THURSDAY, JANUARY 8, 1891.

THE SOUTH KENSINGTON AND PADDINGTON SUBWAY RAILWAY.

WE print elsewhere a letter from General Webber, in which he begins by stating that the land at South Kensington which is under the control of the Exhibition Commissioners, was originally, to use the words of the late Prince Consort,

"At no distant date to form the inner court of a vast quadrangle of public buildings, rendered easily accessible by the broad roads which surround them, buildings where science and art may find space for development, with the air and light which are elsewhere well nigh banished from this overgrown Metropolis."

If General Webber had been merely a company-promoter he would never have supplied us with so apt a quotation, telling so dead as it does against his underground railway; but the fact is, his appreciation for science has unconsciously led him to give the scientific rather than the Stock Exchange view of the matter. By all means, we urge, let the destiny foreshadowed by the late Prince Consort be fulfilled, especially as regards the "buildings where science may find space for development."

Again, General Webber's sincerity compels him to be quite silent about the disastrous disturbance that his railway, if ever constructed along the route now proposed, would cause to the pursuit of scientific investigation in the various present laboratories in Exhibition Road. The only remedy he can suggest is that "laboratories and observatories for original research used for the instruction of a very few experts may have to find a home elsewhere." Now, even this remedy he would never have advanced had he realized that his "very few experts" are in actual fact some hundreds of students, all of whom are now daily using the very apparatus, and making the very experiments, that his railway would render impossible. And since many of these students are being trained to become teachers, the education now given to hundreds daily in Exhibition Road is indirectly the teaching of thousands throughout Great Britain.

But we will go farther and say that, even if the interests of large numbers of present and future students were not at stake, and even if General Webber's contention were correct, that the work of only a few experts would be stopped by his railway, that alone should compel the railway to seek another route. For the work of these few experts at South Kensington, some of whom are rapidly advancing pure science, and others of whom are as rapidly advancing applied science, will produce a far wider effect on the future of this country than the utilization or non-utilization of the existing subway by the South Kensington and Paddington Railway Syndicate.

Towards the end of his letter General Webber feels bound, however reluctantly, to act the true "company-promoter," and so he refers to "the steady annual decline in the records of the numbers of visitors to the permanent institutions," as an argument to show the necessity of a new mode of locomotion. Probably this reference to "the steady annual decline" is only a quotation from the prospectus of the "South Kensington and Paddington Subway Company," and so ought to be taken with that liberal seasoning of salt appropriate to prospectuses of new companies. For, as a matter of fact, "the steady annual decline in the records of the numbers of visitors" is disposed of at once on examining the records themselves, which give the following figures:—

Number of visitors to the South Kensington Museum, the Natural History Museum, the Indian Museum, and the Museum of Scientific Apparatus.

During	1886	1,206,741
,,	1887	1,146,590
,,	1888	1,270,027
,,	1889	1,235,854
,,	1890	1,187,142

from which we see that the numbers during the last two years together were actually larger than the numbers during the first two together.

General Webber says that by his proposed railway "easy and rapid communication will also be afforded to hundreds of thousands of persons living to the north of Hyde Park to visit and use all the great institutions collected at South Kensington." Would not the communication be just as easy and just as rapid if the railway went down Queen's Gate, and connected the Gloucester Road Station on the District Railway with Paddington? For the main entrances to the Albert Hall, the Natural History Museum, and the Imperial Institute, are just as near Queen's Gate as Exhibition Road. The South Kensington Museum is no doubt on the Exhibition Road side of the space, but to balance this the Exhibition of Machinery and Inventions is on the Queen's Gate side of the area in question.

The public will be equally well served along whichever of the two alternative sides of the Albert Hall the railway goes. But, while an untimely selection of the eastern, or Exhibition Road, route would mean a disastrous stoppage of the scientific work now being carried on, the trains might run along the western, or Queen's Gate, route without causing practically any magnetic or mechanical disturbance to the delicate instruments in use at the existing laboratories.

We shall probably be answered that, unless the present subway (the length of which, as a matter of fact, is but one-tenth of the whole length of the proposed railway) be utilized, the District Railway and General Webber's syndicate will not make quite so much money out of the scheme. Possibly not; but is the London home of scientific research to be destroyed merely to enable General Webber's syndicate to pay extra dividends?

"All attempts hitherto made to complete the work" (that is, the subway) "as far as the Albert Hall have failed," says General Webber. May his present scheme, the ill-omened child of many failures, say we, walk in the footsteps of its fathers.

ARE THE EFFECTS OF USE AND DISUSE INHERITED?

Are the Effects of Use and Disuse Inherited? An Examination of the View held by Spencer and Darwin. By William Platt Ball. (London: Macmillan and Co., 1890.)

THE question which constitutes the title of this essay still continues—and is likely for some time to continue—the most important question in the field of

Darwinian thought. On the one side we have the school of Weismann, which answers the question with an unequivocal negative. On the other side we have the writings of Darwin himself, which entertain the so-called Lamarckian factors as subsidiary to natural selection, or as lending considerable aid to natural selection in carrying out the work of adaptive evolution. Again, we have the writings of Herbert Spencer, which attribute a still higher proportional value to the Lamarckian factors; while, lastly, we have the self-styled neo-Lamarckians, who regard these factors as of even more importance than natural selection. Amid so great a conflict of opinions on a matter of such extreme importance, any survey of the actual evidences in favour of the Lamarckian factors cannot fail to be opportune, even though the value of such an attempt must depend upon the care and the judgment with which it is undertaken. Now, we are glad to say that the essay before us is, in all respects, as admirable as it is opportune. Scientific in spirit, and logical in execution, it deals with its subject in a manner at once concise and exhaustive. Restricting his ground for the most part to the domain of fact, Mr. Ball has made a full inventory of the cases, or classes of cases, which have hitherto been adduced in evidence of the transmission of acquired characters, and briefly weighs the value of the evidence in each. In the result he concludes that there is no real evidence for any of the cases; and as his work is throughout performed in a thoughtful and painstaking manner, we deem it the most instructive contribution which has hitherto appeared upon the subject of which it treats. Of course the writings of Galton and Weismann present the greater merit of having been the first publicly to challenge the doctrines of Lamarck; but this they did on grounds of general reasoning, and by viewing the evidences of those doctrines, as it were, *en masse*. The merit of Mr. Ball's work, on the other hand, consists in its detailed analysis of each of the facts and arguments which have ever been brought forward in support of what he conveniently calls "use-inheritance."[1] He thus restricts himself to the one question of fact, whether or not there is any good evidence of the transmission of acquired characters, without embarking upon any general theory of heredity. And, as already remarked, he has done this purely analytical work in an exceedingly able manner. So much, indeed, is this the case, that we can find but little to say in the way of criticism; and that little must take the form of pointing out the particular cases where it seems to us that his examination is not quite so thorough as it usually is.

He begins by taking *seriatim* "Spencer's examples and arguments," and the first of these is "diminution of the jaws in civilized races." Here he shows that "cessation of the process by which natural selection favoured strong thick bones during ages of brutal violence might bring about a change in this direction;" and he points to the simultaneous thinning of the skull, &c., as virtual proof that such is the true explanation. Nevertheless, in the next section, which treats of "diminished biting muscles of lap-dogs," he invokes the principle of artificial selection to explain the facts, thus: "The conscious or unconscious selection of lap-dogs with the least tendency to bite, would easily bring about a general enfeeblement of the whole biting apparatus—weakness of the parts concerned favouring harmlessness" (pp. 12, 13). But surely, if the cessation of selection is sufficient to account for the diminution of the biting apparatus "in civilized races of mankind," it is no less capable of explaining a similar diminution in the case of "lap-dogs." If artificial selection has taken any part in the matter at all, it must have done so in the direction of "favouring harmlessness" by acting on instincts or dispositions rather than on jaws and muscles (see NATURE, vol. xxxvi. p. 405).

In opposing Spencer's argument drawn from the facts of co-adaptation, Mr. Ball appears somewhat unduly to assume the character of a special pleader. The argument is, that the more the utility of any co-ordinated set of parts depends upon their co-ordination, the more difficult does it become to see how natural selection alone could have produced the mechanism. For if the mechanism be such that its utility depends on all its parts simultaneously co-operating, no utility can arise unless all the parts are developed simultaneously in the same individuals. Now, Spencer represents that the chances must be enormously against an accidental occurrence of many concomitant changes in any single individual, where such changes are thus without benefit to the individual save when they do occur in combination; while, of course, the inherited effects of use and of disuse of all the parts concerned would explain their simultaneous evolution. Mr. Darwin answered this argument by showing how natural selection might be held to operate in the production of these effects; but in doing so he continued to attribute a probably large share of the work to the Lamarckian (*i.e.* Spencerian) factors. Now, Mr. Ball, in quoting Darwin's opinion, ignores this latter point. For instance, after giving it as Darwin's view "that natural selection alone 'would have sufficed for the production of this remarkable quadruped'" (*i.e.* the giraffe), he omits the conclusion of Darwin's sentence, viz. "but the prolonged use of all the parts together with inheritance will have aided in an important manner in their co-ordination." Again, while referring to what Darwin has said touching another of Spencer's examples—the elk—Mr. Ball omits to notice the sentence: "Although natural selection would thus tend to give to the male elk its present structure, yet it is probable that the inherited effects of use, and of the mutual action of part on part, have been equally or more important."

In bringing together all the cases adduced by Darwin to support the theory of use-inheritance, Mr. Ball begins by observing that

> "he [Darwin] appears to have acquired the belief in early life, without first questioning and rigorously testing it, as he would have done had it originated with himself. In later life it appeared to assist his theory of evolution in minor points, and in particular it appeared absolutely indispensable to him as the *only* explanation of the diminution of disused parts in cases where, as in domesticated animals, economy of growth seemed to be practically powerless. He failed to adequately notice the effect of panmixia, or the withdrawal of selection, in causing or allowing degeneracy and dwindling under disuse."

[1] This term is coined by him as equivalent to Darwin's "inherited effects of use and disuse," to Spencer's "inheritance of functionally produced modifications," to Weismann's "inheritance of somatogenetic characters," &c.

These remarks are very judicious, as likewise are those where, further on, he alludes to the important distinction between selection as merely withdrawn (panmixia) and as actively "reversed" (through economy of growth, &c.). For it is shown that, if Darwin had not thus failed adequately to notice the former principle without reference to the latter, he could not have continued to regard our domesticated animals as furnishing any conclusive proof of use-inheritance; or, indeed, any better evidence than is furnished by animals in a state of nature. Having satisfied himself that there can be no economy of growth in our highly-fed domesticated animals, seeing that parts may reappear in them which are obsolete in their parent or undomesticated types, he concluded that, where other parts presented diminution, the fact could not be attributed to a reversal of selection. Therefore he attributed it to the inherited effects of disuse as to the only imaginable alternative. But, as Mr. Ball—following Weismann and others—plainly shows, the mere cessation of selection, without any reversal of selection, is in all such cases bound to produce the effects observed; with the consequence that the former principle "invalidates Darwin's strongest evidence for use-inheritance."

Passing from such general remarks on Darwin's attitude of mind with respect to the question of use-inheritance, Mr. Ball deals separately and exhaustively with all the particular instances that Darwin has given. The criticism here is uniformly good, and as regards some of the cases—*e.g.* "wings and legs of ducks and fowls"——remarkably so. But as we have no space to consider these numerous instances in detail, it must be enough to say, in general terms, that Mr. Ball has certainly reduced their evidential value to very small dimensions. Even the results of Brown-Séquard's experiments on the apparent transmission of injuries are shown to be of a more doubtful character in relation to the question of use-inheritance than anybody else has hitherto indicated.

There are, however, two or three remarks which seem worth making on Mr. Ball's treatment of some of Darwin's examples. Thus, in considering the "larger hands of labourers' infants" as compared with those of infants belonging to the upper classes, he attributes the phenomena to "sexual selection in the gentry." And in many other cases, both in man and the lower animals, he shows how the apparent effects of use-inheritance may be due to this cause. Such, of course, is a very reasonable position for Mr. Ball to adopt; but what is to be said about all such cases by the school of Wallace, which rejects the theory of sexual selection as well as that of use-inheritance? These cases, of course, are cases where the theory of natural selection cannot be applied; and therefore it would seem that the school of Wallace must either confess them inexplicable, or else devise some additional theory for the purpose of explaining them.

This allusion to the views of Mr. Wallace leads to a consideration of an important argument recently published in his "Darwinism"—namely, that, even if use-inheritance be physiologically possible, it can never be allowed to act, inasmuch as natural selection will always effect the required alterations more rapidly than they could be effected by use-inheritance. The only answer to this argument appears to be, as Mr. Ball puts it, "that slight changes in each generation need not necessarily be matters of life and death to the individual, although their cumulative development by use-inheritance might eventually become of much service." This answer he disposes of by adding: "But selection would favour spontaneous variations of a similarly serviceable character." This, however, does not appear to meet the requirements of the case. For the whole point of the answer to Mr. Wallace's argument lies in the consideration that "slight changes in each generation need not necessarily be matters of life and death to the individual," *i.e.* that the amount of change due to use-inheritance may be so small *in each successive generation* as not to make any appreciable difference in the struggle for life. Hence this amount of change need not be in any degree "serviceable," although, by an accumulation of such changes in the same line of change, a high degree of serviceability may be attained without the necessary aid of selection. Of course, this answer to Mr. Wallace's argument can only go upon the supposition on which the argument itself is founded—namely, that use-inheritance is physiologically possible; and therefore both the argument and the answer are irrelevant to the question of such possibility as raised by Weismann.

Once more in the same section of this essay—namely, that which is concerned with Darwin's examples of use-inheritance—Mr. Ball repeatedly adduces the case of neuter insects as demonstrative evidence against use-inheritance. But he does not consider the possibility of the instincts of neuters being survivals from a time when all the female insects of a hive were both fertile and industrious. He quotes, indeed, an absurd passage from Büchner upon this subject, but does not allude to what Perrier has said. We are far from maintaining that Perrier has made out his case; but we think that the question raised by him ought to have been gone into by Mr. Ball. Moreover, in the event of there being a second edition of his essay, it is to be hoped that Mr. Ball will consider the ingenious suggestion with regard to these instincts which has just been published by Prof. Lloyd Morgan in his admirable treatise on "Animal Life and Intelligence," pp. 440–42.

The concluding part of the treatise is devoted to "Miscellaneous Considerations." These all appear to us both apposite and cogent, except the sections which argue that use-inheritance, even if it were physiologically possible, would prove of more harm than good in the matter of adaptive evolution. In the first place, there was no necessity for Mr. Ball to propound such a question—his aim elsewhere being to show that, as a matter of fact, there is no evidence of use-inheritance having ever been in operation. In the next place, this foreign and superfluous argument is not well sustained; for although Mr. Ball shows that in many instances use-inheritance would be an "evil," he not only disregards the vastly greater number of cases in which it would helpfully co-operate with natural selection, but he also disregards the important consideration that in all cases the "minor factor" would require to be under control of the "major factor"—with the result that, where harmful, its effects would not be allowed to develop.

Although we have thus devoted considerable space to a review of this little book, we regret our inability to devote more. For, having restricted ourselves to points where criticism seems possible, we must have failed to give a sufficient idea of all the rest of the essay. In conclusion, then, we must add that Mr. Ball's analysis as a whole appears to us to stagger the theory of use-inheritance more seriously than ever it has been staggered before; and, therefore, that no one who henceforth writes upon the subject can afford to disregard his treatment of the question, "Are the Effects of Use and Disuse Inherited?" George J. Romanes.

TECHNICAL EDUCATION.

Manual Training in Education. By C. M. Woodward, A.B., Ph.D. (W.U.), &c. With Illustrations. (London: Walter Scott, 1890.)

THIS is in some respects a valuable work, and therefore it is the more to be regretted that its title is in a double sense misleading. A very little reflection would have taught the author that "manual," as from *manus*, the hand, means anything which hands are capable of effecting. He, however, defines "manual training" as limited to teaching and learning the use of tools and working materials; these, according to his system, being limited in turn to wood and metal work, such as turning, carpentry, and smithing. The immense range of work of which children are capable is not included, therefore, in "Manual Training." Again, we find in the book that the education in question not only does not include that of girls, but actually takes no note of young children of either sex. "Starting with boys in their teens," Prof. Woodward shows, what has always been well known, that such boys can be taught the rudiments of certain "trades." But as the very great majority of children leave school early in their teens to go into active life, what parents or the public chiefly wish to know is, what manual training can be imparted to all children while they are yet at school? Extensive experiment has perfectly shown that they can be taught to draw, model, and execute much useful art work even from six years of age, and what is of more importance is that, as one hour of sleep before midnight is worth two after it, so those who learn to draw in early childhood acquire a certain dexterity and skill such as is rarely, if ever, attained after thirteen years of age. "The proper mental maturity," says Prof. Woodward, "rarely comes before the fourteenth year. I think of the class as about fifteen years old." "The minimum rate of admission" (to my school) "is fourteen years"—meaning, we suppose, that no younger pupils are received. If this means anything, it is that, according to the author, manual training in education should not begin till "the proper mental maturity" is attained. But what we expect from education is that pupils shall be trained *before* their minds are matured.

There are many persons who will buy this work under the impression that it will teach all the details of manual training. But, in fact, of its 310 pages only 77 are devoted to practical instruction in drawing, wood and iron work. These are truly excellent of their kind, so much so that we cheerfully wish *si sic omnes*—"would that all were like it"—since in that case we should have had a work of practical use, although there is at present no lack of admirable if not better hand-books for such training in England. In the remaining part of the book Prof. Woodward advances theories which no man of culture and intelligence can admit, and which imperatively call for refutation, since at present the British public in its bewilderment as to "technical education" is being extensively deluded by them.

It has been said by some journalist that the aim of the Socialists seems to be to make of all society a well-organized poor-house. It is not in the least an exaggeration to say that Prof. Woodward's idea of education is that every male in a community shall be, first of all, a mechanic. He admits, it is true, a moderate amount of culture in literature and other branches, but exacts that *three hours a day* shall be given to drawing and manual labour, while with boys above fourteen it may be more. That is to say, three hours in school, with two hours of home study, are to be given to mathematics and book-keeping, science (*i.e.* geography, zoology, botany, chemistry, physics, physiology) and literature (which is to include "some choice specimens of modern prose and poetry"), and *one* foreign language—French, German, or Latin. But it is very evident that Prof. Woodward considers that in all cases, with every pupil, the manual labour should be the most thoroughly taught, and that carpentry and metal work are of paramount importance. The main force or tendency of all education should be to form a mechanic, and give to every youthful mind the habit of regarding all things from a mechanic's point of view.

