THURSDAY, JANUARY 1, 1891.

THE COMMON SOLE.

A Treatise on the Common Sole (Solea vulgaris), considered both as an Organism and as a Commodity. Prepared for the Marine Biological Association of the United Kingdom by J. T. Cunningham, M.A., F.R.S.E., late Fellow of University College, Oxford; Naturalist to the Association. (Plymouth: Published by the Marine Biological Association, 1890.)

THIS handsome monograph is (passing over the "Journal") the first-fruits of the Plymouth Laboratory. From the very outset, the Marine Biological Association has given a prominent place in its programme to economic matters. In the speeches at the foundation meeting, held on March 31, 1884, in the rooms of the Royal Society, and again on the occasion of the opening of the Laboratory at Plymouth, on June 30, 1888, the investigation of the habits and life-histories of important food-fishes, and other similar problems having a practical bearing upon fishing industries, was put forward as a primary function of the Association; and, in fact, the objects of the Association were officially stated to be "to promote accurate researches leading to the improvement of zoological and botanical science, and to an increase of our knowledge as regards the food, life-conditions, and habits of British food-fishes and molluscs." In response to this declared intention of dealing with the practical applications, the Fishmongers' Company have given most valued support to the Association from the beginning, and have subscribed largely to the funds, while H.M. Treasury has added £500 for five years to the annual income, on certain conditions.

In order to carry out efficiently this economic side of their work, and to fulfil their pledge with the Government and the public, the Council of the Association determined, in the summer of 1887, to appoint a skilled naturalist (in addition to the Resident Superintendent, now Director), who would carry on investigations into the natural history of British marine food-fishes, under the direction of the Council. To this post, Mr. J. T. Cunningham, who had been engaged for some years at similar work in connection with the Scottish Marine Station at Granton, was duly appointed, with special instructions from the Council to investigate the life-history of the common sole, with a view to practical results, and with the object of preparing an illustrated monograph on this fish. We now have the results of Mr. Cunningham's investigations before us.

It is now pretty generally recognized that results of economic value in connection with the fisheries can only be obtained, or looked for, as the result of properly conducted scientific investigations and carefully collected statistics. This first monograph issued by the Association is an excellent combination of material from these two sources of knowledge. It is a union of pure science and economics, made with the view of applying the results of scientific study to the extremely practical question of increasing the supply of soles to the market; and we find that it discusses that most important food-fish from almost every point of view. In determining the scope of the investigations and the general plan of the book, the guiding hand of Prof. Ray Lankester (to whose enthusiasm and energy the Association owes its existence and its success, and who has now succeeded Prof. Huxley in the presidential chair) is acknowledged by the author in the preface.

The work is divided into four parts, viz. Taxonomical, Morphological, Bionomical, and Economical. The first part deals with the classification and the characteristics of flat-fishes in general, and of the sole in particular. Throughout the book there is evidence that it is written, not for the professional biologist alone, but for the general public. The descriptions are not always in purely technical language, and at the beginning of each section, or when a fresh subject is entered upon, such as embryology (p. 84), we find a certain amount of elementary explanation which cannot be intended for the specialist, but may perhaps enable an intelligent non-professional reader to gain a general idea of the nature, objects, and results of each investigation, although he is not able to appreciate all the details. With the help of the hen's egg, the protoplasm and the food-yolk and the more important changes which take place in the young embryo are explained in a manner for which many may be grateful who are not aware of the significance of Kupffer's vesicle, and have no particular views on the nature of the periblast.

When, however, such explanations were being given, it would surely have been well to supplement the account of how to draw up the specific description of a flat-fish by an actual numerical example. The table on p. 15, or a part of it, or even a reference to it, might with advantage have been given at the foot of p. 10. There is no bibliography, beyond what naturally occurs in the taxonomy, and one is struck, in some parts of the book, by the absence of any reference to the work of previous investigators, so that it is difficult to know precisely how much is put forward as original; for example, under embryology there is no mention of the observations made by Prof. McIntosh and others.

In the morphological section there is a good account of the anatomy of the chief systems of the body, in which the most interesting passages are naturally those dealing with the modifications of structure caused by the remarkable "asymmetry of the Pleuronectidæ," which was first ably elucidated by Dr. Traquair more than twenty years ago. Mr. Cunningham, from his detailed study of the eye-muscles, supports the view that the twisting of the facial region of the skull, and the migration of the left eye on to the right (or upper) side in the adult is not the result of selection alone, but has been aided by the inheritance of characters or modifications acquired during the life of the individual. But this does not seem to follow as a necessary consequence of his observations and argument. For it is perfectly conceivable that anything which can be produced by the inheritance of acquired characters can also be brought about by the selection of congenital variations. It is true that "a man cannot lift himself up in a basket," and that (p. 53) "the eye-muscles could not by physiological effects have removed themselves bodily by their own action to a new position," but if there were enough men in baskets they might occur piled up ("congenitally") in such positions that some of them would form a series extending from the lower level to a higher; and, similarly, a series of selected congenital variations in muscles may form a gradation from the original position to a new one. I am not, however, objecting to a certain amount of inheritance of acquired characters coming in occasionally as a secondary factor—it is what I have always argued for of late years—but it does not seem to me that the present case yields any fresh evidence.

It is scarcely correct to say (p. 51) that evolutionists are divided into two schools at present, those who would interpret all modifications as the result of natural selection alone, and those who believe that acquired characters are inherited and that the modification "would take place without selection, while selection could not produce adaptations without the inheritance of acquired characters" (p. 52). These are rather the two extremes, and many biologists—judging at least by recently published opinions, and by the discussions in Section D at the last few meetings of the British Association—hold views of an intermediate nature. Probably all grades of opinion between the two extremes discussed by Mr. Cunningham will be found represented amongst leading evolutionists.

The remarks in regard to the origin of the follicle cells of the ovum are of interest in view of the controversy which has long waged as to the origin of the follicular epithelium and the "testa-zellen" in the Tunicata. Mr. Cunningham appears to incline to the view that the follicular epithelium is derived in bony fishes, not from the germinal epithelium, but from amœboid lymph-cells in the connective tissue. In Ascidians the most recent authorities agree in regarding the follicle cells as homologous with ova, and as being derived from germinal epithelium. Another interesting point is the modification of the right dorsal branch of the fifth nerve, and its explanation (p. 70).

One would like to know on what principles Mr. Cunningham has used his italics. It seems impossible to discover any from the book. This is a minor point, but not altogether unimportant. It is confusing to the reader to have italics used inconsistently, and besides it renders the difference of type meaningless. We find italics used indifferently for family titles, generic and specific names (for which they might with advantage have been retained), popular or local names, ordinary words requiring emphasis, and names of bones and other structures. But the type is not even used consistently for all of these. Why (on p. 50) should the "*recti*" muscles be in italics and not the "oblique"? Why (top of p. 67) should "*infundibulum*" be in italics when "cerebellum" and "medulla oblongata" are not, and why should "*hypophysis cerebri*" be favoured rather than "lobi inferiores" and "crura cerebri"? Then, again, we find (p. 68) "recti muscles" sometimes in italics and sometimes not.

At the end of the section on morphology there is a description of *Phyllonella soleæ*, a Trematode parasite which frequently occurs adhering to the skin of the fish (as shown in plate v.). By the way, why is there no explanation of the absence of Figs. 6 and 6*a*, which ought to illustrate this parasite, from plate xiv.? The woodcuts on p. 93 are probably the missing figures, but the matter as it stands, without any note of explanation, is rather mysterious. Plate xiv. and its description do not tally, and the figure labelled 6 is evidently 7.

Under the head of Part III., "Bionomical," are interesting articles on the geographical distribution, the habits, food, parasites, and enemies, the colours, and the breeding, development, and growth of the sole. Mr. Cunningham has determined the spawning season of the sole in his district to be from the middle of February to the end of April, but McIntosh and Prince state (Trans. R.S. Edin., vol. xxxv., p. 848) that off the eastern shores of Scotland the period extends to August. The observations on the colour, both in its detailed markings and its general effect, the variations under diverse circumstances, and the relation to environment, are particularly interesting, and the range, as shown in plates i. to iv., is certainly very wide. Mr. Cunningham finds that the change of colour which takes place when the fish is removed from one background to a differently coloured one is very rapid; a sole placed in a white porcelain dish begins to get lighter almost immediately.

The very beautifully coloured plates (plates i. to ix.) which illustrate especially this section on colour, are reproductions of water-colour drawings by Miss Annie Willis, and no doubt the original sketches are even more artistic representations of the actual objects. If one who has not seen the specimen drawn may criticize—and it is the very excellence that invites the critical spirit—one notices an absence of sufficient light and shade, and especially of high lights, in plate i. It is all too much at a dead level, as if a steam roller had been passed over fish and gravel, and had reduced them to a mosaic; but this is a fault common to many chromo-lithographs. In the present instance the effect of the plate is greatly improved by placing it at a considerable distance from the eye. If it is laid on the ground in a good light at 3 or 4 yards distance and looked down on, it looks as real and natural as can be expected from any representation which is produced by a mechanical process and therefore wants the subtle vivacity of the artist's actual hand-touch.

The fourth and last part, "Economical," is divided into sections on artificial propagation, the sole fishery, and practical measures. Looked at from the economic point of view, this most estimable fish is of interest to everyone. It is said that many Americans come over to England every year for the express purpose of indulging in fresh fried soles for breakfast. Is it with a view of affecting the demand that Mr. Cunningham tells us that our sole may have been swallowed by a *Lophius* a day or two before we meet it at table, and then disgorged on the deck of the trawler? It would be kinder of him to reduce the market price (which appears to be rising steadily, and has increased fourfold since 1856) by increasing the supply of soles by some such process of artificial fertilization on a large scale as that proposed on p. 147. A considerable practical difficulty will, however, probably be encountered in the remarkably unenlarged condition of the testes in the male sole, and the difficulty of distinguishing the milt from mucus and sea-water, especially if the stripping of the fish is to be conducted by the trawlers themselves. Everyone, both on the scientific and on the economic side, will look with deep interest for the results of the further observations and experiments to be made by the staff of the Plymouth Laboratory on these practical questions.

A few misprints (none of great moment) have been noticed in reading over the book, viz. "ectetmoid" (p. 53), "an" for on (p. 61), "Kupher's" vesicle (p. 120), and Fig. 3 for Fig. 5 (p. 123, foot); while a slip occurs near the foot of p. 50 in the phrase "those of the dorsal or right eye," since the dorsal eye in the sole is the left.

The book, as a whole, is good, and forms an interesting and important contribution to our slender list of monographs of British animals, and on account of its economic side will, no doubt, interest a wider circle of readers than is usually the case with zoological works. The Marine Biological Association are to be congratulated on the appearance of their first memoir, and it is hoped that others of the series will speedily follow.

W. A. HERDMAN.

WOOD-WORKING.

Exercises in Wood-working, with a Short Treatise on Wood, written for Manual Training Classes in Schools and Colleges. By Ivin Sickels, M.S., M.D. (New York: D. Appleton and Company. London: W. Allen and Co., 1890.)

THIS is, in some respects, a more interesting work than its title would seem to indicate. Of late years, since it has been recognized that manual or practically industrial training of some kind should form a part of the education of every child, books of this kind have greatly improved in every respect, because it has become more necessary to make them thorough in details, and at the same time present them in such clear and succinct language that they may be perfectly intelligible to youthful minds. And as the interest in the subject of technical education is rapidly increasing, and with it the demand for good manuals, it is not without pleasure that we welcome a work which fulfils admirably what is requisite for its purpose. The author, in an introduction which we could wish had been longer, remarks that the tendency of modern systems of education is towards a proper distribution of practical with theoretical training—that is, manual with "literary" education—and that the *mind* is to be aided in its development by the action of the eye and hand. "The prime object of all manual training is to aid mental development." This is a very great and little-known truth, which was first fully set forth in a work entitled "Practical Education," by C. G. Leland, in which it was shown that, out of 110,000 children in the public schools of Philadelphia, the 200 who attended industrial art classes, and who were chosen at random for them, were the first in *all* studies, such as arithmetic or geography; that is to say, it has been proved by years of careful experiment on a very large scale, that if we take two children of equal capacity, following the same studies in the same school, and let one of these have at the same time from two to four hours' weekly training in design, modelling, wood-carving, and carpenters' work, &c., the latter will invariably take precedence in all the ordinary school studies, so much is the mind impressed by industrial art culture. The recognition of this principle by a practical teacher like Dr. Sickels indicates that his work is written in accordance with the most advanced ideas on the subject of technical education.

The author begins by giving in Part I. a sufficiently detailed description of the structure of wood of different kinds, its composition and manner of growth, the season for cutting trees, drying of wood, and warping, its properties, measure, and value, with an account of the twenty-five kinds most generally used. In describing wood-working trades, he declares there are only two, carpentry and joinery, carving and turning being only "adjuncts" to joinery; a degradation of wood-carving with which we should suppose few would agree. It is difficult for us, with a magnificent fourteenth century carved wood image of the Virgin and Child before us as we write, to comprehend what it has in common with "joinery," while, as regards turnery, the great and admirable work of John J. Holtzapfel, certainly establishes its claim to be an independent art. "But these be trifles." The principal part of the work, or about one hundred pages, which is copiously illustrated with pictures which are all intelligible at a glance (not an *invariable* thing in such works), is, on the whole, without errors. It treats of carpenters' tools (those for wood-carving are not included, but should have been) and their use in detail, this portion being admirably executed; the construction of joints, which is, on the whole, the most difficult and interesting part of the wood-worker's art, being very well written. The student who wishes, however, to be perfect in this, should also consult "Forty Lessons in Carpentry Practice," by C. F. Mitchell (London: Cassell and Co.), a little book of great value. The mysteries of the mitre joint, stretchers, dowels, and dovetailing, have, however, never been treated more cleverly or clearly than by Dr. Sickels. Among remaining topics are drawers, framing—of which we have by no means enough, though "good, what there is of it"—laying floors, trimming, and the construction of all the minor details, such as doors, stairs, sashes, and hand-rails. This portion of the work would be of great practical value to settlers in the wilderness, who would often like to build for themselves houses, yet know not how. We believe that the world, however, still lacks a work teaching men all the art of making shelters and homes, from building wigwams or adjusting stones and boughs for a night's lodging, up to log huts, Pictish "bee-hives," box cottages, "and so wider." As regards typographical details, paper, and binding, this work is all that could be expected.

BACTERIA.

Les Bactéries et leur rôle dans l'Étiologie, l'Anatomie, et l'Histologie pathologiques des Maladies Infectieuses. Par A. V. Cornil et V. Babes. Third Edition, in Two Volumes. (Paris: Félix Alcan, 1890.)

THIS enlarged edition treats of the whole range of pathogenic and to a certain extent also of non-pathogenic bacteria in a fairly exhaustive manner. The classification, the chemical nature, the biological characters, the methods used in their study, both in cultures and in staining them in animal tissues, are described in a lucid though rather a dogmatic fashion. The *rôle* and the nature of the *rôle* the pathogenic bacteria play in relation to infectious diseases are, as the title indicates, the principal subjects of the book, and we have no hesitation in saying that the authors have produced a very creditable work. But there are a number of diseases of the lower animals described here, in which the demonstration of bacteria in microscopic sections through one or the other of the diseased organs is sufficient for the authors to assign to those bacteria a specific character; more than this, every granule which appears stained in those sections is regarded by the authors as a microbe. The exact proof, and even attempt at exact proof, that this is so is omitted in many of these cases.

The illustrations accompanying the text are very numerous, many of them in colours: amongst these latter some are very excellent and true to nature, *e.g.* those illustrating the chapter on tuberculosis; many others are decidedly bad and erroneous, *e.g.* most of the illustrations of cultivations, and some of the sections stained in one or two colours, while many others are badly printed. We fail to see the use of such a confusing and diagrammatic crowd of illustrations as form plate i. The authors give numerous references to other workers, but there is, we think, a rather large dose of reference to their own works, such reference occurring in remarkable sameness. In several instances they do not seem to have read the original statements of their references. Thus, for instance, Cornil and Chantemesse in 1887 described and illustrated the pathology of the disease known in England as swine fever; they describe it under the name of "pneumo-entérite des porcs," both the name and the pathology of which disease have been minutely described and copiously illustrated in the Reports of the Medical Office of the Local Government Board for 1878-79, under the name of "infectious pneumo-enteritis of the pig." If they had really read the text in that English Blue-book, and inspected the illustrations of the pathology of our infectious pneumo-enteritis of the pig, they could not have failed to see that they are using the same name for the same disease, viz. they would have recognized the identity of the English and French disease; as a matter of fact, they give us erroneously to understand that our infectious pneumo-enteritis is the French "rouget de porcs," a disease utterly different from it.

There are various assumptions as regards the biology of certain specific bacteria, for which not even an attempt at proof is undertaken. Thus the authors ascribe to the bacillus of diphtheria and the bacillus of typhoid fever the power to form spores, and this deduction they make from certain differentiated granules in the bacilli brought out by staining; but every bacteriologist knows very well that such proof is valueless, unless the alleged spores have been observed to be able to germinate; and unless certain experimental evidence (drying, heating, action of antiseptics) is forthcoming, no one would be justified in concluding that those granules in bacilli are spores.

Though the work, as has been remarked, is not all that could be wished, it nevertheless deserves to take a high place amongst the text-books on bacteriology. It is written in a very lucid style, and abounds in valuable and original observations.

OUR BOOK SHELF.

Verslag omtrent den Staat van 'S Lands Plantentuin te Buitenzorg en de daarbij behoorende Inrichtingen over het Jaar 1889 ("Report on the Condition of the State Gardens at Buitenzorg"). (Batavia: Government Printing Office, 1890.)

UNDER the able direction of Dr. Treub, the author of the Report before us, the botanic garden at Buitenzorg in Java has developed into one of the most important establishments of the kind in existence, and has become an active centre of both scientific and practical botany. The present Report renders an account of the staff, publications, library, herbarium, museum, botanical laboratory, chemical-pharmacological laboratory, botanical garden, experimental garden for trials of trees producing gutta-percha, together with copious lists of plants and seeds distributed and received. As usual during the past few years, a number of foreigners, chiefly Germans, have occupied tables in the laboratory, having travelled from Europe expressly for the purpose of availing themselves of the facilities there afforded for original researches. This Report is recommended to botanists contemplating work in the tropics. W. B. H.

The Electric Light. Fifth Edition. By A. Bromley Holmes, M.Inst.C.E. (London: Bemrose and Sons, 1890.)

QUITE recently we had occasion to refer to the rapid growth that has taken place during the last few years in the development and application of electricity, and at the same time we gave an illustration of one of the latest forms of gas-engines for its production. This work places before the reader (who is assumed to be quite ignorant of electrical science) a popular and intelligent account "of the means used for producing electric light." The author, at the commencement, conscious of the difficulty of employing technical terms, explains each in simple language, so that their meanings may be fairly grasped by the reader.

