THURSDAY, NOVEMBER 19, 1874

ELIE DE BEAUMONT

THE life of the science of geology has been short; that of many of its illustrious votaries has been long. There still survive a few whose recollections go back to the early triumphs of the science in the days of William Smith and Cuvier. But their number grows rapidly less. One by one the links which bind us personally with the glories of the past are being snapped asunder. The grand old oaks under whose branches the younger saplings have grown up are fast dropping down. Within the last few years we have lost in this country our Murchison, Sedgwick, and Phillips; Austria her Haidinger; Germany her Gustav Rose, Bischof, and Naumann; America her Agassiz, and France her D'Archiac and De Verneuil. To this list we have now to add the well-known name of L. Elie de Beaumont. To the expressions of regret with which the friends and pupils of that father in science have followed his remains to the tomb, geologists in every country will add their sympathy. Those who knew him best have eulogised his love of truth, his piety, and his generous feeling for younger and struggling men of science.

The name of Elie de Beaumont is chiefly known out of France by its association with two theories—*Cratères de soulèvement* and the *Résau pentagonale*—which he espoused and vigorously defended, but neither of which has met with general acceptance, though no one can peruse the writings in which they are developed without admiring the wonderful industry of Elie de Beaumont in the accumulation of facts and the felicitous imagination with which he marshalled these facts in support of the theory to which he had pledged himself. It is not easy for geologists in other countries to understand the vast influence which for nearly half a century he has held in France. We must bear in mind the system of centralisation which controls even scientific enterprise in that country, and the fact that Elie de Beaumont held official posts in Paris which gave him a powerful sway over geological and mining matters, especially such as were under the guidance of the State. Hence it was not merely his great reputation, but his official position, which enabled him for so many years in great measure to control the progress of physical geology in his native country.

This eminent geologist was born in the year 1798. In 1817 he entered the Ecole Polytechnique, where he greatly distinguished himself, leaving it in the first rank for the Ecole des Mines. At that institution he showed a strong tendency towards geological pursuits, and such capacity for their prosecution that he was soon chosen to perform one of the most onerous tasks which had ever been undertaken by the Mining Department of France. The publication of Greenough's geological map of England, and the reception of a copy of it in the year 1822 at the Ecole des Mines, revived a project which political considerations had displaced, of constructing a geological map of France. When the decision to undertake this great work was formed, Elie de Beaumont, with his fellow-pupil and future friend and associate Dufrénoy, was selected to carry out the necessary surveys. With the view of giving them still further training for their task, the authorities sent them over to study the geology of England, particularly the arrangement of the secondary rocks of this country, which by the genius of William Smith had become a type for all parts of Europe. Six months were spent in this preliminary work, some portion of the time being devoted to a careful study of British mines and mining, on which the two young engineers furnished some voluminous and skilful reports. It was the year 1825 before they received orders to begin their surveys. France was separated into two sections, the eastern half being allotted to Elie de Beaumont. The two observers, however, met frequently, and after the main part of their labours was concluded they went over portions of the ground together, so that in the end, agreeing on all main points, they produced a harmonious and magnificent work. In ten years they had completed their surveys. The engraving necessarily occupied some five years more, after which the indefatigable authors produced two large and exhaustive quarto volumes of explanations of the map, wherein the geological structure of their country was well described.

Of all the achievements of Elie de Beaumont, this, his first, is probably that on which his fame will ultimately most securely rest. It was a great work, most conscientiously and skilfully performed, amid difficulties which can only be adequately realised by those who have essayed geological mapping, and who know the nature of the ground over which the French explorer had to trace his lines.

During the twenty-three years (1825-48) which elapsed between the beginning and the completion of the map and its accompanying text, Elie de Beaumont had made his name widely known by other important contributions to science. A few years after the mapping had begun, and while engaged in exploring the high grounds in the east of France, he was struck by the relations which could be traced between the direction of different lines of mountain and the nature and position of the strata along these lines of elevation. In 1829 he published the first sketch of the theory which afterwards grew into the well-known *Réseau pentagonale*. He likewise adopted and defended Von Buch's *Erhebungs-krater* theory, publishing in its support an elaborate essay on the structure of Etna (1836). One of his best essays was published in 1847, "Sur les Emanations Volcaniques et Métallifères," a luminous exposition from the point of view of a cataclysmist of the history of the volcanic phenomena of the globe. One of his best separate publications is his "Leçons de Géologie pratique," a work full of knowledge and research, which may be usefully studied by all who take interest in dynamical geology. It would take some time to enumerate even the titles of his various contributions to the transactions and journals of his day. They include short notes and long memoirs of original research of his own, elaborate reports upon the writings of others (of this style he was a master), instructions to exploring expeditions, &c.; and they are not confined to physical geology, but embrace also the allied sciences—chemistry, mineralogy, and palæontology. One feature which characterises them is the endeavour after exactitude. Their author had a mathematical mind, and sought for mathematical precision in his development of a subject.

Elie de Beaumont in the course of his long career filled many offices of distinction. As far back as 1827 we find him lecturing for his master at the Ecole des Mines, and afterwards succeeding to the chair. In 1832, on the death of Cuvier, he was chosen to fill the only chair of Natural History at the Collége de France. He thus stood at the head of the geological tuition of the country. The mining engineers and others who required geological instruction for State certificates or appointments passed through his hands. His fame likewise attracted many from a distance, so that as a teacher his influence must be regarded as having been very great. Moreover, he became Inspector-General of Mines, member and perpetual secretary of the Academy of Sciences, and was an associate of many of the learned societies of Europe and America. His scientific renown and high personal character led to his being chosen as senator and raised to the rank of Grand-Officier of the Legion of Honour. Full of honours, therefore, he has closed a long life with his faculties unimpaired to the last, and in the midst of the activity which had marked his long and honourable career.

This is perhaps hardly the place or the time to pass any judgment on the work of the illustrious man who has just gone from among us. His name will ever be associated with the history of geology, linked with those of Cuvier, Brongniart, Dufrénoy, and others who led the way to all that has since been achieved in the geology of France.

ARCH. GEIKIE

FLÜCKIGER AND HANBURY'S "PHARMACOGRAPHIA"

Pharmacographia: a History of the principal Drugs of Vegetable Origin met with in Great Britain and British India. By Friedrich A. Flückiger, Ph.D., Professor in the University of Strassburg; and Daniel Hanbury, F.R.S., Fellow of the Linnean and Chemical Societies of London. (Macmillan and Co., 1874.)

THERE was a stir of anticipation and inquiry amongst pharmacologists when it first became known that Prof. Flückiger and Mr. Hanbury were engaged upon a work of joint authorship. Speculation was busy as to what was to be the nature of the book, to what particular objects it would be directed, what extent of ground it would cover, and so forth. Upon a single point all were agreed, namely, that it would *not* be one of those composite treatises on drugs—organic and inorganic—therapeutics, pharmacy, and toxicology, enlivened by traditional botany and old-fashioned chemistry, which have passed current amongst us as "Manuals of Materia Medica."

One generation after another of compilers have produced volumes supposed to be suited to the wants of the time, in which the same sort of information has been given, the same errors perpetuated often in almost identical words, until the very term "Materia Medica" has come to be looked upon with suspicion by scientific men. Perhaps the origin of the shortcomings of the general run of such works may be traced to the fact that they have often been written by practising physicians who were lecturers in medical schools, and have been designed primarily as handbooks for medical students. Nor need it be a matter of wonder that, with no special facilities for acquiring original information as to the history of drugs, and with few opportunities for verifying the statements of others, authors so situated were content to transcribe without examination what had been already recorded as fact, and to devote their better energies to the more purely medical relations of the subject—the aspect of chief interest both to themselves and those for whom they wrote.

The question has often been raised, and once at least on very high authority, why the overcharged curriculum of medical study should still be encumbered with Materia Medica; why, in view of the separation which is gradually taking place between the practice of Medicine and that of Pharmacy and of the scientific education now received by the pharmaceutist, such matters as the physical characters sources, and chemistry of drugs should not be referred to those whom they primarily affect.

This, perhaps, is scarcely the place to discuss such questions in detail, but they inevitably present themselves on a comparison of the present book with any of those to which allusion has just been made.

It is generally no very difficult thing to give an intelligible account of a work embodying the results of scientific research. It is not requisite that the knowledge of the reviewer should be co-extensive with that of the author to enable him to form a just estimate of its strong and weak points, or even to exercise the critical faculty where opinions rather than facts are advanced. But the task of introducing suitably a closely printed volume of 700 pages, containing scarcely anything but facts—an unusual proportion of which are stated for the first time, and those which are old assuming a new importance from their fresh verification, the whole given with a condensation of style that refuses page-room to a superfluous word—is not one that can be performed by the ordinary method of summarising results.

The scope of the "Pharmacographia" and the intention of its authors can hardly be better told than by a few extracts from the Preface. After defining the word Pharmacographia as "a writing about drugs," the authors state that "it was their desire not only to write upon the general subject and to utilise the thoughts of others, but that the book which they had decided to produce together should contain observations that no one else has written down. It is in fact a record of personal researches on the principal drugs derived from the vegetable kingdom, together with such results of an important character as have been obtained by the numerous workers on Materia Medica in Europe and America."

Restricting the field of their inquiry by the exclusion of Pharmacy and Therapeutics, "the authors have been enabled to discuss with fuller detail many points of interest which are embraced in the special studies of the pharmacist."

"The drugs included in the work are chiefly those which are commonly kept in store by pharmacists, or are known in the drug and spice market of London. The work likewise contains a comparatively small number which belong to the Pharmacopœia of India: the appearance of this volume seemed to present a favourable opportunity for giving some more copious notice of the latter than has hitherto been attempted."

Now as to the manner of treatment. A uniform subdivision into sections has been adopted throughout the work. In the first place, "Each drug is headed by the Latin name, followed by such few synonyms as may suffice for perfect identification, together in most cases with the English, French, and German designation.

"In the next section, the *Botanical Origin* of the substance is discussed, and the area of its growth or locality of its production is stated."

"Under the head of *History*, the authors have endeavoured to trace the introduction of each substance into medicine, and to bring forward other points in connection therewith, which have not hitherto been much noticed in any previous work."

"In some instances the *Formation*, *Secretion*, or *Method of Collection* of a drug has been next detailed: in others, the section *History* has been immediately followed by the *Description*, succeeded by one in which the more salient features of *Microscopic Structure* have been set forth."

The next division includes the important subject of *Chemical Composition*; then follows a section devoted to *Production and Commerce*; and lastly, observations, chiefly dictated by actual experience, on *Adulteration* and on the *Substitutes* which in the case of certain drugs are occasionally found in commerce, though scarcely to be regarded in the light of adulterants.

"The medicinal uses of each particular drug are only slightly mentioned, it being felt that the science of therapeutics lies within the province of the physician, and may be wisely relinquished to his care."

The reader must not judge the Preface by the disconnected sentences which have been quoted to serve a particular purpose. Only sufficient has been copied to explain briefly, and as far as possible in the authors' own terms, the general scheme of their work.

The plan, as will be seen, is one of great comprehensiveness, and the execution throughout is of characteristic thoroughness. A single article taken at random from the book would be better evidence than any criticism, of the exhaustive character of the treatment; but unfortunately, considerations of space preclude anything more than a few general remarks suggested by a first perusal.

The investigation of the botanical origin of drugs is one which Mr. Hanbury has made his own, and few writers have set at rest so many debated questions in this division of the subject. Completeness and accuracy of the information now collected is exactly what might have been expected. The student who knows only the British Pharmacopœia will find much to learn, and something to unlearn, concerning the origin of many common medicinal substances. In some cases the corrections necessary arise merely out of questions of priority in botanical nomenclature, but in others the errors are founded in the wrong identification of the plants. For instance, *Jateorhiza palmata*, Miers, is the name accepted, for reasons given in the text, for the plant yielding calumba root, rather than the alternative specific terms of the Pharmacopœias. Oil of cajuput is assigned to *Melaleuca leucadendron*, L., whilst in the British Pharmacopœia and the Paris Codex it is referred to *M. minor*, DC., and in that of the United States to *M. cajuputi*, Roxb. Sumbul Root, the botanical history of which in our Pharmacopœia is stated to be unknown, appears as the product of *Euryangium Sumbul*, Kauffman, a plant of the natural order Umbelliferæ. On the other hand, in speaking of the botanical origin of Myrrh, which the Pharmacopœia, without show of doubt, assigns to *Balsamodendron myrrha*, Ehrenb., it is stated that "the botany of the myrrh trees is still encompassed with uncertainty, which will not be removed until the very localities in which the drug is collected shall have been well explored by a competent observer." It would be easy to multiply examples, but beyond a passing allusion to Pareira Brava as the root of *Chondodendron tomentosum*, Ruiz et Pav., a fact determined by Mr. Hanbury's researches, this portion of the subject need not be dwelt upon.

The information given under the head of "History" has a general as well as a technical value. All sorts of writers, ancient and modern, have been laid under tribute; and the glimpses one obtains, not only of the medical but of the domestic employment of drugs in past times, are full of interest.

This running commentary need not be extended to all the headings under which the treatment of each substance is arranged. The term "Substitute" as distinct from "Adulteration," perhaps needs a word of explanation. It is employed to comprise substances occasionally met with in commerce, the product of plants more or less closely allied to the officinal one; for instance, the wood of *Quassia amara* instead of that of *Picræna excelsa*, the occurrence of the root of *Aristolochia reticulata* in place of *A. serpentaria*, or of the dried plant of *Piper aduncum* in lieu of the true Matico.

The notices of Indian officinal drugs have the interest of novelty to European students, but beyond this leave little room for present remark. In course of time some of them may be introduced at home, and in any case, with the amount of communication which exists between England and her Eastern possessions, nothing which concerns the one can be unimportant to the other. Indian medical men are largely drawn from this country, and by them, at least, they will be gratefully received.

The only department of the book which does not yield unalloyed satisfaction is that which refers to "Microscopical Structure." The descriptive paragraphs are, no doubt, as good as words can make them, but mere words are insufficient for the purpose. If anyone doubts this, let him try to construct a drawing of microscopic structure from a description, and then compare it with the reality; or, on the other hand, let him endeavour to identify one vegetable production out of a number closely allied, by means of a mere verbal definition of characters. Either task is difficult at best, sometimes impossible. It is not to our credit that there should be no British work of reference containing a complete series of illustrations of the anatomy of drugs. What is wanted is not so much an elaborate atlas, like that of Dr. Berg, with large, ideal, diagrammatic drawings, suggested by the microscopic appearance of the various vegetable products used in medicine, as a set of figures of characteristic portions of structure presented in a form in which the working student may recognise them. How welcome such an addition to the book would have been from Prof. Flückiger's skilful hand. It is only just to the authors to state that they make no claim for completeness in this division of the work; indeed, they are so fully aware of what is needed, that one might almost indulge in the hope of seeing a second edition with a supplementary volume of plates.

In a brief and imperfect notice like the present but scanty justice can be done to a book like the "Pharmacographia," a work which, from the amount of its original matter, the laborious verification of its facts, the accuracy of its references, and the extent of general erudition it reveals, will be received with no grudging welcome, and will be recognised at once and without misgiving as the standard of authority on the subjects of which it treats.

HENRY B. BRADY

SULLY'S "SENSATION AND INTUITION"

Sensation and Intuition: Studies in Psychology and Æsthetics. By James Sully, M.A. (Henry S. King and Co.)

A YOUNG aspirant to the woolsack had as part of his first examination the question, "To whom was the Declaration of Rights presented?" To refresh his memory he cast his eyes on the paper of the gentleman on his left, who had written William I.; willing to give himself every advantage, he next stole a glance at the paper of the gentleman on his right, where he saw William III. "Ah!" thought he, with a knowing twinkle of the eye, "I'll strike the happy medium"—and down went William II. Mr. Sully, in the first of this collection of interesting essays, has struck the happy medium between the evolution and the individual experience psychologies.

