THURSDAY, OCTOBER 3, 1872

BOTANICAL MUSEUMS

THE question of the future relations of the national herbarium at the British Museum to that at Kew is at present engaging the attention of the Royal Commission on Science. The minute of 24th July last shows that it will presently be dealt with by the Treasury. On the motion of Messrs. Bentham and Ball, at the recent meeting of the British Association at Brighton, the Committee of the Biological Section secured an instruction to the Council of the Association to take action in connection with this question in the interests of botanical science; and the question has been submitted to the readers of NATURE in an anonymous article on "Botanical Museums," published on the 23rd March last year.

The authorship of this article was unknown to me until I was summoned to give evidence before the Royal Commission on Science. At the request of the President of the Commission, the proof of Mr. Bentham's evidence was placed before me; I then saw that the article alluded to was incorporated in it, and in the statement with which Mr. Bentham introduced it I read his history of its preparation and publication.

In dealing with this part of Mr. Bentham's evidence, I informed the Commissioners (Q. 7,739-40) that the opinions and arguments stated in it had been, two years before, submitted by Dr. Hooker, through the Board of Works, to the trustees of the British Museum, and had been answered by my predecessor, Mr. Bennett, to the satisfaction, as I then understood, not only of the trustees, but of the authorities at the Board of Works, and that the article on "Botanical Museums" was merely a reproduction of this official paper, without any reference to its answer.

After supplying the date of this official document, my examination, under the guidance of the Commissioner who was then dealing with me, took another direction. On subsequent reflection I therefore felt it necessary to ask leave to present a detailed reply to the Commissioner, which was granted. This reply is printed with the other evidence in the recently printed Blue Book (p. 44).

That the readers of NATURE who have already perused Mr. Bentham's article may have the opportunity of considering my answer, I submit it to them, earnestly desiring that this question now raised should be fully and exhaustively considered, and that no hasty or one-sided judgment should be arrived at; being thoroughly convinced that the action to be taken by the Treasury, whatever it may be, will seriously influence the whole future of the science of botany in England.

I desire to submit to the Commissioners my views:—(I.) On the statements contained in Mr. Bentham's paper, and (II.) on the matters naturally flowing out of these statements.

I. *The statements contained in the paper*

1. The views expressed by Mr. Bentham regarding the main purposes of a botanical museum and herbarium, and the requirements of a collection for such a close study of plants as would supply a "sound foundation upon which the science of botany can be usefully established," arise from his estimating the science of botany as limited to that particular department of it to which he has devoted his life, and in which he has done important service. The profound study of plants is, in his view, "their accurate determination and practical classification," and he states that he requires for its prosecution nothing more than an exhaustive herbarium of the fragments of plants supplying the diagnostic characters at present employed for distinguishing genera and species, with a complete library and staff of officers. This is, in my opinion, a very defective estimate of the science of botany, and of the materials required for its advancement.

Robert Brown took a very different view of the profound study of plants, and in the Botanical Department of the British Museum he tried to develope that masterly grasp of the science which is to be found in his works, by illustrating, as far as possible, the structure of all plants from the lowest to the highest, both existing and extinct. Accordingly, the National Herbarium, large as it is, forms but a part of the botanical collections. The specimens placed in the outer rooms, which exhibit chiefly the form and structure of the stems and roots of plants, are as necessary a part of the purely scientific collection as the dried foliage and flowers in the herbarium. While such specimens "excite the interest," and "gratify the curiosity" (and, what is more important, instruct the minds) "of the general public," these are very far from being their principal, still further from being their only purpose in a botanical museum, as Mr. Bentham appears to imply. The scientific investigator whose notion of systematic botany is somewhat larger than ascertaining the technical name and order of a plant, consults these specimens as he does the herbarium. It is, therefore, a mistake to suppose that they, "when once placed, require no further handling."

The purely scientific collection of the British Museum consists of:—1. The herbarium, comprising (*a*) the general herbarium, (*b*) the British herbarium, (*c*) various separate small and complete herbaria of historical interest. II. The structural series, comprising (*a*) the fruit collection, (*b*) the collection of gums, resins, and other natural products, (*c*) the general collection, exhibiting the form and structure of plants, and consisting of the larger specimens chiefly exhibited to the public; and (*d*) the microscopical preparations, illustrating the minute structure of recent and fossil plants.

2. The limitation of the science of botany to the plants now existing on the earth is another grave defect. No subject has recently received more attention from biologists than the relation between existing and extinct plants and animals. Every philosophic estimate, or systematic classification of the one kingdom or the other must include the fossil as well as the recent. This is fully acknowledged and acted upon by zoologists, and no better illustration can be adduced than Prof. Huxley's "Introduction to the Classification of Animals" (1869). In botany also, in the standard and only complete *Genera Plantarum*, by Endlicher, the fossils are ranged in their systematic position with the recent plants. It is true that the *Genera Plantarum* now in progress, of which Mr. Bentham is one of the authors, ignores all extinct plants. This retrogade step is in entire accordance with the views expressed by Mr. Bentham in NATURE. A systematic account of the *Lycopodiaceæ* which took no notice of the arborescent forms of palæozoic age, or of the *Cycadeæ* which ignored the numerous forms and remarkable variations of this order in the secondary rocks, would be obviously very incomplete and unsatisfactory. In forming a collection to supply a sound foundation for the science of botany, it would be as reasonable to exclude the plants of any existing botanical province—say Australia—as to omit those which have existed at any particular period of the earth's history—say that of the Wealden.

3. The distinction which Mr. Bentham draws between a herbarium "for the close study of plants" and one for their "rapid determination without dissection" is most

undesirable, and, in my opinion, practically impossible. No botanist has so extensive an acquaintance with the vegetable kingdom as to be able to make "a close study," in his necessary work, of every group of plants he may be naming or arranging; he must in many groups make a "rapid determination without dissection." If Mr. Bentham's distinction were in force, and the two herbaria he proposes existed, he would himself, when rapidly naming some of the important collections which have passed through his hands, have often been driven from the great scientific collection to work in his single specimen herbarium with the "general naturalist," "the palæontologist," and "the mere amateur." Every systematic botanist is at first, and more or less all along, a "comparer" of plants. The man who begins as a mere comparer naturally becomes a close student under the influence of the collection he is consulting, and the workers he encounters in that consultation.

4. Mr. Bentham's single specimen herbarium is chiefly intended for the palæontologist, and in addition he proposes to provide him with "separate collections of leaves and fruits, . . so arranged as to enable them to be rapidly glanced over," and these, it is added, "would be most useful." No better testimony to the utter worthlessness of such materials for the purpose proposed can be adduced than the criticisms of Mr. Bentham himself, on the evidence for the existence of the natural order *Proteaceæ* in Europe, from leaves found in Tertiary strata. Mr. Bentham was specially fitted to deal critically with the hundred fossil species referred to this order, as he had just made the analysis and detailed descriptions of between five and six hundred *Proteaceæ*. The Order is also the best fitted to test the value of the leaf characters on which the fossils had been referred to it, because, as he testifies, it "is one of the most distinct and most clearly defined amongst phanerogams," and is without "a single plant intermediate in structure between that and the nearest allied orders." With regard, then, to the leaves of this order, Mr. Bentham says, "I must admit that there is a certain general *facies* in the foliage of this order that enables us in most, but not in all cases, to refer to it with tolerable accuracy—leafy specimens known to have come from a proteaceous country, even without flowers or fruit—but as to detached leaves, I do not know of a single one which, in outline or venation, is exclusively characteristic of the order, or of any one of its genera." I cannot reconcile this declaration by Mr. Bentham to the Fellows of the Linnean Society as their President in May 1870, with the statement published by him within a year thereafter, that such a collection of detached leaves not for a limited and exceptionally defined order, but for the whole vegetable kingdom, "would be most useful."

I must further observe that Mr. Bentham has overlooked the fact that a large proportion of fossil plants have been determined from their internal structure, that is, on evidence which no mere herbarium, however extensive, can supply, far less one for rapidly determining plants without dissection, or a collection of detached leaves. The palæontologist requires the most extensive collections possible for his work, and he must be a working zoologist or botanist. All such work done by mere "geologists" and on such data as Mr. Bentham proposes to supply would always deserve strong condemnation.

II. *The matters flowing out of these statements*

In considering the matters naturally flowing out of Mr. Bentham's paper, and the views I have now expressed, I venture, *firstly*, to submit the reasons which make it desirable in my opinion to retain the two herbaria as separate and independent institutions.

1. The two herbaria already exist, and are to a considerable extent parallel collections. Mr. Bentham, whose extensive private herbarium formed the foundation of the public herbarium at Kew, declared, in 1858, "that a great portion of the additions to the Banksian herbarium since Sir Joseph's death are duplicates of those already at Kew." As the Banksian plants form less than a quarter of those now existing in the British Museum herbarium, the duplicates would be, according to Mr. Bentham, about three-fourths of the whole. Sir William Hooker, also, whose large collections form the great bulk of the Kew herbarium, testified, in 1858, that "the Museum specimens are to a great extent duplicates of those at Kew." And the present Director of Kew Gardens corroborated this statement at that time. In 1860 Sir William Hooker further said, in reference to the transfer of the National Herbarium to Kew as affecting the herbarium there, "To Dr. Hooker and myself it literally and truly can be a matter of no consequence."

2. The two herbaria have been under different management, and to some extent express different results of "the close study of plants." The important bearing of this consideration on botanical science in Britain can scarcely be overestimated. One practical illustration may be adduced. The most varied views are entertained by botanists as to the limits of a species, and consequently as to what constitutes a duplicate. Thus, in the case of the indigenous flowering plants of Britain, Mr. Bentham considers them to form 1,274 species; Dr. Hooker, in his recent Flora, makes 1,473 species; Prof. Babington increases the number to 1,648 species; while a botanist adopting the views which Jordan and some continental authors have applied to local floras, would make them three or four times more numerous than even the last estimate. It is quite obvious that these different botanists have each very different notions as to "duplicates," and that a distribution undertaken by Mr. Bentham would certainly result in the loss to the herbarium of plants which Dr. Hooker would consider good species, and the "duplicates" distributed by Mr. Bentham or Dr. Hooker would include numerous plants which would be of the utmost value in M. Jordan's eyes. The two herbaria, existing, as they do, under different directors, to a considerable extent counteract these and other analogous evils.

3. The objects of the two herbaria are fundamentally different, and in as far as they fulfil these objects, they are employed for totally different purposes. The National Herbarium at the British Museum was founded in 1827 for the use of the scientific botanist; while that at Kew was, as Dr. Hooker says, "originally maintained expressly for the use of the gardens." This was the primary object for which Sir W. J. Hooker accepted the private herbarium of Mr. Bentham in 1855. Before that year the gardens had been fulfilling their proper functions without a scientific herbarium attached to them. The two editions of the "Hortus Kewensis" are the best testimony to the efficiency of the gardens, and to the value of the collections brought together there under the Aitons. No herbarium of any kind, I believe, existed at the gardens during their time. The Banksian Herbarium was often and for a long time systematically used for naming the Kew plants; and the strictly scientific portion of the "Hortus Kewensis" was the work of Solander, Dryander, and Brown, the successive curators of the Banksian Herbarium. Even Sir W. J. Hooker, the successor of the younger Aiton, who raised the gardens to their present eminence, had no public herbarium from the time of his appointment in 1841 to 1855. It is, therefore, evident that a great scientific herbarium is not a necessity to the efficiency of the gardens at Kew.

It is, however, certain that such a herbarium as Sir W. J. Hooker and Dr. Hooker desired, that is, one sufficient to enable the officials to name the plants in the gardens, would be a most useful adjunct at Kew, as it would save the great waste of time which would be incurred in consulting a herbarium at a distance. Inasmuch as growing plants are, to the extent that they are developed, perfect, and permit thorough examination, it is obvious that the single specimen herbarium proposed in NATURE would meet all the requirements at Kew, and this could be kept up as suggested by Mr. Bentham from the duplicates not required in the great National Herbarium, all being accurately named before being sent.

4. The practical difficulties in the administration of two separate, and to some extent independent, herbaria would be numerous and serious, and in the course of time a condition of things similar to what at present exists would result. It is needless to speak of a London herbarium, consisting of single specimens of each species, because such a herbarium, if practicable, would, as I have already shown, be utterly worthless for the purposes to which it is proposed to be applied. If the London herbarium were to contain only specimens sent by the keeper of a herbarium, whose notion of the science of botany was confined to the "accurate determination and practical classification" of herbarium specimens, it is obvious that the palæontologist would not find there the materials for prosecuting his work. If, on the other hand, the London herbarium were constituted to be of real use to the palæontologist, the keeper must have the power of acquiring as opportunity offered the suitable materials, and he would necessarily secure collections which a future agitator might demand to be transferred to Kew, with as pertinent reasons as those Mr. Bentham now employs.

5. It is not an unimportant consideration that the continued separate existence of these two great herbaria is a great security against their destruction by fire.

6. The expense of the two herbaria is very small. I am unacquainted with the amount granted for Kew herbarium, but it cannot greatly differ from that required by the National Herbarium, which amounted for the financial year lately completed to 1,767*l.* I know of no way in which the country can at once advance the interests of science and encourage its students, at a smaller cost and with more important results than by maintaining in their full efficiency the two botanical collections at present existing.

But, *secondly*, it must be admitted that the formation of a single great national botanical establishment, comprising the two public herbaria now existing within a comparatively small distance from each other, is a very attractive scheme, and should the Commissioners think that its realisation is desirable, I submit the following considerations as in my opinion essential :—

1. It must form part of the National Museum of Natural History. Such a museum, as far as it is an exhibition of biological science, will consist of animals and plants, both existing and extinct. It is absolutely necessary in the study of geology that the plant remains should not be separated from the animal remains ; and further, it is as necessary for the satisfactory interpretation of the fossil plants, as well as for forming a true estimate of the vegetable kingdom that the recent plants should not be separated from the fossil. The separation of any one department would be a serious injury to all.

2. It must represent the whole science of botany, and not consist of only dried foliage and flowers, which constitute a herbarium properly so called ; and consequently it must be formed on the principle adopted by Robert Brown, and exhibited in the Botanical Department of the British Museum, and not on the imperfect plan advocated by Mr. Bentham.

3. It must be placed in the position in which it will be most serviceable to the public and most accessible to botanists, and that place is beyond all question London. The statistics which I submitted on the occasion of my former examination establish this, by showing the extent to which the botanical collections at the British Museum are made use of. Further, it is universally acknowledged that a herbarium for scientific use must exist in London. The long experience of Mr. Brown and Mr. Bennett in the National Herbarium made them entertain and express very decided views as to this necessity. My shorter experience has been long enough to convince me that its removal to Kew would be practically placing it out of the reach of the busy men who frequently use it to the advantage of science. Of course the working botanist who devotes himself exclusively to the science would follow the collections wherever they went ; but the active professional man, and the man of business, who devote their spare hours to botany, would be deprived of the assistance necessary to their work which they now obtain at the British Museum. That such men do a large proportion of the scientific work of the country may be shown in many ways, as for instance, by the fact that out of the nineteen botanical memoirs contained in the last two volumes of the Linnean Transactions, four are produced by professional botanists, and fifteen by others.

The late Prof. Henfrey, as representing the botanical teachers of London, Sir Charles Lyell, for the palæontologists, and Dr. Falconer, Mr. Bentham, and Dr. Hooker, have recorded it as their decided opinion that the interests of science require that a public herbarium should exist in London. Such a herbarium, even if used only by palæontologists, must be, as I have shown, as extensive as possible ; otherwise, it will tend to mislead, like all other imperfect sources of information.

I would further add in favour of London being the proper site for the national botanical collections, that important collections of plants, both recent and fossil, accessible to students, but not to the general public, now exist and must still remain in London. These are : (1) the Linnean herbarium, containing the plants described by Linnæus ; (2) the great Wallichian herbarium ; (3) the Smithian herbarium of British plants ; all belonging to the Linnean Society ; (4) the collection of fossil plants belonging to the Geological Society ; and (5) the extensive public collection of fossil plants in the Museum of Practical Geology. The removal of the National Botanical Collection from London would so separate them from these collections as seriously to injure their value to scientific investigators.

4. The accommodation provided for the Botanical Department in the New Museum of Natural History, the plans of which have been accepted by the trustees of the British Museum, will be in every way superior to any that exist in the world, and will be amply sufficient to accommodate the proposed single national herbarium, as well as fully to display the structural, histological, and palæontological departments of the science. All the requisites specified by Mr. Bentham for the close study of plants, excepting the connection with a garden, exist to a greater or less degree at the British Museum, and some of them in a greater degree than at Kew. That living plants are a requisite adjunct to a herbarium is in opposition to the testimony of Mr. Brown and Dr. Falconer, to the effect that there is no necessary connection between a herbarium and a garden ; and is opposed moreover to the testimony of Mr. Bentham himself, as well as to his declaration that his extensive systematic labours have all been based on herbarium specimens, although they have been carried on in close proximity to the finest scientific garden in existence.