After showing how electricity can be produced by batteries, he explains how it may be produced by mechanical means, leading up to the latest forms of dynamos. A very meagre description is given of these dynamos, descriptions of which, if they had been more fully treated, would have added great interest to this part of the work.

Conversion of electricity into light, and storage of electricity, are the subjects of the next two chapters, in the former of which are described the various lamps employed in lighting, with an account of the many self-adjusting arrangements for the purpose of keeping the carbon poles at a constant distance from each other. The remaining chapters deal with the distribution and measurement of electricity, testing and necessary precautions, and selection of light, in which the selection of lamp and the arrangements for any particular purposes are discussed. The author also treats of motive power and cost, concerning the latter of which he gives some interesting statistics relating to the comparison of gas and electricity.

On the whole, the author has produced an interesting book, which explains in a simple manner the elements of electric lighting.

Maps and Map-Drawing. By William A. Elderton. "Macmillan's Geographical Series." (London: Macmillan and Co., 1890.)

THIS little book will be found to form a most useful supplement to the works on geography published in the same series. In it an excellent brief account is given concerning the history of maps from the records of the Egyptians down to the present day. Then follow various methods of making surveys, including descriptions of the various instruments employed, such as the prismatic compass theodolite, sextant, &c. In the section on the globes, a short summary is given of ancient and curious globes, succeeding which are descriptions showing how latitudes and longitudes, day and night, &c., are measured, also the principles of great circle sailing. Part iv. deals with map-drawing, in which a brief but plain description is given of the various methods of projection, such as orthographic, gnomonic, stereographic, conical, &c.; reference also is made to the different symbols used in map-drawing. The last two parts treat of map copying and memory-maps, of which the latter will be found of great importance, for, by using the method adopted, and carrying out the suggestions, the learner may remember much that might otherwise be forgotten.

Krystallographisch-chemische Tabellen. Von Dr. A. Fock. (Leipzig: Wilhelm Engelmann, 1890.)

THIS little work of ninety pages supplies a much-needed want in chemical crystallography. The increase of late years in the number of original memoirs, describing the crystallograpical characters of newly-discovered chemical compounds, and pointing out relations between many of the longer known ones, has been so great that the text-books now in use, such as that of Rammelsberg, are very much out of date. Dr. Fock presents the data acquired up to the present in a very condensed and easily referable form; a form, moreover, which at once exhibits such relationships as have been noticed between chemical composition and crystalline form. The tables will be found to include brief descriptions of all the more recent measurements contributed to the *Zeitschrift für Krystallographie*, as far as regards the crystalline system and the elements of the crystals measured. The arrangement adopted is somewhat similar to that in Groth's "Tabellarischer Uebersicht des Mineralien," but including in addition almost all the known chemical salts, and all the measurements of organic compounds yet made. The information furnished concerning each of the compounds mentioned consists of (1) its symmetry, (2) the ratio of its axes, (3) the axial angles of monoclinic and triclinic compounds, and (4) the observer by whom the measurements were made. In many places, where important relations have been noticed, additional information of a character extremely useful for lecture purposes is given. It is quite evident from the whole character of the work that great care and a large expenditure of time have been involved in its preparation, and those who are interested in the subject must feel greatly indebted to Dr. Fock for collecting such a valuable store of information in so handy a form.

A. E. TUTTON.

LETTERS TO THE EDITOR.

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

Mr. Wallace on Physiological Selection.

BY his second letter Mr. Wallace leaves no possibility of doubt touching (1) the manifest agreement, and (2) the alleged difference, between his recent theory of cross-infertility in relation to the origin of species, and the preceding theory on the same subject, as published by Mr. Catchpool, Mr. Gulick, and myself.

(1) The manifest agreement consists in supposing, as he says, that some amount of infertility characterizes the distinct varieties which are in process of differentiation into species; and that such "incipient infertility" is of so much importance in this "process of differentiation" that its absence may be regarded as one of the *usual* causes of the failure of varieties to become developed into distinct species.

(2) The only point of difference alleged, is, that while Mr. Wallace says this incipient infertility can *never* arise alone, or except in association with some other and preceding varietal difference, we (according to him) have represented that it must *always* arise "alone, in an otherwise undifferentiated species," and therefore always constitute the *initial* change in the way of varietal divergence.

Such being the only point of difference alleged, it is obvious in the first place that the allegation, even if valid, has reference to a point of but secondary importance. For if we are all agreed that the "incipient infertility," whenever it does arise, is a factor of such high importance in the origination of species as Mr. Wallace now admits, surely the question whether it can ever arise before (or can only arise after) incipient varietal characters of any other kind becomes a question of comparatively little consequence. But now, in the second place, the allegation is not valid, being, in fact, the very opposite of the truth. Taking first my own presentation of the theory, both in "the original paper and in the summary of it published in NATURE," I not only expressly stated, but carefully argued, that the incipient infertility may arise *either before or after* variations of any other kind; and, in order to emphasize this distinction, I devoted one part of the paper to the first class of cases, while relegating to another part my consideration of the second class. Therefore it is merely by an eclectic method of quotation that Mr. Wallace now represents that I began by setting forth only one side of "physiological selection," or the cases where incipient infertility is the prior change. Why he should persistently ignore all the other part of the same paper, or the cases where I show that incipient infertility need not be the prior change, I do not care to inquire. But at least the omission cannot be due to any want of clearness on my part, inasmuch as in his first criticism of the paper, which he published several years ago, he displayed a complete understanding of what I had said upon this point.[1]

After this much explanation it seems almost needless to say that I stand by every one of the "eight quotations" which Mr. Wallace has given. For "it [still] appears to me much the more rational view that the primary specific distinction is likewise, as a rule, the primordial distinction; and that the cases where it has been superinduced by the secondary distinctions are comparatively few in number;" "it is [still] on what may be called spontaneous variability of the reproductive system itself that I mainly rely for evidence of physiological selection;" I still continue to ask, "Why should we suppose that, unlike all other variations, it [*i.e.* the physiological variation] can never be independent?" And so on through all the eight selected sentences, provided that any regard at all be paid to their context and relation to other parts of the paper. For no one of these sentences in the smallest degree affects the position which from the first I have consistently and persistently held—viz. that it makes no difference to the theory in what proportional number of cases the physiological change has been the prior change. Indeed, the *immediate* context of the first of the above quotations sets forth that it would make no difference to the theory even if we were to suppose that in *no* case can the physiological change have been the prior change.[2] In other words, it is expressly stated that, even if we were to adopt the identical opinion on which alone Mr. Wallace now relies as constituting any difference at all between his theory and my own, still the latter, in its "principle" or "essence," would be in nowise affected. Yet Mr. Wallace now accuses me of "an absolute change of front" on the sole ground that I repeat these statements![3]

So much for my own paper. Mr. Catchpool's enunciation of the theory was much too brief to admit of any fair criticism of the kind which Mr. Wallace now passes upon it. But Mr. Gulick's elaborate essays—which he abstains from mentioning—are quite another matter; and, as stated in my last letter, they considered much more fully than mine had done the subordinate

[1] *E.g.*, "Mr. Romanes then goes on to argue that, as a rule, these physiological variations are those which occur first, and form the starting-point of new species. He admits that in some ['possibly in many'] cases sterility may be a secondary character, due perhaps to the constitutional change indicated by the external variation; but even in that case physiological selection plays an equally important part, because if it [*i.e.* the incipient infertility] does not arise, either coincidently with the ordinary external variation, or as a consequence of it, then that variation will not be preserved, but will rapidly be extinguished by intercrossing with the parent form" (*Fortnightly Review*, 1886, p. 302). This brief extract is enough to show how widely Mr. Wallace's first representation of my "original paper" differs from his last, as regards the only point now in question.

[2] See NATURE abstract, vol. xxxiv. p. 339.

[3] There are several other distortions of my views in Mr. Wallace's letter, but space prevents me from dealing with them.

question at present before us. In the result Mr. Gulick completely agreed with me, that it cannot signify *how* or *when* the physiological variation of initial cross-infertility arises; for to whatever *causes* it may be due, and at whatever *time* in the process of varietal divergence it may first occur, it must alike furnish as highly important a condition to the origination of species as Mr. Wallace has eventually himself assigned to it.

I say "eventually," because Mr. Wallace has never before expressed himself to the effect that, in his opinion, cross-infertility is a factor of such prime importance in the origination of species. Why has he never done so? Surely the matter is one of sufficient magnitude to have justified some mention in one or other of the many valuable "contributions" which he has made to the theory of evolution. Or, not to go further than his past criticisms of my own paper on the subject, what pages of controversy he might have saved in this journal and elsewhere by stating, at any time within the last four years, that he had no disagreement with me touching the probable occurrence, and the important consequence, of some degree of infertility characterizing varieties which afterwards, and on this account, develop into species; but merely doubted whether any degree of infertility could ever arise before differentiation of some other kind had begun to take place. Such criticism would have been mild indeed. But hitherto the crown and front of opposition to the theory of physiological selection has been that, in representing cross-infertility as a factor of any great importance in the origination of species, the theory is not only untrue in itself, but tends to "shrivel up natural selection to very small dimensions." Now, however, criticism "changes front." It is no longer denied, but actually upheld, that "selective fertility" is as highly important a "co-operative cause in the origination of species" as I have ever claimed; and the new attack is directed only to a very subordinate point—a point, moreover, which both Mr. Gulick and myself had expressly anticipated, fully discussed, and shown not to belong to "the essence of the theory."[1]

Oxford, December 22, 1890. GEORGE J. ROMANES.

Molecular Dispersion.

IN the notes that appeared in your journal of December 11 (p. 133), you gave a very full account of some papers lately published in the *Bulletin de la Société Chimique de Paris*, on optical dispersion, by Messrs. Barbier and Roux.

This investigation is a remarkable instance of how laborious and intelligent work may be almost wholly thrown away for want of the knowledge of what has been previously done in the same direction. The authors commence their first paper with the astounding statement: "Dispersion has never been studied from the point of view of the relations which connect this physical property of bodies with their composition, their molecular weight, and their chemical constitution." This, however, was attempted by Sir John Herschel more than half a century ago, though with little success: and most of the scientific men who have best elucidated the subject of molecular refraction, such as Mascart, Brühl, and Nasini, have paid some attention also to dispersion. Mr. Dale and I gave numerical values for dispersion equivalents, analogous to Landolt's "refraction equivalents," for CH_2, Cl, &c., as far back as 1866. The dispersion of isomeric bodies was treated by me in 1881; and within the last few years I have communicated papers on the subject of molecular dispersion to learned Societies in England, France, and Switzerland. Some of the substances worked on by Messrs. Barbier and Roux have not, I think, been optically examined before; but the value of their careful observations is much diminished by their having measured two lines of the tin spectrum, instead of the A and H of the solar spectrum, or the α and γ of that of hydrogen; thus their results cannot easily be compared with the hundreds of measurements of dispersion already published by others. They have unfortunately employed Cauchy's formula, and have taken as the "specific dispersive power," $\frac{B}{d}$; that is, Cauchy's B divided by the density of the substance, instead of $\frac{n_a - n_b}{d}$, the difference between the refraction of two lines divided by the density. Brühl commenced to work in the same way, but shows in a paper in *Liebig's Annalen* for August 1886 that the method is unsatisfactory. The generalizations of our present authors are definite, and apparently correct, but they could have been mostly foreseen and explained if they had more clearly grasped the idea of molecular dispersion.

In the early summer I sent Messrs. Barbier and Roux copies of my papers on the subject; on August 1 they acknowledged receipt, and promised to refer in a forthcoming paper to previous work. As the paper reviewed in NATURE appeared in a preliminary abstract in the *Comptes rendus* of July, it is evident that they have yet something to bring before the public.

J. H. GLADSTONE.

17 Pembridge Square, December 27, 1890.

Weighing with a Ternary Series of Weights.

IN a former communication (NATURE, November 13, p. 30) I omitted, in order to avoid prolixity, to show how readily any given number may be expressed in the notation therein proposed. To effect this, it is only necessary to express the given number in the ternary scale, and then, beginning on the right, to substitute for 2, wherever it occurs, − 1, whilst increasing the previous figure by 1. When 3 occurs in the application of this rule, it must of course be replaced by 0, the previous figure being again increased by 1.

Examples:—

41 is in ternary notation	1112,	in new notation	11111.
500 ,, ,, ,,	200112,	,, ,,	1101111.
71 ,, ,, ,,	2122,	,, ,,	10101.

Or (still more briefly) in dividing by 3, to express the number in the ternary scale, we may substitute for the remainder 2, wherever it occurs, − 1, and increase the quotient by 1.

Examples:—

425		474		500	
142 remainder	− 1	158 remainder	0	167 remainder	− 1
47 ,,	1	53 ,,	− 1	56 ,,	− 1
16 ,,	− 1	18 ,,	− 1	19 ,,	− 1
5 ,,	1	6 ,,	0	6 ,,	1
2 ,,	− 1	2 ,,	0	2 ,,	0
1 ,,	− 1	1 ,,	− 1	1 ,,	− 1
0 ,,	1	0 ,,	1	0 ,,	1
3 + 27 + 729 − (1 + 9 + 81 + 243)		729 − (3 + 9 + 243)		27 + 729 − (1 + 3 + 9 + 243)	

Bradford, December 13, 1890. J. WILLIS.

PROF. EVERETT's rule (NATURE, December 4, p. 104) is needlessly complicated. All that is necessary is to express the given weight in the ternary scale with digits that are either 0, + 1, or − 1. His example is thus solved:—

```
3)500
 3)167 − 1
  3)56 − 1
  3)19 − 1
   3)6 + 1
   3)2 + 0
     1 − 1
```

or $500 = 1\bar{1}01\bar{1}\bar{1}\bar{1}$ ternary

$= 729 - 243 + 27 - 9 - 3 - 1.$

We thus see generally that, to express weights in units and powers of n, we must be provided with $\frac{1}{2}n$ or $\frac{1}{2}(n-1)$ balance-

[1] P.S.—Mr. Wallace alludes to my "standards of scientific reasoning and literary consistency." As regards the former, I am satisfied with a full and independent corroboration by a consistent and a logical mind. As regards the latter, it is enough to quote the concluding words of my reply to Mr. Wallace's first criticism of four years ago:—"The *main* feature of the theory is what my paper states it to be—viz. that sterility with parent forms is one of the *conditions*, and not *always* one of the *results*, of specific differentiation. But, if so, is it not evident that all causes which induce sterility are comprised by the theory, whether these causes happen to affect a few individuals sporadically, a number of individuals simultaneously, or even the majority of an entire species?" (*Nineteenth Century*, January 1887). And is it not equally evident, as elsewhere stated, that it does not signify whether the sterility arises before or after the "differentiation" has begun; seeing that, in either case, without the sterility the differentiation (as Mr. Wallace now says) will usually fail to proceed to the formation of distinct species? I have no space to discuss Mr. Darwin's views on this subject; but assuredly they are far from those which are expressed either here or in "Darwinism."

weights of each sort, according as n is even or odd; and that then any given weight can be expressed in a single way only if n is odd.

R. E. B.

I AM glad to have elicited a simple and direct rule for the required distribution, to obviate the necessity of a tedious tentative process, or of the reference to tables suggested in Mr. Willis's first letter. My own rule effected the desired object, but the device of admitting negative as well as positive remainders in the successive divisions by 3 is a decided improvement. R. E. B.'s method is identical with Mr. Willis's second method, and is undoubtedly the best. Its relation to my method is seen by noting that $\frac{1}{2}(2^7 - 1)$ is ternary 1111111, which, being added to $1\bar{1}01\bar{1}\bar{1}\bar{1}$, converts it into 2012000. My rule might have been generalized by adding (instead of *the least* value) *any* value of $\frac{1}{2}(2^n - 1)$ that exceeds the given weight.

In connection with Mr. Willis's suggestion (in his first letter) of tables for finding what number a person has thought of, I may mention that I published through Simpkin and Marshall, nearly forty years ago, a set of 4 cards for this purpose under the name of "Sibylline Leaves," of which a few specimens are still in my possession. Taking advantage of the fact that weights of 1, 3, 9, 27 will make up any integer from -40 to $+40$, that is 81 different integers when 0 is included, the numbers on the cards ran from 1 to 81. The computation consisted in taking 41 to start with, and adding or subtracting 1, 3, 9, or 27, two kinds of type being employed to distinguish between addition and subtraction.

J. D. EVERETT.

The Composition of Sea-Water.

COULD any reader of NATURE who may have given attention to the subject offer some explanation of the fact that the water of the sea contains such a very large excess of sodium salts relatively to salts of potassium?

Many of us have thought about the question, and heard it discussed, but I am not aware that any satisfactory conclusion has been arrived at.

I think it is usually assumed that the salts in the ocean have been mainly derived from the solutions carried in by rivers; solutions formed during the waste of rocks at the surface, or brought up by springs. Alkaline salts in such solutions will be principally due to the decay of felspars; and if we consider the rocks of the earth's crust, we find that the potash-felspars very much exceed in quantity the soda-felspars. It used to be considered that in earlier geological periods the "acid," mainly potash-bearing rocks, so enormously exceeded the "basic" rocks, in which the soda-felspars occur principally, that these latter were relatively quite insignificant in amount. Later petrographical work tends to show that this preponderance may not have been so large as was once supposed, but still there is no question that the excess of the potash-bearing rocks was, and is, very great. We might therefore look for more potash than soda in the drainage waters. We find, however, that in the sea, and in the rivers, the reverse is the case. Instances are, indeed, quoted (Roth, "Chemische Geologie") of rivers with more potash than soda in solution, but only as exceptions, and at points where only granite and gneiss had been drained.

This excess of soda in river-waters may be explained by the fact that though more potash-rocks are exposed than soda-rocks, yet the more rapid decay of the soda-lime felspars causes the proportions of the dissolved salts to be such as we find them; though this would hardly suffice to explain the great difference we find in the ocean.

Some people, again, assume that the composition of sea-water, though it may have been modified by the river-waters, is due to the constituents which were contained in the original first ocean as it condensed on the surface of the cooling earth—the "Urmeer" of the Germans; and Roth points out, in discussing analyses of river-waters and sea-water, that the composition of the former is such that they never could be the cause of the present composition of the latter.

This going back to the original ocean may be, as indeed it seems to be, the only explanation open to us, but it is not without its weak points. If potash-salts so greatly exceed soda-salts in the earth's crust now, we may assume that the same relative proportions existed, more or less, when the whole mass was still gaseous; and it is not easy to see any reason why, when cooling and condensation had allowed of the formation of a mass of molten silicates, the sodium-salts should have remained in great excess in the still uncondensed heated atmosphere out of which the "Urmeer" would eventually be deposited on the cooling crust. Mere difference of volatility of the respective salts would not suffice to account for this; and if we are to take the hypothesis at all, we must perhaps assume that when a low enough temperature had been reached to allow of the combination of the respective elements to molten silicates, the potassium, as the stronger base, would be taken more largely into these combinations by preference to the sodium, which would partly remain in the still intensely heated atmosphere, to be condensed with other vapours at a later period.

M.

Birds' Nests.

