THURSDAY, NOVEMBER 23, 1893.

WATSON'S KINETIC THEORY OF GASES.

A Treatise on the Kinetic Theory of Gases. By Henry William Watson, D.Sc., F.R.S. Second Edition. (Oxford: at the Clarendon Press, 1893.)

THE rather pointed reference to myself, which Dr. Watson makes at the end of this new edition of his work, seems to call for an answer. Had this call come some five or six years ago, when the questions once again at issue were debated in a somewhat lively way, I should have had little difficulty in rising to it:—but I have in the interval been so busy with questions of a totally different nature that I am taken at a disadvantage, especially as I cannot at present find time to read up again the discussions of that period. I remember enough about them, however, to make the very positive assertion that the questions then raised turned on points of logic, relevancy, and consistency, much more than upon physical ideas or mathematical processes; and a perusal of Dr. Watson's volume shows me that he has reproduced from Boltzmann and others much of what I then objected to. I believe that I gave, in 1886 (*Trans.* R S.E. vol. xxxiii.), the first (and possibly even now the sole) thoroughly legitimate, and at least approximately complete, demonstration of what is known as Clerk-Maxwell's Theorem, relating to the ultimate partition of energy between or among two or more sets of hard, smooth, and perfectly elastic spherical particles. And I then pointed out, in considerable detail, the logical deficiencies or contradictions which vitiated Maxwell's own proof of 1859, as well as those involved in the mode of demonstration which he subsequently adopted from Boltzmann. Dr. Boltzmann entered, at the time, on an elaborate defence of his position; but he did not, in my opinion, satisfactorily dispose of the objections I had raised. Of course I am fully aware how very much easier it is for one to discover flaws in another man's logic than in his own, and how unprepared he usually is to acknowledge his own defects of logic even when they are pointed out to him. But the only attacks which, so far as I know, have been made on my investigation, were easily shown to be due to misconception of some of the terms or processes employed.

Dr. Watson's little work has been for many years the recognized text-book on the kinetic theory of gases:—and there can be no doubt that, considered in the fierce light of the Examination Hall, it is well adapted to the wants alike of actual Moderators and of would-be Wranglers. It is so framed as to be easily dissected into compact and thoroughly self-contained pieces of book-work:—from "easy" up to "rather stiff":—and, were these to be answered at all nearly in the words and formulæ of the text, few Examiners would venture to refuse full marks. From this point of view nothing more could be desired; for incorrect historical notices, such as the ascription of the origin of the theory to J. Bernouilli (properly D. Bernoulli) instead of R. Hooke, will injure no man's place in the Tripos. The purely mathematical part, mainly a series of exercises in the transformation (by functional determinants) of differential elements from one system of variables to another, though elegant enough, presents an aspect of sameness. *Toujours perdrix!* To this point we will recur.

Considered as a scientific treatise, however, and as practically the only one in Britain which deals at all fully with the subject, the work is not quite so deserving of commendation. Much of course has, in all cases, to be allowed for the almost necessary defects of a book which deals in any way with questions of probability. It has been the good fortune of but a very few, even among the most gifted of mathematicians, to be able to thread their way in safety through the countless traps and pitfalls which lurk unnoticed, often undiscoverable till they have done their worst, in every part of every region of this fascinating domain:—not, as in other subjects, in the partially explored nooks and crannies alone. But probability is only one application of logic:—and, in the passages we most object to, it is in general ordinary logic which we think is somewhat lightly treated. We do not require to go far in search of an example.

At the very commencement of the work, while dealing with Maxwell's well-known result for the permanent distribution of velocities among a number of equal, smooth, spherical particles, Dr. Watson says:—

"We assume that in the permanent state the distribution of the spheres throughout the space occupied by them is homogeneous in all respects; that is to say, on an average of any long time there are the same number of spheres in a given volume wherever that volume may be situated, and the law of distribution of velocities is the same throughout that volume as in the whole region under consideration."

On this statement we would remark that it is rather vague and incomplete:—for surely it is meant that the distribution is isotropic as well as homogeneous; and the word "long" has absolutely no meaning until the time-unit is assigned. Dr. Watson then proceeds to investigate the circumstances of an individual (but typical) collision. Here, however, logic steps in, and says:—"Halt! You have already *assumed* all that you need learn from collisions, so far at least as concerns the solution of the problem before you." In fact the assumption, read as above, leads at once to Maxwell's Law, by the very process which its discoverer first employed; a process depending on principles freely used throughout the text of this book. When Dr. Watson has found the state of motion (F), of two spheres after collision, in terms of the state (E) before it, he proceeds thus:—

"For permanence of distribution . . . it is sufficient that the number of collisions of pairs of spheres in state E during the time dt should be equal to the number of collisions of pairs in the state F during the same time."

This leads, of course, to Maxwell's result. But it is not hypercritical to ask whether the above-mentioned requirement is not merely "sufficient" but *much more than* sufficient:—so exacting, in fact, as to be absolutely unattainable. Note the consequences of it. From the very nature of the data, the whole motion in the present case is strictly *reversible*, so as exactly to retrace its entire history. But, if we were to reverse it, we should still have the "permanent" state:—*i.e.* one which could never have been otherwise than as it is! This principle of reversion underlies a great part of the theory; and a mere reference to it would, in many of the later pages of the book, usefully take the place of a multitude of imposing but superfluous symbols.

Another notable point in the investigation, to which attention should be drawn, is the mode of obtaining the expression for the probability of collision. This is given as proportional to the component of the relative speed along the line of centres at impact; and *not* to the relative speed itself, though this is *proved* to be the case in § 5. The final result, however, is rendered correct by means of a compensating error in the specification of the element of space really involved. This procedure cannot fail to bewilder a thoughtful reader.

All these remarks, it is to be observed, are made on the very first proposition in the work:—the "green tree," as it were! What might not be expected in the "dry"; *i.e.* the demonstration of Boltzmann's Theorem, to which the book gradually leads up? But I must not now recapitulate the objections which I made (about 1886-8) to Boltzmann's methods, nor the modes in which he defended them. Those who are curious about the matter may be referred to the *Phil. Mag.* for that period. All I need here say is that I do not think that Dr. Watson's book meets any of my objections.

From the experimental point of view, the first great objection to Boltzmann's Theorem is furnished by the measured specific heats of gases; and Dr. Watson's concluding paragraphs are devoted to an attempt to explain away the formidable apparent inconsistency between theory and experiment. In particular he refers to a little calculation, which I made in 1886 to show the grounds for our confidence in the elementary principles of the theory. This was subsequently verified by Natanson (*Wied. Ann.* 1888) and Burbury (*Phil. Trans.* 1892). Its main feature is its pointing out the absolutely astounding rapidity with which the average amounts of energy per particle in each of two sets of spheres in a uniform mixture approach to equality in consequence of mutual impacts. Thus it placed in a very clear light the difficulty of accepting Boltzmann's Theorem, if the degrees of freedom of a complex molecule at all resemble those of an ordinary dynamical system.

P. G. TAIT.

A HISTORY OF CRUSTACEA.

A History of Crustacea. Recent Malacostraca. By the Rev. Thomas R. R. Stebbing, M.A. With numerous illustrations. (The International Scientific Series, Vol. lxxiv.). (London: Kegan Paul, Trench, Trübner, and Co., Ltd., 1893.)

"THE ambition of this volume," writes the author in his preface, "is that it shall be one to which beginners in the subject will naturally have recourse, and one which experienced observers may willingly keep at hand for refreshment of the memory and ready reference." A most laudable ambition, and one that the author, we doubt not, set out with an intention to fulfil. The want of a volume of this very sort upon this subject had been often felt by both the student and the expert. The advance in our knowledge of the group had made it impossible to annotate effectively that model "History of the Crustacea," written by Milne Edwards, and Mr. Stebbing's painstaking, excellent memoir on the Amphipods of the *Challenger* Expedition had pointed him out as a possible author of a useful manual. To write, however, a useful manual or history requires that one should take a wide and all-sided view of the subject, so as to secure a fair symmetry in its treatment; once the amount of detail to be given has been determined upon it should be rigidly adhered to, and, needless almost to add, no useless or unnecessary matter should be allowed to obtrude itself. Therefore, to a knowledge of the subject there must be added certain powers of judgment, to which it would be well to join certain gifts of style, in order that a satisfactory result might be obtained.