In the event then of its being resolved to maintain only one great national botanical collection, I would submit that it should not be cut off from the allied biological collections, but be placed with them in the same building in London. And that for this end the collections presented by Mr. Bentham to the public, and all that have been added to them by purchase or presentation, be removed to London and incorporated with the national herbarium ; and further, that the extensive botanical library formed at the national expense at Kew be made, with the Banksian library, the foundation of that national natural history library which will be required for the National Museum of Natural History.

It is necessary, in dealing with Mr. Bentham's printed and publicly expressed views on this matter, to bear in mind that he cannot be considered an unprejudiced witness. I have frequently referred to his relations to the herbarium attached to the Royal Gardens at Kew. He has thus stated the reasons by which he was influenced in presenting his herbarium and library to the public in 1855:—"I thought that at that time there was no herbarium and library in London sufficiently open for the use of botanists, and I presented them on condition that they should form the nucleus of a national herbarium and botanical library, to be kept at the expense of Government, and open to the free use of botanists." I can assert in opposition to Mr. Bentham's belief—and a similar opinion has been, I understand, recently expressed—that at that time the National Herbarium and the national library, as far as it is an adjunct to the herbarium, were fully and freely accessible to botanists, and were largely used by botanists; and this I am able to maintain from the contemporary records of this department, as well as from the testimony of botanists who were then in the habit of consulting the collections. Under the influence of this erroneous supposition, Mr. Bentham made his own herbarium a national institution, and a rival to the Banksian herbarium, and under the influence of this same spirit of rivalry, he now believes that there exists "a state of continual competition" between the two herbaria. I am sure that Dr. Hooker and the authorities at Kew will as strongly repudiate this statement as I do now, if it is meant to imply a competition in any way to the injury of science or the public. It is only in keeping with the motives which actuated him at the first that Mr. Bentham now agitates for the incorporation of the Banksian herbarium with that of which his own forms the nucleus.

William Carruthers

FRESENIUS'S QUALITATIVE ANALYSIS

Qualitative Chemical Analysis. By Dr. C. Remigius Fresenius. Eighth edition. Translated from the 13th German edition, by A. Vacher. (London: J. and A. Churchill)

THE present edition of Fresenius is one which will be looked at by chemists with interest. In the last so-called edition of Fresenius's Chemical Analysis, published in 1869, the text had been so altered and curtailed that the volume could scarcely be recognised as Fresenius of old, it having been reduced to about one half of its original size. It appears, however, that this condensation did not coincide with the author's views, and in this edition we have simply a translation of the original text. The editor in his preface confesses that he then, in the last edition, took too broad a view of his duty. It certainly seemed strange that a work which had passed through twelve German editions with a gradual and steady enlargement, could be condensed to about one-half without losing a considerable amount of its clearness and usefulness. We confess that we heard with pleasure of the appearance of the eighth English edition of this work, but, unfortunately, our pleasant anticipation has been to some extent marred.

In criticising this book, it will be necessary to consider it from two points of view; in the first place, to consider the work of the translator; and in the second, the author's responsibilities, and the book itself. To commence with the translator's work: it appears, on the whole, to be very well executed, although by far the greater part of the book is identical with the sixth edition, published in 1864, and edited by J. Lloyd Bullock; in fact, at first sight, there does not appear to be very much difference between the two editions; but on a closer acquaintance with the present edition, there is found a considerable amount of new matter, and some little alteration in the old. As is almost certain in a book of the size, we have found some sentences which would have been better for a little more attention; to quote one instance, p. 51, "Solution of ammonia, although formed by conducting ammoniacal gas ($N H_3$) into water, and letting that gas escape on exposure to the air, and much quicker when heated, may also be regarded as a solution of oxide of ammonium ($N H_4 O$) in water, the first acceding equivalent of water ($H O$) being assumed to form $N H_4O$ with $N H_3$." This sentence cannot certainly claim precision and clearness as its chief characteristics, and we have much doubt whether a young student would understand its meaning. Taking the translation, however, as a whole, it is clear and well expressed.

It will be seen from the above quotation that there are some points about this edition which will not recommend themselves to the generality of English teachers; we refer of course to the nomenclature and notation, which have not been altered since the edition of 1864. Of course Mr. Vacher is not responsible for this; if Dr. Fresenius said the work was to be literally translated, there was no help for it, the old notation must be used, but still we must consider that it is a great mistake. Generally speaking, at the present day for a book to be published in the old notation is sufficient to limit its use to a very small number of students. It certainly seems a great pity that this, which until lately has been looked up to as the best and most reliable text-book on qualitative analysis, should not have progressed side by side with modern chemistry; for English chemists, almost without exception, have adopted the new system of atomic weights, and the new and more systematic nomenclature now in use. The adoption of the old notation in the present volume will add a considerable amount of trouble to the teacher's work, and in many cases may probably lead to the adoption of another text-book. Looking at the very general, in fact almost universal, use of the new notation both in England and on the Continent, it certainly appears that this book is about five years behind the times. On the other hand, the present edition will, perhaps, on this account prove more useful to our manufacturing chemists, who seem very loth to adopt the new notation.

Although we do not feel satisfied, and, in fact, are disappointed, with the book in this respect, we cannot help feeling pleased with the substance matter itself. As might be expected from the numerous editions of his manual which the author has already published, he continues to keep the information contained in the work quite up to the progress of the science. Many parts of the work show alterations, though the very fact of the accuracy of the author's work precludes any very great change. It is in the parts on the rare metals, and on the alkaloids, that there appears to be the greatest amount of new matter. Thus the sections on berylla, thoria, zirconia, tellurium, vanadium, iridium, and didymium, show a considerable increase in our knowledge of these substances; whilst indium, which is not to be found in the edition of 1864, has here received a very good notice. Again, in the edition of 1864, only one method is given for the separation of lanthanum and didymium, but in the present there are no less than four distinct methods given. On the subject of the alkaloids, we notice three new articles on digitaline, picrotoxine, and atropia, which in the previous edition are not noticed. In the portion which treats of the acids, and in Part II. "On the Course of Analysis," there does not appear to be much alteration; but we must not omit to mention that the index to this edition is far more complete, and in every way better than in the previous editions. The general plan of the work is too well known to need any detailed account, and the number of editions through which it has passed is a sufficient guarantee of its usefulness and trustworthiness. A few new illustrations have been introduced, and a new table of spectra, but in the general style and plan of the book there has been no alteration. For our own part, although we have a great admiration for Fresenius's book, more especially as a work of reference, we scarcely think that his system is perfect for educational purposes, and perhaps not so good as those of some others, such as Valentin's or Galloway's; no doubt a student working conscientiously through the work under review will be able to make good and correct analyses, but we doubt whether he will learn much beyond the mere analytical details, for in this book there seems little room for the student to use his powers of originality, and nothing to stimulate him to reason, from his accumulation of facts, to general principles. There appears about the book almost too much of the system of telling this and showing that, for the book to be perfect as an educational agent, and we fancy that better results in this direction may be obtained from works which give more opportunity and encouragement for original and individual reasoning; and this we believe is the case in the two other works we have mentioned, as they tend to exercise and strengthen the student's originality, and will at the same time give him as full and complete a knowledge of qualitative analysis as he would obtain from Fresenius's book.

OUR BOOK SHELF

Verhandlungen des naturhistorischen Vereines der preussischen Rheinlande und Westphalens. 28er Jahrgang. 1te u. 2te Hälfte. (Bonn, 1871.)

THE volume of these Transactions for 1871 opens with biographical memoirs of Wirtgen and Haidinger. S. Simonowitsch contributes a paper on the Bryozoa of the Greensand of Essen, illustrated by four lithographic plates, which is introduced by a critical account of the anatomy and systematic position of the Bryozoa. From Prof. Förster we have a Review of the Genera and Species of the Family of Plectiscoideæ. F. G. Herrenkohl follows with a list of the Phanerogams and Vascular Cryptogams of Cleve and the neighbourhood. R. Bluhme gives a series of analyses of the water of different wells in the vicinity of Bonn, compared with that of the Rhine. In addition to these papers printed at length, a large number of other subjects connected with medical and natural science are treated in the Reports of the Proceedings of the Lower Rhine Society for Natural History and Medicine. Among these we may refer especially to a valuable paper by Dr. Brandis on the climatic conditions which principally affect the growth of forests in the British East Indies. The Indian climate is characterised by its long period of uninterrupted drought; and where the rainy season falls in spring or autumn, the summer heat is excessive. Where, however, the rainy season falls in the summer, as is the case in Burmah, Bengal, and a portion of Central India, the climate presents the peculiarity that the hottest period is in the spring, from March till May and the commencement of the monsoon, and again in the autumn, Calcutta having again a comparatively cold winter. The great obstacle to the growth of forests is the prevalence of fires towards the close of the dry season, which do incalculable damage every year; but of late years something has been done to limit their ravages. The growth of tree vegetation is extraordinarily rapid in India when young, but the forests do not eventually attain such luxuriance as in Ceylon, Brazil, and some extra-tropical countries.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Oceanic Circulation

THE letters of Prof. Everett and Mr. Wallace (NATURE, Aug. 22) establish one point which must go a good way towards the settlement of the disputed question of the cause of oceanic circulation, viz., that in order to maintain the bare mechanical possibility of the gravitation theory, it is necessary to assume that water is so nearly quite devoid of molecular resistance to motion that, were it not for the impediments offered by continents, water flowing from a low to a comparatively high latitude would be revolving eastwards with the velocity of an arrow. In the southern hemisphere, where continents are "few and far between," and where a comparatively open channel exists through which the waters may circulate round the globe at any velocity without much impediment, this rapid general eastward motion of the ocean ought to be developed to a large extent. But the fact remains that no such motion has ever been observed. Dr. Carpenter says:—"It is well known to navigators that there is a perceptible 'set' ot warm surface water in all the southern oceans towards the Antarctic Pole; this 'set' being so decided in one part of the Southern Indian Ocean as to be compared by Capt. Maury to the Gulf Stream of the North Atlantic" (NATURE, March 24, 1870). This general motion of the water in the southern hemisphere Dr. Carpenter adduces as strong evidence in favour of his theory. But why is not the "set" as much to the east as to the south? If the presence of the Antarctic continent does not hinder the motion of the water polewards, why should the presence of the continents of Australia or the southern portion of South America hinder the motion of the water eastward, seeing that rotation performs about 1,500 times more work in deflecting the water eastward than the difference of specific gravity performs in impelling the water southward? The very fact that the water does not turn to the east but moves straight towards the Antarctic continent, shows that the waters must be impelled by a force immensely greater than that derived from difference of specific gravity, because it must be greater than that derived from rotation, or else the "set" would be as much to the east as to the south. There are, it is true, a few currents in the southern hemisphere with an eastern motion, but these the advocates of the gravitation theory would call "mere surface drifts produced by the winds." Besides the majority of the currents in that hemisphere move in wrong directions to be explained either by difference of specific gravity or by rotation.

That the explanation given by Prof. Everett and Mr. Wallace does not even touch the difficulty which besets the gravitation theory, far less removes it, will, I trust, be further evident from the following considerations, viz., a current in mid-ocean a thousand miles from land, flowing from a low to a higher latitude, has its eastward motion due to rotation as effectually checked and diminished as though it abutted against a continent. This retardation cannot be attributed to the presence of continents, for it occurs equally the same whether the land be one thousand, two thousand, or five thousand miles to the east. It is the resistance of the molecules of the water through which the current moves that destroys the eastward motion. No matter how slow the current may flow polewards, by the time the water reaches, say latitude 60°, each pound has lost at least 9,000 of the eastward velocity which it possessed when it left the equatorial regions. It is a matter of indifference in what way this energy is consumed by the molecules of the water, whether it be in friction in rotation, or whether it becomes potential in the raised water through which the current flows; for in either case it is the resistance offered by the stationary molecules which causes the moving molecules to lose their velocity. The resistance being molecular, that which holds true of eastward holds equally true of westward motion. This is proved also by the fact that a current flowing from a higher to a lower latitude has its westerly motion due to rotation as effectually checked and diminished as a current flowing from a lower to a higher latitude has of easterly. And what holds true of motion to the east or to the west, holds equally true of motion to the south or north, for there is no reason why the resistance should be less in one direction than in another.

It therefore follows that it is impossible that 6 foot-pounds could impel a pound of water from the Equator to latitude 60° against the molecular resistances to its motion, when during the passage of the pound of water it requires 9,000 foot-pounds to overcome the resistance to the easterly deflections which take place. Or if the molecular resistance of water be so infinitesimal that 6 foot-pounds is sufficient, then it is impossible that molecular resistance could consume 9,000 foot-pounds during the easterly deflection which takes place.

I respectfully submit that this is a clear and obvious demonstration of the mechanical impossibility of the gravitation theory of oceanic circulation.

Prof. Everett says that Mr. Ferrel's argument from the tides is quite conclusive in showing that the forces arising from difference of temperature are of sufficient magnitude to keep up an oceanic circulation. If. Prof. Everett, like Mr. Ferrel, really supposes that a slope produced by the moon is the same as one produced by difference of density, and that the process by which the water tends to regain its level is the same in both cases, I am not surprised he should consider Mr. Ferrel's argument conclusive.

I beg to refer Mr. Wallace to the *Philosophical Magazine* for October 1871, p. 244, for an explanation of the fallacy of Dr. Carpenter's famous experiment to which he alludes.

Had the present state of my health permitted, I should have entered somewhat more fully into some of the above points, but in the meantime I must withdraw from any further discussion.

Edinburgh, August 27 JAMES CROLL

The Aurora of Feb. 4

THE Hon. Rawson Rawson, Governor of Barbadoes, has favoured us with the following note:—

"Memo. for his Excellency the Governor-in-Chief.

"The aurora of the 4th of February last, to which you allude, and notices of which you kindly sent me in NATURE, was seen here, and caused much concern.

"I was not myself an eye-witness, but I have descriptions of it from trustworthy persons. I was first informed of it by a servant of mine, who has the overlook of Mont Grace.* He was returning to his home near the Fort after 7 P.M., when, about a mile away from Mont Grace, his attention was arrested by what he imagined was a 'great fire'—the trees at Mont Grace and all about the yard were lighted up and clearly seen. My brother, who leased a sugar plantation in the neighbourhood of Government House, and Mr. Gordon, who owns one in that immediate neighbourhood, happened to be both in Scarborough at the time, and, seeing the 'great glare,' were both seized with the notion that their respective properties were on fire, and hastened out to them to find that the supposed fire was farther away.

"Mr. Taylor, master of the barque *Tobago*, was riding at anchor at Courland Bay,† and was a witness of this aurora. He described it to me as of 'a dark-red colour, extending half way up to the zenith, and very brilliant, its situation being about N.W. by W.' The labourers exclaimed that St. Vincent was on fire.

"This aurora lasted till half-past nine. Such a phenomenon, if not altogether unknown in this latitnde, is at any rate very rare.

"Tobago." "DUGALD YEATES"

* Mr. Yeates' property, three miles from Scarborough, Island of Tobago. (Tobago is in 11° 9′ N. lat., and 60° 12′ W. long.)

† On the opposite side of the island, *i.e.*, the northern side.

The Solar Spectrum

I HAVE lately obtained and read "Schellen's Spectrum Analysis," translated by the Misses Lassell, and edited by Mr. Huggins, and feel at length constrained to dissent from a statement which I there find—in this the present standard work on the subject—distinctly and repeatedly made, as I have seen it made elsewhere before, a statement belief in which has tended and must always tend to deter many from prosecuting independently a most interesting study. I refer to the passage beginning "The Possibility of Observing" (p. 382) to end of paragraph, italicising the words "ordinary" in l. 7 of p. 383 'by increasing the number of prisms" three lines below, "highly dispersive power" in line 22.] The italics are mine, and are intended to indicate that to which I object, not that the particular passages in which they occur are explicitly incorrect, but that they implicitly convey the incorrect notion, that the "highly dispersive power" is essential to the primary success of the observation "of the lines of the prominences in bright sunshine."

The reason of my objection will be found in the following extract from an unpublished letter dated May 3, 1869:—

"I think it will surprise you to hear that I have just seen Mr. Lockyer's three bright solar lines at several parts of the sun's circumference with the Royal Society's telescope and spectroscope *without any appliances or devices whatsoever (sic)*, and that with the greatest ease and certainty. Had I merely *looked* for them, or for anything of the kind, a twelvemonth ago, I do not see how I could have failed to see them!