IN addition to your "curious places for birds' nests," I give you my own experience between 1842 and 1882 at Highfield House, Nottinghamshire.

Redbreast for four consecutive years on a shelf.

Redbreast in a fern (*Platycerium alcicorne*) for four consecutive years. Greenhouse.

Redbreast in a *Strelitzia regina* plant. Greenhouse.

Hedge Warbler in a tall Fuchsia in a greenhouse.

Chiff Chaff in a fern (two years).

Pied Wagtail on a shelf in vinery (two years).

Flycatcher on hinge of door (ten consecutive years).

Flycatcher on ledge of thermometer stand (three years).

Wren in a Daniels's hygrometer stand.

Shirenewton Hall, Chepstow. E. J. LOWE.

Butterflies Bathing.

IN answer to the inquiry of Mr. G. A. Freeman (NATURE, vol. xlii. p. 545) as to the food and habits of *Papilio macleayanus*, the butterfly which has been observed to visit water apparently for the purpose of performing its ablutions, I may inform him that the species is commonly found about Sydney, where it feeds in its larval condition on the camphor laurel (*Laurus camphora*), and the tender shoots and leaves of the orange. It certainly is not aquatic during any part of its life, nor do the plants upon which it feeds grow near water; the insect simply follows the example of its brothers, depositing its eggs singly, and undergoing the transformations on the food-plant as any reasonable butterfly should. Mr. G. Lyell's note as to the bathing habits of *P. macleayanus* is most interesting, and as far as I am aware the observation is entirely new, although many butterflies of the family Lycænidæ frequent pools on very hot days, settling on the mud at their margins, probably in search of a little moisture. Only recently at Toowoomba, in Queensland, I noticed a number of *Holochila absimilis* settled about puddles formed on the roads by a passing shower.

A. SIDNEY OLLIFF.

Department of Agriculture, Macquarie Street, Sydney, November 11, 1890.

THE RESEARCHES OF DR. R. KŒNIG ON THE PHYSICAL BASIS OF MUSICAL SOUNDS.[1]

I.

NOT often does it fall to the lot of a scientific man to become the mouthpiece of another whose researches have lasted over a quarter of a century; yet this is the enviable position in which I find myself on this occasion as the spokesman of Dr. Rudolph Kœnig, who is known not only as the constructor of the finest acoustical instruments in the world, but as an investigator of great originality and distinction, and author of numerous memoirs on acoustics. Dr. Kœnig, who has of late made very important contributions to our knowledge of the physical basis of music, using apparatus immeasurably superior to any hitherto employed in experimental investigations of this subject, has on various occasions, when I have visited him in Paris, shown me these instruments, and repeated to me the results of his researches. Important as these

[1] By Prof. Silvanus P. Thompson. (Communicated by the author, having been read to the Physical Society of London, May 16, 1890.)

are, they are all too little known in this country, even by the professors of physics. It was, therefore, with no little satisfaction that the Council of the Physical Society learned that Dr. Kœnig was willing to send over to London for exhibition on this occasion the instruments and apparatus used in these researches. And their satisfaction to-day is heightened by the fact that Dr. Kœnig has himself very kindly come over to demonstrate his own researches, and has given us the opportunity to welcome him personally amongst us.

The splendid apparatus around me belongs to Dr. Kœnig, and forms but a very small part of the collection which adorns his *atélier* on the Quai d'Anjou. He lives and works in seclusion, surrounded by his instruments, even as our own Faraday lived and worked amongst his electric and magnetic apparatus. His great tonometer, now nearly completed, comprises a set of standard tuning-forks, adjusted each one by his own hands, ranging from 20 vibrations per second up to nearly 40,000, with perfect continuity, many of the forks being furnished with sliding adjustments, so as to give by actual marks upon them any desired number of vibrations within their own limits. Beside this colossal masterpiece, Dr. Kœnig's collection includes several large wave-sirens, and innumerable pieces of apparatus in which his ingenious manometric flames are adapted to acoustical investigation. There also stands his tonometric clock, a timepiece governed, not by a pendulum, but by a standard tuning-fork, the rate of vibration of which it accurately records.

It is not surprising that one who lives amongst the instruments of his own creation, and who is familiar with their every detail, should discover amongst their properties things which others whose acquaintance with them is less intimate have either overlooked or only imperfectly discerned. If he has in his researches advanced propositions which contradict, or seem to contradict, the accepted doctrines of the professors of natural philosophy, it is not that he deems himself one whit more able than they to offer mathematical or philosophical explanations of them: it is because, with his unique opportunities of ascertaining the facts by daily observation and usage, he is impelled to state what those facts are, and to propound generalized statements of them, even though those facts and generalized statements differ from those at present commonly received and supposed to be true.

At the very foundations of the physical theory of music stand three questions of vital importance:—

(1) Why is it that the ear is pleased by a succession of sounds belonging to a certain particular set called a scale?

(2) Why is it that when two (or more) musical sounds are simultaneously sounded, the ear finds some combinations agreeable and others disagreeable?

(3) Why is it that a note sounded on a musical instrument of one sort is different from, and is distinguishable from, the same note sounded with equal loudness upon an instrument of another sort?

These three queries involve the origin of *melody*, the cause of *harmony*, and the reason of *timbre*.

The theories which have been framed to account for each of these three features of music are based on a double foundation—partly physical, partly physiological. With the physiological aspect of this foundation we have to-night nothing to do, being concerned only with the physical aspect. What, then, are the physical foundations of melody, of harmony, and of timbre? Demonstrable by experiment they must be, in common with all other physical facts, otherwise they cannot be accepted as proven. What are the facts, and how can they be demonstrated?

We are not here, however, to fight over again the battle of the temperaments, nor do I purpose to enter upon a discussion of the origin of melody, which, indeed, I believe to be associative rather than physical. I shall confine myself to two matters only, with which the recent researches of Dr. Kœnig are concerned—the *cause of harmony* and the *nature of timbres*. Returning, then, to the ratios of the vibration numbers of the major scale, we may note that two of these, namely, the ratios 9 : 8 and 15 : 8, which correspond to the intervals called the major whole tone and the seventh, are dissonant—or, at least, are usually so regarded. It will also be noticed that these particular fractions are more complex than those that represent the consonant intervals. This naturally raises the question: *Why is it that the consonant intervals should be represented by ratios made up of the numbers* 1 : 6, *and by no others?*

To this problem the only answer for long was the entirely evasive and metaphysical one that the mind instinctively delights in order and number. The true answer, or rather the first approximation to a true answer, was only given about forty years ago, when von Helmholtz, as the result of his ever-memorable researches on the sensations of tone, returned the reply: *Because only by fulfilling numerical relations which are at once exact and simple can the "beats" be avoided which are the cause of dissonance.* The phenomenon of beats is so well known that I may assume the term to be familiar. An excellent mode of making beats audible to a large audience is to place upon a wind-chest two organ-pipes tuned to $ut_2 = 128$, and then flatten one of them slightly by holding a finger in front of its mouth. Von Helmholtz's theory of dissonance may be briefly summarized by saying that any two notes are discordant if their vibration numbers are such that they produce beats, maximum discordance occurring when the beats occur at about 33 per second; beats if either fewer than these, or more numerous, being less disagreeable than beats at this frequency. It is an immediate consequence that the degree of dissonance of any given interval will depend on its position on the scale. For example, the interval of the major whole tone, represented by the ratio 9 : 8, produces four beats per second at the bottom of the pianoforte keyboard, 32 beats per second at the middle of the keyboard, and 256 beats per second at the top. Such an interval ought to be discordant, therefore, in the middle octaves of the scale only.

To this view of von Helmholtz it was at first objected that, if that were all, all intervals should be equally harmonious provided one got far enough away from being in a bad unison: fifths, augmented fifths, and sixths minor and major, ought all to be equally harmonious. This no musician will allow. To account for this von Helmholtz makes the further supposition that the beats occur, not simply between the fundamental or prime tones, but also between the upper partials which usually accompany prime tones. This leads me to say a word about *upper partial tones* and *harmonics*. I believe many musicians use these two terms as synonymous; but they ought to be carefully distinguished. The term harmonics ought to be rigidly reserved to denote higher tones which stand in definite harmonic relations to the fundamental tone. The great mathematician Fourier first showed that any truly periodic function, however complex, could be analyzed out and expressed as the sum of a certain series of periodic functions having frequencies related to that of the fundamental or first member of the series as the simple numbers 2, 3, 4, 5, &c. Thirty years later, G. S. Ohm suggested that the human ear actually performs such an analysis, by virtue of its mechanical structures, upon every complex sound of a periodic character, resolving it into a fundamental tone, the octave of that tone, the twelfth, the double octave, &c. Von Helmholtz, arming himself with a series of tuned resonators, sought to pick up and recognize as members of a Fourier series, the higher harmonics of the tones of various instruments. In his researches he goes over the ground previously traversed by Rameau, Smith, and Young, who had all observed the co-existence, in the tones of musical instruments, of higher partial tones. These higher tones correspond to higher modes of vibration in which the vibratile organ—string, reed, or air-column—subdivides into two, three, four, or more parts. Such parts naturally possess greater frequency of vibration, and their higher tones, when they co-exist along with the lower or fundamental tone are denominated *upper partial tones*, thereby signifying that they are higher in the scale, and that they correspond to vibrations *in parts*. It is to be regretted that Prof. Tyndall, in his lectures on sound, rendered von Helmholtz's *Oberpartialtöne* by the term *overtones*, omitting the most significant half of the word. To avoid all confusion in the use of such a term I shall rather follow Dr. Kœnig in speaking of these as *sounds of subdivision*. And I must protest emphatically against calling these sounds harmonics, for the simple reason that in many cases they are very inharmonious. It is a matter to which I shall recur presently.

Returning to the subject of beats, the question arises, What becomes of the beats when they occur so rapidly that they cease to produce a discontinuous sensation upon the ear? The view which I have to put before you in the name of Dr. Kœnig is that they blend to make a tone of their own. Earlier acousticians have propounded, in accordance with this view, that the *grave harmonic* of Tartini (a sound which corresponds to a frequency of vibration, that is the difference between those of the two tones producing it) is due to this cause. Von Helmholtz has taken a different view, denying that the beats can blend to form a sound, giving reasons presently to be examined. Von Helmholtz considered that he had discovered a new species of combinational tone—namely, one corresponding in frequency to the *sum* of the frequencies of the two tones, whereas that discovered by Tartini (and before him by Sorge) corresponded to their *difference*. Accordingly, he includes under the term of combinational tones the differential tone of Tartini and the summational tone which he considered himself to have discovered. To the existence of such combinational tones he ascribed a very important part in determining the character, harmonious or otherwise, of chords; and to them also he attributes the ability of the ear to discriminate between the degrees of harmoniousness possessed by such intervals (fifths, sixths, &c.) as consist of two tones too widely apart on the scale to give beats of a discontinuous character. He also considers that such combinational tones are chiefly effective in producing beats, the summational tones of the primaries beating with their upper partial tones; and that this is the way in which they make an interval more or less harmonious.

The whole fabric of the theory of harmony, as laid down by von Helmholtz, is thus seen to repose upon the presence or absence of beats; and the beats themselves are in turn made to depend, not upon the mere interval between two notes, but upon the timbres also of those notes, as to what upper partials they contain, and whether those partials can beat with the summational tone of the primaries. It becomes, then, of the utmost importance to ascertain the precise facts about the beats and about the supposed combinational tones. What the numbers of beats are in any given case: whether they do or do not correspond to the alleged differential and summational tones: these are vital to the theory of harmony. Equally vital is it to know what the timbres of sounds are, and whether they can be accurately or adequately represented by the sum of a set of pure harmonics corresponding to the terms of a Fourier series.

In investigating beats and combinational tones, Dr. Kœnig deemed it of the highest importance to work with instruments producing the purest tones; not with harmonium reeds or with polyphonic sirens, the tones of which are avowedly complex in timbre, but with massive steel tuning-forks, the pendular movements of which are of the simplest possible character. Massive tuning-forks properly excited by bowing with a violoncello bow, or, in the case of those of high pitch, by striking them with an ivory mallet, emit tones remarkably free from all sounds of subdivision, and of so truly pendular a character (unless over-excited) that none of the harmonics corresponding to the members of a Fourier series can be detected. No living soul has had a tithe of the experience of Dr. Kœnig in the handling of tuning-forks. Tens of thousands of them have passed through his hands. He is accustomed to tune them himself, making use of the phenomenon of beats to test their accuracy. He has traced out the phenomena of beats through every possible degree of pitch, even beyond the ordinary limits of audibility, with a thoroughness utterly impossible to surpass or to equal. Hence, when he states the results of his experience, it is idle to contest the facts gathered on such a unique basis. The results of Dr. Kœnig's observations on beats are easily stated. He has observed primary beats, as well as beats of secondary and higher orders, from the interference of two simple tones simultaneously sounded.

When two simple tones interfere, the primary beats always belong to one or other of two sets, called an *inferior* and a *superior* set, corresponding respectively in number to the two remainders, positive and negative, to be found by dividing the frequency of the higher tone by that of the lower.

This mode of stating the facts is a little strange to those trained in English modes of expressing arithmetical calculations; but an example or two will make it plain. Let there be as the two primary sounds two low tones having the respective frequencies of 40 vibrations and 74 vibrations. What are the two remainders, positive and negative, which result from dividing the higher number, 74, by the lower number 40? Our English way of stating it is to say that 40 goes into 74 once, and leaves a (positive) remainder of 34 over. But it is equally correct to say that 40 goes into 74 twice all but 6: or that there is a negative remainder of 6. Well, Dr. Kœnig finds that, when these two tuning-forks are tried, the ear can distinguish two sets of beats, one rapid, at 34 per second, and one slow, at 6 per second.

Again, if the forks chosen are of frequencies 100 and 512, we may calculate thus: 100 goes into 512 five times, plus 12; or 100 goes into 512 six times, minus 88. In this actual case the 12 beats belonging to the inferior set would be well heard: the 88 beats belonging to the superior set would probably be almost indistinguishable. As a rule, the inferior beat is heard best when its number is *less* than half the frequency of the lower primary, whilst, when its number is *greater*, the superior beat is then better heard. Dr. Kœnig has never been able to hear any primary beat which did not fall within this rule.

Dr. Kœnig will now illustrate to you the beats, inferior and superior, as produced by these two massive tuning-forks,[1] each weighing about 50 pounds, and each provided with a large resonating cavity consisting of a metal cylinder, about 4 feet long, fitted with an adjustable piston. One of them is tuned to the note $ut_1 = 64$. The other also sounds ut_1; but by sliding down its prongs the adjustable weights of gun-metal, and screwing in the piston of the resonator, its pitch can be raised a whole tone to $re_1 = 72$. Dr. Kœnig excites them with the 'cello bow, first separately that you may hear their individual tones, then together. At once you hear an intolerable beating—the beats coming 8 per second. This

[1] These splendid forks, with their resonators, along with other important pieces of Dr. Kœnig's apparatus, have since been acquired by the Science and Art Department for the Science Collection at South Kensington.

is the inferior beat, corresponding to the positive remainder; the superior beat you cannot hear. Dr. Kœnig will raise the note of the second fork from re_1 to $mi_1 = 80$; and the beats quicken to 16 per second. Raising it to $fa_1 = 85\frac{1}{3}$, and then to $sol_1 = 96$, while the first fork is still kept at ut_1, the beats increase in rapidity, but are fainter in distinctness. If Dr. Kœnig now substitutes for the second fork one tuned to $la_1 = 106\frac{2}{3}$, you may be able to hear two beats, the inferior one rapid and faint at $42\frac{2}{3}$ per second, and the superior one slower, but also faint, at $21\frac{1}{3}$ per second. Still raising the pitch to the true seventh tone = 112, the rapid inferior beat has died out, but now you hear the superior strongly at 16 per second. If it is raised once more to $si_1 = 120$ (the seventh of the ordinary scale), and the beats are still stronger and slower at 8 per second. Finally, when we bring the pitch up to the octave $ut_2 = 128$, we find that all beats have disappeared: there is a perfectly smooth consonance. The facts so observed are tabulated for you as follows:—

TABLE I.

Primary Beats.

Primary Tones.	Ratio.	Inferior Beats.	Superior Beats.
ut_1 re_1 / 64 72 ...	8 : 9	8	—
ut_1 mi_1 / 64 80 ...	4 : 5	16	—
ut_1 fa_1 / 64 $85\frac{1}{3}$ ...	3 : 4	$21\frac{1}{3}$	—
ut_1 sol_1 / 64 96 ...	2 : 3	32	32
ut_1 la_1 / 64 $106\frac{2}{3}$ ...	3 : 5	$42\frac{2}{3}$	$21\frac{1}{3}$
ut_1 (7) / 64 112 ...	4 : 7	—	16
ut_1 si_1 / 64 120 ...	8 : 15	—	8
ut_1 ut_2 / 64 128 ...	1 : 2	—	0

Suppose now, keeping the lower fork unaltered, we raise the pitch of the higher note (taking a new fork that starts at the octave) from ut_2 to sol_2 by gradual steps, we shall find that there begins a new set of primary beats—an inferior set, which are at first slow, then get more rapid and become undistinguishable, but succeeded by another rapid and indistinct, which grow stronger and slower, until, as the pitch rises to sol_2, the frequency of which is exactly three times that of ut_1, all beats again vanish. This range between the octave and the twelfth tone may be called the second "period," to distinguish it from the period from unison to the first octave, which was our first period. Similarly, the range from the twelfth tone to the second octave is the third period, and from thence to the major third above is the fourth period, and so forth. In each period, up to the sixth or seventh of such periods, a set of inferior and a set of superior beats may be observed; and in every case the frequency of the beats corresponds, as I have said, to one or other of the two remainders of the frequencies of the two tones. No beat has ever been observed corresponding to the sum of the frequencies, even when using the slowest forks. None has ever been observed corresponding to the difference of the frequencies, save in the first period; where, of course, the positive remainder is simply the difference of the two numbers.

That you may hear for yourselves the beats belonging to one of the higher periods, Dr. Kœnig will take a pair of forks which will give us some of the superior beats in the fourth period. One of the forks is the great $ut_1 = 64$, as previously used. The other is $mi_3 = 320$; their ratio being 1 : 5. Sounded together they give a pure consonance, but if the smaller one is loaded with small pellets of wax to lower its pitch slightly, and then bow it, at once you hear beats. It was in studying the beats of these higher periods that Dr. Kœnig made the observation that whereas the beats of an imperfect unison are heard as alternate silences and sounds, the beats of the (imperfect) higher periods—twelfth tone, double octave, &c.—consist mainly in variations in the loudness of the lower of the two primary tones; an observation which was independently made by Mr. Bosanquet, of Oxford.

