THURSDAY, NOVEMBER 29, 1888.

HAUPTMANN ON HARMONY AND METRE.

The Nature of Harmony and Metre. By Moritz Hauptmann. Translated and Edited by W. E. Heathcote, M.A. (London: Swan Sonnenschein and Co., 1888.)

THE author of this singular book was a noted man in the German musical world. He was not only an eminent violinist (pupil and intimate friend of Spohr), but a composer of merit, and, what is more to our purpose here, a great authority on the theory of music and the laws of composition. In 1842 he was appointed, on Mendelssohn's recommendation, Cantor and Musik-Director of the celebrated Thomas-Schule at Leipzig, and Professor of Counterpoint and Composition at the new Conservatorium in that city, filling these posts till his death in 1868. He was undoubtedly the foremost teacher in Europe, and his legion of pupils comprised many who afterwards became musicians of great eminence, including F. David, Curschmann, Burgmüller, the Baches, Joachim, von Bülow, Mr. Cowen, and Sir Arthur Sullivan. He was, moreover, a highly educated man and a Doctor of Philosophy. Hence any writings on music by him could not fail to command attention. His best-known work is a series of letters to his intimate friend, Hauser, the Director of the Conservatoire at Munich, full of spirit, admirable criticism, and deep musical knowledge.

Hauptmann's scientific turn of mind led him to study earnestly the problem which has occupied so many thinkers ever since music has had any consistent form—namely, to discover how the structure which, for some reason or other, seemed most appropriate for it, could be accounted for and explained on philosophical principles. The Greeks talked vaguely of the music of the spheres, and learned men of later days referred it to arithmetical proportions; both with as much reason as modern English musicians join in the *ignis-fatuus* hunt after "natural systems of harmony." Hauptmann sought the explanation in the depths of German metaphysics, and having satisfied his own mind on the matter, he brought out, in 1853, an elaborate volume which he called "Die Natur der Harmonik und der Metrik, zur Theorie der Musik," of which the book now before us is an English version. It appears by a part of the original preface (omitted by the present translator) that he had already introduced the system into his "musikalisch-theoretischen Cursus," and there is no doubt it attracted considerable attention in Germany. A second edition of the book was published in 1873.

But in the meantime the matter had assumed a new shape, through the labours of another German philosopher, Helmholtz. He had the advantage over Hauptmann of a large practice in experimental physics; hence it occurred to him to attack the problem on the physical side, and the result was the well-known epoch-making work, published in 1863, "Die Lehre von den Tonempfindungen als physiologische Grundlage für die Theorie der Musik." He investigated, far more thoroughly than anyone had done before, the physical properties of musical sounds, and their physiological effects on the ear. He showed, with the aid of the facts thus obtained, that the structure of music generally in use was due partly to the nature of the sounds of which it was composed, and partly to artificial considerations which had presented themselves to musical composers on æsthetic and artistic grounds. This swept away the necessity for any abstruse metaphysical speculations; and we strongly suspect that if Hauptmann had waited till Helmholtz's discoveries had been made known, the present treatise would never have appeared.

It is a question, under these circumstances, how far it was worth while to reproduce the book in English; however, here it is; and anyone interested in musical metaphysics, or metaphysical music, and who is unable to read the original, may puzzle over it to his heart's content. It is a handsome volume, and the translation is (so far as we have been able to compare it) carefully and intelligently done. Some of the language may seem uncouth, and much of it obscure, but this is inevitable from the nature of the original, the rendering of which must often have been a tough job. The translator has prefixed an introductory essay, and there is an appendix, entitled "A Short Analysis of Hauptmann's Treatise," which, as we do not find it in the original, we presume is the translator's also, though he does not say so.

The main feature of the work is the application, to musical structure, of the philosophy of Hegel. It would be useless here to attempt to discuss this elaborate subject, or even to describe it. All we can do is to insert a few extracts to give an idea of what sort of thing a person must expect who would take up the work.

As a general point of departure, the translator tells us that—

"The fundamental idea of the philosophy is that every notion—as key, scale, seventh chord, resolution, and so on—is made up after this fashion, *i.e.* that it possesses three elements, involving an antithesis and a reconcilement, and that one of the three elements is the root from which the other two, and so the whole construction, spring. This, Hauptmann regards as self-evident, and it is the basis of Hegelian metaphysics."

The author then says:—

"For the first step it will only be requisite to acquire an inward conception of the notion of the formative process in its wholeness, in the unity of its three elements with which we become acquainted in their first utterance, as the intervals of the octave, fifth, and third. This notion is and remains everywhere the same, in every formation and transformation. It is the notion that something, which at first subsists for intuition in immediate totality (octave), parts from itself into its own opposite (fifth), and that then this opposite is in its turn abolished to let the whole be produced again as one with its opposite (triad), as a whole correlated in itself. Going in the universal sense of this notion, we shall soon be obliged to grant that it no less than comprehends in itself the elements of all knowing, and that anything further for knowledge is not conceivable (*dass ein Weiteres für die Erkenntniss nicht mehr denkbar ist*)."

Sound is defined as

"the coming to be of the being which subsists absolutely during rest, and which is alternately abolished and restored. Not being in self, or dead persistence in rest, nor yet being out of self (*ausser-sich-seyn*) in the motion, is sounding, but coming to self (*zu-sich-kommen*)."

The octave is

"the expression for the notion of identity, unity, or equality with self."

The fifth

"contains acoustically the determination that something is divided within itself, and thereby the notion of duality and inner opposition (*sich selbst Entgegengesetztes*)."

The third contains

"the notion of identification of opposites; of duality as unity."

Of the scheme of a major key, as derived from triad construction, it is said:—

"The organic property of a membered whole can never be represented exhaustively, either by symbols and numbers or by words; it can only be spiritually indicated to intellectual feeling, *i.e.* reason, that meets it half-way. . . . . Now, because this notion has to unite both union and separation, it can only be fulfilled in endlessly continued passage into contrary and comprehension of all opposites. Thus it must be conceived as an infinite process, and consequently as the notion of eternal becoming, living or being real. This is Nature, who, produced as duality from the prime unity (*Ureinheit als Zweiheit hervorgegangen*), and busied continually in making her opposites be absorbed into one another, is live being itself and reality."

This is the sort of way in which harmony is treated; and the same system is adopted in the latter part of the book in regard to metre and rhythm. Most elaborate explanations, illustrated with tables and diagrams, are given of all varieties of time, rhythm, and accent, and they are all etherealized in the same transcendental style. For example, in regard to four-timed metre, we are told that—

"It is this inner reconcilement of separation in unity and unity in separation, the completed negation of every negativing excluding element, that speaks to us here in metrical determination as the essence of the triad, but in combinations of notes as the perfection of harmony; and generally in any guise of phenomenon as the perfected notion of determinate reality."

But we must, in justice to the book, explain that it is not entirely given up to Hegel. The translator properly remarks that, independently of the theory involved, it contains an account, written by a skilful musician and experienced teacher, of harmony and metre, in which the received rules are explained on general principles, while upon particular points new and unexpected light is frequently cast. This is true, for, though Hauptmann loved Hegel well, he loved music more, and nothing could repress his desire to amplify his explanations upon it. It is quite delightful to find him now and then throwing the metaphysics overboard, and breaking out into intelligible common-sense language on the facts and rules of harmony. One can only regret that this sort of thing is not general instead of exceptional.

We have good precedent for this regret. Hauptmann's book was well known to Helmholtz. He studied it carefully, made himself master of all its intricacies, and commented on many of his predecessor's statements and opinions with much respect. Yet what judgment did he pass upon Hauptmann's work as a whole? We will give the passage in the original, as it is really a most true and effective criticism. He says (original edition, p. 427):—

"Ich kann mich nur dem Bedauern anschliessen, welches C. E. Naumann ausgedrückt hat, dass so viele feine musikalische Anschauungen, welche dieses Werk enthält, unnöthiger Weise hinter der abstrusen Terminologie der Hegel'schen Dialectik versteckt und deshalb einem grösseren Leserkreise ganz unzugänglich sind."

It is, indeed, a great pity that so much excellent musical matter, so many "refined musical views," as this work contains, should be hidden in such an atmosphere of impenetrable transcendental fog, and thus rendered inappreciable by any large circle of readers.

There is another reason which adds seriously to the difficulty of understanding these portions—that is, the use of a complicated system of alphabetical letters in varied types to express musical notes, chords, and passages, instead of the ordinary musical notation. The latter conveys its idea at a single glance; the former requires mental labour which is most wearisome. It is true the author explains that he wants in some cases greater accuracy than the ordinary notation will give; but this is obtained at a fearful sacrifice of clearness and intelligibility, and surely the object might have been accomplished in some less repulsive manner.

It seems odd that the translator, in his elaborate explanatory introduction, never mentions or notices Helmholtz in any way. One would hardly have expected that in the present day, when the science of music has come to be based chiefly on Helmholtz's investigations, a philosophical essay on the subject could have been penned without some allusion to them. There are many passages where the comparison seems invited; for example, Mr. Heathcote says: "The science of music is concerned more with the form than the materials." This is directly at variance with the work of Helmholtz, who bases all his science on the nature of the materials of music—namely, musical sounds—leaving the form to develop itself therefrom. Again, in explaining to us how we are to proceed to learn and follow out the Hegelian system, the translator says:—

"Therefore, when musical sound is said to be unity, we are not to think of it as existing as it does now, and capable of being distinguished into chords, notes, and scales. . . . . Musical pitch, tone, or quality of sound, which depend upon the triad, must not be thought of as existing before the triad exists, and still less as contributing to the formation of the triad."

How different this is from the philosophy of Helmholtz, who demonstrates to us that the triad is a product directly springing out of the physical nature of musical sound!

To sum up, the book is a very remarkable one, the product of a great mind, and of a great authority on the subject it treats of. But we should fear that its interest to English readers will be very limited. The Hegelian portion of it, although, as the translator remarks, it may serve the metaphysician as an application of this peculiar philosophy to a concrete subject, can be of little use to the musician as an explanation of the principles of his art. And as to the technical musical matter, which is really very valuable, the way to render its value appreciable by the public would be to translate it, not merely into English, but into a kind of language with which musicians are familiar, and which ordinary minds would be able to comprehend. W. POLE.

BOTANY OF SOCOTRA.

Botany of Socotra. By Isaac Bayley Balfour, M.D., F.R.S., &c., assisted by other Botanists. Forming Vol. XXXI. of the Transactions of the Royal Society of Edinburgh. (Edinburgh: Grant and Son. London: Williams and Norgate. 1888.)

UNDER the auspices of the Royal Society of London and the British Association for the Advancement of Science, Dr. Bayley Balfour proceeded to Socotra early in 1880, accompanied by Mr. A. Scott, a gardener, and joined at Aden by Lieut. Cockburn, for the purpose of investigating the natural history of the island. Originally it was intended that Colonel Godwin-Austen should lead the Expedition, but circumstances prevented him; and the Committee, through Sir Joseph Hooker, thereupon requested Dr. Balfour—who had recently completed an account of the results of his botanical exploration of the Island of Rodriguez—to undertake the task. He had just been appointed to the Botanical Chair in the University of Glasgow, and the duties connected therewith demanded his presence after the middle of April, therefore the duration of the expedition was necessarily very limited, and the time actually spent on the island was little more than six weeks, ending on March 30. Nevertheless, the botanical collections included specimens of between five and six hundred species of Phanerogams, besides Cryptogams; though, as might be expected from the hasty manner in which the work had to be performed, and the short season during which the plants were collected, many of the specimens were imperfect. The following spring a party of German naturalists visited the island, among them the distinguished African traveller and botanist, Dr. Schweinfurth, to whom Dr. Balfour sent a catalogue of the plants he himself collected, with the gratifying result presently to be explained.

Fortunately the German Expedition entered upon its labours a fortnight later than the time when the British Expedition finished its work. Vegetation was then at a later stage of development; and Dr. Schweinfurth was able to supplement largely Dr. Balfour's botanical collections, especially in the direction of much more complete material. The unselfish devotion to science displayed by Dr. Schweinfurth is eulogized by Dr. Balfour in the following words:—

"With a generosity which is as pleasing as it is rare, he subsequently sent his collections to me in England, in order that the whole flora might be worked out in one. I have already had opportunity to express publicly my lively appreciation of this act of friendship and self-abnegation, and I wish here to put the fact again on record, and to say how much Dr. Schweinfurth's specimens have contributed to the satisfactory working out of the details of the flora. The value of his collection must not be measured either by the number of the species, or by the species he found which we had not gathered. In the excellence of his specimens and their completeness, and the way in which they so frequently supplemented, in flower and fruit characters, deficiencies in ours—therein lay the value of Schweinfurth's plants, and I cannot appraise it too highly."

Dr. Balfour elaborated the joint collections at Kew, and it was announced in NATURE five years ago that he had completed the descriptive part; but delays in connection with the production of the plates, which need not be particularized, we are informed in the preface, prevented the issue of the volume until this year.

It is satisfactory, however, to be able to congratulate Dr. Balfour and the Royal Society of Edinburgh on the quality of the work now first presented to the public in a complete form. Although it may fall considerably short of being an account of all the plants that inhabit the island, it is sufficiently full to be of the utmost value to the student of systematical and geographical botany. Dr. Balfour's 521 pages of letterpress are illustrated by 100 plates, drawn by W. H. and J. N. Fitch, Mrs. Thiselton Dyer, and Miss M. Smith, though mainly by the latter lady. Apart from the descriptive matter, the geographical distribution of the individual species, and the affinities of the flora as a whole, are given in ample detail in the form of an introductory essay, which will rank high among recent contributions to phytogeography. There is also a map on a scale of about 1¼ inch to 5 miles, and a brief sketch of the position, physical features, and geology of the island.

Socotra is situated off the north-east corner of Africa, about 500 miles from the entrance to the Red Sea, between 12° and 13° N. lat., and 53° and 54° E. long. Its extreme length from east to west is about 72 miles, its breadth 20; and it is about 140 miles from the nearest point of the African coast, and a little more distant from the Arabian. The surface is mountainous, the interior averaging 1000 feet in altitude, with granite peaks exceeding 4000 feet. The main plateau is of limestone deeply cut into ravines and valleys. In some places the hills rise abruptly from the sea, in others there are intervening sand-plains several miles in width. The climate is cooler and more humid than in the adjacent parts of Africa and Arabia, and in some parts there are perennial streams; but the character of the vegetation generally is that of a dry sterile country.

Out of 575 Phanerogams, or flowering plants, collected, 10, including the date and palmyra palms, orange, cotton, tamarind, and castor-oil, were undoubtedly planted, and many others weeds of wide dispersion; whilst 206 are apparently endemic; and there are 20 endemic genera. Unfortunately, as I think, Dr. Balfour has not deducted the certainly and probably introduced species in his analysis of the composition of the flora, hence his figures representing the percentages of endemic genera and species do not reveal the true facts. Thus he calculates that 36·5 per cent. of the species, and 6·3 per cent. of the genera, are endemic; and these are high percentages, considering the short distance the island is separated from the mainland, and indicate a very ancient flora; but had he eliminated the species in question, these percentages would have stood even higher. On the other hand, it is a mistake to say (Introduction, p. xlix) that the percentage of endemic species is about the same as in Madagascar, where the proportion of endemic genera is double that in the Socotra. The percentage of endemic species in the Madagascar flora is probably at least double that in the Socotran flora, and Mr. Barron, in a paper he recently read before the Linnean Society, estimated the endemic element at 80 per cent. of the species. In British India it is as high as 68 per cent.; in Mexico and Central America combined it reaches 70, and in the whole of Australia 80 per cent.

A comparison of the flora of Socotra with that of the

Bermudas, which are more than three times the distance from the nearest mainland, will give some idea of the differences in the age of the two. The latter contains no endemic genus, and only about half a dozen species, and these are not of a highly differentiated character—one is a portion of a very old flora, and the other a flora of recent derivation. The small flora of Juan Fernandez (which lies about 400 miles from the coast of Chili) contains 21 per cent. of endemic genera and 78 per cent. of endemic species.

The endemic element in Socotra is distributed over fifty-four natural orders, and includes some highly curious types, such as *Cocculus Balfourii*, remarkable for its thick rigid cladodes and often leafless condition ; *Thamnosma socotrana*, a member of a Mexican genus ; *Dirachma*, a geraniaceous genus of American affinities ; *Dendrosicyos*, unique in the Cucurbitaceæ for its arboreous character ; *Trichocalyx*, a new genus of the Acanthaceæ ; *Cockburnia*, a new genus of the almost exclusively African Selagineæ ; and *Cælocarpus*, a new genus of the Verbenaceæ, having strong American affinities. Of the 136 genera to which the endemic species belong, 98 are only known to be represented by endemic species ; and of the 20 endemic genera, 18 are monotypic. The isolated types of American affinities are a repetition of what has also been observed in the fauna and flora of Madagascar.

In summing up, Dr. Balfour finds that the affinities of the flora are essentially tropical African and Asian, the former more pronounced. A former, though very ancient, land connection with Africa he regards as conclusively proved, and the evidence strongly favours the supposition that it was also united with Arabia. With regard to the element of strongest American affinity, its presence is still an unsolvable problem. W. B. H.

THE METALLURGY OF GOLD.

The Metallurgy of Gold; a Practical Treatise on the Metallurgical Treatment of Gold-bearing Ores. By M. Eissler. (London : Crosby Lockwood and Son, 1888.)

THE title suggests that this little volume is a more comprehensive treatise than the author has attempted to write, but it is nevertheless likely to be useful to a large class of readers.

There is a wide-spread belief that, as much of the gold in Nature is found in the "native" or metallic state, its metallurgy must be comparatively simple ; and so it would be in nearly all cases if it were not for the fact that the precious metal often occurs in a very fine state of division, or in association with sulphides and tellurides of other metals. Ignorance as to the true nature of such ores has led to their being considered to be "base" or "rebellious," and has entailed much loss and disappointment.

It is asserted, on p. 5, that native gold is never quite pure, being almost invariably alloyed with silver ; reference might, however, have been made to the interesting deposit of gold of exceptional purity recently discovered at Mount Morgan, in Australia.

The author points out that "the loss on working gold ores, even with our most modern appliances, is still enormous"; and he gives, among other statistical statements, the results of seven years' working in Colorado, where the average value of the precious metal in the ore, by assay, was £7 18*s.* per ton, while the amount actually extracted was only £3, showing a loss of over 60 per cent. A large section of the work is devoted to the consideration of methods of concentrating the free gold lost during crushing and amalgamating, and the processes for extracting the gold either by amalgamation or by chlorination. The author writes with a practical experience of the processes he describes, but his information is in many cases not up to date.

The illustrations are usually very clear, but in no instance is the scale on which they are drawn given, and there are too few references to the dimensions of the appliances described. The appearance of the illustrations often suggests that they have been borrowed from the trade circulars of the makers of mining machinery. It would surely have been possible to give a more intelligible section of a chlorination works than the one (p. 142) which was suitable enough for its purpose when it originally appeared as an incidental illustration to an Official Report to the United States Government published in 1873 ; and a far more useful drawing might have been found to accompany the description of the refining of gold by Miller's process than the diagrammatic one which was drawn eighteen years ago by the writer of the present review in order to make the nature of the process clear in an Official Memorandum.