"When the slit is placed parallel to the limb, the red line is vivid across a bright solar spectrum, and the line near D (there is no doubt about its position when seen in connection with the solar spectrum) which is less prominent, as also the line at or near F, are easily seen."

Also in another letter of the same date:—"Before I went into camp last December (while still rumours only of Janssen's observations were current) I resolved to try with coloured glasses. They were not received till too late; the instrument was packed up, and I was away. By the time I returned the question had passed on; but I still wished to carry out original intentions, and prepared accordingly, and was proceeding to direct the telescope this morning when I saw the red line in the undefended part of the slit, where I was focussing on the sun's limb. Of course, I saw at once that I could do without my coloured glass, which, practically, limited my field of view, and accordingly removed it, and examined various parts of the limb with no screen at all. At nearly all I could see the same three lines. At one place the red was so bright that an outsider looking in at the spectrum would certainly have carried away the impression of a coloured ribbon with a bright line of red near the end." As a matter of fact my wife had no difficulty in seeing at any rate the red line.

Now, the application of this is to be found in the fact that the spectroscope in question contained a single equilateral prism and no more.

Were there any merit in a fortuitous discovery of the kind, it would suffice for me to declare that I was in complete ignorance of the methods and appliances by which MM. Janssen and Lockyer had succeeded in doing long before that which I now found so obvious. What I do insist upon is that the visibility not of the prominence *lines* only (see also Proc. R.S., No. 113, 1869, in this connection), but of the prominences themselves does *not* require a high dispersion. I have a fair acquaintance with prominence forms; but it has been derived almost entirely from a study of them with *an open slit*, the use of which I had learnt long before, and a *single prism* spectroscope.

Of course, I do not contest that the power of extended examination depends directly upon increase of dispersive power, only that the *lower* limit is fully attained by a single 60° prism.

That high telescopic power is not essential either is proved by the fact that I have examined prominences by applying a $1\frac{3}{4}$ in. object glass to the end of the sliding tube of the spectroscope at solar-focal distance, and using the instrument on its "soirée" stand on a table—by way of experiment—with an amount of success which, in 1868, would have made some sensation.

It is obvious to remark in reply that probably the explanation of the ready visibility asserted is to be found in more favourable climatic conditions. I cannot admit it. By night, it is true, there is often a remarkable translucency; but the dusty, agitated state of the atmosphere (in May) under a tropical sun, and at an inland station, can certainly not be considered favourable for observations of this particular character. And even were such an explanation in some measure correct, clear skies are not so unknown in Europe and America (nor indeed in England) that the illumination of the atmosphere can be broadly said to "completely overpower" that which "in an ordinary instrnment" is by no means overpowered in India in ordinary states of the sky.

The erroneous notion to which I refer has been promulgated again and again. It is unnecessary that I should specify the various passages—in lectures and elsewhere—and ultimately in Mr. Proctor's work on the Sun*: but having now found it so distinctly enunciated in the above cited passage, and elsewhere, in "Schellen," the time seemed to have come when it ought no longer to pass unnoticed; the more so as I have never been able to understand the real reason why the momentous discovery was not made earlier. It has been said that *knowing where to look*, the main difficulty was overcome. But two days after my first experience of these three known lines, I recognised the presence of three more hitherto unknown ones—and subsequently of a seventh. From that time to this however I have not seen any others, with the same dispersive power.

If asked how it was that, with the very same power at command, I had not myself seen them before; I need only reply that I had small leisure by day, and was under the impression that the experiment had been fruitless in more experienced hands—the identical reason which P. Secchi has given for a like remissness, in this very matter.

Bangalore, Aug. 25 J. HERSCHEL

Botanical Terminology

I VENTURE, as no one else has done so, to make a few remarks on Mr. Kitchener's letter.

I suppose the necessity will not be denied of employing some technical terms in studying subjects which do not fall under ordinary observation, and for the discussion of which ordinary language is consequently insufficient. When these technical terms are first devised, it is natural, indeed unavoidable, that they should reflect the scientific ideas current at the time. But inasmuch as knowledge progresses, we find ourselves, sooner or later in every branch of science, in the predicament of having to give effect to new views in terms which are an inheritance from old ones. We are able to do this because things themselves remain the same though our ideas about them change, and the names they once received with an intelligible meaning have now become purely arbitrary. No man bearing the name of, say, Baker, would probably change his name because he did not make bread. Nor do chemists discard the term oxygen because there are acids of which it is not a constituent. In the same way the morphological analogy implied in the use of the term "ovule," in the case of plants, is undoubtedly incorrect, but any one must have a singularly tender conscience who would object to it on that ground.

To save, therefore, confusion, and preserve uniformity in scientific literature, there is a tacit convention to treat in a great many cases as arbitrary terms words which once implied acquiescence in a theory. That a word in common use belongs to "a pre-Adamite stage of botanical knowledge," as Mr. Kitchener calls it, is not, I take it, sufficient ground for replacing it with another if there is no ambiguity in its application.

Next I would remark that Mr. Kitchener appears to me to have an exaggerated notion of the copiousness of botanical terminology. The number of terms really indispensable is not large. For example, he speaks of the troop of words ending in "tropous." Was this particular noun of multitude suggested by the termination? because as a matter of fact the troop consists of three. Prof. Henslow found no difficulty in teaching the terms contained in Prof. Oliver's Lessons in Elementary Botany to girls in a village school. Surely the Rugby boys cannot be less apt.

That Professor Henslow succeeded seems to dispose of the objection that a knowledge of Greek is "a necessary *open sesame* to the correct remembering and spelling of botanical terms." To teach these terms as "unintelligible gibberish" is only what in any case must be done with whole hosts of words not very different in form. Why should it be insuperably difficult for a boy, even if ignorant of Greek, to remember spell and apply the term hypogonous when he cannot possibly evade sometime or other having to face hypothesis, hypochondria, and hypocrisy, to say nothing of hydrostatics, hydraulics, hydrogen, and hydrocephalus?

* See particularly p. 286, footnote.

I can see no reason why, as Prof. Henslow was in the habit of doing, technical terms carefully reduced to the smallest number absolutely required (and text-books bristle with unnecessary ones) should not be taught to boys as mere arbitrary names. Syngenesious, as a mere matter of taste, seems to me preferable to "united by dust-pouches."

If this be done, Mr. Kitchener's further difficulty as to "gamogenetic analogy" disappears.

The teacher, of course, may himself reasonably exercise some liberty. Thus no one would, I suppose, object to quincuncial being expressed by $\frac{2}{5}$, though quincunx is to be found in any dictionary, and is a word for which botanists are not responsible. Again, the suggestion to express by a fraction the depth of leaf-incision is really commendable, even to technical descriptive botanists.

October 1 W. T. THISELTON DYER

The Hassler Expedition

UNDER this heading in your number for August 29, p. 354, is this sentence, "One lesson I must confess to having learned at Indefatigable Island (Galapagos). I saw there indisputable proof that the surf of the sea is capable of rounding angular fragments of lava into pebbles somewhat resembling in shape (but not at all in polish and grooving) glacial boulders. I had always from boyhood doubted the power of the sea to make angular fragments round. I had supposed that the action of the surf upon such fragments would be simply to pack them into a sort of McAdam's roadway. And even now, having had the proof that under peculiar circumstances the sea can make a tolerable imitation of drift, I am not a whit more ready to believe that the sea made the drift itself. You may prove to me experimentally that flour can be made from wheat with a pestle and mortar, but that will not convince me that the flour markets of the world are thus supplied."

If the countless myriads of tons of beach on the shores of this globe could be passed through the hands of this writer, he would not detect a single "angular fragment" (McAdamised) among them. On the shore each lump of rock is successively worn into a boulder, each boulder into a pebble, and finally each pebble into sand. This is the main source of the sand which lies between the beach and the ooze-bed of the ocean.

But the sea-shore factory of boulders and drift is not the only factory, or even the largest factory of boulders and drift. The rocky gullet is the main boulder factory. Lyell (Principles), speaking of Etna, attributes "the enormous rounded boulders of felspar, porphyry, and basalt, a line of which can be traced from the sea from near Giardini, by Mascali and Zapharana to the Val del Bove" to one flood of melted snow. The valleys of the low part of Teneriffe, away from the Peak and near Santa Cruz, are almost all dry except in rain. The beds of the upper parts of these valleys are sheer rock, the middle parts wear the appearance of torrents of boulders, the lower parts are *alluvial plains* of boulders, and opposite the mouths of these valleys are very commonly deltas and bars of boulders. Behind these bars, after each rain, large deposits of earth and sand are formed which the people collect diligently. Where permanent streams exist, they are usually lost at a considerable distance above the mouths of the valleys. That is, except in rains, they percolate to the sea beneath the plains deltas and bars of boulders.

From the sides, hundreds or thousands of torrents of boulders fall into these rivers of boulders. Sometimes these lateral shoots have formed barriers of boulders across the main valley behind which large beds of boulders and earth have accumulated, again to be cleared out and thrust down to the sea-shore by heavy longitudinal rain floods.

So, in Madeira, who does not know the sea-shore boulders of the Praya-Formosa? and for fresh water boulders, the stream at Funchal brings down such a crop at every flood as to choke the channel through its delta of boulders, and unless the channel is kept clear of them artificially, the lower town is subject to the most disastrous inundations.

I mention Teneriffe and Madeira because, like the Galapagos, they are deep-sea volcanic islands. Their surfaces have been ejected when they were already above the sea, and they have been coated and re-coated thousands of times by floods of melted rock when they had long been *sub dio*. So that I conclude that even Agassiz would not attribute the moulding of their surface to the "Glacial epoch." But leave volcanic islands or volcanic mountains out of the question, there is not a mountain stream or streamlet in the wide wide world which is not at this moment a boulder-factory. Take Europe; in Scotland, Switzerland, and Norway you may see the whole of the hill-side streaked with streams of boulders. They are hurled into Romsdal now by every cascading river and rivulet or dry gullet which scores its magnificent mountain sides when flooded by rain or its equivalent melted snow. Every cascade of water above forms a cascade of boulders below ending in a somewhat vertical triangle or delta of boulder talus. That is, these triangles or deltas of boulders are horizontal where there is room, while they approach the vertical directly as the narrowness of the valley and the consequent steepness of its sides.

Distinct from the clays resulting from atmospheric disintegration, this inland grinding of rock into boulders and pebbles is the main source of the sand which is found mingled with boulders in the parallel terraces into which raised *marine* alluvial plains are cut, and of the *inland* parallel terraces on the opposite sides of each soft valley above each hard gorge. It is the source of the sand of the Scottish Kames and of the Irish Eskers. The so-called northern drift and glacial drift are the combined result of atmospheric decomposition and marine and inland grinding of rock, sized and sorted by water.

This is all going on now, as it ever has gone on *qualis ab incepto* and according to the *fortuna locorum*. That is according to the circumstances of the *place*, not the *period*. And nothing can be more absurd than the expression a "drift period" or a "boulder period" or a "pluvial period" or a "diluvial period" or a "gravel period" or a "period of invertebrates" or an "age of reptiles," or other mistakes between place and period.

GEORGE GREENWOOD

Brookwood Park, Alresford, Sept. 14, 1872

P.S.—Since this was written, I have had the honour to receive from the Smithsonian Institution the Report of the Survey of Wyoming, by Mr. Hayden, United States Geologist.

As far as I have read, he appears to attribute the moulding of the earth's surface, after upheaval, not to glacial but to atmospheric agency and the erosion of rivers. With regard to the inland grinding of rock into boulders, pebbles, and sand, he describes, page 14, the "worn masses of iron ore" "in the bed of the Chugwater," and ends thus: "thousands of tons have been washed down to the valley of the Chug and distributed among the superficial drift. As we leave the ore beds themselves, these strong masses are larger and more angular, and as we pass down the Chug they dwindle to minute pebbles and disappear."

An Entomological Query

I FIND the subjoined note in the recently-published "3e Livraison of Fauvel's Faune Gallo-rhénane; Coléoptères, p. 11. Will some entomologist kindly say whether Fauvel's observation has been since verified or not? If correct, it is one of the most curious of the many curious phenomena connected with beetle-life in formicaries.

"J'ai remarqué ailleurs (Bull. Soc. Linn. Norm. 1861, v. 252), que, sur un assez grand nombre d'individus capturés dans les fourmillières, il ne se trouvait pas un seul ♂. J'engage les entomologistes à vérifier ce fait, si l'occasion s'en présente. Il peut avoir de l'importance au point de vue des mœurs peu connues de nos espèces myrmécophiles."

The note has special reference to *Micropeplus staphylinoides*.

W. W. SPICER

Itchen Abbas, Alresford, September 28

Cats' Teeth

DOUBTLESS the case mentioned by Mr. Lydekker is somewhat unusual; but the mere fact of an animal possessing an extra tooth can hardly upset Prof. Owen's theory. It is by no means an uncommon thing to meet with examples of supernumerary teeth in man, and these rarely disturb the arrangement of the others, (mostly occurring on the palatal or lingual sides of normal teeth), I do not therefore see why (judging from analogy) it should be very unusual for the lower animals to possess like peculiarities, although they are not often met with on account of the limited number of skulls examined. Mr. L. does not tell us whether the extra tooth occurs in the superior or inferior maxilla.

4, Finsbury Square W. G. RANGER

PHOSPHORESCENCE IN FISH

WHILE off the Land's End, Cornwall, or between the "Wolf Rock" and "Longships" Lighthouses, in the screw-steam-ship *Cumbrae* (*ex* Plymouth for Belfast and Glasgow), on the night of Thursday, August 27, my attention was directed to one of the most beautiful marine phenomena that could well be imagined. At some distance ahead of the vessel the sea appeared quite luminous over large portions of its surface. This luminosity, observed at intervals, on a nearer approach proved to be nothing more nor less than the phosphorescence of immense shoals of fish—mackerel or pilchards, probably both—which could be distinctly seen near the surface; they of course appeared somewhat large, owing to the light which they emitted.

It was a dark, rough night, a strong breeze blowing off the Atlantic at the time; and as a consequence, the vessel, as may be supposed from her description, was lurching and pitching considerably—in fact more so than I had ever experienced in any other vessel, or on any previous occasion.

Having taken up a position on the forecastle, and secured myself by a tight hold on the stanchions immediately over the bow, I watched these fish with intense interest—so much so that at times I could scarcely restrain myself from a loud burst of laughter, so exceedingly interesting were their movements. As the vessel rolled and dipped, these fish, evidently startled by her movements, could be seen near the surface, ahead and on the starboard and port bow, darting forward in quantities as close as I should think it was possible they could well swim together. It was a sight long to be remembered.

I may add, that as the sea broke over the fore part of the vessel, the spray rested on me in drops or globules of, as it were, fatty matter, and much resembled in its luminosity, which lasted for some time, the appearance of so many glow-worms; doubtless this was given off by the fish themselves.

Some interesting particulars of the nature of phosphorescence in fish appeared in NATURE (Notes) vol. iv. p. 287 (Aug. 10, 1871), as presented in a memoir to the Association of Naturalists and Physicians at Turin, by Sig. Panceri of Naples, from which I extract the following:—

"The phosphorescent substance in fishes, in whatever part of the body it may be situated, is always fat" (this bears out my former remarks) "and the phenomenon is due to its slow oxidation in contact with air."

Further particulars appeared in NATURE, vol. v. p. 132 (Notes), December 14, 1871, as derived from the same author, of which the following is also an extract:—

"In all cases the phosphorescence is due to matter cast off by the animal—it is a property of dead separated matter, not of the living tissues.

"He" (Sig. Panceri) "also finds that this matter is secreted by *glands*, possibly special for this purpose, but more probably the phosphorescence is a secondary property of the secretion. Further, the secretion contains epithelial cells in a state of fatty degeneration, and it is these fatty cells and the fat which they give rise to which are phosphorescent. It is due to the formation in decomposition of a phosphoric hydro-carbon, or possibly of phosphuretted hydrogen itself."

Are there any special conditions of weather, or season, during which this phenomenon of phosphorescence is more readily observed than at others? Although by no means a stranger to the sea, I have never, on any occasion, seen anything approaching to it.

I made a trip from Plymouth to the Eddystone Lighthouse and back on the previous night, and although nets were out (as known by their floats) at some distance from land for the purpose of securing the fish that I have mentioned, no phosphorescence of the kind was to be seen; the sea on this occasion was comparatively smooth.