None will deny to the author of this history of Crustacea a knowledge of his subject, and the immense amount of facts that he has condensed into the four hundred small pages of this volume will astonish those who peruse it. But for all this there is abundant evidence that it was begun without any sort of judicious calculation as to its scope, and the reader will be as sorry on the discovery as we fancy its author was, that the dire necessity of space has made what purports to be a history of the Crustacea into only a manual of the Malacostraca, and not even a complete manual of this sub-class, for at page 436 we read: "To complete the sketch of the Malacostraca, the sub-order of the Amphipoda *remains to be described*." Chapters describing this sub-order had been written, when it appeared they overflowed the utmost space that could be allowed, and with this statement the "History of the Crustacea" ends. Our sympathies are with the author, for might we not have expected something excellent about a sub-order that he had made so peculiarly his own, and had we not a right to expect, after reading the first fifty pages, some information about the vast swarms of Entomostraca and Cirripedia. Apparently it was all ready, but the author was met with a "to so far you may print, and not a page further." The promises of what may be in the future seem too uncertain to depend upon.

Having thus expressed our disappointment about what we have not been given, we proceed to record our opinions as to what the publishers have allowed. The volume opens with an introduction of some fifty pages, which treats of the classification of the Crustacea in outline, giving us brief details of the sub-classes and orders, notes on the geographical distribution, hints as to collecting, statements about size and description of the segments and their appendages. This is followed by a table of the sub-classes, orders and sub-orders, and the account of the Malacostraca, as far as the end of the Isopoda. The plan adopted is to give a short diagnosis of the orders and sub-orders, the tribes and families; under these last, the principal genera and some of the more important species are given.

While dates are appended to the genera, and the names of the describers are given, yet there are only in two or three instances any indications as to where the descriptions are to be found. It would have added much to the value of the work had it been possible to have given these, but of course it would have added (whatever scheme was adopted) very greatly to the amount of the text. Possibly a little more information of this sort might have been squeezed in had the pen been struck through a number of useless sentences which rather detract from the scientific aspect of the volume; such as the statement of the views of the "very intelligent student" on the subject of the eyes of the shrimp (p. 225); the suggestion that the "Sea-devil" of the Mediterranean might well be the "great fish" referred to in the Book of Jonah (p. 222); the criticism on Spence Bate's description of *Parathanas immaturus*, apparently only given to afford the opportunity of quoting an ungallant saying about women (p. 233), and several such like; or we could have been spared three pages about *Birgos latro*, or the half-page of a justification for giving Hansen's most excellent synoptic table of the Cymothoid group. Indeed, the author's desire not to make this manual a "dry and repulsive catalogue" has made him write a number of sentences which the seriously-minded reader will find it better to pass over with a very cursory eye. To conclude all we have to say on this aspect of the volume, we have strong objections to urge to the page headings, as being an attempt not to help but to confuse. Possibly the author may not be accountable for these; they have often so little to do with the subject of the matter in the pages, that it is not unlikely that they were selected by some one as ignorant of the subject as of good taste; as examples we quote the following: "The tail unique," "A box of branchiæ," "An affectionate squeeze," "Perils of baby-farming," "Looking like a buffoon," "How genera are generated," and many such like.

With all these little defects, which might so easily have been avoided, this volume will be indispensable to the student of this class of Arthropods; it brings together in an intelligible form an immense mass of literature. In some of the orders most complete lists of genera and species are given, notably among the Isopods. Those species interesting either for their morphological, geographical, or bathymetrical distribution, are invariably mentioned, and so far as we can judge, all the British species are named. Most useful will this volume, compact in size and well-packed with information, be to collectors. There is at present no one work that can compete with it. Perhaps the day may come when our great National Museum may publish a revised list of all known Crustacea, as they have done of the fishes, reptiles, and birds; till then Mr. Stebbing's volume will not lose its value, a value that would be greatly increased should a companion volume be published giving the history of the remainder of this interesting group. The work is embellished by nineteen plates and thirty-two illustrations in the text.

OUR BOOK SHELF.

An Elementary Treatise on the Geometry of Conics. By A. Mukhopadhyay. (London: Macmillan, 1893.)

THIS work is well adapted for junior students. It treats of the principal properties of the curves, and may well be read after a pupil has mastered his six books of Euclid. The starting point is from the focus and directrix definition, and no modern methods (as projections) are employed, nor are the curves shown to be obtainable from plane sections of the cone. Each curve has a chapter allotted to its discussion, which is conducted, as far as possible, on uniform lines. To the parabola are assigned twenty-five propositions, to the ellipse thirty-five propositions, and to the hyperbola thirty-seven propositions, with an additional five for the rectangular form. The order of treatment is mechanical description, chord properties, and then tangent properties. The proofs should be readily mastered by a boy who knows his Euclid, for they are clearly and simply put, and the author does not assume the truth of a converse proposition, as we have noticed some writers do. Mr. Mukhopadhyay has read far and wide in his subject, and has brought together in his 800 exercises a large collection of the most interesting problems. Many of these he accompanies with full solutions, and to very many more he furnishes suggestive hints. The figures are white on a black ground. The book appears to be very correctly printed; at any rate, we have detected very few (easily corrected) misprints. The book appeals successfully to a larger public than the students of the Indian colleges.

The Geometrical Properties of the Sphere. (Univ. Corr. Coll. Tutorial Series.) By William Briggs and T. W. Edmondson. (London: W. B. Clive, 1893.)

IN these fifty pages the authors have brought together most of the chief geometrical properties of the sphere, intending the book to be used as a companion to their larger one, on mensuration of the simpler figures, by students preparing for the intermediate examinations in Arts and in Science of the University of London. The three chapters into which the subject is divided lead the reader from the elementary definitions relating to great and small circles, poles, lunes, &c., through the numerous geometrical properties of spherical triangles and their antipodal triangles, polar triangles, supplemental triangles, and finally to the determination of the area of lunes, spherical triangles, spherical polygons, and the spherical excess. The definitions and theorems are expressed quite clearly throughout, while the figures leave nothing to be desired. As an introduction to works on spherical trigonometry, students will find this book a most helpful guide. Two minor slips in construction will be found: one on page 6, line 6, where for CT read TC; and the other on page 18, line 9, where for *oa* and *ob* read *ao* and *bo*.

A Key to Carroll's Geometry. By J. Carroll. (London: Burns and Oates, Ltd., 1893.)

THIS key contains the solutions of the exercises in orthographic projection and solid geometry, which are given in the author's book on geometry. The solutions seem to have been thoroughly and carefully worked out. The figures are generally drawn to full scale, but sometimes half-scale has been employed. Lines of projection are clearly indicated—an important factor in some of the more complicated figures. The key should prove a help to beginners, who should study well the questions and their accompanying figures.

LETTERS TO THE EDITOR.

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"Geology in Nubibus."—A Reply to Dr. Wallace and Mr. LaTouche.

DR. WALLACE has taught us a great deal, and among those lessons is the supreme virtue in scientific controversy of courage and candour. He must forgive me therefore for answering promptly, and I hope frankly, his last letter in NATURE. In this letter he appeals from your columns to a non-scientific magazine in which he is writing, and where, like the sermon from the pulpit, what is said cannot be answered. This appeal is not to my taste, for I agree with the late Lord Tweeddale, that truth is never so free from difficulty as when the good grain has been thrashed out by the flails of controversy.

The position we are fighting about is too important, however, to go by default, for upon it rests a vast deal of induction in other fields besides geology.

My contention is, and I am speaking to every man of science, geologist or otherwise, that before Dr. Wallace can appeal to ice as the excavator of lake basins on level, or nearly level, plains *far* away from the slopes where glaciers grow, he must establish two postulates. (1) That ice can convey thrust for more than a very moderate distance. (2) That glaciers such as we can examine and report upon are anywhere at this moment doing the excavating work which he postulates. Without these postulates, his appeal to ice seems to me absolutely outside science altogether, and to be a mere resort to some *Deus ex machina*, such as the mediæval schoolmen based their reasoning upon.

In regard to the first postulate the experimental evidence seems to me to be conclusive, and I have quoted it in my work on the glacial nightmare. Mallet, writing on the modulus of ice, says: "A few experiments have been made which show that the height of this modulus cannot exceed a few hundred feet." "Let it be assumed, however, that it is as great as 5000 feet, or a mile. It is then obvious that a mass of ice, no matter how deep or wide, lying in a straight, smooth, frictionless valley, cannot be pushed along by any extraneous force, in the line of the valley, through a distance of more than a single mile, for at that point the ice itself must crush, and the direct force cease to be transmitted further. This, of course, is far from being the whole of the question of the transmission of force through ice, for when and wherever crushing takes place, a certain portion (though a small one) of the direct pressure is transmitted laterally by the crushed fragments, especially if mixed with water. For this to take place however, in the direction of the length of the ice-filled valley, supposes the ice must be considerably more than a mile in vertical depth." Mr. Oldham has carried the question further, and I have quoted his arguments and experiments on pages 596–597 of my book. His conclusion, after postulating a quite transcendant modulus, as tested by observation, is: "The greatest distance to which a glacier could be forced *en masse* is about five miles, so that a glacier debouching on a plain could not exert any erosive power on that plain for more than five miles from the commencement of its level course, and consequently could not scoop out a lake basin of more than that length, whatever its depth might be."