Passing from the beats themselves, I approach the question, What becomes of the beats when they occur too rapidly to produce on the ear a discontinuous sensation? On this matter there have been several conflicting opinions: some holding, with Lagrange and Young, that they blend into a separate tone; others, with von Helmholtz, maintaining that the combinational tones cannot be so explained, and arise from a different cause. Let it be observed that, even if beat-tones exist, it is quite possible for beats and beat-tones to be simultaneously heard. A similar co-existence of a continuous and a discontinuous sensation is afforded by the familiar experiment of producing a tone by pressing a card against the periphery of a rapidly-rotating toothed wheel. There is a certain speed at which the individual impulses begin to blend into a continuous low tone, while yet there are distinguishable the discontinuous impulses; the degree of distinctness of the two co-existing sounds being dependent on the manner in which the card is pressed against the wheel—that is to say, on the nature of the individual impulses themselves. The opponents of the view that beats blend into a tone state plainly enough that, in their opinion, a mere succession of alternate sounds and silences cannot blend into a tone different from that of the beating tone. Having said that the beats cannot blend, they then add that they do not blend; for, say they, the combinational tones are a purely subjective phenomenon. Lastly, they say that even if the beats blend they will not so explain the existence of combinational tones, because the combinational tones have frequencies which do not correspond to the number of the beats.

In the teeth of all these views and opinions, Dr. Kœnig—without dogmatizing as to how or why it is—emphatically affirms that beats do produce *beat-tones*; and he has pursued the matter down to a point that leaves no room for doubting the general truth of the fact. The alleged discrepancy between the frequency of the observed combinational tones and that of the beats disappears when closely scrutinized. Those who count the beats by merely taking the difference between the frequencies of the two primary tones, instead of calculating the two remainders, will assuredly find that their numbers do not agree in pitch with the actual sounds heard. But that is the fault of their miscalculation. Those who use harmonium reeds or polyphonic sirens instead of tuning-forks to produce their primary tones must not expect from such impure sources to reproduce the effects to be obtained from pure tones. And those who say that the beats calculated truly from the two remainders will not account for the summational tones have unfortunately something to unlearn—namely, that, when pure tones are used, under no circumstances is a tone ever heard the frequency of which is the sum of the frequencies of the two primary tones.

The apparatus which Dr. Kœnig has brought over enables him to demonstrate, in a manner audible, I trust, to the whole assembly in this theatre, the existence of the beat-tones. His first illustrations relate to tones of primary beats, some belonging to the inferior, others to the superior set, in the first period.

He takes here the fork $ut_6 = 2048$, five octaves higher than the great ut_1. To excite it, he may either bow it or strike it with an ivory mallet. With it he will take the fork one note higher, $re_6 = 2304$. When he took the same interval with ut_1 and re_1, the number of beats was 8.

The *ut* and *re* of the next octave higher would have given us 16 beats, that of the next 32, that of the next 64, of the fourth octave 128, and that of the fifth 256. But 256 per second is a rapidity far too great for the ear to hear as separate sounds. If there were 256 separate impulses, they would blend to give us the note ut_3 = 256. They are not *impulses*, but *beats*: nevertheless, they blend. Dr. Kœnig strikes the ut_6, then the re_6, both shrill sounds when you hear them separately; but when he strikes them in quick succession one after the other, at the moment when the mallet strikes the second fork you hear this clear ut_3 sounding out. I am not going to waste your time in a disputation as to whether the sound you hear is objective or subjective. It is enough that you hear it, pure and unmistakable in pitch. It is the grave harmonic; and the number 256, which is its frequency, corresponds to the positive remainder when you divide 2304 by 2048.

Now let me give you a beat-tone belonging to the superior set: it also will be a grave harmonic, if you so please to call it; but its frequency will correspond neither to the difference nor to the sum of the frequencies of the two primary tones. Dr. Kœnig takes ut_6 = 2048 as previously, and with it si_6 = 3840. Let us calculate what the superior beats ought to be. 2048 goes into 3840 twice, less 256. Then, 256 being the negative remainder, we ought to hear from these two forks the beat-tone of 256 vibrations, which is ut_3, the same note as in our last experiment. He strikes the forks, and you hear the result. The beat-tone, which is neither a differential tone nor a summational tone, corresponds to the calculated number of beats.

If I take ut_6 = 2048 and sol_6 = 3072, the two remainders both come out at 1024, which is ut_5. Dr. Kœnig will first sound ut_5 itself, separately, on an ut_5 fork, that you may know what sound to listen for. Its sound has died away; and now he strikes ut_6 and sol_6, when at once you hear ut_5 ringing out. That sound which you all heard corresponds to the calculated number of beats. That is enough for my present purpose.

The next illustration is a little more complex. I select a case in which the beat-tones corresponding to the inferior and the superior beats will both be present. We shall have four tones altogether—two primary tones and two beat-tones. The forks I select are ut_6 = 2048, as before, and a fork which is tuned to vibrate exactly 11 times as rapidly as ut_3—it is the 11th harmonic of that note, but does not correspond precisely to any note of the diatonic scale. It has 2816 vibrations, and is related to ut_6 as 11 : 8. The two remainders will now be 768 and 1280, which are the respective frequencies of sol_4 and mi_5. Dr. Kœnig will first sound those notes on two other forks, that you may know beforehand what to listen for. Now, on striking the two shrill forks in rapid succession, the *two* beat-tones are heard.

If I select, instead of the 11th harmonic, the 13th harmonic of ut_3, vibrating 3328 times in the second, to be sounded along with ut_6, the same two beat-tones will be produced as in the preceding case; but mi_5 = 1280 is now the inferior one, corresponding to the positive remainder, whilst sol_4 = 768 is the superior tone, corresponding to the negative remainder. It is certainly a striking corroboration of Dr. Kœnig's view that the beat-tones actually heard in these last two experiments should come out precisely alike, though on the old view, that the combinational tones were simply the summational and differential tones, one would have been led to expect the sounds in the two experiments to be quite different.

One other example I will give you of a beat-tone belonging to the second period. The two primary notes are given by the forks ut_5 = 1024 and re_6 = 2304. The beat-tone which you hear is ut_3 = 256, which corresponds to the positive remainder.

It will be convenient to draw up in tabular form the results just obtained. These may be considered as abbreviations of the much more extended tables drawn up by Dr. Kœnig, which hang upon the walls, and which are to be found in his book, "Quelques Expériences d'Acoustique."

TABLE II.

Sounds of Primary Beats.

Primary Tones.			Ratio.	Inferior Beat-tone.	Superior Beat-tone.
ut_6 2048	re_6 2304	...	8 : 9	1 { ut_3 256	—
ut_6 2048	si_6 3840	...	8 : 15	—	1 { ut_3 256
ut_6 2048	sol_6 3072	...	8 : 12	4 { ut_5 1024	4 { ut_5 1024
ut_6 2048	(11th) 2816	...	8 : 11	3 { sol_4 768	5 { mi_5 1280
ut_6 2048	(13th) 3328	...	8 : 13	5 { mi_5 1280	3 { sol_4 768
ut_5 1024	re_6 2304	...	4 : 9	1 { ut_3 256	—

(*To be continued.*)

THE ORIGIN OF THE GREAT LAKES OF NORTH AMERICA.

AT one time glaciers—perhaps in the co-operative society of an ice-sheet—were gravely suspected of having excavated even the great lakes of North America. This, however, is hardly probable. The *a priori* difficulties in the hypothesis are great. Apart from objections which have often been pointed out, the work done would be on so gigantic a scale that a longer period must be assigned to the glacial occupation of the region than seems probable from other considerations. Further, the direct evidence which will presently be noticed seems conclusive against the hypothesis; but it may be affirmed with better reason that ice has indirectly aided in the process, though to what extent we can, as yet, hardly venture to say.

During the last few years numerous observations have been made, both in Canada and in the United States, upon the configuration of the lake beds, and the elevation of their ancient margins. To some of these Dr. Wright refers in his volume on "The Ice Age in North America," and Prof. J. W. Spencer (who has been engaged on this subject for several years) brings them into a focus in a paper recently published in the Quarterly Journal of the Geological Society of London.[1]

At first sight the great lakes, from Superior to Ontario, are suggestive of glacial excavation. They seem to occupy true rock basins. Superior discharges into Huron over the ledges—once a "portage"—of Sault Ste. Marie. Huron, as it were, leaks into Erie, the fall between the two sheets of water being only nine feet. Erie flows towards Ontario over the rocky rapids and the final precipice of Niagara; and the St. Lawrence, after leaving Ontario, gives frequent evidence of a rocky bed, the level of which is considerably above that of the bottom of the lake, for the depth of this near its eastern end is more than seven hundred feet. But more careful investigation of the lakes has shown that in these apparently perfect basins (as is sometimes discovered in household affairs) hidden cracks exist, which, under different physical conditions, would have let the water run out. This indeed is not the whole story; another agency must be presently mentioned; but that these apparent basins once had

[1] Vol. xlvi. p. 523 (read April 16, 1890).

outlets, by which they would have been drained, at any rate partially, seems beyond question.

The following is a brief statement of the results of sounding in the water and boring on the land. The surface of Lake Superior is 630 feet above sea-level, the deepest part of its bed 375 feet below that datum plane. The fall from the shore line is generally rather rapid; a large part of the basin is more than 300 feet deep, and a considerable area is below 600 feet. The original outlet, according to Dr. Wright, was on the southern side; and by this, in pre-glacial times, the drainage was discharged towards the Mississippi. But, apparently, the information in regard to the ancient valley-system of the Lake Superior area is less complete than in the case of the other lakes.

The Huron-Michigan basin at once arrests attention by its extraordinary outline. Michigan is a gigantic backwater without inclosing hills at its southern or upper end. Huron proper is almost divided from Georgian Bay by the Indian Peninsula and a chain of rocky islands, of which Manitoulin Island is the chief. All this is far more suggestive of submergence than of any other mode of formation. Closer study has confirmed first impressions. Michigan really consists of two basins, divided by a plateau submerged at a maximum depth of 342 feet; the northern and larger basin sinks to a depth of 864 feet, the southern one only descends to 576 feet. Hence, if the level of the water were lowered by 350 feet, Michigan would be divided into two lakes.

Of these, the former must have drained into the north-west end of Lake Huron. It is true that the deepest soundings at the present outlet do not exceed 252 feet, but near this a fjord-like channel has been traced in the shallower part, trending northward, with a depth of 612 feet, and there are indications of other buried channels. Thus there can be little doubt that in pre-glacial times the northern basin of Michigan communicated, as the lake still does, with that of Lake Huron. But as to the outlet of the southern basin there is some dispute. Prof. Spencer, however, states that a buried channel has now been traced along the valley of the Grand River, across the peninsula of Michigan to Saginaw Bay on Lake Huron. Its exact depth has not been ascertained, but it has been pierced in several places to depths of from 100 to 200 feet below the level of the lake, and in one case the drift was found to extend 500 feet below the surface of the ground, and 350 below that of Lake Huron.

Next as to the drainage of this lake. Submerged channels resembling river valleys have been traced along its bed. One is a prolongation, in a north-easterly direction, of that which has just been mentioned. Another runs to join it from the south—that is, in the opposite direction to that of the present flow of the water; and a third is a continuation of the channel which drained the northern basin of Michigan. These three ultimately come together, and the united valley rounds Cabot's Head, and makes for the southern end of Georgian Bay, keeping near its south-western side. Here also an ancient outlet has been found. Across the low flat land separating the waters of Georgian Bay from Lake Simcoe a buried channel has been struck in borings, at various depths—in one case 280 feet—below the surface of the latter. Between this and Lake Ontario, well-borings indicate that the drift is very deep, and that it conceals an ancient channel, which entered Lake Ontario some thirty miles west of Toronto. Lake Erie, which is generally less than 84 feet deep, also exhibits a buried system of ramifying valleys, and the line of discharge into Ontario was not over the lip of Niagara but by a deep valley, now choked with drift, which can be traced several miles to the west of the present course of the St. Lawrence. In Ontario, also, a channel has been found, the greatest depth of which is over 700 feet below the surface of the lake. This runs near the southern shore, and receives other valleys from this direction.

The conclusion to which these investigations point is that in pre-glacial times the great lakes did not yet exist, but their site formed part of a system of river valleys, which ultimately coalesced in one main channel, now concealed beneath the waters of the eastern part of Ontario. Of these valleys, the one was cut off from the united system of the other tributaries at Detroit, and the head waters of these were parted by the plateau now buried beneath Lake Michigan. Some, indeed, have contended that the water of these rivers passed from the Ontario region towards the Hudson, but Prof. Spencer considers that they were even then tributary to the St. Lawrence.

But it would not suffice to block these channels with glacial drift. Parts of Lake Superior, the southern basin of Michigan, a little of Huron, and the eastern end of Ontario are beneath the sea-level: the last as much as 491 feet below it. We must assume in addition a considerable downward movement of the whole area, otherwise these valleys could never have emptied themselves into the sea. To drain the valley occupied by Ontario would require, at the least, an elevation of more than 700 feet; Southern Michigan, of not much less, perhaps of more. This hypothesis, however, presents no real difficulty, for it can be proved that many regions have been affected by movements, both upwards and downwards, in glacial or post-glacial times. The coast of Norway and many parts of northern America have been affected by a great downward movement—amounting not seldom to at least a thousand feet, and sometimes even as much as a thousand yards. This, again, after the ice had melted away and the main physical features of the district were sculptured, was followed by one in the contrary direction, which may be occasionally measured by some hundreds of feet; as, for example, at the beaches of Novaya Zembla, the terraces of the Varanger Fjord, and of many another inlet in Norway. Of this movement also there is proof on the Fraser and other rivers in America.

But to convert Lake Ontario into a river valley it would not be enough to give a general uplift and clear away the dams of glacial drift. Differential movements of the earth's crust are required. That these have sometimes occurred has been long since proved, in the case of Norway. Now, careful observations, by Prof. Spencer and others, show the reasonableness of the hypothesis in the district of the great lakes. Around their shores are old terraces, which extend in some cases to a height of 1700 feet above the present water-level, and are indicative, in Prof. Spencer's opinion, of a depression to that amount. A series of careful measurements undertaken on different terraces and around more than one American lake prove that these terraces do not correspond with the existing contour lines, but have been affected by a differential uplift, amounting in one case to as much as 4 feet per mile.

Hence, it follows that the great North American lakes are of comparatively modern date, and are nothing more than a great system of river valleys, which have been converted into a chain of huge lakes, partly by the blockage of old channels, partly by differential movements of the earth's crust. If this view be established, and the evidence in its favour (which finds much support from other regions) appears very strong, it will help in elucidating several important questions, bearing on not only the history of the Glacial epoch and the exact mode of the accumulation of the *débris*, but also on the cause of the movements of a crust which is asserted by physicists to be rigid. But into these questions, fascinating as they are, want of space precludes us from inquiring on the present occasion.

T. G. BONNEY.

THE ISOLATION OF HYDRAZINE, $\begin{array}{l}NH_2\\ | \\ NH_2\end{array}$.

IN the June of 1887, Prof. Curtius, of the University of Erlangen, announced, in the *Berichte* of the Berlin Chemical Society, the important fact that he had succeeded in preparing the hydride of nitrogen, $\begin{array}{l}NH_2\\ | \\ NH_2\end{array}$, di-amidogen or hydrazine, and an account of his preliminary experiments was given in NATURE (vol. xxxvi. p. 185). Since that time several further contributions to the history of the base and its salts have been published by Drs. Curtius and Jay (see NATURE, vol. xxxix. p. 377, and vol. xli. p. 547), and the work is now completed by the publication, in the current number of the *Journal für praktische Chemie*, by Drs. Curtius and Schulz, of full details of the perfected methods of preparation of the base and its most important salts, together with the results of determinations of its vapour density and of its molecular weight when dissolved in water.

It will doubtless be remembered that free hydrazine was supposed by Prof. Curtius to be a gas, possessing such an exceedingly powerful affinity for water that it was found almost impossible to isolate it from its hydrate. This difficulty has since to a large extent been overcome, and the nature of the gas itself more certainly ascertained. The hydrate is a very definite compound of the composition $N_2H_4 . H_2O$, and it serves as the best starting-point for the preparation both of the free gaseous hydrazine itself and of the salts of the base.

Preparation of Hydrazine Hydrate.

Hydrazine hydrate is best prepared by distilling the sulphate, $N_2H_4 . H_2SO_4$, with alkalies. Hydrazine sulphate is a beautifully crystalline salt consisting of colourless rhombic tables, the macropinacoid being the most fully developed face. It is obtained most readily by the method of Curtius and Jay from triazoacetic acid.

The perfected method of preparing hydrazine hydrate from these crystals of the sulphate is as follows:—About one hundred grams of pure caustic potash are dissolved in 250 grams of water. After cooling, a hundred grams of the finely powdered crystals of the sulphate are added, and the mixture subjected to distillation. Owing to the tremendously active properties of hydrazine, the distillation apparatus requires to be constructed throughout of pure silver. The form employed by Prof. Curtius consists of a silver flask of about a litre capacity, which can be screwed in a gas-tight manner to the silver condenser. The connecting-tube between the flask and the condenser is bent into a double U shape, so that particles cannot possibly be projected from the flask into the condenser; a thermometer is inserted in the limb of the U nearest the condenser. The flask is protected from the flame by an asbestos cushion. Cork and india-rubber connections must be rigorously excluded, as hydrazine most energetically attacks them. When the powdered sulphate is added to the strong potash solution in the flask, considerable heat is generated and the thermometer rapidly rises. Heat is then carefully applied, and liquid begins to distil. As soon as the thermometer indicates 119° C., the boiling-point of hydrazine hydrate, the receiver is changed. It is then found that the temperature remains constant at 119° until almost the last drop, the liquid distilling at this temperature being almost pure $N_2H_4 . H_2O$. The distillate collected before the thermometer reached 119°, generally amounting to about 250 c.c., should then be fractioned. It is a most curious fact that, although the boiling-points of water and hydrazine hydrate are so close together, only very small quantities of the latter are carried over by steam. More than two-thirds of the 250 c.c. pass over on redistillation before the mercury rises above 100°. Between 100° and 106° a distillate is obtained which does not contain more than 1 per cent. of hydrazine. The 106°–117° fraction contains still only about 11 per cent., while that passing over between 117° and 119° contains 60–62·5 per cent. of hydrazine. The liquid obtained in the first distillation, while the thermometer indicated 119°, contains 64 per cent. of the free base, the theoretical quantity for the formula $N_2H_4 . H_2O$. From one hundred grams of hydrazine sulphate 36 grams of the pure hydrate are obtained in a careful experiment.

As regards the analysis of the hydrate, the amount of base, N_2H_4, may be determined by means of standard sulphuric acid, all the usual indicators except phenol phthalein being available. The nitrogen and hydrogen may be estimated by combustion with copper oxide, and the nitrogen also by reduction of platinum chloride, the whole of the nitrogen being evolved in the free state in accordance with the equation

$$N_2H_4 . H_2O + 2PtCl_4 = 2PtCl_2 + 4HCl + H_2O + N_2.$$

Molecular Composition of Hydrazine Hydrate.