JOHN JAMES HALL

ON THE RETENTION AND COLOURING OF EGGS, AND THE PROTECTIVE MIMICRY OF SOUNDS

1. IS it known for how long a time a bird possesses the power of retaining its egg?

Last summer, from the number of nests in this neighbourhood, the writer was able to study the habits of kingfishers (*Halcyon vagans*) with more facility than usual. The movements of one pair excited much interest. On the 19th October this pair were observed to be busily engaged in excavating a home in the back of the turf chimney of an empty cottage. After many days spent in hard labour, this was abandoned; subsequently several tunnels were commenced, in some of them considerable progress was made; then they were in like manner deserted. The seventh resting-place, begun November 26 (there must still be a witchery about number seven even at your Antipodes), was finished, occupied, and therein, on December 24, a brood was hatched. Can there be reason to doubt that the eggs in the ovary of the female must have been in a forward state in the third week in October? At the close of that month the first egg to be laid must have been ready for extrusion. From personal observation we know that our kingfisher lays nearly every morning till the clutch of eggs is completed; the number of eggs to a clutch varying from five to seven. Here we have a bird engaged in laborious, almost incessant exertion, for quite six weeks, physically in a condition analogous to that of a pregnant animal. Three of the homes excavated and abandoned were so far finished that the chamber was hollowed out, so that a deposit of eggs must have been imminent on three occasions during that period of six weeks. It is well known that the domestic fowl, on a change of quarters, will, in its strange home, sometimes retain the egg for hours beyond the usual time of laying, often depositing what is called a double-yoked egg, but we have to do with the freedom of wild nature. It is easy to suggest that our kingfisher relieved itself by dropping its egg; obviously that would be opposed to the marked instinct of so persevering and painstaking a nest-builder; besides, would that mode of acquiring ease be twice repeated by a bird that endured such toil to make a hiding-place for its progeny—toil only to be appreciated by those who have watched its daily work?

2. Can a bird influence the colour of its eggs protectively?

A proposition that few physiologists would answer in the affirmative, yet naturalists have held, perhaps still do maintain, diverse opinions as to the cause of abnormally coloured eggs. The following facts are laid before your readers for information:—Rather late in last summer a female bittern (*B. poicilopterus*) was slightly wounded and secured. It was kept within a *grassy* enclosure. While thus confined it laid an egg of a pale bluish green colour, precisely like that of a heron. The egg of our bittern is about the same size; its normal colour of a similar olivaceous buff as that of your *B. stellaris*. This buffy olivaceous tint harmonises well with the half-faded leaves of aquatic plants of which the nest is often built, such as those of *Typha angustifolia*, *Carex virgata*, &c.; in fact, a bittern's nest is by no means an offensively obtrusive object.

Having had eggs from several nests under observation, I have noticed that bittern's eggs do now and then vary in tint from buffy brown to pale olivaceous; but in no case approximately to that blue green of the heron's egg.

In the instance cited, was the peculiar colouring used as a means of securing for the egg the protection of the *verdure of the grass* in which it was deposited? or was it merely the effects of a brief confinement and a slight local wound? The conditions under which this egg was laid may be considered as somewhat analogous to those under which the cuckoo laid No. 26 specimen in the aimous series of eggs formed by Herr Baldamus (see vol. f.p. 508); nor is the occurrence of this peculiar-looking bittern's egg without its use in estimating the value to be accorded to certain abnormally coloured eggs as illustrating and supporting a theory not adverse to the proposition—Can a bird influence the colour of its eggs protectively?

3. Are the eggs of the cuckoo ever approximately coloured like those of its dupe for protective purposes?

In vol. v. p. 501, may be found a brief note, stating that the eggs of our whistler, or small cuckoo, were not coloured approximately to those of its dupe, nor, indeed, would such precaution appear necessary, when the form of the nest of its victim was considered. Last season one of the writer's children brought in a nest of the blight bird (*Zosterops lateralis*) containing four eggs, one of which was a puzzle indeed; it was found on comparison, that although a shade darker in colour, it resembled the rest of the eggs in the nest, pale green-blue, spotless and unstained as those of the homely russet-clad hedge-sparrow. In size and shape it was like that of the small cuckoo. Hundreds of eggs of the *Zosterops* (a new colonist, yet already one of our commonest birds) have passed under the notice of the writer, but none have resembled the specimen alluded to. That it might possibly have been laid by a whistler seemed at length the only solution of the problem, how an egg of that size and shape came into that nest. The *Zosterops* does not belong to the purely indigenous genera of New Zealand; like the *Chrysococcyx* and its usual dupe, the *Gerygone*, it is to be found in other colonies far beyond the bounds of ferny Maori-land. It builds a suspended nest, another indication of its foreign origin. It is quite likely that in warmer climes the small cuckoo may readily avail itself of the advantages presented by this mode of construction, as ensuring a greater degree of safety from reptilian egg-robbers. The open cup-shape of the *Zosterops'* nest would disclose to its owner the marked contrast between its own clear blue-green eggs and the large, greenish-dun egg of the parasite; hence the effort at protective mimicry. This would be unnecessary, as before pointed out, in the dimness of the domed structure of the *Gerygone*. If the egg described is that of the small cuckoo, it is the first instance known to the writer of the *Zosterops* being used as a dupe. It should be noticed that last season the small cuckoo appeared in greater numbers than usual in this neighbourhood, where the nests of the blight bird, in the aggregate, now outnumber those of every other species of bird.

These facts are communicated under the impression that they may be of interest to ornithologists, and in fairness should not be withheld, the rather as the writer does not yet give in his adhesion to the theories of Herr Baldamus.

4. On the Mimicry of Sounds.

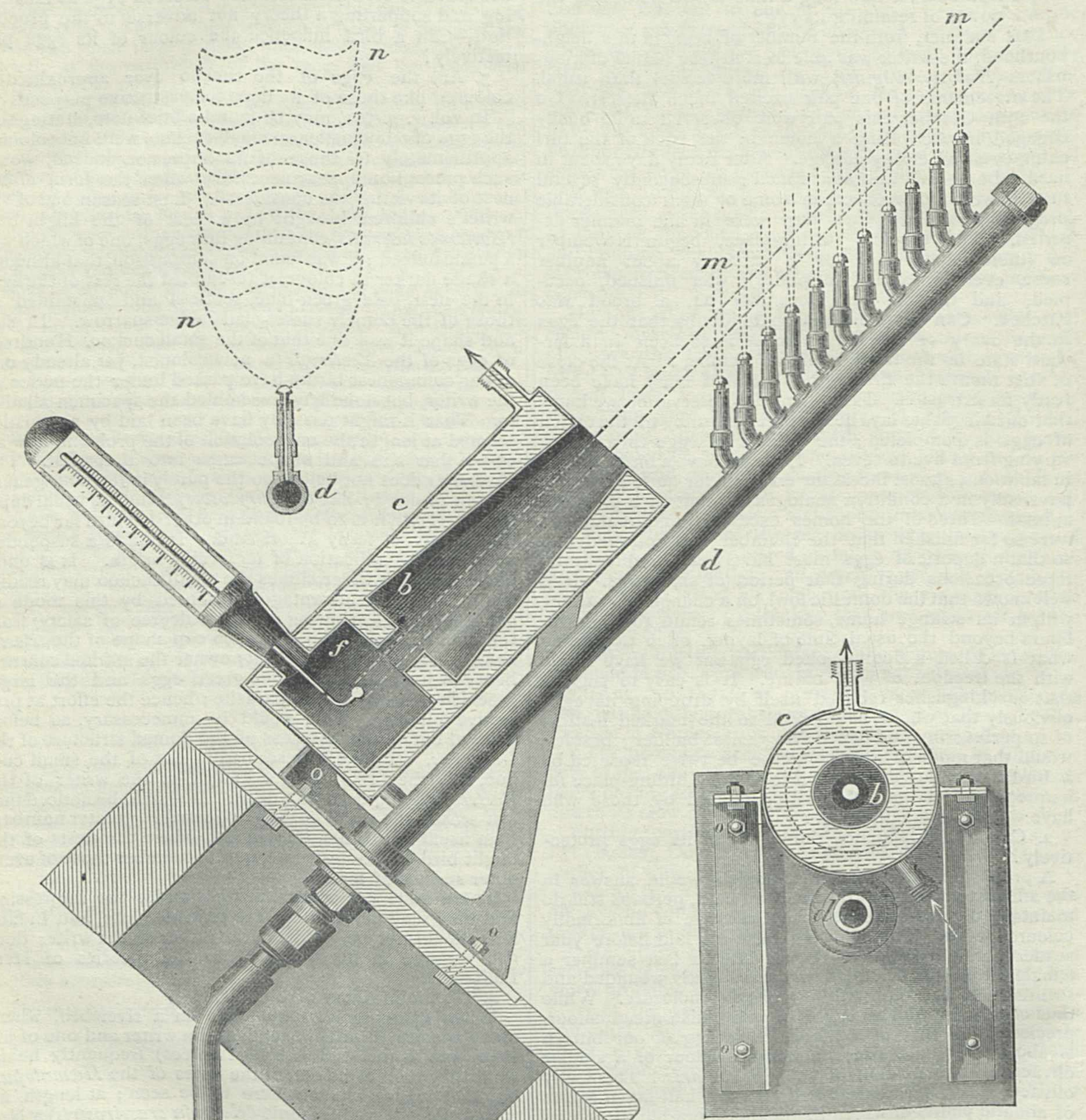
When camping for some days on a river-bed, where many species of birds abounded, the writer and one of his sons (well acquainted with bird voices) frequently heard what they took to be one of the notes of the *Hæmatopus*, but that wader was nowhere to be seen; at length we traced the call to the Piopio (*Keropia crassirostris*) a bird with feeble powers of flight, yet one that delights in the open glades of river-beds. The mimic cry was always given when near to a stream, just where the red-bill (*Hæmatopus*) would be likely to be found. A pair of red-bills can drive away a hawk; now a hawk, "from his place on high," perceiving something near the water, might forego its swoop on hearing the mimicked note of the wary, yet bold red-bill. We have observed our grey warbler give an exact imitation of the cry of our common tern (*S. antarctica*) one of the boldest birds in defence of its young.

THOMAS H. POTTS

Ohinitahi, June 29

THE SUN'S RADIANT HEAT

THE readers of the *Comptes Rendus* are aware that Père Secchi addressed a letter to the Academy of Sciences at Paris, some time ago (Comp. Rend. tome lxxiv. pp. 26–30), containing a review of my communications to NATURE, published July 13, October 5, and November 16, 1871, in which he questions the correctness of the reports which I have published containing tabulated statements of the temperature produced by solar radiation. His reason for questioning the reliability of my



tables, appears to rest on the supposition that my instruments do not furnish correct indications. "It is astonishing," he says, "that Mr. Ericsson should find with his instrument a higher stationary temperature in winter than in summer. This (even bearing in mind the greater proximity of the sun in winter) makes me think that there must be something very singular in his apparatus, possibly making all its indications deceptive. Even under the beautiful sky of Madrid, M. Rico y Sinobas found, in

December, for the solar radiation, 12^{div}, by his actinometer, and, in June 25^{div}, 56"." It is not my purpose to prove the fallacy of M. y Sinobas' actinometric observations; but I think "that, there must be something very singular in his apparatus," since in North America in lat. 40° 42' (the latitude of Madrid is 40° 24'), solar intensity at noon during the latter part of June is 64°·5; while when the sky is clear at noon during the latter part of December, the temperature under similar atmospheric conditions, reaches 58°·7. But observations made in the morning or evening during the month of June at the hour when the sun's altitude is the same as at *noon* in December, show that the intensity of the radiant heat in June is only 53°·08 against 58°·7 in December. Actual observations have thus established the fact that for *corresponding zenith distance*, the temperature produced by the radiant heat when the earth has nearly reached perihelion, is 58·7—53·8=4°·9 *higher* than at midsummer. Referring to the table published in NATURE, vol. v. p. 47, it will be seen that, owing to the greater proximity of the sun, the increase of absolute intensity of solar radiation is 5°·88 Fah. during the winter solstice. Père Secchi will do well to examine the subject more carefully, and make himself better acquainted with the character of the investigations which had led to an exact determination of the temperature produced by solar radiation.

The readers of the *Comptes Rendus* who have examined the review referred to, ignorant of the contents of the articles in NATURE, will be surprised to learn that I have not, as the reviewer asserts, questioned the power of vapour to diminish solar intensity. Having stated the result of numerous observations of the sun's radiant power at corresponding zenith distance, and proved that the temperature during midwinter is higher than at midsummer, I made the following remark in NATURE, Nov. 16, 1871: "In the face of such facts it is idle to contend that the temperature produced by solar radiation under corresponding zenith distance and a *clear sky* varies from any other cause than the varying distance between the sun and the earth." It is absurd to suppose that a person having devoted many years to the investigation of solar radiation should deny the retarding influence of vapour, since not one observation in a hundred indicates maximum solar intensity, owing to the presence of vapour in the atmosphere.

The following brief description of the actinometer will show that there is not, as Père Secchi supposes, anything very singular in this apparatus tending to render its indications deceptive. The principal part of the instrument consists of an air-tight cylindrical vessel, the axis of which is directed towards the sun, the upper end being provided with a thin lens covering an aperture of 3in. diameter. The bulb and part of the stem of a mercurial thermometer is inserted through the upper side, at right angles to the axis; a small air pump being employed for exhausting the air from the cylindrical vessel. The latter is surrounded by a casing through which water is circulated by means of an ordinary force pump and flexible tubes, connected with a capacious cistern containing water kept at a constant temperature of 60° Fah. The bulb of the thermometer is cylindrical, 3 ins. long, its contents bearing a very small proportion to its convex area. The upper half is coated with lamp black, while the lower half of the bulb is effectually protected against loss of heat from undue radiation. The diminution of energy attending the passage of the sun's rays through the lens is made good by the concentration effected by its curvature; hence the entire energy of the radiant heat will be transmitted to the cylindrical bulb. The inclination of the latter, it should be observed, promotes a rapid upward current of the mercury on the top side, and a corresponding downward current on the lower side, thereby rendering the indication prompt and trustworthy. The water in the surrounding casing being maintained at a constant temperature of 60° Fah., it will be evident that the *zero* of the thermometric scale of the actinometer must correspond with the line which marks sixty degrees on the Fahrenheit scale. It scarcely needs explanation that the height reached by the mercurial column after turning the instrument towards the sun, will be due wholly to solar energy, since the radiation of the exhausted vessel towards the bulb of the thermometer is only capable of raising the column to the actinometric zero (60° Fah.)

The readers of NATURE will remember that one of the articles reviewed by Père Secchi, the one published in vol. v. pp. 449-452, contained a demonstration accompanied by several diagrams, proving that the radiant heat emitted by the chromosphere and outward strata of the solar envelope is inappreciable at the surface of the earth. It will be remembered also that the mode adopted in deciding the question whether the solar atmosphere is capable of emitting heat rays of appreciable energy, was that of shutting out the rays from the photosphere, and collecting those from the chromosphere and envelope in the focus of a parabolic reflector. Scarcely any heat being produced notwithstanding the great concentration by the reflector, we proved the fallacy of Père Secchi's remarkable assumption that the high temperature at the surface of the photosphere is caused by *radiation* "received from all the transparent strata of the solar envelope." It is surprising that notwithstanding the completeness and positive nature of the demonstration referred to, no allusion whatever is made to the same in the review put forth as a careful examination of the contents of the article under consideration. Ignoring the evidence furnished by actual trial, in proof of the extreme feebleness of the radiating power, the reviewer proceeds to state "that the outward strata might be less hot, and that the effect which we measure is the aggregate of the quantities of heat which are added, emanating from the various transparent strata." How the outward colder strata cause an elevation of temperature by their radiation towards the solar surface, is not explained; but reference is made to the result of an experiment with three small flames, in support of the assertion that the high temperature of 10,000,000° C., assigned to the surface of the sun, is owing to radiation received from all the transparent strata surrounding the photosphere. The reviewer states: "a very simple experiment, made at my request by P. Provenzali, has shown that, if a heating of 2°·5 can be obtained with one flame, with two flames placed one before the other 4°·5 are obtained, with three flames 5°·4; a result, which of course could be easily foreseen, for everybody knows that flames are transparent."

My practical demonstration establishing the feebleness of the radiating power of the matter composing the solar envelope having received no consideration, while the reviewer, in support of his singular theory of solar temperature, points to the result of the rude experiment conducted by Père Provenzali, I have deemed it necessary to show that transparency of flames is too imperfect to warrant the inferences drawn.

The accompanying illustration represents an apparatus by means of which the exact degree of transparency of a series of flames has been ascertained.