Not only does this conclusion involve the postulating of quite an impossible modulus for ice, but it also supposes that the whole thrust of the ice coming down a slope is available, which it clearly is not. A great deal of this thrust, as Mr. Irving has shown, is expended in overcoming cohesion, in causing the differential motion of a glacier, in forming crevasses which largely intercept the thrust, and in causing the well-known Bergschrund. To quote my own words, "a considerable amount of the force of the gravity contained in a glacier is used up within the glacier itself, and is not available either to give it a forward thrust along a horizontal surface, or for eroding purposes."

So far as I know, this is a perfectly candid statement of the available evidence. Regelation has nothing whatever to do with it. Directly ice crushes, the thrust is dissipated, the greater part of it passing off in the direction of least resistance. To me the case seems conclusive, but, says Dr. Wallace: "All this is beside the question from my point of view. The work of the ice on the rocks is as clear as that of palæolithic man on the flints . . . and there is clear evidence that ice *did* march a hundred miles, mostly uphill, from the head of Lake Geneva to Soleure, whatever transcendental qualities it must have possessed to do so."

This form of dogmatic argument is assuredly incomprehensible. I wonder Dr. Wallace is not afraid of the ghosts of his own recent emphatic pronouncement on the glaciation of Brazil, which he has now entirely abandoned, namely: "If the whole series of phenomena here alluded to have been produced without the aid of ice, we must lose all confidence in the method of reasoning from similar effects to similar causes which is the very foundation of modern geology."

No, true geology is not founded upon hypotheses outside the laws of nature; its secrets, when properly read, must be consistent with those laws. Nor can the geologist who hopes to see his work live, base his reasoning upon a peculiar scheme of mechanics which experiment refuses to verify.

If glaciers travelled further in former days, it was doubtless because glaciers were larger in former days, because they descended longer slopes, and had larger gathering grounds; that is to say, because the country where they grew was more elevated. All this I, of course, admit was the case. That ice could travel then any more than it can travel now over a considerable distance of level ground, or excavate hollows in its track, by virtue of the *vis a tergo* given it in its sloping cradle, is, it seems to me, a subjective dream, and not an empirical conclusion.

So much for the first postulate necessary to establish Dr. Wallace's conclusion. In regard to the second, I have little to say. Glaciers exist in many countries. In some they have retreated in historical times; in others, we can travel underneath them for some distance. I know of no case, under any conditions, where it can be shown that they have excavated rock basins, small or big. If Dr. Wallace can quote any, it would be an important addition to the case he makes. I must therefore conclude that, so far as our evidence goes, ice cannot excavate lake basins on level plains, and that it is contrary to the laws of the mechanics that it should do so.

Dr. Wallace says, "No glacialist of the extremest school would claim the rock basins of Bahia as proofs of glaciation." This is an extraordinary statement. Why, the report on these basins made by Mr. Allen, and incorporated by Hartt, was among the most powerful pieces of evidence adduced by the latter for the former glaciation of Brazil, which evidence Dr. Wallace urged upon us a short time ago was completely unanswerable. Lastly, in regard to Tasmania I do not quite follow him. He says, "No doubt the conclusions of the various writers will be fully harmonised by a more complete study of the whole subject." They are harmonised already. *They all agree* that on the plateaus and in the central district of Tasmania, where the lakes abound, there are no traces of glaciation. So far as I know, the only person who disputes it is Dr. Wallace himself, who has never been there. What needs to be harmonised is his theory with the facts as observed by all observers.

I have replied at some length to Dr. Wallace's letter, not only because I consider the issue a most critical one, but also because of the distinction of its writer, who on so many questions has taught us lasting lessons, but who on this one seems determined to set himself against the general conclusions of those geologists who have most closely and laboriously studied ice at work.

I must now turn to Mr. LaTouche, whose courteous criticism of my views appeared in a previous number of NATURE. I am not quite sure how far we differ, for he apparently repudiates the theory favoured by Ramsay and by Dr. Wallace, that the great Alpine and Scotch lakes were excavated by glaciers. He limits himself to certain rock basins in highly glaciated regions. In regard to these having been excavated by ice, Mr. LaTouche reminds me that ice is a viscous body, and moves, as Principal Forbes argued that it does, almost entirely as a viscous body. If Mr. LaTouche had favoured me by looking into my last book, he would have found a long and very laborious chapter devoted to establishing this very conclusion, but I do not see how it assists his position. A viscous body, unless the viscosity approaches that of a liquid, cannot move by mere hydrostatic pressure, since the internal friction and the resistance and mutual support of its particles prevent it. The viscosity of ice is very slight indeed, hence we cannot postulate for the nether layers of a glacier with an uneven surface the movements we should postulate in a liquid under the same conditions. With the forces known to be requisite to make it shear, it seems to me that ice cannot be supposed to move by hydrostatic pressure.

Its actual motion is due almost entirely to its layers rolling over each other as they do in pitch and other viscous bodies. Now this movement in thick ice we know is appreciable at the surface, but the same conditions of friction and of drag, already quoted, retard each successive layer as we go down, until when we reach the lowest layers the motion due to viscosity is exceedingly slight if it is even appreciable. Hence I cannot see where the mechanical agent is to come from to excavate basins, and how it is to work.

When ice is moving on a slope, and the viscous movement is helped by gravity, then no doubt the ice-foot shod with stones becomes a tolerable *eroding* agent; but I cannot understand under what conditions it can become an excavating one, and how it can hollow out basins, &c.

When ice moves away from the slope which gives impetus to a glacier, the motion rapidly slackens and presently stops. The distance travelled over the level ground is a function of the weight of the glacier, of the amount of the slope, the friction of its bed, &c., *i.e.* of the elements making up the *vis a tergo*; but in the very largest glaciers, so far as observation goes, the motion rapidly ceases on level ground. This is the evidence wherever the phenomenon has been observed and reported upon.

This being so, I altogether question not only the arguments of those who champion the excavation of lake basins by ice, but also of that larger school who invoke movements of ice over level plains of many hundreds of miles in extent in order to explain the drift phenomena. They do it, so far as I know, on the ground that they cannot appeal to any other cause without doing injustice to that modern metaphysical bogey, "The Doctrine of Uniformity." My small boy might just as well, on the same principle, attribute the excavation of his porringer to the porridge in the bowl. True rock basins were no doubt very largely due to the weathering of rocks which exfoliate, and whose structure is not homogeneous. This is a very old explanation, but like many sober old inductive truths it is not so attractive nowadays as an appeal to the imagination, combined with a good, sturdy, consistent loyalty to some *à priori* postulate, which would have won the hearts of the old schoolmen.

HENRY H. HOWORTH.

30 Collingham Place, Cromwell Road, November 16.

Rock Basins in the Himalayas.

THERE is one statement in the interesting communication of my colleague, Mr. T. D. LaTouche, which seems to require qualification. After a tolerably extensive experience of the Himalayas, I should be inclined to say that rock basins are of fairly frequent occurrence, of all sizes from the largest to the smallest, but they are almost without exception filled with stream deposits, and only occasionally can their formation have been due to glaciers; for they are usually found where there are no traces of glacial action to be seen, and at levels to which we have no reason to suppose that glaciers ever reached. In the hills of eastern Baluchistan, where the rainfall is much less than in the Himalayas, rock basins more or less filled by recent surface deposits are even more common, and here their origin by deformation of the surface can generally be established. The same cause probably accounts for the Himalayan rock basins, as there are abundant proofs that the elevatory movement has been far from uniform, and that the variations in its intensity have been both extensive and often extremely local. There are frequent occurrences of surface deposits which appear to have originally been formed in rock basins, but have since been cut into by the streams, owing to the corrasion of the barrier, and we may attribute the absence of lakes in the Himalayas to the rapid current and large burden carried by the streams, in consequence of which they have been able to fill up the basin, and often to corrade the barrier, as fast as it was formed.

R. D. OLDHAM.

"Composite" Dykes.