Mr. Sully has read and pondered all the learning of his subject; but the thoroughgoing evolutionist is not unlikely to accuse him of having done more than "shaded for a moment the intellectual eye from the dazzling light of the new idea." If, as we are told, "it is far from improbable that a fuller investigation of the processes by which our conceptions of *space* are built up, will render superfluous the supposition of their innateness," it is not at all probable that *any* other conceptions are inherited. And the evolutionist will not, we fear, be able to draw much comfort from the assurance that "the psychologist, when satisfied of the presence of distinct mental phenomena not traceable to the action of his own laws, will gratefully avail himself of the additional hypothesis supplied to him by the philosopher of evolution;" for it not unfrequently is very difficult indeed to satisfy the psychologist of the presence of anything not traceable to the operation of his own laws. An authority in psychology writing in "Chambers's Encyclopædia," says that the assertions with regard to the instinctive perceptions of distance and direction by the newly hatched chick are, "in the present state of our acquaintance with the laws of mind, wholly incredible." We now know that the chick has not the least respect for those laws of mind; and we have already in these columns (NATURE, vol. vii. p. 300) argued that we have no sufficiently accurate acquaintance with the alleged acquisitions of infancy to justify the doctrine that they are different in kind from the unfolding of the inherited instincts of the chicken. To what we then said Dr. Carpenter has replied on one point in his "Mental Physiology" (p. 179). While admitting that human beings require no education to enable them "to recognise the direction of any luminous object," he maintains "that the acquirement of the power of visually guiding the muscular movements is *experiential* in the case of the human infant." In support of this somewhat inconsistent position, he gives facts within his own knowledge which we do not feel to be in the least inimical to the doctrine against which they are arrayed. Mr. Sully is more consistent; he thinks it proveable that the eye has no instinctive knowledge of either the distance or the direction of a visual object. He relies greatly on "Recent German Experiments with Sensation" (the subject of his third essay), which, like Dr. Carpenter's facts, appear to us in perfect harmony with the theory they are supposed to disprove. Without doubt, there is no higher scientific authority than Helmholtz, and just for this reason is it specially instructive to observe how readily even he accepts as statements of fact what never could have been more than the suggestions of theory. In the last of his admirable course of lectures on "The Recent Progress of the Theory of Vision," he says: "The young chicken very soon pecks at grains of corn, but it pecked while it was still in the shell, and when it hears the hen peck, it pecks again, at first seemingly at random. Then, when it has by chance hit upon a grain, it may, no doubt, learn to notice the field of vision which is at the moment presented to it." In this list of assertions, even the one that might seem most certainly true is a mistake. The chicken does not peck while still in the shell; though that it does so is, we believe, the universal opinion, the actual mode of self-delivery having never been observed. The movement is just the reverse of pecking. Instead of striking forward and downward (a movement impossible on the part of a bird packed in a shell with its head under its wing), it breaks its way out by vigorously jerking its head upward and backward, while it turns round within the shell. With the advance of knowledge, theories will have, though it may be reluctantly, to accommodate themselves to facts; and after the din of the battle is over, it will be found that the real facts had never had any difference among themselves.

Mr. Sully differs from Mr. Spencer as to the relation of the evolution hypothesis to the question of realism and idealism. He is aware that Mr. Spencer "distinctly affirms that the reality of an independent unknowable force is necessarily involved in his theory of evolutional progress. But this," Mr. Sully observes, "can only mean that every distinct conception of subject and object involves this postulate; and this assumption can hardly fail to strike one as a *petitio principii*, inasmuch as able thinkers have undertaken to find the deepest significance of this antithesis in purely phenomenal distinctions." Perhaps Mr. Spencer might be able to produce instances in which the facts of the universe have turned out not exactly what able thinkers had undertaken to find them. Considerable strain is put by Mr. Sully on Mr. Mill's formidable definition of matter—that it is "a permanent possibility of sensation;" but we greatly fear that when brought to close quarters the idealist that puts his trust in this verbal monstrosity will find himself left in the lurch. Somehow through "processes of repeated experience and sharpened intellectual action, the mind comes," we are told, "to conceive a possible impression as the originating cause of a present one, and so to arrive at that vast stream of objective events which flows on beyond, and independently of, the actual series of feelings making up its own individual life." To follow this from the idealist's point of view is quite beyond us. A belief in permanent possibilities of sensation that flow on independently of our feelings is in some danger of being mistaken for realism. Mr. Sully, however, is very sure that the realists are wrong; and as a psychologist he must be able, by aid of his science, to explain their error, just as an astronomer accounts for an eclipse. This is how our realistic philosophers go wrong. Under the influence of a refined sentiment of awe, they see what is not there. Not only does this emotion "lead the mind to anticipate the presence of insoluble mystery where a calmer intellectual vision sees only clear regularity, but it serves to support conceptions of an unknowable where the closest observation and most accurate reasoning fail to detect any signs of such an existence." The superstitious terror of the rustic transforms a white calf into a ghost; the awe of the philosopher sees a ghost where there is no calf.

In a very suggestive essay Mr. Sully handles the difficult subject of "Belief: its Varieties and its Conditions." He finds "the primitive germ of all belief, the earliest discoverable condition that precedes in its influence that of action, in the transition from a sensation to an idea." In thus attempting to understand how the state of mind called belief resembles, differs from, and is related to other states of consciousness, Mr. Sully is, we think, on the right track. He is, however, by no means free from the crude, popular notion, that belief and volition, considered as facts of consciousness, have some special causal connection with the bodily movements. Indeed, he thinks that Prof. Bain "has succeeded most completely in showing the will to be a secondary and composite state of mind, inferable from more rudimentary states," one of these so-called rudimentary states being spontaneous bodily movements, which occurring by "a coincidence purely accidental" along with states of consciousness, these unlike things get somehow stuck together by "an adhesive growth, through which the feeling can afterwards command the movement." We have repeatedly maintained that while on the one hand there are reasons which seem to compel the belief that on his physical side man is a machine whose movements can never escape by a hair's breadth from the inexorable rule of physical law, there is on the other hand no "better ground for the popular opinion that voluntary movements take their rise in feeling and are guided by intellect, than a superficial observer ignorant of the construction of the steam-engine might have for a belief that the movements of a locomotive take their rise in noise and are guided by smoke." * That Prof. Huxley's bold advocacy of this view at the recent meeting of the British Association has not called out more angry criticism is surely a most hopeful sign of the times.

It is with regret that we must now take leave of this collection of essays, which we have read with pleasure and profit; and we hope that our mode of expressing our criticisms will not be misunderstood or supposed to indicate a want of appreciation. To touch on all the points we had marked for observation would more than double the length of this review. Especially do we regret not being able to say a few words about "The Æsthetic Aspects of Character." If Mr. Sully could admit that conduct cannot be beautiful in so far as it involves struggle, mental effort, for example, in so far as it is moral or virtuous on the subjective side, very little would then stand between him and one commanding generalisation.

DOUGLAS A. SPALDING

* NATURE, vol. ix. p. 179: "The Relation of Body and Mind."

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. No notice is taken of anonymous communications.*]

Sounding and Sensitive Flames *

II.

ANOTHER example of a highly sensitive flame was recently described to me which seems to show that air-currents flowing through gauze at a proper speed are sensitive without the intervention or simultaneous superaddition of a flame. A special kind of Bunsen burner was made with a spiral mixing tube coiled in an inverted cup, at the centre of which is a small chamber covered with wire-gauze at the foot of a short tube or flame-pipe. The gas is admitted by a single jet passing through a cap of wire-gauze covering the conical opening of the spiral tube, the object of this cap of gauze being to distribute the air in its approach, and to protect the gas-jet from ignition. The gas-flame burns with a small bright green cone, surmounted by a larger envelope of pale reddish flame, and it is intensely hot. The green cone indicates combustion of the most complete explosive mixture of air and coal-gas, and when the burner is properly adjusted it can only burn on the top of the flame-tube, where it finds the additional required supply of oxygen; but it descends to the wire-gauze at the foot of the tube if the air-supply exceeds, or the gas supply falls short of the right proportion. In some of these burners the slightest noise of the kind that commonly affects sensitive flames causes the cone of green flame to retreat into the tube and settle on the wire-gauze at its foot, whence it rises again immediately to the top of the tube, when the sound ceases. The explanation seems to be that the air-current entering the mixing-tube through the outer gauze cap is in a sensitive condition, and that when thrown into disturbance by the external sounds, it is more quickly seized and is drawn into the mixing-tube more rapidly by the gas-jet than when it is flowing over the jet in a tranquil state. The inventor of these burners, Mr. Wallace, assures me that some of them exhibit the most sensitive of sensitive flames, and that he has more than once thought of sending one of them as a most singularly effective illustration of such flames to Prof. Tyndall.

The explanation here given of the sensitiveness of Wallace's Bunsen-flame appears to be in great part correct; but the behaviour of the flame, which by Mr. Wallace's kindness I have seen since the above was written, differs considerably from that described; and some experiments connected with it lead me to modify to some extent the foregoing theory of the origin of sensitiveness in wire-gauze flames, and even, apparently, to except the gauze itself from any considerable share of mechanical action in the process. The gas in this burner is first turned low, until the green cone at the centre nearly disappears, and merges into the outer border of the flame from less effective mixture of air with the gas at a low speed of the jet. The flame is now sensitive to the smallest sound, mounting fully one-half higher at every word, or even syllable of a speaker, and at the stroke of a bell, or other acute sound, reaching about twice its ordinary height. It undergoes at the same time no change in its appearance, showing that the contents of the mixing-tube and chamber are merely urged out of the flame-tube with greater speed by some forward impulse of the jet behind. If the sound is continued, as by constantly ringing a bell, the expanded flame gradually subsides, from the expulsion of all the inferior gas-mixture in the burner, reaches its first stature, and passes into a condition of more concentrated combustion corresponding to a fuller, and therefore more rapid admission of gas to the jet; when the sound ceases, the contracted flame gradually recovers its first size and diffuseness from the same cause, namely, the expulsion of all the well-aërated gas

* Continued from p. 6.

in the burner by an inferior mixture which succeeds it at a slower speed.

From the following experiment and considerations I am inclined to attribute the observed action of the disturbed flame almost entirely to direct influence of the sound upon the gas-jet, rather than to its effect upon the current of air passing through the conical cap of gauze that surrounds it. The current through the gauze is so slight that ascending smoke, slowly creeping round it, is not visibly drawn into its meshes. The sensitive action of the flame remains equally perfect when all but a very small aperture of the gauze is closely covered with thin sheet india-rubber. To determine if a naked jet, unsurrounded by wire-gauze, would by itself produce a flame so sensitive, I easily obtained with a Ladd's tapering brass jet a flame of this description. Laying it upon its side with its point inclining downwards, and inserting this into a brass tube about ½ in. wide and 15 in. long, also inclined, the flame at the lower end of this tube, when full gas was used, resembled a Bunsen-flame; but if the gas-supply is lowered, it becomes luminous; and at the lowest point at which it will continue to burn, the slight current in the tube appears to consist only of nearly pure coal-gas, and is of course (a useful point in the manipulation) quite inexplosive. A stamp, a cough, or other deep-pitched sound, as the exclamations Oh! and Ah! caused this flame to emerge from its hiding-place in the end of the tube into which it had retreated, and to rise in a tall tongue of light. It was not sensitive to notes of high pitch, to a hiss, nor to some of the acuter vowel-sounds of the voice, unless very strongly uttered; but a short groan or growl called it forth at once. The lower the speed of a jet the slower, possibly, may be the vibrations required to affect and sensibly to disturb its equilibrium. With a very perfect gas-meter the question might also be decided how much of the large additional gas-volume in the flame which occasionally reached a height of about 2 in., and which could easily be maintained permanently at a height of about 1 in. by continued stamping on a stone floor, is derived from the gas-jet itself, and how much from increased admixture with it of the surrounding air. As the jet is constantly being bent, as it leaves the fixed nozzle, into the shape of a corkscrew, or of some other wave-curve by the air-vibrations, it probably draws more air along with it, in the same way that a coarsely twisted rope in hair-rope pumps raises more water than a smooth belt or a perfectly smooth and straight rope would do. Something of this kind, perhaps, may be supposed to take place; and contrary to the opinion which I at first entertained, above, of the cause of the sensitiveness at low gas-pressures of Barry's sensitive wire-gauze flame, it seems more probable that the flurry and depression of the flame produced by external sounds is the result of their action upon the gas-jet below, mixing the gas more thoroughly with air, and giving it explosive properties before it passes through the gauze. The gauze-flame must be regulated by lowering the gas-jet, until the brink of its stability and tendency to collapse and burn noisily on the gauze is nearly reached, in order to make this destruction of its equilibrium by external noises possible; and the explanation thus offered of the sensitiveness of the gauze-flame at lower gas-pressures than those used with other flames depends upon no assumption of mechanical actions of unusual delicacy, or indeed of any peculiar kinds of undulation taking place among the perforations of the gauze.

I have quite recently seen an instrument connected very closely with the acoustical properties of flames burning on wire-gauze, showing how well instrument-makers have appreciated them, and how actively they are engaged in representing them in a convenient form. It resembles Geyer's sounding modification of Barry's sensitive flame so nearly, that but for its having received no such title from the maker, the source of its original invention might scarcely be considered doubtful; but it appears more probable, as will be seen from a description in NATURE (to be shortly again referred to) by Dr. A. K. Irvine (vol. x. p. 273), of Glasgow, of identically the same instrument patented many years ago for a very different purpose, that the designer of this singing tube may also have been guided by a knowledge of that invention. Even allowing for the general knowledge of the acoustical properties of wire-gauze flames that has for a long time existed, the instrument shows signs of originality of design that cannot easily be accounted for without some such consideration. It consists of a brass stand with two sliding brackets, one of which supports, in a split cork, a glass tube tapered above to a point to mix a jet of gas with air. The other arm supports a brass tube five-and-a-half inches high, and about an inch and three-quarters wide, closed at the bottom with a disc of gauze held there against a fixed rim in the tube by a wire ring. The position of the gauze close to the bottom of the tube and that of the tapering gas-jet under it, as well as the dimensions of the tube, are the counter-part of Mr. Geyer's experiments with Barry's sensitive flame, only differing in want of adjustibility of the relative positions of the tube and diaphragm of wire-gauze from his arrangement. The arrangement itself is, however, on the other hand, exactly that which Mr. Irvine patented, as will soon be seen, twelve years ago, for use in a new description of miner's safety lamp. The sound produced, when the flame is lighted on the wire-gauze inside the tube and the jet below it is fixed at a proper height, is, as might be anticipated from its high pitch, answering to the short length of the open tube, an excruciatingly piercing note.

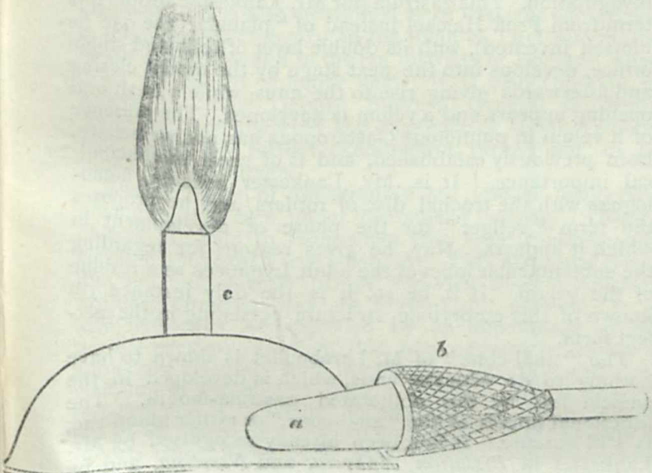
I was not aware that the effect of heat alone in gauze-diaphragms to produce musical sounds in open tubes had been observed and investigated, as it is stated to have been by Prof. Barrett, so thoroughly by Prof. Rijke, of Leyden; and a perusal of that author's description of his experiments, and of his comments upon them, would undoubtedly be of exceeding interest. That the experiment has often been repeated since, and has been varied in many ways by those who were acquainted with it, is a consequence that I was fully prepared to learn, from its great beauty, would follow very speedily upon the first publication of its discovery.