Now, while this is a commendable education for a blacksmith, and while we may admit that it amuses and pleases youth, it is evident "on the face of it" that the literary course prescribed is utterly inadequate to *properly* prepare pupils for any career above that of the workshop. And this tone pervades the whole book: "there's nothing like leather" appears on its every page. The author quite forgets, what is also left out of sight by most of our own reformers in education, that, absolutely necessary as it may be to educate the majority to become mechanics, the world requires a very respectable number of professional, literary, and really scientific men, who could not be properly trained for such pursuits on one language, even with a knowledge of "some choice specimens of *modern* prose and poetry."

It is true that the author admits that, when "a lack of mechanical interest or power" manifests itself, the lad should unquestionably be sent to his grammar and dictionary rather than to the laboratory and drafting-room." That is to say, when the bit of wood is fit for nothing else we may make a god of it. And a master mechanic is to decide as to who shall thus take the back seats in education, and occupy the inferior positions of men of science and literature!

There is a class of boys, according to Prof. Woodward, "who are so constituted that their controlling interests are not in the study of words, the forms of speech, or the boundless mass of information which is given in books." These appear to be his favourites. "The claims of this class of boys," he asserts, "have been set forth by no one so eloquently as by General Francis A. Walker." It may interest the reader to see what is regarded as surpassing eloquence by one who prescribes the limits of all literary education.

"It not infrequently happens that the boy who is regarded as dull because he cannot master an artificial system of grammatical analysis, who isn't worth a cent for giving a list of the kings of England, who doesn't know and doesn't care what are the principal productions of Borneo—has a better pair of eyes, a better pair of hands, a better judgment, and even by the standards of the merchant, the manufacturer, and the railroad president, a better head than his master."

The reader has possibly heard such brilliant eloquence at a certain class of popular meetings, where the assertion that the ignorant and book-hating man is the cleverest in the community is always received with cheers. Prof. Woodward himself asks if there "may not be something mischievous in the power of attention to certain book-learning, as shown by the tendency of bookish people to dislike manual labour, and sometimes to become bad citizens?" It is evident enough that "bookish people" do not stand high in his graces. He assures us that his manual education is carried on in the interest of rational intellectual training and culture, but we confess that we fail to see it. These extracts indicate exactly what his standard of "culture" must be. Indeed, he declares that his system, far from being too narrow, is rather too broad.

We cordially believe that Prof. Woodward is an accomplished, skilful, very earnest and successful teacher of wood and metal work to boys over fourteen or fifteen years of age, but we have rarely met with a writer who needed more the caution not to go beyond his last. His work is to all intents and purposes inspired with the belief that all schools should be like his own, and all education for all classes be based on the very limited mechanical training with which he is familiar. That it is admirable to a certain degree, but most inadequate to all the demands of a really good education, is apparent on every page. As he handles all his adversaries without gloves, sparing no one who does not believe his method of education to be perfection, we have the less scruple in setting forth the truth regarding it in plain words. Singular as it may seem, not only to him, but to a great number of reformers, the education of the future will require a much higher stimulant or a far better basis than mere mechanical drawing, hammering, and filing.

OUR BOOK SHELF.

Monographie der baltischen Bernsteinbäume: Vergleichende Untersuchungen über die Vegetationsorgane und Blüthen, sowie über das Harz und die Krankheiten der baltischen Bernsteinbäume. Von H. Conwentz. Pp. 151, mit achtzehn lithographirten Tafeln. (Danzig. London: Williams and Norgate, 1890.)

DR. CONWENTZ was long associated with Goeppert and Menge in the investigation of the "Amber Flora," and indeed wrote the whole, or nearly the whole, of the second volume of the "Flora des Bernsteins," comprising the angiospermous fossils found in Baltic amber. Engaged upon a third volume of this work, devoted to cryptogamous or spore-bearing plants, Dr. Conwentz became impressed with the necessity of first working out the relationships of the trees which yielded the resin now found in a fossil state and known as amber, and those who know the admirably executed plates and exhaustive text of the two volumes issued of "Die Flora des Bernsteins" will not be disappointed with this companion volume.

We cannot attempt a critical review of this work, and must therefore be content with giving the result of Dr Conwentz's investigations, as set forth by himself. During the Tertiary period the division or distribution of land and water, especially in Europe, was very different from what it is at the present time; and in Eocene times, the beginning of this period, the Scandinavian continent extended southward nearly to the Samland district of northern West Prussia and Mecklenburg, and supported a vegetation whose principal types now characterize the flora of the southern parts of the temperate zone and the northern subtropical zone. Evergreen oaks and beeches flourished, associated with palms, laurels, magnolias, and camellias; also the amber trees and various kinds of cypress.

Chief among these amber trees were four species of *Pinus*, not one of which was very closely related to *Pinus sylvestris*, the characteristic fir or pine-tree of the region of the present period. One of the species, with leaves or needles in pairs, recalls some of the North American species belonging to the section *Parrya;* a second resembles the Japanese *Pinus Thunbergii;* and a third, having the leaves in fascicles of five, is near *P. Cembra* and the Japanese *P. parviflora.* In addition there was a kind of spruce more nearly allied to *Picea ajanensis*, of the extreme east of Asia, than to the silver spruce of Germany.

It is probable, Dr. Conwentz believes, that all these different trees and shrubs did not form a mixed forest, but were peculiar to separate regions, and then the amber trees formed dense forests interspersed here and there only with other trees. Dr. Conwentz pictures the conditions under which he imagines these amber trees existed and discharged the resin now so universally employed. The trees he assumes were exposed to the ravages of almost numberless enemies, animal and vegetable, insomuch that there was scarcely a sound tree in the forests. The trees thus enfeebled would be swept down or mutilated by violent storms, causing an abnormal exudation of resin. It seems, however, hardly necessary to insist on the general unhealthiness of the trees to account for the large deposits of resin. Clouded amber is said to be due to the presence of cell-sap in the resin. The majority of the plates illustrate the destruction of the tissues by various fungus parasites. W. B. H.

The Birth and Growth of Worlds. By A. H. Green, M.A., F.R.S. (London: Society for Promoting Christian Knowledge, 1890.)

THIS little work is the extension of a lecture delivered by Prof. Green. It contains an account of various cosmical theories from 1684, when Thomas Burnet, D.D., a learned divine, expounded his "Sacred Theory of the Earth," to recent times. The researches of Prof. Lockyer on the constitution of the heavenly bodies are well described; as are also the hypotheses of Kant and Laplace. A list is given of the chief works bearing on the subject of the lecture. This, in conjunction with the descriptive text, and a few well-chosen illustrations, renders the book extremely useful as a popular short exponent of the many attempts that have been made to fathom the origin of celestial species.

Chambers's Encyclopædia. New Edition. Vol. VI. (London and Edinburgh: W. and R. Chambers, Ltd., 1890.)

THE present volume of the new edition of Chambers's well-known "Encyclopædia" takes in words ranging from "Humber" to "Malta." In every respect it is up to the level of the preceding volumes; and, as usual, scientific subjects have been entrusted to thoroughly competent writers. Under "Hydrophobia" M. Pasteur sketches his discoveries and practices in regard to rabies, while Mr. J. Arthur Thomson contributes "a brief unargumentative review of current adverse criticism." There is an excellent article on insanity by Dr. T. S. Clouston. Prof. James Geikie writes on igneous rocks and other subjects; Prof. C. G. Knott on hydrodynamics and terrestrial magnetism; Dr. J. Anderson on lake dwellings; Dr. Alfred Daniel on light, lenses, and magnetism; Mr. R. T. Omond on lightning; and Mr. F. E. Beddard on the lion and the leopard. Iceland is described by M. Hjaltalín; India by Sir Richard Temple; the Indian Ocean by Dr. John Murray; the geography of Italy by Mr. W. D. Walker; and Madagascar by the Rev. J. Sibree.

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.*]

Shaking the Foundations of Science.

MY attention has been drawn to the article in NATURE under the above title, and, as it presents to your readers the proposed South Kensington and Paddington Subway in an aspect which the writer erroneously describes as "the hosts of Mammon threatening the domains of science," I feel sanguine that a scientific journal such as yours will be even more than ordinarily desirous to have the correct view set before them.

The writer appears to ignore the fact that the rectangular area bounded by Kensington, Cromwell, Exhibition, and Prince Albert (now Queen's Gate) Roads forms part of the estate purchased out of the surplus funds of the Great Exhibition of 1851, and vested in a body of Commissioners by Royal Charter; and that this land was originally destined, to use the words of the late Prince Consort, "At no distant day to form the inner court of a vast quadrangle of public buildings, rendered easily accessible by the broad roads which surround them, buildings where science and art may find space for development, with the air and light which are elsewhere well nigh banished from this overgrown Metropolis."

The site of the Exhibition of 1862, and of the buildings of the South Kensington and Science and Art Departments, originally formed part of that estate.

When the Royal Albert Hall was first projected, a pneumatic railway from it to the South Kensington Station *viâ* Exhibition Road formed part of the original scheme, which was promoted by the Commissioners who own the estate, and the paramount necessity of such a means of communication has never been lost sight of.

When the existing subway was constructed in 1885, its continuance as far as the Albert Hall was a recognized part of the project, and the route alongside of the Eastern Arcades was then decided upon by the Commissioners. When the District Railway Company obtained their Act of Parliament for the subway, its use with traction was sanctioned.

The proposal now before Parliament is for absolutely no more and no less, so far as the estate of the Commissioners of 1851 is concerned, than the completion of that subway, with nearly its present form and dimensions, and with almost the same powers to use it in the same way.

All attempts hitherto made to complete the work as far as the Albert Hall have failed, and the existing incomplete portion of the subway is practically closed, and is a dead loss financially.

At last a practical solution of the difficulty has been found, with the additional advantage that, besides the means of covered access being afforded from South Kensington Station through the length of the estate, easy and rapid communication will also be afforded to hundreds of thousands of persons living to the north of Hyde Park to visit and use all the great institutions collected at South Kensington, to which covered access will thus further be secured from all parts of the metropolis.

In his eagerness to prove that the present means of access are all that can be desired, your contributor refers to the congregations of visitors attracted by former Exhibitions. The steady annual decline in the records of the numbers of visitors to the permanent institutions is not the only evidence which entirely refutes such a misleading line of argument.

I do not dispute for one moment that Urania must have her quiet retreats secluded from the movements and throng of men and vehicles, and it is hardly surprising that, as your contributor states, the repose she loves cannot even now be found at South Kensington.

But it is pure affectation to contend that the interests of the highest scientific education for the many will suffer, because laboratories and observatories for original research used for the instruction of a very few experts may have to find a home elsewhere.

If the vibration caused by omnibuses by day and market carts by night, passing in the Kensington and Cromwell Roads at each end of Exhibition Road, to say nothing of "small and earlies," are found already to affect the ultra-sensitive nerves of Urania, the question may be asked why the Professors continue to countenance the expenditure of the "vast sums" mentioned by your contributor, and also, if it is right that any more public money should be allowed to be spent in a way which experience has already shown to be injudicious.

It appears, therefore, evident that the degree to which the traffic in the proposed subway might augment this inconvenience does not affect the true merits of the case as set up by your contributor. C. E. WEBBER.

17 Egerton Gardens.

The Darkness of London Air.

THE interesting article on the above subject which appeared in your issue of December 18 (p. 152) is one of those periodical reminders that Englishmen—as if not content with the innumerable climatic ills to which they are unhappily heirs—are yearly endeavouring their "level best" to make city life more and more polluted and noxious. It is apparently in vain that they are constantly shown how injurious to human comfort in every way, how fatal to plant life, and how destructive to architecture, is this accumulation of unburnt fuel in the air; neither, strange to say, are they more heedful of the enormous annual waste, sheer waste, of fuel: to all arguments and expostulations they oppose stolid apathy. Seeing how hopeless it is to induce any active interest in this question among the inert mass of citizens, your contributor very sensibly suggests an appeal to the County Council to take up the matter. My object in writing is to offer a further suggestion on the *modus operandi*. Why not invite the Councils of all our learned and scientific Societies and institutions to combine in a memorial to either the County Council or Parliament direct, asking for legislation on this smoke question, and for simple machinery to enforce such legislation? I cannot but think that such a memorial, signed by, *e.g.*, the Royal Society, the Chemical, Linnean, Astronomical, and the various other Societies, by the College of Surgeons, the Royal Academy, and in fact by the representatives of all such corporations would carry very great weight. F. H. P. C.

A Remarkable Flight of Birds.

I HAVE not noticed any reference to the extraordinary flight of birds that was observed in many parts of Devon on the morning of December 21, after the first heavy fall of snow took place at the beginning of the present severe weather. At eight o'clock on Sunday morning I was astonished at a continuous stream of skylarks flying overhead in a westerly direction. The flight continued for more than an hour after that in the most astonishing numbers. Over 500 were counted in three minutes, and the cloud of birds seemed endless in every direction. An old farmer here said that he had seen a similar thing about ten years ago. The birds then were found on the estuaries, and by the sea-coast of Cornwall, where they died by thousands. Several letters have appeared in the local papers announcing a similar migration on the same morning, so that there must have been millions of birds on the wing. One correspondent mentions other birds, thrushes and blackbirds, &c., as well, but here I saw only skylarks. I have seen no record of their destination. It would be interesting to know if any of your readers could tell us where the birds went. They were all flying towards Cornwall. I observed also large detached flocks of plover, flying towards Dartmoor, on the edge of which I live, in a southerly direction. The appearance of these birds all hasting away in perfect silence was almost weird in the dead stillness, all the ground and every twig and bush being covered with deep snow, and not a breath of wind stirring.

The event has certainly justified their instincts, for until to-day, January 1, it has been almost impossible for the birds to obtain any food, except from the berries, which this year are exceptionally plentiful.

Large flocks of fieldfares have taken possession of my garden, where there are a great many hollies, and at any noise they rush out of the bushes like a swarm of flies. It is curious to watch them from the windows in the morning, some ten or a dozen sitting in the snow under the bushes, mere dejected heaps of feathers, occasionally pecking at the berries that their busy comrades have knocked off. The thrushes are in the wildest excitement. They sit above the hollies, quivering and chattering, and occasionally darting upon a luckless fieldfare, whose unwonted presence they resent most strongly. I do not know how these birds discover the berries. It cannot be by their colour, for there are two large hollies within ten yards of each other; one of them was for days full of birds constantly flying past the other, which was almost a mass of brilliant red berries. One tree was almost stripped bare, and the birds all went to an adjoining field for three days. Then one morning I found them in the remaining bush, which they speedily stripped as bare as the rest.

E. C. SPICER.

Throwleigh Rectory, Devon, January 1.

On the Flight of Oceanic Birds.

THE oceanic soaring birds undoubtedly take advantage of the air-currents as sailing-vessels do, trimming their wings so as to be acted on by the wind to the best advantage. I have frequently observed their flight in high southern latitudes, and have seen the sooty albatross sail round, and up and down, for ten minutes and more with never a flap of the wings; but with a pair of binoculars the tail, head, and portions of the wings could be seen to move slightly with each change of direction or elevation.

There are two well-marked ways of flying, as follows: (*a*) when the bird is flying with the wind blowing sideways on to it, as represented in Fig. 1, where it will be seen the whole of the

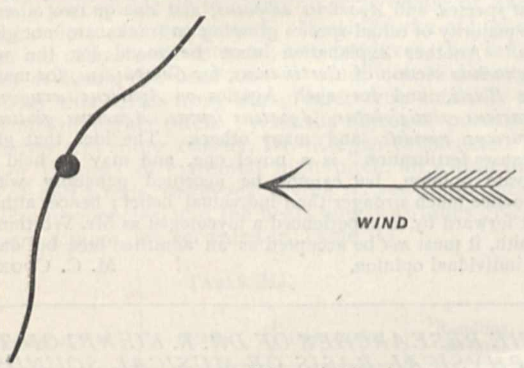


FIG. 1.—"On a wind."

under surface is exposed to the wind; and (*b*) when the bird is flying either directly towards or from the direction of the wind, as in Fig. 2, in this position a slight movement of the tail sending the bird up or down, and of the wings (not a flap) altering the direction.

Roughly speaking, the area of wing surface exposed to the wind is about 3 square feet, and the weight of the bird 7 pounds (sooty albatross). In all these true oceanic birds the wings are long and narrow, and the birds appear to have great power over the movements of the different joints.

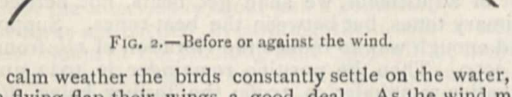


FIG. 2.—Before or against the wind.

In calm weather the birds constantly settle on the water, and when flying flap their wings a good deal. As the wind makes and increases, the flaps become less in number, until the birds sweep round and under the stern of the ship in immense circles at the rate of 20 to 30 miles per hour, in most cases as represented in Fig. 3, at the part marked (*a*) close to the sea surface, and at (*b*) high in the air.

It is a common thing in high winds to see them apparently motionless for some time over the mast-heads, the ship at the same time going through the water at a speed of 10 or 11 knots per hour.

I would not pretend in any way to explain their marvellous flight, but one thing should be remembered, that the velocity of the air certainly increases from the surface of the sea to the altitude to which they attain, viz. about 200 feet; and that

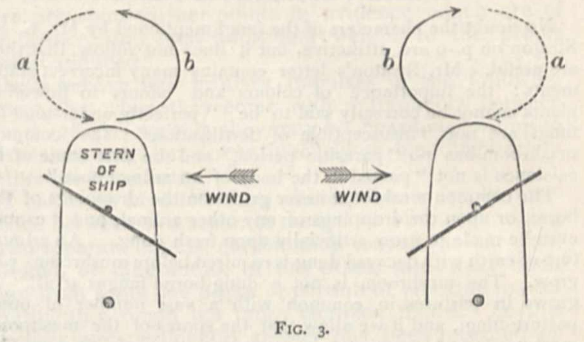


FIG. 3.

when the wind is strong the sea is thrown into considerable waves, which may affect the horizontal movement of the wind in immediate contact with them. There certainly seems to be some connection between the way the wings are spread to the wind's direction, the sliding up and down in their sweeping circle (Fig. 3), and their method of flight.

DAVID WILSON-BARKER.

The Locomotion of Arthropods.