PROF. JUDD's excellent paper in the current issue of the *Quarterly Journal of the Geological Society* (p. 536) calls to my mind some common and similar examples among the "elvans" of Cornwall (which are dykes in the ordinary acceptation of the term), and but little has been published offering some explanation of their bearing on surrounding rocks. I have observed, notably in the district of Cligga Head (nine miles N.W. of Truro), the marked difference between the structures exhibited by dykes in the parts in contact with the rock through which they intrude (in the Cligga instance Devonian slate), and their centre, amounting almost to a rock distinction.

In the appended sketches I have endeavoured to illustrate my meaning from actual instances.

Fig. 1 represents a section of an elvan or dyke outcropping slightly to the north of Cligga promontory, and from its position apparently connected with the main mass of Cligga Head granite. It bursts through the slate. The centre (*b*) of the dyke consists of a rock of homogeneous texture, quartzo-felspathic base, and some scattered porphyritic felspar crystals. The sides (*a a*) in contact with the slate (*s s*) show a rock of apparently similar base, but shot with long acicular crystals of schorl, the whole rock being of a very dark colour, due probably to the presence of wolfram.

Fig. 2 is a section of a very common form of Cornish elvan, consisting of alternate laminæ of granite (*d d*) and "schorl rock," that is, rock consisting of schorl and quartz, generally in about equal proportions (*c c*).

These bands are very common in the slates and in the granitic bosses. Further, an analysis of a typical "schorl rock" of this class showed a silica percentage of 67·6 (*vide* Judd's paper,

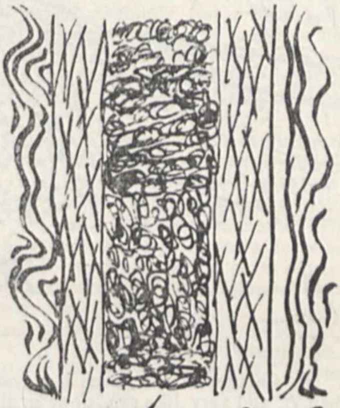


FIG. 1.

p. 545), and of a typical granitic band of 74·8 (De La Beche, "Report on Geology of Cornwall, Devon, and West Somerset," p. 189). It is very doubtful, however, if either of the above instances is a case of a dyke putting on such differences in mineralogical and chemical character in its several parts as to amount to a difference of rock species.

As De La Beche points out, the schorl rock may be simply a granite in which the felspar and mica are replaced by schorl. An instance, however, of a rock one may call "a dyke within a dyke" is the Cligga mass itself, which is nothing but a gigantic dyke. De La Beche, in his work above cited (p. 164), has figured it. The dyke is so strikingly split into layers as to

FIG. 2.

appear stratified, the hard comparatively small-grained layers standing out in bold relief from the contiguous layers of more easily decomposed rock with their large porphyritic felspar crystals.

Besides the difference in size of the felspar crystals, the harder rock is much darker in colour (being of a red hue) than the softer, which is pale pink and in places whitish. These physical differences, however, count for little in drawing a distinction of rock species between the layers, and I was unfortunately unable to avail myself of any published analyses of the different parts, but their superficial characters are so distinct as to render the stratified appearance of the rock very marked at comparatively great distances from them.

There is in many cases a crack marking the junction of contiguous layers.

As an illustration of these "composite" dykes, I append a diagrammatic sketch representing a section of the coast about 200 or 300 yards south of the Cligga promontory, which is very difficult of approach.

A has all the appearance of a bed of sandstone, the strata curved, owing to the intrusion of the dyke *B* (granitic; *C* is an

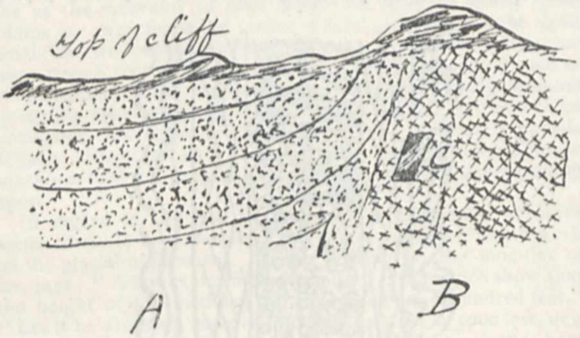


FIG. 3.

old tin burrow. As a matter of fact, each is a granitic dyke, *A* finer grained than *B*, and very like sandstone in all petrological features.

The remarkable fact is the apparent stratification of the beds *A*, which are really bands of several dykes—a continuation of those figured at p. 164 in De La Beche's book. He does not seem to have observed this instance, or at any rate does not mention it; his figure is from the cliff immediately in contact with the Cligga promontory, and north of that I have figured.

Further instances of this very interesting kind of composite dyke would help in many cases to unravel the seeming complexity of such geological features as those I have touched upon in Cornwall. HENRY E. EDE.

46 Walker Terrace, Gateshead-on-Tyne, October 4.

Weismannism.

I NEVER answer reviews, save in so far as they may be misleading on matters of fact. As this is the case with "P. C. M.'s" notice of my "Examination of Weismannism" (NATURE, November 16), I should like to say a few words touching the more important of such matters.

It seems that in seeking to do justice to all sides in the heredity question, I have been too careless in expressing my own view. At all events, any one reading the review must gather from it that I am a Lamarckian engaged in fighting the theories of Prof. Weismann. In the book, however, it is stated that I have been an adherent of the theory of Stirp ever since it was published by Mr. Galton in 1875. It is also stated that this theory is, in my opinion, identical, as regards all main principles, with that of Germ-plasm in the present phase of its numerous metamorphoses. Therefore, far from fighting the Weismannian theory of heredity, I see in all its main features, as it now stands, a "re-publication" of the one which I have held for close upon twenty years.

It is further stated that the only points of much secondary importance wherein I can perceive the two theories to differ are, (*a*), that while Galton confined himself to publishing a theory of Heredity, Weismann proceeded to rear upon this basis (*i.e.*, the hypothesis of "continuity") a further and elaborate theory of organic evolution; and, (*b*), that Weismann has not gone so far as Galton did in expressly recognising the possibility of an occasional transmission of acquired characters, in faint though presumably accumulative degrees. As regards these two points of difference, I have endeavoured to show, (*a*), that Weismann has now himself withdrawn nearly all his previous generalisations with regard to organic evolution, while largely modifying his theory of heredity; and, (*b*), that he has only to expand certain hints which he has already given—and which, if expanded, would entail much less modification of his original system than those which he has now made in other parts thereof—in order as fully to recognise as Galton did the possibly occasional transmission of acquired characters.

Hence, such opposition as I have found any reason to express with regard to Weismann's system in the latest phase of its development arises, almost exclusively, against the inordinately speculative character of his method. The history of science furnishes no approach to such a disproportion between deduction and induction.

Thus it seems to me that any writer on Weismannism who aims at impartiality must fail in his aim, if he does not give due prominence to this the most distinctive feature of Weismann's method. And, unless the reviewer is prepared to defend such a method as scientific, he has no reason to quarrel with what he calls my "hard words," since they all have reference to it, and are statements, not of opinions, but of facts.

On the other hand, I have endeavoured by "soft words" to fully recognise the great merit of Weismann's work in constituting the heredity question one of world-wide interest. And any bias that I may have with regard to this question is assuredly on the side of "continuity," although I cannot hold that the subordinate question is closed—*i.e.*, as to whether such continuity can never, under any circumstances or in any degrees, be interrupted. GEORGE J. ROMANES.

Hyères, November 20.

Correlation of Solar and Magnetic Phenomena.

MR. ELLIS, in his letter (NATURE, November 9), has discussed the coincidence between Carrington's observation of a solar outburst in 1859 and the magnetic movements observed at Kew and Greenwich. He comes to the conclusion that the disturbance of the magnets corresponding to this outburst was small, and that, although many greater magnetic movements have occurred since, no corresponding manifestation has been seen, although the sun has been so closely watched.