I have never examined sounding and sensitive flames with revolving mirrors; but the result could scarcely fail to prove very instructive. The indications of his own essays in pursuit of this method contained in Prof. Barrett's letter, both where I have been able to consult the original writings and drawings that he quotes, and where he offers us a short account of further results apparently more noticeable than those obtained before, of the appearance of a particularly active and impressionable sensitive flame affected by the vowel sounds, when viewed in a moving mirror, show that the characteristic comportment of these flames is eminently adapted for examination and discussion by such a mode of observation.

Similar experiments on the chirruping, whistling, trumpeting, and other sounding open flames, obtained by the collision of two jets, examined by Prof. Tyndall and Mr. Cottrell, here suggest themselves; but I must hasten to bring this long excursive letter to a close. I cannot, however, do so without expressing my obligation to Prof. Barrett for the valuable references and information that he has been good enough to supply, and for the prompt and ingenuous manner in which he kindly rectified my oblivious association of his name with Mr. Barry's in certain recent observations of the sensitiveness of wire-gauze flames. The notices contained in a short space in his most interesting letter gave me a better acquaintance with the progress of this wide and curious subject, than repeated and anxious inquiries concerning it for several months previously in the scattered pages of many recent scientific journals had enabled me to acquire.

I must also add my acknowledgments to Mr. T. S. Wright and to Mr. A. K. Irvine for the interesting notes that they have furnished in NATURE (vol. x. p. 273, and p. 286) on the early use of wire-gauze flames to produce vociferously loud sounds in open tubes. That large iron tubes specially fitted inside with gauze-covered (or the so-called "smokeless") gas burners, to produce a mighty sound, should be preserved as working instruments of a chemical laboratory in Edinburgh as long ago as the year 1842; and that as much as twelve years since a kind of safety-lamp for mines was patented by Mr. Irvine in this and other countries, sounding a loud alarm note when the lamp-flame lights the explosive mixture of fire-damp entering the bottom of the wick-tube through a wire-gauze disc placed there to cover it, are facts that need no comments to show that the surpassing power of such flames to excite and sustain musical sounds has long been known and used successfully. The excellent character and performance of the instruments used in 1842, as described by Mr. Wright, makes it probable that frequent illustrations of the same kind must already have preceded them. On the other hand, from the well-known scientific eminence of their possessor, Dr. David Boswell Reid, as a skilful director of large works of ventilation, it may also be presumed that they probably presented to his views novelty of some special kind, either of invention or of construction, or of both combined, the result of which was the production of several such superior instruments. It may not be impossible from this consideration, at least if no evidence of considerably earlier origin could be produced, to fix the time, and perhaps the authorship by Dr. Reid himself, or by his brother the chemist, Dr. William Reid of

Edinburgh, in whose laboratory Mr. Wright practised with them, of the first use of smokeless coal-gas flames in acoustical experiments as not long anterior to the date named by Mr. Wright as that of his practical experience of their use. But it must be borne in mind that of all highly inflammable and intensely heating gases next to hydrogen, the most easily procurable since the general extension of the use of coal-gas, is an explosive mixture of the latter gas with air; and the experiments of Sir H. Davy, in 1816, having demonstrated that such a mixture may be prepared safely underneath wire-gauze and may be safely burned above it, the use of the wire-gauze flame for laboratory heating purposes, and also to illustrate very suitably the chemical harmonicon, must have been a very early suggestion. Its unwieldy size and stentorian proportions for the latter purpose, however, have not impossibly led to its comparative abandonment and



J. WALLACE'S TABLE BUNSEN-BURNER.

a.—Conical and spiral mixing-tube coiled inside the foot, terminating at the centre in a small chamber closed with wire-gauze at the top, at the foot of the flame-tube. *b.*—Conical wire-gauze cap, strengthened by three wires to support the gas-tube, to protect the gas from ignition, to keep off draughts, and to distribute the current of air to the gas (*junctions* all soldered). *c.*—Short flame-tube, closed at the bottom with wire-gauze to prevent the flame from flashing back when the gas is turned on or off. Whole height about $2\frac{1}{2}$ in. Height of flame, $1\frac{3}{4}$ in. or 2 in. Height of central bright flame, exactly $\frac{1}{2}$ in.

disappearance from the scene of modern laboratory experiments, and to its general replacement, in coal-gas illustrations of the chemical harmonicon, by various modifications with different forms of jets, of the much more portable, convenient, and easily adaptable Bunsen-burner. Thus a long-recognised and important application of gauze-topped gas-burners in the student's scientific practice might have fallen into oblivion, or into disuse and comparative neglect, if contemporaneous experiments like those of Irvine, Barry, Govi, Geyer, Rijke, and it may safely be prophesied of many other active fellow-workers in the same field of discovery and research, did not revive the discussion, and continue to develop the observation of these flames with multiplied results that appear to be in perfect accordance with the principles, and to furnish the most beautifully effective illustrations possible of important properties of effluent gas-currents, which would perhaps otherwise escape detection. The laws of the flow of escaping gas-jets, their powers of producing ventilation and exhaustion, and, on the other hand, the means of providing for their escape with as little waste of their energy as possible, are questions of practical importance in so many useful industrial applications, that they amply deserve the increased measure of scientific attention which the beautiful succession of modern discoveries of sensitive and sounding flames has been very materially instrumental in attracting, and appears still further to be eminently capable of directing towards them.

Newcastle-on-Tyne, Oct. 19 A. S. HERSCHEL

Insects and Colour in Flowers

IN his second letter (NATURE, vol. xi. p. 28) Mr. Mott passes to the discussion of the general question whether beauty is an "object in nature." On that point my feeling is that our knowledge is as yet far too limited for us to presume to declare with any confidence what is an object in nature. Still less should we venture to assert what is *not* an object, and least of all have we any right to affirm that beauty is not an object, when we see developed, beauty of form, of colour, of sculpture and marking, so constantly throughout the organic world, and by such a great variety of means. Sometimes beauty of colour undoubtedly exists when, so far as we can see, it confers no benefit whatever on its possessor. Mr. Darwin instances arterial blood and the autumnal tints of leaves. More frequently it is accompanied by some advantage, direct or indirect; and the question is whether in *such* cases it has been acquired through the operation of sexual or natural selection, more particularly whether in the case of flowers the selection has been effected through the agency of insects, which have favoured the most conspicuously coloured. It remains with Mr. Mott to show in what way the facts detailed in his original letter (I hope he will pardon me for taking him back to it) fail to harmonise with that doctrine. To my mind the fact that a cultivator, by carrying out a like selection, propagating from plants which bear the largest and brightest, double or showy sterile flowers, can produce like results, supports and corroborates the doctrine rather than militates against it. Nor can I see anything discordant in the fact that the colour of fruits has been acquired through the medium of an entirely different selecting agent.

One circumstance appears to me to present some difficulty; and, although it is in no way connected with Mr. Mott's letter, I should like to mention it in the hope that others may be able to supply a satisfactory explanation: it is the case of flowers that are coloured on the outside, but white within. Where such flowers from their position or form present to view principally their exterior, as *Tulipa celsiana*, this is an adaptation that can be readily understood; but some display mostly their interior, and it is then difficult to understand the acquirement of colour outside only. I would instance *Simethis bicolor*, *Gypsophila cretica*, *Daphne jasminea*, and several species of white-rayed Compositæ. *Bellidiastrum michelii*, for example, has frequently the inner surface of the ray florets quite white, and when the flower is open nothing else is seen; the colour on their outer surface only becomes visible when they close over the disc, as in dull and rainy weather. THOMAS COMBER

Newton-le-Willows, Nov. 16

WITH reference to this question, is cross-fertilisation so desirable for the plant as is stated?

In this country, and I believe as a rule elsewhere, brilliant flowers are produced by shrubs, climbing and herbaceous plants, while the inflorescence of trees is comparatively inconspicuous. Does it not seem probable that beauty of colour is gained at the expense of strength, majesty, and longevity? J. S. H.

Droseræ

I FIND that during my absence from England many applications have been made for plants of the Droseræ and Pinguiculæ, and from the replies which have been sent on receipt of the plants they seem to have given satisfaction. Lately, however, in consequence of the weather, there has been some difficulty in obtaining *D. intermedia*, but before this is printed in your columns, all existing applications will be cleared off.

I wish to add, that in winter these plants can scarcely be expected to be as active as in spring and summer, and observers must wait patiently until spring before they may hope to obtain successful results from their observations: it cannot be necessary, I think, to *feed carnivorous plants artificially during the winter*; and a hot-house or conservatory cannot be absolutely necessary, as they have no such advantages in their native wilds.

G. H. HOPKINS

Suicide of Scorpions

THAT scorpions do commit suicide, as described by your correspondent last week, is a well-known fact. My grandfather often related how he had seen these creatures, when surrounded by a circle of glowing embers, make for the inner side of their fiery prison, then deliberately move round the inside of the circle, and when arrived at the exact spot from which they started, turn back their tails and sting themselves to death.

Clyde Wharf, Nov. 16 M. L.

The Cry of the Common Frog

In NATURE, vol. x. p. 461, Mr. Mott notices the cry of the common frog when annoyed. One of the greatest enemies of this frog in the United States is the common striped snake (*Tropidonotus tænia*, Dekay). He seizes the frog by the hind legs for the purpose of swallowing him, when the latter will utter a most pitiful cry. I have detected them in this condition at a distance by the frog's note. I have amused myself by taking a frog by the hind legs and dragging him slowly backwards on the ground in a serpentine direction, when he will exhibit his characteristic wail to perfection; and, when released, he will frequently utter some apparently intelligent imprecations as he hops off out of reach. I have noticed the same effect produced by a playful kitten amusing itself by teasing the frog, seemingly for the purpose of hearing him cry. Sliding a stick after him like a snake will produce the same results in a still more striking manner. A. T. T.

Oswego, U.S., Oct. 29

Phylloxera Vastatrix

CAN any of your readers kindly inform me where a specimen of *Phylloxera vastatrix* can be obtained?

Ipswich A. HARWOOD

A Nest of Young Fish

WHILE on the point of taking my accustomed morning plunge in one of the clear pebbly streams that find their way into the plains from the northern mountain ranges of the island of Trinidad, my attention was attracted by the eccentric movements of a small fish of the perch tribe. In general this fish is extremely shy, scudding off into deep water or under some overhanging bank on the approach of man; on this occasion, however, on putting my hand into the water, the fish, to my astonishment, darted forward again and again, striking my hand with considerable force. Rather at a loss to account for such temerity in a fish only 4 in. long, I watched its movements narrowly, and at last found out the cause. In a small hollow close by, about the size of half an egg, artistically excavated from the bright quartz sand, a multitude of tiny fish were huddled together, their minute fins and tails in constant motion. They had apparently been only very recently hatched, and were no larger than common house flies; the parent fish kept jealous watch over her progeny, resenting any attempt on my part to touch them.

Next morning, accompanied by my father and brothers, I returned to the spot which I had carefully marked the day before. For some time, however, we searched in vain for the fish and her young; at length, a few yards further up stream, we discovered the parent guarding her fry with zealous care in a cavity similarly scooped out of the coarse sand; any attempt to introduce one's finger into the hollow was vigorously opposed by the watchful mother. This is the first and only instance that has come under my notice of a fish watching over her young, and conveying them, when threatened by danger, to some other place. The clear streams that flow along the valleys among the northern mountain ranges of the island abound with fish of the variety I refer to; they are in general of a bright yellowish brown, with two or more silvery stripes on the sides, and seldom exceed five inches in length; but in the sluggish turbid rivers of the plains, the bright colours change to a dull brown; the fish are larger, however, varying in size from eight to ten inches. Extremely tenacious of life, these fish, in common with several other species, have the power of existing in a semi-torpid state for weeks, and even months, buried during severe droughts in the mud of dry watercourses, where they are dug up by the Creole peasants, who prize them as food; but from the peculiar earthy flavour common to many varieties of freshwater fish frequenting the muddy rivers of the low lands, they are not relished by the more fastidious palate of the European.

ROBERT W. S. MITCHELL

THE DEVELOPMENT OF MOLLUSCA

MR. RAY LANKESTER, in the current number of the *Quarterly Journal of Microscopical Science*, gives the results of his examination of the embryo of the common Pond Snail (*Limnæus stagnalis.*) These are of great importance; first, because they show how much may be done by trained observation, with improved methods, of a very common form, which has already been studied by excellent anatomists; and secondly, because Mr. Lankester's previous investigations into the development of cuttles, *Pisidium*, and several marine gasteropods, enable him to form a sound judgment of the bearing of his discoveries upon questions of homology and of classification.

In Limnæus, Mr. Lankester finds that the process of segmentation (which is well illustrated by drawings of the egg in various positions at the several stages) is followed by the formation of a gastrula through a process of invagination. This gastrula (for Mr. Lankester adopts this term from Prof. Hæckel instead of "planula," the one he himself invented), with its double layer of cells and single orifice, develops into the next stage by the mouth closing and afterwards giving rise to the anus, while a fresh oral opening appears and a velum is developed. The presence of a velum in pulmonate Gasteropoda has not, we believe, been previously established, and is of great morphological importance. It is, Mr. Lankester believes, homologous with the trochal disc of rotifers, and he proposes the term "veliger" for the phase of development in which it appears. Nay, he gives reasons for regarding the subtentacular lobes of the adult Lymnæus as a residue of the velum. If it be so, it is the only instance yet known of this embryonic structure persisting in the perfect form.

The "anal cone" of M. Lereboullet is shown to have nothing to do with the anus, which is developed in the pedicle left by the obliterated gastrula-mouth. The functional import of the "anal-cone," or rather gland-sac, is still obscure. It has been already recognised by Mr. Lankester in *Pisidium*, *Aplysia*, and *Neretina*, and by Hermann Fol in embryo Pteropoda. It is possibly homologous with the basal gland described by Keferstein and Kowalevsky in *Loxosoma* among Bryozoa, and with a similar structure in *Terebratula*. The more difficult questions of its homogeny with the rudimentary internal shell of the slug, and with the pen-sac of cuttles, are also discussed. One of the most curious facts about this "shell-gland" is that it frequently becomes filled with a homogeneous refracting secretion apparently chitinous in composition, which is a morbid, or at least an abnormal change, and associated with irregular development of the embryo.

Not the least valuable point established in this interesting memoir is that the rotation of the embryo Lymnæus is caused by numerous short cilia on the annular band which afterwards forms the velum. The discovery of these cilia, which were sought by Lereboullet without success, is probably due to Mr. Lankester having used perosmic acid, a reagent which is exceedingly useful in examining transparent Tunicata, and seems equally suited for displaying cilia anywhere.

The gastrula form appears apparently in all groups of animals but the highest and the lowest, in some form or other; but the "shell-gland" forms a valuable additional link between the Brachiopoda and Polyzoa on the one hand and the higher Molluscs on the other. If this be admitted, it is probable that Tunicata may be again admitted to the same great stem in spite of their undoubted affinities to vertebrates by *Amphioxys*, and to worms by *Balanoglossus*.