I HAVE been making some observations on the locomotion of various insects, and find that in the case of those which move quickly the best method for observation is instantaneous photography. Instantaneous photographs of moving flies show that they move the front and hind leg of one side almost simultaneously with the middle leg of the other, while they stand on the other three. When the tripod which is moving has come to the ground, the other tripod is raised, and so on. The photographs show, however, that while no leg of one tripod ever moves simultaneously with any leg of the other, yet there is a succession in the movements of the legs of each tripod. The hind leg on one side is first moved, then the middle on the other, and when the hind leg has been moved forward and almost reached the ground, the front leg of that side is raised. The middle leg and the front leg of the opposite sides come to the ground almost simultaneously. It is usually just when the hind leg is reaching the ground, and the front leg is being raised, that the tripod on which the fly is resting thrusts the body forward. After the movement of each tripod there appears to be a short pause, during which all six legs are on the ground together.

I have observed this "tripodic" walk in earwigs, water scorpions, aphides, and some beetles. In the case of some slowly moving beetles and aphides which can be observed without photographic means, quite irregular movements have been observed. By cooling aphides, they can be made to move very slowly. In this condition one was observed to move its legs in slow succession in the following order: (1) right hind, (2) right middle, (3) right front, (4) left hind, (5) left middle, (6) left front. This walk was continued for some time, occasionally interrupted by the following order, or some other quite irregular walk: (1) right hind, (2) right middle, (3) left hind, (4) left middle, (5) left front, (6) right front.

In caterpillars the legs forming a pair seem to move simultaneously; the motion begins at the posterior end of the body, and proceeds regularly forward till the most anterior pair of legs are moved.

The above few observations, which were made in the Physical Laboratory of Trinity College last spring, formed the subject of a recent communication to the Dublin University Experimental Association. I hope to be able to extend this application of photography to the other groups of Arthropoda.

H. H. DIXON.

Physical Laboratory, Trinity College, Dublin.

P.S.—In taking the photographs a small camera with magnifying lens and fast shutter were used, and the analyses of the motions arrived at by the comparison of a large number of "snap" pictures. The insect was in each case placed on a white ground within a shallow box covered with glass and illuminated by oblique sunlight. In this way the disposition of the shadows became of great service in determining the positions of the legs.

Attractive Characters in Fungi.

No doubt the characters of the fungi mentioned by Mr. C. R. Straton on p. 9 are attractive, but it does not follow that they are useful. Mr. Straton's letter contains many incorrect statements: the importance of colours and odours to flowering plants cannot be correctly said to be "perfectly understood"; fungi are not "insusceptible of fertilization"; the common mushroom has no "parasitic period," and the first stage of its existence is not "passed in the body of an animal host."

The common mushroom never grows on the droppings of the horse, or upon the droppings of any other animal, and it cannot even be made to grow artificially upon fresh dung. An admixture of earth with decayed dung is required before mushrooms will grow. The mushroom is not a dung-borne fungus at all. It grows in pastures in common with a vast number of other pasture-fungi, and if we allow that the spores of the mushroom must needs first germinate in an animal stomach, it would be but reasonable to assume that the spores of other pasture-fungi would have a similar habit. The mycelium so commonly seen on horse dung is not the mycelium of the mushroom; it belongs to various dung-borne *Coprini*, and it is certainly not necessary for the spores of these *Coprini* to pass through a horse's intestinal canal, as they will readily germinate, not only on and in dung and its juices, but on wet blotting-paper, and on almost any other non-corrosive damp and warm material.

Mr. Straton seems to have a remarkable idea of the nature of a "parasite"; because an animal swallows spores, therefore the spores on being swallowed become "parasites." He might as reasonably describe the seeds eaten by graminivorous animals as "parasites," or the seeds of the strawberry and raspberry, when eaten with the pulp by man, as "parasites" in the human system.

As Dr. Cooke, in his interesting letter on p. 57, has not mentioned one special character of certain fungi which has greatly interested me, I may venture to advert to it here. I refer to *gluten*. Many fungi, especially Agarics, are highly glutinous, and if this gluten is not "attractive," it is, I think, useful.

Agaricus radicatus is one of our commonest stump-fungi; it has, when young, a *perfectly dry* pileus. As the fungus reaches maturity, the pileus becomes *glutinous*. The change reminds one of the change in the condition of the stigma in flowering plants. If sections are made through the pileus of *A. radicatus* in different stages of growth, it will be found that as maturity is gradually reached the centre of the pileus softens and swells, and through the now soft mass an enormous number of cystidia protrude themselves. The cystidia exactly resemble the cystidia of the gills. The cystidia open at the apex, and gluten exudes through the open mouths on to the cap of the fungus. If the sticky material from a mature example is microscopically examined, it will be found more or less full of germinating spores, which have been wafted on to the sticky pileus by the wind. The gluten possibly aids germination, if it does not—as I believe it does—cause fertilization.

Agaricus mucidus has a highly glutinous pileus, and it grows in such a fasciculose and closely imbricated manner on beech trunks, that all the spores from the upper examples fall of necessity on to the thick gluten of the examples immediately underneath. If the gluten is microscopically examined, it will be found full of the germinating spores of *A. mucidus*. The uses of the gluten here seem obvious. The simple spores of an Agaric might have but a poor chance of effectually germinating and growing upon a beech trunk, but if the spores had previously germinated and formed a mycelium in a highly glutinous material, they would have a fairly good chance. The sticky pileus would at length be blown from the beech trunk by the wind, and its mucidous character would cause it to stick to the first trunk or branch it might be blown against, and so the gluten and its living mycelium would become attached to a fresh host. *A. mucidus* is peculiar to beech woods.

I could extend these notes to other gluten-bearing fungi, as *A. adiposus*, &c., as well as to Agarics with glutinous stems; but the two examples above given may serve as a hint to other observers.

How have truffles acquired their subterranean habit? They are always eagerly eaten by pigs, squirrels, rats, and other animals when they grow near the surface, and the deeper placed examples alone survive. Would the constant destruction of tubers near the surface at length bring about a subterranean habit? WORTHINGTON G. SMITH.

Dunstable.

UNDOUBTEDLY there are numerous glutinous fungi, and the coating of gluten has a useful purpose. Whether Mr. Worthington Smith has quite apprehended the nature of that purpose may be an open question. The majority of the species in the genus *Hygrophorus* are glutinous, and this genus, as a whole, is about the latest in its time of appearance in the autumn. It is very suggestive to observe them apparently unharmed by frost, whilst the Agarics have collapsed, and are in rapid decay. That the glutinous coating is in this instance a protection from frost can scarcely be denied. Dr. Quelet, the French mycologist, has stated that in the Vosges some species of *Hygrophorus* do not appear until the early frosts have commenced, and he has borne testimony to the fact that they flourish in frosty weather without apparent injury. Both the species of Agaric to which Mr. Smith alludes, *Agaricus mucidus* and *Agaricus radicatus*, may be found late in the season, apparently indifferent to the frost, which affords a suspicion that the glutinous coating is a protection from frost. *Agaricus carbonarius* is viscid, but much more so as cold increases, and for two consecutive years we have watched it growing uninjured far into January, when no other Agaric could be seen. We do not contend that the "useful purpose" is in *all* cases a protection from frost, because in some early species we imagine it serves primarily as a protection against evaporation. Presuming that Agarics may contain more than 80 per cent. of water, such a protection would be of service to species growing in exposed situations. If Mr. Smith's suggestion as to *Agaricus mucidus* is accepted, it can only apply to that species, and *Agaricus adiposus*, and one or two others, as the majority of tufted species growing on trunks are not glutinous. Another explanation must be found for the whole *Myxacium* section of *Cortinarius*, for *Gomphidius*, for many of the *Boleti*, and for such Agarics as *Agaricus æruginosus*, *Agaricus semiglobatus*, *Agaricus lentus*, *Agaricus glutinosus*, *Agaricus roridus*, and many others. The idea that gluten "causes fertilization" is a novel one, and may be held as a private opinion, but cannot be accepted generally without evidence much stronger than individual belief; hence, although put forward by so experienced a mycologist as Mr. Worthington Smith, it must *not* be accepted as an admitted fact, but only as an individual opinion. M. C. COOKE.

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

II.

SO far we have been dealing with primary beats and beat-tones; but there are also secondary beats and secondary beat-tones, which are produced by the interference of primary beat-tones. An example of a secondary beat is afforded by the following experiment. Recurring to the preceding table of experiments, it may be observed that when the two shrill notes, ut_6, sol_6, giving the interval of the fifth, are sounded together, the inferior and superior beat-tones are both present, and of the same pitch. If, now, one of the two forks is lightly loaded with pellets of wax to put it out of adjustment, we shall get beats, not between the primary tones, but between the beat-tones. Suppose we add enough wax to reduce the vibration of sol_6 from 3072 to 3070. Then the positive remainder is 1022, and the negative remainder is 1026; the former being ut_5 flattened two vibrations, the latter the same note sharpened to an equal amount. As a result there will be heard four

[1] By Prof. Silvanus P. Thompson. (Communicated by the author, having been read to the Physical Society of London, May 16, 1890.) Continued from p. 203.

beats per second—secondary beats. Similarly, the intervals 2 : 5, 2 : 7, if slightly mistuned, will, like the fifth, yield secondary beats. Or, to put it in another way, there may be secondary beats from the (mistuned) beat-tones that are related (as in our experiment) in the ratio 1 : 1, or in the ratios 3 : 4, 3 : 5, &c., and even by those of 1 : 2, 4 : 5, 4 : 7, and so forth.

I have given you an example of secondary beats: now for an example of a secondary beat-tone. This is afforded by one of the previous experiments, in which were sounded ut_6 and the 11th harmonic of ut_3. In this experiment, as in that which followed with the 13th harmonic, two (primary) beat-tones were produced, of 768 and 1280 vibrations respectively. These are related to one another by the interval 3 : 5. If we treat these as tones that can themselves interfere, they will give us for their positive remainder the number 256, which is the frequency of ut_4. As a matter of fact, if you listen carefully, you may, now that your attention has been drawn to it, hear that note, in addition to the two primary tones and the two beat-tones to which you listened previously.

In von Helmholtz's "Tonempfindungen," he expresses the opinion that the distinctness with which beats are heard depends upon the narrowness of the interval between the primary tones, saying that they must be nearer together than a minor third. But, as we have seen, using bass sounds of a sufficient degree of intensity and purity, as is the case with those of the massive forks, beats can be heard with every interval from the mistuned unison up to the mistuned octave. Even the interval of the fifth, ut_1 to sol_1, gave strongly-marked beats of 32 per second. When this number is attained or exceeded, the ear usually begins to receive also the effect of a very low continuous tone, the beats and the beat-tone being simultaneously perceptible up to about 60 or 70 beats, or as a roughness up to 128 per second. If, using forks of higher pitches but of narrower interval, one produces the same number of beats, the beat-tone is usually more distinct. Doubtless this arises from the greater true intensity of the sounds of higher pitch. With the object of pursuing this matter still more closely, Dr. Kœnig constructed a series of 12 forks of extremely high pitch, all within the range of half a tone, the lowest giving si_6 and the highest ut_7. The frequencies, and the beats and beat-tones given by seven of them, are recorded in Table III.

TABLE III.

Frequencies of Forks.	Ratio.	Beats (Calcd.)	Resulting Sound.
ut_7 4096 and si_6 3840 ...	16 : 15	256	ut_3
,, 3968 ...	32 : 31	128	ut_2
,, 4032 ...	64 : 63	64	ut_1
,, 4048 ...	256 : 253	48	sol_{-1}
,, 4056 ...	512 : 507	40	mi_{-1}
,, 4064 ...	128 : 127	32	ut_{-1}
,, 4070 ...	158 : 157	26	—

The first of these intervals is a diatonic semitone; the second of them is a quarter-tone; the third is an eighth of a tone; nevertheless, a sensitive ear will readily detect a difference of pitch between the two separate sounds. The last of the intervals is about half a comma.

These forks are excited by striking them with a steel hammer. Some of the resulting beat-tones will be heard all over the theatre; but, in the case of the very low tones of 40 and 32 vibrations, only those who are close at hand will hear them. The case in which there are 26 beats is curious. Most hearers are doubtful whether they perceive a tone or not. There is a curious *fluttering* effect, as though a tone were there, but not continuously.

We have seen, then, that the beat-tones correspond in pitch to the number of the beats; that they can themselves interfere, and give secondary beats; and that the same number of beats will always give the same beat-tone irrespectively of the interval between the two primary tones. What better proofs could one desire to support the view that the beat-tones are caused, as Dr. Young supposed, by the same cause as the beats, and not, as von Helmholtz maintains, by some other cause? Yet there are some further points in evidence which are of significance, and lend additional weight to the proofs already adduced.

Beats behave like primary impulses in the following respect, that when they come with a frequency between 32 and 128 per second, they may be heard, according to circumstances, either discontinuously or blending into a continuous sensation.

It has been objected that, whereas beats imply interference between two separate modes of vibration arising in two separate organs, combination-tones, whether summational, or differential, or any other, must take their origin from some one organ or portion of vibratile matter vibrating in a single but more complex mode. To this objection an experimental answer has been returned by Dr. Kœnig in the following way. He takes a prismatic bar of steel, about 9 inches in length, and files it to a rectangular section, so as to give, when it is struck at the middle of a face to evoke transversal vibrations, a sound of some well-defined pitch. By carefully adjusting the sides of the rectangular section in proper proportions, the same steel bar can be made to give two different notes when struck in the two directions respectively parallel to the long and short sides of the rectangle. A set of such tuned steel bars are here before you. Taking one tuned to the note $ut_6 = 2048$, with $re_6 = 2304$, Dr. Kœnig will give you the notes separately by striking the bar with a small steel hammer when it is lying on two little bridges of wood, first on one face, then on the other face. If, now, he strikes it on the corner, so as to evoke both notes at once, you immediately hear the strong boom of $ut_3 = 256$, the inferior beat-tone. If Dr. Kœnig takes a second bar tuned to ut_6, and $si_6 = 3840$, you hear also ut_3, this time the superior beat-tone. If he takes a bar tuned to ut_6 and the 11th harmonic of ut_3 (in the ratio 8 : 11), you hear the two beat-tones sol_4 and mi_5 (in ratios of 3 and 5 respectively) precisely as you did when two separate forks were used instead of one tuned bar.

Dr. Kœnig goes beyond the mere statement that beats blend to a tone, and lays down the wider proposition that any series of maxima and minima of sounds of any pitch, if isochronous and similar, will always produce a tone the pitch of which corresponds simply to the frequency of such maxima and minima. A series of beats may be regarded as such maxima and minima of sound; but there are other ways of producing the effect than by beats. Dr. Kœnig will now illustrate some of these to you.

If a shrill note, produced by a small organ-pipe or reed, be conveyed along a tube, the end of which terminates behind a rotating disk pierced with large, equidistant apertures, the sound will be periodically stopped and transmitted, giving rise, if the intermittences are slow enough, to effects closely resembling beats, but which, if the rotation is sufficiently rapid, blend to a tone of definite pitch. Dr. Kœnig uses a large zinc disk with 16 holes, each about 1 inch in diameter. In one set of experiments this disk was driven at 8 revolutions per second, giving rise to 128 intermittences. The forks used were of all different pitches from $ut_3 = 256$ to $ut_7 = 4096$. In all cases there was heard the low note ut_2 corresponding to 128 vibrations per second. In another series of experiments, using forks ut_2 and ut_3, the number of intermittences was varied from 128 to 256 by increasing the speed, when the low note rose also from ut_2 to ut_3.

From these experiments it is but a step to the next, in which the intensity of a tone is caused to vary in a periodic manner. For this purpose Dr. Kœnig has constructed a siren-disk (Fig. 1), pierced with holes arranged at equal distances around seven concentric circles; but the sizes of the holes are made to vary periodically from small to large. In each circle are 192 equidistant holes, and the number of maxima in the respective circles was 12, 16, 24, 32, 48, 64, and 96. On rotating this disk, and blowing from behind through a small tube opposite the outermost circle, there are heard, if the rotation is slow, a note corresponding to the number of holes passing per second and a beat corresponding to the number of maxima per second. With more rapid rotation two notes are heard—a shrill one, and another 4 octaves lower in pitch, the latter being the beat-tone. On moving the pipe so that wind is blown successively through each ring of apertures, there is heard a shrill note, which is the same in each case, and a second note (corresponding to the successive beat-tones) which rises by intervals of fourths and fifths from circle to circle.

These attempts to produce artificially the mechanism

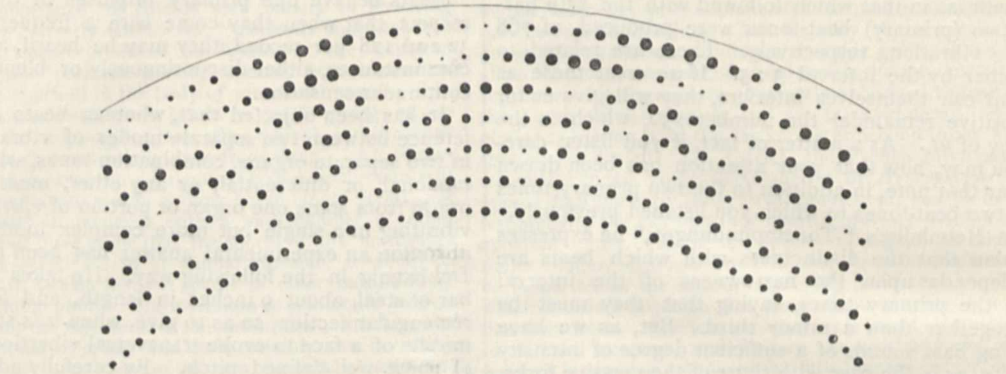


FIG. 1.

of beats were, however, open to criticism; for in them the phase of the individual vibrations during one maximum is the same as that of the individual vibrations in the next succeeding maximum; whereas in the actual beats produced by the interference of two tones the phases of the individual vibrations in two successive maxima differ by half a vibration; as may be seen by simple inspection of the curves corresponding to a series of beats. When this difference was pointed out to Dr. Kœnig, he constructed a new siren-disk (Fig. 2), having a similar series of holes of varying size, but spaced out so as to correspond to a difference of half a wave between the sets. With this disk, beats are distinctly produced with slow rotation, and a beat-tone when the rotation is more rapid.