He appears to have overlooked an observation made at Sherman, by Prof. Young, which shows a very striking *series* of coincidences, and which is described in his work, "The Sun" (p. 156), in the following words:—"On August 3, 1872, the chromosphere in the neighbourhood of a sun-spot, which was just coming into view around the edge of the sun, was greatly disturbed on several occasions during the forenoon. Jets of luminous matter of intense brilliance were projected, and the dark lines of the spectrum were reversed by hundreds for a few minutes at a time. There were three especially notable paroxysms at 8.45, 10.30, and 11.50 a.m., local time. At dinner the photographer of the party, who was making our magnetic observations, told me, before knowing anything about what I had been observing, that he had been obliged to give up work, his magnet having swung clear off the scale. Two days later the spot had come round the edge of the limb. On the morning of August 5, I began observations at 6.40, and for about an hour witnessed some of the most remarkable phenomena I have ever seen. The hydrogen lines, with many others, were brilliantly reversed in the spectrum of the nucleus, and at one point in the penumbra the *C* line sent out what looked like a blowpipe jet, projecting toward the upper end of the spectrum, and indicating a motion along the line of sight of about 120 miles per second. The motion would die out and be renewed again at intervals of a minute or two. . . . . The disturbance ceased before eight o'clock, and was not renewed that forenoon. On writing to England, I received from Greenwich and Stonyhurst, through the kindness of Sir G. B. Airy and Rev. S. J. Perry, copies of the photographic magnetic records for those two days. . . . . On August 3, which was a day of general magnetic disturbance, the paroxysms I noticed at Sherman were accompanied by peculiar twitches of the magnet in England. Again, August 5 was a quiet day, magnetically speaking, but just during that hour, when the sun-spot was active, the magnet shivered and trembled. So far as appears, too, the magnetic action of the sun was instantaneous. After making allowance for longitude, the magnetic disturbance in England was strictly simultaneous, so far as can be judged, with the spectroscopic disturbance seen on the Rocky Mountains."

These observations of Prof. Young's seem to invalidate Mr. Ellis's statement that "no second occurrence similar to that of 1859 has come to light," and that although there undoubtedly exists a relation between sun-spots and magnetism, "it has not yet been found possible to trace direct correspondence in details."

Cambridge, November 12. A. R. HINKS.

THE circumstances spoken of by Prof. Young, as alluded to in the accompanying letter, tell of special solar activity at the time of magnetic disturbance, observed solar paroxysms occurring apparently in correspondence with magnetic movements; but the question whether definite connection exists, is the really critical point, as in the Carrington observation of 1859. Prof. Young himself says ("The Sun," p. 159):—"*So far as appears*, the magnetic action of the sun was instantaneous. After making allowance for longitude, the magnetic disturbance in England was strictly simultaneous, *so far as can be judged*, with the spectroscopic disturbance seen on the Rocky Mountains." (The italics are mine.) Without being over-critical, it may be remarked that the terms "instantaneous" and "strictly simultaneous" are somewhat strong, in the circumstances of the case.

Feeling that too much importance had been by various writers attached to the Carrington observation, I may have been led to the expression of a too pronounced opinion thereon. Rather it might be said that direct connection is not proved. It is to be remembered that the cases of recorded occurrence together of solar and magnetic phenomena are few, whilst solar change (such as is sometimes actually observed, or as is remarked in the changed solar appearance from day to day) without magnetic action, and very frequently magnetic action without recorded solar change, both occur in greater degree than, on the supposition of direct connection between the two classes of phenomena, would be expected. Prof. Young, indeed, further says:—"No two or three coincidences such as have been adduced are sufficient to establish the doctrine of the sun's immediate magnetic action upon the earth, but they make it so far probable as to warrant a careful investigation of the matter—an investigation, however, which is not easy, since it implies a practically continuous watch of the solar surface." One main difficulty is here pointed out. Continuous magnetic registration is easily maintained, but how far the observation of solar change is adequate (in spite of the numbers of observers) for the purposes of such an inquiry is possibly somewhat doubtful. The problem of a sufficiently comprehensive and satisfactory comparison of the irregularities in solar and magnetic changes is evidently one of very considerable difficulty.

Greenwich, November 14. WILLIAM ELLIS.

Artifical Amœbæ and Protoplasm.

I REVIEWED in NATURE, No. 1251, Prof. Bütschli's recently published work "Mikroskopische Schäume und das Protoplasma." The book is distinctly polemical, and on pages 5 and 6 the author refers to his own, and his colleague Prof. Quincke's work, and states his indebtedness to the latter's investigation upon physical emulsions, but accuses him of having adopted his own view as to the structure of protoplasm, and that without acknowledgment.

"Ich habe Herrn Collegen Quincke, bevor er seine Hypothese der Plasmabewegungen veröffentlichte, mehrfach meine Ansicht ueber die wahrscheinliche Structur dieser Substanz gesprächsweise mitgetheilt und betont, dass gewisse Eigenschaften des Plasmas wohl mit dieser Bau direct zusammenhängen dürften. Quincke hat in seiner Mittheilung von 1888 das Plasma noch als einfache Flüssigkeit behandelt, von einer Schaumstructur desselben nirgends gesprochen; wenn er später (1889), nach Veröffentlichung meines ersten Berichtes (1889) die Schaumstructur betont, so kann ich darin nur den Einfluss meiner Erfahrungen erkennen, auch wenn er derselben in dieser Publication, welche über das Plasma und seine Bewegungserscheinungen handelt, nirgends gedenkt."

(*Trans.*)—In the course of conversation, and before he published his hypothesis of protoplasmic movement, I frequently mentioned my view as to the probable structure of this substance to my colleague Quincke, and I emphasised the probability of a direct relation between certain properties of the plasma and this structure. In his note of 1888 Quincke still treated the plasma as a simple fluid, and nowhere made mention of the foam-like structure. When, later on, in 1889, after the publication of my first report, he emphasises the foam structure, I cannot but recognise the influence of my own experiences, though he makes no mention of them in this publication, which treats of the plasma and of the phenomena of its movement.

In NATURE, No. 1253, a letter appeared from Prof. Quincke, stating that he "was the first to point to the foamy nature of protoplasm, which was later on further investigated by Prof. Bütschli."

Prof. Quincke is evidently annoyed that his prior claim to the discovery, if discovery it be, was not made clear by me in the review. But my duty as a reviewer was with Prof. Bütschli, whose views as to the foamy nature of protoplasm I sketched to the best of my ability, and I ventured to criticise them adversely. If Prof. Bütschli was not the first to describe the foamy nature of protoplasm, and if he was anticipated by Prof. Quincke, then it is the latter's duty, not mine, to make this clear. I could not possibly be expected to deal with such a controversy in a review, for such an extended historical inquiry as this would imply, would hardly have found acceptance.

As Prof. Bütschli distinctly states that before 1889 Prof. Quincke looked upon protoplasm as a simple fluid, the latter, in order to establish his position, has only to send definite quotations from one of his publications prior to this date, in which it is clear that the foamy nature of protoplasm was described by him.

I scarcely think that Prof. Quincke can himself have read my review, for had he done so he would hardly have accused me of slighting his well-known and valued scientific work. Prof. Quincke charges me with calling "his investigations" "toys for the physicist." I never referred to him at all in this connection, but spoke definitely of the preparations of foam as manufactured by Prof. Bütschli. I moreover would point out to Prof. Quincke that we cannot compare an "investigation" with a "toy," for one is an *action*, the other a *thing*.

I regret exceedingly that the "Q" in Prof. Quincke's name appeared as "N," and take to myself the sole responsibility. I write the capital "Q" not unlike an "N," and omitted to notice the mistake in the proofs. JOHN BERRY HAYCRAFT.

Physiological Laboratory, University College, Cardiff.

THE ROYAL SOCIETY CLUB.

THERE are not many social institutions which can point to an antiquity of a century and a half, and this is what the Royal Society Club was able to celebrate on Thursday, the 16th instant.

The club is almost, if not quite, the oldest club in existence. The Dilettanti Society, which was founded a year earlier, in 1742, is not a club, and has, from the first, imposed a fine on any of its members who should apply that designation to it.

The Royal Society Club was formally inaugurated on October 27, 1743, but its very act of inauguration recognises the existence of a still earlier body. This "Memorandum of Association" is headed as follows: "Rules and Orders to be Observed by the Thursday's Club, called the Royal Philosophers."

We hear of the Virtuoso's Club, meeting on Thursdays, among the clubs of London in 1709, and in the year 1742 the club was described by Hutton as "Dr. Halley's Club." It is possible that the inaugural meeting of October 27, 1743, may have been the reorganisation of the club after Dr. Halley's death in the previous year.

The title of "Royal Philosophers" lasted till 1786, when the dinner bills were charged to "the Royals." The full title Royal Society Club was adopted later.

The history of the club was drawn up in 1860 by Admiral W. H. Smyth, and privately printed, under the title of the "Rise and Progress of the Royal Society Club." Many interesting particulars may be gathered from this compilation.

At the very first, Fellowship of the Society was not a necessary condition of membership of the club, as it now is. Mr. Colebrooke, who was treasurer of the club in 1743, was not elected into the Royal Society till 1755.