It is a most satisfactory sign of the revival of embryology in England, that in the same number of the *Quarterly Microscopical Journal* which contains this important memoir by Mr. Lankester, there is also the preliminary account of the development of Elasmobranchii, by Mr. Balfour, which excited so much interest at the late meeting of the British Association.

*ON MIRAGE**

THE name of "Mirage" is applied to certain illusory appearances due to excessive bending of the rays of light in their passage through the atmosphere. These appearances are by no means uniform.

Sometimes, especially in hot countries, the observer loses sight of the ground beyond a certain distance from his position, and sees in its stead, what looks like a sheet of water, either calm or with movements resembling waves; and if any distant objects are sufficiently lofty to be seen above this apparent lake, their images are seen beneath the objects themselves, inverted as if by reflection in this imaginary water. The dry and hot soil of Egypt is famous for the production of this form of the phenomenon. It is also mentioned as of frequent occurrence in the plains of Hungary, in the plain of La Crau in the South of France, and in the fen districts of England when dried up by the summer heat. It is also common in Australia. The Deputy Surveyor-General of South Australia once reported the existence of a large inland lake, which on further examination turned out to be nothing but a mirage.

Another class of appearances are known (especially among nautical men) under the name of *looming*. Distant objects are said to loom when they appear abnormally elevated above their true positions. This abnormal elevation not unfrequently brings into view objects which in ordinary circumstances are beyond the horizon. It is also frequently accompanied by an appearance of abnormal proximity (though this may perhaps be rather a subjective inference from the unusual elevation and clear visibility of the objects than a separate optical characteristic), and it is further accompanied in many, though not in all cases, by a vertical magnification, the heights of objects being many times magnified in comparison with their horizontal breadths, so as to produce an appearance resembling spires, pinnacles, columns, or basaltic cliffs. Some beautiful descriptions of these latter appearances, with illustrative plates, are given in Scoresby's "Greenland," the objects thus magnified being icebergs; and a very full and interesting account of the phenomena of mirage, as observed in high latitudes, will also be found in the "Arctic Regions" of the same author.

It is usually across water that looming is observed; and as a surface of water stands naturally in contrast with a sandy desert or a surface of parched land, so also the optical effects produced are, in a manner, opposite. The inverted images which are often presented in looming are not beneath the object, as in the case of mirage on dry land, but above it, as is formed by reflection in the sky. The only examples that I have myself seen of mirage were of this kind. They were seen across sheets of calm water, the hills on the other side being seen with fictitious hills upside down resting on the tops of the real hills. In rare instances, two or even three of these images are seen one above another, vertically over the real object; but these multiple images are usually too small to be seen without the aid of a telescope—the objects whose images they are being so distant as to appear mere specks to the naked eye.

There is always more or less of change observable in the images formed by mirage, and the changes are greatest and most sudden when the images are most distorted, as compared with the true forms of the objects. The appearances also change with the height of the observer's eye. Looming is seen to the greatest advantage from an elevated position, such as the mast-head of a ship. The mirage of dry land is sometimes visible at any moderate height, but in other cases—especially in countries which are not very hot—the range of height from which it is visible is extremely limited. A very fine mirage, recently observed in the fen districts, was only seen when the observer was on the top of the marsh wall. But this case seems to have been peculiar. It was accompanied by the further peculiarity that a strong wind was blowing—the general rule being that mirage is only seen in calm weather. Observers of mirage on the sands of Morecambe Bay, and of the Devonshire coast, state that it could frequently be only seen by stooping.

Mirage is seldom seen in winter. The hot shining of the sun seems to be an invariable antecedent; and this is true even of the polar regions, where Capt. Scoresby attributes the phenomenon to "the rapid evaporation which takes place in a hot sun from the surface of the sea, and the unequal density occasioned by partial condensations, when the moist air becomes chilled by passing over considerable surfaces of ice."

Time will not allow me to do much in the way of quoting the very numerous records which exist. Scoresby's accounts alone would almost suffice to occupy the evening, and I would again refer to them as models of accurate observation and effective description. I will content myself with quoting nearly in full the account of a mirage observed at Hastings and neighbouring parts of the south coast of England in 1798, as given in the Philosophical Transactions for that year, the narrator being Mr. Latham, F.R.S.:—

"On Wednesday last, July 26, about five o'clock in the afternoon, whilst I was sitting in my dining-room at this place (Hastings), which is situated upon the parade, close to the sea-shore, nearly fronting the south, my attention was excited by a great number of people running down to the sea-side. Upon inquiring the reason, I was informed that the coast of France was plainly to be distinguished with the naked eye. I immediately went down to the shore, and was surprised to find that, even without the assistance of a telescope, I could very plainly see the cliffs on the opposite coast, which at the nearest part are between forty and fifty miles distant, and are not to be discerned from that low situation by the aid of the best glasses. They appeared to be only a few miles off, and seemed to extend for some leagues along the coast. I pursued my walk along the shore to the eastward, close to the water's edge, conversing with the sailors and fishermen on the subject. They at first could not be persuaded of the reality of the appearance, but they soon became so thoroughly convinced, by the cliffs gradually appearing more elevated and approaching nearer, as it were, that they pointed out and named to me the different places they had been accustomed to visit, such as the Bay, the Old Head or Man, the Windmill, &c., at Boulogne, St. Valéry, and other places on the coast of Picardy, which they afterwards confirmed when they viewed them through their telescopes. Their observations were, that the places appeared as near as if they were sailing at a small distance into the harbours.

"Having indulged my curiosity upon the shore for near an hour, during which the cliffs appeared to be at some times more bright and near, at others more faint, and at a greater distance, but never out of sight, I went upon the eastern cliff, which is of a very considerable height, when a most beautiful scene presented itself to my view; for I could at once see Dungeness, Dover cliffs, and the French coast, all along from Calais, Boulogne, &c., to St. Valéry, and, as some of the fishermen affirmed, as far to the westward as Dieppe. By the telescope, the French fishing-boats were plainly to be seen at anchor, and the different colours of the land upon the heights, together with the buildings, were perfectly discernible. This curious phenomenon continued in the highest splendour till past eight o'clock, . . . when it gradually vanished. The day was extremely hot, . . . not a breath of wind was stirring the whole of the day. . . . A few days afterwards I was at Winchelsea, and at several places along the coast, where I was informed the above phenomenon had been easily visible.

* A Paper read by Prof. J. D. Everett, M.A., D.C.L., before the Belfast Natural History and Philosophical Society.

"I should also have observed that when I was upon the eastern hill, the cape of land called Dungeness, which extends nearly two miles into the sea, and is about sixteen miles distant from Hastings, in a right line, appeared as if quite close to it, as did the fishing-boats and other vessels which were sailing between the two places. They were likewise magnified to a great degree."

I have stated that the phenomena which constitute mirage are due to the bending of rays of light in the atmosphere, and I now proceed to point out the principles by which this bending is governed.

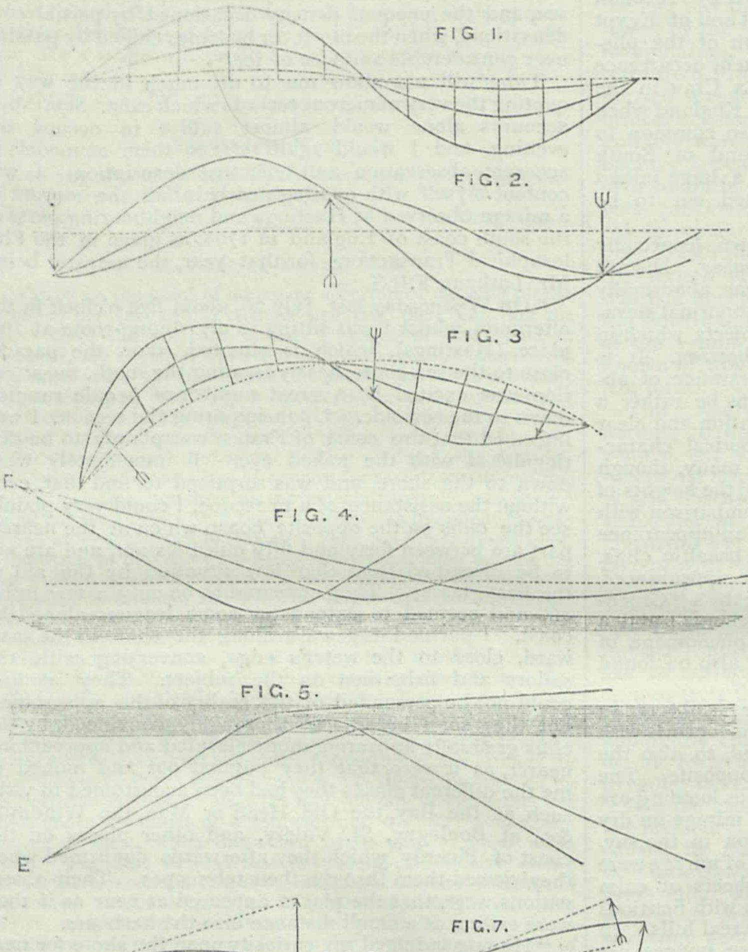


PLATE I.

My esteemed colleague, Dr. James Thomson, has greatly contributed to the clearness of our knowledge, as regards the disturbing effect of the atmosphere upon the direction of a ray of light. He has recently published an investigation,* which, to say the least, is simpler and more satisfactory than any before given, of the precise law which determines the curved path of a ray through the air.

Referring you for the details to the last chapter but one of my own recently published edition of Deschanel's "Natural Philosophy," I will merely say that when a ray is passing through a portion of air which is not equally dense all round it, it is deflected towards the side on which the density is greatest; and that the sharpness of the curvature, as measured by the change of direction for a given length of the ray, is directly proportional to the rate at which the density varies along the normal. Strictly speaking, I ought, instead of "density," to have said "absolute index of refraction, diminished by unity;" but experiment has shown that the difference between these two statements, when there is no substance in question except air and aqueous vapour, is quite insignificant.

Supposing the stratification of the air to be strictly horizontal, it follows that a ray travelling vertically will not be bent at all, since there is no variation of density in the direction of its normal; and of all rays which traverse the same point, those which are horizontal will be bent the most, because the whole change of density is normal to them, and has a direct tendency to bend them downwards. For rays which are *nearly* horizontal, the curvature will be very nearly the same; and, as it is by such rays that we see the images which constitute mirage, the maximum bending of atmospheric rays is available for the explanation of the phenomena. In the average state of the atmosphere, the curvature of rays which are horizontal, or nearly so, is about one-fifth or one-sixth of the curvature of the earth's surface; though it is to be remarked, by way of caution, that the connection between these two curvatures is merely accidental; the curvature of the earth is not the cause, nor even a partial cause, of the curvature of rays.

Other things being equal, the curvature of rays should be greater in cold than in warm air, and greater with high than with low barometer; but these are not the *principal* modifying elements. The circumstance which it is most important to know, at any time, in order to predict the degree of curvature, is the rate at which the temperature changes with the height. The average change is a fall of about $\frac{1}{300}$ of a degree Fahr. per foot of ascent. A fall of one fifty-third of a degree per foot of ascent would make the air equally dense at all heights, and would cause rays to travel in absolutely straight lines. A more rapid fall than this would render the air aloft denser than that below, and would cause rays to bend up instead of down. The existence of denser, and therefore heavier air aloft, is obviously incompatible with stability of equilibrium; but unstable equilibrium may endure for a time, even under statical conditions; and when there is a powerful cause at work, tending to raise the temperature of the lower strata, it is quite conceivable that the lower air may be heated faster than it can get away (if I may be allowed a somewhat loose expression); so that, although there is a perpetual diffusion going on, the heated air ascending, and cooler air from above taking its place, there is, nevertheless, a difference of temperature perpetually maintained, exceeding one-fiftieth of a degree per foot. The circumstances under which the Egyptian form of mirage is observed are precisely such as are fitted to produce this state of things. A fierce sun scorching the parched

* British Association Report, 1872, p. 41.

ground, while the air is excessively transparent to his rays—flatness of surface, eminently conducive to the maintenance of unstable equilibrium—and absence of wind—such are the conditions under which this form of mirage appears. On the other hand, if the decrease of temperature upwards is slower than usual, the ordinary downward bending of rays will be increased, and if any physical cause, such as warm winds commencing aloft, before they are felt at the earth's surface, produces a reversal of the ordinary distribution of temperature, so that there is an *increase* upwards, instead of a decrease, this change will favour the downward bending of rays, which will, accordingly, be exaggerated; for the lower air, being not only under greater pressure, but being also colder than the upper air, will for a double reason be denser.

Capt. Scoresby states that "the curious refractions of the atmosphere in the polar regions are most frequent on the commencement or approach of easterly winds," and he elsewhere states that easterly and southerly winds are mild.

An increase of temperature upwards, at the rate of about one-sixteenth of a degree Fahr. per foot, would make the curvature of rays equal to that of the earth, so that a ray might encircle the globe. Any increase in the downward bending of rays increases the range of vision, by enabling them to bend round the horizon, which previously limited the view. The visible effect is precisely the same as if the convexity of the surface of the earth were diminished. And not only will objects which were previously beyond the horizon be brought into view, but objects which were previously visible near the horizon will become plainer, inasmuch as the rays by which they are seen will not pass so close to the intervening surface as before, but will traverse a higher portion of the air, which is less liable to be obscured by impurities.

Having now laid down the first principles, to which all effects of atmospheric refraction must be traced, we will proceed to some more particular applications.

I have recently been considering the question—what must be the law of density (or, more strictly, of refractive index) in a horizontally stratified atmosphere, in order that images formed by mirage may be perfectly sharp? and some of the diagrams placed before you will serve to explain the results which I have obtained.