More useful information than is to be found in the twenty-one pages devoted to assaying might easily have been condensed into them, and it is much to be regretted that no attempt has been made to deal with the treatment of tellurides, which are so troublesome to the smelter, and have occasioned so much loss. In a future edition it would also be well to give a description of the process of collecting the precious metals in lead, which plays so important a part in the smelting of complex auriferous ores of lead and copper. Hydraulic mining, also, should find a place. Certain defects of style will no doubt be corrected in a second edition ; this will be welcome, for, although the work is hardly in sufficient detail to justify its being called a "practical treatise," it will be useful, especially to men who are engaged in smelting. The author suggests that it contains sufficiently full information for "investors and others interested in gold-mining operations who may wish to gain an intelligent insight into the *modus operandi* at the gold-mines." To them it may be warmly recommended, for, although the element of speculation can hardly be separated from genuine investment in gold-mines, the "adventurers," to use the old name, often deliberately neglect all investigation into the nature of the methods by which they hope to profit. W. C. ROBERTS-AUSTEN.

OUR BOOK SHELF.

Viaggio di L. Fea in Birmania e regioni vicini. II. "Primo saggio sui Ragni Birmani." Del Prof. T. Thorell. (Genova : Tipographia del R. Istituto Sordo-Muti, 1887.)

DR. THORELL deserves our best thanks for having begun a faunistic work on the spiders of Burmah ; still greater would have been our gratitude had his minute and exhaustive descriptions been accompanied by figures of the numerous new species recorded. One of the greatest hindrances in the study of exotic araneology is the paucity of such works. The present, however, is not the first contributed, by Dr. Thorell's labours, for the supply of works on the spider-fauna of exotic regions: witness his extensive work, "Studi sui Ragni Malesi e Papuani," in three volumes, 1877–81, describing and recording over five hundred species of the spiders of that richest of all known exotic regions, the Malay Archipelago. Up to the time of the publication of the present volume, but few Burman spiders were known, the earliest being an Epeirid, described by the late Dr. Stoliczka in 1869. This was followed, in 1878, by two others recorded by Mr. T. Workman, of Belfast; eight more were recorded in 1881 in Dr. Thorell's work above mentioned (on the spiders of the Malay Archipelago); and finally, twenty-two more by M. Eugène Simon in 1884. The total number of species of Burmese Araneidea now known is 163, by far the larger portion being new to science. Dr. Thorell records 145 species, of which 90 are new to science, and 130 new to Burmah. These figures doubtless give a very meagre idea of the spiders of such a rich zoological district as the Burmese Empire. Not to mention its situation in the tropics, Burmah has much in common with the productions of China, Siam, India, and the Malay Archipelago. We may therefore safely hazard a conjecture that the figures given by Dr. Thorell can scarcely represent a twentieth part of the spiders of Burmah. It is to be hoped that since the—still comparatively few—species as yet known have been collated in the present work, an impetus will be given to natural history collectors to add to our knowledge. For the want of figures in Dr. Thorell's work there is some compensation in the concise diagnoses which head each lengthened description. Excepting the introduction, which is in Italian, the work is written in Latin. It is exceedingly well got up, forming a handsome volume of over 400 pages, and is dedicated to the Rev. O. P. Cambridge, General A. W. M. Van Hasselt, and Dr. Ludwig Koch. The greater part of the Burmese spiders described by Dr. Thorell were collected by Signor Leonardo Fea, mostly in Upper (or North) Burmah; and of the rest some were collected in its southern and some in its central districts.

O. P. CAMBRIDGE.

An Introduction to Practical Inorganic Chemistry. By Wm. Jago, F.C.S., F.I.C. (London: Longmans, 1888.)

THIS volume of 72 pages is chiefly taken from the author's "Inorganic Chemistry, Theoretical and Practical," and has been separately issued by request. It is doubtless sufficiently exact for students who are preparing themselves for the "growing number of examinations in which the practical analysis of one or more simple salts is required," and therefore presumably it will serve the end for which it was designed; but it contains many statements that would tend to confuse the genuine student, and give his teacher unnecessary trouble. For instance, on p. 15 it is stated that a "brown ppt. of SnS" is soluble in "SAm_2, re-pptd. by HCl"; and a little lower down that "Arsenic, arsenious compounds . . . Heated in tube—all salts sublime." Both of these statements have so slender a foundation in fact that their direct tendency is to deceive the student. These are mere examples that might be multiplied considerably. Towards the end of the book, tables are given for the "Examination of Mixtures," and wherein they differ from the methods in common use they do not appear to be improvements upon them.

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.*]

Engineers *versus* "Professors and College Men."

SOME of your readers may recollect that last year (NATURE, vol. xxxv, p. 462) I was led to put the following question to the *Engineer*:—"What is the result of dividing 10 eggs per minute by 2 eggs? Would it, or would it not, be 5 eggs per minute"? To this I have not yet received an answer! It must be very difficult!

I ventured to put the question because the *Engineer* had (in a leading article!) asserted that the result of dividing 3,942,400 foot-pounds per minute, by 33,000 foot-pounds, is 119·4 horse-power. It had actually accused me of ignorance for pointing out the error of such a statement!

But even this remarkable dictum is outstripped in absurdity by some of the *Engineer's* more recent assertions. I quote only a few, any one of which would be sufficient for my present purpose, leaving the vast storehouse to be fully ransacked by other inquirers more curious, and less busy, than myself.

On the 10th of August last the readers of the *Engineer* were treated to the following *ex cathedrâ* pronouncement:—

". . . it is almost impossible to point to anything of value in trade, or manufactures, or engineering which has emanated from the highly-trained Professor or College man."

No comment whatever need be made on this.

A fortnight later, in reviewing a book on the steam-engine, the *Engineer* remarks of Carnot's principle:—

". . . it will soon be understood that with steam this is not true. . . ."

There is nothing in the context to qualify this assertion, rather there is much to intensify and aggravate it. There is manifestly a confusion between temperature and pressure; but it is difficult to find its exact nature; and the only at all analogous case that I can remember is embodied in the indignant outburst of a Celtic student, hard pressed in an "oral," "D'ye mane to till me, Sor, that wather boils at a hundthred degrees in Oireland?"

It might well be thought that we had now gauged the maximum of possible absurdity. But, on September 28, the *Engineer* undertook to enlighten its readers on the subject of Energy; and the effort resulted in some astounding information.

Speaking of "the equation $E = \frac{Mv^2}{2g}$" (a form which will, perhaps, please my friend Prof. Greenhill, though to me it seems to denote merely the product of a mass by a length) our instructor says:—

"The received idea is that, so long as we get E equal to a given number of foot-pounds, it is of no consequence whether we vary v or vary M, . . . but if we introduce the element Time, *which ought not to be left out* [the italics are mine]. . . . The word Energy is unfortunately very vague. . . ." I shall presently recur to this.

October 5 is not without its little novelty. For, as we are gravely informed,

"That much despised faculty, common-sense, has always told engineers that when a given volume of air is passed through a channel or trunk, its pressure will fall as the trunk augments in dimensions."

Even the despised scientific man knows better than that!:—but, true to his convictions, the *Engineer* ignores the conclusions of such mere "Professors and College men" as Bernoulli, Willis, and Sir W. Thomson.

A week later, commenting on some sensible protests (which it prints, in order to refute (?) them) against its marvellous ideas about Energy, the *Engineer* gets back to something like last year's confusion, and mixes up work and horse-power in a manner truly amazing. This peculiar phase of his own mind is what the *Engineer* designates as the "vagueness" of the word Energy!

But the crowning feat is to come. In a very sensible and able introductory lecture to his engineering class at Leeds, Prof. Barr took occasion to speak of the advantages of preliminary scientific training for engineering students, and protested against the mischievous doctrines too often expressed by so-called "practical men" and their literary champions. He took the opportunity of giving a much-needed warning against the inaccuracies of the *Engineer*; referring (among others) to that already quoted about the failure of the Second Law of Thermodynamics.

He has had his reward! His difference of opinion from the *Engineer* is, of course, to be ascribed to youth, inexperience, want of breadth of knowledge, &c. The *Engineer* goes so far as to suggest such ugly ideas as "disregard for truth"; and, having done so, immediately proceeds to quote me by name as the author of a statement which, with the utmost possible distinctness, I had ascribed to its true author, the late Clerk-Maxwell.

Prof. Barr is quite able to hold his own against such antagonists as the *Engineer*, were they to come, like sorrows, "in battalions":—but what of those many humble but ardent students who have had no scientific instruction, and who have been led by circumstances to rely upon such scientific information as they can pick up from the technical or professional journals, too often the only sources available to them?

That such a question can be asked, and with good reason, in Britain and in this nineteenth century, is a matter for profound humiliation.

How correct is the remark of Paulus Pleydell:—"There are folks before whom one should take care how they play the fool."! The gigantic joke perpetrated in the recent Presidential address to the British Association has been too often taken in earnest, and is already bearing fruit of a very different kind from any that could have been contemplated by the witty author. It is recorded that a practical joker once managed to block up the Strand with gaping idiots, simply by staring at the lion on Northumberland House, and muttering to himself, "It *did* wag." What a comment on this is furnished by the never-to-be-excelled discovery recently made by the *Engineer* that "the Engineer and the Engineer alone is the great civilizer," in whose train "the man of science follows"; with its correlative and complementary theorem "The world owes next to nothing to the man of pure science"!

The *Engineer* may read once more, and (I will hope) with profit, that memoir of Rankine which he has so strangely misquoted. I would commend to his special notice the following lines:—

"(Rankine) did not, indeed, himself design or construct gigantic structures, but he taught, or was the means of teaching, that invaluable class of men to whom the projectors of such works intrust the calculations on which their safety as well as their efficiency mainly depend. For behind the great architect or engineer, and concealed by his portentous form, there is the real worker, without whom failure would be certain. The public knows but little of such men. Not every Von Moltke has his services publicly acknowledged and rewarded by his Imperial employer! But he (*i.e.* the man of pure science) who makes possible the existence of such men confers lasting benefit on his country."

P. G. TAIT.

The Great Modern Perversion of Education.

I THINK Mr. Victor Dickins will admit on second thoughts that he has hardly taken pains enough to slay the dragon that confronts him. In his letter to you he says, "I have shown above that competition does not produce any of the evil results complained of in the protest," but the special—if not the only—point to which he addressed himself was, I think, to show that the great prize-winners carried on their success into after-life. Now, the protest never asserted or implied that many prize-winners did not succeed fairly well in after-life. Could this be asserted, the charge against such examinations would be so overwhelming and so easy of proof, that the hours of their survival would be few to count. What the protest asserted was that from time to time—"fairly often," might perhaps stand as the translation of the words "again and again"—the great promise of the brilliant young man comes to nothing; that is to say, this happens sufficiently often to warn us, even if no other warnings existed, that our system may be injuring instead of benefiting, may be restricting and destroying mental powers instead of enlarging them.

The point, however, is not the most fruitful one to discuss. It occupied but an insignificant position in the protest—I think less than six full lines in a paper amounting to about three hundred—and, as far as I know, is not a point on which any one of the assailants of competitive examinations has laid much stress. And one reason is plain. We should all differ so much as to what is success. If you pointed me out either a lawyer who successfully stated his case, a public man who got up a question in a few days, and at the end of the time embodied his remedy in a popular Bill to be laid before the House, or a journalist who came down to his office and wrote a brilliant article upon both the evil and the remedy, whilst admitting the useful qualities that each possessed, I should not consider that such qualities—however vigorously and effectively displayed—necessarily afforded any justification of a particular course of educational training. The world has need of such qualities; it rewards them liberally; and whether competitive examinations exist or not, such qualities will abound quite sufficiently under our present conditions.

If, on the other hand, it could be shown that Mr. Robert Browning, Lord Tennyson, Mr. Herbert Spencer, Mr. Darwin, Mr. Huxley, Mr. J. S. Mill, Mr. Buckle, the two Stephensons, the Duke of Wellington, and many such another, had been the product of competitive examinations, I should at once admit that the defenders of the system had greatly strengthened their case by showing that such training could—if not produce—at least not destroy some of the higher excellencies of mind. But I am afraid that no list of fairly successful politicians or lawyers or doctors enjoying fair practice will affect the case in a vital way. Success of an ordinary kind indicates certain valuable qualities, but they are not the qualities, I submit, that should indicate what form the higher education should take.

If Mr. Dickins writes to arrest the movement that has begun, there are certain points to which I think he should address himself,—points, that if he can successfully deal with, he will deprive us remonstrants of much public sympathy. We charge competitive examinations with lowering the higher motives that belong to education, and exercising a bad intellectual and moral influence upon both pupil and teacher. Admitting a good side,—admitting that success in examination implies self-denial and perseverance, and probably such qualities as quick perception, readiness, and good memory,—we still say that in presence of these great examinations the student learns much of what he has to learn in the wrong way. He cultivates what has been called the portative memory. He learns so as to forget. He loads himself with an immense quantity of detailed knowledge that no man in practical life desires to possess. He learns so as to make a display of knowledge rather than to be the real master of it. He strains after effect. He gives himself up to calculations and dodges. He studies the question of marks. He is learned in summaries, footnotes, and manuals. He does not follow out for himself the points that arouse his intellectual interest, but he throws himself as much as possible upon skilled guidance. He works under pressure, assimilating but a small part of what he takes in, and looking intently forward to the day of relief. He is without the great ideals that belong to learning. He is not primarily influenced by the desire of cultivating his own faculties, of learning how to know, of understanding the world in which he has to live; but by the desire of obtaining a favourable verdict from the man who holds the scales by which his success or his failure is to be determined It is a highly artificial system, and gives throughout a wrong twist to the student's mind, just like the older system of disputations, which is said to have lowered the sacredness of truth, and to have led men into every shift and wile to disallow their ignorance or the weakness of their cause.

Now, these are general statements, and therefore they apply more to certain kinds of examinations than to others; and to certain characters than to others. It is perfectly true that what Mr. Latham calls art-matter can be tested with less injury to the student than knowledge-matter. That is to say, that you can examine a student more profitably to himself in the arts of playing an instrument, performing a dissection, working a mathematical problem, or translating a language, than you can examine him as regards his knowledge of history, literature, philosophy, or natural science. But, in the first place, he would be a bold man who would propose to fashion education according to the necessities of examination, and only to teach those subjects which lent themselves to examination. In the second place, all arts and all knowledge are so intimately allied, that it is easy to see what a narrowing and stunting influence, as regards intellectual development, there would be in a system that demanded anatomy without physiology, a power of translating a language without other knowledge of the history or literature or genius of such language, that demanded even in music simply a power of execution, and in mathematics simply an unlimited ingenuity in working problems on paper. Let anyone think steadily of such treatment of any of these subjects, and he will, I suspect, escape with difficulty from a sense of nightmare; especially if he think of a whole generation of young minds so manipulated for the sake of the examiners. In the third place, of all the undesirable things to achieve, a generally recognized standard of how to do a thing is the most undesirable. Such a standard you must have, when the examined are brought together from all parts of the kingdom to compete in the same examination; and a better-laid plan for the gradual degradation of an art can scarcely be conceived. We ought by now to have learnt this great truth, that standards which make for uniformity are the greatest enemies of improvement.

As regards the teachers, the effect must be as disastrous as regards the pupils. If the learner cannot learn in the right way, it follows that the teacher cannot teach in the right way. He necessarily becomes an accomplice in the pressure, the hurry, the preparation for a special moment, the skilful handling of a subject so that a sort of examiner's essence may be extracted from it, and nothing more. Just as the pupil must treat many great subjects in an unworthy manner, not giving himself up wholly and devotedly to them, not following out the many questionings which such subjects naturally arouse in minds which have not lost their freshness and originality, not seeking the higher ends,—love of knowledge and the power of understanding this life which we all have to live,—but engrossed in what, from the point of view of self-cultivation, are the lower ends,—the desire of an intellectual triumph, and perhaps of the position which may reward the triumph,—so must the teacher co-operate hand in hand and step by step in all these inferior motives and inferior uses of a great vocation. Indeed, he will be fortunate if after some years of such work he resist the cynical influence which belongs to the system, and do not begin to believe that both young men and their teachers were specially designed for contests in intellectual cockpits, and that in no other way could the young be induced to forego the pleasures and attractions of life at twenty. I have keen recollections of an old keeper, who used greatly to impress my schoolboy's mind by the intense conviction that he had, that a cock would not have known how to use its spurs, a pike would not have been blessed with its appetite for a silvered spoon, and a fox with its scent, if some ulterior intentions had not existed somewhere on behalf of English sportsmen. Had he only understood our educational system, perhaps he would as stoutly have maintained that our young men were born to be examined, and their seniors to examine them.

AUBERON HERBERT.

Old House, Ringwood, November 18.

Mr. Dyer on Physiological Selection.

If the strength of a theory may be measured by the weakness of criticism, I have good reason to be hopeful for the future of "Physiological Selection." On this account I am glad that Mr. Dyer has sought to justify his remarks by giving his reasons for them, although I regret what appears to me the needless asperity of his tone. However, disregarding the personalities in which he has clothed his reply, I will endeavour to show that the reply itself is about as unfortunate as a reply could well be.

Taking his points *seriatim*, I am in no way responsible for the notices of my paper which appeared in the *Times* or in any other periodical, except, of course, those which I have published with my own signature. But, although not responsible for what the newspapers said, I should have corrected any "absurd misrepresentation," had I met with such. The passage, however, which Mr. Dyer now quotes from the *Times* of more than two years ago (and which, I presume, is correctly quoted) does not appear to me a misrepresentation at all. On the contrary, I gather from it that the writer must have perfectly well understood my paper. What he states is that, in my view, natural selection is a theory of the origin of adaptations "*rather*" than a theory of the origin of species. Mr. Dyer appears to regard this as identical with his own statement—viz. that in my view natural selection is not in any sense a theory of species, but "*only* of adaptations." In other words, the former statement correctly imputes to me the opinion that Mr. Darwin's theory is *primarily* a theory of adaptations wherever these occur, and, consequently, *also* a theory of species in every case where species differ from one another in respect of adaptive characters; while the latter affirms unequivocally that in my opinion "specific differences are not adaptive," and, consequently, that Mr. Darwin's theory is a theory of adaptations *to the total exclusion of species*—for an explanation of the origin of which "it follows that we must look to Mr. Romanes himself." Now I must say that if Mr. Dyer cannot see the distinction between these two statements, I may well cease to regret on my own account the difficulty which he says he experiences in understanding my papers.