The meetings were at first held at the Mitre Tavern in Fleet Street, for forty years from 1743. The club then moved to the "Crown and Anchor" in the Strand, where it remained until 1848, when it went to the Freemasons' Tavern. On the removal of the Society to Burlington House in 1857, the club followed it westwards to the Thatched House Tavern, and subsequently to Willis's Rooms. On the final closing of the last-named establishment, in 1889, the club migrated to Limmer's Hotel, where it now meets.

As the club grew older, the price of its dinners grew with it, from "one shilling and sixpence, for eating," in 1743, to ten shillings in 1843, at which latter price it has remained ever since. The time of dinner has also changed first from 1 o'clock to 2, and then successively to 3, 4, 4½, 5, 5½, 6, and 6½, the time of serving now.

The bill of fare for the commemorative dinner last Thursday was copied, spelling and all, from the earliest *menu* preserved, that of March 28, 1748, and the price to the members was 1s. 6d., the same as in the earliest days of the club.

The bill of fare was as follows:—

Two dishes Fresh Salmon, Lobster Sauce.
Cod's Head.
Pidgeon Pye.
Calve's Head.
Bacon and Greens.
Fillett of Veal.
Chine of Pork.
Plumb Pudding.
Apple Custard.
Butter and Cheese.

The members are indebted to the managers of Limmer's Hotel for the readiness with which they entered into the project of reproducing a dinner on the ancient model.

As the month was November, salmon was not to be had, so that other fish was substituted. An important addition was made to the *menu*, for a haunch of venison was presented to the club by one of its members.

In early days whole bucks, haunches of venison, turtles, and barons of beef were not unfrequently presented, the donors being in each case elected honorary members for the then current session.

These contributions became rather inconvenient, and on July 29, 1779, it was "resolved that no person in future be admitted a member of this Society in consequence of any present he shall make to it."

The club consists of fifty ordinary members, and this number is increased by *ex officio* members (present or past office-bearers in the Royal Society) and by a few honorary (octogenarian) and supernumerary members, until the total in 1893 has reached sixty-one. Of these forty-four were present on the 16th, with twenty-three guests, making a total of sixty-seven.

From the earliest times each member of the club has had the privilege of bringing one guest with him, the President for the day being not limited to one. This practice of bringing guests has been generally carried out, and a study of the list of visitors given in Admiral Smyth's "History" shows that many of the leaders of European science have at various times entered their names in the club records. Berzelius, Cuvier, Gay-Lussac, Linnæus, and Volta were guests of which any club may justly be proud.

We may also fairly assert, in conclusion, that since the middle of the last century, there are but few names really prominent in British science which do not appear in the list of ordinary members of the Royal Society Club at some time of its existence.

THE DE MORGAN MEDAL.[1]

THE duty has this year devolved upon the Council of making the fourth triennial award of the medal which was instituted in memory of our first President, the distinguished logician and mathematician, Augustus De Morgan. In making their award, the Council are not restricted in their choice to mathematicians of this country, or to the recognition of excellence in any particular branch of mathematical science. It will scarcely, however, be imputed to them that they have been influenced by feelings of patriotism rather than by scientific impartiality in having selected as the first three recipients of the medal, Prof. Cayley, Prof. Sylvester, and Lord Rayleigh. The position of those eminent mathematicians suffers no depreciation, if our survey is extended beyond the borders of our own country. On the other hand we shall, I think, be equally exempt from adverse criticism in the choice we have this year made of Felix Klein, Professor of Mathematics in the University of Göttingen, as the next recipient of the honour which we are privileged to confer.

Prof. Klein, who has for many years been enrolled in our books as an honorary member of our Society, has attained the highest distinction as a mathematician. In estimating the value of his work, a mere consideration of the advance due to him in our knowledge of the details of special subjects would be sufficient to place him in the first rank; the wide influence of his work must be apparent to anyone who studies the memoirs of writers, of whatever country, on those subjects to which he has set his hand. Let me in particular refer to his contributions to the geometry of complexes, and to non-Euclidean geometry, to his memoirs on the theory of equations, on the transformation of elliptic functions, on the general theory of functions, especially in exposition and development of Riemann's theory, to his discussion of Riemann's surfaces, and, in more recent times, his researches on Abelian and Hyperelliptic functions, to his treatment of the polyhedral functions, automorphic functions, and of the elliptic Modular functions, the last of which is expounded in the treatise by Fricke on the subject. One must not forget to record the fact that his important memoir on the transformation of elliptic functions in the *Mathematische Annalen*, vol. xiv., was preceded by a communication made to our Society in 1878; Prof. Klein thus doing us the honour of indicating in advance the principal results he had obtained (*Proceedings*, vol ix. p. 23).

But, in the necessarily brief remarks to which I must limit myself this evening, to indicate Prof. Klein's claims to distinction by dwelling upon individual subjects which he has treated, would, I think, be wanting in perspective and proportion. Great as is the reputation which he has acquired in connection with particular branches of mathematical research, that which would seem to be his especial merit is the comprehensiveness of his view, and the uniformity of his treatment. For him the study of one of his special subjects is the study of all; the binding influence b ing the theory of discrete groups, a theory he has made his own. With this unity of conception he combines a great power of simple, elegant, and interesting expression. The expositions of his method contained in his early "Comparative Review of Recent Researches in Geometry," and his more recent "Lectures on the Icosahedron," in which the formal identity of investigations apparently the most diverse is made apparent, belong to the romance of mathematics. The important influence which his mode of investigation has had and is destined to have on the progress of the higher mathematics, the encouragement of largeness of view, rather than the elaboration of minutiæ, and the stimulating influence he exercises upon pupils who now hold positions of eminence in Germany, must take a foremost place among the grounds upon which we honour Prof. Felix Klein to-day by the award to him of the De Morgan Medal.

[1] Address to the London Mathematical Society, on the occasion of the presentation of the De Morgan Medal, November 10, 1893, by the President, A. B. Kempe, F.R.S.

NOTES.

THE agricultural exhibit of Sir John Lawes and Sir Henry Gilbert at Chicago appears to have been much appreciated by our American cousins. The Association of American Agricultural Colleges and Experiment Stations have passed a special resolution expressing the value they attach to the exhibit, and the Director-General of the Exposition has forwarded the same to England, with the added thanks of the Exposition, for "the great benefit done to American agriculture by this excellent and instructive exhibit."

A PASTEUR Institute has been opened in New York, with Dr. Paul Gibier as its director.

M. O. CALLANDREAU, of the Paris Observatory, has been appointed Professor of Astronomy at the École Polytechnique.

DR. TREADWELL has been appointed Professor of Analytical Chemistry in the University of Zurich.

DR. A. K. E. BALDAMUS, known for his work in connection with ornithology, died on October 30, at the age of eighty-two.

MR. J. BAILEY DENTON, whose name has long been known to agriculturists and civil engineers, died on November 19, at Stevenage, Herts, in his seventy-ninth year.

WE regret to announce the death, at the age of eighty-one, of M. Chambrelent, a member of the Rural Economy Section of the Paris Academy of Sciences.

MR. WILLIAM DINNING, of Newcastle, a lover of natural science and a promoter of its interests, died on November 13. Shortly before his death, he offered his collection of fossils from the coal measures to the Newcastle Natural History Society, on the condition that the society would provide cases to properly exhibit it and the collection already existing in the local museum. The society was without the necessary means, but Lord Armstrong has promised to contribute a sum of £1500 for this purpose. Mr. Dinning was an engineer by profession, but all his leisure was devoted to scientific pursuits. His death will be greatly felt in local circles.

IT is reported that a severe shock of earthquake was experienced on November 17 in Kashan, Western Asia, a large part of the town being destroyed. Great damage was also done at Samarcand.

MR. LLOYD BOZWARD, Worcester, informs us that on November 17 a fine shower of Leonid meteors was seen throughout the night. The meteors are said to have been so numerous that several persons unacquainted with their nature mistook the display for an exhibition of fireworks.

THE Swiney prize of a cup, value £100, and money to the same amount, to the author of the best published work on jurisprudence, will be awarded by the Society of Arts and the College of Physicians in January next. The prize is awarded every fifth year, the recipient in 1889 being Dr. C. Meymott Tidy, for his work entitled "Legal Medicine."

AT the beginning of next year the first number of an "Index der gesamten chemischen Litteratur" will be published by H. Bechhold, Frankfort. The index will appear monthly, and after the end of each year an index comprising all the papers published during the year in pure and applied chemistry will be issued. The editor of the forthcoming publication is Dr. Julius Ephraim.