First.—Neglect the curvature of the earth, and suppose the surface of uniform index to be plane; then the law required is as follows:—There must be a plane of maximum index, at which the rate of variation of index with height must be zero; and as we ascend or descend from this plane of reference the rate of variation of index must continually increase in direct proportion to the distance. The rate must also be the same at equal distances above and below this plane of reference. The curvature of a horizontal, or nearly horizontal ray, will thus be simply proportional to distance from the plane of reference, and the bending from either side will be towards this plane. Rays may accordingly pierce this plane (which is indicated by a dotted line in Figs. 1 and 2) again and again, any number of times, and every time that they do so they will undergo a reversal of curvature. The curvature at the point of crossing will be *nil*. The curves described will be what are called "harmonic curves," or "curves of sines," such as are represented in Figs. 1 and 2; subject to the restriction that we have only to do with rays which are so nearly horizontal that the cosines of their inclinations may be treated as unity. The distance between consecutive intersections will be the same for all

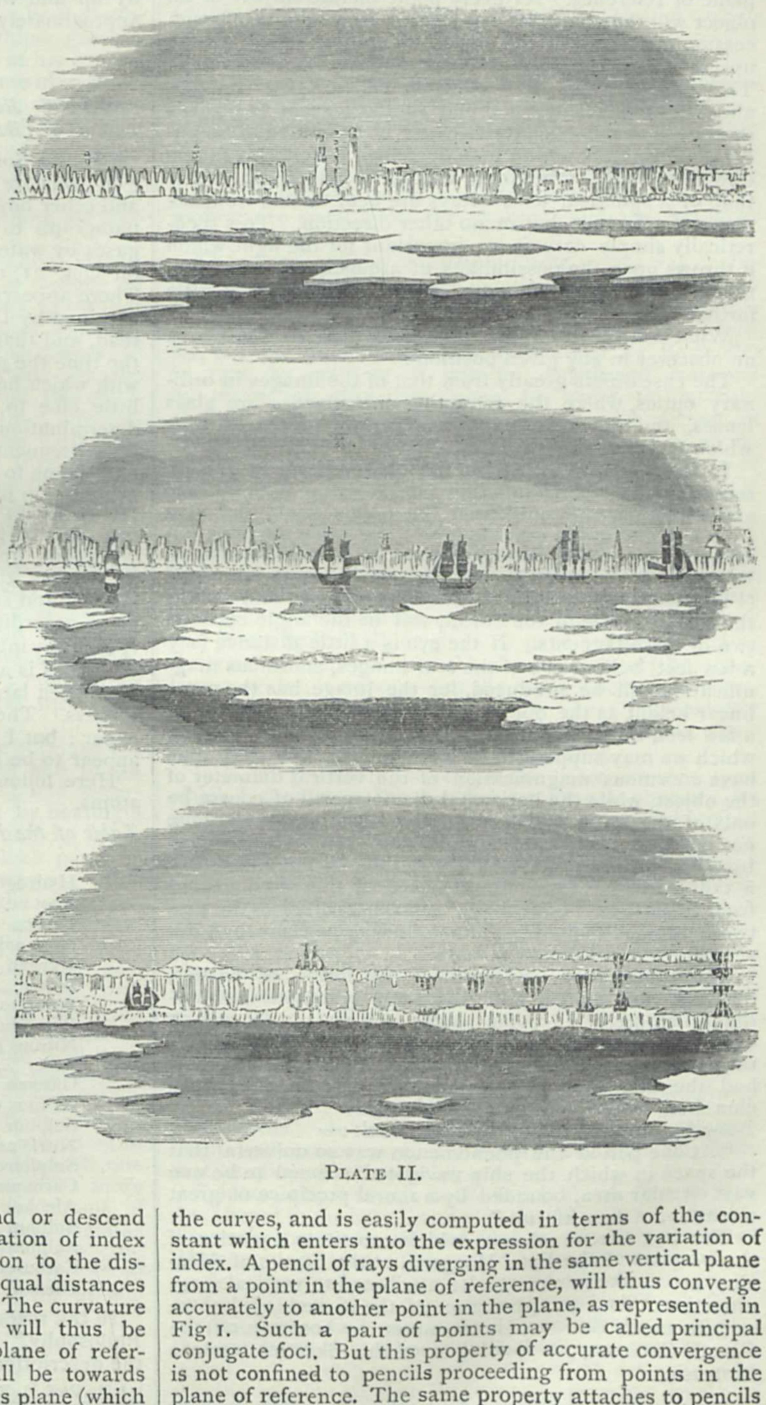


Plate II.

the curves, and is easily computed in terms of the constant which enters into the expression for the variation of index. A pencil of rays diverging in the same vertical plane from a point in the plane of reference, will thus converge accurately to another point in the plane, as represented in Fig 1. Such a pair of points may be called principal conjugate foci. But this property of accurate convergence is not confined to pencils proceeding from points in the plane of reference. The same property attaches to pencils diverging from any point whatever; the conjugate focus being always a point at the same distance on the other side of the plane of reference, and the horizontal distance between the two being the same as in the preceding case. This property is illustrated by Fig. 2.

It is obvious that the conjugate foci will occur not in pairs merely, but in sets of unlimited number; that is to say, raised proceeding originally from any one point will converge in succession to an indefinite number of other points, which will be alternately on opposide sides of the plane of reference. As every point on the surface of an object will thus have its conjugates, we shall have a succession of images of the object. The first image will be upside down, the second erect, and so on alternately. They will be what are technically called "real" images, and will be precisely equal and similar (except as regards inversion) to the object itself. It is of course to be understood that the action here described is confined to one dimension only, resembling that of a cylindrical rather than of a spherical lens. Rays are bent to and from the plane of reference, but in no other direction. This theoretically simple case is so important for the light which it throws upon the possibilities of atmospheric refraction, that we shall examine some of its consequences a little further.

What will be the appearance presented to the eye of an observer in any given position?

The case differs greatly from that of the images in ordinary optics, where the refracting instruments are glass lenses, and the eye sees the image by means of rays which travel in straight lines.

In the case now before us, the observer will in general see a virtual image, differing considerably, both in size and direction, not only from the object itself, but also from any one of the real images. The apparent direction of any point of the visible image is of course determined by drawing a tangent to the ray which enters the eye* (Figs. 6 and 7); and the visual angle, or, as we may call it, the apparent size of the object, will be the angle between two of these tangents. If the eye is a little distance (say a few feet) behind one of the real images, enormous magnification will be produced, for the image has the same linear height as the object, and is seen from a distance of a few feet, instead of from the real distance of the object, which we may suppose to be a few miles. We shall thus have enormous magnification of the vertical diameter of the object, while the horizontal diameter will of course be only of the natural size, since the rays have undergone no bending except up and down. An object whose breadth is equal to its height will thus be magnified into a tall column. Some appearances of this kind, copied from Scoresby's "Greenland," are represented in the first two figures of Plate II. The following is Scoresby's description ("Greenland," p. 96):—

"Hummocks of ice assumed the forms of castles, obelisks, and spires, and the land presented extraordinary features. In some places the distant ice was so extremely irregular, and appeared so full of pinnacles, that it resembled a forest of naked trees; in others it had the character of an extensive city crowded with churches, castles, and public edifices."

Again, on page 163 of the same work:—

"At one period the phenomenon was so universal that the space in which the ship navigated seemed to be one vast circular area, bounded by a mural precipice of great elevation, of basaltic ice."

The magnificent columns which constitute a portion of the wonders of the Fata Morgana, at the Straits of Messina, are in like manner to be attributed to vertical magnification. And an appearance of the same kind, known as "the merry dancers," is often seen by boatmen off the Giant's Causeway, in looking over the Skerries towards Portrush.

If we could have density distributed symmetrically round an axis, instead of on the two sides of a plane, we might of course have magnification without distortion. But we can scarcely conceive of any arrangement at all resembling this existing in the atmosphere.

It is further to be remarked, that the apparent distance of one of our columnar images from the observer's eye is an ambiguous quantity. If judged by left and right displacement, it is the real distance of the object. If judged by up and down displacement, it is much less, being approximately the distance of the real image.

(To be continued.)

* The letter E, in all the figures, denotes the position of the observer's eye.

*SOME REMARKS ON DALTON'S FIRST TABLE OF ATOMIC WEIGHTS**

As the Society is aware, the first table, containing the relative weights of the ultimate particles of gaseous and other bodies, was published as the eighth and last paragraph to a paper by Dalton on the absorption of gases by water and other liquids, read before this Society on Oct. 21, 1803, but not printed until the year 1805. There appears reason to believe that these numbers were obtained by Dalton after the date at which the paper was read, and that the paragraph in question was inserted at the time the paper was printed. The remarkable words with which he introduces this great principle give us but little clue to the methods which he employed for the determination of these first chemical constants, whilst in no subsequent publication, as in none of the papers which have come to light since his death, do we find any detailed explanation of how these actual numbers were arrived at. He says,† "I am nearly persuaded that the circumstance" (viz., that of the different solubilities of gases in water) "depends upon the weight and number of the ultimate particles of the several gases: those whose particles are lightest and single being less absorbable, and the others more, according as they increase in weight and complexity. An inquiry into the relative weights of the ultimate particles of bodies is a subject, as far as I know, entirely new. I have been lately prosecuting this inquiry with remarkable success. The principle cannot be entered upon in this paper; but I shall just subjoin the results, as far as they appear to be ascertained by my experiments."

Here follows the table of the relative weights of the atoms.

Table of the Relative Weights of the Ultimate Particles of Gaseous and other Bodies.

Hydrogen	1
Azot	4·2
Carbon	4·3
Ammonia	5·2
Oxygen	5·5
Water	6·5
Phosphorus	7·2
Phosphuretted hydrogen	8·2
Nitrous gas	9·3
Ether	9·6
Gaseous oxide of carbon	9·8
Nitrous oxide	13·7
Sulphur	14·4
Nitric acid	15·2
Sulphuretted hydrogen	15·4
Carbonic acid	15·3
Alcohol	15·1
Sulphurous acid	19·9
Sulphuric acid	25·4
Carburetted hydrogen from stagnant water	6·3
Olefiant gas	5·3

In the second part of his "New System of Chemical Philosophy," published in 1810, Dalton points out, under the description of each substance, the experimental evi-

* By Prof. H. E. Roscoe, F.R.S.; read before the Literary and Philosophical Society of Manchester, Nov. 17, 1874.
† Manch. Mem., vol. i., Second Series, p. 286.

dence upon which its composition is based, and explains, in some cases, how he arrived at the relative weights of the ultimate particles in question. Between the years 1805 and 1810, however, considerable changes had been made by Dalton in the numbers; the table found in the first part of the "New System" being not only much more extended, but, in many cases, the numbers differing altogether from those given in the first table published in 1805. It is therefore now, to a considerable extent, a matter of conjecture how Dalton obtained the first set of numbers; all we know is that it was mainly by the consideration of the composition of certain simple gaseous compounds of the elements that he arrived at his conclusions, and in order that we may form some idea of the data he employed, we must make use of the knowledge which chemists at that time (1803 5) possessed concerning the composition of the more simple compound gases.

As I can find no record of any explanation of these early numbers, I venture to bring the following attempt to trace their origin before the Society to whom we owe their publication.

The first point to ascertain, if possible, is how Dalton arrived at the relation between the atomic weights of hydrogen and oxygen given in the table as 1 to 5·5 (but altered to 1 to 7 in 1808). The composition of water by weight had been ascertained by the experiments of Cavendish and Lavoisier to be represented by the numbers 15 of hydrogen to 85 of oxygen, and this result was generally accepted by chemists at the time, amongst others doubtless by Dalton. Whether in those early days Dalton had actually repeated or confirmed these experiments appears improbable. At any rate, he formed the opinion that water was what he called a binary compound, *i.e.*, that it is made up of one atom of oxygen and one atom of hydrogen combined together. Hence, if he took the numbers 85 to 15 as giving the composition of water, the relation of hydrogen to oxygen would be 1 to 5·6, or nearly that which he adopted. It does not appear possible to explain why Dalton adopted 5·5 instead of 5·6 for oxygen; it may, perhaps, have been a mistake, as there are two evident mistakes in the table, viz., 13·7 for nitrous oxide instead of 13·9, and 9·3 for nitrous gas instead of 9·7.

Let us next endeavour to ascertain how he obtained the number 4·3 for carbon (altered to 5 in 1808 and to 5·4 later on). Lavoisier, in the autumn of 1783, had ascertained the composition of carbonic acid gas by heating a given weight of carbon with oxide of lead, and he came to the conclusion that this gas contained 28 parts by weight of carbon to 72 parts by weight of oxygen. Now Dalton not only was acquainted with the properties and composition of carbonic acid, but he was aware that Cruikshank had shown in 1800 that the only other known compound of carbon and oxygen, carbonic oxide gas, yields its own bulk of carbonic acid when mixed with oxygen and burnt; and also that Desormes* analysed both these gases, finding carbonic oxide to contain 44 of carbon to 56 of oxygen, whilst carbonic acid contained to 44 of carbon 112 of oxygen, being just double of that in the carbonic oxide. Dalton adds: "This most striking circumstance seems to have wholly escaped their notice." Hence Dalton assumed that one atom of carbon is united in the case of carbonic oxide with one atom of oxygen, whilst carbonic acid possessed the more complicated composition and contains two atoms of oxygen to one of carbon. Now, if carbonic acid contains carbon and oxygen in the proportion of 28 to 72, carbonic oxide must contain half as much oxygen, viz., 28 of carbon to 36 of oxygen; and assuming that the atomic weight of oxygen is 5·5, that of carbon must be

$$\frac{28 \times 5 \cdot 5}{36} = 4 \cdot 3.$$

Having thus arrived at the number 4·3 as the first atomic weight of carbon, it is easy to see why Dalton gave 6·3 as the atomic weight of carburetted hydrogen from stagnant water, and 5·3 as that of olefiant gas. The one represents one atom of carbon to two of hydrogen, the other one of carbon to one of hydrogen; or, olefiant gas contains to equal quantities of carbon only half as much hydrogen as marsh gas. This conclusion doubtless expressed the results of Dalton's own experiments upon these two gases, which were made, as we know from himself, in the summer of the year 1804. He proved that neither of these gases contained anything besides carbon and hydrogen, and ascertained, by exploding with oxygen in a Volta's eudiometer, that if we reckon the carbon in each the same, then carburetted hydrogen contains exactly twice as much hydrogen as olefiant gas does, and that "just half of the oxygen expended on its combustion was applied to the hydrogen, and the other half to the charcoal. This leading fact afforded a clue to its constitution." Whereas, in the case of olefiant gas, two parts of oxygen are spent upon the charcoal, and one part upon the hydrogen.

The atomic weight of nitrogen (azote = 4·2) was doubtless obtained from the consideration of the composition of ammonia, whose atomic weight is given in the table at 5·2. Ammonia was discovered in 1774 by Priestley, but the composition was ascertained by Berthollet in 1775 by splitting it into its constituent elements by means of electricity, when he came to the conclusion that it contained 0·193 parts by weight of hydrogen to 0 807 parts by weight of nitrogen. Dalton assumed that this substance is a compound of one atom of hydrogen with one of nitrogen, and hence he obtained for the atomic weight of azote $\frac{807+1}{193} = 4 \cdot 2$; and $4 \cdot 2 + 1 = 5 \cdot 2$ as the atomic weight of ammonia. It is also probable that Dalton made use of the composition of the oxides of nitrogen for the purpose of obtaining the atomic weight of nitrogen. If we take the numbers obtained partly by Davy and partly by himself, as given on page 318 of the "New System," as representing the composition of the three lowest oxides, it appears that the mean value for nitrogen is 4·3 when oxygen is taken as 5·5. In all probability the number in this table (4·2) was obtained from an experiment of Dalton's made at an earlier date.

It is not possible to ascertain the exact grounds upon which Dalton gave the number 7·2 for phosphorus; its juxtaposition, however, in the table, to phosphuretted hydrogen, shows that it was probably an analysis or a density determination of this gas which led him to the atomic weight 7·2, under the supposition that this gas (like ammonia) consisted of one atom of each of its components. In the second table, published in 1808, Dalton gives the number 9 as that of the relative weight of the phosphorus atom, and we are able to trace the origin of this latter number, although that of 7·2 is lost to us. On p. 460, Part II. of his "New System," Dalton states that he found 100 cubic inches of phosphuretted hydrogen to weigh 26 grains, the same bulk of hydrogen weighing 2·5 grains. Hence $\frac{26 - 2 \cdot 5}{2 \cdot 5} = 9$ gives the atomic weight of phosphorus. It was probably by similar reasoning from a still more inaccurate experiment than this one, that he obtained the number 7·2.

Sulphur, which stands in the first table of 1803 at 14·4, was altered in the list published in the "New System" to 13. These numbers were derived from a consideration (1) of the composition of sulphuretted hydrogen, which he regarded as a compound of one atom of sulphur with one of hydrogen, and (2) of that of sulphurous acid, which he supposed to contain one atom of sulphur to two of oxygen. Dalton knew that the first of these compounds contained its own volume of hydrogen, and he determined its specific gravity, so that by deducting from the weight of one volume of the gas that of one volume of hydrogen, he

* Ann. der Chemie, tome 39, p. 38.

would obtain the weight of the atom of sulphur compared to hydrogen as the unit. The specific gravity he obtained was about 1·23—corresponding nearly, he says (p. 451) to Thénard's number, 1·23. Hence (as he believed air to be twelve times as heavy as hydrogen) he would obtain the atomic weight of sulphur as (12 × 1·23) − 1 = 13·76, which number, standing half way between 14·4 as given in the first table, and 13 as given in the second, points out the origin of the first relative weight of the ultimate particle of sulphur. So from sulphurous acid he would obtain a similar number, taking the specific gravity as obtained by him (Part ii., 389) to be 2·3, and remembering that this gas contains its own bulk of oxygen (p. 391), he obtained (2·3 − 1·12) × 12 = 14·16 for the atomic weight of sulphur. As, however, we do not possess the exact numbers of his specific gravity determinations, and as we do not exactly know what number he took at the time as representing the relation between the densities of air and hydrogen (in 1803 he says that the relation of 1 : 0·077 is not correct, and that $\frac{1}{20}$ is nearer the truth), it is impossible to obtain the exact numbers for sulphur as given in the first table.