Finding this result from the spacing out of apertures to correspond in position and magnitude to the individual wavelets of a complex train of waves, it occurred to Dr. Kœnig that the phenomena of beats and of beat-tones might be still more fully reproduced if the edge of the disk were cut away into a wave-form corresponding precisely to the case of the resultant wave produced by

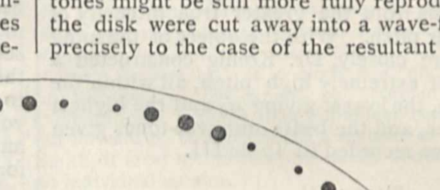


FIG. 2.

the composition of two interfering waves. Accordingly, he calculated the wave-forms for the cases of several to the form of the desired wave. Two such wave-disks, looking rather like circular saws with irregular teeth, are

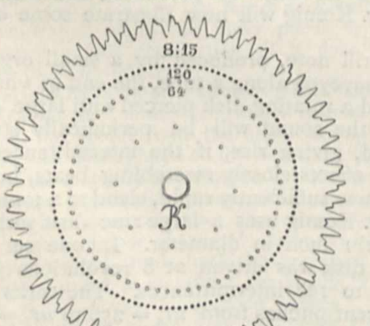


FIG. 3.

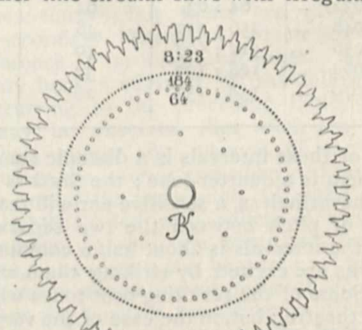


FIG. 4.

intervals, and, having set out these curves around the periphery of a brass plate, cut away the edge of the plate depicted in Figs. 3 and 4. These correspond to the respective intervals 8 : 15 and 8 : 23. A number of such wave-disks corresponding to other intervals lie upon the table; these two will, however, suffice. In the first of these the curve is that which would be obtained by setting out around the periphery a series of 120 simple sinusoidal waves, and a second set of 64 waves, and then compounding them into one resultant wave. In order to permit of a comparison being made with the simple component sounds, two concentric rings of holes have been also pierced with 120 and 64 holes respectively. Regarding these two numbers as the frequency of two primary tones, there ought to result beats of frequency 8 (being the negative remainder corresponding to the superior beat). An interior set of 8 holes is also pierced, to enable a comparison to be made. To experiment with such wave-disks they are mounted upon a smoothly running whirling-table, and wind from a suitable wind-chest is blown against the waved edge from behind, through a narrow slit set radially. In this way the air-pressures in front of the wave-edge are varied by the rush of air between the teeth. It is a question not yet decided how far these pressures correspond to the values of the ordinates of the curves. This question, which involves the validity of the entire principle of the wave-siren cannot here be considered in detail. Suffice it to say that for present purposes the results are amply convincing.

The wave-disk (Fig. 3) has been clamped upon the whirling-table, which an assistant sets into rotation at a moderate speed. Dr. Kœnig blows first through a small pipe through one of the rows of holes, then through the other. The two low notes sound out separately, just a major tone apart. Then he blows through the pipe with a slotted mouth-piece against the waved edge; at once you hear the two low notes interfering, and making beats. On increasing the speed of rotation the two notes become shrill, and the beats blend into a beat-tone. Notice the pitch of that beat-tone: it is precisely the same as that which he now produces by blowing through the small pipe against the ring of 8 holes. With the other wave-disk, having 184 and 64 holes in the two primary circles, giving a wave form corresponding to the interval 8 : 23, the effects are of the same kind, and when driven at the same speed gives the same beat-tone as the former wave-disk. It will be noted that in each of these two cases the frequency of the beat-tone is neither the difference nor the sum of the frequencies of the two primary tones.

To be continued.)

DR. HENRY SCHLIEMANN.

THE death of Dr. Schliemann comes on his friends not only as a sorrow but as a surprise, for though he had tried his constitution, his strength was so great that it seemed equal to many more labours. He was essentially a self-made man, not merely as the architect of his own fortunes, but as having at an early age deliberately adopted certain purposes in life, and having acomplished them with astonishing success. These deliberate purposes made his career brilliant, and his character manly and sturdy.

In the preface to "Ilios" (1880) Schliemann gives a sketch of his early life and his excavations which reads like a romance. Before he was ten years old, he had made up his mind that the mighty walls of Troy could not have entirely disappeared, but must have been only buried by the dust of ages, and that he would himself some day bring them to light. But a romantic boyhood ended in a bitter struggle with poverty, until at one time he had to sell his last coat, and after shipwreck arrived at Amsterdam a penniless outcast. Obtaining there a post worth £32 a year, he spent the half of that sum on living, and the other half on self-education, his dinner costing him twopence, and a fire being an unknown luxury. By sheer business talent he rose from this poverty to great wealth; but this has been done by hundreds of uninteresting men: the interesting thing about Schliemann is that he never looked on wealth save as a means for accomplishing his darling purposes. While never ceasing to pray that some day he might have the happiness of learning Greek, he began on modern languages, which he mastered one after another in an incredibly short space of time, learning not merely to read, but to write and speak fluently, Swedish, Polish, Russian, Arabic, and many other tongues.

In 1871, he began his career as an explorer by an attack on the hill of Hissarlik, where he expected to find the remains of the Troy of his early dreams. In the art of excavation he seems to have had no teacher, and there can be no doubt that he has opened a new era in that art through his scientific genius. Literary genius may be erratic and incalculable, but genius works in science through clear discernment of means and by infinite pains. Schliemann's method was simple—to remove all the earth of a site and pass it through a sieve, taking care that, though hundreds of hands were at work, every one worked as an immediate organ of his own intelligence and design. The quality of insight came in principally in the choice of sites.

Splendid as were the results of Schliemann's excavations at Hissarlik, at Mycenæ, and at Tiryns, both as regards the recovery of antiquities, and as regards the advancement of knowledge, scholars recognize that he was not to be followed in his interpretation of his own discoveries. His individuality was too strongly marked, his imagination too fervent, to allow him to walk safely in the narrow ways of archæological science. And as a man who was born a fighter, and thoroughly carried out the old rule as to loving one's friends and hating one's enemies, he could not be impersonal in the choice of theories. To the judgment of his allies he often gave way with a delightful simplicity and modesty, but to attack he was impervious. One of his most interesting appearances in London was when he came in hot haste from Athens, accompanied by Dr. Dörpfeld, at the invitation of the Hellenic Society, to meet in open debate the objections, which he characteristically called "calumnies," which some archæologists had brought forward against his theory in regard to the prehistoric palace at Tiryns. Though Schliemann was a decided believer in Providence, there was in him an immense force of tough old Teutonic heathenism.

Yet he was a man of the world, and cosmopolitan in a sense in which few can claim to be so, for in almost any country he could have made himself at home as an active citizen. His ideal was Greece, and he succeeded in a very difficult task by finding in modern Greece the materials for a splendid ideal, when worked up with traditions of ancient glory. In this respect he was a greater poet than Byron, who spoke of some modern Greeks as "craven crouching slaves." Athens has indeed good reason for the gratitude which has assigned him a public grave at Colonus. At Athens he will leave a great gap. His palace, reflecting in every corner his career and his enthusiasms, was a place where the most open hospitality was accorded to men of all nations. In the living-rooms were Homeric texts; the servants bore high-sounding Greek names; the basement rooms were full of the spoils of Troy; while on the summit stood replicas of celebrated Greek statues. The host poured forth the simplicity of his heart and the overflowings of his enthusiasm, talking with an unconventional plainness sometimes disconcerting to Western ladies. Not even Madame Schliemann, long as she has shared her husband's labours and pursuits, can fill the blank which he has left. All archæologists must regret that his purposes of further excavation at Hissarlik, in Crete, and elsewhere, have dropped, or at any rate must be carried out by others who have not his marvellous power of finding valuable remains which others have failed to find.

THE METEORITE OF OSCHANSK.

A METEORITE which has lately been added to the collections of the Natural History Museum of Paris is described in *La Nature* by M. Stanislas Meunier; and by the courtesy of the editor of that journal we are enabled to reproduce the figures by which the article is illustrated. The meteor, of which this is a fragment, fell in Russia, on August 18 (30), 1887. Before it fell it was seen by various persons in the south-western part of the Government of Perm, and in the Government of Viatka—principally in the districts of Perm, Oschansk, Kungur, Osoo, and Sarapul. Between Perm and Oschansk, according to an inhabitant, it appeared about 12.30 p.m. in a clear sky, leaving behind it an almost horizontal train of great brilliancy. Detonations were heard, resembling a discharge of musketry rather than thunder. A little afterwards it fell in a shower of incandescent stones, which buried themselves more or less deeply in the earth. They were very numerous, and weighed from one to 330 kilogrammes. Fig. 1 represents the meteor as it was seen by M. Selivanof, a professor of the seminary of Perm. This observer writes:—

"On August 18, a little before one o'clock p.m., I returned to the seminary. The weather was calm, and the sky covered with small fleecy clouds. Just as I was about to cross the threshold, I happened to look towards the

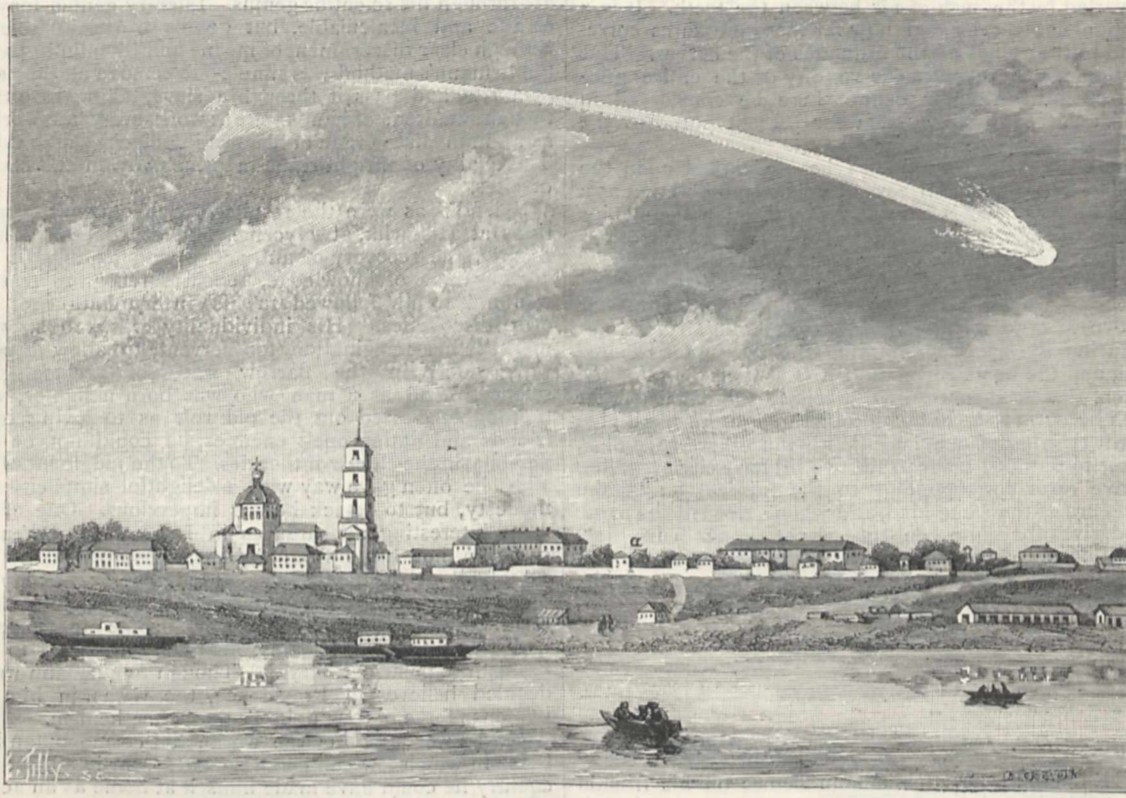


FIG. 1.—Passage of the meteorite of Oschansk, above Perm. *a*, the point of observation.

south, and saw a brilliant body like a shooting-star, or, rather, like a piece of iron glowing at the forge, gliding from east to west in a direction almost horizontal or slightly inclined towards the earth. The meteor made scarcely more noise than a rocket, and at first I believed it was one. Its course was sufficiently rapid, and during two or three seconds I followed the bolide over the space of a small number of degrees. It left a luminous train, which was very rapidly extinguished. Perhaps this train resulted simply from the persistence of the luminous impression upon the retina. The case, however, was otherwise with a pale nebulous band, which persisted about five minutes."

The majority of the meteorites brought by this fine meteor have certainly been lost. Only six of them have been found—five at Oschansk, one at Tabor. At the moment of the fall, M. Nagibine was in a street of Oschansk, and heard the noise which announced it. About half a minute after the cessation of this noise, he observed a blackish stone which hissed through the air as a cannon-ball might have done. Several workmen ran and found the meteorite at the bottom of a hole—about 50 centimetres deep—which it had hollowed in the ground. It was as large as a child's head, and was still hot; it weighed 1·790 kg.

At Tabor the phenomenon was noticed by two peasants who were working in a field. Surprised by the detonations and the rumbling sound, they looked up, and saw the bolide, of a dark red colour, followed by a white smoke, which the wind agitated; and sending forth an odour of sulphur (Fig. 2). The mass seemed to be at a height of about 200 metres, and by the shock of its fall raised a column of dust. One of the peasants, who was mounted on a corn-rick, was thrown to the ground by the atmospheric disturbance. At the place where the stone fell they found a hole 4·20 metres deep, 2·10 metres wide (Fig. 3). The stone was too hot to be removed immediately. Next day it was taken out in fragments, one of which weighed 98 kilogrammes, the others from 100 grammes to 11 kilogrammes. There were about 82 kilogrammes of *débris*. The entire weight of this single stone is estimated at 328 kilogrammes.

From information furnished by the observers it is concluded that the bolide moved from 10° to 15° from

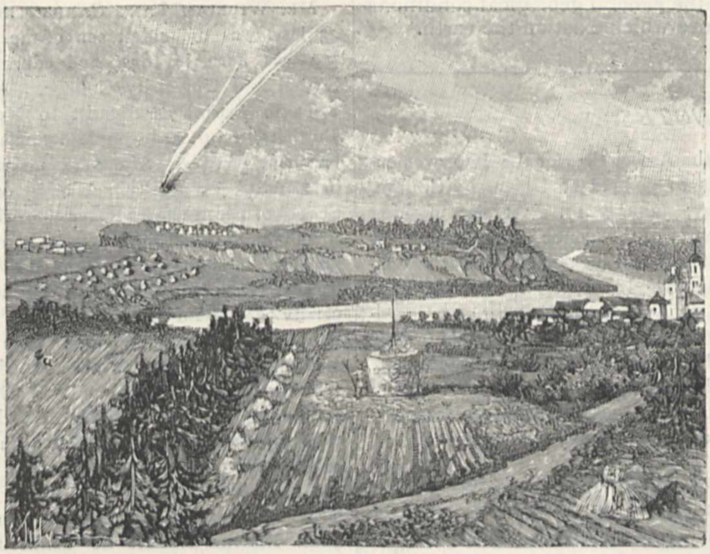


FIG. 2.—Passage of the meteorite at Tabor.

the east towards the south, and that, from the point of observation in the field, the angle of the fall appeared to be about 55° above the horizon.

Another stone, which has not been found, fell into the River Kama, at Tabor. A forester who was near the spot, says that the banks of the river trembled, and that the water, after having been thrown up in a high column, continued to bubble for a long time. The air was so violently agitated that a troop of 50 or 60 horses, which were drinking at the river, were thrown to the ground.

When the fragments found at Tabor were pieced together, the stone presented the form of a polyhedron with

FIG. 3.—Meteorite of Tabor, after its fall. *a*, its largest fragment (98 kg.); *b*, part taken away by the public; *v*, grey part; *g*, fragment weighing two kilogrammes; *d*, small fragments.

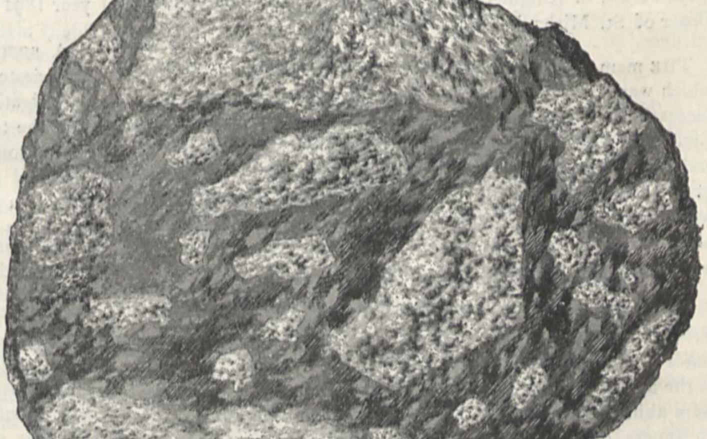


FIG. 4.—Fragment of the meteorite of Oschansk (in the Museum of Natural History, Paris).

angles very much blunted. It was enveloped in the usual black crust, but this was exceptional in presenting flaws as big as a pea, or even larger.

Fig. 4 represents the appearance of the specimen in the Paris Museum. M. Stanislas Meunier has made it a subject of chemical and microscopical study, which has led him to the conclusion that the meteorite of Oschansk belongs to a lithological brechiform type, which he described twenty years ago under the name of "Canellite." To the same type belonged, among others, the meteorites of La Baffe (Vosges), September 13, 1842; of Assam (India), 1846; of Canellas (Spain), May 14, 1861; of

Feid-Chair (Algeria), August 16, 1876. The stone consists of fragments of two very oolithic rocks, one called "Limerickite," of a dark violet colour, the other, called by M. Meunier "Montréjite," quite white. From the juxtaposition of different rocks in this as in some other meteorites, M. Meunier argues that, in the *milieu* where these stones are formed, the general geological conditions must be analogous to those which exist on the earth.

NOTES.

THE general meeting of the Association for the Improvement of Geometrical Teaching is to be held at University College, Gower Street, on Saturday, January 17. At the morning sitting (10.30 a.m.), the reports of the Council and Committees will be read, and the new officers will be elected. On the conclusion of the elections Miss Wood will read a paper "On the use of the term 'Abstract' in Arithmetic." After an adjournment for luncheon at 1 p.m., members will re-assemble for the afternoon sitting (2 p.m.). Papers will be read by Prof. Minchin, on "Another Voyage to Laputa"; by Mr. E. T. Dixon, on "The Foundations of Geometry"; and by Mr. E. M. Langley, on "Some Notes on 'Statics and Geometry.'"

PROF. RUDOLF VIRCHOW will reach his seventieth birthday on October 13. It has been decided that the occasion shall be commemorated by the striking of a large gold medal in his honour.