MESSRS. W. H. ALLEN AND CO. have in preparation a series of volumes founded upon Jardine's Naturalist's Library. The editor of the series, Dr. Bowdler Sharpe, will undertake several of the ornithological volumes. The authors of other sections are Mr. R. Lydekker (Mammalia), Mr. H. O. Forbes (Mammalia and Birds), Mr. W. R. Ogilvie Grant (Birds), Mr. W. F. Kirby (Insects), Prof. R. H. Traquair, F.R.S. (Fishes), The first volumes will be issued early in 1894, and will consist of British Birds, vol. i., by Dr. R. Bowdler Sharpe; Monkeys, by H. O. Forbes; and Butterflies (with special reference to British species), by W. F. Kirby.

WE have previously referred to the fact that on the first of this month Italy adopted the time of Central Europe. All the Italian time-tables have, by order of the Minister of Public Works, been printed with the hours marked up to twenty-four from midnight to midnight. The railway clocks have also been modified, and the hours from 13 to 24 printed in red Arabic characters in a circle interior to the old one. It may be well to remember that at the Paris Exhibition in 1867, Sig. G. Jervis, the Keeper of the Royal Industrial Museum of Turin, exhibited a clock face having a double series of hours, the higher numbers being placed on the exterior circle on account of the greater space there available. He also exhibited a time-table drawn up on the 24-hour plan, and possessing many advantages over those in use even at the present time. Mr. Jervis has thus had the satisfaction of seeing the adoption of the improved clock-dial and the 24-hour time-table, proposed by him nearly a third of a century ago.

DURING the past week this country has experienced some of the most destructive, if not the most violent storms that have occurred for some years. The reports received by the Meteorological Office on Thursday morning, the 16th instant, showed that a deep disturbance was approaching our shores, and storm signals were hoisted on our coasts. On the afternoon of that day the storm broke with great violence over the west of Ireland, the direction of the wind being south-easterly with a moderately high temperature, while the barometer was below 29.5 inches. The centre of the storm passed across England and Scotland, taking the ordinary north-easterly course, and by 6 p.m. on Friday it lay off the extreme north-east coast of Scotland, and the barometer had fallen to 28·5 inches; the wind, as usual in the rear of storms, shifted to the north-westward, causing a sudden fall of temperature with heavy snow and hail in many places. At this point the track of the disturbance took a very unusual direction, and during Friday night the centre moved quickly to the south-eastward down the North Sea, and at 8 a.m. on Saturday, the 18th instant, the centre lay off the north-east coast of England, while the pressure rose rapidly over the western portion of the kingdom, causing steeper gradients and bitter northerly and north-easterly winds. The storm first broke over London and the southern parts of the country on Saturday afternoon, and blew in terrific squalls during the whole of Saturday night, the centre again resuming an easterly course across the North Sea to the Dutch and North German coasts. The greatest strength of the wind appears to have been experienced near Holyhead, where the force on Saturday morning was reported as 12 of the Beaufort scale, while force 11 was reported from Wick and Scilly. In the neighbourhood of London the heaviest gusts were experienced in the early part of Saturday evening; the anemometer at Greenwich recorded a pressure of 17 pounds on the square foot at about 6 p.m. On Sunday and Monday the wind force was still high in the south-east of England, as well as in the English Channel, and a very high sea was running on our coasts. The storms were also very violent on the other side of the Channel, and were accompanied by heavy falls of snow in many parts of the Continent.

THE Pilot Chart of the North Atlantic Ocean shows that the first half of October was marked by much bad weather north of latitude 45° between Newfoundland and the British Isles. One of the storms caused immense loss of life and property in Louisiana, owing to a tidal wave encroaching over the low-lying lands. A supplement issued with the chart shows clearly the actual weather conditions between the 23rd and 28th of

August last, west of longitude 50° W. During this period two severe storms occurred; the first struck the coast in the vicinity of New York, where much damage was done to shipping, the second struck the coast near Savannah, and occasioned frightful loss of life along the coasts of South Carolina and Georgia, the barometer falling to 28·29 inches at 6 a.m. on August 28. This was the storm referred to in our issues of 31st August and 7th September.

THE annual meeting of the Royal Geological Society of Cornwall was held at Penzance on November 10, when the president, Mr. Howard Fox, delivered an address, in which he reviewed the past history of the society. In the course of his remarks, he said that the rocks of West Cornwall had been subjected to precisely the same conditions as those of Moffat and Girvan, in South Scotland, described by Prof. Lapworth. They show, in fact, a repetition of the same phenomena, except that as yet no band of fossiliferous rock characteristic of a special geological zone has been discovered as a horizon of reference. It is, therefore, worth consideration whether the radiolarian cherts of Mullion Island, described in the *Quarterly Journal of the Geological Society* for this year, will not answer the same purpose. The Mullion Island cherts consist of easily-recognised bands of mostly black flint-like rock, generally reticulated with thin but conspicuous white quartz veins. They are extremely hard and resistant of both atmospheric and subterranean agents of destruction. They are of sufficient thickness to form a distinctly marked feature in the ascending sequence, and having been originally deposited on the floor of a deep ocean as radiolarian ooze, they necessarily occupy a wide horizontal extent of country. They occur in distinct bands, mostly in shales or crushed dark slates; they break with a conchoidal fracture, and when sheared or impure the microscope can generally determine their nature. The fossils are radiolarian deep-sea forms like those of the present day. Messrs. Teall and Lapworth have traced these cherts with Mr. Fox for 800 yards in the cliffs and on the foreshore north of Porthallow in-Meneage, and during the past summer they have been traced at intervals through the parishes of Veryan, Gorran, and Caerhayes. Pebbles have been found on the north coast, which under the microscope show radiolaria with their structure still visible, but the parent rock has not yet been found *in situ*. It will therefore be agreed that these cherts most certainly should be traced wherever they appear in Cornwall. Their age is undoubtedly Ordovician, yet the precise zone to which they belong can only be determined by discovering some typical fossils in the shales and slates associated with them. In South Scotland officers of the geological survey have recently traced such cherts with radiolaria from sea to sea just beneath the Llandeilo rocks, fixing horizons exactly. The cherts in Cornwall, possibly of the same age, and certainly of the same character, are equally promising in the midst of the entangled rocks around, to form the datum line, or clue to the succession.

A COLLECTION of land and marine shells of the Galapago Islands was made during the voyage of the U.S. Fish Commission steamer *Albatross* in 1887-88. A report on this mollusc fauna, prepared by Dr. R. E. C. Stearns, has recently been issued from the U.S. National Museum. It is not an exhaustive review of the collection, but includes the principal collections previously made, and also a few notes of interest. The extreme tenacity of life of land snails in every stage of growth is well known. Dr. Stearns gives the following instances that came under his own observation:—"In December, 1865, the Stearns collection, now in the National Museum, was enriched by the acquisition of several examples of *Helix Veatchii*, Newcomb, now regarded as a variety of *H. areolata*, that were collected by Dr. Veatch on Cerros or Cedros Island off the coast of Lower California in 1859. The specimens were given by Dr. Veatch to Thomas Bridges, and upon the death of the latter came into my possession with the remainder of the Bridges shells. One day, upon a careful examination, I discovered that one of the specimens was apparently still alive, and placed it in a box of moist earth; after a while it protruded its body from the shell and commenced moving about, and seemed to be no worse for its long fast of at least *six years*. *H. Veatchii*, it will be observed, beat the time of the famous British Museum example of *H. desertorum*, which lived without food within a few days of *four years*. In March, 1873, Prof. George Davidson, of the United States Coast Survey, while at San José del Cabo, Lower California, collected a number of specimens of *Bulimus pallidior*, and subsequently gave me a part of them, which I put in a box, where they remained undisturbed until June 23, 1875, when they were placed in a glass jar with some chick-weed and a small quantity of tepid water. They soon woke up and began to move about apparently as vigorous as ever after their long nap of *two years, two months, and sixteen days*."

THE delicacy of the sense of taste among Indians has been tested by Mr. E. H. S. Bailey, and the results compared with those obtained from whites (*Kansas University Quarterly*). The method of testing was by solutions of different strengths, the substances quinine sulphate (bitter), sulphuric acid (sour), bicarbonate of soda (alkaline), cane sugar (sweet), and common salt (salt) being selected as representing classes of the common familiar substances most likely to be recognised. The only one of these that experience has shown is not familiar is the alkaline taste. From an examination of the results it appears that the order of delicacy is about the same for the two races. By this it is meant that the smallest proportion of quinine was detected; acid solutions come next in the order of action upon the organ of taste, and then salt. In the case of whites, sweet solutions were more detectable than alkaline ones, but the reverse was the case with the Indians. This does not count for much, however, as the Indians had great difficulty in distinguishing between the alkaline and salt solutions. As might have been expected, the ability to detect the different substances when they are in very dilute solution is less in the Indians than in the whites. The males of both races are able to detect a smaller quantity of salt than the females, but in all other cases the females appear to have the more delicate organ of taste.