But although it does not appear that the *Times* misunderstood me in this matter, it is quite true that Mr. Wallace did; and soon after my paper was published, he misrepresented me in exactly the same way as Mr. Dyer misrepresents me now. But I immediately and most emphatically repudiated this astonishing interpretation at the time, in a general answer to criticisms which was published in the *Nineteenth Century* for January 1887. Therefore, whatever Mr. Dyer may think about the reiterated contradiction which I gave in these columns a week or two ago, he is plainly and entirely in the wrong where he refers to it as "a denial that comes rather late in the day." He appears to have adopted Mr. Wallace's interpretation without deeming it worth his while to glance at my reply, before republishing to his audience at Bath a misrepresentation which I had long ago repudiated with all the resources of the English language that I could command.

Why, indeed, any such "denial" on my part should ever have been required has always been to me unintelligible. The original paper itself over and over again insists that I do not at all doubt the important (though, as Darwin says, "not exclusive") part which natural selection has played in the origination of species. Some of these passages I republished in my last letter (October 25), and thus it is for your readers to judge whether the smallest degree of ambiguity attaches to them. But, again ignoring these passages, even as now re-quoted, italicized, and especially addressed to himself, Mr. Dyer seeks to justify what I have now so often had to designate as this "absurd" rendering of my views, by pointing out that in my paper one of the sections is headed "Natural Selection not a Theory of the Origin of Species." This is the only justification that he attempts. Let us see what it is worth.

I readily acknowledge that, to have been quite accurate, the heading of this section ought to have been "Natural Selection not strictly speaking a Theory of the Origin of Species." But I submit that the oversight of here leaving out the words "strictly speaking" (which are elsewhere supplied), was an oversight which could not possibly have misled any reader as to my meaning—unless, of course, he confined himself to reading only the headings of my sections. For it was in the short section thus headed that the very passages occur which I selected from my whole paper to quote in my last letter, as furnishing "direct contradiction" to Mr. Dyer's statement. In other words, following immediately and repeatedly upon the heading in question, there are passages which carefully and unequivocally guard against the very imputation which Mr. Dyer now seeks to force upon me. As a critic of my writings, therefore, he is here trebly in the wrong. First, because his statement of my views admits of being flatly falsified by my original paper itself;[1] secondly, because he ignores all that I have since written upon the same subject; and thirdly, because he now fails to withdraw what I have told him is a travesty of my meaning.

But even this is not all. For Mr. Dyer goes on to say:—"Everybody knows that the idea of evolution of organic Nature existed in men's minds long before Mr. Darwin. He did not originate it; what he did originate was the theory that 'natural selection' is the mechanical means by which that evolution has been brought about. Mr. Romanes says roundly that it is not, or words have ceased to have meaning." Now, if, *without divorcing them from their immediate context*, Mr. Dyer will be considerate enough to point to any words which I have ever writen or ever spoken from which such an interpretation as this can, by any amount of twisting, be extracted, I shall indeed begin to believe that words have so far ceased to convey a meaning of any kind as to be practically useless for purposes of expression. Because I have insisted that, in the great drama of "evolution," natural selection has been *everywhere* the one great agent in the causing of adaptations; because I have said that, on this account, we should take much too narrow a view of so vast an agency were we to regard it as concerned *only* in the origin of "species," or as having to do *only* with such adaptations as happen to be of but specific value; because I have advocated a larger and more correct view of the stupendous importance of this "mechanical means" by which *all* "evolution" in organic Nature has been brought about, with the exception only (as I say in my paper) "of mutual sterility and trivial details of structure, form, and colour," which alone I attribute to the "supplementary factor" of physiological selection,—because I have said all these things, Mr. Dyer now tells me that I have roundly denied the agency of natural selection altogether! After this, I can only feel that

[1] For instance:—"Let me not be misunderstood. In saying that the theory of natural selection is not, properly speaking, a theory of the origin of species, I do not mean to say that the theory has had no part at all in explaining such origin. Any such statement would be in the last degree absurd. What I mean to say is that the theory is one which explains the origin and conservation of adaptations, whether structural or instinctive, and whether these occur in species, genera, families, orders, or classes."

it is hopeless to continue discussion with so extraordinary a disputant. Indeed, as he had published this statement in bald and obvious contradiction to all my writings, nothing more remains to be said: I simply challenge justification as plainly impossible.[1]

The next point in Mr. Dyer's criticism is where he says that if a large proportional number of specific differences are, as I allege, useless, this "would be quite as effective as proving the proposition universally in inflicting a deadly blow on the Darwinian theory, the very essence of which is that specific differences must be advantageous." Now this I deny *in toto.* It is no part of the essence of Mr. Darwin's theory to assume that all specific characters must be advantageous, nor does it even belong to this theory to decide in what proportion as to number advantageous characters stand to indifferent ones. Without going over the ground already traversed with regard to this matter in my previous letter, perhaps it may produce some effect on Mr. Dyer's mind if I quote Mr. Darwin's own opinion upon the subject. After stating what *would* be "absolutely fatal" to his theory, he proceeds (italics here and elsewhere mine), "I fully admit that *many* structures are now of no use to their possessors, and may never have been of any use to their progenitors. . . . *It is scarcely possible to decide* how much allowance ought to be made for such causes of change as the definite action of external conditions, so-called spontaneous variations, and the complex laws of growth; but, *with these important exceptions*, we may conclude that the structure of every living creature either now is, or formerly was, of some direct or indirect use to its possessor" ("Origin of Species," sixth edition, p. 160). Mr. Huxley expresses himself to exactly the same effect in his recently-published obituary notice, where he says that, so far as the theory of natural selection is concerned, a species may present "*any number*" of characters "which are neither advantageous nor disadvantageous, but indifferent, or even slightly disadvantageous" (Proc. R.S., vol. xliv. No. 269, p. xviii.). After all the controversy which I have had upon this subject with Mr. Wallace, I am exceedingly glad to find Mr. Huxley speaking out so "roundly" on the Darwinian side of it. Mr. Dyer, indeed, still objects that he thinks "Mr. Huxley is disposed to make too great concessions." Of course Mr. Dyer is entitled to have his own opinion upon the matter; but I submit he is not entitled to set up this opinion as so authoritative that I am *ipso facto* bound to accept its statement as constituting the very essence of the Darwinian theory. No doubt "with regard to plants" he is "competent to speak"; but he must surely be aware that other botanists who have more thoroughly considered this question are dead against him in his general conclusion. In particular, the late Prof. Nägeli made this subject the matter of a careful inquiry "with regard to plants," the result of which was very materially to influence the judgment of Mr. Darwin.[2]

Next, Mr. Dyer says that because I am not what he calls a "practised naturalist," my "method is the very inverse of that of Mr. Darwin." Now, without at all recognizing Mr. Dyer's right to lecture in this way on the subject of scientific research, I may nevertheless refer him to the history of every other theory which has ever been published with reference to generation, from the "provisional hypothesis" of pangenesis by Darwin himself, through the plastidule of Haeckel and the idio-plasma of Nägeli, to the keim-plasma of Weismann. In all these cases the "method" has been the same as mine—viz. to collate the known facts bearing on the principle suggested, and to leave for future work such experimental verification as may be possible. Moreover, even the theory of natural selection (to which, I suppose, Mr. Dyer more especially alludes) was established by general reasonings from the bringing together of facts already known;[3] and when we remember the much greater importance of this theory, as well as the whole change of thought which it has produced with reference to the general doctrine of evolution, I cannot feel that, relatively speaking, I was over-precipitate in publishing my views on physiological selection.

[1] In another part of his letter Mr. Dyer says that my theory of physiological selection "shrivels up the part played by natural selection to very small dimensions." Here, again, I can only request that some explanation should be given of the process of reasoning whereby my critic has arrived at this most astonishing conclusion

[2] "I now admit, after reading the essay by Nägeli on plants, and the remarks by various authors with respect to animals, more especially those recently made by Prof. Broca, that in earlier editions of my 'Origin of Species' I perhaps attributed too much to the action of natural selection. . . . I did not formerly consider sufficiently the existence of structures, which, as far as we can at present judge, are neither beneficial nor injurious; and this I believe to be one of the greatest oversights as yet detected in my work."—"Descent of Man," second edition, p. 61. See also, for emphatic passages to the same effect in the "Origin of Species," sixth edition, pp. 171, 176, 421.

[3] "Belief in natural selection must at present be grounded entirely on general considerations, ... and chiefly from this view connecting under an intelligible point of view a host of facts."—Letter of Darwin to Bentham, "Life," &c., iii. p. 25.

Further on Mr. Dyer objects to my names for the principle in debate—*i.e.* "physiological selection" and "segregation of the fit"—and says he is "surprised that Mr. Romanes has taken so far no notice" of this objection as originally presented in the *Times*. But here again "the demon of inaccuracy" pursues him. In my general reply to criticisms, already referred to above, and prominently so in these columns at the time, I fully considered this objection; and therefore, if on my side there were still any room left for "surprise," I might have here expressed a certain degree of wonder that before writing the letter which he has now published he should not have taken the trouble to read the author whom he is somewhat intemperately attacking.[1]

In point of fact, however, his attack is everywhere delivered with so complete an absence of judgment, as well as of information on what he is writing about, that it amounts to a mere hitting at random. This, I think, I have now sufficiently proved. Nevertheless, although truly "rather late in the day," I commend to his consideration my article in the *Nineteenth Century* of nearly two years ago, as disposing still more effectually than space will here permit of every one of his general statements and detailed objections. I may add that the bias shown by thus repeating borrowed criticisms, without first consulting my answers, quite deprives his opinion, in my estimation, of the weight to which I might otherwise have felt that it was entitled.

In conclusion, it is useless to conjecture with Mr. Dyer what Mr. Darwin would have thought of physiological selection as a theory; and I have already given my reasons for holding it improbable that he ever considered it (NATURE, vol. xxxiv. p. 545). On the other hand, I should like to remark, that although what he complained of as "the great power of steady misrepresentation"[2] has seriously prejudiced the theory in this country, such has not been the case abroad, where in many quarters it has been received with unqualified favour. This remark—which applies to botanical as well as zoological authority—is added merely in order that the theory may have fair play.

GEORGE J. ROMANES.

Geanies, Ross-shire, N.B., November 4.

P.S.—Proofs of this letter have been accidentally delayed in transmission.—G. J. R., Edinburgh, November 22.

[1] The following is what I said with regard to this criticism in the *Nineteenth Century* for January 1887, and it leaves nothing further to be said now:—"This is a point of no real importance, and I readily concede that in some respects physiological isolation would be a better name than physiological selection. The reasons which inclined me to adopt the latter in preference to the former will be gathered from what has just been said. If the theory is sound at all, a process of true survival takes place, in some cases of the primary [*i.e.* sexual], in other cases of those secondary [*i.e.* morphological] specific characters which are capable of inducing the primary; and in either event it is only certain changes of character, or particular variations, which are *selected* to survive as new species. Moreover, the term physiological selection does not exclude the term physiological isolation, any more than the term natural selection excludes the term survival of the fittest. . . . The 'fitness' of the individuals affected is guaranteed by the fact of their having reached the breeding age. This latter point is important, because Mr. Wallace accuses me of having lost sight of the consideration that my physiological variations must conform to the law of natural selection. . . . If these physiological varieties ever occur at all, *ex hypothesi* they must have so far passed muster with respect to general fitness as to be allowed to propagate their kind. It was for the sake of emphasizing this feature of my theory that I gave the latter the alternative title of 'segregation of the fit.'"

[2] "Origin of Species," p. 421. The whole paragraph, read in connection with the present controversy, is curiously interesting, and to me very consoling. Moreover, the scientific creed which it rehearses is in every particular identical with my own, while differing considerably from that of my critic.

Cleistogamy.

I SHOULD like to add a few words to the extract from the minutes of the Scientific Committee of the Royal Horticultural Society, quoted in NATURE, November 22, p. 86. The causes of the cleistogamous condition of some of the plants mentioned I would attribute to their stunted habit induced by mowing, coupled with a relatively cold season. For, while some of them, *e.g.* *Cerastium*, *Montia*, and *Alchemilla*, rarely open their buds, the *Veronica*, *Sagina*, and *Trifolium*, are more inclined to do so, if allowed to grow more vigorously, and if the temperature be higher. Cleistogamy is of course only a relative quality. Thus chickweed and spurry will open their flowers widely in hot weather; but are cleistogamous and abundantly self-fertile all through autumn and winter, if it be mild. With regard to *Trifolium subterraneum*, as it was about thirty years ago when observed it, I cannot now be certain that it was actually cleistogamous; but it grew with just the same habit as the above, and was most probably self-fertile as they are.

GEORGE HENSLOW.

Nose-Blackening as Preventive of Snow-Blindness.

I BEG to send you an extract from a letter just received from my son, of the Indian Geological Survey Department, and who is at present engaged by the Maharajah of Kashmir in exploring and reporting on his sapphire mines. Since it refers to former communications in NATURE (vol. xxxviii. pp. 7 and 101), upon a subject of interest to travellers, it may be of use.

I may here mention that my son speaks of having found the Eocene Nummulitic limestone in Zanskar at a height of 18,500 feet above the sea. Sir J. D. Hooker tells me that he has previously observed the Nummulites in Tibet, at a height of 18,000 feet.

J. D. LA TOUCHE.

Stokesay, Craven Arms, November 20.

"Some time ago there was a letter in NATURE describing a method of protecting the eyes from sun-glare, when crossing snow, by blackening the nose and cheeks under the eyes. I tried the dodge the other day, when I was crossing the snow-fields and glaciers from Zanskar, and found it very successful. My shikari and some of the other natives were much amused when I produced a piece of charcoal, and proceeded to blacken my face; but they also tried it, and said that it relieved them very much. I do not know how the effect is produced, but it was much the same as when one went off the snow on to a patch of moraine or rocks clear of snow. The blackening seemed to stop the reflected rays in some way. The natives expressed the feeling by saying that it cooled their faces. I found it quite possible to walk over the snow for many miles without glasses, which are a nuisance, especially on rough ground; but without the blackening I had to put them on. The sun at these high altitudes has much greater effect than in England when the ground is covered with snow."

Amber.

IN NATURE (vol. xxxvi. p. 63), I find the following note:—"The largest piece of amber ever discovered was recently dug up near the Nobi's Gate, at Altona. It weighed 850 grammes." I beg to state that a piece of amber, weighing 5·6 kilogrammes, is in the possession of Messrs. Stantien and Becker, in Königsberg, and that pieces weighing 6·5 and 9·5 kilogrammes can be seen in the Berlin Mineralogical Museum, both discovered off the sea coast of North Germany. Even as far inland as Silesia, a piece of Baltic amber, weighing 3 kilogrammes, has been found in the bed of the River Oder, near Breslau. Baltic amber occurs in Silesia also as high as 1400 feet above the level of the sea.

A. B. MEYER.

Royal Museum, Dresden, November 19.

ON THE MECHANICAL CONDITIONS OF A SWARM OF METEORITES.[1]

II.

THE next point to consider is the mass and size which must be attributed to the meteorites.

The few samples which have been found on the earth prove that no great error can be committed if the average density of a meteorite be taken as a little less than that of iron, and I accordingly suppose their density to be six times that of water.

Undoubtedly in a meteor-swarm all sizes co-exist (a supposition considered hereafter); for even if originally of uniform size they would, by subsequent fracture, be rendered diverse. But in the first consideration of the problem they have been treated as of uniform size; and, as actual sizes are nearly unknown, results are given for meteorites weighing $3\frac{1}{8}$ grammes. From these, the values for other masses are easily derivable. It is known that meteorites are actually of irregular and angular shapes, but certainly no material error can be incurred when we treat them as being spheres.

The object of all these investigations is to apply the formulæ to a concrete example. The mass of the system is therefore taken as equal to that of the sun, and the limit of the swarm at any arbitrary distance from the present sun's centre. The theory is of course more severely tested the wider the dispersion of the swarm, and accordingly in a numerical example the outside limit of the solar swarm is taken at $44\frac{1}{2}$ times the earth's distance from the sun, or further beyond the planet Neptune than Saturn is from the sun. This assumption makes the limit of the isothermal sphere at a distance 16, about half-way between Saturn and Uranus.

In this case the mean velocity of the meteorites in the isothermal sphere is $5\frac{1}{3}$ kilometres per second, being $\sqrt{\frac{6}{5}}$ of the linear velocity of a planet revolving about a central body with a mass equal to 46 per cent. of that of the sun, at distance 16. In the adiabatic layer it diminishes to zero at distance $44\frac{1}{2}$. This velocity is independent of the size of the meteorites. The mean free path between collisions ranges from 42,000 kilometres at the centre, to 1,300,000 kilometres at radius 16, and to infinity at radius $44\frac{1}{2}$. The mean interval between collisions ranges from a tenth of a day at the centre, to three days at radius 16, and to infinity at radius $44\frac{1}{2}$. The criterion of applicability of hydrodynamics ranges from $\frac{1}{60000}$ at the distance of the asteroids, to $\frac{1}{3600}$ at radius 16, and to infinity at radius $44\frac{1}{2}$.

All these quantities are ten times as great for meteorites of $3\frac{1}{8}$ kilogrammes, and a hundred times as great for meteorites of $3\frac{1}{8}$ tonnes.

From a consideration of the tables in the paper it appears that, with meteorites of $3\frac{1}{8}$ kilogrammes, the collisions are sufficiently frequent even beyond the orbit of Neptune to allow the kinetic theory to be applicable in the sense explained. But if the meteorites weigh $3\frac{1}{8}$ tonnes, the criterion ceases to be very small at about distance 24; and if they weigh 3125 tonnes, it ceases to be very small at about the orbit of Jupiter. It may be concluded then that, as far as frequency of collision is concerned, the hydrodynamical treatment of a swarm of meteorites is justifiable.

Although the numerical results are necessarily affected by the conjectural values of the mass and density of the meteorites, yet it was impossible to arrive at any conclusion whatever as to the validity of the theory without numerical values, and such a discussion as the above was therefore necessary.

I now pass on to consider some results of this view of a swarm of meteorites, and to consider the justifiability of the assumption of an isothermal-adiabatic arrangement of density.

With regard to the uniformity of distribution of kinetic energy in the isothermal sphere, it is important to ask whether or not sufficient time can have elapsed in the history of the system to allow of the equalization by diffusion.

It is shown therefore in the paper that in the case of the numerical example primitive inequalities of kinetic energy would, in a few thousand years, be sensibly equalized over a distance some ten times as great as our distance from the sun. This result, then, goes to show that we are justified in assuming an isothermal sphere as the centre of the swarm. As, however, the swarm contracts, the rate of diffusion diminishes as the inverse $\frac{5}{2}$ power of its linear dimensions, whilst the rate of generation of inequalities of distribution of kinetic energy, through the imperfect elasticity of the meteorites, increases. Hence, in a late stage of the swarm, inequalities of kinetic energy would be set up, there would be a tendency to the production of convective currents, and

[1] Abstract of a Paper read before the Royal Society on November 15 by Prof. G. H. Darwin, F.R.S. Continued from p. 83.

thus the whole swarm would probably settle down to the condition of convective equilibrium throughout.

It may be conjectured, then, that the best hypothesis in the early stages of the swarm is the isothermal-adiabatic arrangement, and later an adiabatic sphere. It has not seemed worth while to discuss this latter hypothesis in detail at present.