The vapour density of the compound has been determined by Hofmann's method in the Torricellian vacuum, using a jacket of steam. The molecular weight corresponding to the formula $N_2H_4 . H_2O$ is 50. The numbers obtained from three such density determinations were 48·79, 51·67, and 49·48, showing that the simple formula $N_2H_4 . H_2O$ expresses in all probability the molecular condition. When vapour density determinations at ordinary pressure by Victor Meyer's method are carried out, some rather surprising results are obtained. At the temperature of boiling aniline (183°) the numbers 28·25 and 23·52 were obtained, about half those at 100° *in vacuo*; it appears probable, therefore, that at this temperature and under ordinary pressure complete dissociation into N_2H_4 and H_2O occurs, the molecules of the hydrazine existing side by side with those of water vapour without combination. At higher temperatures the molecular weight appears to increase again, a phenomenon which requires further investigation.

The molecular weight of the hydrate dissolved in water has also been determined by Raoult's method, by noticing the lowering of the freezing-point of water brought about by dissolving a small quantity of the hydrate in it. The numbers obtained from three experiments were 69·73, 70·71, and 71·25, corresponding almost exactly to a hydrate of the formula $N_2H_4 . 2H_2O$.

It appears, therefore, from the above experimental results, that the vaporized hydrate posesses the composition $N_2H_4 . H_2O$. At 183° these molecules appear to dissociate into free hydrazine and water vapour. When the hydrate dissolves in water, it takes up another molecule of water, becoming $N_2H_4 . 2H_2O$.

Properties of Hydrazine Hydrate.

Hydrazine hydrate is a colourless, highly-refractive, but not very mobile, liquid, which fumes when brought into the air. In closed vessels it may be preserved unaltered for any length of time. Its odour is quite different from that of ammonia, and is indeed comparatively weak compared with that of free gaseous hydrazine. It tastes like alkalies, and leaves on the tongue a burning sensation. It possesses strongly corrosive properties, at once destroying cork or caoutchouc. The boiling liquid rapidly attacks glass. It is hygroscopic, and also extracts carbon dioxide from the atmosphere. It mixes with water and alcohol in all proportions, but not with ether, chloroform, or benzene. It solidifies when surrounded by a mixture of solid carbon dioxide and ether to a mass of leaf-like crystals, which again liquefy from some reason or other below −40°. Although hygroscopic, when a drop of the liquid is allowed to fall into a cylinder of water, it remains for a long time at the bottom without mixing.

The specific gravity of the pure liquid of boiling-point 118°·5 (corr.) is 1·0305 at 21°.

A peculiar phenomenon is noticed during the distillation of hydrazine hydrate mixed with water. When a certain stage of concentration is reached, the drops falling from the end of the condenser into the glass receiver before dissolving take the form of extended filaments, often fine threads, which do not adhere to the walls of the receiver. The liquid hydrate reacts very strongly alkaline to vegetable colouring matters. Its reducing properties, as mentioned in the earlier notes in NATURE before referred to, are extraordinarily marked, most of the ordinary salt solutions of silver, gold, platinum, ferric iron, and cupric copper being reduced, and in the case of silver beautiful metallic mirrors deposited. Mercuric oxide reacts with it so energetically as to bring about an immediate explosion.

Free Gaseous Hydrazine.

Owing to the extreme affinity of hydrazine for water to form the hydrate, the free base is only liberated from the latter compound with the greatest difficulty. When the liquid hydrate is dropped upon barium oxide in a fractionating flask, the mass becomes very hot; but on distilling, almost the total quantity of hydrazine hydrate distils over unchanged. By repeated distillation over recently ignited baryta, a portion of the water in the hydrate is certainly retained, however, and the distillate fumes more strongly in the air. When the mixture of hydrate and baryta is heated in a sealed tube to 100° C. the reaction goes much further; and when the temperature is increased to 170°, at which temperature under ordinary pressure hydrazine hydrate is dissociated into N_2H_4 and H_2O, the decomposition is complete, the tube containing barium hydrate and gaseous hydrazine at high pressure. When the end of the tube is softened at the blowpipe, a tremendous rush of the free gas occurs, forming as it escapes a long rod-like white cloud with the moisture of the air, and rendering the atmosphere almost unbearable by its fearfully penetrating odour, compared with which that of the liquid hydrate is extremely weak. Owing to the difficulty of collecting and preserving such an active gas, which reminds one irresistibly of fluorine, Prof. Curtius has not been able to experiment further with it.

Action of Halogens and Halogen Acids upon Hydrazine Hydrate.

The halogen salts of hydrazine have been examined in great detail, and present some most interesting phenomena, throwing considerable light upon the constitution of the base.

Two kinds of salts are found capable of existing—mono-salts of the type $N_2H_4 \cdot HR$, where R represents a halogen element, and di-salts of the type $N_2H_4 \cdot 2HR$.

When the hydrate diluted with water is mixed with hydrofluoric acid, the dihydrofluoride, $N_2H_4 \cdot 2HF$, is obtained in crystals belonging to the regular system melting at 105°. This difluoride appears to sublime unchanged. It may also be obtained by adding hydrofluoric acid to the alcoholic solution of the hydrate, and then precipitating with ether.

The dihydrochloride, $N_2H_4 \cdot 2HCl$, is similarly obtained when hydrazine hydrate is evaporated with hydrochloric acid, or by action of hydrochloric acid upon the alcoholic solution of the hydrate. It is also formed when gaseous chlorine is led into a flask containing hydrazine hydrate; after a few minutes' passage of the gas, the separation of beautiful octahedrons begins, and bubbles of nitrogen escape. The reaction is as follows:—

$$3(N_2H_4 \cdot H_2O) + 2Cl_2 = 2(N_2H_4 \cdot 2HCl) + 3H_2O + N_2.$$

It is interesting to note that this same chloride was obtained by Curtius and Jay by boiling a compound which they prepared, termed benzalazine, $N_2(CH \cdot C_6H_5)_2$, with hydrochloric acid—

$$N_2(CH \cdot C_6H_5)_2 + 2H_2O + 2HCl = N_2H_4 \cdot 2HCl + 2C_6H_5CHO.$$

The monohydrochloride, $N_2H_4 \cdot HCl$, is obtained in long needles by heating the dihydrochloride to 140°. Prof. Curtius recommends heating the dihydrochloride in an apparatus heated by vapour of xylene, until no further loss of weight occurs.

The dihydrobromide, $N_2H_4 \cdot 2HBr$, may be prepared by direct evaporation of a mixture of hydrazine hydrate and hydrobromic acid; but if an alcoholic solution of the hydrate is employed, curiously enough the monohydrobromide separates on the addition of ether. If the pure hydrate is added to excess of liquid bromine, the substance decomposes completely with liberation of nitrogen and hydrobromic acid gases, nothing but the excess of bromine remaining. But if the bromine is allowed to act upon the hydrate suspended in chloroform, the monohydrobromide separates as a white mass of crystals, nitrogen being also evolved.

$$5(N_2H_4 \cdot H_2O) + 2Br_2 = 4(N_2H_4 \cdot HBr) + 5H_2O + N_2.$$

Recrystallized from alcohol, the mono-salt may be obtained in large prisms.

The dihydriodide, $N_2H_4 \cdot 2HI$, is only obtainable by one method, the decomposition of the benzalazine above mentioned with fuming hydriodic acid. It is singular that none of the methods applicable to the fluoride, chloride, and bromide yield it. When tincture of iodine is added to a dilute alcoholic solution of hydrazine hydrate, the colour disappears completely, in accordance with the quantitative reaction symbolized by the following equation:—

$$5(N_2H_4 \cdot H_2O) + 2I_2 = 4(N_2H_4 \cdot HI) + 5H_2O + N_2.$$

Nitrogen is evolved, and if the addition of iodine is continued until a permanent coloration is afforded, colourless prismatic crystals of the monohydriodide are obtained on evaporation. These crystals melt at 127°, and in doing so detonate violently. The reaction of hydrazine with iodine may be used volumetrically to estimate the amount of the base in a solution.

In addition to the above iodides, a third, of the composition $N_6H_{12} \cdot 2HI$, or $3N_2H_4 \cdot 2HI$, is obtained in well-defined crystals when an insufficient amount of iodine to form the monohydriodide is used.

Finally, a series of molecular weight determinations of these halogen salts by Raoult's method, using water as the solvent, have been made, with most interesting results. The mono-salts all give numbers agreeing with the molecular weight $\frac{N_2H_4 \cdot HR}{2}$, showing that in solution they are dissociated into hydrazine N_2H_4, and the acid HR. The tri-iodide yields numbers corresponding to $\frac{3N_2H_4 \cdot 2HI}{5}$, a fact which appears to indicate a dissociation into three molecules of N_2H_4, and two molecules of HI. Similarly the sulphate gives values corresponding to $\frac{N_2H_4 \cdot H_2SO_4}{2}$, pointing to its dissociation into separate molecules of N_2H_4 and H_2SO_4. The di-salts afford results indicating a molecular weight $\frac{N_2H_4 \cdot 2HR}{4}$, as if there were a dissociation of the kind $\frac{NH_3 \mid I}{NH_3 \mid I}$. The fluoride appears to behave abnormally, yielding numbers corresponding to $\frac{N_2H_4 \cdot 2HF}{2}$, as if the hydrofluoric acid liberated in the dissociation possessed the constitution H_2F_2.

A. E. TUTTON.

THE ALPINE FLORA: WITH A SUGGESTION AS TO THE ORIGIN OF BLUE IN FLOWERS.

WHEN in Colorado, I made notes on the plants of the Rocky Mountains, and collected some memoranda from various sources bearing upon alpine floras. Recently, when going over my records to ascertain as far as possible the definite results of environment on high alpine vegetation, I arrived at some conclusions which seemed to throw light on what had puzzled me before. These are at present more or less hypothetical, but I hope to work out the subject in more detail later on, so as to ascertain more clearly whether they will satisfy all the necessities of the case.

High alpine plants differ from those of the lowlands in one or more ways. They may be dwarfed, the leaves more divided or more thickly clothed with hairs, and the flowers are more often blue or pink. For the present, the dwarfing and the colour of the flowers need only be considered. Dwarfing may be—doubtless often is—the direct result of environment, as lack of nourishment. The dwarfed trees grown by the Japanese artificially in small pots are well known.

If this were the only cause of dwarfing, the alpine flora would present clear evidence for the transmission of acquired characters, as the character has undoubtedly become a *specific* one in several mountain plants. Thus, oaks are normally trees: the Rocky Mountain oak, *Quercus undulata*, is usually a shrub. The genus *Phlox* contains fine herbaceous plants, but *P. cæspitosa*, which I have gathered at about 12,000 feet on the Sangre de Cristo Range in Colorado, is so dwarfed as to be called (with other dwarfs growing beside it) "flowering moss." It appears, however, that natural selection may come into play, the low stature of these plants benefiting them in three ways:—

(1) They may escape the violence of the high winds which prevail at those altitudes; taller plants being broken off before the seed matures.

(2) They may obtain some additional warmth from their close proximity to the ground and partial shelter.

(3) The short summer of the mountain-tops necessitates very rapid development, and requires every energy to be thrown into the essential function of producing flowers and seed, leaving nothing to spare for the production of branched stems and diffuse foliage. In short, the plants are obliged to develop as *katabolically* as possible, and those which fail in the race are ruthlessly cut off by the autumn storms.

The evidence at hand tends to show the reality of this rapid development and the necessity for it. Every alpine traveller knows how rapid is the bursting into bloom of the plants but lately covered by snow and ice. Their impatience, if one may so call it, is astonishing, some plants will begin to develop under the snow. Then the flowers that are produced are very brilliant, lasting but for a short season (like some alpine Lepidoptera), and especially is it to be noted that they are nearly always *large in proportion to the size of the plant*. That is, the plants have dwarfed, but the flowers *have not become dwarfed at the same time*. This were strange, if bad nourishment were the only factor: in the light of the facts considered above it is exactly what one might expect.

The intense blue of some high alpine flowers is most noticeable, and the large number of blue flowers is equally remarkable. I never saw anything equalling in *blueness* the *Omphalodes nana* var. *aretioides* of the Colorado mountains above timber-line. The brilliancy of these flowers is almost dazzling. The large and beautiful blue flowers of species of *Gentiana*, *Polemonium*, &c., are conspicuous on high mountains. Next to the blues come the pinks. I have shown in the *Bulletin of the Torrey Botanical Club* how species of *Castilleia*, which are scarlet or yellow at lower altitudes, become crimson-bracted as they ascend. Many other examples could be given.

I have found no one to dispute the reality of this change of colour, at least in many groups of plants. The cause of it seemed much more obscure. It is generally agreed that, developmentally, reds come after yellows, crimson follows red, and blue crimson. Further, it is supposed that this series of colours is the result of different degrees of pigmental complexity, perhaps of the nature of, or similar to, various degrees of oxidation. Katabolism, strong metabolism, produces the higher colours—the crimsons, the blues. Moisture, slow development, great growth, with an expansion of the parts—these are favourable to the yellows, and, in a less degree, to the reds; of course the green, the primitive chlorophyll, especially.

May not all this, therefore, be correlated with the dwarfing? Strong metabolism is necessary; every energy must be thrown into the inflorescence; and, as a side-result, we get the crimsons and blues of mountain flowers.

It may be, even, that all, or nearly all, blue flowers were produced in this way. Very few of those groups of plants which are never alpine have blue flowers. There will be some exceptions, of course—for instance, I do not know how to account for the blue water-lily—and there are some groups in which a blue flower, however alpine the species, seems to be unproduceable. But even some of the very refractory genera, as *Erysimum* and *Troximon*, do go so far as to produce an occasional purple flower at high altitudes.

If I am right in my suggestions, the need for the selection by bees, made so much of by various authors, no longer exists. My own observations make me very doubtful whether bees do really prefer blue flowers at all, as a general thing—and I can hardly bring myself to believe, though the present hypothesis fall to the ground, that blue in flowers is a result of insect-selection.

T. D. A. COCKERELL.

NOTES.

AN International Scientific Congress will be held in Moscow in 1892. It will be divided into two sections, one of which will deal with zoology, the other with anthropology and ethnography. It is expected that the meetings will be largely attended. According to the *Revue Scientifique*, there is some talk of excursions to the Caucasus and to Turkestan.

WE learn from *Science* that the United States Coast and Geodetic Survey Office lately received from its sub-office in San Francisco a telegram announcing that an agent of the Alaska Commercial Company had arrived from St. Paul, Alaska, bringing letters from the Coast and Geodetic Survey parties who have been engaged in making explorations and surveys on and near the 141st meridian of longitude (the boundary between Alaska and the British possessions). These two parties were commanded by Messrs. J. E. McGrath and J. H. Turner, assistants of the Coast and Geodetic Survey. The party under Mr. McGrath ascended the Yukon River to the boundary-line, and there made its head-quarters, while that headed by Mr. Turner went up the Porcupine River to the Rampart House (the Hudson Bay Company's trading-post in the vicinity of the boundary), and there camped for the further prosecution of their work. Both parties were at their posts early in the autumn of 1889. The records made by Mr. McGrath's party comprise a set of magnetic and meteorological observations for a year; a set of specimens of sediments obtained from filtering certain measured quantities of the water of the Yukon River, made at regular intervals; certain botanical specimens; and a series of photographs. Mr. McGrath also gathered considerable information from some of the most intelligent of the Indians whom he encountered at Forty-Mile Trading-Post Owing to the stormy weather, Mr. McGrath was unable to obtain a sufficient number of astronomical observations to justify him in returning last autumn; and his party will therefore remain until next spring, and then descend the river, doing what work they can in the cause of science on their way down. Mr. Turner's party were much more favoured by the weather than the other party. They completed the necessary astronomical observations for the determination of the geographical position of their station on the Porcupine River at the boundary-line, also a set of magnetic and meteorological observations, and made a topographic map (on a scale of 1 : 5000) of the river in the vicinity of their camp, and a survey (on a scale of 1 : 200,000) from the boundary to Fort Yukon, a distance of about 100 miles. A small scheme of triangulation was undertaken to "locate" three monuments placed to mark the boundary-line. An exploring expedition was sent during the months of March and April to explore the line northward to the Arctic Ocean. The party visited Herschel Island. During May another trip was made about forty miles to the southward, as far as Salmon Trout River. Mr. Turner reached St. Michael's on August 30, 1890, with his party, too late to catch the steamer going south. The party will winter there, and in the spring carry the triangulation toward the mouth of the Yukon River, until relieved by orders from the Coast and Geodetic Survey Office.

THE next Deutsche Geographentag will be held in Vienna on April 1, 2, and 3.

THE other day, on the occasion of the sixtieth anniversary of the first doctorate of M. de Quatrefages, the anthropologist and zoologist, a small group of his most intimate friends and pupils met at his house and presented him with a very fine copy of an etching of himself, prepared without his having heard anything about the matter. M. de Quatrefages is over eighty years of age; he is in excellent health, and works hard. A few weeks ago he was elected President of the French Geo graphical Society.

IN St. Petersburg, an Institute for Experimental Medicine has been founded by the liberality of Prince Alexander Petrowitch of Oldenburg. It is intended chiefly for the study of the causes of infectious disease, and methods of prevention and cure. The Prince proposes to invite eminent men of science to make experiments in bacteriology, physiology, chemistry, biology, and the veterinary art. The building, which is being elaborately fitted up, stands in the Lapukins Kaja Street, on the banks of the Neva. Like the Pasteur Institute in Paris, it will have a department for clinical observation.

AT the meeting of the Photographic Society of Great Britain, on December 9, 1890, Mr. W. S. Bird spoke of the work done by the Committee which had been deputed to consider various questions relating to the proposed Photographic Institute. The labours of the Committee had resulted in the drawing up of a large scheme and of a smaller scheme. The larger scheme was a project of the future, but the smaller one was of a more practicable nature, and the Committee had had the advantage of conferring with the Lord Mayor on the subject, who thought that it might be possible to collect a sum which had for its limit £10,000, provided that the project was shown to be of public utility. If this was so, he thought that the photographic community generally should take some steps towards defining practical methods of carrying the scheme forward, and then they could approach the Lord Mayor with this project, and with some promises of financial support. In foreign countries the Government provided the means by which such institutions were founded. In England this was not the case, and he (Mr. Bird) felt that if this movement was to be a success, it must begin at home, and the Photographic Society ought to use what influence it possesses to assist in the work. After some discussion the following resolution was adopted :—"That the project be submitted, in the form of a circular letter, to the different provincial and metropolitan Societies, to elicit the opinion of the photographic public generally upon the scheme now brought forward."

AT the Annual Conference of Principals of University Colleges, held at Bangor, on December 23, the following subjects were discussed :—(1) The University of London: (*a*) the proposed scheme for reconstituting the University; (*b*) local centres at provincial Colleges for the honours examinations of the University. (2) Day Training Colleges: (*a*) relations of the University Colleges in this connection to the Education Department and the Science and Art Department; (*b*) organization necessary to make these relations operative. (3) County Councils: (*a*) relation of Coun ty Councils to educational institutions outside the strict county area; (*b*) devotion of local taxation money to purposes of technical instruction. An invitation from University College, London, to meet in London in 1891 was accepted.