Description: *b* conical vessel open at the top, the bottom communicating with a cylindrical chamber, *f*, by an intervening narrow passage, the whole being enclosed in an exterior vessel *c* charged with water kept at a constant temperature, precisely as in the actinometer. A thermometer is applied near the bottom of the cylindrical chamber, the centre of the bulb coinciding with the prolongation of the axis of the conical vessel. A gas pipe, *d*, provided with a series of vertical burners, is firmly secured to a table, in a position parallel to the axis of the conical vessel. The burners are provided with caps in order to admit of any desirable number of jets being ignited at one time. When gas of ordinary pressure is admitted into the pipe *d*, the side view of the flames will be as indicated by the dotted lines at *m*, *m*, the thickness of each flame being nearly 0·20 ins, while the width, shown by the dotted lines *n*, *n*, somewhat exceeds 3 inches from point to point. It will be observed that the prolongation of the axis of the conical vessel upwards passes through the central portion of the flames at the point of maximum thickness and intensity. Supposing that the instrument (attached to a table turning on declination axis within a revolving observatory) is directed towards the sun, it will be evident that all the rays of a beam the section of which corresponds with that of the bulb of the thermometer, will pass through the flames before reaching the said bulb. Now the temperature of the flames at the point pierced by the solar rays, is fully 2000° Fah., while the intensity of the rays does not exceed 60°. The illustrated device enables us to ascertain whether the rays thus entering at a temperature 1,940° lower than that of the incandescent gas, have their intensity augmented or diminished during the passage through the heated medium. But before we can determine this question, it will be necessary to ascertain what temperature is communicated to the thermometer by the radiant energy of the flames, independent of solar heat. Accordingly, a series of experiments have been made, the result of which is recorded in the annexed table. The nature of the investigation will be readily understood from the following explanation. The instrument being turned away from the sun and the upper flame *m* ignited, while the external casing *c* is kept at a constant temperature of 60°, the column of the thermometer at *f* slowly rises to 61°·76. The radiant heat, therefore, of a single flame produces a differential temperature of 61·76 − 60 = 1°·76. The second flame being ignited, the temperature rises to 62°·88, thus increasing the differential temperature to 2°·88. The ignition of the third flame augments the differential temperature to 3°·80. The remaining flames being ignited in regular order downwards, their combined radiant energy elevates the temperature to 67°·90. Deducting the temperature of the enclosure *c* (60°), the trial shows that although the single flame at the maximum distance from the bulb, is capable of producing a differential temperature of 1°·76, the energy of the *ten* flames together produces only 7°·90. This fact furnishes conclusive evidence of the imperfect transparency of the flames. Assuming that the heat rays are capable of passing freely through the incandescent medium, it will be perceived that the entire series of flames should produce a differential temperatue of $1·76 \times 10 = 17°·60$, showing a retardation of $17·6 - 7·9 = 9°·7$. And if we take into account the diminished distance of the lower flames from the bulb of the thermometer, it will be found that the actual retardation greatly exceeds this computation. We have thus demonstrated that flames are most transparent, as supposed by Père Secchi. Consequently, the inferences drawn from the experiment to which the distinguished *savan* refers in his letter to the French Academy of Sciences are wholly unwarrantable.

Having disposed of the question of transparency, and ascertained the degree of temperature communicated to the thermometer by the radiant energy of the flames alone, let us now suppose that the instrument has been turned towards the sun. The temperature produced by the combined energy of solar radiation, and the radiation of the flames, after directing the instrument towards the luminary, will be found recorded in the fifth column of the table. Our space not admitting of a detailed statement, we must dispense with an examination of the energy transmitted for each flame separately, and at once consider the effect produced by passing the sun's rays through the entire series. It has already been stated that the radiation of all the flames combined imparts a differential temperature of 7°·90 to the thermometer. By reference to the table it will be seen that, the temperature produced by the sun's rays is 21°·69 when the flames are extinguished.* Consequently the temperature, after lighting the whole series, ought to be $21·69 + 7·90 = 29°·59$, since solar heat, under analogous conditions, is capable of increasing definitely the temperature of substances whatever be their previous intensity.† Referring again to the table, it will be found that the maximum increase of temperature attending the passage of the comparatively cold solar rays through the incandescent gas, is 2°·31, while the radiant energy of the flames produces a differential temperature of 7°·90. This extraordinary discrepancy points to an increase of molecular energy within the incandescent gas, notwithstanding its temperature being fully 1,900° higher than that produced by the sun's radiant heat.

The Instrument turned from the Sun.			The Instrument directed towards the Sun.		
Number of flame from the top.	Distance of flame from bulb.	Temperature produced by radiation of the flames.	The sun's rays acting directly on the bulb.	The sun's rays passing through the flames.	Increment of temperature attending the passage of the solar rays through the flames.
	Inches.	Deg. Fah.	Deg. Fah.	Deg. F h.	Deg. Fah.
1	24·8	1·76	21·60	21·90	0·30
2	23·8	2·88	21·61	22·20	0·59
3	22·8	3·80	21·62	22·49	0·87
4	21·8	4·58	21·63	22·75	1·12
5	20·8	5·24	21·64	22·99	1·35
6	19·8	5·84	21·65	23·22	1·57
7	18·8	6·38	21·66	23·43	1·77
8	17·8	6·91	21·67	23·63	1·96
9	16·8	7·42	21·68	23·82	2·14
10	15·8	7·90	21·69	24·00	2·31

* Notwithstanding the low temperature indicated by the thermometer of the experimental apparatus, 21°·69, the actual solar intensity during the investigation, ascertained by the actinometer, has at no time been less than 50°, a very instructive fact proving the futility of attempting to measure solar intensity by thermometers the bulbs of which are exposed to the refrigerative action of surrounding air. For the purpose in view, however, that of measuring the comparative radiant power of flames and solar heat, the unavoidable exposure of the bulb to atmospheric influence, is unimportant, provided the enclosure be kept at a constant temperature during the experiments.

† Père Secchi reminds us, in "Le Soleil," that Mr. Waterston found by his solar intensity apparatus, that, when the thermometer was enclosed in a heated vessel imparting upwards of 400° Fah. to the bulb, the same degree of differential temperature was reached by exposure to the sun, as when a cold enclosure was employed which reduced the indication of the enclosed thermometer to that of ordinary atmospheric temperature.

J. Ericsson

NOTES

Mr. Darwin's forthcoming work on "Expression in Man and Animals" bids fair to be of a more popular character than any of his other publications. It will commence with a statement of the general principle of Expression;—that serviceable actions become habitual in association with certain states of the mind, and are performed, whether or not of service, in each particular case. This will be illustrated in the case of expression of the various emotions in man and the lower animals. The means of expression in animals will then be discussed, and the special expressions of animals and man, such as the depression of the corners of the mouth in grief, frowning, the firm closure of the mouth to express determination, gestures of contempt, the dilatation of the pupils from terror, the causes of blushing, &c. In conclusion, the bearing of the subject will be spoken of on the specific unity of the races of man, the part will be discussed which the will and intention have played in the acquirement of various expressions, and the question of their acquisition by the progenitors of man will be referred to. Seven heliotype plates reproduced from photographs will illustrate the work.

History is said to repeat itself. It is singular to find that six months ago the colony of Victoria was involved in a similar controversy to that which has recently agitated the scientific world at home. Baron Friedrich von Mueller is the Dr. Hooker of Australia. The Botanic Gardens at Melbourne have become under his management as truly scientific an institution as those at Kew, and their Director has performed similar eminent services both to the colony and to the mother country in spreading a knowledge of the value of the indigenous vegetable products of Australia. After twenty years' service, however, it is discovered that von Mueller is "not a landscape gardener," and an agitation is set on foot—we do not, however, hear that it originates in the Board of Works, if there is such a department in Victoria, nor do we know who is the Ayrton of the Antipodes—to deprive him virtually of the control of the Botanic Gardens. Next to the removal of Dr. Hooker from Kew, botanical science all over the world could receive no severer blow than the deposition of von Mueller from the position he occupies at Melbourne.

We believe that, in addition to the 4,000*l.* which we recently noted, another 8,000*l.* will be voted in the next French Budget, to be devoted to the preparations necessary for the observation of the forthcoming Transit of Venus.

The Californian Academy of Sciences experienced a genuine sensation at its meeting on September 9, in welcoming Prof. Agassiz returning from the *Hassler* Expedition, on his first arrival on United States' territory, where he was received by Prof. Davidson, the president of the Academy; Prof. Gillman, the principal of the University of California, Prof. Torrey, of Columbia, the Nestor of American Botanists, and others. In his address on the occasion, Agassiz alluded in the following terms to the growth beneath his eyes of the great Cambridge Museum:—"I went single-handed to Cambridge, to teach natural history, twenty-five years ago. When I delivered my first lecture there was not in the University a single specimen which I could use to illustrate what I had to say. And yet a little band of students, feeling an interest in what they could learn in the lecture-room, and others, thought such a pursuit was worth encouraging, and by-and-by the idea arose that a museum would be of use, and the means were gradually forthcoming, at first sparingly in small contributions, but gradually more liberally in larger sums, until at this moment, after fourteen years only, the museum at Cambridge stands in my estimation without parallel in the world." This he followed up by a statement of what could be done in a new country like California for the advancement of science:—"What would you think of the man who would raise himself his food when he is engaged in the law business or in the medical profession? You would think he was wasting his time. Now I say the scientific man is wasting his time or is obliged to waste his time when he is not provided with the appliances with which he can work, and which he is capable of producing. And I hold that it is one of the duties of those who have the means to help those who have only their head, and who go to work with an empty pocket. So I think that one of your duties, besides fostering and nursing the interest you individually feel for science, is to arouse that general interest in the community, which will make every true patriot, every lover of his State, every philanthropist, every man who has the heart to leave a good repute and an honourable memory, desirous of contributing to your progress." We know there are men in California who both can and will respond to this generous appeal.

PROF. TYNDALL left England on Saturday last by the *Russia* from Liverpool, on a visit to the United States.

THE Council of the Bedford (Ladies') College, Bedford Square, have decided on attempting to introduce some branch of Natural Science into the regular programme of the college studies, and will make a commencement with a class of Vegetable Physiology Mr. A. W. Bennett, who will conduct the class, will also give the Introductory Lecture of the session on Wednesday, October 9th, on "The Place of Natural Science in a Liberal Education."

A VACANCY has occurred in the staff of the Royal Observatory, Greenwich, by the resignation of Mr. James Carpenter, whose services have been transformed to a private engineering firm.

FROM the second report of the Cambridge Natural Science Club we learn that seven meetings have been held during the long vacation, at which the following papers have been read:— "On Turacin," by Mr. R. M. Lewis, B.A., Downing College; "On the old Glaciers of Wales," by Mr. G. E. Paget, Caius College; "On Colour," by Mr. C. T. Whitmell, B.A., B.Sc., Trinity College; "On Sponges," by Mr. A. F. Buxton, Trinity College; "On Thermo-Magnetism," by Mr. J. E. H. Gordon, Caius College; "On *Peronospora infestans*," by Mr. H. M. Martin, M.B., D.Sc., Christ's College; "On Absorption Spectra," by Mr. R. M. Lewis, B.A., Downing College. The club consists of twelve members, of whom seven or eight were in residence during the Long. The attendance at the meetings was good, averaging six or seven members, and two or three visitors. The rule, requesting that members shall give "such practical illustration as the subject admits of," has been very well observed, as almost all the papers have been illustrated by experiments or specimens.

THE opening (public) lecture of the Literary and Scientific Society attached to the Whitechapel Foundation School, was given on Tuesday evening last. Among the lectures to be delivered during the session, will be one on the "Early History of Man," by E. Clodd, F.R.A.S., and on one of the "Divisions of Light," by W. Spottiswoode, F.R.S.

MR. R. PRATT, late Master of the Queen's School of Art, and gold medalist of the Department of Science and Art, has been elected Art-master in the Hartley Institute, Southampton; and Mr. J. R. Brittle, Associate of King's College, London, and late Whitworth Scholar, has been appointed Lecturer on Engineering at the same institution.

THE following classes in connection with the Manchester Mechanics' Institution have been commenced for the season, under the management of Mr. Robert Routledge, B.Sc.:—Applied Mechanics, Steam and the Steam Engine, Acoustics, Light and Heat, Magnetism and Electricity, Organic Chemistry, Inorganic Chemistry, and Practical Chemistry.

THE management of the Islington Youths' Institute has just issued the programme for the winter session. Amongst the various subjects taught at this Institution, those in connection with the Science and Art Department form a conspicuous feature, and six classes in Art and eight in Science are announced. Among these we may mention Drawing (Freehand, Model, and Geometrical), Building Construction, Machine Construction, Geometry (Plane, Practical, and Solid), Electricity and Magnetism, Physical Geography, Physiology, Acoustics, Light and Heat, and Inorganic Chemistry. At the examinations held in May last, out of 129 who competed for the Government prizes, only 13 failed to pass.

THE Council of the Institution of Civil Engineers have awarded the following premiums and prizes for work done during the session 1871-2:—A Telford medal and a Telford premium in books to each of the following gentlemen: Bradford Leslie, for his "Account of the Bridge over the Gorai River, on the Goalundo Extension of the Eastern Bengal Railway;" Carl Siemens, for paper on "Pneumatic Despatch Tubes: the Circuit System;" W. Bell, for paper on "The Stresses of Rigid Arches, Continuous Beams, and Curved Structures;" J. H. Latham, for description of "The Soonkêsala Canal of the Madras Irrigation and Canal Company;" G. Gordon, for paper on "The Value of Water, and its Storage and Distribution in Southern India;" A Telford premium in books to F. A. Abel, F.R.S., for paper on "Explosive Agents applied to Industrial Purposes;" and the same to Bashley Britten, for paper on "The Construction of Heavy Artillery, with reference to Economy of the Mechanical Forces Engaged." The Manby premium in books to C. Andrews, for paper on "The Somerset Dock at Malta." A Miller prize each to Oswald Brown, for paper on "Sewage Utilisation;" A. T. Atchison, for paper on "Railway Bridges of Great Span;" J. Addy, for paper on "The most suitable Materials for, and the best mode of Formation of, the Surfaces of the Streets of Large Towns;" A. E. Preston, for paper on "Wood-Working Machinery;" W. P. Orchard, for paper on "The Education of a Civil Engineer."

WE are informed of the early publication of the first number of the *Telegraphic Journal*, a Monthly Illustrated Review of Electrical Science. It will be edited by the Rev. William Higgs, M.A., sometime assistant to Sir Charles Wheatstone.

THE present autumn has been remarkable for the appearance in scattered localities all over the country of one of our rarest and most beautiful butterflies, the Camberwell Beauty, *Vanessa Antiopa*, very few British specimens of which exist in our cabinets. The *Entomologist* records the capture of upwards of 200 specimens in all parts of the country, from the Channel Islands to Aberdeen. It is very remarkable that they nearly all differ in colouring to a perceptible extent from the Continental variety, the border being creamy white instead of buff-coloured. If they are genuine natives their spasmodic appearance in this manner is very singular, and worthy of careful observation. Several other rare butterflies, especially *Argynnis Lathonia*, *Pieris Daplidice*, and *Colias Hyale*, have also been unusually abundant this season.

A PLAGUE of butterflies is a rare occurrence. A short time ago, however, the town of Florence was invaded by a prodigious quantity of these insects. All the distance of the Long'arno between the Piazza Manin and the Barriera and in all the adjacent streets the passage was almost obstructed by an extraordinary quantity of butterflies that had swarmed in such thick clouds round the gaslights that the streets were comparatively dark. Fires were immediately lighted by order of the Municipality and by private citizens, in which the butterflies burnt their wings, so that half an hour afterwards one walked upon a layer formed by the bodies of the butterflies an inch thick ! ! ! They were of a whitish colour, and some of the streets appeared as if covered with snow, at least so say the Italian papers.

DR. PATERSON, of Bridge of Allan, writing to the *Scotsman* of September 9, says that on Saturday last he captured in his garden on the flowers of *Lilium auratum* a fine specimen of the Striped Hawk Moth (*Deilephila Livornica*). Dr. Paterson believes this insect has been captured only once before in Scotland.

THE REV. M. J. BERKELEY describes in the *Gardener's Chronicle* a very remarkable instance of luminosity in fungi. It occurred in the mycelium of an unknown species growing on a trunk of spruce or larch, and was so powerful as to make a perfect blaze of white light in the track where the trunk had been dragged, and vividly illuminating everything in contact with it. It gave almost light enough to read the time on the face of a watch, and continued for three days.