In reviewing the experimental basis upon which Dalton founded his conclusions, we cannot but be struck with the clearness of perception of truth which enabled him to argue correctly from inexact experiments. In the notable case, indeed, in which Dalton announces the first instance of combination in multiple proportion (Manch. Mem. vol. i., series ii., p. 250), the whole conclusion is based upon an erroneous experimental basis. If we repeat the experiment as described by Dalton, we do not obtain the results he arrived at. Oxygen cannot as a fact be made to combine with nitric oxide in the proportions of one to two by merely varying the shape of the containing vessel; although by other means we can now effect these two acts of combination. We see, therefore, that Dalton's conclusions were correct, although in this case it appears to have been a mere chance that his experimental results rendered such a conclusion possible.

INTERNATIONAL METRIC COMMISSION AT PARIS

THE Permanent Committee of the International Metric Commission, elected from among the members at their general meeting at Paris, in 1872, has just concluded a series of meetings, the first of which was held on October 6. The Committee were directed to meet at least once a year, in order, amongst other things, to examine the progress of the work of the French Section, to whom the construction of the new standards was entrusted, with a view to the concurrence of the Committee as the executive organ of the Commission.

At their recent meetings, the Committee fully considered and discussed a detailed report of the proceedings of the French Section since the melting of the great ingot of platinum-iridium on May 13 last, from which all the new International Metric Standards are to be made (an account of which was given in NATURE, vol. x. p 130); and, generally speaking, the Committee expressed their unanimous concurrence and satisfaction at the mode in which the French Section have hitherto executed the duties entrusted to them by the Commission, and they also gave their decisions on certain points submitted to them for the guidance of the French Section in their future operations.

The first operation to which the great ingot of 250 kilogrammes of platinum-iridium was submitted, when in its rough state, and cleansed from all extraneous matter, was to have all the inequalities on its surface, that had been in contact with the lime of the calcined furnace, removed with a cold chisel. The ingot with its surface thus smoothed was found to weigh 236·330 kilogrammes. In this state it was exhibited to the Académie des Sciences at their *séance* of July 2, 1874. A portion of this large homogeneous mass of metal, when analysed by M. Henri Saint-Claire Deville, showed the proportion of iridium to be 10·29 per cent.

The ingot was next forged by M. M. Farcot under a steam hammer weighing 5,000 kilogrammes, until by successive hammerings and annealings, in a single day, it was brought to the form of a bar five centimetres square in section. By similar operators this bar, divided into convenient lengths, was afterwards further reduced to eight bars 2·5 centimetres square in section, and of a total length of 16·405 metres.

A remarkable phenomenon was observed by M. Tresca during the forging of these bars, and was communicated by him to the Académie des Sciences at their *séance* of July 9. At the moment when the hammer struck the bar, lines of light were seen to pass downwards from the edges of the hammer, and to cross each in the form of an X on each of the side surfaces of the bar. These lines continued afterwards distinctly visible in a certain light, appearing like slightly burnished marks.

The next operation was to prepare the bars for drawing into the X form, by cutting longitudinal grooves along the middle of each of the four sides of the bars by means of a planing machine. A further object of cutting these grooves was to ascertain if there were any flaws on the surface of the metal so exposed, as it was found absolutely necessary to remove any such flaws, else they would remain as blemishes on the surfaces of the bars when drawn.

The eight bars were next submitted by M. Gueldry, at the Audincourt foundry, to successive operations of drawing out and annealing, until they were accurately reduced to the X form of the Tresca section, when each was extended to a length sufficient to make three or four metre bars. The first of the grooved bars was passed through the dies no less than 220 times, and was as often subjected to annealing. It was afterwards ascertained that the rigidity of the drawn bars was but little affected by the process of annealing, their co-efficients of elasticity being found as follows:—

Before annealing	21·2085
After annealing	21·0073

Their co-efficient of expansion was also found to be but very slightly changed, and in the opposite direction, viz.—

	Co-efficient of expansion for 1° C. at mean t. 40° C.	Variation for mean t. 1° C.
Before annealing	0·00000880,2	0,84
After annealing	881,9	0,86

When divided into finished bars of the X section, 1·02 m. in length, each bar is made perfectly straight by special arrangements contrived for this purpose. Four straight edges of steel are made exactly to fit into the grooves of the X bar, and to form, when so fitted, a rectangular bar two centimetres square in section. This squared bar is then enclosed between the plane surfaces of four solid rectangular iron bars; and all being tightly compressed with iron clamps in the form of hollow squares and with iron wedges, the whole is heated in a furnace till red hot, when the clamps are further tightened and the mass of metal is left to cool. By this operation, each of the X metre bars is made perfectly straight. Up to the present time bars of the X section have been made sufficient for more than thirty metres.

The polishing of the surface of the X bars next follows. This is effected by the use of polishing powder and powdered charcoal. Particular attention is given to the polishing and subsequent burnishing of that portion of the surface of the metal on which the defining lines are to be cut. Several experiments which have been made tend to show that the best surface for cutting the lines will be obtained by the final operation of slightly impressing a stamp of highly polished steel, of the dimensions of 3 mm. by 2 mm. By this means an identical surface for receiving the defining lines may be given to every one of the new metres.

The apparatus for cutting the lines is connected with the new longitudinal comparing apparatus, carrying a microscope with its micrometer. The microscope is 0·8 m. in length and magnifies more than 200 times; and the whole apparatus is placed in the cold chamber, which has been constructed at the Conservatoire des Arts et Métiers, and can be maintained constant at the normal temperature of 0° C. The polishing of the bars, as well as the cutting of the defining lines, the position of which must necessarily be the result of the most precise comparisons with the primary standard metre, are both entrusted to M. Tresca and his son, M. Gustave Tresca.

The lines are to be cut with a diamond point. Each transverse defining line will be crossed at right angles by two longitudinal lines 0·1 mm. apart, and the portion of the transverse line so intercepted between the two lines will define the length of the metre. The width of these lines will probably be about 0·002, or at most 0·003 mm., or 3 microns (μ). This will be about one-fourth of the thickness of the defining lines of our standard yards, which are cut with a steel knife upon the polished surface of a gold stud, and are viewed through microscopes magnifying about sixty times.

Great progress has been made in the construction of the series of new thermometers, two of which are to accompany each international standard metre. These thermometers are being constructed by M. Baudin. Their length is 0·45 m., and their external diameter 5 mm. The bulbs have the same external diameter, and the two thermometers can thus be placed in the groove of the X metre bar for determining the temperature of its measuring axis during comparisons under the microscopes. The scale of the thermometers ranges from -5 to $+50°$ C., and each degree is subdivided into tenths. Every 1° corresponds with a length of about 7 mm. Four standard thermometers have been constructed for the purpose of verifying the new metre thermometers. They have an arbitrary scale from 0° to 100° C., graduated in half-millimetres by hydrofluoric acid on the glass tubes, and the value of the several graduations has been accurately determined by calibration. The length of these standard thermometers somewhat exceeds 0·50 m.

The construction of the new international kilogrammes and of the standard *mètres-à-bouts* will be deferred until the completion of the number of *mètres-à-traits* required. Meanwhile, several balances of the greatest precision have been obtained for the weighings, some of which are fitted with mirrors for observing the extent of the oscillations through a telescope by means of a vertical graduated scale fixed to the telescope and reflected in the mirror, according to the principle adopted by Gauss for observing variations of the magnetic needle.

For ascertaining the atmospheric pressure during the weighings, the standard barometer of the Conservatoire des Arts et Métiers, constructed by Fastre, is proposed to be used, by which the height of the mercury can be read to 0·01 mm. An ingenious apparatus has been constructed by M. Mendeleef, which shows the slightest variation of pressure during the process of weighing, by means of a small U-tube containing oil of petroleum. One end of this tube is closed and contains a certain volume of dry air maintained at a constant temperature, whilst the other end is open to the air. The instrument being accurately adjusted by means of a mercurial plunger connected with the bottom of the U-tube, so that the petroleum is exactly on a level on the two branches of the tube, it is found to be so extremely sensible that the slightest variation of atmospheric pressure is shown by an alteration of the level, and the amount of this alteration can be measured with the greatest precision.

It is expected that the whole series of new *mètres-à-traits* will be completed by the French Section and ready to be handed over to the Comité Permanent by October 1875, and that the construction of the new kilogrammes and *mètres-à-bouts* will also be far advanced by that date.

During their late meeting, the question of the convocation of a Diplomatic Conference at Paris with the view of providing the requisite means for enabling the committee to execute all the definite comparisons of the new metric standards, and for securing the due preservation of the new international metric prototypes and regulating their use for future comparisons, was further considered by the Committee. In pursuance of their resolution of last year upon this subject, the requisite communications were made by the French Government to the Governments of the several countries interested, and the Committee have now passed a resolution that considering the numbers of Governments who have agreed to take part in such conference, the French Government be requested to convoke it with as little delay as possible. Information has been received of the willingness of the French Government to accede to the request, and the Conference will probably be held in the spring of next year. H. W. CHISHOLM

NOTES

It is with the greatest pleasure and with something like a sense of relief that we are able at last to announce definitely that at a Cabinet Council held last Saturday it was decided that there should be an Arctic Expedition, at the expense of Government, to sail next spring. The welcome intelligence was thus announced by Mr. Disraeli to Sir Henry Rawlinson:—"Her Majesty's Government have had under consideration the representations made by you on behalf of the Council of the Royal Geographical Society, the Council of the Royal Society, the British Association, and other eminent scientific bodies, in favour of a renewed expedition, under conduct of Government, to explore the region of the North Pole, and I have the honour to inform you that, having carefully weighed the reasons set forth in support of such an expedition, the scientific advantages to be derived from it, its chances of success, as well as the importance of encouraging that spirit of maritime enterprise which has ever distinguished the English people, her Majesty's Government have determined to lose no time in organising a suitable expedition for the purposes in view." Steps have, we believe, been already taken to carry into effect this resolution, which reflects so much credit on her Majesty's Government. Admiral M'Clintock left for Dundee on Tuesday with an engineer and shipwright, to buy two steam whalers, which will be fitted out under the tried explorer's superintendence at Portsmouth. Capt. A. H. Markham, who went to Baffin's Bay last year, will probably occupy an important post in the expedition, the route of which will, of course, be Smith's Sound. Now that the thing has been decided on, there is no doubt that it will be thoroughly well done; and now that Englishmen have once more got the chance, we may expect something like real work, if, indeed, they do not take the last step in the solution of the Arctic mystery.

We take the following from the *Times*:—The medals in the gift of the Royal Society for the present year have been awarded by the Council as follows, and will be presented at the anniversary meeting on the 30th inst.:—The Copley Medal to Prof. Louis Pasteur, of the Academy of Science, Paris, For. Mem. R. S., for his researches on Fermentation and on Pebrine. The Rumford Medal to Mr. J. Norman Lockyer, F.R.S., for his spectroscopic researches on the sun and on the chemical elements. A Royal Medal to Prof. William Crawford Williamson, F.R.S., of Owens College, Manchester, for his contributions to zoology and palæontology, and especially for his investigations into the structure of the fossil plants of the coal-measures; and a Royal Medal to Mr. Henry Clifton Sorby, F.R.S., for his researches on slaty cleavage and on the minute structure of minerals and rocks, for the construction of the micro-spectroscope, and for his researches on colouring matters.

WE are very glad to be able to announce that Prof. Maskelyne's lectures on Crystallography to the Chemical Society are likely to be well attended. The first lecture will be given on Monday evening next, at 8.30, at Burlington House.

LAST week some engineers visited the National Library, Paris, on behalf of the Japanese Government, to take measurements for the purpose of building a large public library in Japan on the same plan. The magazine and reading-rooms of Paris have, with some improvements, been built on the system of the British Museum.

THE report of the Potato Disease Committee of the Royal Agricultural Society has been recently published. It will be recollected that three years ago Earl Cathcart offered a prize of 100*l*. for essays on the prevention of the disease. Although no fresh practical information was elicited, and it may perhaps be said no direct good came from this well-meant offer, the Society took the subject up and offered prizes for potatoes reputed to be proof against disease. Two prizes were offered for the commencement of this year, for potatoes of varieties already known, and two are to be awarded five years hence for varieties that may be produced by cultivation before that period. Six different varieties were sent in, 1 ton (twenty bags of 1 cwt.) of each. The Society arranged to have these practically tested. Twelve stations in England, four in Scotland, and four in Ireland were selected, and 1 cwt. of each variety sent for planting, of these so-called disease-proof potatoes. During the summer the botanic referee of the Society visited all the localities, and in all cases disease was found. Much valuable information is likely to arise from the statistics that have been collected, for although it seems that no indication is given of how the disease can be prevented, yet under certain conditions, principally influenced by moisture, its effect is but small. Prof. de Bary has worked out the scientific questions that occur as to the origin of the disease. It is owing to a fungus (*Peronospora infestans*), which attacks the leaves first, and after absorbing the nutriment of them, utilises the petiole, and thus reaches the tubes. A further report of the Committee, based on the statistics sent in, is shortly to be expected.

WE greatly regret to announce the death of Mrs. Hooker, the wife of the Director of the Royal Gardens, Kew, and President of the Royal Society, which took place on Friday, Nov. 13, very suddenly. She was the translator of Le Maout and Decaisne's "Traité général de Botanique. She will be missed by a large circle of scientific friends.

THE death of Dr. Archibald Campbell will be regarded as a severe loss by his colleagues in scientific societies and by many of the Indian public. He was sixty-nine years of age, and till lately appeared hale and hearty. As Superintendent of Darjeeling, he became a leading authority of reference on the natural history, geography, and ethnography of Thibet, Nepaul, Sikkim, and Bhootan. He was distinguished as an administrator, and under his government and auspices Darjeeling has risen from an obscure sanitarium for invalid soldiers to be a settlement of some consideration. He was the author of several memoirs and notes.

WE have to record the death, on Monday last, in his fifty-sixth year, of Dr. Edward Smith, F.R.S., Assistant Medical Officer, for Poor-law purposes, to the Local Government Board. Dr. Smith's excellent observations on quantitative physiological cyclical phenomena, many of which were conducted on himself, are too well known to require special mention; they indicate an amount of energy and willingness to experience personal inconvenience for the sake of his favourite subject which is very rarely to be met with. His observations on dietaries, especially with regard to the Manchester cotton famine, are also of considerable importance.

WE hear that a new method has been proposed for crossing the Channel; this is to construct an artificial isthmus between the French and English sides, leaving a very small space in the centre for the passage of ships. The expense would not be much larger than that of boring a tunnel, and the advantages would in some respects be greater.

THE International Congress of Orientalists has been the means of originating in Paris a new society under the title of Société d'Études, Japonaises, Chinoises, Tartares, and Indo-chinoises. The number of members already amounts to sixty. At a recent meeting of the Society, M. Bourset exhibited a game for teaching children in a few hours the elements of which Chinese letters are made—*omne tulit punctum qui miscuit utile dulci*. M. Bourset has also shown another invention for diminishing the number of letters which must be cut, and therefore of diminishing the cost of printing Chinese works.