SEVERAL men of science were included in the list of New Year honours. A baronetcy was conferred on Dr. Richard Quain, and Prof. George Humphry, Cambridge. Prof. Ball Director of the Museum of Science and Art, Dublin, was made a C.B.; Dr. Theodore Cook, Principal of the College of Science, Poona, became a Companion of the Most Eminent Order of the Indian Empire; and Mr. Frederick McCoy, C.M.G., Professor of Natural Science in the University of Melbourne, was promoted to be an ordinary member of the second class, or Knight Commander of the Most Distinguished Order of St. Michael and St. George.

THE memorial concerning the ancient monuments of Egypt, which was lately presented to Lord Salisbury, has been so far successful. It is to be forwarded to H.M.'s Agent and Consul-General at Cairo, for presentation to the Egyptian Government; and Sir E. Baring will be instructed to state that if an official inspector is appointed the question of his nationality will not be considered important, the only desire of the British Government being that adequate steps shall be taken "to preserve the monuments from further destruction or mutilation."

WE regret to have to record the death of Mr. John Marshall, F.R.S., President of the General Medical Council, and Professor of Anatomy to the Royal Academy. He died on January 1, at the age of seventy-two. Mr. Edward Bellamy, whose studies were akin to those of Mr. Marshall, died on the 4th inst. after an illness of only three days. He had been for many year lecturer on artistic anatomy at the South Kensington School.

PROF. CASEY, F.R.S., Fellow of the Royal University of Ireland, died on January 3, at the age of seventy. He was an eminent mathematician, and much regret at his death has been expressed in Ireland.

A CIRCULAR letter from the Societa Italiana di Scienze Naturali of Milan, dated January 2, announces the death of their President, the Cav. Abate Antonio Stoppani, Professor of Geology in the R. Istituto Tecnico Superiore. The President died on New Year's Day, at the age of sixty-six.

PROF. W. J. STEPHENS, President of the Linnean Society of New South Wales, died on November 22, 1890. At the meeting of the Society on November 26, resolutions were passed, expressing high appreciation of his services and sympathy with his family.

MR. WILLIAM LANT CARPENTER, who died on December 23 last, was the eldest son of the late Dr. W. B. Carpenter. He was born in 1841, and educated at University College School and University College. At the age of eighteen he went to Bristol as chemist to Messrs. C. J. Thomas and Brothers, soap and candle manufacturers, in which firm he became a partner. Afterwards he gave up business, and settled in London, eventually becoming one of the managers of the School of Electrical Engineering in Hanover Square. For some time past he has been widely known, more especially in the north of England, as one of the most successful lecturers in connection with the Gilchrist Educational Trust and the University Extension movement. In 1885 he brought out "A Treatise on the Manufacture of Soap and Candles, Lubricants, and Glycerin"; and he contributed a few papers on kindred subjects to the Journal of the Chemical Society, and the Proceedings of the British Association. Under the title "Energy in Nature" he published in 1883 a popular exposition of the doctrine of the conservation of energy, being the substance of six lectures delivered for the Gilchrist Educational Trust two years previously; and in conjunction with the late Prof. Balfour Stewart he contributed some papers to the Proceedings of the Royal Society on the periodic changes of sun-spots, and their connection with terrestrial and magnetic phenomena.

ON January 2 the Institution of Civil Engineers reached its seventy-third anniversary. The numbers of the various classes now on the books comprise 1700 members, 2880 associate members, 427 associates, 19 honorary members, and 927 students—together 5953.

THE Calendar of the Department of Science and Art for the year 1891 has been issued.

A SPECIAL Commission has been for some years established at Trieste by the Austrian Government for the oceanographic investigation of the Ionian and Adriatic Seas. During the past year the transport *Pola* has been set apart for the study of the sea-bottom, currents, temperature, &c., of the Adriatic, and has obtained important results. At a depth of 2000 metres abundance of a green Alga, *Halosphæria viridis*, was found.

THE marine laboratory of the Johns Hopkins University is to be opened next spring.

A PHOTOMICROGRAPHIC laboratory has been opened at Rimini by Count R. Sernagiotto, for the preparation and publication of photographic reproductions. Among the microphotographs already issued, *Notarisia* mentions, as of remarkable clearness and beauty, one of *Pleurosigma angulatum*, 5000 diams., of *Bacillus radiciformis*, 1000 diams., and a tangential section of the stem of the vine, 75 diams.

THE American National Educational Association will hold its next meeting at Toronto in July. Many teachers of the Dominion have become members, and *Science* says that "they will meet in Toronto in full force, and will prepare an exhibit giving a complete view of Canadian systems of education."

UNDER the auspices of the Penzance Natural History and Antiquarian Society and the Committee of the Penzance Library, a fund is being raised for the purpose of erecting a suitable monument or tombstone in Penzance Cemetery, as a memorial of the late Mr. John Ralfs, who died July 14, 1890. Subscriptions should be sent to Mr. W. Bolitho, Jun., Penzance, Treasurer of the Ralfs Fund.

DR. JULIUS WORTMANN, one of the editors of the *Botanische Zeitung*, has been appointed Director of the physiological experimental station of the Royal Institution for instruction in fruit- and vine culture at Geisenheim on the Rhine.

THE Trustees of Columbia College, U.S.A., have appointed as Curator of their Herbarium Dr. Thos. Moring, who has just returned from a botanical exploration of southern South America with very large and valuable collections.

THE Horace Mann School, Boston, Mass., recently celebrated its twenty-first anniversary. The Hon. Gardiner G. Hubbard delivered an interesting address on the occasion, sketching the history of the articulating system for the deaf in America. In 1860, Mr. Hubbard's daughter, who was then four years old, lost her hearing through a severe illness. Mr. Horace Mann and Dr. Howe had been studying the various methods of educating the deaf in Europe, and had reported in favour of the oral system which had for some time been practised in Germany. Mr. Hubbard therefore consulted Dr. Howe, who recommended that the child's relatives should talk to her, and teach her to recognize the spoken words of their lips. He especially urged that they should not have recourse to signs, and that they should persistently refuse to understand them. The plan was tried, and the result was so successful that a little school for the deaf, with Miss Rogers as teacher, was opened at Chelmsford in 1866. By and by a charter was granted, and the school was transferred to Northampton. The system attracted the attention of Mr. Dexter S. King, and through his efforts a school was started in Boston in 1869. This institution was afterwards called the Horace Mann School, and its success was to a large extent due to the intelligence and enthusiasm of Miss Fuller and Miss Bond, to whose work Mr. Hubbard, in his address, did ample justice. Now the advantages of the oral system are widely recognized, and, as Mr. Hubbard pointed out, the influence of the Horace Mann School has been felt in England as well as in many different parts of the United States.

IN connection with a meeting of the American Electric Light Association, which is to be held at Providence in February, there will be an exhibition of electrical apparatus and appliances, especially such as are used in the furnishing of light and power. This meeting will practically mark the close of the first decade of "electric lighting commercially." It has been suggested, therefore, as we learn from *Science*, that efforts should be made to show the progress in the art by the exhibition of the earlier forms of apparatus and appliances, along with those embodying the latest improvements.

AT the meeting of the French Cremation Society on December 13, an able address on the subject of cremation was delivered by M. Frédéric Passy, member of the Institute. Dealing with the common objection that incineration would facilitate poisoning, he urged that the traces of vegetable poisons vanish rapidly, and that if mineral poisons were used most of them could be detected in the ashes. Moreover, he insisted that there are poisons the presence of which in a human body does not necessarily prove that a crime has been committed; and that if cremation were generally adopted greater care would be taken to determine the precise causes of death. That there is in many minds a strong prejudice against cremation he admitted; but this he attributed to the influence of ancient custom. Irresistible evidence, he maintained, showed that the existing system cannot but be injurious to public health.

THE Administration of the schools of Caucasia is continuing the publication of the excellent linguistic works of Baron Uslar. The fourth volume, which is now out, contains his study of the Lakh language, as well as (in an appendix) his letters to Prof. Schiepner and a Kazy-kumukh alphabet.

THE British Museum (Natural History) has issued the third edition of a Catalogue containing a list of all the subjects of which moulds and store-casts have been prepared for the purpose of supplying museums, and the public generally, with copies of the most striking objects in the collection that will admit of being moulded without injury. The casts are chiefly reproductions of specimens of fossils in the Department of Geology. The prices (revised to date) have been approved by the Trustees.

AN excellent number completes the third volume of the *Internationales Archiv für Ethnographie*. Dr. P. Schellhas, of Berlin, contributes (in German) a most careful paper embodying the results of comparative studies in the field of Maya antiquities. These antiquities he divides into three classes—architectural remains; Maya MSS.; and smaller objects. The latter class has been greatly enriched by the Yucatan collection now in the Berlin Museum für Völkerkunde. Dr. J. H. Spitzly, military surgeon in the Dutch West Indian army, brings together some instructive notes (in English) on three stone adzes from Surinam (Dutch Guiana), and on eight stone implements from the islands of St. Vincent and St. Lucia. A paper (in German) on old Mexican and South American throwing-sticks is contributed by Dr. H. Stolpe, of Stockholm; and Dr. J. D. E. Schmeltz, Director of the Leyden Ethnographical Museum, offers (in German) various facts and suggestions relating to the ethnography of Borneo. The illustrations, as usual, are remarkably good.

WE have received the volume of Records of the meeting of Russian Naturalists and Physicians which was held at the beginning of this year at St. Petersburg. It is a well printed octavo volume of about 1000 pages, with several engravings and maps, and it is sold at the extremely moderate price of five roubles. It contains the proceedings of all the meetings, and abstracts of the chief papers read, or the papers themselves; thus giving a series of most valuable contributions and notes in mathematics and astronomy, physics, chemistry, botany, zoology, geology, geography and anthropology, scientific agriculture, and scientific medicine. Some of the papers are very elaborate.

AN earthquake in Northern California, on January 2, is reported by Prof. Holden, of the Lick Observatory, to have been the most severe experienced in that district since 1868. The ceilings of the Observatory were cracked, the plaster falling to the floor. The large equatorial telescope is, however, believed to be uninjured.

LIEUT. J. P. FINLEY has published his long-promised storm-track, fog, and ice charts of the North Atlantic Ocean, in a quarto atlas. Part i. consists of track-charts for each month; these form a very tangled skein, but the *general* drift of the storms can be clearly seen: the majority of them pass to the north-west of the British Isles. Part ii. consists of 13 charts representing the storm frequency by shaded areas. A careful study of these charts will materially assist shipmasters to determine the general locality and frequency of storms in different months. If the main object were safety instead of speed, a more southerly route than the "lanes" generally followed would be preferable. In voyages *from* America vessels sail more or less with the prevailing direction of the wind, and *vice versâ*. The fog and ice charts show the average probable and extreme eastern and southern limits at which these may be encountered in each month. Icebergs are most frequently met with from July to September inclusive; from November to June the drift of the bergs southwards is impeded by ocean ice in the friths.

THE *Bulletin Mensuel* of the Zi-ka-Wei Observatory (near Shanghai) contains a discussion of the monsoons of the Yang-tse-Kiang from the observations at the stations of the maritime customs established from the entrance of that river as far as Hankow. The results are given in the form of wind-stars, which show that at the mouth of the river the influence of the two monsoons is sharply marked: the winter monsoon blows from between north-west and north-east, the north-west winds being most prevalent, while the summer monsoon blows from between south and south-east. After leaving the mouth the distinction is not so plainly marked; the prevalent winds tend more to the north-east in winter, and in summer more to the east and even north-east.

THE new Russian *Meteorologitcheskiy Sbornik*, published by the Russian Academy of Sciences, to the first volume of which we referred the other day (p. 183), contains Russian translations and Russian originals of papers published in German and French in vol. xiii. of the *Repertorium für Meteorologie*. The two publications will be parallel, under the Director of the Central Observatory, H. Wild.

IN a report to the Foreign Office, recently published, Colonel Stewart, the British Consul-General at Tabreez, calls attention to the curious system of lakes in that region, situated at a great elevation above the sea-level. These are the lake of Urumia, situated 4100 feet above the sea, Lake Van, and the Guektcha lake. Lake Van is in Turkish territory and the Guektcha lake in Russian territory, though both are near the bottom of the Persian province of Azarbaijan, in which is situated the lake of Urumia, the largest and most important. It is 84 miles long and 24 miles broad, and is probably the saltest piece of water on earth, being much salter than the Dead Sea. The water contains nearly 22 per cent. of salt. Its northern coasts are encrusted with a border of salt glittering white in the sun. It is said that no living thing can survive in it, but a very small species of jelly-fish does exist in its waters. Many streams pour down from the Kurdish mountains which border Turkey, and render the country between them and the lake of Urumia very green and fertile. This part of the country looks more like India than Persia, but the climate is severe in winter. The whole country being situated from 4000 feet to 5000 feet above ocean level, the snowfall in winter is great. At night in winter the thermometer falls frequently below zero of Fahrenheit, but in the day-time it rises considerably, generally reaching 28° or 30°, and this with a bright sun overhead. Many people are frozen to death on the roads in winter while crossing the various passes. The winter climate may be compared to that of Canada, but the summer approaches that of Northern India.

THE area under the administration of the Bengal Forest Department, according to its last Report, during 1889–90 consisted of 5195 square miles of reserved forest, 2239 square miles of protected forests, and 4034 square miles of unclassed State forest and waste lands, aggregating 11,468 square miles, which is 5½ per cent. of the total area of the province, viz. 193,198 square miles. The forests are, however, confined to the districts bordering on the sea, the sub-Himalayan tracts and the plateau of Central India, so far as it stretches into Chota Nagpore and Orissa. An area of 207 square miles was added to the reserves during the past year, and 25 square miles of protected forests in the Sunderbunds were farmed out for reclamation. The title of Government to existing reserves is being completed by a compliance with the requirements of the Act, and the inquiries incidental to these proceedings will also secure the record and protection of private easements. The special measures taken for the protection of forests from fires have been increasingly successful, 95 per cent. of the areas thus dealt with having escaped, in spite of the dryness of the season, against 72·9 per cent. in the previous year. The financial results of the past year show a net surplus of Rs. 3,78,454, against Rs. 3,08,738 in 1888–89.

TOADS have been observed by some persons to feed willingly on bees and even wasps; and M. Hiron-Royer, who has noticed the fact, says that *Hyla versicolor* is positively frantic about wasps. He has seen one prefer them to every other kind of food, and devour them eagerly, although the sting does sometimes bring the creature to temporary grief.

ONE of the last volumes of the *Mémoires* of the Kazan Naturalists (vol. xxii. 5) contains a most valuable work by P. Krotoff and A. Netchaieff, upon the geology of the Kama region, being the first part of a general description of that region which the Society intends to publish. The oldest geological formation of the region is that succession of red and grey sandstones and limestones, as well as of "variegated marls," which is considered as Permian by the Kazan geologists, and Triassic by other Russian explorers. The chief interest of the Lower Kama region is, however, in its Post-Pliocene deposits. They cover nearly the whole of the territory, and belong to two different series: the yellow loess-like sandy clays—containing the usual terrestrial shells (*Helix*, *Pupa*, *Succinea*, *Limnæus*, *Pisidium*, &c.), together with bones of Mammoth and Rhinoceros—and the Caspian deposits. The latter attain a quite unexpected development, as they are found in all the valleys, thus bearing unmistakable evidence of the former extension of gulfs of the Caspian Sea up the valley of the Kama (almost as far as its junction with the Vyatka) and its tributaries. Deposits, containing the undoubtedly Caspian species of *Adacna plicata*, *Cardium edule*, *Dreissena polymorpha*, and *Valvata piscinalis*, as well as teeth of fishes, reach the heights of from 530 to 540 feet above the sea-level; and their presence has been noticed on the Byelaya river as far as Angasyak, as also in the north of the Kama at the town of Laishev, and in the lower parts of the Mesha river. It is thus certain that a gulf of the Caspian Sea penetrated up the valley of the Volga and its tributaries as far as 55° 23′ of northern latitude, and that the upper parts of this gulf contained a water less salt than that of the Caspian Sea. It is very difficult to ascertain whether the deposits just mentioned belong to the later parts of the Pliocene period, or to the earlier parts of the Post-Pliocene, and Russian geologists are divided upon this point. But the Kazan authors point out the absolute identity of the fossil species of *Adacna*, *Cardium*, *Dreissena*, *Didacna*, and *Hydrobia*, with those now living in the Caspian Sea; while the discovery in a fossil state of a species of *Corbicula*, and Prof. Sintsoff's *Hydrobia novorossica*, which are not met with now in the Caspian Sea, is not considered as sufficiently proving the greater antiquity of the Kama deposits. Part of these deposits have been denuded by the rivers and mixed with the above-mentioned loess.

OUR ASTRONOMICAL COLUMN.

PERIHELIA OF COMETS.—In *Astronomische Nachrichten*, No. 3005, Dr. Holetschek discusses the apparent connection between the heliocentric longitudes of comet perihelia and the heliocentric longitude of the earth at the times of their perihelion passages. It is evident that any one comet appears brightest when its *perihelion* passage occurs at the same time as that of *perigee*. The possibility of discovering and observing comets decreases, therefore, with the increase of the arc contained between the heliocentric longitudes (l) of comet perihelia and the heliocentric longitude (L) of the earth during the perihelia.

Dr. Holetschek has tabulated the value of (l - L ± 180°) for every known comet, and the values obtained are as follows:—

(l - L ± 180°)	Number of elliptic comets.	Number of parabolic comets.	Total number with perihelion distances greater than 0·3.
0° to 30°	16	90	106
30 ,, 60	10	45	55
60 ,, 90	3	46	49
90 ,, 120	2	26	28
120 ,, 150	—	21	21
150 ,, 180	—	22	22
Total	31	250	281

It will be seen that the majority of all the known comets have the longitude of their perihelion at a small angular distance from that of the earth. This grouping is especially well marked in the case of elliptic comets of short period. The following table exhibits the results found for comets with perihelion distances less than 0·3 times the mean distance of the earth from the sun:—

l - L	Number of parabolic comets.
0° to 60°	19
60 ,, 120	10
120 ,, 180	7
Total	36

Another interesting result brought out by Dr. Holetschek's investigations is that the positive sign occurs with very nearly the same frequency as the negative sign in the angle l - L ± 180°. This indicates that the perihelion points, when reduced to the ecliptic, lie, with equal frequency, in front of and behind the earth.