The same investigation also gives the coefficient of viscosity of the quasi-gas, and shows that it is so great that the meteor-swarm must, if rotating, revolve nearly without relative motion of its parts, other than the motion of agitation. But as the viscosity diminishes when the swarm contracts, this would probably not be true in the later stages of the history, and the central portion would probably rotate more rapidly than the outside. It forms, however, no part of the scope of this paper to consider the rotation of the system.

The rate of loss of kinetic energy through imperfect elasticity is next considered, and it appears that the rate, estimated per unit time and volume, must vary directly as the square of the qaasi-pressure, and inversely as the mean velocity of agitation. Since the kinetic energy lost is taken up in volatilizing solid matter, it follows that the heat generated must follow the same law. The mean temperature of the gases generated in any part of the swarm depends on a great variety of circumstances, but it seems probable that its variation would be according to some law of the same kind. Thus, if the spectroscope enables us to form an idea of the temperature in various parts of a nebula, we shall at the same time obtain some idea of the distribution of density.

It has been assumed that the outer portion of the swarm is in convective equilibrium, and therefore there is a definite limit beyond which it cannot extend. Now a medium can only be said to be in convective equilibrium when it obeys the laws of gases, and the applicability of those laws depends on the frequency of collisions. But at the boundary of the adiabatic layer the velocity of agitation vanishes, and collisions become infinitely rare. These two propositions are mutually destructive of one another, and it is impossible to push the conception of convective equilibrium to its logical conclusion. There must, in fact, be some degree of rarity of density and of collisions at which the statistical treatment of the medium breaks down.

I have sought to obtain some representation of the state of things by supposing that collisions never occur beyond a certain distance from the centre of the swarm

Then from every point of the surface of the sphere, which limits the region of collisions, a fountain of meteorites is shot out, in all azimuths and at all inclinations to the vertical, and with velocities grouped about a mean according to the law of error. These meteorites ascend to various heights, without collision, and, in falling back on to the limiting sphere, cannonade its surface, so as to counterbalance the hydrostatic pressure at the limiting sphere.

The distribution in space of the meteorites thus shot out is investigated in the paper, and it is found that near the limiting sphere the decrease in density is somewhat more rapid than the decrease corresponding to convective equilibrium.

But at more remote distances the decrease is less rapid, and the density ultimately tends to vary inversely as the square of the distance from the centre.

It is clear that according to this hypothesis the mass of the system is infinite in a mathematical sense; for the existence of meteorites with nearly parabolic and hyperbolic orbits necessitates an infinite number, if the loss of the system shall be made good by the supply.[1]

But if we consider the subject from a physical point of view, this conclusion appears unobjectionable. The ejection of molecules with exceptionally high velocities from the surface of a liquid is called evaporation, and the absorption of others is called condensation. The general history of a swarm, as sketched at the beginning, may be put in different words, for we may say that at first a swarm gains by condensation, that condensation and evaporation balance, and finally that evaporation gains the day.

If the hypothesis of convective equilibrium be pushed to its logical conclusion, we reach a definite limit to the swarm, whereas if collisions be entirely annulled the density goes on decreasing inversely as the square of the distance. The truth must clearly lie between these two hypotheses. It is thus certain that even the small amount of evaporation shown by the formulæ derived from the hypothesis of no collision must be in excess of the truth; and it may be that there are enough waifs and strays in space ejected from other systems to make good the loss. Whether or not the compensation is perfect, a swarm of meteorites would pursue its evolution without being sensibly affected by a slow evaporation.

Up to this point the meteorites have been considered as of uniform size, but it will be well to examine the more truthful hypothesis that they are of all sizes, grouped about a mean according to a law of error.

It appears, from the investigation in the paper, that the larger stones move slower, the smaller ones faster, and the law is that the mean kinetic energy is the same for all sizes. It is proved that the mean path between collisions is shorter in the proportion of 7 to 11, and the mean frequency of collision greater in the proportion of 4 to 3, than if the meteorites were of uniform mass equal to the mean. Hence the numerical results found for meteorites of uniform size are applicable to non-uniform meteorites of a mean mass about a third greater than the uniform mass; for example, the results for uniform meteorites of $3\frac{1}{3}$ tonnes apply to non-uniform ones of mean mass a little over 4 tonnes.

The means here spoken of refer to all sizes grouped together, but there is a separate mean free path and mean frequency appropriate to each size. These are investigated in the paper, and their values illustrated in a figure. It appears that collisions become infinitely frequent for the infinitely small ones, because of their infinite velocity, and again infinitely frequent for the infinitely large ones, because of their infinite size. There is a minimum frequency of collision for a certain size, a little less in radius than the mean radius, and considerably less in mass than the mean mass.

For infinitely small meteorites the mean free path reaches a finite limit, equal to about four times the grand mean free path; and for infinitely large ones, the mean free path becomes infinitely short. It must be borne in mind that there are infinitely few of the infinitely large and infinitely small meteorites. Variety of size does not then, so far, materially affect the results.

But a difference arises when we come to consider the different parts of the swarm. The larger meteorites, moving with smaller velocities, form a quasi-gas of less elasticity than do the smaller ones. Hence the larger meteorites are more condensed towards the centre than are the smaller ones, or the large ones have a tendency to fall down, whilst the small ones have a tendency to rise. Accordingly, the various kinds are to some extent sorted according to size.

An investigation is made in the paper of the mean mass of meteorites at various distances from the centre, both inside and outside of the isothermal sphere, and a figure illustrates the law of diminution of mean mass.

It is also clear that the loss of the system through evaporation must fall more heavily on the small meteorites than on the large ones.

After the foregoing summary, it will be well to briefly

[1] It must also be borne in mind that the very high velocities which occur occasionally in a medium with perfectly elastic molecules, must happen with great rarity amongst meteorites An impact of such violence that it *ought* to generate a hyperbolic velocity will probably merely cause fracture.

recapitulate the principal physical conclusions which seem to be legitimately deducible from the whole investigation; in this recapitulation qualifications must necessarily be omitted or stated with great brevity.

When two meteorites are in collision, they are virtually highly elastic, although ordinary elasticity must be nearly inoperative.

A swarm of meteorites is analogous with a gas, and the laws governing gases may be applied to the discussion of its mechanical properties. This is true of the swarm from which the sun was formed, when it extended beyond the orbit of the planet Neptune.

When the swarm was very widely dispersed, the arrangement of density and of velocity of agitation of the meteorites was that of an isothermal-adiabatic sphere. Later in its history, when the swarm had contracted, it was probably throughout in convective equilibrium.

The actual mean velocity of the meteorites is determinable in a swarm of given mass, when expanded to a given extent.

The total energy of agitation in an isothermal-adiabatic sphere is half the potential energy lost in the concentration from a condition of infinite dispersion.

The half of the potential energy lost, which does not reappear as kinetic energy of agitation, is expended in volatilizing solid matter, and heating the gases produced on the impact of meteorites. The heat so generated is gradually lost by radiation.

The amount of heat generated per unit time and volume varies as the square of the quasi-hydrostatic pressure, and inversely as the mean velocity of agitation. The temperature of the gases volatilized probably varies by some law of the same nature.

The path of a meteorite is approximately straight, except when abruptly deflected by a collision with another. This ceases to be true at the outskirts of the swarm, where the collisions have become rare. The meteorites here describe orbits under gravity which are approximately elliptic, parabolic, and hyperbolic.

In this fringe to the swarm the distribution of density ceases to be that of a gas under gravity; and as we recede from the centre the density at first decreases more rapidly, and afterwards less rapidly than if the medium were a gas.

Throughout all the stages of its history there is a sort of evaporation by which the swarm very slowly loses in mass, but this loss is more or less counterbalanced by condensation. In the early stages the gain by condensation outbalances the loss by evaporation; they then equilibrate, and finally the evaporation may be greater than condensation.

Throughout the swarm the various meteorites are to some extent sorted according to size; as we recede from the centre the number of small ones preponderates more and more, and thus the mean mass continually diminishes with increasing distance. The loss by evaporation falls principally on the small meteorites.

A meteor swarm is subject to gaseous viscosity, which is greater the more widely diffused is the swarm. In consequence of this a widely extended swarm, if in rotation, will revolve like a rigid body without relative motion (other than agitation) of its parts.

Later in the history the viscosity will probably not suffice to secure uniformity of rotation, and the central portion will revolve more rapidly than the outside.

The kinetic theory of meteorites may be held to present a fair approximation to the truth in the earlier stages of the evolution of the system. But later, the majority of the meteors will have been absorbed by the central sun and its attendant planets, and amongst the meteors which remain free the relative motion of agitation must have been largely diminished. These free meteorites—the dust and refuse of the system—probably move in clouds, but with so little remaining motion of agitation that (except perhaps near the perihelion of very eccentric orbits) it would scarcely be permissible to treat the cloud as in any respect possessing the mechanical properties of a gas.

The value of this whole investigation will appear very different to different minds. To some it will stand condemned as altogether too speculative; others may think that it is better to risk error in the chance of winning truth. To me, at least, it appears that this line of thought flows in a true channel; that it may help to give a meaning to the observations of the spectroscopist; and that many interesting problems, here barely alluded to, may perhaps be solved with sufficient completeness to throw light on the evolution of nebulæ and planetary systems.

EDISON'S PERFECTED PHONOGRAPH.

THE marvellous results attained by Mr. Edison's recent improvement on, or, more properly perhaps, resurrection of, the original phonograph of 1878 have induced us to present a view of the latest form of the instrument, together with a short description of its main features and most recent performances.

Mr. Edison is still occupied in perfecting the instrument, and scarcely a week passes without his sending over to his European colleague, Colonel Gouraud, substantial evidences of progress towards perfecting the arrangements either for the recording and reproducing of all kinds of sounds, or else in the construction and the postal conveyance of phonograms.

Although, therefore, the instrument can hardly at present be said to have reached its final stage of development, in its chief constructive points it may be regarded as practically perfected; while some recent trials of it show that it is capable not merely of recording, but of reproducing, every kind of sound with which we are acquainted, including articulate speech, with a fidelity little short of absolute perfection.

When Léon Scott invented his phonautograph, he unconsciously came near the phonograph, though he merely contented himself with reproducing the vibrations pictorially on a blackened surface. Prof. Helmholtz, on the other hand, by his profound studies in the analysis and synthesis of speech into fundamentals, accompanied by varying combinations of subsidiary harmonics, seems to have created quite a scare among the phonographists, by showing them what a terribly complicated affair articulate speech was. In the phonograph, however, we have a machine which not only differentiates all these complicated systems of vibrations, checks, and harmonics, but integrates them equally well. It is, moreover, capable of repeating the integrations practically as often as we please.

This perfected power of record, reproduction, and preservation of sound has been accomplished partly through the substitution of a specially prepared wax for the original tinfoil, but also a good deal through other improvements in the diaphragms, needles, &c. The vibrations of the recording diaphragm are transferred by means of a cutting-needle to the wax, which is thus carved and indented into a series of hills and valleys, which represent in *intaglio* the resultant form of the original sound-vibrations, including part, if not all, of the minor inflections due to the presence of the subsidiary harmonics or overtones.

The tinfoil used in the original machine of 1878 only very partially fulfilled the office of a recording surface, and since every indentation in it necessarily involved a corresponding rise of the material on either side, the vibrations of the recording style, and *a fortiori* of the air itself, were only very imperfectly reproduced on its surface. The hollow character of the undulations, moreover, caused them to be easily effaced after a few repetitions.

The records on the wax, on the other hand, have been recently reproduced over 3000 times.

As a good many of the specifications describing the new improvements are not yet made public, we are not at liberty to describe minutely the various parts of the instrument. We can, however, give the following outline of its leading features:—

(1) There is a brass cylinder on which the wax phonogram is placed.

(2) A rocking holding arm, which carries what is termed the "spectacle," containing the recording and reproducing diaphragms, and which by means of a traveller arm is made to engage two rotating screws—one called the feeding screw, very finely threaded, which causes it to travel slowly from left to right over the rapidly rotating phonogram, and thus trace out a long spiral on its surface; the other, a coarser reversely-threaded screw, which enables it to be moved back more rapidly opposite any required position on the phonogram. By means of a screw-head attached to a turning bar with an arrangement of cams, the rocking holding arm can be made to engage either the feeding or reversing screw, or else lift the diaphragm and its recording or reproducing needle entirely off the surface of the phonogram. These back and forward movements can be preferably made by a treadle attached to either the turning bar or rocking arm, leaving the hands free for other purposes.

(3) Another very practically useful adjunct consists of a projecting arm attached to the turning bar, by which the operator when desiring to stop and think what he is going to say next can completely disengage the diaphragm and and its accompanying recording needle from the surface

Showing reproducing diaphragm in pos'tion, the operator listening through the tubes, and standing behind the instrument so as to allow it to be seen

of the wax; and if the pause is a long one, and he does not desire to waste his phonogram, he can reverse the movement of the diaphragm until it is opposite the point on the phonogram where he left off. By combinations of arrangements of this sort, all the actions of an ordinary writer, stopping to think, looking over, and even correcting what he has written, can be imitated.

(4) The arrangement by which a phonogram can be removed and another replaced, simply consists in having one of the centres on which the brass cylinder turns, attached to a movable arm.

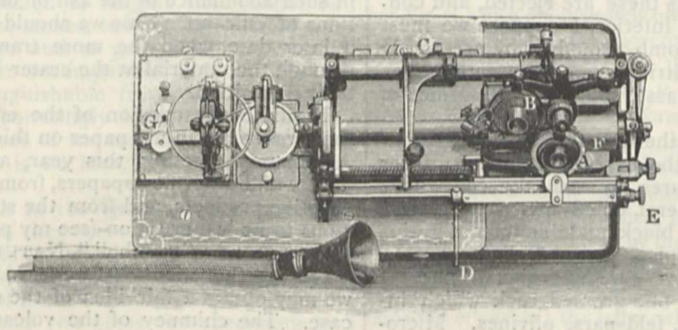
(5) Though, theoretically, one diaphragm could effect both record and reproduction, it is found that the same shape of needle or style is unsuited for both purposes. Consequently, there are two diaphragms, one a recorder and the other a reproducer.

The style of the recording diaphragm is made of such a shape as to be more of the nature of a graving tool, while that attached to the reproducing diaphragm is inclined at an angle to the surface of the wax, and glides over its indentations without destroying them. In front of the recording style is placed the cutting-out tool, whose function is somewhat analogous to the dibble of the gardener. It not only prepares a furrow in the wax in which the record can be made, but it also destroys any previous record. So that the making of a fresh record and the effacing of a previous one can proceed simultaneously.

The use of tubes to the ear raises a point about which a good deal of misapprehension appears to exist. The machine at present is not intended for reproducing sounds with their original intensity, but rather for the perfect reproduction of articulate speech and music. Many persons, therefore, on hearing the reproduction through the magnifying funnel, are disappointed to find the effect below their expectations. As soon, however, as they listen through the tubes, they are proportionately surprised at the loudness and the clearness of the sound and articulation. While for most practical purposes audition through the tubes is quite sufficient, Mr. Edison is, we understand, constructing a means by which the sounds can be greatly magnified. Even as it is, with the present funnel the reproduction can be heard very well throughout a large room. For example, at a lecture on November 10, before Harrow School, a perfect *mélange* of speaking, singing, and whistling, made by Colonel Gouraud on the spot, was plainly heard all over the lecture theatre, in which about 600 persons were present.

Other improvements comprise an electric motor and speed governor, by which the phonogram and the feeding screws can be rotated at a constant speed. As this electric motor is itself the subject of a separate patent, we are only at liberty to say that it is an electro-dynamic multipolar motor, in which a ring armature acts as a fly-wheel, and that it is adjustable to different speeds—a necessary point in order to preserve the same pitch where the rapidity of utterance is subject to variation. The circuiting of the motor and governor is ingeniously arranged so that the field of the armature can be opened without interfering with the field-magnet circuit, thus securing greater sensitiveness and an absence of sparking.

The phonograms themselves are divided into two sorts, office-grams and mailing-grams. The former are cylinders, capable at the present time of yielding from thirty to fifty surfaces for record, which number, as Mr. Edison says in a letter only received a few days ago, can now, by improved methods, be increased to two hundred. Obviously, however, such a cylinder would be an awkward affair to send by post. Mr. Edison has therefore



Instrument turned over so as to present a top view with recording diaphragm in position. A, recording diaphragm; B, reproducing diaphragm; C, rocking holding arm; D, bar for arresting record; E, turning bar; F, wax cylindrical phonogram; G, electric speed governor.

met this want by constructing the mailing-gram, and though this may seem a small matter from a scientific point of view, yet we venture to prophesy that among all his many achievements there will be none to which he will look back with greater pride, or which are destined to work a greater revolution in the history of the world than this apparently simple little mailing-gram. To say that it is capable of being posted, and reproduced at the other end without injury to the record, may perhaps give some idea of its practical value.

Regarding the way in which all this is accomplished, it is needless to say anything, except that the device bears the true stamp of genius, viz. simplicity. This portability of the phonograms is, in fact, one of the salient features by which the phonograph of 1888 stands out in marked contrast to the imperfect machine of 1878, and this improvement, in combination with the greater perfection and permanence of the record, at once raises it from the level of a pretty scientific toy or curiosity to one of immediate utility.

The practical working of the instrument, which has been greatly improved even upon what it was at the meeting of the British Association at Bath this year, may be gathered from the fact that Colonel Gouraud dictates all his correspondence through it, speaks to it in different languages, applies every conceivable test to try its powers, and with results which not only astonish him and everybody else, but even the inventor himself.

The purposes to which such an instrument can be applied—scientific, commercial, domestic, artistic, military—seem countless. The dreams which were indulged in when the phonograph of 1878 appeared, can now be realized; and we owe to Mr. Edison another substantial addition to the long list of direct results of scientific labour achieved during the present century.

FURTHER NOTES ON THE LATE ERUPTION AT VULCANO ISLAND.

MY friend Signor Gaetano Platania, who accompanied me on my trip through the Lipari Islands in June 1887, and stayed some days with me at Vulcano, has undertaken the task of describing that interesting event and the subsequent phenomena. He has very kindly forwarded me specimens of the ejectamenta, and to him I must express my thanks. He being already well acquainted with the products of that volcano, his observations will be of considerable value when published.