THE Brighton Aquarium has lately received two pair of beautiful specimens of the Paradise or Peacock fish. This fish came first from China, and has been acclimatised by M. Carbonnier, the great pisciculturist of Paris ; they are very lovely little creatures. Some of their habits are singular ; thus M. Carbonnier states that "as the eggs are laid the male carries them away in his mouth, and deposits them in a nest which he builds for them. He will not allow the female to come anywhere near the nest, and if she ventures to approach he swings himself round, and drives her away."

THE Secretary of the U.S. Navy has recently received, by way of Copenhagen, a letter from Captain C. F. Hall, of the *Polaris*, written on the 24th of August, 1871, at Tossak, North Greenland, latitude 73° 21', longitude 56° 5' west. Although this is but a few days later than the despatch brought home by the frigate *Congress* nearly a year ago, it renews the assurance of the harmony existing on board the vessel between the members of the expedition, and the perfect satisfaction of all with the equipment and preparations for the coming winter. It is well known that no efforts were spared by the Navy department to render this expedition the most perfect and complete in its equipment of any ever sent to the North ; and the success of these endeavours must, therefore, be a source of great gratification to it. Governor Elberg, of the Upernavik district, accompanied the *Polaris* as far as Disco, and brought back the despatches, which have thus been a year in their journey to Washington. Through his help Captain Hall obtained sixty strong, healthy young Esquimaux dogs, and a large supply of reindeer furs, seal-skins, &c. At Upernavik Hans Christian, well known to the readers of Kane's narrative, joined them as hunter and dog-driver, and was accompanied by his wife and three children, who with Joe, and Hannah, and their child, Captain Hall's faithful companions in previous years, made up quite a party. It will be remembered that Captain Hall met the returning Swedish expedition at Holsteinbourg, and that its commander supplied him with charts and copies of such of his notes as promised to be of service to him. Partly in consequence of the suggestions of the commander, Baron Von Otter, and of other scientific men whom he met in Greenland, Captain Hall concluded to abandon the Jones Sound route, and intended to cross Melville Bay to Cape Dudley Digges, and thence to steam directly to Smith Sound, with a view of finding a passage on the west side of the sound from Cape Isabella to Kennedy Channel. Captain Hall speaks very favourably of the steaming qualities of the *Polaris*, her passage having been perfectly satisfactory from port to port. The entire steaming time from New York to Disco was twenty-seven days, seven hours, and thirty minutes.

MR. HULL, of the Irish Geological Survey, has published a letter in the *Dublin Morning Mail* in reference to the quantity of coal available for use in the Irish coal-fields, in which it is stated that the net tonnage available in Ireland is 182,280,000 tons—in Ballycastle, County Antrim, 16,000,000 ; in Tyrone, 32,900,000 ; in Queen's County, Kilkenny, and Carlow, 77,580,000 ; in Tipperary, 25,000,000 ; in Clare, Limerick, and Cork, 20,000,000 ; and in Connaught (Arigua district) 10,800,000 tons.

WE are glad to observe from the eighth Report on the Melbourne Observatory that the southern half of the heavens is being observed to good purpose, though both the Board of Visitors and Mr. Ellery think that several improvements in details are much needed in order that the work may be done with anything like sactisfactoriness. The great telescope continues to give increased satisfaction, though there seem to be serious defects in the Magnet House, and a great want of clerical assistance in reducing the great number of stars observed. The number of stars observed up to the period of the report was 48,672, the number reduced being only 36,917. It appears that a large number of drawings of nebulæ and other celestial objects observed with the Great Melbourne Telescope has accumulated, and we earnestly hope that the Board's request to Parliament to supply the funds necessary to publish these may meet with a favourable reply. Naturally the Board and the Government Astronomer express regret that the Eclipse Expedition should have turned out a failure from the unfavourable weather. Still the colony deserves the greatest commendation for the gallant endeavour it made. We are glad to see that vol. iv. of the "Melbourne Astronomical Observations" is now in the press, that a General Catalogue for 1870, containing the results of all the transit work at the observatory, is in preparation, and that in January last the publication of a series of monthly meteorological observations was commenced. Moreover, photographic pictures of the moon are being taken, and promise, we are told, to be both excellent in themselves as works of art, as well as useful in aiding the scientific observations now taking place in Europe. Altogether the Report reflects the greatest credit on Mr. Ellery, the Government Astronomer, and his too limited staff.

THREE slight shocks of earthquake were felt on the evening of July 27 at Valparaiso and at Caracas.

A SLIGHT shock of earthquake was felt at Chopea in Khandeish on the evening of Friday, July 12, at about seven o'clock. The shock lasted about a minute, and appears to have been felt at the same time at Amainer, Dhurrangaon, Dhulia, and Julgaon. Its course was from west to east.

ON the 15th of April a very violent volcanic eruption took place from the volcano Merapi in Java, which had been quiet since 1863. Great destruction of lives and property occurred, many villages being totally destroyed. The outburst was entirely unexpected, and the showers of stones and ashes and the streams of lava were very destructive. At Solo and other places the showers of ashes lasted for three days, and it became so dark that the lamps had to be lit. By the last accounts some 200 dead bodies had been found on one side of the volcano.

THE BIRTH OF CHEMISTRY

I.

Introduction—Ancient Science—Origin of Chemistry—Derivation of the Name—Definitions of Chemical Science—Early Ideas relative to the Formation of the World.

THE history of a natural science resembles in many respects the history of a nation. In each instance the object is first to obtain a knowledge of causes, then to frame laws. The first are those causes which most promote the well-being of the nation, the second those causes which produce the phenomena of the Universe. In each instance we start with an absence of all law, and we may observe the slow efforts of the human mind to trace each effect to its proper cause, to group together causes, and finally to connect them by one bond. The main difference is this, that in the case of the nation man has to deal with laws which must be founded upon a just study and close observance of every phase of that particular community, influenced as it is by numberless external causes, such as race, climate, religion, habit of thought, tradition; while in the case of the science he has to evolve pre-existent laws, also by the close observance of facts, which are hidden from him by the complex mechanism of nature. M. Taine would tell us that the laws which influence the development of peoples are just as absolute, definite, and pre-existent, as those which govern the affairs of nature; but we are quite disinclined to admit this, even in regard to one particular race, in one particular locality. In both histories we have similar forms of government, similar assemblies of lawgivers; we have our aristocracies, oligarchies, democracies, republics: we have at some period or other Conservatives and Liberals of every shade. We know not what Conservative rule can compare with the dominance of the science of Aristotle for twenty centuries, and we cannot be too ready to welcome the Liberal-conservative era of Copernicus and Giordano Bruno, the Liberal era commenced by Galileo and Francis Bacon, which by easy stages is passing, if it has not passed, into the right Radical era of modern scientific thought. The "Republic of Learning" is no empty phrase.

No one would venture to deny the value of a knowledge of the history of nations, and we are inclined to believe that the history of the natural sciences is not without its uses. It is neglected because during the last century new discoveries have quickly succeeded each other, old sciences have augmented, while new sciences have arisen; in fact, the progress of science has been so extraordinarily rapid that we have scarcely time to turn aside and look at its past history; the present is sufficient for us, and if we once get out of the main current of thought we have difficulty in regaining lost ground. Yet we may no more forget that we owe our present wise laws and great constitutional system to the labours of ten centuries of men, than that our science of to-day represents the accumulation of the scientific thought of twice ten centuries. Intellectual revolutions have not been less frequent than social revolutions, nor battles of the pen than battles of the sword; the crash of a fallen philosophy has often been louder than that of a fallen throne; the wail of the last Phlogistians rent the heavens; the Aristotelian physics died with groanings and gaspings and a discoloured visage.

In tracing the history of a science, we are first led to inquire whether the Ancients possessed any knowledge of it, and whether it originated among them. Now the Ancients made but little progress in any of the natural sciences. They divided all human knowledge into three parts: Logic, or mental philosophy; Physics, or natural philosophy; Ethics, or moral philosophy. Some placed logic first, some ethics, but no one physics. Philosophy was compared to an egg—logic the shell, physics the white, ethics the yolk; or, again, it was compared to a living creature—logic the bones, physics the flesh, ethics the soul. Plato separates logic as the knowledge of the immutable, from physics the knowledge of the mutable. The Cynics sought a complete freedom from any object or aim in life, and renounced all science. Sokrates aimed at logical definition, and affirmed that the true nature of external objects can be discovered by thought without observation. The knowledge of one's self (γνῶθι σεαυτόν) is the true object and aim of all philosophy. Knowledge obtained from external sources is worthless; there is nothing to be learned from fields and trees. A certain philosopher is said on this principle to have put his eyes out, in order that his mind might not be influenced by external objects, and might be left to pure contemplation. (How curiously this contrasts with the plaint of Galileo just before his death, "*Proh dolor!* the sight of my right eye, that eye whose labours, I dare say it, have had such glorious results, is for ever lost. That of the left, which was and is imperfect, is rendered null by a continual weeping.") Others of the ancients allowed that geometry might be employed for the measurement of land, and astronomy cultivated so far as it might be of use to sailors, but on no account as serious subjects of mental occupation.

Thus it happened that natural science made but little progress among the ancients; thus it happens that a schoolboy of twelve knows more about earth, and fire, and water, than was dreamt of in the philosophies of the greatest thinkers of antiquity. Let us, however, give them their due; let us confess that Plato possessed the "finest of human intellects, exercising boundless dominion over the finest of human languages;" that Aristotle was the greatest genius the world has ever seen; that as pure intellectual evolutions they have handed down to us a mass of grand philosophy; ten thousand noble efforts of the human spirit. Everything favoured the exercise of the unaided intellect, while it is hard to estimate the difficulties which presented themselves in the investigation of nature. At one period it was considered impious to attempt to explain the manifestations of the gods. There was an outcry in Athens, a popular demonstration, when the thunderbolts of Zeus were referred to common fire produced by the collision of clouds. The feeling was of the same nature as that conveyed by Campbell's stanza:—

> When Science from Creation's face
> Enchantment's veil withdraws,
> What lovely visions yield their place
> To cold material laws!

only the feeling existed in an intensified form, for here the first of the gods was derided—the Olympian Zeus, Lord of the Air, he who rides upon the storm, and hurls the thunderbolt. For a length of time, therefore, any investigation of nature was impossible for religious reasons. Men were to worship nature, to be filled with awe and wonder—δεισιδαιμονία—in presence of great natural phenomena, but not to inquire too closely into their causes. Twenty centuries later the Doctors of Salamanca who interrogated Columbus, the Inquisitors of the Sacred College who examined Galileo, upheld the same old doctrines, albeit the old gods had passed away. But the investigation of nature was impossible among the Greeks; their capabilities were very limited, they had no instruments for observations or experiments of any kind, neither had they the faculty of observation; their minds were untutored in that particular direction. Then they had to contend against their own particular habit of thought, the extreme tendency to concretion, to hasty generalisation from purely mental premisses; or if an observation had been made, a broad general law was deduced from it without further observation. So also the Chaldæans and Parsis had to contend against the mysticism, the astrology, and magic, which originated among them; and the ancient Hindu was so given to extreme abstraction, and to the evolution of all manner of strange metaphysical dogmas, that we could scarcely look for much science from an Eastern source. Egyptian learning was monopolised by the priests, and they so wove together the real and the unreal, and were so secret withal in their actions, that although much of the Greek learning came direct from Egypt, we cannot trace it to its direct source, or point to one Egyptian writer on philosophy. The Greeks, too, received much from the Phœnicians; but here also we find no record. We will presently inquire more fully into the exact amount of science possessed by the ancients.

We have chosen for our historical survey one of the oldest of the natural sciences, for obvious reasons, the chief being that it will enable us to observe more minutely the early thoughts of ancient peoples in regard to certain phenomena of nature. The science of chemistry does not owe its existence to any one people, or to any sudden process of development. The basis of the edifice is sunk deep in Eastern soil; the time when the foundation stone was laid is too remote to be even suggested; the walls were slowly and laboriously raised during the Middle Ages, and were completed by Lavoisier, Black, and Priestley; the men of our day are working at the roof. We neither hold with M. Goguet that Moses possessed considerable knowledge of chemistry, because he dissolved the golden calf, nor with M. Wurtz, when he says "La chimie est une science Française. Elle fut instituée par Lavoisier d'immortelle mémoire." Chemistry was not a science until long after the time of Moses; it was a science long before the time of Lavoisier. We wonder what Dr. Hermann Boerhaave of Leyden (whose large quarto "Elementa Chemiæ" was published in 1732, nine years before the birth of Lavoisier), would say to the proposition of M. Wurtz. Short of this, it would be difficult to overrate the services which Lavoisier rendered to chemistry. But the science has grown up by a gradual process of evolution; upon its surface we find the impress of many and diverse phases of thought and of action; the science of to-day is the summation of many intellectual efforts produced by the constant struggle of the human mind for truth. How often that truth has been hidden by a mass of sophistries; how often it has been absorbed by some false philosophy to appear again untarnished in due time; how often the attempt has been made to crush it under foot; and how it has ever risen to the surface at last, all who read the history of faiths, nations, ideas, must know. It will be our object to show this is the study of the particular science which now engages our attention.

The word χημεία first occurs in the Lexicon of Suidas, a Greek writer of the eleventh century; he defines it as "the preparation of gold and silver." In the "Lexicon Græco-Latinum" of Robertus Constantinus, published in 1592, the same definition is given, and Suidas is quoted as the authority. According to Olaus Borrichius, however, there were Greek writers on alchemy before this date; there is said to be a Greek MS. of the fifth century on alchemy in the King's Library in Paris, and others of a somewhat later date in the libraries of Munich, Milan, Venice, Hamburg, and Madrid; but we are inclined to doubt whether any of these were written before the ninth or tenth century. They are probably the work of monks living at Alexandria and Constantinople; indeed, one of them is entitled, "Cosma the Monk, his Interpretation of the Art of making Gold." The titles of some of the others will prove to us that we can place but little faith on any date which may be assigned to them:—

"Heliodorus on the Art of making Gold" (περὶ χρυσοποιήσιος).

"John the High Priest, in the Holy City, concerning the Holy Art."

"Isis the Prophetess to her son Orus."

"Moses the Prophet on Chemical Composition" (περὶ χημευτικῆς συντάξιος).

"Cleopatra on the Art of making Gold."

"Democritus the Abderite, the Natural Philosopher, on the Tincture of Gold and Silver, and on Precious Stones and Purple."

Equally worthless, we believe, are the Greek derivations of the word chemistry. Many (among others M. Hoefer) derive the word from χέω, to fuse or melt, because the majority of old chemical operations were effected by fire—witness calcination, ignition, distillation, sublimation, desiccation, reverberation. The earliest chemical arts, such as the smelting of metals and the production of glass, were also operations of fire. Indeed, the science has been called *Pyrotechnia* (πῦρ τέχνη, the art of fire), because, says Lemery, in his "Cours de Chimie," "we in effect produce all chemical operations by means of fire." Others derive chemistry from χῦμα—that which is poured out, a liquid, in allusion to the various liquids used in chemical operations; but this derivation is not worth a moment's notice. We must rather look to an Egyptian source. Plutarch tells us that Egypt was called *Chemia*, on account of the black colour of the soil, and that the same term was applied to the black of the eye, which symbolises that which is obscure and hidden. This word is related to the Coptic *khems* or *chems*, which also signifies obscure, occult, and is connected with the Arabic *chema*, to hide. It is probable that we have here the true derivation of the word chemistry. The first treatise on the science, the date of which is known with any certainty, was written by the Arabian Yeber or Geber, and at that time (the eighth century), Arabic learning had considerable influence on European culture. The science was called the *occult*, or *hidden*, because it related principally to the secret art of the transmutation of metals, as the definition of Suidas, given above, and the earlier works on the science prove. The term *black art* has been applied both to alchemy and to the magical arts so often associated with it, and clearly agrees with the above derivation. The *al* in alchemy is the Arabic particle *the*, so that alchemy signifies "the hidden science" *par excellence*; we notice the same prefix in *al*koran, *al*cohol (the burning liquid), *al*kali (the acrid substance), *al*gebra, *al*embic (the cup-shaped vessel), and in the names of many stars, as Aldebaran, Algenib, Alpheratz,—all words of Arabic origin.