Mr. W. E. Plummer points out, in this month's *Observatory*, that the cause of the abnormal distribution exhibited by comets whose orbits are greatly inclined to the elliptic has still to be explained.

GASEOUS ILLUMINANTS.

I.

THE autumn course of Cantor Lectures at the Society of Arts was delivered by Prof. Lewes, who, after discussing the nature of various gases capable of burning with luminosity, gave a review of the theories which have been advanced to explain the light-giving power of certain flames.

Sir Humphry Davy first propounded his theory that the cause of luminosity in ordinary flames was the incandescence of nascent carbon—a theory which was accepted as the true one until the researches of Dr. Frankland in 1868, on the effect of pressure on non-luminous flames, showed that, under certain circumstances never likely to arise in a gas-flame, luminosity might be due to other causes. Later observers have shown that luminosity in a flame is to a great extent affected by temperature. These factors, though of the greatest importance, do not affect the truth of the original theory.

In the Philosophical Transactions for 1817, Sir Humphry Davy says, while alluding to a paper published in one of the early numbers of the *Journal of Science and Arts*: "I have given an account of some new results on flame, which show that the intensity of the light of flames depends principally upon the production and ignition of solid matter in combustion."

This definition, however, has been gradually altered until it is more often stated that "the presence of solid particles suspended in the flame (or in immediate contact with the burning gas) is essential to its luminosity"—an idea which Davy never had, as is shown by him later in the paper defining flame as follows: "Flame is gaseous matter heated so highly as to be luminous;" and again: "When, in flames, pure gaseous matter is burnt, the light is extremely feeble." Moreover, he alludes to "common flames"—evidently meaning the flames of candles, lamps, or gas; in all of which cases I think it can be proved beyond a doubt that his theory, as expounded by himself, was perfectly correct.

On June 11, 1868, Prof. E. Frankland read a communication before the Royal Society, in which he described experiments which led him to doubt Sir Humphry Davy's theory. He points out that the deposit of soot formed when a cold surface is held in a gas or candle flame is not pure carbon, but contains hydrogen, which can only be got rid of by prolonged heating in an atmosphere of chlorine. Also that many flames possessing a high degree of luminosity cannot possibly contain solid particles. Arsenic burnt in oxygen gives a bright white light; yet as arsenic volatilizes at 180° C., and the arsenic trioxide forms at 218° C., it is evident that at the temperature of incandescence (which is at least 500° C.) there can be no solids, but simply vapours present in the flame; and for the same reason, the intense light resulting from the burning of phosphorus in oxygen cannot be explained by the solid particle theory. From these results, Dr. Frankland considers that "incandescent particles of carbon are not the source of light in gas and candle flames, but that the luminosity of these flames is due to radiations from dense but transparent hydrocarbon vapours;" and he further shows that non-luminous flames, such as that produced by carbon monoxide and hydrogen, can, when burning in an atmosphere of oxygen, be rendered luminous if the ordinary atmospheric pressure is increased to 10 atmospheres, so as to prevent or retard as far as possible expansion during combustion. From Dr. Frankland's experiments, there is no doubt that the luminosity of a flame is increased by pressing around it the atmosphere in which it is burning, and also that rarefaction has the opposite effect—a point also worked at by Davy; but his experiments do not show that incandescent particles of carbon are not the principal source of luminosity in a gas-flame. He also shows that, the higher the density of the vapours present in a flame, the more likely is it to be luminous.

In 1874, Soret attempted to demonstrate the existence of solid particles in a luminous hydrocarbon flame, by focussing the sun's rays on the flame, and examining the reflected light by means of a Nicol prism; but neither his research nor that of Burch, who repeated his experiments, using the spectroscope instead of the prism, showed more than that solid particles are present. Herr W. Stein, in considering Dr. Frankland's objections to Davy's theory, pointed out that the soot which is deposited from a candle or gas flame, and which Frankland looked upon as a condensed hydrocarbon, contains 99·1 per cent. of carbon and only 0·9 per cent. of hydrogen, which is about the quantity of hydrogen one would expect to be occluded by carbon formed under these conditions, and he also pointed out that if the soot were a heavy hydrocarbon condensed by a cold surface, cooling the vapour present in the flame, it ought to again become volatile at a high temperature, which it does not. The next steps in the controversy were the attempts made by Hilgard, Landolt, and Blochman, to trace the actions taking place in various flames by withdrawing the gases from different parts of the flame and determining their composition.

The experiments so made show that of the ordinary constituents of the gas the hydrogen is the first to burn, as one would expect from its relatively low igniting point and great rapidity of combustion. The burning of the carbon monoxide cannot be traced in the same way, as it is formed more rapidly (by the incomplete combustion of the marsh gas) than it burns, so that a steady increase in the proportion present takes place while the marsh gas steadily burns away, until a height of 1½ inches is attained, when its combustion becomes very rapid. Practically the illuminants do not undergo any change at first—indeed, they slightly increase in quantity from the decomposition by heat of some of the marsh gas into acetylene. They only begin to decompose at a height of 1½ inches above the orifice of the burner; and then burn rapidly in the highest part of the flame. Moreover, a most important fact to be noted is that at the height of 1½ inches there is a sudden rise in the quantity of carbon monoxide at the moment that the illuminating olefines begin to disappear—a result undoubtedly due to the action of the nascent ignited carbon on carbon dioxide.

The illuminants in the flame consist of various hydrocarbon gases and vapours which in the lower part of the flame are reduced by the heat to simpler hydrocarbons, and finally in the luminous zone become decomposed to methane and carbon; and it is the carbon in excessively minute particles which at the moment of liberation is heated to incandescence, and "principally" gives the light of the flame—the marsh gas originally present, and also that formed from the heavier hydrocarbons, adding its quota to the luminosity by still further decomposition during combustion, and finally becoming carbon dioxide and water. In 1876, Dr. Karl Heumann made a most important contribution to the theory of luminous flames in some papers published in *Liebig's Annalen*, in which he carefully went over the work of previous observers, and, by a large number of original experiments, proved that Davy's theory was correct, but that other causes also affected the degree of luminosity in a gas or candle flame.

In the ordinary atmospheric burner in which a mixture of coal gas and air burn with a non-luminous flame, it was supposed that the admixture of air, by supplying oxygen to the inner portion of the flame, caused immediate and complete oxidation of the hydrocarbons, without giving time for the liberation of carbon in the flame, and consequently luminosity. More modern researches, however, have proved this to be utterly wrong. The loss of luminosity is due to two causes—first, to the diluting action of the air introduced; secondly, to the fact that when a gas is so diluted, it requires a far higher temperature to break up the hydrocarbons present than when the gas is undiluted, and therefore the temperature which serves to liberate carbon and render the undiluted gas-flame luminous, is totally insufficient to do so in the diluted gas. Consequently the hydrocarbon burns to carbon dioxide and water without any such liberation, and hence with a non-luminous flame. The truth of this theory can be easily proved by the fact that diluting the gas with nitrogen, carbon dioxide, or even steam, serves to render it non-luminous, and therefore more rapid oxidation has very little or nothing to do with it, while the non-luminous flame can again be rendered luminous either by heating the mixture of air and gas just before combustion, or by heating the air with which the gas is diluted. This being so, it is evident that in the non-luminous flame we have the same hydrocarbon present as in the luminous flame; and anything that will tend to break them up, and liberate the carbon before the hydrocarbons are consumed, should again make the flame luminous.

That heat will do this has been already shown; but it can be demonstrated in a still more striking way. It is well known that chlorine gas and bromine vapour will both support the combustion of a gas containing much hydrogen, but that the combustion is very different from that of the same gas burning in air, as the chlorine or bromine, having no affinity for the carbon, combines with the hydrogen only, and deposits the carbon in clouds of soot; in other words, at the temperature of flame, chlorine will break up the hydrocarbons and liberate solid carbon. If now a small quantity of chlorine is led into the non-luminous Bunsen flame, it at once becomes luminous, proving conclusively that luminosity is due to solid particles of carbon liberated in the flame. Again, Heumann points out that a small rod held in the luminous flame becomes rapidly covered on its lower side with a deposit of soot; that is to say, the soot is present in particles in the flame, and the uprush of the gas drives it against the rod and deposits it there. If the soot were present in the flame, as Frankland supposed, in the state of vapour, and the rod merely acted by cooling and condensing it, the soot should be deposited on all sides of the rod; while a still further proof is, that if the soot existed as vapour in the flame, then, if the rod were heated to a high temperature, no soot should be deposited on it, whereas the soot deposits on a heated surface just as well as on a cool one.

It has been objected to the "solid particle" theory that, if it were true, solid carbon particles introduced into a non-luminous flame should render it luminous and make it look like an ordinary gas flame, whereas it simply gives rise to a cloud of sparks. But it must be remembered that the "nascent" carbon, as it is liberated from the decomposing hydrocarbons, is in the molecular condition, and has a very different degree of coarse-grainedness to any preparation of charcoal or lampblack we can make; and that, although our finest particle is a mass which takes so long to burn that it leaves the flames only partly consumed, and is projected into the air as a spark, the molecular particles of carbon are consumed as soon as they are rendered incandescent, and a steady luminosity, free from sparks, results. It is possible, however, to make the particles in a luminous flame roll themselves together, when they can be either deposited in a very coarse kind of soot, or be seen as glowing sparks and particles in the mantle of the flame. This can be done when two luminous flames are allowed to rush against each other or against a heated surface. Heumann also shows that the luminous mantle of a flame is not altogether transparent, and that the thicker the flame-layer, and the greater the number of solid particles contained in it, the less transparent does it become. If a non-luminous flame—say, hydrogen—is charged with the vapour of chromyl dichloride (CrO_2Cl_2), chromic oxide is produced; and this flame, which undoubtedly contains solid particles, is quite as transparent as the hydrocarbon flame. Finally, those flames which undoubtedly owe their luminosity to the presence of finely-divided solid matter produce characteristic shadows when viewed in sunlight; the only luminous flames which do not throw shadows being those which consist of glowing vapours and gases. Luminous gas flames, oil-lamp flames, and candle flames produce strongly-marked shadows in sunlight, and therefore contain finely-divided solid matter; and that this can be nothing but carbon is evident from the fact that all other substances capable of remaining solid at the temperature of the flames are absent.

From these considerations it is evident that Sir Humphry Davy's theory as propounded by himself is perfectly true as regards the ordinary illuminating flames.

In the second lecture, Prof. Lewes pointed out that, in the various analyses of illuminating gases, the heavy hydrocarbons are, as a rule, expressed as "illuminants," and were formerly considered to consist mainly of ethylene. This is an idea which the researches of the last few years have shown to be totally erroneous, as, besides ethylene, there is undoubtedly present benzene, propylene, butylene, and acetylene, and probably such members of the paraffin series as ethane, propane, and butane, while under certain circumstances, crotonylene, terene, allylene, and others, are present. The determination of the illuminants is therefore by no means the simple process one would imagine it to be from the directions given in most text-books on gas analysis.

The illuminants present in any given sample of coal gas depend upon (1) the kind of coal used, (2) the temperature at which it is distilled, and (3) the length of time the gas is in contact with the heated sides of the retort, as well as with the liquid products of the distillation.

Having dealt with this part of the subject at considerable length, Prof. Lewes went on to criticize the various methods of gas analysis, showing that all analyses of coal-gas have hitherto been founded on the idea that the "illuminants"—*i.e.* the heavy hydrocarbons responsible for the illuminating power—could be absorbed by fuming sulphuric acid, chlorine, or bromine. This, however, he said, is undoubtedly not the case. Mr. Wright has shown that, when coal gas has been treated with this acid, the residual gas still retains from 32 to 55 per cent. of its original luminosity; and although this may, to a certain extent, be owing to the methane, which at high temperature becomes slightly luminous, it is certain that a considerable percentage is due to the higher members of the paraffin series which are not absorbed by the acid, and which the methods of analysis usually employed utterly fail to detect. Indeed, given a gas containing any member of the paraffin series other than methane, the analytical results are not only incorrect, but misleading, as the percentages of hydrogen and methane present will be absolutely nullified by a very small quantity of the higher hydrocarbons.

All researches on the composition of coal gas point to the presence of ethane, and probably of higher members of the marsh gas series; while in carburetted gases they are undoubtedly present to a far higher extent. Ethane, propane, and butane have all been shown to be present in small quantities; and as ethane gives double, propane three times, and butane four times its own volume of carbon dioxide, it is evident that exploding with oxygen and taking the volume of carbon dioxide as representing marsh gas will undoubtedly give too high results with ordinary coal gas, while with a carburetted gas it will render the whole analysis useless. Moreover, the free oxygen is next absorbed, and the remainder taken as nitrogen; and the volume of gas after absorption by Nordhausen acid, less the marsh gas and nitrogen, is taken as representing the hydrogen in the gas. The result is that the hydrogen is always far too low, not only because the volume of marsh gas is too high, but because the residual nitrogen, having to bear the brunt of all the errors of analysis throughout some seven or eight absorptions, is also nearly always too high. These palpable errors in the quantity of marsh gas and hydrogen also render worthless the calculations of the carbon and hydrogen density of the gas, on which great stress has been laid by previous observers. On the whole, therefore, it is not to be wondered at that no relation has been discovered between the carbon and hydrogen density and the illuminating value of the coal gas.

The method of analysis which Prof. Lewes finds best is as follows :—

Two of Stead's apparatus are taken and placed with the entrance tubes end to end, and filled—one with distilled water saturated with air, and the other with clean pure mercury. The gas to be tested is collected in one of the Stead absorbing tubes, over water, so as to be saturated ; it is then transferred over mercury in the eudiometer tube of the second apparatus, and measured and passed into sodic hydrate, in order to absorb the small trace of carbon dioxide to be found in the highly-purified London gas. When present in only small traces, the amount of carbon dioxide lost by water saturation cannot be detected. After the absorption of the carbon dioxide, the gas is run into the second apparatus, and the oxygen estimated by absorption with alkaline pyrogallate, which must be strong and fresh, containing about 25 grammes of pyrogallic acid dissolved in 50 grammes of sodic hydrate in 200 c.c. of water. It is absolutely essential that the solution should be fresh, as after some time it will evolve a considerable amount of carbon monoxide. The heavy hydrocarbons have now to be estimated ; and inasmuch as benzene is one of the most valuable illuminants in the coal gas, it would be of great value if any absorbent could be found that would separate the benzene and ethylene series. Unfortunately this does not exist as far as is known ; the usual absorbents having the following drawbacks :—(1) Nordhausen sulphuric acid, in which sulphur trioxide has been dissolved until it will solidify on cooling, absorbs both ethylene and benzene, and therefore cannot be used to separate them. (2) Fuming nitric acid is a good absorbent for both series. (3) Bromine water acts far more rapidly on ethylene than on benzene, but undoubtedly does absorb a considerable quantity of the latter if left long in contact with a mixture of the two. (4) None of the foregoing affect methane in diffused daylight. The nearest approximate result is obtained by treating the gas first with strong bromine water, but not leaving it too long in contact with it, and then removing bromine vapour over sodic hydrate—the absorption being taken as the ethylene series ; while the benzene is absorbed by fuming nitric acid or saturated Nordhausen acid—acid fumes being removed in the sodic hydrate tube before measurement over water. After absorption with nitric acid gas, is run back into the eudiometer, and measured over water. It is then passed into an absorption tube filled with a fresh solution of ammoniacal cuprous chloride. This must not be used for more than six determinations of an ordinary coal gas containing (say) 3 to 6 per cent. of carbon monoxide, or 3 of a carburetted water gas, as, after much carbon monoxide has been absorbed, the solution has a tendency to again give up small quantities of the gas. The gas is now returned to the mercury eudiometer tube ; and, after measurement, it is passed into an absorption tube containing ordinary paraffin oil (previously heated until everything that will distil at 100° C has gone off), which absorbs ethane, propane, butane, and a good deal of the methane. The residue is then washed and mixed with oxygen, which has itself been analysed, so that the percentage of nitrogen and foreign gases in it is known, and the mixture exploded over mercury. The carbon dioxide formed is estimated ; and its volume *plus* the volume of gas absorbed by the paraffin gives the volume of gases in the methane series. A fresh portion of gas is now taken over mercury, and is exploded with excess of analyzed oxygen. The carbon dioxide is absorbed by sodic hydrate, and the oxygen by pyrogallate ; and the residue will be the nitrogen—the hydrogen being determined by difference. In this way an analysis of South Metropolitan gas shows—

	Hydrogen		47·9	
Illuminants	Ethylene series	approx.	3·5	Total Hydrocarbons 45·6 per cent.
	Benzene series	approx.	0·9	
	Methane series	by paraffin	7·9	
		by explosion	33·3	
	Carbon monoxide		6·0	
	Carbon dioxide		0·0	
	Oxygen		0·5	
	Nitrogen		0·0	
			100·0	

In such an analysis, the lecturer remarked, no pretence was made that the exact percentage of each illuminant was given, but the total of the hydrocarbons is accurate ; and their rough subdivision gave a far clearer insight into the characters of the gas than the more pretentious and more faulty analysis upon which it has been customary to argue. He said it must be clearly borne in mind that he only put forward this scheme of analysis to meet the need now rapidly arising for a method which would show whether ordinary coal gas enriched by cannel, coal gas carburetted with either gasoline or oil gas, or coal gas enriched by highly carburetted water gas, was being dealt with. In the first case, the ethylene and benzene series would be found well represented, while the carbon monoxide was low ; in the second, the amount of hydrocarbons in the methane series would have become greater, and if oil gas had been used, a small increase in carbon monoxide might also be noticed ; while the presence of carburetted water gas at once brought up the quantity of carbon monoxide, and the methane series became more important illuminants.

Prof. Lewes went on to show that the light-giving value of the hydrocarbons present in coal gas varies very greatly, the illuminating power increasing very rapidly with the number of carbon atoms in the molecule ; and concluded by fully discussing the effect which the various diluents present in coal gas had upon its illuminating value.

(*To be continued.*)

ON THE ORBIT OF α VIRGINIS.

PROF. H. C. VOGEL has contributed a further discussion o the orbit of α Virginis (Spica) to the *Astronomische Nachrichten*, No. 2995. The following is a translation of the greater part of his paper :—

"After the periodical approach and retreat of this star, which was suspected from last year's observations, had been proved by some spectrographic observations made this year, an examination of the spectrum has been made at every favourable opportunity. So far, it has been possible to observe Spica on twenty-four evenings, thus affording material which allows its period to be determined with a greater degree of accuracy. Before communicating the results of the measurements of the photographs it will be necessary to preface a few remarks relative to the accuracy attainable.