M. LEVERRIER is constructing, in the recently annexed garden of his observatory, a basis for comparing accurately, by superposition, standard measures of length with the metre. The first comparison will be made between the Archives metre and the celebrated Boscowitz rule, which was used more than a century ago for determining the length of two degrees in the Papal States.

IN a paper read before the Paris Société d'Acclimatation, Dr. Turrel suggests that the rapid spread of the *Phylloxera vastatrix* in France may be due to the scarcity of small birds in that country. Forty years ago, he says, linnets, tits, &c., were numerous in Provence, and in the autumn they could be seen posted on the vine branches, carrying on a vigorous search after the insects, and larvæ and eggs of insects, concealed in the cracks of the stem and leaves of the plant. Since the commencement of the present century, however, it is easy to perceive that the destruction of small birds has been carried on more and more generally; and that, concurrently with this war of extermination against the feathered tribes, the numbers of destructive insects have increased at an alarming rate. Dr. Turrel thinks that, though it cannot be absolutely maintained that the oidium and the Phylloxera, the two latest forms of vine disease (the one a vegetable, the other an insect parasite), owe their frightful extension to the scarcity of small birds, yet it is unquestionable that a plant like the vine, weakened by the attacks of insects, is less in a condition to withstand the ravages of parasites; and that, deprived of its feathered protectors and left to the successive and unchecked onslaught of the vine grub and other normal enemies, it has been predisposed to succumb before the ravages of its new enemies. The obvious moral is that the French are themselves partly to blame for their indiscretion in killing the useful small birds.

THE commotion created in the Paris School of Medicine by the false rumour spread by the *Figaro* has been beyond bounds; not only was M. Wurtz, the Dean, cheered, but M. Chauffard, one of the professors belonging to the clerical party, was hooted, and unable to deliver his lecture. The disorder having been renewed in spite of all precautions taken by M. Wurtz, the School of Medicine has been closed for a month. If students again exhibit a riotous spirit, the ringleaders will be prosecuted before a Council of War; which is a lawful proceeding, Paris being placed under a state of siege.

STROMBOLI is reported to have recently shown symptoms of revived action.

THE next Triennial Prize of 300*l.*, under the will of the late Sir Astley P. Cooper, Bart., will be awarded to the author of the best essay or treatise on "The Anatomy, Physiology, and Pathology of the Sympathetic Nervous System."

WE learn from *Hansa* of the 15th inst. that the following amounts have been included in the estimates for 1875, presented to the Imperial German Parliament for the service of the "Deutsche Seewarte":—

A.—Salaries and Remunerations.		
1. Central Station	... ...	39,000 marks
2. Branch Stations	... ...	11,000 ,,
B.—Contingent Expenses.		
1. Central Station	... ...	20,000 ,,
2. Branch Stations	... ...	4,800 ,,
Total	... ...	74,800 marks

which, at the rate of twenty marks to the sovereign, amounts to 3,700*l.* Two new departments are to be added to that established at Hamburg for Marine Meteorology, viz., for Storm-warnings and Magnetism.

A HONG KONG telegram of the 16th inst. states that the *Challenger* had arrived there from Australia.

WE hear that a Horticultural Club is about to be formed in London, and the preliminary steps that have been taken promise well.

THE last number of the *Gardener's Chronicle* states that a specimen of *Aralia sieboldi* at Kew is now in bloom, and that a new garden plant, *Raphidophora lancifolia*, is now in cultivation in this country.

A SLIGHT shock of earthquake was felt in Carnarvonshire and Anglesea on Sunday morning.

FROM a private letter dated Mauritius, Oct. 15, we learn that Lord Lindsay had not yet arrived at that island, that the Germans were expected on the 25th, that the Dutch were at their post at Bourbon, and the English the same at Rodriguez.

THE Earl of Derby has been elected by the Edinburgh students as their Lord Rector, and Mr. Disraeli has been re-elected by the *ingenui adolescentes* of Glasgow University.

EVERY term at Dulwich College a course of scientific evening lectures is given, open to the students and their friends. This term, for the first time, the applications for tickets have exceeded the accommodation of the lecture theatre. The present course is on Geology, by Prof. Harry G. Seeley, the titles of the lectures being, "The Origin and Internal Structure of the Earth," "The Origin and Succession of the Strata," "The Succession of Life on the Earth," and "The Influence of Geological Phenomena on Men and Animals."

THE Committee of Directors of the Crystal Palace Company's School of Art, Science, and Literature have made arrangements for the delivery of successive short series of lectures on special subjects by gentlemen of eminence in art, science, and literature. These lectures will be purely educational in character, and, as far as possible, complete in themselves, but will not in any way supplant the permanent private classes, to which they are designed to be accessory. They are intended to stimulate independent thought, and to lead the student to a conception of some of the ulterior aims of the studies she pursues. They will be delivered in the largest class-room of the school, generally on Fridays, in the afternoon; and the most moderate fee that is possible in each case will be fixed. Ladies only will be admitted. The first course will be of six lectures on "The Interpretation of Nature as it relates to Man and his Education," by the Rev. Chas. Pritchard, M.A., F.R.S., Savilian Professor of Astronomy in the University of Oxford. Fridays—November 13, 20, 27; December 4, 11, 18; to commence each day at half-past three.

AT Emmanuel College, Cambridge, there will be an examination for open scholarships in natural science, commencing the 6th of April, 1875. There is no limit as to age, but all candidates will have to satisfy the examiners that they possess such a knowledge of mathematics and classics as will enable them to pass the Previous Examination. The subjects of examination are botany, chemistry, chemical physics, geology and mineralogy, zoology, comparative anatomy, and physiology. Candidates must send their names, with copy of register of birth and a certificate of good conduct from some M.A. of the University, to the tutor of Emmanuel, on or before March 31. A candidate for a scholarship may also be eligible without further examination for a scholarship at Christ's or Sidney Colleges, in default of properly qualified candidates at those colleges.

A JOINT examination will be held at Clare College and Gonville and Caius College, Cambridge, on Tuesday, March 16, 1875, and three following days, when two scholarships for natural sciences will be offered for competition to students intending to commence residence in October 1875, each of the value of 60*l.* per annum, tenable for two years, but subject to extension or exchange for scholarships of longer tenure. Candidates are required to send their names, with certificates of age and testimonials of good conduct, to one or other of the respective tutors, the Rev. N. M. Ferrers, tutor of Caius, or the Rev. W. Raynes, tutor of Clare, stating at which college they prefer to be elected; but if not elected at such college it will be understood that they are candidates also at the other college. Further particulars may be obtained on application to the tutor of Clare or the tutor of Caius.

THERE was a meeting of the members of the Cambridge University Senate on the 12th inst., to discuss the report issued last June of the Board of Natural Science Studies, recommending alterations in the examination for the Natural Science Tripos. Its main recommendations consist of a division of the Tripos. The recommendations met with the unanimous approval of the Senate.

THE following appears in the *Times*:—Where the excavations for laying the water-pipes are being made near Rideau Hall, on the grounds of the Governor-General of Canada, the workmen have made a strange geological discovery. It is a stratum of fossil rock several feet thick, containing the most accurate and beautiful petrified winged insects. There are some like butterflies, with the delicate fibre of the wings in a most perfect state of preservation. Several persons in New Edinburgh have secured excellent specimens.

ON Thursday, Nov. 5, the members of the Geological Society Club dined together at the Pall Mall Restaurant, to celebrate the fiftieth year of the meetings of the Club. There was a good gathering of the members, and among them were the Earl of Enniskillen, Sir Charles Lyell, Profs. Huxley and Ramsay, Mr. Godwin Austen, Mr. Prestwich, Capt. Galton, &c.; some of the past retired members were also present. Letters apologising for absence were read from Mr. Jesse Watts Russell, an original member, the Duke of Devonshire, Earl of Selkirk, Lord Overstone, Mr. Darwin, Sir C. Fox Bunbury, and others. The president of the Geological Society, Mr. J. Evans, took the chair, and the vice-chair was occupied by Mr. Mylne, the treasurer of the Club; some toasts were given, and Sir Charles Lyell, one of the only two original members now living, responding in the name of the Club, took occasion to remark that great as had often been the differences of opinion in the Geological Society from the time of Buckland, Conybeare, De la

Beche, Fitton, Sedgwick, and Murchison, down to the present day, there had always been perfect harmony in the Club. He further congratulated the younger men not only on the zeal and talent displayed among them, but on the progress of opinion and freedom of expression gained by scientific thought in the course of half a century.

ICEBERGS are reported to have been met with in the Bay of Biscay during very rough weather, by the *Mongolia*, which arrived at Southampton on Monday last. Icebergs have been met with as far south, but generally well out in the Atlantic Ocean.

WE invite the attention of all interested in technical education to the very excellent examination scheme of the Society of Arts, intended to promote such education among the working men of the country. No doubt a prospectus of the scheme will be forwarded to anyone writing for it to the Society's offices in London.

IN one of its last sittings the Municipal Council of Paris will have to vote on a proposition, supported by forty of its members, asking the National Assembly to establish a system of public instruction, gratuitous, obligatory, and secular. The motion will probably be agreed to by the Municipal Council, but rejected altogether by the National Assembly.

THE additions to the Zoological Society's Gardens during the past week include eighteen Lancelets (*Amphioxus lanceolatus*) from the Mediterranean Sea, presented by the Director of the oological Station at Naples; a Pine Marten (*Martes abietum*), British, presented by Mr. J. Francis; a Red-shouldered Starling (*Agelacus phoeniceus*) from N. America, presented by Mrs. Boxwell); two Aztec Conures (*Conurus aztec*) from S. America, purchased.

SOCIETIES AND ACADEMIES

LONDON

Linnean Society, Nov. 5.—G. J. Allman, M.D., president, in the chair.—W. H. Archer, R. A. Pryor, and W. W. Wilson were elected Fellows. Mr. J. E. Howard read a paper on the appearances of *Lobelia dortmanna* on the floating island in Derwentwater.—Mr. J. A. Jackson exhibited leaves of Liquidambar and Perottia, exhibiting remarkably beautiful autumn tints.—Mr. J. G. Baker read a paper on Asparageæ, a section of Liliaceæ. The author commenced by discussing the limits of the natural order Liliaceæ. He proposed to regard it as consisting of three great series, and in addition several abnormal tribes, all of which have some claim to be regarded as distinct orders. The three series are:—Liliaceæ proper, characterised by capsular fruit with loculicidal dehiscence, united styles, and introrse anthers (1200 to 1300 species); Colchicaceæ, marked by capsular fruit with septicidal dehiscence, free styles, and extrorse anthers (130 species); and Asparagaceæ, marked by baccate fruit (260 species). The aberrant tribes are Liriopeæ (Ophiopogoneæ), Gillesieæ, Conantheræ, Stemoneæ (Roxburghiaceæ, Lindley), and Scoliopeæ. All these have anatropous ovules: and he advocated the separating of Smilax from Asparageæ, with which it has been commonly joined by recent writers, and the retention of it as the type of a separate order marked by orthotropous ovules, and by its habit of growth, woody often prickly stems, minute polygamous umbellate flowers, stipular tendrils, and decidedly stalked exogen-like leaves with venules reticulated between the palmate main nerves. The tribes and genera of Asparageæ, which are as follows, to a considerable extent represent the non-bulbous tribes of the two capsular series:—(1) *Dracæneæ*: Shrubs with proper leaves, hermaphrodite flowers, and introrse anthers; genera, Dracæna, Toetsea (= Cordyline, but used on ground of priority), and Colmia; represents Yuccoideæ in Euliliaceæ. (2) *Sanseviereæ*: Undershrubs with coriaceo-carnose leaves, hermaphrodite flowers, and extrorse anthers; genera, Sanseviera, Lomatophyllum; represents closely Aloineæ in Euliliaceæ. (3) *Convallarieæ*: Herbs with proper leaves, gamophyllous hermaphrodite flowers, and introrse anthers; genera, Reineckia, Convallaria, Polygonatum, Hylonome; represents Hemerocallideæ in Euliliaceæ. (4) *Tovarieæ*: Herbs with proper leaves, polyphyllous hermaphrodite flowers, and introrse anthers, dehiscing longitudinally; genera, Theropogon, Speirantha (new genus founded on *Albrica gardeni*, Hook.), Maianthemum, Tovaria (an earlier name for Smilacina), Drymophila, Geitonoplesium, and Eustrephus. (5) *Dianelleæ*: Herbs with proper leaves, hermaphrodite flowers, and anthers dehiscing by terminal pores; genera, Dianella, Luzuriaga. (6) *Aspidistreæ*: Acaulescent herbs, with fleshy, often eight-lobed perianths, hermaphrodite flowers, introrse anthers with longitudinal dehiscence, and large peltate complicated stigmas; genera, Aspidistra, Plectogyne, Tupistra, Campylandra (new genus from East Himalyas), Gonioscypha (new genus from Bhotan), Rohdea. (7) *Streptopeæ*: Herbs with proper leaves, hermaphrodite flowers, and extrorse anthers, with longitudinal dehiscence; genera, Medeola, Clintonea, Prosartes, Streptopus, Callixene, Kruhsea; represents Colchicaceæ in the capsular series. (8) *Asparageæ*: Herbs or shrubs with leaves degraded down into spurred bract-like membranes, and their place filled by an abundant development of branches in their axils; flowers often polygamous, with introrse anthers dehiscing longitudinally; genera, Asparagus (including Asparagopsis and Myrsiphyllum), Ruscus, Semele, and Danae; the most specialised type of the baccate series, not represented by any tribe in the two capsular sets. The most noticeable points of structure in the series are that, in the first place, such a thing as a bulbous rootstock or a narrow fleshy lorate leaf of the hyacinth type does not occur in Asparageæ at all. As regards distribution, it is noticeable that whilst the bulbous tribes of Liliaceæ possess a distinctly-marked geographical individuality, this does not hold good of the non-bulbous half of the natural order; and that the 260 species are scattered all over the world, and not concentrated in any particular geographical area. The most curious structural peculiarity in the group is the degradation of the leaf-organ which marks the tribe Asparageæ. The leaves have an alternate arrangement, and are invariably developed in the form of a minute membranous scale. This has a spur at the base, which in many of the shrubby species of Asparagus is developed out into a woody spine, as firm in texture as the indurated branchlet of the sloe or hawthorn. The function of the leaf is fulfilled by branches, which are developed singly or in fascicles in the axils of these bract-like proper leaves. Sometimes these branches are needle-like (cladodia), without any flattening, as in the common garden asparagus; and sometimes, as in Myrsiphyllum and Ruscus, they assume all the appearance of proper leaves (phyllocladia). The flowers in the 100 species of the genus Asparagus are remarkably uniform, and it is principally upon characters furnished by the shape and arrangement of these barren branches that the species are marked. The stigma of the Aspidistreæ is a very curious and complicated organ. It is a plate with eight troughs radiating from a raised central umbilicus, separated from one another by raised walls, and it closes in the tube of the perianth, in which the anthers are placed so thoroughly that it is difficult to tell how fertilisation is effected; but upon turning it upside down four minute holes may be seen, through which it would be possible for a very small insect to creep. The paper was illustrated by plates of the three new genera, and one to show the structure of the stigma of these Aspidistreæ; and a large number of new species, especially in the genus Asparagus, were described. In the discussion which followed, Dr. Hooker, Dr. Masters, and others expressed their sense of the great value of Mr. Baker's labours.