Whatever difficulties there may be in determining the precise derivation of the word chemistry, there can be none in defining the science as distinctly and definitely the science which treats of the *changes* which matter undergoes; while physics proper treats of the action of various forces—heat, light, electricity, magnetism—upon matter, in all cases unaccompanied by any change of composition. If we heat a piece of iron to redness, or cause it to convey an electric current, or place it in contact with a magnet, it has been submitted to various actions, but when they are removed it returns to its original condition. On the contrary, if we fuse it with sulphur a chemical change takes place, a new substance is formed, and the iron does not return to its original condition. This idea of change is the fundamental chemical conception. The first man who made glass, or extracted a metal from its ore, effected a chemical change; the idea became most sovereign and dominant in alchemy, the attempt to change base metals into gold; it reigned throughout the period of phlogistic chemistry, for was not phlogiston a subtle entity which effected changes in matter according as it was assimilated by matter or rejected from it? It is equally the character of the chemistry of Lavoisier and Cavendish, of Davy and Dalton, of Bertholet and Cannizzaro. The "philosopher's stone" (of which much more anon) was a substance supposed to *change* all things into gold; the "elixir vitæ" was a substance which was to *change* old men into youths; the "universal solvent" was to *change* everything to a liquid form. Let us look at some of the definitions of chemistry. Boerhaave says, "Chemistry is an art which teaches the manner of performing certain physical operations, whereby bodies cognizable to the senses, or capable of being rendered cognizable, and of being contained in vessels, are so changed by means of proper instruments, as to produce certain determined effects, and at the same time discover the causes thereof, for the service of various arts." Sir Humphrey Davy writes as follows:—"Most of the substances belonging to our globe are constantly undergoing alterations in sensible quantities, and one variety of matter becomes, as it were, transmuted into another. Such changes, whether natural or artificial, whether slowly or rapidly performed, are called chemical; thus the gradual and almost imperceptible decay of the leaves and branches of a fallen tree exposed to the atmosphere, and the rapid combustion of wood in our fires, are both chemical operations. The object of chemical philosophy is to ascertain the causes of all phenomena of this kind, and to discover the laws by which they are governed." Quite recently Dr. Miller defined chemistry as "the science which teaches us the composition of bodies," and such knowledge we can only obtain by pulling matter to pieces (analysis), or by building it up (synthesis). Dr. Hofmann of Berlin has defined the vast body of so-called organic chemistry as "the history of the migrations of carbon," and is not migration change of place?

Chemistry, then, is the science which treats of the various kinds of matter, whether simple or compound, of which the world is composed, their properties, and the laws which govern their combination with, and separation from, each other. We shall first discuss any ideas of the ancients which bear upon changed matter in any form or condition: thus their early cosmogonies; the knowledge they possessed of metals and compound bodies; and their various technical operations, such as glass-making and smelting, alike demand our attention.

If we compare all the earliest ideas as to the formation of the world, we find them resolve themselves into the belief that the ether and chaos, mind and matter, were the original principles of things. The ether, a subtle vivifying principle, "passing as a mighty breath over the chaos; the chaos a boundless watery expanse without form." It was thus according to Sanchoniathon in the belief of the Phœnicians, and the twenty-five principles of the Hindu philosophy of San'chya are finally reduced to these—matter and spirit, nature and soul. The Egyptian deity was called Nûm as the spirit moving over the face of the waters, Pthah as the principle of production. The Hindu deity Brahme typified the productive force of nature. Among more western nations Gaia, the personification of earth, was held to be the first that sprung from Chaos, and the wife of Ouranos. Okeanos was their son, and according to Homer was the source of all the gods. The worship of the elements, and of the sun and moon, was among the very earliest forms of worship; thus we have in India, Agni the god of fire, Indra the god of the firmament; the sun was sometimes worshipped as a symbol of the deity, sometimes as a deity; fire was worshipped by the ancient Persians as a symbol of the deity; in the Homeric religion we find the Olympian Zeus, lord of the air, who possesses absolute and Universal power. We must notice, too, Aïdoneus, the brother of Zeus, and lord of the Underworld, said by some of the Greek philosophers to designate earth, and undoubtedly an old nature power. Again, "Hephaistos," says Mr. Gladstone, "bears in Homer the double stamp of a nature power representing the element of fire, and of an anthropomorphic deity who is the god of art at a period when the only fine art known was in works of metal produced by the aid of fire." He is also one of the seven star-deities of Chaldæa, the signs and names of which were given at an early date to the seven metals.

G. F. RODWELL

THE AMERICAN EXPLORING EXPEDITIONS *

THE various Government exploring expeditions, the departure of which to the fields of operation for the season we have already announced, are busily engaged in carrying on the important work entrusted to them; and it will be safe to expect as the result a larger addition to our stock of detailed information respecting the western regions of America than has ever been brought together during a single year. The most important of these parties are the northwest boundary survey, the geological explorations of Mr. Clarence King along the fortieth parallel, and the surveys of Lieutenant Wheeler in Nevada and Arizona, under the War Department; that of Prof. Hayden, in two divisions, under the Interior Department; and that of Major Powell in Colorado, under the Smithsonian Institution.

Perhaps the most thoroughly equipped and elaborate exploration is that of Lieutenant Wheeler, which is now fairly in the field, and engaged in carrying on its work. This has for its object a thorough investigation of the region west of the hundredth meridian, for the purpose of determining its geographical positions, thoroughly working out its topography, and investigating its geology, natural history, and climatology.

As the basis of this work, it is proposed by Lieutenant Wheeler to divide the region referred to into eighty-five rectangles of equal size, and to mark their corners with great precision, then, taking each one in detail, to determine its astronomical, physical, and natural history features. This, of course, will require considerable time for its completion; and it is hoped that Congress will grant the necessary authority, so that the work may be accomplished as speedily as possible. As each rectangle is elaborated, it will, of course, join on to those previously investigated; and an index map is to be carried along simultaneously for the more ready understanding of the details. Eight rectangles have been completed by Lieutenant Wheeler in his previous expeditions, and it is expected that thirteen will be finished by the end of the season.

To carry out this programme certain points are to be determined astronomically with great precision, and these as nearly as possible along a continuous parallel. Those already selected are, according to the *New York Herald*, a point near Beaunois, near north-western Kansas; the crossing of the Union Pacific and the western boundary of Nebraska; Cheyenne; the eastern limit of the survey of the fortieth parallel by Clarence King; Sherman, the highest point on the Union Pacific; Fort Steele; Laramie City; the crossing of the Union Pacific and the western boundary of Wyoming; the crossing of the Central Pacific and the 120th meridian; and a point on the western boundary of Nevada.

Telegraphic determination of the longitude will be used very freely, and for this purpose Brigham Young has kindly permitted the employment of his well-equipped observatory in Great Salt Lake City. It is proposed to establish a principal station at or near Sherman, the position of which will be determined with the utmost accuracy, and to use this as a point of reference for the other stations referred to. The work of the present season will be carried on almost simultaneously in Utah, Arizona, and Nevada, several divisions of the main party having already been organised and set to work. The southern and south-western portions of the Salt Lake basin are to be explored; also the mining regions on the Virgin and in Eastern Nevada. It is proposed to establish astronomical points, by means of which to determine with greater accuracy the location of the mineral veins. The Wasatch Mountains will constitute the eastern limit of operations during the year.

The expedition, as organised, embraces the following among the more important of the *personnel*:—Lieut. George M. Wheeler, United States Engineers in command; Lieuts. R. L. Hoxie and W. L. Marshall, U. S. Engineers; Dr. H. C. Yarrow, surgeon and naturalist; T. V. Brown, hospital steward and meteorologist; G. K. Gilbert and E. E. Howell, geologists; J. H. Clark and E. P. Austin, astronomical observers; Louis Nell and John E. Weyss, chief topographers; H. W. Henshaw, assistant naturalist; M. S. Severance, ethnologist; and William Bell, photographer.

At the latest advices the latitude and longitude of Beaver, in Utah, were being determined by Mr. Clark, Mr. Austin being stationed at the Salt Lake City Observatory. Pioche, in Nevada, will be the next point to be occupied. One branch of the expedition, under Lieut. Hoxie, and accompanied by Dr. Yarrow as naturalist, is exploring the regions west of Great Salt Lake City; while the other, under Lieutenant Wheeler, is surveying the Wasatch and the Sevier River regions east of it. From these main divisions parties are sent out to examine the water-courses and mountain regions of the country traversed. They will all concentrate at Beaver, Utah, about October 1, and proceed together toward the south.

* Communicated by the scientific Editor of *Harper's Weekly*.

ITALIAN SPECTROSCOPY *

PROF. TACCHINI presented the matter for the fourth issue of the *Giornale degli Spettroscopisti*, consisting of two memoirs, one by Prof. Blaserna, on the displacement of the lines of the spectrum according to the heat of the prism; the other, by Prof. Donati, on observations of the spectra of solar spots made at Florence with a new spectroscope.

The new spectroscope of Prof. Donati contains twenty-five prisms. They are so arranged that the eye receives only Fraunhofer's line C, and a small portion of the red to the right and left of that line. With this spectroscope Donati has succeeded in seeing clearly the line C reversed on the nucleus of the spots. It does not appear that any of the Italian observers have yet seen the prominences on the disc, a result announced by Lockyer in 1869.

Prof. Tacchini further directed the attention of the Society to his last spectroscopic observations of the sun. For the last few days the number of the protuberances had been rather small, but the chromosphere had been greatly developed, and the vapours of magnesium mixed with it had occupied regions of vast extent. He exhibited a drawing of the spectroscopic image of the Sun's edge, taken on the morning of the 6th of May, 1872, showing the continuous presence of magnesium over an arc of 168°, extending from the north pole to distances of 50° and 118°. This was the first time that he had observed a magnesium region of such vast extent in the sun. And taking account also of isolated tracts, there results a total of 222°, that is to to say, nearly two-thirds of the entire edge, occupied by magnesium vapours more or less intense. The drawing likewise shows the usual correspondence between the faculæ, the magnesium regions, and the portions of the edge at which flames arise to the height of 14 to 28 seconds.

Lastly, Prof. Tacchini gave an account of some spectroscopic observations maae at Geneva by Prof. E. Gautier, and exhibited the drawings of a protuberance observed by Gautier on the 15th of April of this year, which serve to confirm the observations made at Palermo on the solar rains, that is to say, masses of luminous hydrogen suspended in the sun's atmosphere, which gradually separate, and ultimately unite at the edge of the disc, and then present all the appearance of eruption, whereas their formation actually takes place by a directly opposite process.

Prof. Blaserna said that he had heard with much interest of Donati's attempt to observe the reversal of the lines on the solar spots. He had also, in accordance with the admirable conferences of Prof. Tacchini in January last, occupied himself with the problem of observing the protuberances on the full solar disc. He then wrote to Prof. Tacchini a detailed letter, proposing two different methods of arriving, if possible, at the solution of this important problem.

The first of these methods, already applied by Janssen and Lockyer to the protuberances on the solar edge, and now adopted by all spectroscopists, consists in using spectroscopes of continually greater power. Prof. Donati has also pursued this method, and has now arrived at the construction of a spectroscope of twenty-five prisms. Theoretically, it is highly probable that in this manner the protuberances might ultimately be seen in full sunshine. But for this it would be necessary to go much further with the number of prisms, increasing them to 50, 73, or perhaps even to 100. This, however, involves a great practical difficulty, and, moreover, it is doubtful whether so powerful a

* Societa di Scienze Naturali ed Economiche di Palermo, May 18, 1872.

dispersion would not weaken the phenomenon to such an extent as perhaps to render it impossible to see anything at all.

The second method, to which he is inclined to give the preference, consists in attaching to the eye-piece of a good telescope a spectroscope which shall form a real spectrum, well defined and sufficiently extended. A diaphragm is provided with a fine moveable slit, adjusted so as to permit the passage only of the Fraunhofer line C and the line D_3. This slit acts like the slit of a second spectroscope of high dispersive power.

The advantage of this construction consists in intercepting all the solar rays excepting those which correspond to the lines which it is desired to study, or those in their immediate vicinity. The extraneous solar light is thereby arrested, and by dispersing this isolated beam by means of a second powerful spectroscope, Prof. Blaserna believes that we must ultimately succeed in seeing the protuberances on the full solar disc.

The importance of such a fact for spectroscopy induced him to associate himself with Professors Cacciatore and Tacchini, for the purpose of putting it to the test; but the means at their disposal were too slender, and neither did nor could yield any result. For this reason he believes that it will be useful to explain the method, in the hope that some other spectrocopist, and perhaps Donati himself, may follow it out with better means and greater success.

SCIENTIFIC SERIALS

Journal of Anatomy and Physiology, vol. vi., part 2, May. A large portion of this number of the Journal is occupied by a series of papers on Myology, by Prof. Humphry; among them by far the most important is one in which the writer indicates a general plan on which the muscles of vertebrate animals are arranged. Prof. Humphry's scheme is simply this:—The locomotory system of a vertebrate animal consists fundamentally of a successional series of alternating skeletal and muscular planes, having generally a transverse direction between the axial line and the circumference. The skeletal planes, "sclerotomes," are represented in the high vertebrate classes by the vertebral processes, ribs, limb and hyoid girdles, tendons of the dorsal muscles, Poupart's ligament, tendinous inscriptions on the rectus abdominis, &c. The muscular planes, "myotomes," are made up of muscular fibres, the general arrangement of which is in an antero-posterior direction. The muscles of the trunk may be grouped under two heads, the dorsal muscles and the ventral muscles, the latter being disposed in three layers. The muscles of the limbs are derivatives from the middle stratum of the ventral muscle with a funnel-shaped investment derived from the external stratum. Prof. Humphry's other papers are on the arrangement of the muscles of the Lepidosiren, the Ceratodus, the smooth dog-fish, and the glass-snake.—Prof. Turner furnishes a description of this sternum of the sperm whale. Hitherto in the specimens of the cetacean that have been examined the sternum was incompletely ossified, so that the present communication fills up a gap in our knowledge.—Dr. Hollis, in a paper entitled "Tissue Metabolism, or the artificial induction of Structural Changes in Living Animals," describes some experiments made with mechanical and chemical irritants on the now nervous, now vascular tissues of Actinia. The results point to nothing beyond what has been before observed, a swelling and softening of the tissues, with a proliferation of the nuclear elements. Dr. Hollis also furnishes a short paper "On the Homology of a Mandibular Palp in certain Insects," and a note "On the Growth of the Masticatory Organs of Isopod Crustaceans."—Dr. Garrod, in a paper "On Sphygmography," points out the objections to the ordinary "knife-edge" sphygmograph, and describes a new instrument by Bregnet, in which these defects are remedied by a rack-work plan of construction. He further points out most clearly and forcibly the direction in which this apparatus is most useful as a means of observation, both to the physiologist and physician.—Dr. Braxton Hicks brings forward some most valuable evidence against the idea of a placental sinus system into which the fœtal silli protrude, and almost proves that normally no blood exists among the silli.—Prof. Traquair describes the caudal fin of the tailless trout of Islay.—Mr. Stirling notes Trichiniasis in a rat caught in the neighbourhood of a dissecting-room. Several anatomical anomalies occurring in the human subject are recorded in this journal. Mr. Bradley provides some notes on myological peculiarities.—Mr. Champneys describes a communication between the external Ilian and Portal veins.—Dr. Watson mentions a case of the termination of the thoracic duct at the junction of the *Right* subclavian and internal jugular veins; and Mr. Galton reports from Vienna the case of a man possessing two supernumerary teeth behind the upper median incisors. The number concludes with the usual review of books and the reports on the progress of anatomy by Prof. Turner, and on physiology by Drs. Rutherford, Brunton, and Ferrier.

Journal of the Chemical Society, May.—This number opens with the proceeding at the anniversary meeting of the Chemical Society, and also the address of the president on that occasion. Dr. Frankland in his address reviewed the present condition of chemical research in this country, as exemplified by the number of original papers received and read before the society, pointing out that during the past year only 22 papers have been received from the members, the number of whom has reached 656, 32 of these being foreign members; whilst, on the other hand, the German Chemical Society, which numbers 528 native members, has received during the same period the results of no less than 238 original researches. Dr. Frankland mentioned one fact which he believed to be one of the principal causes of this comparative lethargy on the part of English chemists. It is that our Universities and examining bodies do not recognise original research, but are content to accept book knowledge to a great extent; and that, on the other hand, in Germany a candidate for a scientific degree has to submit a memoir or dissertation on some original investigation before he is admitted to examination. The only original communication in this number of the journal is by Dr. Debus, on "The action of sodium amalgam on alcoholic solution of ethylic oxalate." In the year 1864 Friedlander, experimenting on this subject, obtained a substance which he named glycolinic acid, to which he assigned the formula $C_2H_4O_4$, that is isomeric with glyoxylic acid. Dr. Debus has now carefully repeated Friedlander's experiments, but has not succeeded in obtaining this body, but instead of this the sodium salt of glycolic acid. Several attempts were made under varying conditions, but all failed to produce the first-named body, sodium glycollate being obtained. As one of the by-products of the reaction in question, Dr. Debus has isolated tartaric acid. It is probably formed by the action of a molecule of hydrogen on one of oxalic ether, which would yield ethylic glyoxalate and alcohol; and it will then be seen that one molecule of hydrogen, combining directly with two molecules of ethylic glyoxalate, would yield ethylic tartrate. The abstracts of foreign papers contain many of great value, several of which have already been noticed in these pages.