"In No. 2896 of the *Astronomische Nachrichten* I detailed the first results which were obtained by means of the new spectrograph. The measurements were then made on some stars with spectra of the second class, and the increased accuracy given to these observations by the fact that the measurements were not made on the Hγ line only, but on some exceedingly sharp lines in the neighbourhood, has been fully confirmed by the now completed measurements of all the stars of the second and third class accessible to the Potsdam instruments.

"In the case of stars of class I.*a*, however, in which the Hγ line is more or less broad and fuzzy at the edges, and there are no other lines near it, greater difficulties than I at first expected opposed themselves to the satisfactorily exact measurement of their spectra. At one time I had the intention of making the measurements, not on the hydrogen line, but on the better defined lines of another metal, such as magnesium, which possesses a strong line at 448 μμ. I abandoned this intention, however, because by adopting this means a number of stars, in whose spectra the said line of magnesium is weak or invisible, must have been excluded.

"The absorption lines of hydrogen in star spectra show by their appearance the following four types :—

"(*a*) Dark lines, with somewhat undefined edges, *e.g.* Capella.

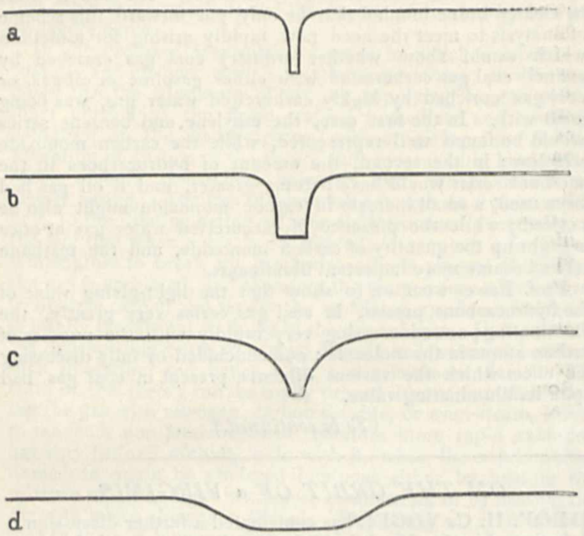
"(*b*) Broad dark lines, with somewhat undefined edges, *e.g.* Rigel.

"(*c*) Broad bands, gradually fading away, but with a more or less broad and well-defined maximum of intensity in the centre, *e.g.* Sirius.

"(*d*) Broad bands, undefined at the edges, without any remarkable maximum of intensity in the centre (Spica).

"The four types are graphically represented by the curves in the accompanying figure."

Prof. Vogel notes that when the maximum of intensity of the form (*c*) lies outside the line of comparison, the measurement of the distance between the two is attended with no special difficulties. When, however, the comparison line overlaps the line in the star spectrum under examination, or in the case of spectra of the form (*d*), special devices have to be used to determine the distance from the centre of the absorption line to the line of comparison, due to the star's motion in the line of sight. These methods



Diagrammatic representation of the appearance of hydrogen absorption-lines in four types of stellar spectra.

are described in the paper. The values obtained from measurements of photographs of the spectrum of Spica are shown in the following table:—

No.	Potsdam Mean Time. 1889.	d.	h.	Observed Motion (Mean).	Reduction to the Sun.	Star minus Sun.
1.	April	21	9.25	−11·9	−0·6	−12·5
2.	,,	29	11.17	−12·7	−1·1	−13·8
3.	May	1	10.97	+10·3	−1·3	+ 9·0
	1890.					
4.	April	4	11.50	− 4·0	+0·6	− 3·4
5.	,,	9	10.50	−14·2	+0·2	−14·0
6.	,,	10	11.50	− 0·9	+0·2	− 0·7
7.	,,	11	10.83	+ 8·9	+0·1	+ 9·0
8.	,,	13	10.83	−14·4	−0·1	−14·5
9.	,,	15	11.00	+11·8	−0·2	+11·6
10.	May	1	10.25	+10·8	−1·2	+ 9·6
11.	,,	4	10.00	− 1·4	−1·4	− 2·8
12.	,,	7	9.42	−13·7	−1·6	−15·3
13.	,,	8	10.43	− 0·4	−1·7	− 2·1
14.	,,	9	10.08	+13·4	−1·7	+11·7
15.	,,	17	10.57	+13·6	−2·2	+11·4
16.	,,	18	10.08	+ 1·5	−2·2	− 0·7
17.	,,	23	10.50	−13·7	−2·5	−16·2
18.	,,	24	10.67	+ 0·3	−2·6	− 2·3
19.	,,	25	10.12	+12·1	−2·6	+ 9·5
20.	,,	26	10.72	+ 1·8	−2·7	− 0·9
21.	,,	27	10.18	−10·7	−2·7	−13·4
22.	,,	28	10.11	− 2·0	−2·8	− 4·8
23.	,,	31	10.29	−11·3	−2·9	−14·2
24.	June	4	10.27	− 9·7	−3·1	−12·8

(+= Recession; −= Approach.)

With regard to these results, Prof. Vogel remarks:—

"If an early circular orbit be accepted, the observations here described allow of its possessing the form and period given by me in my first communication on α Virginis (*Sitzungsber. d. Akad. d. Wissensch. zu Berlin*, April 24, 1890), and it is shown that the period of revolution of 4·011 days is a safe one for a year's time, for the observations of this year are not in accordance with the epochs computed from the periods 3·967 and 4·055 days. Only a slight increase of the first period, 4·011 days, is indicated by the better reduction of last year's observations. The following elements have been deduced from the collected material:—

"Epoch t_0 (when the total orbital motion of the two components in the line of sight = 0) = 1890 May 4d. 10.50h. Potsdam mean time.

"Period p = 4·0134 days.

"Motion of the star system = − 2·0 geographical miles.

"Velocity in the line of sight, when the translation of the system has been deducted, $= 12{\cdot}3 \sin\left(\frac{t-t_0}{p}\,360°\right)$ geographical miles.

"The difference of four miles between the mean maximum positive and negative velocities might be explained by the hypothesis that the orbit of the star deviates considerably from a circle, and that the major axis of the ellipse is almost perpendicular to the line of sight. The observations, however, are not sufficient to decide this point, and as the motion of the system of −2·0 miles (obtained by taking a circular orbit) corresponds to the average movement arrived at from the Potsdam observations, no foundation is afforded for any other hypothesis. . . . In consequence of my first communication on the orbit of α Virginis, Prof. Bakhuyzen has examined whether the Greenwich observations, notwithstanding their very slight accuracy, would afford data for a somewhat more exact determination of the period, as they extend over several years; and he has arrived at a period of 4 days 0·357 hours. From further communications I have learned from Prof. Bakhuyzen that his determination only rested on the comparison of some observations made at Greenwich in the years 1883 and 1886 with the Potsdam observations for the same years, but that no comparison of all the observations had been made. A somewhat more exact repetition of the calculation with respect to the times of observation, has, however, shown that, in consequence of the unaccustomed reckoning of time from midnight to midnight, which has been in use at Greenwich since 1885, Prof. Bakhuyzen had counted an observation as having been made on May 4, 1886, whereas according to astronomical reckoning it was made on May 3. When this oversight is corrected, the period obtained was 4 days 0·324 hours. I mention this, because Mr. Christie, in his annual report for 1890, has referred to the above-named period of 4 days 0·357 hours.

"I have examined the Greenwich observations somewhat carefully, and have come to the conclusion that they cannot be looked upon as sufficient to contradict the results of the Potsdam observations, and that no correction to the discovered period can be deduced from them with any certainty."

After a statement of the results of twenty-six observations of α Virginis made at Greenwich between 1876 and 1889, for motion in the line of sight, Prof. Vogel remarks:—

"If, taking twenty-six observations of α Virginis, the average result be counted, and no orbit introduced, it amounts to ±5·8 geographical miles. In the case of α Leonis it is ±4·4, of α Ophiuchi ± 6·7, and of α Aquilæ ± 6·5 (putting on one side the observations of 1874, 1875, and 1876, which give the value of ± 4·5 miles). Hence it is seen that, according to the Greenwich observations, there is no more cause for suspecting a periodical change of motion for α Virginis than for the other stars examined.

"If values are computed back to 1886 from the elements of the orbit previously given, a minimum of motion in the line of sight is obtained for 1886 April 30, 13·12 hours, and a maximum of negative motion on May 3, 13·36 hours, Potsdam time. These results agree very well with the Greenwich observations of those days. From the interval of time between the two minima in 1886 and in 1890, periods of 3·970 days and 4·058 days were obtained. With these two periods, and that best represented by the Potsdam observations, viz. 4·0134 days, I have computed some of the Greenwich observations according to the formula $12{\cdot}3 \sin \frac{t-t_0}{p}\, 360°$, after the translation of the system deduced from the Potsdam observations had been subtracted. It did not seem allowable to carry the reckoning any further back, as even the period of 4·0134 days may be doubtful in the third decimal place."

A comparison of nine observations made between April 1886 and May 1889 with calculated motions based on the assumed period of 3·970 days, gave a residual of ± 4·4; with the period 4·0134 days it amounts to ± 6·6; and with the period 4·058 days reaches ± 8·5. If the star be considered to have no orbital motion, the residual error is ± 7·2 miles. It appears, therefore, that the Greenwich observations of 1886 to 1889 are best represented by the period 3·970 days. Prof. Vogel then goes on to say:—

"I repeat, with the new values, the calculation of the extent of the orbit and the mass of the stars, which I gave in my first communication on this subject. Upon the supposition of a circular orbit, the resulting period is 4·0134 days, while the two components have equal masses and a velocity of 12·3 miles per second. The mass of the system is 2·6 that of the sun, and the distance of each component from the common centre of gravity is 679,000 geographical miles. With a parallax of 0″·2 the maximum apparent distance amounts to 0″·014, so that the satellite cannot be seen with the strongest instrument.

"Finally, I must not omit to say that, by means of repeated examinations of the photographic plates and measurements of the spectrum, it appears more and more certain that the satellite of *a* Virginis has made itself apparent on the impressions. On several plates, taken at the time of maximum movement, one edge of the Hγ line seems to be rather more undefined and more gradually to diminish in intensity than the other edge; also on some other plates, taken at the time of minimum motion, the Hγ line seems to be rather narrower. These plates also show other lines in the spectrum more plainly on account of their being less faint. These appearances go to show that the satellite has a similar spectrum to the primary star, and that the Hγ line is also broad and faint, but so faint in comparison to the line in the spectrum of the primary star that its presence is only suspected after most careful examinations of the photographs. The phenomenon observed has no influence on the measurements. If, however, the spectrum of the satellite were stronger, it might exercise an influence on the measurements of the maximum motion by causing the differences to measure somewhat less than they really are.

"The satellite, if we grant that it really exists, is probably of about the third magnitude, and it may be possible that powerful reflectors may demonstrate its presence by showing more distinctly the slight periodic changes in the composite spectrum."

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 11, 1890.—"Photometric Observations of the Sun and Sky." By William Brennand. Communicated by C. B. Clarke, F.R.S.

The paper begins with a short account of the various papers communicated by Sir H. Roscoe, and published in the Transactions of the Royal Society.

My observations were made at Dacca, East Bengal, in 1861–66, repeated at Milverton, in Somersetshire, during the last two years. My first experiments were directed to ascertaining the action of the sun on sensitized paper exposed at right angles to the solar rays for different altitudes of the sun, and largely to ascertaining the laws of distribution of the actinic power in the sky.

I take no observations except when the sky is quite clear.

The method of measurement I adopted is the darkening produced in sensitized paper. I cut strips from one uniform sheet of ordinary photographic paper. My observations being relative, I obtain the same results (ratios) with any paper. I compare ultimately the effects of the sun and of a candle on this same paper.

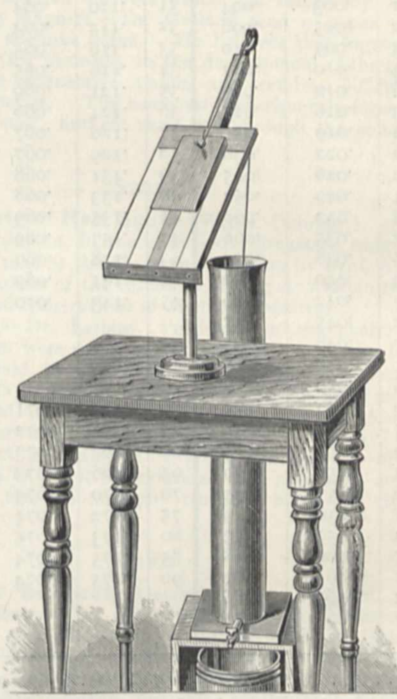
I assume that, in burning a stearine candle, the chemical action is proportional to the material consumed; I have taken as my unit (*i*) of measure of chemical action the darkening produced at a distance of 1 inch from the wick of the candle when 100 grains were consumed, which in the candle I used in India occupied about forty-seven minutes. My observations, being almost entirely relative, are independent of these assumptions, which affect hardly any of my results except comparisons with the absolute unit measures of Sir H. Roscoe.

The water-motion actinometer, with which observations of the action of sun and sky were made, is the instrument depicted in the photograph (see figure).

[The water-motion actinometer was shown and explained. By it, a shutter is removed with uniform velocity from a strip of sensitized paper, which is thus, by any light to which it is exposed, tinged with a gradually intenser depth from 0 to 16 seconds (the paper being suddenly covered at the end of 16 seconds). If, for example, at a particular altitude of the sun, the action of the sky alone in 6 seconds produced the same depth of tint that the action of the sun alone produced in 10 seconds (in a similar strip cut from the same paper), then I estimate the chemical effect of the sun to have been $\frac{6}{10}$ that of the sky, *i.e.* it would be 0·6 if the effect of the sky was taken as the unit of measurement. In the same unit, the effect of the sun and sky together would be 1·6; that is, it would require $3\frac{3}{4}$ seconds' exposure to sun and sky together to produce the unit tint of measurement in the same paper, which calculated value could be immediately verified by another strip cut from the same paper. I have repeatedly made such verifications, which give me a test of the degree of accuracy with which tints can be matched.

By comparing a strip thus gradually shaded in proportion to the time, with another strip shaded as the inverse square of the distance from a near source of light, it is readily shown that the effect of a light acting at the distance unity for four seconds is equal to that of the same light acting at the distance $\frac{1}{2}$ for one second.

Observations of these ratios can be made with any *uniform* piece of paper from which the strips are cut. In the early experiments at Dacca I prepared sheets of sensitized paper with great care. I have found, in later experiments at Dacca and in Somersetshire, that any sheet of fairly good photographic paper gives satisfactory results; *i.e.* the error introduced by



Water-motion actinometer.

want of uniformity in one sheet of paper is inappreciable in comparison with the margin of possible error in the "reading," *i.e.* the matching of tints.

I originally took as my unit of absolute measurement a particular tint produced in a piece of particular paper by a Dacca candle. I found that at an altitude a of the sun this tint was produced (by the effect of the sun alone) in the same paper in (say) 16 seconds, *i.e.* the effect of the sun alone at altitude a was represented by the number $\frac{1}{16} = 0·0625$. By the system of ratios, I found the corresponding numbers for all altitudes 10° to 45°. These ratios were the means of very numerous observations extending over several cold seasons at Dacca where I had a perfect sky for months. If at any time I wish to recover the Dacca unit of measurement *in any particular paper*, I take a sun strip (at any altitude of the sun, γ) in the water-motion actinometer; I see in my table (B) that for the altitude γ the effect of the sun alone is (say) 0·12; I make a mark on my sun strip at the point where it has been exposed $8\frac{1}{3}$ seconds; the effect at that point in that paper is my Dacca unit.

My English observations have been taken in this manner without any reference to a candle. The observations of Sir H

Roscoe, apart from my own very numerous observations, show the chemical effect of the sun to be always the same (in a perfectly clear sky) at the same altitude.]

For obtaining the effects of the sun and sky, I have always experimented mainly by exposing the paper at right angles to the sun's rays. Sir H. Roscoe, on the other hand, exposes his paper on a horizontal plane. Theoretic considerations have led me to another method of observation (with the "octant" actinometer below) which gives directly the measure of the effect really desired.

A table is given of the first observations I made, which afterwards led to the formation of Table B.

TABLE B.—*Chemical Action of the Sun and Sky.*

Sun's altitude.	Sun alone.	Sky alone.	Sun and sky together.	Sun's altitude.	Sun alone.	Sky alone.	Sun and sky together.
1	·001	·003	·004	31	·110	·064	·174
2	·002	·005	·006	32	·113	·064	·178
3	·003	·007	·010	33	·116	·065	·181
4	·004	·010	·014	34	·118	·065	·184
5	·006	·012	·019	35	·121	·066	·188
6	·009	·016	·025	36	·124	·066	·190
7	·012	·019	·031	37	·126	·067	·193
8	·016	·022	·038	38	·129	·067	·196
9	·020	·026	·045	39	·131	·068	·199
10	·024	·029	·052	40	·133	·068	·201
11	·028	·032	·060	41	·135	·069	·204
12	·033	·035	·068	42	·137	·069	·206
13	·038	·038	·075	43	·138	·069	·208
14	·043	·040	·082	44	·141	·069	·210
15	·047	·042	·090	45	·143	·070	·213
16	·052	·045	·097				
17	·057	·048	·105				
18	·061	·049	·110				
19	·066	·050	·116				
20	·070	·052	·122	50	·150	·071	·221
21	·075	·053	·128	55	·157	·072	·229
22	·079	·054	·134	60	·162	·073	·235
23	·083	·056	·139	65	·166	·073	·239
24	·086	·057	·144	70	·170	·073	·243
25	·091	·058	·149	75	·172	·074	·246
26	·094	·059	·153	80	·173	·074	·248
27	·097	·060	·158	85	·175	·074	·249
28	·101	·061	·162	90	·175	·074	·249
29	·104	·062	·166				
30	·107	·063	·170				

N.B.—For sun altitudes 50° to 90°, the figures are not the result of direct observations; for sun altitudes 1° to 10°, the figures are less certain by reason of thin haze often present.

The numbers of the table were obtained, by taking the inverse of the times required at each altitude for producing the darkening of the candle unit.

I found the chemical action of the sun, as far as my experiments went, the same at all hours of the day and at all seasons of the year. And in Somersetshire I got exactly the same chemical action of the sun as at Dacca. Observations near the horizon cannot be depended upon.