AT the meeting of the French Academy on December 8, M. Mascart presented a work by General A. de Tillo on the distribution of atmospheric pressure in the Russian Empire and Asia, from 1836 to 1885. The work consists of an atlas of 69 charts, and a discussion of the monthly and annual values, as well as of the variability of pressure, and the relations existing between the variations of pressure and those of temperature at 136 stations. The highest pressure quoted is 31·63 inches (reduced to sea-level) in December 1877 at Barnaoul, and this is stated to be the highest reading on record. But in the Quarterly Journal of the Royal Meteorological Society for July 1887, Mr. C. Harding quoted, on the authority of Prof. Loomis, a reading of 31·72 inches on December 16, 1877, at Semipatalinsk. In NATURE, vol. xxxv. p. 344, Mr. Blanford quoted the lowest reading on record at any land station, viz. 27·12 (reduced to English standards), which occurred on September 22, 1885, on the coast of Orissa. These readings give a difference of 4·6 inches, probably the maximum range of the barometer ever observed at the earth's surface.

THE weather review accompanying the Pilot Chart issued by the U.S. Hydrographic Office states that very heavy weather prevailed in the North Atlantic during the month of November; westerly gales acco mpanied by heavy rain, hail, or snow, with tremendous seas, having raged over almost the entire region between Newfoundland and the British Isles, with the exception of a few days. Eight storms reached the ocean from the continent of North America, two of which crossed the British Isles. Six severe storms also originated in mid-ocean, most of which more or less affected the weather in these islands. Very little fog was reported during the month, and only one iceberg. It is proposed to publish with the next Pilot Chart a supplement devoted to the subject of ice in the North Atlantic during the season of 1889–90, which is perhaps the most notable ice season on record.

AT the recent Congress of Americanists at Paris, Dr. Seler showed that the name Anahuac had been applied by mistake to the plateau of Mexico. "Anauac" means "on or near water," and by all ancient writers was used in the sense of coast-land. Anauac Ayotlan was the seaboard of the Pacific; Anauac Xicalanco that of the Atlantic. Motolinia alone used the word differently. He did not, however, apply it to the plateau, but to the whole of New Spain. According to Dr. Seler, this also was a blunder, and was due to the phrase "cem anahuãc," which is used for "the whole world." The original meaning was "the entire land down to the sea-shore."

A CIRCULAR has been issued by the Department of Science and Art, calling the attention of secretaries of science classes and schools, to alterations to be adopted in the "staging" of science drawings, which are to be sent up to South Kensington for examination in April next. This "staging" has a distinct meaning different from that of "elementary," "advanced," or "honours" stages as used in respect of the subjects of science examinations held in May. The secretaries are therefore requested to avoid using the terms "elementary," "advanced," and "honours," in connection with the labelling of the drawings from copies, from measurement, and original designs which may be submitted next April. Such drawings are to be labelled according to stages about which precise instructions are given.

THE utility of the microphone for observation of earth-tremors and noises was soon recognized, and in Italy especially attention has been given to adapting the instrument for this use. We learn from the *Rivista Scientifico-Industriale*, that Signor Baratta, finding some defects in a method of mechanical registration of the motions of a seismo-microphone, which he had devised, has substituted a photographic system with advantage. The device is briefly this:—The telephone wire is connected with a subterranean microphone. Before the telephone-diaphragm (vertical), and connected with its centre by a fine aluminium wire, is a short slip of the same metal, fixed below, and having a curved piece at the top, which rests against a small mirror, movable about a horizontal axis. This mirror reflects the light from a lamp and lens to photographic paper on a rotated drum. The light is momentarily shut off every quarter of an hour, by a shutter arrangement, worked electro-magnetically by the clock-work which moves the drum.

THE French Department of Trade and Industry is issuing, in two large octavo volumes, the popular lectures given in the Trocadéro Palace during the Exhibition, for the benefit of visitors. The first volume has just appeared. The subjects are most varied, and many of the lectures are valuable.

A RECENT microscopical study, by Herr Schultz, of the skin of toads and salamanders, has yielded some interesting results (*Archiv für mikroscop. Anat.*). There are two kinds of glands, mucus and poison glands. The former are numerous over the whole body; while the latter are on the back of body and limbs, and there are groups in the ear-region behind the eye; and, in the salamander, at the angle of the jaw, The mucus glands are spherical, have a clear glassy appearance, and contain mucus cells and mucus; the poison glands, which are in regular strips on the salamander, are oval, much larger, and have a dark granular look, from strongly refractive drops of poison, a good reagent for which is copper-hæmatoxylin. The poisonous elements are from epithelial cells lining the glands. The mucus glands are for moistening the skin, and the liquid has no special smell, nor a bitter or acid taste. The poison glands are of course protective, and the corrosive juice is discharged differently in toads and salamanders, on stimulating electrically; in the latter it is spirted out in a fine jet, sometimes more than a foot in length, whereas in the toad, after longer action of the current, it exudes sparingly in drops. The physiological action of the poison has lately been studied by some Frenchmen. There is no reason, according to Herr Schultz, for supposing that the mucus glands sometimes become poisonous.

THE preliminary copy of the new map of the Chin-Lushai country, prepared for the surveys made during the recent military expeditions, shows, according to an Indian paper, what remarkable progress has been made in filling up the blank spaces which disfigured the old survey sheets. In the expedition of 1871–72 the country for some considerable distance to the south of Manipur was surveyed, and now the operations from the Chittagong side and from Upper Burmah have enabled the British officers to map in the hill tracts between Demagiri and Kan. Fort White, to the north-east, has also served as a base for valuable observations, while on the south-east the Chinbok country has also been explored. During the present winter several survey parties will be sent out, and a year hence the whole mountainous region between Manipur and Arakan will probably have been mapped.

THE wren is generally supposed to be a gentle little bird; yet on occasion it seems capable of displaying anything but an amiable temper. In the current number of the Selborne Society's magazine, Mr. Aubrey Edwards gives from his note-book the following account of what he calls "a disgraceful scene" between two male wrens:—"April 15, 1889.—I have just been watching two golden-crested wrens fighting. They first attracted my attention by getting up from the ground almost under my feet, and engaging again and falling to the ground. Then rising again one chased the other into a yew-tree near, where I had a good close view of them as they challenged each other, ruffling their feathers, shaking their bodies, singing and dancing about with crests erected, the sun shining on the orange-coloured crests—such a pretty sight. After they had been talking big at each other for some minutes, the hen arrived on the scene, and a desperate fight ensued, the two cocks falling to the ground in fierce embrace, rolling over each other occasionally, but for the most part lying still on the ground with their claws buried in each other's feathers for about a minute. The hen was close by them on the ground, moving about and looking very much concerned at the affray. Her pale yellow crest contrasted notably with the rich orange of the males. After getting up, renewing the combat in a currant bush, falling again and struggling on the ground, they rose and had a chase round the yew-trees, the hen following to see the fun, and presently went off and were lost to view."

DURING the past year the question of technical education in Burmah was taken up under the orders of the Government of India. A new set of examination standards was considered; and at the same time a survey was made of the industries practised in Lower Burmah. A provincial Board is at present being constituted to consider the results of the survey, and to advise the Government generally in all matters connected with technical and industrial instruction. A sum of Rs. 10,000 has been allotted in the current year's budget, and it is hoped that the cause of technical education has secured a real and substantial foundation for future progress. Four engineering pupils at the Sibpur College were some time ago in receipt of Burmah scholarships, but the grant of these scholarships has now ceased, and no more engineering students will be sent to Calcutta.

THE Government of Bombay, in concluding a recent resolution on education in the Presidency, remarks that the degree to which private enterprise has expanded under the revised Grant-in-aid Code has exceeded all anticipations. In 1884–85 the expenditure on institutions maintained by the Department was Rs. 8,47,260 out of a total expenditure of Rs. 41,42,734. It is now little more than 8 lakhs out of 56½ lakhs of expenditure. The whole growth of ways and means has thus been left to other agencies than Government. In the same year aided institutions under private management received from provincial revenues under 2 lakhs, and they now receive from that source alone nearly 4½ lakhs. Private enterprise is becoming the chief power in educating the people. Another marked change is the growth of technical education and the introduction of various types of instruction. In 1884–85 the greater part of the expenditure by Government on its own institutions, which aggregated, Rs. 8,47,260, was assigned as follows: nearly 6 lakhs to colegiate and secondary education, and $1\frac{3}{4}$ lakhs to special education. In the current year Rs. 2,39,479 out of the 8 lakhs spent on Government institutions were devoted to special education. Private enterprise manages special institutions at a cost of Rs. 1,46,261, whereas the expenditure on this head by aided schools in 1884–85 was only Rs. 39,319. There is thus a greater diversity intro duced into educational activity as well as into educational agency. Collegiate and secondary education of the ordinary type receive more expenditure than they did, but there is still greater expansion in other directions. The vastly increased employment of Municipal and Local Boards in the management of primary education is another marked feature of recent years. The tendency of education to range itself under diverse forms of instruction, and under a great multiplicity of controlling agencies, renders the task of administration and of apportionment of public funds as grants-in-aid more difficult. It is remarked with satisfaction that complaints against the Grant-in-aid Code are never heard, and that the record of each succeeding year shows increasing attendance at schools and the enlistment of larger private resources in the work of education. "The Governor in Council is satisfied that the Report of public instruction for the year ending March 31, 1890, will be generally regarded as affording proof of the sagacity with which the Department was administered, and new impulses given to it during the administration of His Excellency the late Governor" (Lord Reay).

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♀) from India, presented by Mr. P. Boulton; a Tuatera Lizard (*Sphenodon punctatus*) from New Zealand, presented by Captain Worster; two Common Marmosets (*Hapale jacchus*) from South-east Brazil, deposited.

OUR ASTRONOMICAL COLUMN.

GREENWICH SPECTROSCOPIC OBSERVATIONS.—The results of the spectroscopic and photographic observations made at the Royal Observatory, Greenwich, in the year 1889 have recently been issued. Mr. Maunder examined the spectrum of χ Cygni on June 3, and compared it with the spectrum of hydrogen as given by a vacuum tube, and that of carbon as given by a Bunsen flame. C and F were not present as bright lines. There was some uncertainty about the G hydrogen line, a bright spot being suspected at or near its position in the violet region. D_3 was not seen as a bright line, but there was a brighter part about λ 5823, which may have been "a mere effect of contrast, or a local brightening of the continuous spectrum." It is also recorded that "the close correspondence as to position of the green and blue bands of the hydrocarbon spectrum with two of the bright zones or interspaces of the stellar spectrum was very apparent. No such correspondence was evident in the case of the yellow band." The mean wave-lengths obtained from two sets of measures of the more refrangible edges of Dunér's bands VII. and IX. are 5170 and 4766. On June 17 an observation was made of the spectrum of Uranus. The spectrum was traced from about λ 6200 to about λ 4600. Measures of the positions of two "dark bands," and an "ill-defined bright region which seemed to consist of two diffused bright bands," gave the following mean wave-lengths:—

Object measured.	Mean wave-length.
Darkest line	5419
Bright band	5336
Bright band	5276
Dark line	4866

"Other irregularities in the brightness of the continuous spectrum were suspected on either side of the principal dark line."

Measures of two of the bright lines in the spectrum of R Andromedæ were obtained on October 19. The brightest line was found to have a wave-length 4872, and was probably F, whilst the position of a feeble bright line was determined as λ 5136.

The spectrum of Davidson's comet (*e* 1889) was observed on August 29. It is recorded that "one bright band in the green could be distinctly made out when the slit was opened very widely. It coincided nearly, if not exactly, with the green band of the hydrocarbon spectrum." No measures were made of the wave-length of the bright band.

The other observations contained in this publication refer to the motion of stars, the sun, moon, and planets in the line of sight, and measures of photographs of the sun.

The volume of "Greenwich Observations" for 1888 has also been issued, and is of the usual character.

COMETS ZONA AND SPITALER.—The following elements have been computed for Spitaler's comet:—

T = 1890 October 26·50833 Berlin mean time.

$$\left.\begin{array}{l} \pi = 58^\circ\ 24'\ 28''\!\cdot\!2 \\ \Omega = 45\ \ 7\ 51\cdot2 \\ \iota = 12\ 51\ 49\cdot0 \end{array}\right\} \text{Mean Eq. 1890·0.}$$

$$\phi = 28\ 11\ 26\cdot6$$
$$\log a = 0\cdot537532$$
$$\mu = 554''\!\cdot\!2$$
Period = 6·4 years.

Ephemerides for Berlin Midnight.

ZONA'S COMET.

1891.	R A. h. m. s.	Decl. ° ′	Brightness.
Jan. 2	2 1 42	+ 29 14·5	0·37
,, 4	1 57 59	28 53·1	
,, 6	1 54 36	28 32·8	0·33
,, 8	1 51 31	28 13·6	
,, 10	1 48 42	27 55·6	0·29

SPITALER'S COMET.

1891.	R.A. h. m. s.	Decl. ° ′	Brightness.
Jan. 5	4 52 24	+ 40 22·5	0·71
,, 9	4 51 52	40 25·5	
,, 13	4 51 58	40 25·5	
,, 17	4 52 49	40 23·5	
,, 21	4 54 24	40 19·5	

Brightness at discovery = 1.

Zona's comet is therefore still near Triangulum. Spitaler's comet is in Auriga, and not far from Capella.

The sidereal time at 10 p.m. on January 1 is 4h. 45m. 2·5s., and on January 10 is 5h. 20m. 30·1s.

THE LEONID METEORS.—The current number of *Comptes rendus* (December 22) contains a note from Prof. Denza, the Director of the Vatican Observatory, on the observations made in November 1890 under the direction of the Italian Association for the Observation of Luminous Meteors. The results of the observations made at seven stations on November 13–14, 14–15, and 15–16 are tabulated. They show that the number of meteors seen was greater on November 15–16 than on November 14–15, and much greater on these evenings than on November 13–14. In previous years the maximum occurred on November 13 or 14.

The whole of the observations of this year indicate that the Leonids were more frequent than in previous years, hence it is probable that this augmentation will go on until the next shower in 1899.

SPECTROSCOPIC NOTE (D_3).—The Rev. A. L. Cortie, S. J., has brought forward the question, "Has the line D_3 a coincident dark line in the solar spectrum?" From a discussion of available observations, and an examination of some photographs taken by Mr. Higgs with a Rowland grating, it is concluded—

(1) That D_3 has been seen dark in a sun-spot.

(2) That there is a faint dark line in the solar spectrum, at least within two-tenths of a tenth metre from the bright line.

(3) That this line is due to our atmosphere.

(4) That observers are disagreed as to whether the dark and bright lines coincide in position. (*Monthly Notices*, November 1890.)

AMERICAN BLAST-FURNACE WORK.

THE history of the gradual development of blast-furnace working in America, culminating in the production of 2500 tons of pig-iron in the short space of one week, with an expenditure of only 16·8 cwt. of coke per ton of iron, is interesting, especially if compared with ours, where the largest make has not exceeded 1000 tons, with an expenditure of about 1 ton of coke. It is difficult to find satisfactory reasons for this great difference, and the members of the Iron and Steel Institute may well have been astonished at the results laid before them.

It is stated that the first good results were obtained by breaking away from the traditional practice of regulating the quantity of blast or air by the pressure-gauge, and noting instead the number of revolutions made by the blowing engine; probably it would have been better still if some means had been devised of registering the flow of waste gas issuing from the furnace-mouth.

The new era in the manufacture began in 1880, and is divided into three periods:—(1) Old practice—air supply limited, slow driving. (2) Air in excess, excessive driving, attended with a greater output of iron, but increased consumption of coke. (3) Moderate supply of air, rapid driving, great output of iron with decreased consumption of coke. The best results were obtained with air at a pressure of 9 pounds heated to 1100°, blown in at the rate of 25,000 cubic feet per minute, furnace 18,200 cubic feet capacity; a greater quantity, 30,000 cubic feet, proving too much, whilst with less the make of iron diminished. This furnace produced 10,035 tons of iron in one month, with an expenditure of 1834 pounds of coke; the average ratio of CO to CO_2 being 40 per cent. as against about 48 to 50 in this country. Altogether, the volume of air was increased from 16,000 cubic feet "old practice," to 25,000 per minute with improved results. Ultimately it is probable that 300,000 tons of iron may be produced in three years without repairs.

Some of our most prominent metallurgists took part in the discussion on Mr. Gayley's report, and it soon became evident that some long-cherished ideas and formulæ were open to question. One eminent authority was of opinion, however, that on the whole the utilization of fuel, as shown by analysis, was in favour of English practice; although the actual quantity of fuel burnt to CO_2 was 5·97 as against 4·99 cwt. in Pittsburg, accounting for the difference by pointing out the variations in the appropriation of the heat-units or calories under the various items. It was also remarked that the Pittsburg ore was much richer in iron than the English, the latter fusing 28 cwt. of slag as against 10·71 in the former. The loss of heat due to radiation, &c., and in the waste gases, was also less, and these combined causes might be said to account for the remarkable saving.

English metallurgists and engineers were not unanimous in their approval of the new system; such methods might not after all be better than the older and slower processes practised in this country; an expensive array of air-heating stoves, blast engines, boilers, &c., were required, entailing considerable outlay, possibly exceeding the margin of profit. On the other hand, it was contended that the greater output of iron and the saving in fuel, amounting to £24,000 per annum, would more than cover additional expense, and would be sufficient for relining and repairs of the furnace and accessories three or four times over. Fewer furnaces would suffice, and it would be easier and less expensive to manage, say, two furnaces, than four or more. A well-known authority declared in favour of large makes, coupled with rapid driving, but confessed that after all it was not clear where the advantages lay as regards the plant, working, and final results.

The most suitable form of furnace was discussed, but nothing very definite seems to have been said, beyond the general admission that the form facilitating the unchecked free descent of the ores, &c., was most suitable; a large hearth or crucible was also desirable. The equal distribution of the ascending reducing hot gases was not thoroughly discussed, although it is obvious that unequal distribution of the hot gases must correspond to a like varying reduction and heating of the ores, &c., charged. According to Dr. Percy, the equal distribution of the gases, &c., is of the utmost importance, and in his work a furnace is figured, giving the form of furnace most suitable for general uses; he states that, unless the interior parts, from the mouth downwards, are not properly proportioned, the regular production of iron of uniform quality cannot be insured.

It is submitted that the able discussion which has just been summarized, although accounting for a good deal, is still not so satisfactory as might be. The scientific researches on the *rationale* of the chemical reactions occurring in the blast-furnace, may now be termed complete. We can now determine exactly the chemical reactions of the gases passing through, as also the relative distribution of the calories which a given weight of carbon will produce when burnt to carbonic oxide in the furnace. So far, this seems satisfactory; but one pound of carbon, when burnt in the calorimeter, always gives the same number of calories, *i.e.* number of pounds of water heated 1°, "this is all." It is, however, well known that its combustion may be so regulated as to barely suffice for the fusion of a metal like lead; and with the same fuel a temperature may be attained equalling the fusion-point of the softest steel.