The first specimen submitted to me is that of the so-called bombs, common in other eruptions that have taken place from the present active crater of Vulcano. It is undoubtedly the *essential* ejectamenta, although included in the paste is much fragmentary *accessory* material. These so-called bombs are irregular polygonal masses of an obsidian-like material on the outside; the surfaces are traversed by a number of clefts or fissures V-shaped in section, which at their bottom and the deeper parts of their sides are seen to be composed of a spongy glass or even pumice. Their mode of formation is no doubt as follows:—The glassy magma from former loss of heat has become so viscous that the escape of vapours from the underlying magma is arrested until the tension rises, and the superincumbent pasty, almost solid, mass is broken up and ejected. This ejection has been preceded by some expansion and cracking, together with some cooling along the cracks, so that the blocks have partly consolidated as pumiceous obsidian, but when relieved from these conditions by ejection, the hotter material within each block expands, due to the liberation, as vapour, of the dissolved H_2O, and the formation of a vesicular structure, which may progress to such a point as to constitute a true pumice. This is accompanied by fissuring of the external hardened surface, just as the expansion of dough splits open the crust, as the air-bubbles expand before and after the loaf is in the oven—in fact, we could not adopt a better term to define this structure than *bread-crust structure*. These fissures often divide crystals, pieces of included rock, &c., showing that little plasticity was left on the surface when this expansion took place. In fact, the conditions necessary are that the glass be sufficiently cold to break with a strain or blow applied sharply, but to bend when the force employed is gradual in its action, such as may be seen well in all vitreous substances. If we warm a stick of sealing-wax, hardened Canada balsam, &c., we may gently bend it to any form, but if our attempt is too quick, the stick breaks. In these bombs many of the surfaces appear to have first bulged and then broken. As these are ejected, and consist of a hard crust and soft interior, I suppose we must use that unfortunate term bomb, though they are rarely round and certainly do not strike one as resembling as much a bomb as do those masses found on the surface of lava-streams.

The colour is buffish-gray, the surface somewhat glistening and scattered over by the exposed broken surfaces (split by the division fractures between the contiguous blocks) of a dark green mineral, chiefly pyroxene, glassy crystals of feldspar, smaller black metallic lustred grains of magnetite, more rarely typical grains of olivine, quartz, and pyrites. There are also very many grains of different sizes of a darker-coloured fine-grained rock which incloses many of the augites, feldspars, olivines. Microscopically, the vesicular structure is seen to extend, though becoming less marked, to within (in the specimen examined) less than a millimetre of the surface, though in larger blocks from other eruptions preserved in my collection this may attain 2 centimetres or more. The crystals of pyroxene are usually well formed, though often broken. They include, wholly or partly, large, rather irregular magnetites, and in some cases are surrounded by wreaths of either sanidine or, more commonly, triclinic feldspar, probably labradorite. Where included in foreign rock-fragments, this latter is seen to be composed of a network of magnetite, augite, and feldspar microliths, and is often much altered. The feldspars are principally sanidines which may attain half a centimetre long; they are very dirty from inclusions, and somewhat rounded. There are also groups of labradorite crystals, and another triclinic feldspar in which the striations are remarkably close and fine. In some cases a triclinic feldspar seems to be intergrown with the sanidine. Of the latter mineral there are many microcrystals and microliths. Here and there are to be met with a few ill-formed crystals of dark-green amphibole.

What part of these minerals belong to the essential magma, and what are simply imperfectly fused out of the surrounding rock, it is extremely difficult to determine, and chemical analysis of the rock would be obviously useless, on account of the numerous inclusions of other rock-fragments. The association of such basic minerals with a distinctly acid rock would be very remarkable, were it not for the distinct origin of them by inclusion of accessory materials. The eruptive rocks of this island range from a very rich olivine basalt through a dolerite to the typical obsidians and spherulites. There is little doubt that these included minerals are the churned-up fragments in the crater apex which almost certainly cuts through those older rocks, and even part of the present active cone of Vulcano is composed of dolerite.

That these bombs are the primary ejectamenta in this eruption there is no doubt, on account of their freshness and the sharp uneroded angles and edges, as observed by Signor Platania, together with the absence of any solfatarizing. The specimen examined was ejected during the month of August, probably early in that month.

The next specimen is dated August 18, and consists of coarse sand or fine lapilli, about the size of a mustard-seed, with a little fine gray ash. This I made into an artificial breccia, and cut sections of it. It is composed of broken fragments of dolerite and glassy rocks, both often solfatarized, with chips of pyroxene, magnetite, &c., and, no doubt, is chiefly *accessory* ejectamenta derived from the crumbling sides of the crater being churned, ground up, and ejected.

Next is a fine ash of light gray colour, ejected on August 26, which is, in great part, also composed of similar materials to the last, with an abundance of very minute microliths, many of a dark-green colour, and therefore probably pyroxene or amphibole, though they remain dark between crossed nicols, from their great minuteness. I have observed no trace of tridymite found in such abundance in the ash of one of the recent eruptions of Vulcano. This we should expect to be formed at a later date, when the more tranquil vapours, escaping through the material at the crater bottom, would allow of their deposition.

From the description of the eruption by Mr. Narlian that was given in my paper on this subject at the British Association meeting this year, and reproduced in the *Times* and other newspapers, from the examination of the eruptive products, and from the state of the volcano previous to its last eruption (see my paper, "The Islands of Vulcano and Stromboli," NATURE, vol. xxxviii. p. 13) taken together with what we learn from the ejectamenta, we may obtain a fair idea of the eruptive process in this case. The chimney of the volcano was, no doubt, filled by an acid magma, which, perhaps, after the last eruption, was of much higher temperature, and in which fragments of other rocks from the crater and chimney sides had been churned up and partially fused. The temperature and liquidity seem to have been low, as the olivines and augites, although they have apparently been fused out of their original matrix, especially the latter, retain most perfectly their crystalline angles, and no chemical fluxing or reaction seems to have occurred between the basic minerals and the surrounding acid magma. Also, the occurrence of pyrites points in a similar manner to the same physical state. The choking of the crater after the former eruption, together with the gradual cooling of the upper part of the magma column during the intermediate solfataric stage of the volcano, would result in a gradually increasing obstacle to the boiling-off of the H_2O dissolved by the magma lower down. Two processes would therefore be going on, viz. increased superincumbent pressure, and augmenting tension of the part of the subjacent magma within reach of water-supply. The latter must obviously, after a certain time, increase in a greater ratio than the former, until the plug is blown asunder.

This plug in great part would consist of the magma with its inclusions reduced to that critical state between a liquid and solid, as seen in vitreous bodies. When this is broken up by the sudden impulse of the expansion of the subjacent aquiferous magma, it would split into fragments; and, these immediately cooling on their surface by the molecular formation and escape of vapour near that surface, cooling and solidification would result, but before this extended far in, the hotter interior would undergo frothing, and so bend, crack, and fissure the nearly hard coating, producing in this manner the bread-crust structure. These blocks seem from Mr. Narlian's account, to have fallen nearly red hot, as his children's feet were burnt, and part of the house where they fell was burnt. After the first explosion, a series of feebler explosions took place, and, I believe, are still continuing with diminished force, just as is seen in boiling up an extremely thick syrup. Add to this the crumbling in of the crater-sides, their pulverization and ejection, and we have the picture of a typical paroxysmal eruption, tending towards an explosive one, of an obsidian volcano. H. J. JOHNSTON-LAVIS.

Naples, November 3.

Since writing the above, I have received the following letter from Mr. Narlian, which will form a fitting appendix to his former one read at the British Association, and published in the *Times* and elsewhere.

"*Lipari, Italy, November* 3, 1888.

"MY DEAR DR. JOHNSTON-LAVIS,—I have your kind note, for which I thank you. Our crater (*i.e.* Vulcano) is still in a very active condition. The eruptions succeed each other nearly every minute or two. Columns of thick black ashes are ejected to heights that cannot be less than 15,000 feet. The stones, red hot, are also thrown out in immense quantities and to great heights. Sometimes these eruptions are accompanied by loud detonations, which are indistinguishable from those of a gun, only they are so overpoweringly loud that at Lipari they are heard as if a piece of 100 tons had gone off near at hand. Till now there is no lava, and we hope there will not be any.

"I observe a difference in the ejected matter: in the beginning of the eruption they were stones, in time they began to show a burned calcined appearance, became quite black and friable by the action of the fire, and now they are nearly pumice of a dark and rough kind.

"I shall be glad to send you some few specimens by the first boat for Naples.

"I am, dear Sir,

"Yours faithfully,

"A. E. NARLIAN."

This prolonged activity is a most interesting phenomenon, and two explanations are open to us—viz. either the supply of igneous magma has increased, and the volcano is passing from the solfataric stage to a strombolian or Vesuvian phase, or the supply of dissolved H_2O in the magma extends to great depths, or is derived from a very large mass of magma. The change in the ejectamenta would rather point to the latter; as if, the first boiled paste being ejected, the more aquiferous paste from greater depths was undergoing discharge of its vapour. This may possibly be followed by the eventual outpour of lava, indicating the arrival at the surface of still deeper magma, comparatively poor in dissolved H_2O, so that the view of an obsidian stream may be in store for us before long—an event of considerable importance to vulcanological science. H. J. JOHNSTON-LAVIS.

NOTES.

WE lately noted that Mr. J. F. Duthie, Director of the Botanical Department, Northern India, had accompanied the recent military expedition to the Black Mountain country. The Black Mountain forms the northern boundary of the district of Hazara, which forms a long narrow valley, bounded on the west by Cashmir. Extending far into the heart of the outer Himalayan range, it is shut in on either side by mountains, rising to 17,000 feet. The flora is almost wholly unknown. But the time of year was unfavourable for botanical collecting, and Mr. Duthie writes to Kew: "I did not manage to find much of botanical interest on the Black Mountain; excepting the fine bits of forest, composed of *Abies Webbiana* and *Pinus excelsa* on the crests of the mountain, the country is barren in the extreme."

THE Kew Museum has lately received a choice collection of interesting objects from Corea, collected and brought home by Mr. T. Watters, who was Acting Consul in that country from January 1887 to June last. The specimens in question, which consist of hand-screens, fans, &c., made of paper from the paper mulberry (*Broussonetia papyrifera*, Vent.), together with samples of the paper itself, sun-blinds made of split bamboo, &c., illustrate in a remarkable degree the extreme neatness and accuracy of the Coreans in their handicrafts. The following are some of the specimens received and now exhibited in the Kew Museum. A series of different qualities of paper, all made from the bark of the paper mulberry. These comprise plain white or cream-coloured papers of various degrees of finish, used for drawing, writing, packing, &c.; also coloured papers such as are used by the people for writing birthday missives upon. It would seem that the Coreans, like the Japanese, use paper very extensively for a great variety of purposes. Thus, for fans, the handles of which are delicately ornamented, as well as for hand-screens, tobacco-pouches, coverings for hats in wet weather, paper is equally applicable; for the latter purposes, however, it is steeped in oil, which makes it thoroughly waterproof. The hand-screens are made by first forming a foundation of thin strips of split bamboo radiating from the handle, which is afterwards covered so completely on both sides with a thin paper film and varnished that a strong and durable article is the result. Some of the hand-screens presented by Mr. Watters to the Kew Museum were given to him by the King, and are of much finer workmanship than those that are purchasable. The oil-steeped paper tobacco-pouches and hat-coverings are a close imitation of oilskin; the latter, which when opened is cone- or tent-shaped, is used by all classes except the peasantry, even including the soldiers. The Corean boy's kite, which is also made of *Broussonetia* paper, consists of a piece of paper about a foot square with a circular hole in the middle, kept in form by thin strips of bamboo; a thin string is attached to each corner and brought together and connected to a single string, which is wound upon a wooden windlass. The perfection of splitting bamboo into thread-like strips seems to be divided equally between the Chinese and the Coreans, judging from a remarkably fine example of a blind which forms one of the exhibits. These very fine blinds are said to be used only by high mandarins, and the coarser kinds by the lower classes. Another illustration of very fine work is in the utilization of split rattans in the manufacture of articles of clothing, an undershirt and cuffs of very open ornamental workmanship being made entirely from this material, which is both soft and pliable. These shirts are said to be used next the skin in hot weather to prevent the oute shirt adhering to the body.

MR. J. S. JAMESON, of the Emin Pasha Relief Expedition, who died of fever at Bangala Station, on August 17 last, had accumulated a number of carefully-selected trophies and objects of natural history. These objects have been brought together at 166 Piccadilly, and arranged by Mr. Rowland Ward, so that they may be accessible to naturalists, and to his friends, who have been invited to view them to-day.

THE Russian Geographical Society has just published an "Instruction for Observations upon Shifting Sand Regions." The paper was carefully prepared by a Committee of persons thoroughly acquainted with the subject, and might with advantage be translated and communicated to other Geographical Societies.

AN important addition to school laboratories has just been completed at Eastbourne College, where the science teaching is undergoing great development. The laboratory, which has just been built there, affords working accommodation for twenty-four students, and has been thoroughly well fitted. The working benches are ranged down each side of the length of the room, and are all fitted with gas and water; and ample storage room is provided by four capital cupboard-cases. An excellent lecture-table occupies the usual position; the sand-bath and still are of copper, and heated by Fletcher's burners. There is also a convenient master's room, which, when finished, will be very complete.

PROF. H. G. SEELEY, F.R.S., is about to deliver a course of lectures on the practical study of the geology of the country round London. This course is given at the request of students of the London Geological Field Class; and information concerning the lectures may be obtained from Mr. William Dunn, 21 King William Street, Strand, W.C.

THE Penny Science Lectures at the Royal Victoria Hall for the month of December will be as follows:—December 4, on "Nature's Hot Springs," by Dr. S. Rideal; December 11, on "Limestone Rocks and their History," by Mr. E. Wethered. The series will begin again on January 22, 1889.

AT a *conversazione*, given by the Oxford University Junior Scientific Club, on Friday, November 23, in the University Museum, Prof. Milnes Marshall, of the Owens College, delivered a lecture on "Animal Pedigrees," which was highly appreciated by a numerous company of members of the University and their friends. Later in the evening Colonel Gouraud introduced and explained the new Edison phonograph. The members of the Club further entertained their guests by varied scientific exhibits and demonstrations, while the band of the 6oth Royal Rifles enlivened the proceedings with music.

THE Duchess of Albany has consented to become Patroness of the Sanitary Institute.

A GIGANTIC stalactite cave has been discovered near Rübeland, in the Harz Mountains, surpassing the neighbouring Bauman's Cave in size and beauty. Some excellent photographs of different parts of the cave were taken by Dr. Max Müller, of the Brunswick Technical High School, by means of the electric light. These photographs are shortly to be published, accompanied by explanatory notes by Prof. Kloss, of Brunswick. The cave is to be lighted by electricity, and opened to the general public next year, after precautions have been taken to keep it in its present perfect state.

DURING the last summer Dr. Otto Zacharias examined carefully the crater "Maare" (lakes) of the volcanic Eifel. They are inhabited by numerous species of Copepoda, Daphnidæ, Radiolaria, Rotifers, water mites, and insect larvæ. The largest of the "Maare," the Laacher See, which measures about seven miles in circumference, contains a special fauna. Besides this lake, Dr. Zacharias examined four others: the Pulvermaar, Holzmaar, Gemündener Maar, and Schalkenmehren Maar.

AT the meeting of the Royal Meteorological Society on November 21, Dr. A. Riggenbach, of Basle, read an interesting paper on a method of photographing cirrus clouds. Great difficulty is experienced in obtaining photographs of cirrus clouds, the reason being that the blue light of the sky acts with nearly the same actinic energy as the white light of the clouds on the sensitive silver salts of the plate. What is wanted is that this blue light of the sky should be dulled, the light of the clouds being left unaffected, and this can be done by means of the analyzer of a polarizing apparatus. The light from the blue sky is partly polarized, and to the largest extent at the points which are situated 90° from the sun; the plane of polarization passing through the points looked at, the sun, and the eye of the observer. On the other hand, the light coming from a cloud is only polarized to a slight extent. Having spoken of what can be done by the use of a Nicol's prism, Dr. Riggenbach went on to say that we might substitute for a Nicol's prism a dark mirror, a painter's mirror, or, best of all, a plate of obsidian. If such a plate be held so that the plane which passes through the cloud, its reflected image, and the eye, is normal to the line from the observer to the sun, the mirror extinguishes the polarized light from the sky almost completely, and the reflected image of the cloud comes out sharp on a dark background. If such an obsidian plate be fixed before the lens of a photographic camera, so that its plane is inclined at an angle of 33° to the optical axis of the lens, and the camera be placed so that the sun's rays shine perpendicularly on one of its sides, we then turn the whole apparatus round, in the direction in which the sun lies, as an axis, until a cirrus cloud is visible in the camera. If a sensitized plate be inserted, a picture of the cloud can be produced under the most favourable conditions possible. A still simpler mode of obtaining such cloud pictures is to use the surface of a lake as a polarizing mirror. The best clouds for such a purpose are those at sunrise or sunset, at an altitude of about 37°, and in an azimuth either greater or less than that of the sun by 90°.

THE North Atlantic Pilot Chart for November states that the most noteworthy disturbance during October was a West Indian hurricane which developed near Yucatan on the 9th, and reached the south coast of Long Island on the 12th. The tracks of all the depressions moving eastward from the American coast during the first half of the month lay well to the northward of the normal path until reaching the 55th parallel. This is very interesting in connection with the persistence with which an energetic area of high barometer lingered over the middle of the Atlantic, and affords a good illustration of the tendency of areas of low pressure to avoid those of high. Only one iceberg, near Belle Isle, was reported.

WE have received the Report of the Meteorological Service of the Dominion of Canada for the year 1885, which shows continual progress and improvement in the various departments. In addition to the Annual Reports, containing the results for numerous stations, a Monthly Review is published giving a general *résumé* of the weather throughout the Dominion, and an analysis of the daily forecasts and storm warnings. The weather signals carried on the railway cars are much appreciated, both by the farming community and the general public. New stations are established in the more remote districts as opportunities offer, and many valuable observations are also obtained along the line of the Canadian Pacific Railway, in regions where it is difficult to get observers other than the station officials.

TWO fine series of salts of two new platinum bases containing sulphur and organic radicles have been prepared by the Swedish Professor Blomstrand and his assistants, of Lund. They will form a striking addition to the now large number of these remarkable platinum compounds, which have been obtained since the preparation of the first of their class, the well-known green salt of Magnus, in 1828. When a solution of potassium platinous chloride, K_2PtCl_4, is shaken with two molecular equivalents of ethyl sulphide $(C_2H_5)_2S$, a quantitative precipitation of a yellow chloride, of the composition $Pt\genfrac{}{}{0pt}{}{S(C_2H_5)_2 \,.\, S(C_2H_5)_2Cl}{Cl}$, occurs. On treating this somewhat complex substance suspended in water with another two equivalents of ethyl sulphide, the whole eventually dissolves, with the exception of a small quantity of an oily substance, which appears to be formed as a secondary product. On allowing the separated clear liquid to stand, it gradually deposits crystal crusts of greenish-yellow monoclinic tables of the chloride of the first new base, $Pt\genfrac{}{}{0pt}{}{S(C_2H_5)_2Cl}{S(C_2H_5)_2Cl}$. In a similar manner, Prof. Blomstrand has obtained the bromide, $Pt[S(C_2H_5)_2Br]_2$, which crystallizes in reddish-yellow monoclinic prisms; and the iodide, which resembles the bromide very closely, but forms beautiful dark-red monoclinic crystals of considerable size. In addition to these well-defined halogen salts, the nitrite was obtained in large colourless rhombic crystals; the sulphate, $Pt[S(C_2H_5)_2]_2SO_4 + 7H_2O$, in exceptionally large crystals exhibiting a great number of faces; and also the nitrate in very soluble crystals. The halogen salts of the group are readily transformed into the more stable platinic compounds, which are found to be much more difficultly soluble. Thus the chloride, $Cl_2Pt[S(C_2H_5)_2Cl]_2$, was obtained in the form of yellow tables and prisms belonging to the triclinic system. The bromide forms red monoclinic prisms, and the iodide, perhaps the prettiest salt of the whole series, crystallizes from chloroform in dichroic prisms, which appear dark red by transmitted, and dark blue by reflected, light. The second series of salts are precisely analogous, but contain the radicle methyl instead of ethyl. It is interesting to note that Prof. Blomstrand was successful in isolating the base of the second series itself, $Pt[S(CH_3)_2]_2(OH)_2$, as a yellow, strongly alkaline liquid. Between the two series, an interesting mixed sulphine-chloride was obtained, containing both ethyl and methyl, $Pt\genfrac{}{}{0pt}{}{S(C_2H_5)_2Cl}{S(CH_3)_2Cl}$, by the addition of two equivalents of methyl sulphide to the yellow chloride first mentioned above. It will be readily seen that the work thus briefly reviewed forms a most valuable contribution to our knowledge of the platinum bases.