Various observations had led me to suspect that the chemical action of the sky at the same moment was different in different parts of it. To investigate this suspicion, I designed an instrument which I call the mitrailleuse actinometer. I mount a number of similar cylindrical tubes in one plane in a semicircle, to the centre of which the axis of each tube is directed: one extremity of each tube lies in the circumference of the circle; the other extremities lie on a concentric circle of about one-half the radius. In the circumference of this smaller circle is a semicircular series of holes, against which a semicircular block carrying the sensitized paper is pressed by a screw. Each cylinder cuts out of the sky a circle of 8° 28′ angular diameter. One of the tubes near its top carries a small plate of wood on which stands a stile parallel to the tube, by means of which this particular tube can be brought in a line with the sun. By another motion the plane of the tubes can be adjusted to the plane of symmetry (or elsewhere).

[A vertical plane through the sun at any time divides the visible sky into two exactly similar portions. I call this the plane of symmetry.]

The observations (Table C) were taken December 23, 1864, at Dacca (among other similar observations taken in the same cold weather) in the plane of symmetry. The barrels of the mitrailleuse were fixed 10° apart, the altitude of the sun being 42° 28′.

I give the table as an early observation that shows well that there is a point of minimum sky intensity at 90° from the sun. It also appears that if i_a be the intensity for the altitude a of the sun (= 0·12), then the intensity of the sky at a point $\theta°$ from the sun is given (roughly only according to this table) by the formula

$$i_a \operatorname{cosec} \theta.$$

This observation was made in the plane of symmetry: it turns out that the value, $i_a \operatorname{cosec} \theta$, gives the intensity very accurately, for any point, in any other *great* circle, whose distance from the sun is $\theta°$ measured on that circle.

For any altitude of the sun (a), the chemical action of the sky is a minimum at all points in a great circle the plane of which is at right angles to the line joining its centre to the sun.

[This plane I call the plane of minimum intensity (i_a).]

As the whole of the mathematical developments of this paper are founded upon the law that at any point of the sky whose distance is $\theta°$ from the sun

$$\text{the intensity} = i_a \operatorname{cosec} \theta,$$

I have been careful to verify it by numerous observations both at Dacca and in Somersetshire, and also to vary the observations in every way I could devise. Thus the mitrailleuse has been placed in the plane of minimum intensity. In this case all the barrels give the same reading for points not too near the horizon.

Next the mitrailleuse was placed in planes of great circles through the sun at various angles with the plane of symmetry; by turning it round the line joining one of its tubes with the sun the observed chemical actions agree well with

$$i_a \operatorname{cosec} \theta.$$

Next by means of stops I made the aperture of each barrel of the mitrailleuse to be

$$c \sin \theta,$$

where $\theta°$ is the distance of the axis of the barrel from the sun; this mitrailleuse being exposed, the barrel $c \sin 0$ being directed to the sun, the circular darkened spots were found to be very accurately of the same depth.

Further, I calculated the *times* of exposure for a (particular) mitrailleuse with barrels of uniform aperture, which ought, on the law $i_a \operatorname{cosec} \theta$, to give a uniform tint. I exposed this mitrailleuse for these calculated times, first in the plane of symmetry, afterwards in a plane inclined to it at 62°; the results agreed closely with my anticipation, and show $i_a \operatorname{cosec} \theta$ to be a very good approximation.

I have therefore made full use of the expression $i_a \operatorname{cosec} \theta$ for the chemical action of the light of the sky in a circle θ from the sun (whose altitude is a).

I calculate (having given me i_a, the chemical action in the circle of minimum intensity) the total chemical action of the sky, first, on a plane exposed at right angles to the sun; second, on the horizontal plane. The first is an elliptic integral, the second is $2i_a (\pi \sin a + 2 \cos a)$. By these values I am able to compare some of Sir H. Roscoe's observed values with my own.

The mathematical processes in reducing these integrals suggested to me (within the last few months) the construction of the octant actinometer, a new instrument which I tried in Somersetshire in October last. I am fairly satisfied with the results considering the imperfect sky of England. This instrument measures the value i_a directly; and it possesses, moreover, the great advantage of not taking in the low band of sky near the horizon, and thus avoids a principal element of uncertainty in other observations.

Geological Society, December 17, 1890.—Mr. W. H. Hudleston, F.R.S., Vice-President, in the chair.—The following communications were read:—On nepheline rocks in Brazil: ii. the Tingua mass, by Mr. O. A. Derby. In a former paper the general distribution of the nepheline rocks, so far as known, was given, with a particular description of a single one, the Serra de Pocos de Caldas. The present paper treats of a second mass, the Serra de Tingua, a high peak of the Serra do Mar, some forty miles from Rio de Janeiro. This peak is essentially a mass of foyaite rising to an elevation of 1600 metres, on the crest and close to the extremity of a narrow gneiss ridge of a very uniform elevation of about 800 metres. As seen from a distance, the conical outline and a crater-like valley on one side are very suggestive of volcanic topography. In the structure of the mass both massive and fragmental eruptives are found, the former greatly predominating. The predominant rock is a coarse-grained foyaite which is found everywhere in loose blocks about the margins of the mass, but not extending beyond it. In the numerous cuttings in the immediate vicinity, dykes of phonolite and basic eruptives (augitite) are exceedingly abundant, foyaite never appearing in a dyke form. There is, however, abundant evidence that foyaite and phonolite are but different phases of the same magma. Aside from the dyke phonolites, true effusive phonolites associated with fragmental eruptives (tuffa) were found high up in the crater-like valley, proving that the mass was a volcanic centre in the most restricted sense of the word. This conclusion affords an explanation of some of the peculiarities of the foyaite, which has many characteristics of effusive eruptives mingled with those of the deep-seated ones (*Teifengesteine*). These have, aside from the porphyritic structure, a *schlieren* structure revealed by a peculiar fluted weathering (illustrated by a photograph) and the presence of pseudo-crystals in the form of leucite. Stratigraphically the Tingua foyaites lie in sheet-like masses like lava-flows, extending from the higher to the lower portions of the mountain, the underlying gneiss being revealed at nearly all levels, wherever the mass has been scored by streams. The general fragmentary character of the rock seems to be due to the undermining of these sheets. Specimens and photographs illustrating the peculiar pseudo-crystals in the form of leucite that occur in both the foyaites and phonolites of Tingua (although no leucite has been detected in the rock) were exhibited and discussed. After the reading of this paper there was a discussion, in which the Chairman, Mr. Bauerman, Mr. Hulke, Prof. Green, and the author took part.—The variolitic diabase of the Fichtelgebirge, by Mr. J. Walter Gregory.

EDINBURGH.

Royal Society, December 15, 1890.—Prof. Crum Brown in the chair.—Prof. Tait communicated a paper, by Dr. E. Sang, on the extension of Brouncker's method to the comparison of several magnitudes. The method employed is essentially an application of continued fractions.—Prof. Tait also communicated a paper by Mr. A. M'Aulay, on proposed extensions of quaternion powers of differentiation. In this paper, Mr. M'Aulay discusses a proposed modification of quaternion notation which leads directly to remarkable extensions in the mathematical treatment of the theories of elasticity and of electricity. His object in communicating this paper to the Society is to obtain information as to the likelihood of his modified notation—which is entirely opposed to ordinary conventions—being accepted by scientific men.—Prof. Ewing exhibited a model illustrating a molecular theory of magnetism. The model consists of a number of pivoted magnets which are arranged in parallel rows. These magnets are placed in the interior of a rectangular coil of copper wire, around which an electric current can be made to pass. So long as no current passes around the wire, the magnets arrange themselves in positions of stable equilibrium under their mutual forces, some of them pointing in one direction, some in another. This illustrates the condition of non-magnetized steel. If a feeble current be now passed round the coil, each magnet is slightly turned from its first position, which, however, it reassumes when the current is stopped. This illustrates the first stage of the process of magnetization. A somewhat stronger current causes instability among the originally less stable groups of the magnets, so that the magnets composing these groups swing round into a new stable position. As the strength of the current increases still further, more and more groups break up, until all have taken the new position of equilibrium under their own mutual forces and the external directive force. This illustrates the second stage of magnetization, in which the ratio of magnetization to magnetizing force increases with great rapidity. The third stage, in which the above ratio is practically constant, is exemplified by the fact that an infinite force is now needed to make the magnets point exactly in the direction of the external lines of force. If the current be now stopped, a considerable proportion of the magnets retain their final position of equilibrium—in other words, magnetic retentiveness is exhibited. The model may be made to exhibit the effects of strain on the magnetic properties. For this purpose the magnets are placed on a sheet of india-rubber. If the india-rubber is stretched, the magnets are separated out from each other in one direction, and are brought nearer to each other in a direction at right angles to the former. The magnetic susceptibility is increased, or diminished, according as the stability of the magnets is diminished, or increased, by the alteration of relative position. Similarly, the increase of the susceptibility of iron with rise of temperature is explained by the diminution of mutual magnetic influence which results from increased distance. Prof. Ewing suggests that the total loss of magnetization which occurs at a high temperature is due to continuous whirling motion of the magnetic molecules. The dissipation of energy which occurs when hysteresis is exhibited is due to the induced currents which are caused by angular motions of the magnets.—Dr. Gulland read a paper on the development of adenoid tissue. He believes that ingrowth of the epithelium (for example, in the development of the tonsils) compresses the connective tissue, and renders difficult the passage of leucocysts. The leucocysts consequently increase in numbers at the part, and eat their way through the condensed tissue.

SYDNEY.

Royal Society of New South Wales, October 1, 1890.—Dr. Leibius, President, in the chair.—A discussion took place upon the paper read at the September meeting by Prof. Warren, on some applications of the results of testing Australian timbers to the design and construction of timber structures.

November 5.—Dr. Leibius, President, in the chair.—The following papers were read:—Geological notes on the Barrier Ranges silver field, by C. W. Marsh.—Record of hitherto undescribed plants from Arnheim's Land, by Baron Ferd. von Mueller, K.C.M.G., F.R.S.—Some folk songs and myths from Samoa, translated by Rev. T. Powell and Rev. G. Pratt, with an introduction and notes by Dr. John Fraser.—Mr. H. C. Russell, C.M.G., F.R.S., exhibited and described some of the surprising star photographs recently taken at Sydney Observatory.

PARIS.

Academy of Sciences, December 29, 1890.—M. Hermite in the chair.—List of prizes awarded to successful competitors in 1890:—*Geometry:* Grand Prix des Sciences Mathématiques, M. Paul Painlevé; honourable mention, M. Léon Autonne; Prix Bordin (not awarded); Prix Francœur, M. Maximilien Marie; Prix Poncelet, M. le général Ibañez. *Mechanics:* Extraordinary Prize of 6000 francs—this has been divided between M. Madamet, MM. Ledieu and Cadiat, and M. Louis Favé; Prix Montyon, M. le colonel Locher; Prix Plumey, M. Jules-Ernest Boulogne. *Astronomy:* Prix Lalande, M. J. V. Schiaparelli; Prix Damoiseau (not awarded); Prix Valz, Prof. S. de Glasenapp; Prix Janssen, Prof. C. A. Young. *Statistics:* Prix Montyon, Dr. Paul Topinard; honourable mention, M. Dislère. *Chemistry:* Prix Jecker, divided between the late M. Isambert and M. Maurice Hanriot. *Geology:* Prix Vaillant, M. Marcel Bertrand; Prix Fontannes, M. Ch. Depéret. *Physical Geography:* Prix Gay, M. Franz Schrader. *Botany:* Prix Desmazières, M. Maurice Gomont; Prix Montagne, divided between M. Paul Hariot and Dr. Albert Billet. *Anatomy and Zoology:* Prix Bordin (not awarded); Prix Savigny, divided between Dr. Jousseaume and M. R. P. Camboué; Prix Thore (not awarded); Prix Serres, M. Camille Dareste. *Medicine and Surgery:* Prix Montyon, divided between M. Félix Guyon, M. Auguste Ollivier and M. Paul Richer; mentions were accorded to M. Ch. Fiessinger, MM. J. Chauvel and H. Nimier, and M. Ch. Mauriac; Prix Bréant, divided between M. G. Colin and M. A. Layet; Prix Godard, M. Samuel Pozzi; honourable mention, MM. Ch. Monod and O. Terrillon; Prix Barbier, M. Claude Martin; honourable mention, M. Gaston

Lyon and M. B. Dupuy; Prix Lallemand, divided between Mme. Déjerine-Klumpke and M. G. Guinon; Prix Dusgate (not awarded); Prix Bellion (not awarded); Prix Mège, M. Nicaise. *Physiology:* Prix Montyon, divided equally between M. E. Gley and M. E. Wertheimer; honourable mention, M. E. A. Alix and MM. G. Arthaud and L. Butte; Prix Pourat (not awarded). *General Prizes:* Prix Montyon (Unhealthy Industries), M. Casimir Tollet; Prix Jérome Ponti, M. R. P. Colin; Prix Trémont, M. Beau de Rochas; Prix Gegner, M. Paul Serret; Prix Delalande-Guérineau, Dr. Verneau; Prix de la fondation Leconte, M. Prosper de Lafitte; Prix Laplace, M. Bailly.—The following prizes were proposed for the year 1891:—Prix Francœur: For discoveries or useful works tending to further the progress of pure and applied mathematics. Prix Poncelet: For the author of any work tending most to further the progress of pure and applied mathematics. Extraordinary Prize of 6000 francs: Any improvements tending to increase the efficiency of the French naval forces. Prix Montyon: Mechanics. Prix Plumey: Improvement of steam-engines or any other invention contributing most to the progress of steam navigation. Prix Dalmont: For the engineer who shall present to the Academy the best work on bridges or on highways. Prix Fourneyron: Improvements in the theory of steam-engines which take most account of the exchanges of heat between the water and the cylinders and tubes. Prix Lalande: Astronomy. Prix Damoiseau: Improvements of the lunar theory which consider inequalities of long period caused by planets. Prix Valz: Astronomy. Prix Janssen: Astronomical physics. Prix Montyon: Statistics. Prix L. La Caze: The best work on physics, chemistry, and physiology. Prix Jecker: Inorganic chemistry. Prix Delesse: The author of the best work on geological science, or, in default of such, mineralogical science. Prix Bordin: The study of the phenomena of the fecundation of Phanerogamic plants, with particular reference to the division and transport of the cellular nucleus; also the study of the connections which exist between these phenomena and those observed in the animal kingdom. Prix Bordin: Comparative study of the auditory nerves of warm-blooded Vertebrata; Mammiferæ and Birds. Prix Desmazières: The best work on the whole or any part of Cryptogamic flora. Prix Montagne: The author of important works on the anatomy, physiology, development, or description of the lower Cryptogamic plants. Prix Thore: Works on the cellular Cryptogams of Europe, and on the habits or anatomy of any species of European insect, alternately. Grand Prix des Sciences Physiques: On the organs of sense of Invertebrata, from an anatomical and physiological point of view. The prize may be awarded for a complete work on one of the organs of sense in a group of Invertebrata. Prix Savigny: For young zoological travellers. Prix da Gama Machado: On the coloured parts of the tegumentary system of animals, or on the genital matter of living beings. Prix Montyon: Medicine and surgery. Prix Bréant: The discovery of a cure for Asiatic cholera. Prix Godard: On the anatomy, physiology, and pathology of genito-urinary organs. Prix Chaussier: Important works in legal or practical medicine. Prix Barbier: The most important discovery in surgery, medicine, pharmacy, and botany, having reference to the healing art. Prix Lallemand: Researches on the nervous system in the widest sense of the term. Prix Bellion: Works or discoveries serviceable to the health or to the improvement of the human species. Prix Mège: The author of a continuation and completion of Dr. Mège's essay on the causes that have retarded or favoured the advancement of medicine. Prix Montyon: Experimental physiology. Prix Pourat: Functions of thyroid bodies. Prix Martin-Damourette: Therapeutic physiology. Prix Gay: Newly formed lakes and how they become stocked. Prix Montyon: Unhealthy industries. Prix Cuvier: The most remarkable work on the animal creation, or on geology. Prix Trémont: For any naturalist, artist, or mechanic, needing help for carrying out any project useful and glorious for France. Prix Gegner: In aid of any man of science distinguished for his works towards the advancement of the positive sciences. Prix Jean Reynaud: For the most meritorious work produced in a period of five years. Prix Petit D'Ormoy: Pure and applied mathematics, or applied and natural science. Prix Laplace: For the best student leaving the École Polytechnique.

Brussels.

Academy of Sciences, November 8.—M. Stas in the chair.—On the variations in latitude observed at Berlin, Potsdam, and Prague, by M. F. Folie. The periodical changes in the declination of stars, due to aberration, is adduced as a possible cause of latitude variation.—On the Belgium graptolites, by Prof. C. Malaise.—On the points of variation in cubes, by M. Cl. Servais.—On dypnone, $\genfrac{}{}{0pt}{}{C_6H_5}{CH_5}\!\!>\!C = CH \,.\, CO \,.\, C_6H_5$, by M. Maurice Delacre.—On the new species *Posadæa*, belonging to the family of Cucurbitaceæ, by M. Alfred Cogniaux.—The reduction of nitrates to nitrites by seeds and tubercles, by M. Émile Laurent. The author makes known the results of his new researches on the reduction of nitrates. He has experimented on Indian corn, barley, peas, white lupines, broad beans and kidney beans, previously sterilized, but afterwards allowed to germinate. The crops obtained are immersed in a 1 per cent. solution of potassium nitrate, and give rise to the production of potassium nitrite. With regard to this action it is concluded that "La réduction des nitrates en nitrites par les végétaux est, comme la fermentation alcoolique, une conséquence de la vie qui se continue dans un milieu privé d'oxygène libre." This reduction of nitrates by the tubercles of the Jerusalem artichoke, radish, turnip, &c., by their petioles, and by the peduncles and fruits of different plants, has also been observed.

Stockholm.

Royal Academy of Sciences, December 10, 1890.—The third and fourth part of "Erythrææ exsiccatæ" exhibited and commented on by Prof. Wittrock.—On the genera Kurutas, Nidularium, and Regelia, of the family of the Bromeliaceæ, by Dr. C. Lindman.—Researches on the structure of the central nervous system of the Evertebrates and especially of the Crustacea, and in general on the connection between the nervous cells and nervous filaments in the nervous central organs, by Prof. G. Retzius.—Contributions to the question of secular perturbations, by Dr. K. Bohlin.—The influence of the temperature on the capillary constants of some fluids, by Herr Tinnberg.—On the reaction of the iodohydric acid on 1–6 nitro-naphthalin-sulphon-acid-amid, by Herr A. Ekbom.—"Sur la notion de l'énergie libre," by Herr A. Rosén.

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