ammonia is added till the whole becomes clear, the fatty matter rising to the top and its volume being read off on the graduated neck.—On new forms of crystallized silica, note by MM. Michel-Lévy and Munier-Chalmas.—The solubility of some substances in sea-water, by M. J. Thoulet.—On the development of siliceous sponges and the conformation of leaflets among the sponges, by M. Yves Delage.—On the physiological meçhanism of hatching, sloughing, and metamorphosis among Orthopterous insects of the Acridean family, by M. J. Kunckel d'Herculais.—On the great sand dunes of the Sahara, note by M. G. Rolland.—On the gypseous formations of the Paris basin, and on the siliceous deposits which have replaced the gypsum, by M. Munier-Chalmas.—On the physiological action of arseniétted hydrogen, by MM. F. Joly and B. de Nabias.—On the diarrhœic action of cholera cultures, by M. N. Gamaleia.—On the vibration of the earth at Chung-Hai and the movements of the compass at Zi-Ka-Wei during this vibration, by M. Chevalier. It is remarked from observations that the vibrations of the earth are unaccompanied by magnetic disturbances.

BERLIN.

Physiological Society, March 14.—Prof. du Bois-Reymond, President, in the chair.—Dr. Heymans spoke on myelin, giving a concise account of the numerous chemical and scanty microscopical investigations of what Virchow had designated as myelin-formations in peripheral nerves. From a chemical point of view the controversy had turned chiefly upon the existence or non-existence of Liebreich's protagon. The speaker had made investigations on frogs' nerves, from which he concluded that both protagon and lecithin are present in them, and that myelin-formations are due to imbibition, with simultaneous production of an external membrane.—Dr. Goldscheider gave an account of his researches on the sensitiveness of the articular surfaces of joints, based upon experiments on the tibial and metatarsal joints in rabbits. It appeared that the sensitiveness was dependent not so much upon the irritability of the surfaces of the joints, as of that of the epiphyses. The greatest effect was produced by direct stimulation of the marrow of the respective bones, while stimulation of the compact bone-substance showed that this was quite insensitive.

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