Geological Society, Nov. 4.—John Evans, F.R.S., president, in the chair.—The following communication was read:—Notes on the Comparative Microscopic Rock-structure of some Ancient and Modern Volcanic Rocks, by J. Clifton Ward. The author stated at the outset that his object was to compare the microscopic rock-structure of several groups of volcanic rocks, and in so doing to gain light, if possible, upon the original structure of some of the oldest members of that series. The first part of the paper comprised an abstract of what had been previously done in this subject. The second part gave details of the microscopic structure of some few modern lavas, such as the Solfatara Trachyte, the Vesuvian lava-flows of 1631 and 1794, and a lava of the Alban Mount, near Rome. In the trachyte of the Solfatara acicular crystals of felspar show a well-marked flow around the larger and first-formed crystals. In the Vesuvian and Albanian lavas leucite seems, in part at any rate, to take the place of the felspar of other lavas; and the majority of the leucite crystals seem to be somewhat imperfectly formed, as is the case with the small felspar prisms of the Solfatara rock

The order of crystallisation of the component minerals was shown to be the following :—Magnetite, felspar in large or small distinct crystals, augite, felspathic or leucitic solvent. Some of the first-formed crystals were broken and rendered imperfect before the viscid state of igneous fusion ceased. Even in such modern lava-flows as that of the Solfatara considerable changes had taken place by alteration and the replacement of one mineral by another, and is very generally in successive layers corresponding to the crystal outlines. The frequent circular arrangement of the glass and stone cavities near the circumference of the minute leucite crystals in the lava of 1631 was thought to point to the fact that after the other minerals had separated from the leucitic solvent, the latter began to crystallise at numerous adjacent points; and as these points approached one another, solidification proceeded more rapidly, and these cavities were more generally imprisoned than at the earlier stages of crystallisation. In the example of the lava of 1794, where the leucite crystals were further apart, this peculiar arrangement of cavities was almost unknown. The third part of the paper dealt with the lavas and ashes of North Wales; and the author thought that the following points were established :—1. Specimens of lava from the Arans, the Arenigs, and Snowdon and its neighbourhood, all have the same microscopic structure. 2. This structure presents a hazy or milky-looking base, with scattered particles of a light-green dichroic mineral (chlorite), and generally some porphyritically imbedded felspar crystals or fragments of such, both orthoclase and plagioclase. In polarised light, on crossing the Nicols, the base breaks up into an irregular-coloured breccia, the colours changing to their complementaries on rotating either of the prisms. 3. Finely bedded ash, when *highly altered*, is in some cases undistinguishable in microscopic structure from undoubted felstone. 4. Ash of a coarser nature, when highly altered, is also very frequently not to be distinguished from felstone, though now and then the outlines of some of the fragments will reveal its true nature. 5. The fragments which make up the coarser ash-rocks seem generally to consist of felstone, containing both orthoclase and plagiocase crystals or fragments; but occasionally there occur pieces of a more crystalline nature, with minute acicular prisms and plagioclase felspar. 6. In many cases the only tests that can be applied to distinguish between highly altered ash-rock and a felstone are the presence of a bedded or fragmentary appearance on *weathered* surfaces, and the gradual passage into less altered and unmistakable ash. In the fourth division of his paper the author described some of the lavas and ashes of Cumberland of Lower Silurian age. With regard to these ancient lavas, the following was given as a general definition :—The rock is generally of some shade of blue or dark green, generally weathering white round the edges, but to a very slight depth. It frequently assumes a tabular structure, the tabulæ being often curved, and breaks with a sharp conchoidal and flinty fracture. Silica, 59-61 per cent. Matrix generally crystalline, containing crystals of labradorite or oligoclase and orthoclase, porphyritically imbedded, round which the small crystalline needles seem frequently to have flowed; magnetite generally abundant, and augite tolerably so, though usually changed into a soft dark-green mineral; apatite and perhaps olivine as occasional constituents. *Occasionally* the crystalline base is partly obscured and a felsitic structure takes its place. The Cumberland lavas were shown to resemble the Solfatara greystone in the frequent flow of the crystalline base, and the modern lavas generally in the order in which the variour minerals crystallised out. In *external* structure they have, for the most part, much more of a felsitic than a basaltic appearance. In internal structure they have considerable analogies with the basalts. In chemical composition they are neither true basalts nor true felstones. In petrological structure they have much the general character of the modern Vesuvian lavas; the separate flows being usually of no great thickness, being slaggy, vesicular, or brecciated at top and bottom, and having often a considerable range, as if they had flowed in some cases for several miles from their point of eruption. Their general microscopic appearance is also very different from that of such old basalts as those of South Stafford and some of those of Carboniferous age in Scotland. On the whole, while believing that in *some* cases the lavas in question were true basalts, the author was inclined to regard most of them as occupying an intermediate place between felsitic and doleritic lavas; and as the felstone-lavas were once probably trachytes, these old Cumbrian rocks might perhaps be called Felsidolerites, answering in position to the modern Trachy-dolerites. A detailed examination of Cumbrian ash-rocks had convinced the author that in many cases most intense metamorphism had taken place, that the finer ashy material had been partially melted down, and a kind of streaky flow caused around the larger fragments. There was every transition from an ash-rock in which a bedded or fragmentary structure was clearly visible, to an exceedingly close and flinty felstone-like rock, undistinguishable in hand specimens from a true contemporaneous trap. Such altered rocks were, however, quite distinct in microscopic structure from the undoubted lava-flows of the same district, and often distinct also from the Welsh felstones, although *some* were almost identical microscopically with the highly altered ashes of Wales, and together with them resembled the felstone-lavas of the same country. This metamorphism among the Cumbrian rocks increases in amount as the great granitic centres are approached; and it was believed by the author that it took place mainly at the commencement of the Old Red period, when the rocks in question must have been buried many thousands of feet deep beneath the Upper Silurian strata, and when probably the Eskdale granite was formed, perhaps partly by the extreme metamorphism of the volcanic series during upheaval and contortion. The author stated his belief that the Cumbrian volcanoes were mainly subaërial, since some 12,000 ft. of ash- and lava-beds had been accumulated without any admixture of ordinary sedementary material, except quite at the base, containing scarcely any *conglomeratic* beds, and destitute of fossils. He believed also that *one* of the chief volcanic centres of the district had been the present site of Kenwick, the low craggy hill called Castle Head representing the denuded stump or plug of an old volcano. The author believed that one other truth of no slight importance might be gathered from these investigations, viz., that neither the careful inspection of hand specimens nor the microscopic examination of thin slices would in *all* cases enable truthful results to be arrived at, in discriminating between trap and altered ash-rocks; but these methods and that of chemical analysis must be accompanied by oftentimes a laborious and detailed survey of the rocks in the open country, the various beds being traced out one by one and their weathered surfaces particularly noticed.

Physical Society, Nov. 7.—Prof. W. G. Adams, F.R.S., in the chair.—A paper by Mr. G. F. Rodwell was read, on an instrument for multiplying small motions. It consists of a train of multiplying wheels, the first of which is moved by the bar whose elongation is to be measured, while the teeth of the last engage with the threads of an endless screw whose axis is vertical, and carries at its extremity a long index moving over a graduated circle. The multiplying power of the instrument is very great; its defects are its want of steadiness, great internal strain, and the difficulty of bringing the index back to zero when the pressure on the lever connected with the first wheel is removed.—Prof. Foster, F.R.S., made a communication on the geometrical treatment of certain elementary electrical problems. The object of this communication was to illustrate the facility and clearness by which certain of the electrical problems occurring in elementary instruction could be treated by easy geometrical methods. Its application was shown in the following cases: The calculation of the quantity of heat evolved in a galvanic circuit; the calculation of the electromotive force and of the permanent resistance of a voltaic battery from two deflections of a tangent-galvanometer; the determination of the joint resistance of several conductors combined in multiple-arc; and the determination of the distribution of potential and strength of the currents formed by connecting the similar poles of two unequal batteries with the opposite ends of the same conductor.—Prof. Guthrie read a paper on salt solutions and water of crystallisation. The absorption of heat which occurs when a salt is dissolved in a liquid was shown to depend not only on the relative specific heats of the salt and the liquid, but also on the molecular ratio of the resulting solution. This ratio declared itself optically (1) by the singularity of the refractive index when the critical ratio was obtained, (2) by the singularity of density at the same point, (3) by the heat absorbed when (α) a saturated solution was mixed with the medium, and (β) when the salt itself was dissolved in a certain quantity of the medium. The condition of maximum density of water was referred to the existence of a definite hydrate of water. It was shown that every salt soluble in water was capable of uniting with water in a definite ratio (by weight), forming definite solid compounds of distinct crystalline form and constant melting and solidifying points. It was supposed that the ratios of such union are not incommensurable with the ratios of chemical weight, and that the new class of bodies which only exist below

0° C., and may be called *cryohydrates*, are not discontinuous with the hydrated crystalline salts previously known. A few cryohydrates were described as being obtained from the saturated aqueous solutions of the respective salts on the withdrawal of heat. Thus chloride of sodium combines with 10·5 (? 10) molecules of water, and solidifies therewith at — 23° C. Chloride of ammonium combines with 12 molecules of water, and solidifies at – 15° C. The combinations with water were given of the sulphates of zinc, copper sodium, and magnesium, also those of the nitrates of potassium, chlorate of potassium, and bichromate of potassium. As far as experimental results at present indicate, it appears that those cryohydrates which have the lowest solidifying point have the least water. Some suggestions were offered concerning the application of these experimental results to the explanation of the separation of the Plutonic rocks from one another, and the importance was pointed out of the use which these cryohydrates will have in establishing constant temperatures below 0° as fixed and as readily obtainable as 0° itself.

Mathematical Society, Nov. 12.—Dr. Hirst, F.R.S., president, in the chair.—The President informed the meeting of the loss the Society had sustained by the recent death of one of its honorary foreign members, Dr. Otto Hesse, of the Polytechnicum, Munich, and mentioned that it was the intention of the Council soon to fill up the vacancies caused by the deaths of Drs. Clebsch and Hesse. — On the motion of Prof. Cayley, F.R.S., seconded by the Rev. R. Harley, F.R.S., it was ordered that the cordial thanks of the Society be presented to Lord Rayleigh for his munificent donation of 1,000*l.* to the Society, and the chairman was requested to convey the same by letter to his lordship. — The money has been vested, as the treasurer's report mentioned, in 870*l.* Guaranteed Indian Railway Stock, and the interest will be applied, as was stated two or three months since in NATURE, to the purchase of mathematical journals, and also to assist in defraying the expense of printing the Society's Proceedings. The meeting then proceeded to the election of the new Council, and the gentlemen whose names were given in a recent number of this journal were declared by the scrutators to be duly elected.—Instead of giving the usual valedictory address, Dr. Hirst stated what results he had arrived at in the course of his investigations upon "Correlation in Space." The communication was an extension to space of results arrived at in his paper (read before the Society in May last), entitled the "Correlation of Two Planes."—Mr. J. H. Röhrs read an abstract of a communication on "Tidal Retardation." The problem discussed is the superior limit to the tidal retardation in a globe, in all respects similar to our own, except that it is covered entirely by a sea, the depth of which is constant for all places in the same latitude, and is therefore a function of latitude only—not longitude—a function supposed to be known.—A paper by Prof. Wolstenholme on a new view of the porism of the in- and circumscribed triangle was taken as read.

Anthropological Institute, Nov. 10.—Prof. Busk, F.R.S., president, in the chair.—Reports were read by Mr. F. W. Rudler on the Anthropological Department of the British Association in Belfast, and by Mr. Hyde Clarke on the Anthropological Section of the International Congress of Orientalists recently held in London.—A paper was then read by Col. Lane Fox on a series of flint and chert arrow-heads and flakes from the Rio Negro, Patagonia, with some remarks on the stability of form observable in stone implements. The series of specimens exhibited was selected from a collection of 500 gathered by Mr. W. H. Hudson on the margin of the river and over an extent of about ninety miles, and on the numerous lagoons, now mostly dry, with which the valley is everywhere intersected. The valleys in that region run through high terraced table-lands; and on the plateaus above there is no water and but very scanty vegetation, which would seem to indicate the improbability of their having been occupied by man. A great number of the implements were discovered by Mr. Hudson on the sites of villages in the valley and in circular flattened mounds of clay measuring from 6 ft. to 8 ft. in circumference. The different styles of workmanship observed in the different villages were not, in the opinion of Mr. Hudson, to be attributed to the variety of material employed, but to the degree of skill possessed by the inhabitants of each village. The author drew attention to the interesting fact of the arrow-heads having long fallen into disuse among the Tehuelches and other Patagonian tribes, who now and for some centuries past employed the spear. Col. Fox proceeded to describe in detail the various weapons and their varieties of workmanship, and showed that they all presented the same general features as implements found in the United States. He believed that, owing to our inability to understand the uncultured mental condition of savages and prehistoric races, we often lose sight of the inferences deducible from the stability of form observable in their arts and implements, and attach less importance than should be the case to minute varieties of structure.—It was announced that the Council had resolved to publish in the Journal of the Institute bibliographical notices, abstracts and reviews of English and foreign works and papers, and other miscellaneous matter of anthropological interest and importance.

PARIS

Academy of Sciences, Nov. 2.—M. Bertrand in the chair. —The following papers were read :—General results of observations on the germination and first developments of different lilies, by M. P. Duchartre.—Researches on the dissociation of crystalline salts, by MM. P. A. Favre and C. A. Valson.—Results of the voyage of exploration undertaken for the preliminary study of the general track of a railway connecting the Anglo-Indian with the railways of Russian Asia, by M. F. de Lesseps.—Rational treatment of pulmonary phthisis, by M. P. de Pietra Santa.—On new apparatus for studying the phenomena of the combustion of powders, by MM. Marcel-Deprez and H. Sebert.—Theory of electrodynamics freed from all hypotheses relating to the mutual action of two current elements, by M. P. Le Cordier.—Monograph of the anguilliform family of fishes, by M. C. Dareste.—On the existence of a sexual generation in *Phylloxera vastatrix*, by M. G. Balbiani.—On the solution of numerical equations of which all the roots are real, by M. Laguerre.—On an apparatus for determining personal equations in observations of the transit of stars, arranged for the geodesic service of the United States, by MM. Hilgard and Suess.—On the laws of the vibratory motion of tuning-forks, by M. E. Mercadier.—Note on a modification of Fehling's and Barreswil's solutions for the determination of glucose, by M. P. Lagrange.—On the fermentation of fruits, by MM. G. Lechartier and F. Bellamy. The authors have now examined the products from cherries, gooseberries, and figs.—Application of the graphical method to the study of certain points in deglutition, by M. S. Arloing. The author concludes from his experiments that a decided difference exists between the swallowing of liquids and of solids.—On the mechanism of deglutition, by M. G. Carlet.—Results furnished by surgical operations performed on patients in which anæsthesia has been produced by the intravenous injection of chloral, by M. Oré.—Note on a cyclone observed at La Pouëze (Maine-et-Loire) Sept. 30, 1874, at 4.30 P.M., by M. Al. Jeanjen.—The Report of the Commission appointed on August 17 for preparing a reply to the letter addressed by the Minister of Public Instruction concerning the organisation of a Physical Astronomical Observatory in the neighbourhood of Paris, was read at the conclusion of the meeting.

BOOKS RECEIVED

BRITISH.—Meteorological Committee (her Majesty's Stationery Office).—Beauty in Common Things, by the author of "Life Underground" (Society for the Promotion of Christian Knowledge.)

AMERICAN.—Monthly Report of Department of Agriculture, October 1874 (Washington, U.S.)

COLONIAL.—Red Corpuscles of the Blood : R. H. Bakewell, M.D. (Mills, Dick, and Co., Otago, N.Z.)—Centrifugal Force and Gravitation : John Harris (John Lovell, Montreal).—Prodromus of the Palæontology of Victoria (Australia) : John Ferris (Melbourne).

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