Temperature or heat-intensity may play an important part in blast-furnace work. Iron cannot be properly melted below a temperature well above its fusion point, and unless the fuel is burnt under well-understood conditions, *i.e.* very rapidly, it often occurs in practice that fuel is so slowly burnt as to barely suffice for fusion. Fusion is unduly prolonged, and obviously fuel is wasted, for the simple reason that heat-intensity is proportionate to the rate at which it is consumed.

Economical work depends entirely on the difference in temperature betwixt the heating medium and the heated body; and it follows, as the temperature of the furnace flame and gases approximates to that of the metal heated, considerably less heat is absorbed; finally, when the temperature of the metal approximates closely to that of the flame, the heat absorption becomes nearly inappreciable; thus fusion may be unduly prolonged. This final stage may be termed the critical point, sharply dividing the economical use of fuel from the reverse.[1]

The importance of maintaining the temperature at the end of the process of fusion cannot well be over-estimated: a few hundred degrees more or less makes all the difference.[2] Apparently sufficient attention has not been paid to this in blast-furnace work, although it is plain that as regards fusion of the iron and slag, the same law is applicable as when simply fusing iron in ordinary melting-furnaces.

The delivery of large quantities of air at a high pressure through many large openings (technically the *tuyères*), as practised in America, and the consequent rapid consumption of coke, must necessarily raise the heat-intensity (temperature) much above that of furnaces supplied with only a limited quantity of air, and it follows that, in the latter, iron, &c., is fused slowly, thus entailing a greater consumption of coke.

Concurrently with rapid melting, it is self-evident that the furnace drives faster, for it is obvious that the rate of descent of the materials charged is governed by the more or less rapid fusion of iron and the accompanying consumption of fuel. It is pretty generally admitted that rapid driving should be avoided, on the reasonable assumption that time is required for the complete reduction of the ore by the ascending gases. Yet in America only 16·8 cwt. of coke per ton of iron has been consumed in conjunction with rapid driving; but possibly temperature plays an important part throughout the whole smelting process. The law of heat exchanges previously discussed cannot be evaded. We will assume that the temperature at the *tuyères* in one furnace is 500° higher than in another; it follows that the comparatively cool descending materials absorb in a similar ratio, or more plainly a greater percentage of the total heat is utilized in the former than in the latter instance. It is, therefore, not so surprising that the waste gases leave the high temperature furnace at the comparatively low heat of 340°.

It would appear that, on the whole, sufficient attention has not been directed here to the absolute weight or volume of air used per unit of time with its consequent effects, and it may be ques-

1 "When iron has attained a temperature of 2500°, it will, when exposed to a heat-intensity of 3200°, absorb less than $\frac{1}{11}$ of this, and when iron is receiving the last 100° requisite to bring it up to the welding-heat of 3200°, the temperature of the furnace being 3300°, only $\frac{1}{64}$ will enter the iron, the remaining $\frac{63}{64}$ being wasted. By raising the temperature to 3700°, $\frac{1}{16}$ instead of $\frac{1}{64}$ will be utilized, thus exhibiting the fact that an addition of temperature of only $\frac{1}{8}$ increases the efficiency of the furnace fourfold."—*Prideau.*

2 Cause of the remarkable saving of fuel effected by hot blast (Percy, "Metallurgy," p. 425). "It is therefore plain that the mere quantity of heat, *i.e.* the number of units of heat evolved can have little to do with the matter. This being admitted, the inevitable conclusion is that calorific intensity must be concerned, and that the temperature of what may be designated the most active part of the furnace must be higher in the case of hot blast than in the case of cold blast."—*Percy.*

tioned whether as a general rule in this country the area of the air or blast-delivery pipes has been correspondingly enlarged in due proportion to the increased air temperature, often over 1000°, now not uncommon since the introduction of the modern brick-heating stoves. Surely with air expanded to nearly four times its normal volume at 60°, the ordinary blast pipes should be correspondingly enlarged, coupled also with a higher blast pressure. Thus it may be, when using highly heated air, the absolute weight of air (oxygen) supplied may not be properly proportioned to the weight of coke charged, greater or less ; and if this be true, it is not surprising that many blast-furnace managers have condemned the use of superheated air ; it seems just possible that coke may be charged in excess of the air supply, or *vice versâ*. In America they appear, "by dint of sheer practice," to have somehow realized this ; they have even gone further, and ascertained that it is quite possible to deliver too much air. It seems, on the whole, that their superior practice may partly, at least, be explained on the probability that heat-intensity—in other words, temperature expressed in C. or F. degrees—plays its part ; and that calculations based on heat-units alone, undoubtedly useful and necessary as they are, have only a limited application.

THE ZOOLOGY AND BOTANY OF THE WEST INDIES.[1]

THIS Committee was appointed in 1887, and reappointed in 1888 and 1889.

During the past year chief attention has been directed to the exploration of the island of St. Vincent, and two collectors have been maintained in that island at the expense of Mr. F. Du Cane Godman, who has kindly assisted the Committee in this manner in order that the funds at its disposal may be chiefly applied to the remuneration of contributors, to whom would be referred the large collections in zoology, already amounting in Insecta alone to about 3000 species. The plants have been determined at the Herbarium of the Royal Gardens, Kew, and are nearly completed to date. A separate report on the collections in zoology and botany is given below.

It is proposed by the Committee to accept the services of Mr. R. V. Sherring, F.L.S., to make collections in botany in the island of Grenada during the coming winter. Mr. Sherring is well acquainted with the West Indies, and has already made collections there, and added several new species of ferns to the flora of Jamaica.

Zoology.

Since the last Report of the Committee three collections have been received from Mr. H. H. Smith, the collector sent by Mr. Godman to the island of St. Vincent. These collections include a complete set of the birds already known to inhabit the island, and a few additional species ; a small number of reptiles and crustaceans ; a large series of spiders ; and a great many Insecta ; these last amounting, it is thought, to about 3000 species.

In 1889, Colonel Feilden paid a visit to the island of Dominica for the purpose of ascertaining whether the Diablotin (*Œstrelata hæsitata*) has become extinct there, as has been reported by Ober. The account of his expedition that Colonel Feilden has published leaves little doubt that this is the case.

Although Mr. Smith has now been occupied about a year and a half in the exploration of the island of St. Vincent, Mr. Godman has decided, with the concurrence of the Committee, that he shall still continue there, as it is not yet clear that the more inaccessible portions of the island have been sufficiently examined.

Mr. Godman has agreed to give a first set of the zoological specimens obtained by his collector to the National Collection contained in the British Museum, and the Committee is at present endeavouring to find competent zoologists to work out the extensive series of insects and spiders that has been obtained.

Commander Markham, R.N., contributed some specimens in zoology collected by him in the Leeward and Windward Islands of the West Indies, and Captain Hellard, R.E., local secretary to the Committee at St. Lucia, has recently forwarded four boxes of Lepidoptera collected by him in that island.

Botany.

A small collection of plants, numbering 143 specimens, was received from Mr. J. J. Walsh, R.N. This collection included plants from Dominica, St. Martin's, St. Eustatius, St. Kitts, St. Lucia, and Grenada. Most of the plants consisted of common West Indian species, presumably such as would be met with in the more accessible spots in the various places visited.

The remainder of the plants collected by Mr. Ramage at St. Lucia have been determined. Of 84 species sent, 62 have been fully determined. The others include several that are apparently new. They are wholly woody or forest plants, and comprise *Sloanea* sp., *Picramnia* sp., *Xanthoxylum* sp., *Bursera* sp., *Miconia* sp., *Cybianthus* sp., *Lucuma* sp., *Siparuna* sp., *Helosis* sp., *Gymnanthes* sp., and *Cyclanthus* sp. In one or two cases the material is hardly sufficient for satisfactory determination. Two of the above undetermined species have also been collected in Dominica and one in Martinique by earlier collectors.

Three collections have been received from St. Vincent through Mr. Godman, viz. in September 1889, and March and August 1890. The first collection has been determined at Kew by Mr. Rolfe as far as the end of the *Polypetalæ*. Of the 252 numbers (to this point) 47 were duplicates ; thus 205 species were represented. All but about 9 of these were fully determined, the great bulk consisting of widely diffused West Indian plants ; 128, or more than half, appear to have been recorded from the island before.

The undetermined specimens are *Trattinickia* sp., *Stigmaphyllon* sp., *Trichilia* sp., *Meliosma* sp., *Lysiloma* sp., *Moquilea* sp., a species of *Eugenia* obtained by Hahn in Martinique, and two species, probably of *Pithecolobium*, of which the material was somewhat inadequate. Several of these appear to be new, the first-named being specially interesting, because the genus was hitherto only known from Guiana and Brazil. In addition to this may be mentioned that several species of somewhat restricted distribution in the West Indies, more especially from Martinique and St. Lucia, have also been found in St. Vincent.

The second collection from St. Vincent consisted for the most part of ferns. Mr. J. G. Baker has fully worked out these. They include 133 species and well-marked varieties, three of which are new. The specimens are in excellent state of preservation, and it is probable that we have amongst them nearly all the fern flora of the island, both of the mountains and the lowlands.

As our knowledge of the fern flora of St. Vincent may be now regarded as practically exhaustive, it seems probable that some species hitherto attributed to the island, on the authority of specimens collected by the Rev. Lansdowne Guilding, really belong to other islands. This error has arisen from want of precision in exactly localizing the specimens, a practice the importance of which was hardly recognized at the time they were collected.

The collections received in August last contain three additional species of ferns, making the total number collected by Messrs. Smith 136. The added species are *Dicksonia cicutaria*, Sw., *Davallia aculeata*, Sw., *Cheilanthes radiata*, R. Br. In addition there are 389 numbers of flowering plants, and 3 palms. These will be determined later.

The Committee would again draw particular attention to the botanical and zoological bibliography of the Lesser Antilles prepared under its direction, and published as an appendix to the Report for 1888. This bibliography has been widely distributed in the West Indies and in Europe, and has proved of considerable service in carrying out the objects for which the Committee was appointed.

The Committee recommend their reappointment, and that a grant of £100 be placed at their disposal.

[1] Third Report of the B.A. Committee, consisting of Prof. Flower (Chairman), Mr. D. Morris (Secretary), Mr. Carruthers, Dr. Sclater, Mr. Thiselton-Dyer, Dr. Sharp, Mr. F. Du Cane Godman, Prof. Newton, Dr Günther, and Colonel Feilden, appointed for the purpose of reporting on the present state of our knowledge of the Zoology and Botany of the West India Islands, and taking steps to investigate ascertained deficiencies in the Fauna and Flora.

THE HILL ARRIANS OF INDIA.

AT a recent meeting of the Anthropological Society of Bombay, a paper was read by the Rev. A. F. Painter on the Hill Arrians, who live along the slopes of the Western Ghats in the Native State of Travancore, between Quilon in the south and the Travancore-Cochin boundary line in the north. They differ considerably from the ordinary hill tribes of India, and Mr. Painter considers them as Dravidian rather than Kolarian. In colour many of them are remarkably fair. The men average 5 feet 6 inches in height. Their features are, as a rule, well formed. The lips are thin and the nose frequently aquiline. Their villages are situated at a height between 2000 and 500 feet above sea-level. The houses are generally built of split bamboo and mud with grass thatching, but wooden houses such as those used by the inhabitants of the plains are not uncommon. They cultivate the surrounding lands with rice and vegetables.

Their religion and social customs differ considerably from other Malayalam-speaking people, more markedly so in places where they have not come under the power of those living in the plains. Thus in Malabar the law of inheritance through the sister's children prevails. Even the Musnud, or throne, is inherited in this way. But among the Arrians, except where they have been brought under the power of the Hindus, inheritance through the father's children prevails. Even where the former law has been forced upon them they evade the consequences by marrying cousins, so that the property remains in the family. They are divided into *illams*, or clans. Members of the same *illam* may not intermarry; men of a superior *illam* may marry a woman of an inferior *illam*, but the reverse may not be done. There appears to be no difficulty about eating together, but only about intermarriage.

Women occupy a much better position among the Arrians than among the Hindus. They are regarded as equals, move about unrestrictedly, and eat with their husbands, especially at feasts. The fact that a woman eats out of a man's plantain leaf is a sign that she is his wife. The marriage tie is considered sacred, and seldom broken. Polygamy is almost unknown. A man married two sisters, and was considered to have disgraced himself, and was shut out from all feasts, &c. Adultery is considered a great crime. Infant marriage is unknown amongst them, but a curious ceremony prevails, copied from the customs of Nairs and Chogans, though differing in several particulars. As soon as a woman attains maturity, relatives and friends are summoned to a feast. The propitious hour having been fixed, the girl is brought in and made to stand on a plank of jackwood (a tree considered sacred by the Arrians); the father's sister then ties the *tali* or thread round the neck, the feast is then partaken of, and the ceremony is considered complete.

The actual marriage ceremony among the Arrians takes place when the woman is seventeen or eighteen years of age. The horoscopes of the different parties are examined, and the day fixed by the astrologer. Invitations are issued, and a *pandal* erected and the bride placed seated inside. The bridegroom is then brought up by his friends, who demand to know who is inside. The reply is such and such Illakar, as the case may be. If the reply is satisfactory they advance inside, and the bride is brought and placed in the centre. The conductor of the ceremonies on the bride's behalf then proclaims in a loud voice, "I am about to give a woman of such an *illam* to a man of such an *illam*." On the bridegroom's behalf a similar announcement is made. And a set of new clothes is presented by the bridegroom to the bride, and afterwards the happy pair eat out of the same vessel or leaf. This is the crowning part of the ceremony. After a feast the bride is conducted by the wedding party in state to the bridegroom's house, where another feast is spread. The wife lives with her husband in his house.

The Arrians bury their dead. Ancestral worship being practised among them, the ceremonies connected with death are the most elaborate and important. Death brings defilement with it, and none in the house may eat until after the funeral. The body having been washed and betel-nut placed in the mouth, a member of the same clan is appointed, who undertakes to act as master of the ceremonies. He first carefully bathes, then takes a new cloth, and from it tears a narrow strip which he fastens upon himself after the fashion of the Brahminical thread. Going to the place selected for the grave he calls upon the earth to give up 6 feet. He then advances backwards and digs with a hoe, removing three hoesful of the earth. Afterwards he may dig facing the grave. This completed, the body is brought forth and laid in it, the head always lying towards the south. The earth having been thrown, he again advances backwards and draws with a knife three lines round the grave, which are supposed to protect it from evil spirits. A cocoa-nut is broken and some paddy is strewn on the top. In addition to this in some hills a light is placed at the head, another at the foot of the grave. The master of the ceremonies again bathes and returns to the house; two sticks tied crossways are taken and rags soaked with oil tied in the ends and lighted. Taking them in his hands he walks in procession round the house three times, followed by the relations of the deceased. The sticks are then placed, one at the head, the other at the foot of the grave.

After the ceremonies at the grave are over, all concerned in them bathe, a clean new cloth is placed in an inner room of the house, and on it the dead man's property, knife, betel-box, topee, &c., are placed. A feast is prepared, plantain leaves are cut into narrow strips, rice, boiled fowl, plantain, fish, toddy, arrack, and parched rice are placed upon the leaves, lights are lighted, the master of the ceremonies then does obeisance to the spirit which is now supposed to be in the house. The door is closed and the spirit is left to feast. After half an hour it is opened and the things taken forth. At the conclusion of the ceremony the whole assembly partake of a feast consisting of flesh, fish, rice, and arrack. As soon as possible an image of the deceased is prepared, which is brought into the house. Twice a year similar offerings are presented; and in times of drought, ravages by wild beasts, or sickness, vows are made, and prayers such as, "O Ancestor, be not angry with us," are offered.

Female ancestors receive equal honours with males. Of what happens to a soul after death they have no certain belief. The doctrine of transmigration is unknown amongst them. Spirits of men and women for whom no offerings have been made are said to wander about working mischief. If a man dies from accident, such as the fall of a tree, or is killed by a man or wild beast, no ceremonies may be performed for him, nor in the event of a woman dying in child-birth. The spirit is said to wander about working mischief. Ancestral worship is the essential part of their religious system. It consists of a yearly feast and offering to the spirit of ancestors similar to that described as made on the eleventh or fifteenth day after death.

Besides worship of ancestors, there is also the worship of evil spirits or demons, which appears to consist in paying "blackmail" to avoid injury, or bribes to inflict injury on others. The chief demon worshipped by them is the goddess of small-pox, cholera, &c., and it is noticeable that in all their religious festivals and ceremonies strong drink plays a very large part.

ANCIENT MOUNDS AT FLOYD, IOWA.[1]

ON the west side of the Cedar River, one half mile east from Floyd, Iowa, are located a group of three ancient mounds. These mounds, instead of being located on the highest eminence in the region, as is most usually the case, are arranged in a slightly curved line, on a high but level space, fifty feet above, and two hundred and twenty yards back from the stream, and midway between two points (from fifty to sixty rods from each) which face the river, and rise from twenty-five to fifty feet above this level space. The ground, between the mounds and the Cedar, has a rather gently sloping surface. At this point the stream makes a bend to the east, and the mounds thus occupy a position on the south side. The north side of the stream is occupied by a steep, and somewhat broken, wooded bank, which affords a limited though beautiful bit of scenery to this place.

This area, as well as the surface of the mounds themselves, was originally possessed by a heavy growth of timber, but which was cleared away more than twenty years ago, and the soil kept under the plough ever since. These mounds are low and circular, and twenty feet distant from each other. The east, or largest mound, is thirty feet in diameter, and was originally two feet high (so reported by Mr. Sharkey, who first cleared, and still owns the tract), although owing to degradation by the plough it now rises only one and a half feet above the surface of the ground surrounding the mound. The two remaining mounds are smaller and lower than the first one. The third mound—there may be some slight doubt expressed regarding its origin, for the reason that in the south portion of it there is embedded a drift boulder, weighing some seven or eight hundred pounds. This, however, may have been placed here by human hands in the long ago, or the mound may have been an intrusion upon the stone. A partial exploration of the two smaller mounds was made, but without discovering anything.

[1] Reprinted from *The American Naturalist*.

In making a thorough exploration of the larger mound, however, the remains of five human bodies were found, the bones, even those of the fingers, toes, &c., being, for the most part, in a good state of preservation. First, a saucer or bowl-shaped excavation has been made, extending down three and three-fourths feet below the surface of the ground around the mound, and the bottom of this macadamized with gravel and fragments of limestone. In the centre of this floor, five bodies were placed in a sitting posture, with the feet drawn under them, and apparently facing the north. First above the bodies was a thin layer of earth; next above this was nine inches of earth and ashes, among which were found two or three small pieces of fine-grained charcoal. Nearly all the remaining four feet of earth had been changed to a red colour by the long continued action of fire.