Verhandlungen der k. k. geologischen Reichsanstalt, No. 9, 1872. There is not much of special interest for English geologists in this number of the Proceedings. Amongst the papers are the following:—"On the movements which the sedimentary formations of France have undergone," by M. Delesse; in which the author's studies lead him to the conclusion that the sedimentary strata that are buried in the earth's crust, are always in a more or less soft condition; and "A Contribution to Richthofen's theory of the Loess," by D. Stur. The literary notices and reviews which complete the number are unusually full.

PROF. E. D. COPE contributes to the *American Naturalist* for July an exceedingly interesting account of the Wyandotte Cave and its Fauna, to which we shall probably take an opportunity of again referring. Another important article in the same number is by Dr. H. Hagen on Mimicry in the Colours of Insects. Dr. Hagen distinguishes three different kinds of colours as present in insects—viz., colours produced by interference of light, colours of the epidermis, and colours of the hypodermis. The colours produced by the interference of light are only optical phenomena. The epidermal colours belong to the pigment deposited in the cells of the chitinised external skin or epidermis, and are mostly metallic blue, green, bronze, golden, silver, black, brown, and rarely red; they are persistent and never change, either during life or after death.

SOCIETIES AND ACADEMIES

PHILADELPHIA

American Philosophical Society, December 15, 1871.—A sum of money was appropriated for the planting and preservation of a grove of oaks in Fairmount Park, to be called the Michaux Grove, in accordance with the will of the botanist Michaux.—Prof. E. D. Cope read a paper "On the Pythonomorpha of the Cretaceous Strata of Kansas." This embraced a synopsis of the species of the order known from all parts of the world, by which it appeared that America was its home, only four species having been described from Europe. He said that the *Danubiosaurus* of Bunzel had no relationship to the group. The American species were forty-two, distributed as follows: viz., New Jersey Greensand, 15; Rotten Limestone of Alabama, 7; Chalk of Kansas, 17; other localities, 3. The Kansas species were referred to *Clidastes* 3 sp., *Edestosaurus* 4 sp., *Holcodus* 4 sp., *Liodon* 6 sp. Of these *Edestosaurus tortor* and *E. stenops; Holcodus coryphaeus* and *H. tectulus;* and *Liodon curtirostris, L. latispinus, L. glamiferus, and L. crassartus* were described as new.

January 5.—Hon. Eli K. Price read a paper "On some Phases of Modern Philosophy," in which he combated the views of the heterogenists and of the evolutionists. In the latter part of the subject he opposed the views of Darwin, asserting that the variations seen among domesticated animals had no parallel among those in a state of nature, and the fact of their ready hybridisation is an indication of their specific unity. He quoted Prof. Wyville Thomson to the effect that no transition from species to species had ever been observed in palæontological history; and asserted that the variations observed among animals on which the developmentalists relied in evidence of their theory were few and abnormal, and utterly insufficient for the use made of them; that the origin of man from apes was not supported by evidence; lastly, that the theories of evolution are highly injurious to faith and morals, and thus to Christian civilisation.

Janaury 19.—Mr. Benj. Smith Lyman read a paper on "The Oil-bearing Region of the Punjaub," accompanied by a topographical map. He pointed out the tertiary age of the oil-bearing strata.—Prof. Cope read a paper on a new Dinosaurian from the cretaceous strata of Kansas, which was named *Cynocercus incisus.* The vertebral articular faces were deeply excavated above and below, so as to give them a transverse character.—Prof. H. Hartshorne read a paper on "Organic Physics." It explained that the expression "organic physics" is as well justified as "organic chemistry" and "animal mechanics," for vital force is clearly correlated with other physical forces, as heat, light, &c., but the correlation is not identity. Advocates of the continuity theory have endeavoured to make it appear to be identity, but they will not succeed; because the effects of heat, light, electricity, magnetism, and gravitation are known, and they always tend (in the absence of life) to an opposite kind of change to that which occurs under life force; namely, they form of C, H, N, S, P, &c., compounds of few equivalents and stable equilibrium; while under life force the same elements are made to produce compounds of many atoms or equivalents, and of unstable equilibrium. The first are mainly crystalloids, the second always colloids. The directness of this opposition is especially demonstrated by the result of death (arrest of life force), which is attended by the resolution of the complex, unstable, colloidal, organic substances into more simple, stable crystalloids and gases. Eliminating all the functions of living beings otherwise explicable, we must restrict the term "vital action," or "action of life force" to the conversion of inorganic into organic material, with type-formation or organic construction as its result. It is supposable, at least, though not proven, that the assumption of particular forms under given circumstances is (analogous to crystallisation) the property of the bioplasm; *i.e.*, given the matter, the form results as its property or attribute. But chemists have never succeeded in making organisable matter by synthesis; nor is it likely that they ever will. All complex organic substances made in the laboratory (as urea, by Wöhler; fatty acids, &c., by Berthelot; and even, if made, crystallisable neurin) are post-organic (a term first used by the author), *i.e.*, results of the downward or retrograde metamorphosis; produced, not by life force, as such, but by the composition or balance between life force and the other forces. They are not germinal or formative, but formed and effete materials (Beale's terms). The question of the possibility of abiogenesis is not yet finally decided. Crosse gave it momentum with his galvanised acarus; Pouchet and Pasteur have long debated it; Owen, Bennett, Clark, and a few others have of late years reasserted it; Bastian (NATURE 1870) makes an elaborate experimental defence of it. We note concerning it as follows:—(*a*) The manipulation (to avoid introducing minute visible forms) requires an almost or quite impracticable delicacy throughout. (*b*) When heat is used, we have always the alternative, to conclude that certain minute organisms, germs or spores, can resist a higher temperature than was supposed, or to conclude that, taking for granted that the heat employed must have killed all germs, new life afterwards sprang up, without parentage. All experience makes the former much more probable. George Pouchet's experiments with rotifers tend this way. Jeffries Wyman found that, although four hours' boiling would not, five hours would put an end to all manifestations of life. Franklin's experiments (and Calvert's) gave similar results against abiogenesis. Supposing (although Huxley does not) that Bastian could not have mistaken "Brownian" molecular movements for evidence of life, we yet observe that if life sprang up in Bastian's apparatus, it was such life as can exist without air or oxygen; altogether unlike, therefore, ordinary world-life. The assertion of Pasteur is justified, that the *onus probandi* lies with abiogenesists, since there is no experience of any living form more than $\frac{1}{1000}$ of an inch in diameter springing into life out of inorganic matter; it is therefore vastly improbable (needing most cogent evidence to prove), that any form less than $\frac{1}{1000}$ of an inch in size can be made to spring into life from inorganic matter. While abiogenesis is unproved, we hold to the conclusion that vital force is not the mere outcome or resultant of any or all of the other cosmic forces. How does it differ? Of the organic cell, or "physiological unit," the most constant determinable acts or changes are incretion and excretion; atomic or molecular motion, definite in results, is an essential of life. Must not the motion itself be peculiar? More definitely, we find that while in the condensation of matter in the (nebular theoretical) formation of the sun and planets there was integration of matter with dissipation of force, such as heat (H. Spencer), life action involves integration of matter with accumulation of force (stored up physical force in the plant, of Barker; "bottled sunshine," of some one else). This is a striking contrast. Sexual union is closely analogous to chemical union; instead of combustion, it makes construction by detaining products. Again, we notice the analogy between the spiral phyllotaxis of plants (opposite leaves a double spiral), (whorls two or more, and bilateral symmetry of vertebrates and articulates and some molluscs, and radial symmetry of radiates and cœlenterates corresponding) and the spiral helix of the electro-magnet. As the opposite chemical and polar elements of the battery are to the current of the helix, so (may be) the polarities of the sperm cell and germ cell to the spiral phyllotaxis of plants and symmetrical (usually double) organotaxis (a new term) of animals. A close (but reversed) analogy exists between heat force and vital force. A spark of fire may "light," and so burn successively, an indefinite amount of combustible matter. A spark of life may animate an indefinite amount, successively, of organisable matter. The former, combustion, reduces complex substances which are unstable to more stable compounds. The latter, life, elevates simple substances to more complex states, but with constant transmutation of their forms. Such analogies are as yet crude, and do not solve the mystery of life. But the facts on which they rest justify and encourage the physical investigation of vital actions, including their study under physics—organic physics. Such a view of life is in no manner antagonistic to theism or to "teleology," any more than is the now familiar reduction of digestion, circulation, absorption, &c., to the category of chemical or physical phenomena. All such analytical inquiries are moreover, legitimate so long as they are accurate, whether they point to biogenesis or abiogenesis, to the origin of types by interrupted appearances or by evolution.—A discussion on E. K. Price's paper, read January 5th, took place, in which Prof. Hartshorne, Prof. Lesley, Mr. Price, and Prof. Cope took part. Prof. Hartshorne supported the opposition to abiogenesis expressed in the paper, on the ground of insufficiency of evidence in its favour, but believed in the evolution of species. Prof. Lesley objected to the insufficiency of Mr. Price's reasoning against the labours of experts in biological science; and stated that the more attention he paid to the subject the better satisfied he became that man was descended from apes. Prof. Cope stated that Mr. Price's paper was in error as to the facts. That (1) variability of specific type was even more common in nature than under domestication, examples from many so-called "protean" genera being cited. (2) That some wild species did produce fertile hybrids. (3) That transitions between species, both at the present time and in past geological periods, were common, but were concealed by a universal *petitio principii* involved in the practice of naturalists. This consisted in uniting distinct forms or species under the head of one species as soon as the intervening connections were formed. (4) That the known cases of transition were numerous, not few; and that common induction required that we should believe of the unknown that which we see in the known, when other circumstances are identical.

Feb. 2.—Prof. Geo. B. Wood communicated further results of his experiments with salts of potassa on vegetation, and especially on grain and fruits. He stated that in a field of grain devoted to the experiment, in which the soil had been previously exhausted by bad culture, one half was enriched by farm-yard manure, the other with the same with wood ashes added. The effects of the latter were especially marked, and much greater than with the former. The most striking results were attained by the use of the ashes of the poke, *Phytolacca decandra*.—Prof. Cope read a paper on the "Families of Fossil Fishes of the Cretaceous Strata of Kansas." The greater part of these were shown to be *Physostomous Actinopteri*, of three families, viz., the *Saurodontidæ*, the *Pachyrhizodontidæ*, and the *Stratodontidæ*. Of the first, four genera and ten species were described, some of them (*Portheus* sp.) among the most formidable of marine fishes. The peculiarities of the succession of teeth in *Portheus* and *Saurocephalus* respectively were pointed out. Of *Pachyrizodontidæ*, one genus and four species were described; and of *Stratodontidæ*, three genera and seven species. *Stratodus* was a form provided with multitudes of minute shovel-headed teeth.

PARIS

Academy of Sciences, Sept. 9.—M. Faye, President.—The first paper was by M. P. Duchartre, on the bulb of *Lilium Thomsonianum*, &c. The author finds that this Indian plant seldom flowers in Europe, and traces this to the facility with which it propagates itself by means of off-shoots from the bulb. If it is prevented from doing this it flowers well.—A letter from P. Secchi followed on "Observations on the Variation of the Solar Diameter; Observations of the Protuberances and of the Chromosphere; Observations on the Shooting Stars and of the Aurora Borealis observed at Rome on the 10th of August." Father Secchi finds variations of the solar diameter equal to 3, 4, and even 5 seconds of arc (error of observation less than 0·″5 arc). There were minimum epochs in July, the beginning of September, the middle of November, and the beginning of March and April, when the mean diameter was 32′ 1″ 5‴; and maxima in the middle of August, the middle of September, and during the whole of October and December, and the beginning of February when the mean diameter was 32′ 4″ 5‴. The maxima of diameter correspond to the minima of spots and protuberances. The next memoir was by M. Max Marie "On the Elementary Theory of double integrals and their periods" (continuation). A note from M. A. Potier "On the causes of Elliptical Polarisation by reflexion on transparent bodies."—A note from M. Th. Gaffield "On the results produced by insulation on various kinds of glass," was then presented by M. Chevreul.—"On the lines of Summit and of Thalweg" an answer to the observations of M. Boussinesq by M. C. Jordan.—A note was then read on the induction currents developed in the machine of M. Gramme, by M. J. M. Gaugain.—"On Lithurate of Magnesium, a new species of urinary concretion from the ox," was an extract from a note from M. G. Roster.—The empirical formula for the body in question is $C_{30} H_{36} N_2 Mg O_{17}$, it is soluble in boiling water, from which it crystallises on cooling.—A note on the Nutoscope, by M. Ch. V. Zenger, was presented by M. Yvon Villarceau. This was a description of an instrument for illustrating the nature of nutation.—Next followed a note from M. Tarry on the Constitution of the stream of August meteorites.—M. Dumas then communicated some observations on the *Phylloxera vastatrix*.

Sept. 16.—M. Faye, President.—The President read a note relative to a communication from M. Hirn on the conditions of equilibrium in, and the probable nature of the Saturnian rings.—General Morin then read a note on Major General Mayevski's "Treatise on Projectiles." M. Morin states that M. Mayevski, in his eleventh chapter, devoted to the consideration of the penetration of solid bodies and armour plates by projectiles, arrives at the same conclusions as were obtained by the Metz Commission, and by Capt. Noble, R.A., in England.—"Observations on the nature of the various parts of flowers," by M. A. Trécul, followed.—A letter from P. Secchi on the appearance of a meteor in the neighbourhood of Rome, and on stellar spectra, was then read. The latter portion of the letter was an explanation of the Rev. Father's views on stellar types, which he explained were not the same as those of Mr. Rutherfurd, as had been supposed by Messrs. Lockyer and Schellen.—M. le Dr. Netter then read a paper on the treatment of cholera by the administration of enormous quantities of aqueous drinks in successive doses.—Then followed the concluding portion of M. Marie's paper on the "Theory of double integrals and their periods."—Notes were received from M. Pigeon, on cholera; M. Charles, on aërial navigation; M. Bouvard, on the Postulatum of Euclid; M. Hervier, on *Phylloxera*; M. Quattari, requesting the Academy to examine his aërial telegraphic apparatus; and M. Le Comte L. Hugo presented the Academy with an engraving entitled, "The sphere is an equidomoid, or a demonstration of the pre-eminence of polygonal figures," which was submitted to the examination of M. Ossian Bonnet.—M. Yvon Villarceau presented a note by M. Prosper Henry, describing the discovery of a new planetoid 125 at the Paris Observatory. Observations on the above by MM. Ludinard, Tisserand, Paul Henry, and Prosper Henry followed.—An extract from a Report by Dr. Oudemans on the total eclipse of 12th December, 1871, observed in the Dutch East Indies, was also read.—A paper, by M. Ch. V. Zenger, "On the rapidity of transmission of light in simple bodies, and on their crystalline form," followed.—"On the changes of phase produced by metallic reflexion," note by M. A. Potier, was next read; and then an extract from a paper by M. Plateau on the measurement of physical sensations, and on the law which connects the intensity of these sensations to the intensity of the exciting cause, was followed by a posthumous note of M. H. Magnan's, *à propos* of two notes by M. Cayron on the cretaceous formation of La Calape and Corbières.—M. Louis Faucon sent some observations on *Phylloxera*, made by himself and M. Gaston Bazille; and another note on the same subject and on vine disease was received from M. F. E. Guérin-Méneville, who believes that every observation tends to prove that the *Phylloxera vastatrix* is only a secondary agent in producing the vine disease now so destructive.—M. Yvon Villarceau then presented a note from M. Fron on the atmospheric movements which accompanied the auroræ of September 2 and 6, 1872.—M. Georges sent a note relative to the employment of calcic disulphite to the cure of vines tainted with oidium, which was sent to the Phylloxera Commission.

BOOKS RECEIVED.

ENGLISH.—Cardiff Naturalists' Society Report and Transactions. Vol. III., 1870-71, part 1.—Cholera and Efforts towards Framing an Equilibrium Theory of Health and Disease (Thacker and Co., Calcutta).

FOREIGN.—Tableau de l'Astronomie: Ed. Mailly (T. Hayez, Brussels).—De l'Astronomie dans l'Académie Royale de Belgique: E. Mailly (T. Hayez).—(Through Williams and Norgate.)—Lehrbruch der Zoologie: Dr. Otto W. Thomé.—Der Mensch und die Seele: E. Reich.—Etudes sur les Appendiculaires du detroit de Messine: H. Fol.

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NOTICE

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