A LIST of the writings of Dr. Asa Gray, chronologically arranged, with an index, has been printed as an appendix to vol. xxxvi. of the *American Journal of Science*, and is also published separately. The compiler has done his work with great care.

IN an interesting paper on "Musical Sands," Mr. C. Carus-Wilson has discussed the cause of the remarkable sonorous properties exhibited by the sands of various localities, a subject which was referred to in this journal on August 30 and September 27 of the present year. Mr. Carus-Wilson gives the details of numerous experiments. Some of them are of a very ingenious character, which lead him to the conclusion that the vibration of the individual sand-grains is brought about by friction, and that it is the cumulative effect of numerous vibrating particles of the same size that becomes audible. This conclusion differs in some respects from the theories which have been put forward by some other investigators of these very curious phenomena.

THE first part of a useful "Introduction to Entomology," by J. H. Comstock, has been published at Ithaca, U.S.A. The groups of insects have been fully characterized, so that their relative affinities may be learned; and much space has been given to accounts of the habits and transformations of the forms described. The work contains many original illustrations.

A LITTLE book, by Mr. W. Mawer, containing a brief account of the life and discoveries of Darwin, has just been issued by Messrs. Swan Sonnenschein and Co. The volume is likely to be of interest to young people, for whom it has been specially written.

IN the Administration Report of the Madras Government Central Museum for the year 1887-88, Mr. Thurston, the Superintendent, speaks of two tours which he made during the year on behalf of the Museum, viz. to Tuticorin and the Nílgiris. At the former place great assistance was rendered by Captain Phipps, Port Officer, in placing boats and divers at his disposal. Large collections, illustrative of the marine fauna, were made, mainly through the medium of native divers, who displayed no little skill, and some of the specimens, *e.g.* the entire collection of Sponges and Echinoderms, have been sent to the British Museum (Natural History), for investigation and report. The results of this tour will be published after Mr. Thurston has made a further tour of the Gulf of Manaar. During his tour on the Nílgiris a large area of both the plateau and slopes was explored, and large collections of birds, reptiles, butterflies, &c., were made, but as he only returned to Madras a few days before the termination of the official year, he reserves a list for his next annual report.

THE current number of the *Asiatic Quarterly Review* contains an article by Captain A. C. Yate on the Shan States, in which reference is made to the ethnology of the obscure region bounded on the west by Burmah, on the north and north-east by China, on the east by Tonquin, and on the south by Siam and the Karennee. Of the Shans he has a very low opinion: he describes them as sordid and lazy, they live in extreme poverty, they are not brave, and their foot is yet barely on the lowest rung of the ladder of civilization. Still, they are somewhat ahead of the tribes around them. They have a literature: Captain Yate says it lies about dirty and uncared for in pagodas, priests' houses, and travellers' rest-houses. Amongst the other tribes inhabiting the same region are the Palaungs, the Red and White Karens, Kakyens or Kachins, Dunoos, Laos, Was, Kaws, Chins, Yins or Yeins, Yindalaings, Padaungs, Taungthus, Müsüs, and Kakins. All these are believed to speak their own dialects, but none of them have a written language. The Palaungs, Kachins, and Laos are the more important of these. The Kachins are extending all over the region, and are hated and feared by the Shans. They worship or propitiate certain evil forces in Nature which they call "nats." Captain Yate gives some curious details in regard to this and other tribes, but his aim is rather to describe what they are as he saw them, than to discuss the ethnological problems connected with this region and its races and fragments of all but extinct peoples.

IN a paper in the current number of the Journal of the Anthropological Institute, Mr. J. Allen Brown states, on the authority of Mr. Carlyle, late of the Archæological Survey of India, that some few of the rudest aboriginal tribes of the wildest central parts of India still practise a modified and partial sort of tattooing, but only with deep blue or other dark or grey colour, never with red. Hindus use red and white, and sometimes yellow colours superficially, without incisions, which will wash off, as religious sectarial caste marks on their foreheads. These are the only instances, so far as Mr. Brown can learn, where such pigments are now used either for embellishment or religous symbols in India.

WE have received the second part of vol. iii. of the Proceedings of the Linnean Society of New South Wales. The following are among the contents: on additional evidence of the genus *Ichthyosaurus* in the Mesozoic rocks ("Rolling Downs Formation") of North-Eastern Australia, by R. Etheridge, Jun. (Plate vii.); on additional evidence of the occurrence of *Plesiosaurus* in the Mesozoic rocks of Queensland, by R. Etheridge, Jun. (Plate viii.); description of a new *Tripterygium* from Port Jackson, by E. P. Ramsay and J. D. Ogilby; notes on the Mueller Glacier, New Zealand, by Captain F. W. Hutton (Plates ix. and x.); the insects of King's Sound and its vicinity (Part 1), by William Macleay; Australian indigenous plants providing human foods and food-adjuncts, by J. H. Maiden; geographical notes in Malaysia and Asia, by the Rev. J. E. Tenison-Woods; Diptera of Australia (Part 2, the Sciaridæ), by F. A. A. Skuse (Plate xi.); note on sympathy and foster-parentage among birds, by E. G. W. Palmer; on some new and rare Hydroida in the Australian Museum Collection, by W. M. Bale (Plates xii.–xxi.); notes on Australian Coleoptera, with descriptions of new species, by the Rev. T. Blackburn; the development and structure of the pineal eye in *Hinulia* and *Grammatophora*, by W. J. McKay (Plates xxii.–xxiv.).

IN his note on sympathy and foster-parentage among birds, Mr. Palmer tells a curious story of a wood-duck and a hen.

"Some time ago," he says, "a boy brought in an egg found near a waterhole, which was placed with other eggs under a sitting hen, and in due course hatched out a wood-duck. The wood-duck was reared among a clutch of chickens, was as well tended as her other chicks by the mother hen, and reached adult age. On one occasion a hen brought out a brood of chickens, and the wood-duck kept in close companionship with the hen and chicks for several days, until the hen took umbrage at the duck's constant attendance, and several fights between the hen and duck ensued. Eventually the duck drove away the hen, and took sole charge of the chickens throughout the day, the hen following round disconsolately till night-fall each day, when the duck surrendered her charge, allowing the mother to brood over them at night, but again taking charge of them in the morning. This continued till the chickens were able to take care of themselves."

THE writer of the article on "The Opening of the Pasteur Institute" (NATURE, November 22), informs us that by an oversight a misprint occurs on p. 74, in the first line of the second paragraph. Class *C.* should read Class *A.*

THE additions to the Zoological Society's Gardens during the past week include a Black-eared Marmoset (*Hapale penicillata*) from South-East Brazil, presented by Miss B. Pollock; a Malbrouck Monkey (*Cercopithecus cynosurus*) from West Africa, presented by Mr. David Baumann; a —— Toad (*Bufo* ——) from California, presented by Mr. D. E. Cardinall; a North American Turkey (*Meleagris gallo-pavo*) from North America, presented by Mr. F. J. Coleridge Boles; a Tawny Owl (*Syrnium aluco*), European, deposited.

OUR ASTRONOMICAL COLUMN.

COMET 1888 *e* (BARNARD, SEPTEMBER 2).—Dr. L. Becker, who has recently computed elements for this comet by the method of variation of the distances, finds (*Dun Echt Circular*, No. 164) that the most probable orbit is hyperbolic in character, the residuals for the intermediate observations being much larger for the most suitable parabola than for the hyperbolic orbit.

The elements are as follow:—

$$T = 1889 \text{ January } 29{\cdot}90453 \text{ G.M.T.}$$

$$\left.\begin{aligned} \pi - \Omega &= 339^\circ\ 54'\ 32''{\cdot}1 \\ \Omega &= 357\ 15\ 59{\cdot}1 \\ \iota &= 166\ 22\ 24{\cdot}3 \end{aligned}\right\} \text{Mean Eq. } 1888{\cdot}0.$$

$$\log q = 0{\cdot}2595204$$
$$\log e = 0{\cdot}0135800$$

Ephemeris for Greenwich Midnight.

1888.	R.A. h. m. s.	Decl. ° ′	Log Δ.	Log *r*.	Brightness.
Nov. 29	2 28 2	5 57·3 S.	0·0501	0·2971	12·1
Dec. 1	2 15 18	6 21·0			
3	2 3 9	6 41·4	0·0669	0·2928	11·4
5	1 51 40	6 58·4			
7	1 40 50	7 12·2	0·0887	0·2887	10·5
9	1 30 42	7 23·2			
11	1 21 15	7 31·5	0·1131	0·2847	9·6
13	1 12 29	7 37·5			
15	1 4 21	7 41·2 S.	0·1390	0·2810	8·7

The brightness on September 2 has been taken as unity.

Dr. Berberich has, however, computed parabolic elements using an observation made at Karlsruhe on October 28, the ephemeris from which satisfies well an observation obtained at Rome as recently as November 7.

According to Dr. Becker's ephemeris, the comet will pass within 1½′ of a bright nebula, 100 H I Ceti, a little before midnight on December 10, and as the head of the comet is of very considerable size, the nebula will be completely involved in it. The moon will be near setting, so that, though the comet will be low, about 75° Z. D., spectroscopic observations might be obtained if the night were clear. Dr. Berberich's elements would place the transit some three hours later, when the comet would have set to English observers, but makes the transit nearly a central one.

COMETS FAYE AND BARNARD, OCTOBER 30.—The following ephemerides for these objects for Berlin midnight are by Drs. Lamp and Spitaler respectively (*Astr. Nach.*, No. 2867):—

1888.	Comet 1888 *d* (Faye). R.A. h. m. s.	Decl. ° ′	Comet 1888 *f* (Barnard, Oct. 30). R.A. h. m. s.	Decl. ° ′
Dec. 1	8 14 34	2 29·2 N.	10 19 27	8 50·8 S.
3	8 14 36	2 12·8	10 20 47	8 16·9
5	8 14 30	1 57·2	10 21 59	7 41·5
7	8 14 15	1 42·6	10 23 4	7 4·4
9	8 13 52	1 28·9	10 24 0	6 25·8
11	8 13 21	1 16·2	10 24 50	5 45·8
13	8 12 42	1 4·5	10 25 32	5 4·2
15	8 11 55	0 53·9	10 26 4	4 20·7
17	8 11 1	0 44·4	10 26 28	3 35·5
19	8 10 1	0 36·1 N.	10 26 45	2 48·8 S.

The brightness on December 19 of Faye's comet is 1·8 that of the brightness at discovery; of Barnard's, 0·9. Both comets change but slowly in brightness, as the distance from the earth is diminishing, whilst the distance from the sun is increasing.

THE SATELLITE OF NEPTUNE.—Mr. A. Marth pointed out in the *Monthly Notices* (vol. xlvi. p. 506), some two years ago, that the values for the inclination of the orbit of the satellite of Neptune and its ascending node, as deduced from the observations obtained in Malta in the years 1852 and 1864 by Lassell and Marth, and again at the Washington Naval Observatory from 1874 to 1884, show a well-marked, progressive, and regular change. Referring these values to the plane of the orbit of the planet, they are as follows:—

Date.	*n.* °	*i* (motion considered direct). °
1852	176·20	148·33
1864	180·41	146·19
1874	182·59	144·04
1883	184·31	142·38

Mr. Marth offered no theory by which to account for this change, but begged for continued observations. Prof. Asaph Hall also has more recently urged the necessity for further observation, and by fresh observers, avowing at the same time his suspicion that these changes are due to systematic errors in the observations. But M. Tisserand, in a late communication to the Academy of Sciences of Paris, shows that the changes can easily be explained by the theory of a slight flattening of the surface of Neptune. In this case the angle between the plane of the planet's equator and that of the orbit of the satellite will be constant, and the pole of the orbit will revolve in a small circle round the pole of the planet, a complete revolution taking more than 500 years. The inclination of the orbit of the satellite will be considerable, probably greater than 20°; but the flattening of the surface of Neptune can only be slight, too small to be detected by direct measurement. Further observations may enable the amount of the inclination to be more exactly determined, and, at the same time, will show whether the changes in question are due or not to this one cause alone.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *DECEMBER* 2–8.

(**FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.**)

At Greenwich on December 2

Sun rises, 7h. 48m.; souths, 11h. 49m. 49·0s.; sets, 15h. 51m.: right asc. on meridian, 16h. 36·8m.; decl. 22° 5′ S. Sidereal Time at Sunset, 20h. 39m.

Moon (New on December 3, 10h.) rises, 6h. 3m.; souths, 10h. 56m.; sets, 15h. 39m.: right asc. on meridian, 15h. 42·7m.; decl. 15° 23′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	6 26	10 51	15 16	15 37·8	18 26 S.
Venus	10 41	14 27	18 13	19 14·9	24 21 S.
Mars	11 18	15 24	19 30	20 11·8	21 25 S.
Jupiter	8 12	12 12	16 12	16 58·9	22 17 S.
Saturn	21 20*	4 46	12 12	9 32·3	15 38 N.
Uranus	3 6	8 31	13 56	13 17·3	7 30 S.
Neptune..	15 22	23 6	6 50*	3 55·4	18 36 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Dec	h.	
2 ...	9 ...	Mercury in conjunction with and 3° 23′ south of the Moon.
3 ...	17 ...	Jupiter in conjunction with and 2° 38′ south of the Moon.
3 ...	18 ...	Mars at least distance from the Sun.
6 ...	2 ...	Venus in conjunction with and 2° 4′ south of the Moon.
6 ...	22 ...	Mars in conjunction with and 0° 15′ south of the Moon.
8 ...	23 ...	Jupiter in conjunction with the Sun.

Saturn, December 2.—Outer major axis of outer ring = 42″·8 : outer minor axis of outer ring = 10″·0 : southern surface visible.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.
U Cephei	0 52·4	81 16 N.	Dec. 4,	23 46 *m*
Algol	3 0·9	40 31 N.	,, 4,	6 15 *m*
			,, 7,	3 4 *m*
λ Tauri	3 54·5	12 10 N.	,, 4,	18 29 *m*
R Canis Majoris	7 14·5	16 12 N.	,, 2,	23 33 *m*
			,, 4,	2 49 *m*
S Cancri	8 37·5	19 26 N.	,, 6,	22 13 *m*
W Virginis	13 20·3	2 48 S.	,, 2,	3 0 *m*
U Herculis	16 20·9	19 9 N.	,, 8,	*M*
W Herculis	16 31·3	37 34 N.	,, 4,	*M*
R Lyræ	18 51·9	43 48 N.	,, 3,	*M*
V Cygni	20 47·6	34 14 N.	,, 4,	2 0 *m*
			,, 7,	1 54 *m*
δ Cephei	22 25·0	57 51 N.	,, 4,	6 0 *m*
			,, 5,	21 0 *M*

M signifies maximum ; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near η Persei	42	56 N.	Very slow ; faint.
,, ζ Tauri	80	23 N.	Taurids II. Max. Dec. 6. Slow, brilliant.
,, δ Geminorum	110	24 N.	Rather swift.
,, β Ursæ Majoris	163	58 N.	Very swift ; streaks.

GEOGRAPHICAL NOTES.

At the usual meeting of the Royal Geographical Society on Monday, Mr. J. Thomson read before a large audience a paper on "A Journey to the Atlas Mountains." He gave a most interesting account of the scenes visited, in the course of his trip, by himself and his companion, Mr. Harold Crichton-Browne. Describing some of the practical results, he said they had ascended and crossed the Atlas Chain in no fewer than six different places besides making various subsidiary trips into the lower ranges. A large series of barometric and boiling-point observations have been taken, which would assist in forming a more accurate idea of the general elevation of the range. Several glens had been explored, and the head-waters of some important streams had been mapped out. New and important light had been thrown upon the geological structure of the mountains. A small collection of plants from the higher altitudes had been made, and finally a series of photographs (which were exhibited) of the mountains, the inhabitants, and their houses have been obtained. He had reached an altitude in the mountains 1300 or 1500 feet higher than any other traveller.

The December meeting of the Royal Geographical Society will be held on Monday, the 17th, at the University of London, instead of Monday, the 10th, as announced in the sessional programme. Colonel R. G. Woodthorpe will read a paper on explorations on the Chindwin River, Upper Burmah.

The Russian Expedition for the exploration of Tibet, organized by the late General Prjevalsky, is, notwithstanding the death of the famous explorer, to be despatched on its mission. This announcement was made by M. Semenow, the Vice-President of the Russian Geographical Society, at a meeting held by the Society in honour of General Prjevalsky.

THE RENAISSANCE OF BRITISH MINERALOGY.[1]

An ideal Presidential address should treat, not of a special point in the science of interest to a section only of the Society, but of the science in its broader aspects ; and the simplest permanent arrangement of this kind is that which makes it deal with the progress of the Society or of the science during the interval which has elapsed since the delivery of the next preceding address.

But in the case of our own Society we labour under special disadvantages, whether the address is to be on a specific subject or on the progress of the science. Not only is the Society small, but the number of its members able to devote any large part of their time to pure mineralogy is far smaller still. Hence if a set address were expected from the President we should be unnecessarily limited in our selection for that office. Many of our ablest members, men who would make the best of Presidents, men of wide culture and extensive general knowledge, men endowed with ideas and the power of expressing them, men who would bring to us a large experience obtained on the executive of other and larger Societies, though willing, nay anxious, to help us in the management of the affairs of the Society, would be prevented from giving us their services in the chair, owing to the sheer impossibility of devoting the requisite time and thought to the preparation of a purely mineralogical address such as they would consider worthy at once of the Society and of their own reputation.

Again, the number of our London meetings has been up to the present only three a year, and, as far as we can see, it is not yet desirable to increase their frequency. To set aside annually one-third, or even one-fourth, of the time of the Society for consideration of the views of the President, or of the progress and past work of the Society, would seem to be wanting in regard for proportion. A few minutes spent by the members in turning over the pages of the *Magazine* will give a better idea of the work of the Society, and be at the same time more exhilarating, than any summary a President can make. A report by the Council on the finance and general business, as brief as it can be made, seems to me sufficient for all reasonable purposes, and least wasteful of the time of the members and officers.