All the material of the mound, above and around the bodies, had been made so hard that it was with great difficulty that an excavation could be made even with the best of tools. The soil around the bodies had been deeply stained by the decomposition of the flesh. The first (west) body was that of an average-sized woman in middle life. Six inches to the east of this was the skeleton of a babe. To the north, and in close proximity to the babe, were the remains of a large, aged individual, apparently that of a man. To the east and south of the babe were the bodies of two young though adult persons. The bones of the wonan, in their detail of structure, indicated a person of low grade, the evidence of unusual muscular development being strongly marked. The skull of this personage was a wonder to behold, it equalling, if not rivalling in some respects, in inferiority of grade, the famous "Neanderthal skull." The forehead (if forehead it could be called) is very low, lower and more animal-like than in the "Neanderthal" specimen.

This skull is quite small for an adult individual. The inner portions of the brow ridges are slightly prominent.

The distance from the lower portion of the nasal bone to the upper margin of the eye cavities is only four centimetres. A slight portion of this bone has, however, apparently been broken away.

The distance between the eye sockets at a point midway between the upper margin of the eye cavities and the lower portion of the nasal bone is two and three-fourths centimetres. One of the jaws, containing well-preserved teeth, was found. This was rather strong, but the teeth only moderately so. We were at first inclined to consider the strange form of this skull as due to artificial pressure while living, but a critical examination of it revealed the fact that it was normal, *i.e.* not having been artificially deformed. The teeth of the babe were very small, and the skull thick, even for an adult person.

The next skeleton was that of a man nearly six feet in height. The crowns of all the teeth had been very much worn down, some of them even down to the bone of the jaw.

As before stated, the remaining bodies were those of young adult persons, the skull of one of which was small for a full-grown individual. No relics of any description were found with the human remains in this mound. Their burial appeared to be a very ancient one, the limestone fragments in the floor of the excavation being nearly if not all decomposed.

In other mounds opened on the same stream, at Charles City, six miles below, fragments of the same limestone were not infrequently found, but in no case was decomposition visible, except as a thin outer crust, although the human bones, which were usually more or less abundant, were in no case very well preserved, but, on the contrary, often nearly or entirely decomposed. The fine preservation of the remains in the mound at Floyd was due to the method of burial. This being evidenced by the fact that over a small portion of one of the bodies the earth had not been so thoroughly packed, and as a consequence the bones were almost entirely decomposed away, while the other portion of the body over which the soil had been very firmly packed was well preserved. Judging from all the facts gathered, it seems not improbable to suppose that this represented a family burial.

The question has been raised, "How was it that these five persons were all buried here at the same time, their bodies being still in the flesh?" As we have no reason to suppose that these ancient people possessed any means for preserving, for any length of time, in the flesh, the bodies of their dead, it seems plausible to suppose that these individuals were all swept off at about the same time by some pestilence; or else, upon the death of some dignitary of the tribe or people (perhaps represented by the remains of the old man) the other members of the family were sacrificed, similar to the custom which has prevailed among some ancient tribes or races of historic times.

On the same stream, a short distance below this mound, several other mounds occur which promise to yield interesting results, and which we purpose to explore as opportunity offers.

Charles City, Iowa. Clement L. Webster.

SOCIETIES AND ACADEMIES.

London.

Royal Society, December 11, 1890.—"Determinations of the Heat Capacity and Heat of Fusion of some Substances to test the validity of Person's Absolute Zero." By S. U. Pickering.

Person made determinations with eight substances to show that the temperature at which their heat of fusion became nil, $t - \frac{l}{C - c}$ (t=temperature of fusion, l=heat of fusion at $t°$, C=heat capacity of liquid, c=heat capacity of solid), was $-160°$ in all cases. This he called the absolute zero. His conclusion may for several reasons be questioned, the chief reason being that he determined C and c at any temperature which happened to be most convenient, and the value of these is largely dependent on temperature: they should both refer to the same temperature, and this is, necessarily, $t°$. The author deduces his values for C and c at $t°$ from determinations made at a series of different temperatures. The substances examined were, sulphuric acid and its monohydrate, hydrated calcium nitrate, and naphthalene, and their temperatures of no crystallization were found to be $-369°$, $-177°$, $-234°$, and $-214°$ respectively, thus refuting Person's conclusion. Benzene was also examined, but the heat capacity of the solid was found to be greater than that of the liquid; this is probably due to an incipient fusion occurring below the temperature of true fusion.

December 18, 1890.—"On the Generic Identity of *Sceparnodon* and *Phascolonus*." By R. Lydekker, B.A. Communicated by Prof. W. H. Flower, C.B., F.R.S.

In the year 1872, Sir R. Owen described two imperfect lower jaws of a large extinct Wombat, from the Pleistocene of Queensland, under the name of *Phascolomys* (*Phascolonus*) *gigas*. Subsequently he described certain imperfect upper incisors, from Queensland and South Australia, characterized by their peculiarly flattened and chisel-like shape, under the new generic name *Sceparnodon*.

In cataloguing the fossil Mammalia in the collection of the British Museum, the author was struck by the circumstance that, while the upper incisors of the so-called *Phascolomys gigas* were unknown, there were no cheek-teeth which could be referred to *Sceparnodon*, and it was accordingly concluded that the teeth described as *Sceparnodon* were probably the upper incisors of *Phascolomys gigas*, and on this supposition it was considered that the latter was generically distinct from existing Wombats, and it was accordingly entered as *Phascolonus gigas* in the Museum Catalogue.

The author now described incisors of *Sceparnodon* and lower jaws of *Phascolonus gigas* obtained at Bingera, New South Wales, from the evidence of which it was concluded that we are now justified in definitely regarding the so-called genus *Sceparnodon* as based upon the upper incisors of the gigantic extinct Wombat known as *Phascolonus*.

Geological Society, December 10.—Dr. A. Geikie, F.R.S. President, in the chair.—The following communications were read:—On some water-worn and pebble-worn stones taken from the apron of the Severn Commissioners' weir erected across the river at Holt Fleet about eight miles above Worcester, by Henry John Marten, Engineer to the Severn Commissioners. The weir referred to in the paper was built in 1844 of soft red sandstone, and some of the stones composing the apron of the weir showing signs of decay were removed in 1887. The average quantity of water passing over each square foot of the stones composing the apron has been estimated at about 2000 gallons per minute. A large proportion of the stones had been drilled through and through by the action of the current upon small pebbles lodged in hollows or between the joints of the stone; and the author estimates that, as a result of 43 years of erosion, six of the stones of the apron, which may be taken as a sample, had lost the following amounts respectively: 45, 60, 48, 50, 37, and 58 per cent. In answer to various questions, the author said he believed the action was principally abrasive, as there was only a small proportion of lime in the stone which would be the subject of chemical action. The weir was placed diagonally across the river, and the stones referred to, which were average samples, were taken from the apron at the upper end of the diagonal, where the abrasive effect appeared to be greatest. The pebbles were principally of quartzose description. The rock from which the stones were taken was of a homogeneous character. In the case of stone No. 1, had the action been uniform, the abrasion would represent a loss of nearly one foot three inches from the surface of the stone as originally placed in the apron, and the others in proportion.—On the physical geology of Tennessee and adjoining districts in the United States of America, by Prof. Edward Hull, F.R.S., late Director of the Geological Survey of Ireland. The area described in the paper is occupied by the Unaka or Blue Ridge, which may be regarded as one of the parallel ridges of the Alleghanies, and the prolongation of Prof. J. D. Dana's "Archæan Protaxis." It runs in a general south-westerly direction, and attains an elevation of 6760 feet. At its base, and to the north-west of it, is the Valley of East Tennessee, about 40 miles wide, and furrowed by north-east and south-west ridges and depressions, parallel to the strike of the Cambrian and Silurian beds. Through this runs the Tennessee River, which, instead of running south to the Gulf of Mexico, turns to the north-west, some distance below Chattanooga, and cuts through the Cumberland table-land, a prolongation of the Appalachian Mountains, and flows into the Ohio River. The Cumberland table-land has an average height of 2000 feet above the sea, and 1350 feet above the Tennessee River at Chattanooga. It consists of a synclinal of Carboniferous rocks resting conformably upon the Devonian beds, and is bounded along the East Tennessee Valley by a curved escarpment; a similar though more indented escarpment forming its north-western margin, and separating it from the Silurian plain of Nashville. The table-land is about 40 miles wide, and is intersected by the Valley of the Sequachee River, running in a north-easterly direction along a subsidiary anticline from near Jasper for a distance of 60 miles. From the base of the Cambrian beds, the whole Lower and Upper Palæozoic formations succeed each other in apparently conformable sequence, except at the junction of the Upper and Lower Silurian series, where a probable discordance occurs. The prolonged period of subsidence and deposition at length gave way to elevation; acting with the greatest effect along the Alleghanies. Under these circumstances, denudation proceeded most rapidly along the tract bordering the Protaxis, whilst the synclines were protected from erosion to a greater degree; and as the elevatory movement was more rapid along the Unaka range, the flow of the streams was generally westward. At a later period the Cumberland plateau began to be formed by backward erosion of the strata in the direction of the dip; so that it owes its development to the erosion of the Tennessee and Clinch Rivers on the one hand, and to the Cumberland River on the other. Where the Tennessee River flows in a north-westerly direction through the Cumberland plateau, the divide between it and the Gulf of Mexico is only 280 feet above the river-bed, whilst the table-land is 1400–1500 feet above. The author infers, therefore, that when the river began to erode its channel the plateau was relatively lower than the tract to the south of the present course of the stream, but that by denudation the relations have been reversed, whilst the river has never left its originally selected course. The author compares the state of things with that which must have occurred in the case of the northerly rivers running from the centre of the Wealden axis; but mentions that Prof. Safford and Mr. J. Leslie account for the Cumberland plateau by faulting, though he thinks that the well-defined escarpment along the Valley of East Tennessee seems to show that this cause is insufficient. In conclusion, he believes that the denudation was accelerated during the pluvial or "Champlain" period, and calls attention to the "Columbia formation" of the east side of the Alleghanies, and to the deposit of red loam by which the surface of the country of the valleys of the Tennessee and Sequachee is overspread, and which is probably referable to a similar stage. After the reading of the paper there was a discussion, in which Mr. Topley, Prof. Hughes, Mr. Wills, Dr. Hyland, and the President took part. The President found difficulties here, as elsewhere, in realizing the form of the ground when the rivers began to flow, and in discovering whether there were subterranean movements which affected the denudation. He felt that the explanation of the topography might not be so simple as Prof. Hull made out, and would like to have more details as to the structure of the ground. The author, in reply, concurred with the remarks of the President as to the complex character of the subject.—On certain Ornithosaurian and Dinosaurian remains, by R. Lydekker.

Royal Meteorological Society, December 17, 1890.—H. F. Blanford, F.R.S., Vice-President, in the chair.—The following papers were read:—Note on a lightning stroke presenting some features of interest, by R. H. Scott, F.R.S. On January 5, a house near Ballyglass, Co. Mayo, was struck by lightning, and some amount of damage done. A peculiar occurrence happened to a basket of eggs lying on the floor of one of the rooms. The shells were shattered, so that they fell off when the eggs were put into boiling water, but the inner membrane was not broken. The eggs tasted quite sound. The owner's account is that he boiled a few eggs from the top of the basket, the rest were "made into a mummy," "the lower ones all flattened, but not broken."—Note on the effect of lightning on a dwelling-house, by A. Brewin. This is an account of the damage done to the author's house at Twickenham on September 23.—Wind systems and trade routes between the Cape of Good Hope and Australia, by Captain M. W. C. Hepworth. The author is of opinion that the best parallel on which commanders of vessels navigating the South Indian Ocean between the Cape of Good Hope and the Australian colonies should run down the longitude is between the 41st and 42nd parallels during the winter months, and between the 45th and 46th parallels during the summer months.—Report on the phenological observations for 1890, by E. Mawley. Taking the year ending August, the weather of the autumn, winter, and spring, and of the first summer month could scarcely have been more favourable for vegetation, while that of July and August proved altogether as unpropitious.—The climate of Hong Kong, by Dr. W. Doberck. This is a discussion of the meteorological results at the Hong Kong Observatory, and at the Victoria Peak, during the five years 1884–88.

CAMBRIDGE.

Philosophical Society, November 24, 1890.—Prof. G. H. Darwin, President, in the chair.—The following communications were made:—On the beats in the vibrations of a revolving cylinder or bell, and their bearing on the theory of thin elastic shells, by Mr. G. H. Bryan. It is well known that if a vibrating rod of circular section is rotated upon its axis the plane of vibration remains fixed in space instead of turning round with the rod—an experiment frequently used to illustrate the corresponding property of polarized light. If, on the other hand, a tuning fork is rotated, beats will be heard which indicate that the planes of vibration turn with the fork. In this paper it is shown that when a bell or other body symmetrical about an axis is vibrating, and at the same time revolving about that axis, an intermediate effect will in general be observed. The nodal meridians will rotate, but with a smaller angular velocity than the body. This is in the first place proved from general considerations for the case of a rotating ring or cylinder, which, as in the investigations of Hoppe and Lord Rayleigh, is supposed inextensible to the first order of small quantities. In the mathematical investigations which follow, the purely statical effects of centrifugal force are separated more easily by supposing the ring to be also acted on by an attraction to the centre proportional to the distance. Taking the type of vibration which has $2n$ nodes, the author finds that these nodes rotate about the axis with angular velocity

$$\frac{n^2 - 1}{n^2 + 1}\omega,$$

where ω is the angular velocity of the ring. The number of beats heard per revolution of the ring will therefore be

$$2n\frac{n^2 - 1}{n^2 + 1},$$

instead of $2n$, which would be the number if the nodes were to rotate with the ring. Putting $n = 2, 3$, &c., we find the numbers of beats per revolution corresponding to the successive tones to be 2·4, 4·8, 7·059, 9·231, 11·351, &c., approximately. The results of experiment were found to agree fairly closely with theory.—On liquid jets, by Mr. H. J. Sharpe.—Notes on the application of quaternions to the discussion of Laplace's equation, by Mr. J. Brill.—On a simple model to illustrate certain facts in astronomy, with a view to navigation, by Dr. A. Sheridan Lea.—Note on a paper in the Society's Proceedings, by Mr. W. Burnside.

PHILADELPHIA.

The Franklin Institute, November 18, 1890.—Chemical Section.—Alloys of sodium and lead, by Wm. H. Greene and Wm. H. Wahl. Recently, in the course of certain investigations, in which we had occasion to make use of alloys of lead and sodium, we found that the properties of such alloys did not correspond with what we had anticipated from previous publications on the subject, and we were led to an examination of the properties of lead-sodium alloys of definite composition. These alloys may easily be made by direct combination, and the products are then sensibly constant in composition, which is not the case when they are prepared by reducing lead oxide by carbon in presence of soda, or by heating litharge with sodium tartrate, as described by Vanquelin and Serullas. The required quantity of sodium was added to lead melted in a covered crucible, and the alloy was roughly analyzed by determining lead only. Our alloys contained from 3 to 31 per cent. sodium. They are all brittle and crystalline; all decompose water, that containing the least sodium producing a hardly perceptible evolution of gas, while that containing 31 per cent. reacts with violence. The brittleness and oxidability increase with the percentage of sodium. The richest alloy is greenish in colour, and instantly blackens on exposure to air. We made special examinations of the alloys corresponding in composition to Na_2Pb_3, Na_2Pb, and Na_4Pb: the first of these contained 10 per cent. sodium, the second 19·5 per cent., rather more than would be indicated by the formula, while the last contained 31·7 per cent. The densities were determined in aniline, and found to be considerably higher than would be the densities of mixtures of the same composition. Thus the 10 per cent. alloy has a density of 6·91, the 19·5 per cent. a density of 4·61, and the 31·7 per cent. alloy a density of 3·81. The densities of corresponding mixtures would be 5·6, 3·7, and 2·7 respectively. The theoretical and calculated percentages of Na in alloys of above assumed composition compare as follows:—

	Na_2Pb_3.	Na_2Pb.	Na_4Pb.
P.C. Na by theory	10	18·18	30·8
,, found	10	19·5	31·7

PARIS.

Academy of Sciences, December 22, 1890.—M. Hermite in the chair.—On the history of the hydrostatic balance, and some other pieces of apparatus and scientific processes, by M. Berthelot.—On the ultra-violet limit of the solar spectrum, from photographs obtained by Dr. O. Simony at the summit of the Peak of Teneriffe, by M. A. Cornu. The author discusses some photographs of the solar spectrum obtained at Teneriffe, in relation to others taken at Courtenay, for the purpose of determining the influence of the atmosphere on the ultra-violet region. The following is a comparison of results:—

Photographs obtained at	Altitude. Metres.	Wave-length of the last trace of the spectrum.	Wave-length of the beginning of the end.
Teneriffe ...	3700	292·2	293·7
Courtenay ...	170	284·8 (U)	298·0

—Contribution to the natural history of the truffle, by M. Ad. Chatin.—On minima surfaces, by Prof. A. Cayley.—Singular case of germination of the grains of a Cactacea in their pericarp, by M. D. Clos.—Improvement of the culture of the potato, for industrial and fodder purposes, in France, by M. Aimé Girard.—Meteoric period of November 1890, by M. P. F. Denza. (See Our Astronomical Column.)—On the normals to quadrics, by M. Georges Humbert.—Electro-magnetic resolution of equations, by M. Félix Lucas.—Researches on refraction and dispersion in an isomorphous series of crystals having two axes, by M. Fr. L. Perrot. The substances investigated are the double salts formed by the combinations of sulphate of zinc with sulphates of the metals K, Rb, Cs, Tl, and the group NH_4. It is shown that, with the exception of the ammonium salt, the index of refraction increases with the molecular weight.—On a new series of ammoniacal compounds of ruthenium, derived from $RuNOCl_3$, by M. A. Joly.—On the combinations of ammonia with the chlorides and bromides of phosphorus, by M. A. Besson.—A method for obtaining pure phosphoric acid, in solution or in the vitreous state, by M. M. Nicolas.—Colour reactions of aromatic amines, by M. Ch. Lauth.—New process for the detection of fraud in olive oils, by M. R. Brullé.—Experimental researches on cow vaccine, by MM. Straus, Chambon, and Ménard.—Physiological action of morphine on the cat, by M. L. Guinard.—On the action of motor nerves during the desiccation of muscles, by M. N. Wedensky.—On the dimorphism of males of Crustacea amphipoda, by M. Jules Bonnier.—On the reproduction of autolyte, by M. A. Malaquin.—On the apidological fauna of South-West France, by M. J. Pérez.—The relations between the actual deformation of the earth's crust, and the mean densities of rocks and of the waters of seas, by M. A. Romieux. The relation discussed is that between the volume and density of the waters on the earth and that of the land surface.—On the geological history of the Sahara, by M. Georges Rolland.—On the soundings of the lake of Annecy, by MM. A. Delebecque and L. Legay.—On millerite from Morro-Velho, in the Minas Geraes province, Brazil, by Dom Pedro Augusto de Saxe-Cobourg-Gotha.—On offretite, a new mineral, by M. Ferdinand Gonnard.—On the rocks inclosed in trachyte from Menet (Cantal), and on their modifications and origin, by M. A. Lacroix.—On the distinction of two ages in the formation of dunes at Gascogne by M. E. Durègne.—The tornado of August 18, 1890, in Brittany, by M. G. Jeannel.

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