I have referred to the *Magazine*, and have said that a good idea of the work of the Society may be obtained in a few minutes by turning over the pages of one of its volumes. It may be cast in our teeth that the volume is small, but we can proudly and truly retort that few volumes of the same size furnish so vast an amount of heavy reading. The density, indeed, is prodigious—not that of lead, but of gold, refined gold. The volume is intended for transmission to all posterity, and not as a mere addition to the ephemeral literature and scientific gossip of a too prolific century. The present generation, by its careful use of the volume, will doubtless help it to reach its destination.

Bulky publications are, indeed, matters for shame rather than pride : they are the immediate ruin of a small Society, a perennial burden to librarians, and, as their contents are never completely indexed, a terror to subsequent investigators. The Ancients not unwisely refrained from the invention of printing ; they recognized that their duty to themselves was to read only what was worth the vast labour of transcription, and that their duty to posterity was to transmit to it only their masterpieces ; when even these became burdensome, an incendiary, doubtless a librarian, quickly reduced their volume. But for us Moderns the cost of multiplication of copies has become so small that everything, good or bad, is printed and preserved ; and it becomes necessary to spend the greater part of one's life in the preparation and study of indexes rather than of the literature itself. It would be an immense boon to mankind if some impartial and perfect tribunal could be empowered to do on the large scale what the curate and barber did so satisfactorily with Don Quixote's books on the small scale—distinguish the worthy from the worthless, and relentlessly annihilate that which ought not to cumber our shelves or demand even a passing glance.

I have said that the number of our members is small. I am not sure that it would be politic as yet to increase it. Members who have a living interest in mineralogy are most valuable, and of such we cannot have too many ; but mere subscribers of paltry gold would eventually be a source of weakness. After encouraging the Society to extravagant expenditure, they would fall away and leave it in the lurch.

[1] Extracted from an address by Mr. Fletcher, the retiring President, at the annual meeting of the Mineralogical Society, October 30, 1888.

But is it necessary that the number of people in this country with a lively interest in mineralogy should remain so small? We have only to look abroad to see at once that the cause of this smallness of number is not inherent in the subject itself. In Germany, for instance, everyone—of course, with here and there a conspicuous exception—seems to be either a Professor of Mineralogy or a student of it. Periodical publications, at once voluminous and teeming with valuable results of scientific work, are there maintained. Somewhat more than a century ago, the study of minerals was everywhere popular, and received its share of attention from the cultured classes. Students thronged to Freiberg from all parts of the world to hang on Werner's lips.

There is one reason for this decline of general interest in mineralogy which I may mention in passing. In the good old days minerals were named and classified by help of simple external characters, and the facts of the science could be easily grasped without much preliminary training. Since then, the invention of crystallography and of the atomic theory, and the discoveries made in physical optics, have brought about a vast change in the treatment of the subject, and the mineralogy of to-day is even in its elements beyond the range of ordinary mortals. The pages of its text-books are sprinkled with wonderful formulæ designed by perverse chemists, and with unpronounceable hieroglyphics maliciously invented by cruel crystallographers.

But the chief reason for the decline of mineralogical study in our own country is that mineralogy has been almost completely excluded from our educational system. In the older Universities, it is true, mineralogy has been long represented by Professors, but, until lately at least, it has been allowed scarcely any weight in the examinations for a degree. The study of other branches of science has been encouraged, while that of mineralogy has been neglected and forgotten. One of the evils of the examination system is that all the available energy of our youth is concentrated wholly on subjects upon which stress is laid by a not omniscient Examining Board; most students, too, must almost necessarily take up subjects in which there is an opportunity of showing their comparative ability, and by a knowledge of which there is reasonable prospect of being able to gain a future livelihood.

Taught only as a subordinate and unimportant subject at the Universities, and not taught at all outside, pure mineralogy has been in great danger of becoming extinct in this country: a few years ago the capsizing of a coach or the bursting of a balloon might have been the end, and the British Professor or student of pure mineralogy have become a mere tradition.

The discovery of the transparency of thin rock-sections, and the important conclusions which may be arrived at from their microscopical examination, have now turned attention to mineralogy once more, and it seems likely that the knowledge of mineral characters requisite for petrographical work may at last lead to our mineralogical renaissance. Owing chiefly to the patience and perseverance of Prof. Lewis, there is now a certain amount of encouragement to mineralogical study at Cambridge; and if the renaissance is to be brought about, the example of Cambridge must be followed by the other Universities, and mineralogy be assigned a higher place in the examinational system.

Minerals are omnipresent. Is it unreasonable to ask that everyone should be taught their simple characters, and be shown how to recognize such minerals as are met with at every turn? The teaching would improve the capacity for observation, and give fresh interest to many a pleasant ramble—through the workings of a mine. And is not Prof. Ruskin in the right when he claims that a knowledge of the minerals, conveniently grouped as precious and ornamental, should form a part of every gentle education? More especially ought we not to insist upon such elementary teaching for the numerous officials sent out by the nation to the less explored regions of the world?

In the higher teaching of mineralogy, difficulties present themselves, but they might easily be lessened by division of labour; a preliminary training in the elements of mathematics, physics, chemistry, and crystallography being absolutely necessary to the manufacture of a mineralogist. The teaching of crystallography is generally relegated to the Professor of Mineralogy, and the subject regarded as a mineralogical difficulty; but this ought not to be the case. It is true that a mineralogist was the first to discover a relationship between the various crystallized forms of the same substance, and thus to institute a crystallographic science, which has since been found indispensable in mineralogical study. It is true that the mineralogist has been the originator of every advance in crystallographic knowledge. It is true that the mineralogist has in minerals ready-made crystallizations, which in their excellence and variety of form can rarely be imitated in the laboratory. But it is no more the province of a mineralogist to teach crystallography than it is to teach chemistry or the use of a delicate balance. He does teach it indeed, but that is merely because his pupils reach him imperfectly trained for the pursuance of his own subject.

Crystallography should be taught as a special subject; and a knowledge of it should be required not only of the mineralogist but of the chemist, and even of the physicist. Hitherto, at least, the chemists of this country have been too content either to leave the crystalline forms of their artificial products undetermined, or to impose the task of their determination on the already sufficiently occupied mineralogist. It seems obvious that in a satisfactory system of education every chemist should be taught how to measure and describe the crystalline characters of the products which it is his fate to call into existence. On various occasions expression has been given to this view, but the only chemist who has yet seen his way to act upon it is Prof. Henry Armstrong, who, I am happy to say, has introduced the subject into the educational course of the City and Guilds Technical Institute. I trust that before another generation passes away his excellent example will be followed throughout the country. A knowledge of the elements of crystallography, including the mechanics of crystal measurement, ought to be made a *sine quâ non* for a degree in chemistry at every University.

The measurement of the angles of a crystal, the determination of its symmetry, and the calculation of its form, are infinitely less difficult than is generally imagined: given a knowledge of elementary mathematics and the careful use of measuring instruments, the processes are in general extremely simple. The complexity of crystallographic calculation is only apparent, and is due to the existence of text-books: they are generally worse than useless. A voluminous work on crystal-calculation is usually an attempt to provide formulæ which shall enable a student to solve every possible problem by rule of thumb, without his needing to have the faintest idea of what he is really doing. Practically, anyone familiar with the processes of trigonometry can deduce from first principles the formula required for each special case as it occurs in less time than he can discover the rule in the ponderous tome invented for his mystification.

I am, of course, far from asserting that the teaching of crystallography presents no difficulties at all: what I do wish to insist upon is that the kind of crystallographic knowledge requisite for the chemist in his own work is such that it may fairly be demanded of every one of them: the higher flights may be abandoned to the specialist.

At the present time, when Professors of Crystallography are not yet called into existence, there is one step which ought to be at once taken, and which would make mineralogy more possible eventually to a large number of our students. Every student of practical physics is taught how to measure with a reflecting goniometer the angles of an artificial prism: he should further be taught the measurement of the angles of a simple crystal, and the deduction of its symmetry. In his optical studies especially, such a practical knowledge of crystalline symmetry would be a great help to him. The reflecting goniometer in one of the forms used for crystallographic work might well be an instrument in use in every physical laboratory, and would subserve many a useful purpose. As soon as every physicist is taught how to determine the angles and symmetry of a simple crystal, and every chemist is further enabled to define the crystalline forms of his artificial products, we shall have a large army of students for whom the transition to mineralogical work will be easy; then, and not till then, can we hope for any useful increase in the number of the members of this Society; then, and not till then, can we hope that our country will in its study of mineralogy take its proper place among the nations of the earth.

One more point I may mention. Until a few years ago there were two distinct Societies, a Mineralogical and Crystallological: they had objects far from identical, and in a more perfect world might have flourished side by side. The fusion of the two Societies without any extension of the title of the Mineralogical has had for its effect that we cannot satisfactorily demand what the Crystallological could—namely, the support of the organic chemist; and although it was understood at the time of the fusion that papers on the crystallization of artificial products, organic or inorganic, should be within the scope of the joint Society, their inclusion in a *Mineralogical Magazine* would suggest a misnomer. The change or extension of our already lengthy title has its evident inconveniences, but it may be worthy of careful deliberation by the Society at some early date as to whether such an extension is not really necessary for the clearer definition of our objects if we are to enlist the sympathy of many who, though they may feel to be beyond the pale of a Mineralogical Society, yet by their investigation of the crystalline forms of the products of the laboratory may in the future, as in the past, throw light on the crystallography of the Mineral Kingdom.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The following gentlemen have been appointed examiners:—

Mathematical Tripos, Part II.: Dr. Routh (Chairman), Messrs. W. Burnside, J. Larmor, and A. R. Forsyth.

Natural Sciences Tripos and 2nd M.B.: Dr. Hill (Master of Downing), Prof. Cleland (Human Anatomy), Prof. Stirling, Dr. Gaskell (Physiology).

Natural Sciences Tripos: Prof. W. G. Adams, Mr. W. N. Shaw (Physics), Prof. W. A. Tilden, Mr. H. J. H. Fenton (Chemistry), Prof. Lewis, Prof. Story-Maskelyne (Mineralogy), Mr. F. Darwin, Prof. H. Marshall Ward (Botany), Mr. W. F. R. Weldon, Mr. F. S. Harmer (Zoology).

Natural Science Tripos and Special B.A.: Mr. W. W. Watts, Mr. A. Harker (Geology).

M.B. and Special B.A.: Prof. W. G. Adams, Mr. S. L. Hart (Elementary Physics), Mr. H. F. Neville and Mr. H. J. H. Fenton (Elementary Chemistry), Mr. F. Darwin and Mr. S. F. Harmer (Elementary Biology).

2nd M.B. only: Mr. Pattison Muir and Mr. H. Robinson (Pharmaceutical Chemistry).

Messrs. W. Carruthers, F.R.S., and J. E. Marr are appointed examiners for the Sedgwick Prize to be adjudged in 1892.

The University Lectureships in Botany and Advanced Human Anatomy, tenable for five years, are vacant. Candidates must send their names to the Vice-Chancellor on or before November 30.

Open Scholarships and Exhibitions will be competed for at the following Colleges, beginning on the undermentioned dates: Mathematics, Pembroke, December 11; Trinity Hall, December 11; Queens', December 12; Mathematics and Natural Science, Gonville and Caius, December 7; King's, December 10; Jesus, Christ's, and Emmanuel combined, December 11; St. John's, December 11; Trinity, December 11; Sidney Sussex, January 1. The tutors will give full information.

The Sheepshanks' Astronomical Exhibition will be competed for on December 10 and 11 at Trinity College.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, November 15.—"Combustion in Dried Oxygen." By H. Brereton Baker, Dulwich College, late Scholar of Balliol College, Oxford. Communicated by Prof. H. B. Dixon, F.R.S.

In 1884, some preliminary experiments, published in the Journal of the Chemical Society, convinced me that moisture exerted an important influence on the combustion of carbon. Since that time experiments have been made, not only with that element but with several others, and the same influence seems to be exerted on the combustion of some, while no such influence could be detected in the case of other elements. It was discovered very early in the investigation that hydrogen, both free and combined, aided the union of carbon with dried oxygen, and therefore, for the new experiments on this and other elements, special attention was devoted to their purification from hydrogen. It was found that two of these elements, amorphous phosphorus and boron, had, like carbon, a very great power of occluding hydrogen. To eliminate it some of the elements were heated in a current of pure chlorine, while others were heated in sealed tubes with the chlorides of the elements, special precautions being taken to free the purified elements from all traces of the agents used in their purification. In this way the elements—carbon, sulphur, boron, and phosphorus, the latter in both red and yellow modifications—were found to have their combustion influenced by the dryness of the oxygen. Some chemical union was found to take place, the extent of which varied with the dryness of the substances. In no case, however, did it manifest itself by flame. Ordinary phosphorus was obtained so pure as not to glow in the oxygen dried by phosphorus pentoxide, though the pressure was increased and diminished in every possible way. If water was added, rapid combustion at once set in.

The elements—selenium, tellurium, arsenic, and antimony—were purified with as much care as was expended on the elements mentioned above. Their combustion was, however, not found to be affected in any way by the dryness of the gas.

In the course of the investigation two facts were discovered about the combustion (1) of amorphous phosphorus, and (2) of carbon in oxygen. Amorphous phosphorus is generally regarded as being incapable of true combustion. It is asserted that before amorphous phosphorus can be heated to its kindling point, it changes into ordinary phosphorus, which then burns. This has been proved not to be the case. Amorphous phosphorus was heated in a current of nitrogen, free from traces of oxygen, to 260°, 278°, and 300° in three experiments, without undergoing any change to the ordinary modification. If moist oxygen was substituted for the nitrogen, combustion took place at 260°. It seems, therefore, probable that amorphous phosphorus undergoes a true combustion in oxygen without previous change to the ordinary modification.

With regard to the combustion of carbon, it has always been a doubtful question which of the two oxides is first formed. Is carbon monoxide the first product, undergoing further oxidation to the dioxide, or is carbon dioxide the first and only substance formed? The problem seems incapable of direct solution. It is, however, open to indirect attack. When carbon is heated in a current of partially dried oxygen, a slow combustion goes on, and, though the oxygen is in excess, both oxides are produced. The amount of monoxide, however, is twenty times the amount of the dioxide. Experiments also show that this occurs at temperatures at which dry carbon dioxide is not reduced by carbon. The carbon monoxide must, therefore, be produced by the direct union of its elements, its further oxidation being prevented by the dryness of the gases. Confirmatory experiments were performed in which carbon monoxide was found to be produced by the slow combustion of carbon in air at 440°, a temperature too low for the reduction of the dioxide by carbon. It is probable that the ordinary combustion of carbon goes on in two stages, that carbon monoxide is first produced, and, if circumstances are favourable, this is further oxidized to carbon dioxide.

"On the Secretion of Saliva, chiefly on the Secretion of Salts in it." By J. N. Langley, M.A., F.R.S., Fellow of Trinity College, and H. M. Fletcher, B.A., Trinity College, Cambridge.

Heidenhain has shown that when saliva is obtained by stimulating the chorda tympani, the percentage of salts in the saliva depends upon the rate of secretion, so that the faster the secretion the higher the percentage of salts is up to a limit of about 0·6 per cent.

The authors do not find any rate of secretion beyond which an increase in rate fails to increase the percentage of salts in the saliva. The increment in the percentage of salts decreases, however, with each equal successive increment in the rate of secretion.

As a rule, in saliva obtained by injecting pilocarpine, the percentage of salts follows Heidenhain's law; the exceptions are probably due to the action of pilocarpine upon the circulation.

The percentage of salts in saliva obtained by stimulating the sympathetic is higher than corresponds to its rate of secretion, the saliva obtained by stimulating the chorda being taken as a basis of comparison; this sympathetic saliva may be secreted at $\frac{1}{180}$th of the rate of chorda saliva, and yet contain very nearly as high a percentage of salts.

Dyspnœa decreases the rate of secretion of saliva with a given stimulus, and if not too prolonged, increases the percentage of salts, and tends to increase the percentage of organic substance in the saliva.

Clamping the carotid during secretion has the same general effect as dyspnœa, but it causes a still more marked increase in the percentage of salts. Its after-effect is also much greater, and lasts longer.

Bleeding has a similar effect to dyspnœa and to clamping the carotid, but its most marked effect is an increase in the percentage of organic substance.

Injection of dilute salt solution, NaCl, 0·2 to 0·6 per cent., in sufficient quantity, considerably increases the rate of secretion of saliva; the percentage of salts in the saliva decreases, although the rate of secretion of salts usually increases; the percentage of organic substance decreases; that is, increasing the volume of the blood with dilute salt solution chiefly increases the rate of secretion of water.

Injection of strong salt solution increases the percentage of salts in saliva. This is in accordance with the recent observations of Novi, that the chlorine in the salts of saliva is increased for a given rate of secretion by increasing the percentage of sodium chloride in the blood.

Saliva produced by stimulating the chorda tympani, or by injecting pilocarpine, after a small dose of atropine has been given, contains a low percentage of organic substance and of salts.

The authors, like Werther, find that sub-lingual saliva has a considerably higher percentage of salts than sub-maxillary saliva.

If lithium citrate, potassium iodide, potassium ferrocyanide, and pilocarpine are injected into the blood, lithium can be detected in the first drops of saliva secreted, potassium iodide after the first six drops; potassium ferrocyanide cannot be detected at any stage of secretion.

The general result of these experiments is to show that the secretion of water, of salts, and of organic substance are differently affected by different conditions, and that the percentage composition of saliva is determined by the strength of the stimulus, by the character of the blood, and by the amount of blood supplied to the gland.

All, or nearly all, the arguments which have been adduced to prove that the secretion of organic substance is governed by special nerve-fibres, have their counterparts with regard to the secretion of salts, so that we might imagine at least three kinds of secretory fibres to be present. The experiments, on the whole, indicate that this complicated arrangement does not exist, but that the stimulation of a single kind of nerve-fibre produces varying effects according to the varying conditions of the gland cells.

Linnean Society, November 15.—Mr. W. Carruthers, F.R.S., President, in the chair.—On behalf of Mr. H. Bolus, Mr. J. G. Baker exhibited a specimen of *Eriospermum folioliferum*, a plant showing a very remarkable type of leaf-structure. It was figured by Andrews in his "Botanist's Repository" in 1807, and lost sight of until recently refound by Mr. Bolus in Namaqualand.—Prof. Stewart exhibited a substance which had been picked up on the sea-shore, the nature of which it had puzzled many to determine, its structure being regarded by some as animal, by others as vegetable. He proposed to submit it to careful microscopical examination.—Mr. J. E. Harting exhibited a South American bat from Trinidad (*Noctilio leporinus*), alleged to be of piscivorous habits, and remarked upon a similar habit which had been observed in a species of *Pteropus* in India.—A paper was read by Mr. B. D. Jackson, on behalf of Mr. H. Chichester Hart, on the mountain range of plants in Ireland, and was criticized by Mr. J. G. Baker, who gave an interesting sketch of the characteristics of the Irish flora.—Two papers were then read, by Mr. Sladen, on the mammals and birds collected by Mr. H. N. Ridley in Fernando Noronha, in the determination of which the author had been assisted by Mr. O. Thomas and Mr. R. B. Sharpe.

Physical Society, November 10.—Prof. Reinold, President, in the chair.—The following communications were read:—On the calculation of the coefficient of mutual induction of a helix and coaxal circle, by Prof. J. V. Jones. In arranging some experiments for determining resistance absolutely by Lorenz's method, the author had occasion to consider what form of standard coil was most suitable for accurate calculation, and chose a helix of large diameter with a single layer of wire. To obtain a sufficient number of turns requires considerable axial length, and Lord Rayleigh's approximate method of calculating the coefficient was found to be insufficient where an accuracy of 1/100 per cent. is required. A method of calculation is given considering the wire as a helix whose equations are $y' = A\cos\theta'$, $z' = A\sin\theta'$, and $x' = k\theta$, those of the circle being $y = a\cos\theta$ and $z = a\sin\theta$. Applying the formula obtained to a circle of 10 inches diameter placed concentric with a helix of 20 inches diameter and 4 inches long, the value obtained is $M = n \cdot 53{\cdot}259$, whereas Lord Rayleigh's formula gives $n \cdot 53{\cdot}317$. Dr. Fleming described a wooden anchor ring wound like a gramme armature, and having a secondary coil added, which he had devised as a standard of mutual induction, and used for determining capacity absolutely. In reply, the author said he had not considered the wire to have thickness, as he felt sure this would not affect the result for his coil by one part in 100,000. With respect to Dr. Fleming's anchor ring, he considered the difficulty of winding it sufficiently uniformly to be a great drawback to its general adoption.—On the upper limit of refraction in selenium and bromine, by Rev. T. Pelham Dale, read by Mr. Baily. In a former paper (read February 11, 1888) the author showed that an upper limit of refraction for selenium should theoretically exist about the middle of the visible spectrum, and the present communication describes some experiments which tend to confirm the prediction. On placing a thin film of selenium under a spectro-microscope, it was found to be opaque to rays above the green, and previous calculation had given 5295·7 as the limiting wave-length transmissible. Sulphur at ordinary temperatures should have its upper limit beyond the visible spectrum, but theory indicates that increased temperature will lower the limit. It is well known that sulphur darkens when heated, and when a film of boiling sulphur was examined under the spectro-microscope, all but the red end of the spectrum was absorbed. On cooling, the region of absorption gradually retreated towards the violet end. Selenium is also found to become more transparent as it is cooled, and its refractive equivalent is equal to that of sulphur multiplied by the ratio of its chemical equivalent to its density. Important optical as well as chemical relations thus exist between the two elements. The results obtained by bromine films were remarkably similar to those of selenium, the violet rays being entirely cut off. A method of solving the equation $a\sin\theta = \sin m\theta$ (on the limiting solution of which the upper limits of refraction depends), by a table of Eulerian integrals, is given in the paper, and an analogy between total reflection and the upper limit of refraction is traced.—Experiments on glass in polarized light, by Prof. S. P Thompson.—On a new form of standard resistance coil, by Dr J. A. Fleming.

Chemical Society, November 1.—Mr. W. Crookes, F.R.S., President, in the chair.—The following papers were read:—The constitution of the terpenes and of benzene, by Prof. W. A. Tilden, F.R.S. When oxidized under similar conditions with dilute nitric acid, the natural terpenes—australene, terebenthene, and hesperidene—yield less than 2 per cent., dipentene (terpilene) yields 27·6 per cent., and cymene and paraxylene yield 73 to 80 per cent. of paratoluic acid. Each of the four terpenes combines with two molecular proportions of bromine, and no more. Camphene, however, does not combine with bromine, and hence must be regarded as saturated in the usual sense. The author concludes that since the terpenes contain at most four units of available combining capacity, the nucleus of six carbon atoms which they all undoubtedly contain must be united into a closed chain containing at the most two double "bonds." Kekulé's benzene formula is a well-known representation of a ring of six carbon atoms, but must be abandoned, since the author considers that the terpenes are certainly not benzene derivatives. Kekulé's formula is open to the objection that it represents benzene as containing "ethylenic" carbon, for which there is no evidence at all. Moreover, a body of this formula, when treated with nitric acid, ought to yield abundance of oxalic acid. This the terpenes do, but the benzenoid hydrocarbons do not. Referring to this last statement, Dr. Japp, F.R.S., said that phenol on oxidation with alkaline permanganate gave a considerable quantity of oxalic acid; Mr. Groves, F.R.S., added that oxalic acid is obtained in quantity on oxidation of chloranilic acid; and Dr. Perkin, F.R.S., remarked that he had obtained a quantity of oxalic acid in preparing picric acid from phenol. Dr. Perkin also said that the low magnetic rotatory power of American turpentine was a probable indication of the non-existence of a nucleus of six carbon atoms. Mr. Wynne remarked that the production of α-naphthol by the distillation of phenylisocrotonic acid, a compound in which "ethylenic" carbon was undoubtedly present, might be quoted in support of Kekulé's benzene formula; and Prof. Armstrong, F.R.S., expressed the opinion that the evidence at disposal was entirely insufficient to enable us to determine the constitution of the terpenes with any degree of probability.—Some new compounds of magnesia with the halogens, a contribution to the study of the electrolysis of magnesium chloride solution, by Messrs. Cross and Bevan. The authors find that the white substance separated at the cathode under the condition that the solution is not kept in circulation is a chloroxygen compound of magnesium, probably a magnesium hypochlorite. Similar compounds are formed on electrolyzing solutions of magnesium bromide and iodide.—The heat of dissolution of various substances in different liquids, by Mr. S. U. Pickering. The heats of dissolution of the nitrates and chlorides of calcium and lithium in water and in alcohol, and of bromine and iodine in various liquids, have been determined; and the results are held to support the author's view that the affinity of the radicles composing a salt molecule is not entirely saturated by their combination, and that the "residual affinity" of one of these radicles becomes entirely saturated by the solvent; the heat of combination of the atoms and the heat of dissolution of the molecule which they form being thus parts of the same chemical operation.—The criteria of plane and axial symmetry, by Prof. Armstrong, F.R.S. Wislicenus, in his now widely-known paper on the space arrangement of the atoms in carbon compounds, has termed compounds of the form $\begin{array}{c} a.C.b \\ \| \\ b.C.a. \end{array}$ *axially symmetric*, and those of the form $\begin{array}{c} a.C.b. \\ \| \\ a.C.b. \end{array}$ *plane symmetric*. In allocating these formulæ, he has argued, in a case such as is afforded by the two tolane dichlorides, for example, that the compound of higher melting-point (143°) is necessarily the plane symmetric modification, as it is produced on chlorinating tolane $\begin{array}{c} C_6H_5.C \\ \vdots \\ C_6H_5.C \end{array} + Cl_2 = \begin{array}{c} C_6H_5.CCl \\ \colon \\ C_6H_5.CCl \end{array}$, and from the three possible configurations of tolane tetrachloride has arrived at the conclusion that on reduction it should yield the axially symmetric tolane dichloride in the larger quantity. Blank's experiments (*Annalen*, ccxlviii. 1) show that the two isomeric dichlorides are produced nearly in the proportion of two parts of that of lower melting-point (63°) to one of that of higher melting-point, and the axially symmetric structure is therefore, it would seem, as a matter of course, assigned by him to the dichloride melting at 63°. The author regards these views as being based on pure assumptions, and shows that all the facts at present known tend to prove that symmetric compounds—such as the para- or symmetric di-derivatives, or the symmetric tri- and tetra-derivatives of benzene, or the axially symmetric $\alpha\alpha$ or $\beta\beta$ modifications in the case of isomeric di-derivatives of naphthalene—have always the highest melting-point. The tolane dichloride of higher melting-point is therefore probably the axially symmetric modification. The majority of "alloisomerides" are compounds containing unsaturated carbon usually in association with one of the halogens or oxygen, and the author points out that it does not appear that this circumstance has yet been taken into account, or that the extent to which the peculiarities manifested by the negative elements are concerned in and may condition the isomerism has been in the least considered. The symbolic system introduced by Van 't Hoff, and adopted by Wislicenus, tends, moreover, to withdraw attention from the consideration of the possible effect of the peculiarities referred to, inasmuch as a "double bond" is represented as the precise equivalent of two, and a treble bond as that of three single bonds; which all observations show is a misrepresentation of the facts.—Derivatives of methylindole, by Dr. H. G. Colman.—Acetamide and phenanthraquinone, by Dr. A. T. Mason.—The action of ethylenediamine on succinic acid, by the same.

PARIS.

Academy of Sciences, November 19.—M. Janssen in the chair.—On the "Collection of the old Greek Alchemists," by M. Berthelot. The parts now presented to the Academy complete the publication of the Greek text and French translation of this great work. Part v. contains technical treatises of special interest on the goldsmith's art, the tempering and colouring of bronze and iron, bronze casting, iron-gilding, the preparation of gold-leaf, the colouring of artificial gems, the treatment of pearls, the preparation of lye from ashes, beer, soap, &c. Most of these treatises appear to have formed part of a great work on practical chemistry composed in the eighth and ninth centuries, and several are written in the Byzantine dialect. But some are of great antiquity, amongst others one dealing with the phosphorescence of precious stones.—On the satellite of Neptune, by M. F. Tisserand.—On the latitude of the Gambey mural circle at the Paris Observatory, by M. H. Faye. A method is proposed by which the exact latitude of the instrument itself may be directly determined, and the results controlled which have been obtained with other processes by M. Périgaud in the present year, and by others at earlier dates. M. Périgaud determines the latitude of the Observatory as 48° 51′ 10″·9.—Note on the stability of the French seaboard, by M. Bouquet de la Grye. The results of the comparative observations taken for some years past at the ports of Brest, Cherbourg, and Havre, tend mainly to confirm the general researches of Colonel Goulier. From the comparative tables it appears that the mean level diminishes in the direction from Brest to Havre; at Havre the annual subsidence seems to be about 2 mm., at Cherbourg 1 mm., at Brest nearly absolute stability.—A study of submarine navigation, by M. A. Ledieu. A survey is given of the futile efforts made in this direction previous to the invention of Whitehead's regulating pendulum in 1872; which, combined with M. Joseph Farcot's servo-motor, gave the true solution of the problem. From that time dates all real progress in submarine navigation.—On various methods of treating rabies, by M. Odo Bujwid. Since his visit to M. Pasteur's establishment in 1886, M. Bujwid has been treating persons bitten by dogs, either mad or suspected of being mad, in his laboratory at Warsaw. At first he followed the simple processes of inoculation of M. Pasteur, and of M. Frisch, of Vienna, with some failures in both cases. But during the last sixteen months he has adhered exclusively to the intensive or severe treatment, which has been applied to 370 patients without a single fatality.—Observations of Palisa's new planet (281), and of Barnard's comet (October 30, 1888), made at the Observatory of Algiers with the 0·50 m. telescope, by MM. Rambaud, Sy, and Renaux. The observations of the planet are for the period from November 3 to November 8; those of the comet from the 5th to the 8th of the same month.—On the subsidence of the land in France, by Colonel Goulier. In reply to General Alexis de Tillo's remarks on his previous note, the author points out that his conclusions on this subject are not put forward as final, but only as a probable hypothesis to be accepted until proved untenable.—On mountain-ranges and their relations to the laws of deformation of the terrestrial spheroid, by M. A. de Grossouvre. On purely theoretical considerations the author arrives at the conclusion that the zones of foldings are progressively receding southwards. This conclusion is entirely in accordance with the observed facts connected with the dislocation of the earth's crust, and the regularity of the phenomenon is regarded as a confirmation of the theory of the primitive fluidity of the globe.—On equalities of two degrees, by M. Michel Frolov. These researches have reference to the properties of groups of n numbers, whose first and second powers give respectively equal sums, properties which have not yet been studied by any other arithmetician.—Maximum spectrum of Mira Ceti, by Mr. J. Norman Lockyer.—On the mutual relations of meteorites and shooting-stars, by M. Stanislas Meunier.—Note on the tensions of various vapours, by M. Ch. Antoine.—On the decomposition of the haloid salts of silver under the influence of light, by M. F. Griveaux.—Hydrochlorates of benzidine: their dissociation by water, by M. P. Petit.

BERLIN.

Physical Society, November 2.—Prof. von Helmholtz, President, in the chair.—Dr. Brodhun gave an account of experiments which he had made, in conjunction with Dr. König, for the purpose of testing the fundamental law of psychophysics in connection with the sense of sight. These have already been described by Dr. König in a communication to the Physiological Society, previously reported in NATURE (vol. xxxviii. p. 464). In the discussion which ensued, the President spoke on the sensation of light derived from the resting retina (*Eigenlicht*). In connection with the preparation of the new edition of his "Physiological Optics," he had made experiments and calculations which showed that the *Eigenlicht* is much greater than has hitherto been supposed. Taking as a standard the unit used by König and Brodhun in their researches, the *Eigenlicht* has a magnitude of forty to sixty such units. It is characterized by the irregular brightness of the field of vision, giving rise to the sensation of a more or less coarsely punctated image with lighter and darker patches; it further leads to the result that with equally small intensities of illumination large objects are seen more easily than small, so that the liminal intensity (*Schwellenwerth*) for small surfaces is considerably greater than for large.—Prof. Kundt exhibited a large number of photographs of spectra which had been prepared in his laboratory with a view to testing the action of light-absorbing substances on dry-plates. Spectra photographed on dry-plates which had been coloured with chlorophyll were especially interesting. They consisted of a bright strip ending near F, followed by a dark portion intercepted by an extremely bright line at the spot where the absorption-band of chlorophyll is present in the red. Plates stained with eosin similarly showed a bright strip corresponding to the absorption-band of this substance in the yellow, whose brightness was much greater than that of the rest of the spectrum. These experiments showed that the rays of light which are absorbed by the above colouring-matters exert an extremely active chemical action on the plates. Experiments made with a view to determining whether absorption of light has a similar effect on fluorescence yielded negative results. It still remains to investigate whether the maximum brightness of the spectrum photographed on a plate stained with chlorophyll corresponds exactly with the absorption-band of this substance, taking into account at the same time the influence of the solvent used for the solution of the chlorophyll on the position of its absorption-band. Prof. Kundt then exhibited a simple form of bolometer which, based on the principle described by Dr. R. von Helmholtz at a recent meeting of the Society, may be easily and cheaply constructed out of thin tinfoil, and is specially adapted for lecture experiments. The same speaker finally exhibited two selenium cells constructed by Uljanin, of Strasburg, in July 1887, and still extremely sensitive to the action of light.

Meteorological Society, November 6.—Dr. Vettin, President, in the chair.—Dr. Kremser gave an account of meteorological observations made during a balloon voyage on June 23 of this year. For the purpose of carrying out scientific observations on the lines of Welsh and Glaisher in England, and of Tissandier in France, von Siegsfeld had constructed a balloon fourteen metres in diameter.—Dr. Assmann described some ice-filaments which he had observed in a valley of the Harz Mountains during a three days' frost in October.

Physiological Society, November 9.—Prof. du Bois-Reymond, President, in the chair.—Dr. Virchow described, as based on his own original researches, the development of the cylindrical epithelium of the yolk-sac, thus amplifying the communication made at the previous meeting of the Society on the development of the blood, blood-vessels, and yolk-sac of the chick. He distinguished seven stages in the development of the epithelium, each of which he separately described and explained by means of drawings, diagrams, and preparations, which he exhibited.—Dr. Martius made a communication on new experiments which he had carried out with a view to refuting certain attacks which had been made on his investigation of cardiograms as laid before the Society in the previous year. It may be remembered that the speaker had connected the curves traced by the impulse of the apex of the heart on a rotating cylinder with the various phases of a cardiac cycle, by auscultating the heart, and recording each sound he heard by a mark on the same rotating drum. Since he had assured himself that he could record the above without any irregularity due to his own reaction-time, he considered that the point on the cardiogram which corresponded with the mark for the first sound of the heart was synchronous with the closure of the auriculo-ventricular valves: the point corresponding to the mark indicating the second sound of the heart he regarded as coincident with the closure of the semilunar valves. Objections were made to the above method, and the possibility of the observer's reaction-time not being zero was left out of account. In answer to these, Dr. Martius has now made some new experiments with a pendulum beating seconds which closed an electric circuit as it passed through the vertical: the current thus produced attracted an armature, made a clapper-like noise, and recorded the instant of its production on a rotating drum. A second circuit was arranged for momentary closure by the observer, yielding a recording mark on the same drum: this circuit was closed regularly each time the clapper was heard, and the two marks made in the way indicated above coincided to within differences of time of at most 0·03 of a second, an exactitude which amply sufficed for the investigation of the cardiograms. It may be remarked here, as a matter of theoretical interest, that the above is not a case really involving reaction-time, but only the recording of known signals which are repeated rhythmically at regularly recurring intervals. With these rhythmic stimulations the duration of the interval is usually *estimated*, and in such cases the variations in the time-value of the estimate are mostly greater than those just quoted. When, however, after some time the rhythm of the stimulations has been fully grasped by the observer, then the interval is no longer estimated, but the record is made with an exactly similar rhythm.

STOCKHOLM.

Royal Academy of Sciences, November 14.—Musci Asiæ borealis, first part, Livermosses, by Prof. G. O. Lindberg and Dr. H. W. Arnell.—The disguise of the *Decapoda oxyrrhina* through special adaptations of the structure of their body, by Dr. Carl Aurivillius.—On the bladders of the Fucaceæ, by Dr. N. Wille.—Mineralogical notes, by Herr G. Flink.—On the journey of Dr. Nansen on the inland ice of Greenland, by Baron Nordenskiöld.—Contributions to the knowledge of the Cestodean worms which occur in Sweden, by Herr E. Lönnberg.—On a monstrous individual of *Cottus scorpius*, by Herr E. Nyström.—Annotations of some Scandinavian Pyrenomycetæ, by Herr K. Starbäck.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Meteorological Observations at Stations of the Second Order for the year 1884 (Eyre and Spottiswoode).—A Sketch of the First Principles of Physiography: J. Douglas (Chapman and Hall).—Rocks and Soils: H. E. Stockbridge (Trübner).—The Floating Island in Derwentwater: G. J. Symons (Stanford).—Prodromus of the Zoology of Victoria, Decade xvi.: F. McCoy (Trübner).—The Principles of Cancer and Tumour Formation: W. R. Williams (Bale).—The World's Inhabitants: G. T. Bettany (Ward, Lock).—A Treatise on Hydrodynamics, vol. ii.: A. B. Basset (Bell).—Results of Observations at Stonyhurst College Observatory, 1887: Rev. S. J. Perry.—Morphologisches Jahrbuch, 14 Band, 3 Heft (Williams and Norgate).

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