THE question as to whether gases are capable of emitting heat has been investigated by many physicists, most of whom have come to the conclusion that the characteristic spectra of glowing gases are chiefly, if not solely, due to some chemical action going on within the gas. Hittorf heated air in platinum tubes a few centimetres long over a Bunsen, and Siemens treated air, carbonic acid, and steam in a similar manner, using longer tubes and higher temperatures. Both were unable to discover any radiation by the gases when thus simply heated, and Pringsheim, after more recent work, came to the conclusion that emission spectra cannot be obtained by simple heating. Mr. F. Paschen has, however, quiet recently (*Wied. Ann.* No. 11) succeeded in discovering and mapping such spectra by a modification of Tyndall's experiment with gases rising from an incandescent body, substituting the bolometer for Tyndall's thermopile. The hot body employed was a spiral band of platinum forming a narrow tube, which was heated by passing an electric current through the platinum. The gases were sent through this spiral, and thus acquired a temperature of about 1000° C. The temperature was measured by means of a platinum : platinum-rhodium thermocouple with an excessively small junction. The spectrum was formed by a fluorspar prism and two concave mirrors, adjustable automatically to minimum deviation for any wave-length. The gases examined were air, oxygen, carbon dioxide, and steam. Only the last two gave positive results, some small deflection in the first two being due to slight traces of moisture. Carbonic acid showed a very sharp maximum far in the infra-red. The bolometer strip was too broad to determine whether it was a line or a band, but it is most probably a somewhat ill-defined line. Steam showed about eight maxima, which must be described as bands. A comparison of the spectrum of the Bunsen flame with that of the hot products of combustion arising from it showed that the spectra obtained are similar and of almost equal intensity; so that it is very probable that the spectra of hot gases are chiefly due to temperature, and not to chemical action. A curious and hitherto unexplained observation is that of a slight shifting of the maxima towards the more refrangible end of the spectrum when the temperature is lowered.

To determine correctly the values of the critical constants of a substance is a matter of considerable difficulty: the estimation of the critical temperature, however, is generally believed to be both expeditious and accurate. The usual method of taking an observation of the critical temperature consists in heating the liquid in contact with its saturated vapour in a closed space to a temperature above the critical temperature, allowing the substance to cool, and noting the temperature t_c at which the meniscus of the liquid just appears in the misty contents of the closed space. In the current number of *Wiedemann's Annalen*, Herr Galitzini gives evidence to show that t_c thus determined, although it is independent of the amount of substance contained in the closed space, is lower (it may be considerably lower) than the true critical temperature, or the temperature at which liquid and saturated vapour have the same density. By very slow and regular cooling he also finds that the peculiar misty appearances which are usually held to be invariably associated with the critical state are not observed. Amongst other conclusions, his experiments, which it is to be noted were made on ether, lead him to believe that at temperatures even considerably higher than the critical temperature a substance at constant pressure may have different densities, different in some cases to the extent of 25 per cent. This last result the author attributes to the presence of molecular complexes in the substance: if its validity is established, it will of course overthrow the generally accepted idea that at any temperature above the critical temperature for a given value of the pressure there is only one value of the volume.

AN interesting paper on the various electric wave systems obtained by Lecher's method has been communicated to the R. Accademia della Scienza di Torino by Signor Mazotto. The effect of varying the lengths of the primary and secondary wires, and the distance apart of the plates of the condensers, has been studied. As an indicator of the points of maximum difference of potential along the wires, the author uses two short wires partly coiled round india-rubber tubes, which slide along the secondary wires, the ends of the wires being brought to within about 2 cm. of each other. If, when the apparatus is working in the dark, the fingers are brought near the platinum tips of these wires, two small luminous stars appear at these tips when the bridge over the secondary is in the vicinity of a node. When at a node these sparks become very conspicuous, even without the presence of the fingers. This indicator is said to have the advantage over the Geissler tube used by Lecher, that it shows more distinctly the maxima and minima, is less fatiguing to the eyes, and less capricious in its action. The number of nodal systems formed when, the primary system being kept constant, the bridge on the secondary wires is moved to different parts, were found to be more numerous than Lecher's observations would seem to indicate. The "harmonics" of the fundamental system were not the only higher systems that were produced, it being found possible, by altering the position of the bridges to produce any system intermediate between two harmonics It was also found, when the second bridge was placed at a fixed point, that the nodal systems obtained by moving the first bridge were independent, in position and intensity, of the state of the system beyond the second bridge. The wave-lengths obtained experimentally were compared with those given by the formula of Salvioni, and found to agree fairly well. The curves obtained by the author, and a full account of his method, has been published in the *Electrician*, vol. xxxii. p. 60.

THE following translation of a reply given by Prof. Galileo Ferraris to a young lady who asked what electricity was, is given by the *Electrician*. Prof. Ferraris has conferred a great benefit on all those who are supposed to have any knowledge of that magic science electricity, and are therefore continually being asked this question, though whether the reply satisfied the questioner is rather doubtful. His reply was:—"Maxwell has demonstrated that luminous vibrations can be nothing else than periodic variations of electro-magnetic forces; Hertz, in proving by experiments that electro-magnetic oscillations are propagated like light, has given an experimental basis to the theory of Maxwell. This gave birth to the idea that the luminiferous ether and the seat of electric and magnetic forces are one and the same thing. This being established, I can now, my dear young lady, reply to the question that you put to me: What is electricity? It is not only the formidable agent which now and then shatters and tears the atmosphere, terrifying you with the crash of its thunder, but it is also the life-giving agent which sends from heaven to earth, with light and heat, the magic of colours and the breath of life. It is that which makes your heart beat to the palpitations of the outside world, it is that which has the power to transmit to your soul the enchantment of a look and the grace of a smile."

THE current *Comptes Rendus* contains an important correction to the numbers given by M. Blondlot for the velocity of an electric disturbance in high conductivity copper (*Comptes Rendus*, October 23). The values were expressed in kilometres per second instead of thousands of kilometres per second, so each of the velocities given in our issue of November 9 (p. 37) must be increased by one thousand, in order to be correct.

A VERY important paper, by Dr. Uschinsky, on the cultivation of pathogenic bacteria in media, devoid of all albuminoids, appears in the *Archives de médecine expérimentale*, No. 3, 1893. Pathogenic organisms thus grown do not lose their virulent properties and, moreover, elaborate toxic substances, for on passing the media in which they have been cultivated through a Chamberland filter, the filtrate was found to be toxic. In a more recent paper, published in the *Centralblatt für Bakteriologie*, vol. xiv. No. 10, 1893, Dr. Uschinsky states that in order to obtain more satisfactory growths of the bacteria in question, he has introduced some modifications into the composition of the culture medium, which now affords as suitable a pabulum for their cultivation as ordinary bouillon. The following is the composition of this non-albuminous medium:—Water, 1000; glycerine, 30–40; sodium chloride, 5–7; calcium chloride, 0·1; magnesium sulphate, 0·2–0·4; dipotassium phosphate, 2–2·5; ammonium lactate, 6–7; sodium aspartate, 3·4. The organisms of cholera, diphtheria, tetanus, typhoid and others have all been grown successfully in the above. The poisonous substances elaborated by bacteria are, therefore, not necessarily due to their decomposition of the albumen contained in the ordinary culture media employed, but must rather be regarded as the result of synthesis; the materials produced, says Dr.

Uschinsky, belonging in all probability to the proteid bodies, and bearing much resemblance to ferments.

DR. USCHINSKY has made a special study of the tetanus bacillus when grown in this medium, and has examined in some detail the nature of the toxic products thus elaborated. A more satisfactory growth of this organism was procured by adding from one to two per cent. of grape-sugar to the solution, and the anaërobic conditions necessary for its cultivation were obtained by pouring liquid paraffin on the surface. The filtrate of such tetanus cultures was about equal in virulence to that derived from ordinary broth-cultivations of the bacillus. On the other hand, the poisonous properties of the former were far more easily removed than was the case with the broth-cultures, being destroyed by precipitation with alcohol, and also frequently by evaporation *in vacuo* at 33-36° C., this being especially the case when the latter was carried out in the presence of light. By addition of strong alcohol a precipitate was obtained, in which, besides salt, small quantities of albuminous bodies were present, as indicated by Millon's reagent and the xanthoproteic reaction. This precipitate was, however, without any toxic properties.

THE second edition has been issued of a general guide to the Manchester Museum, by Mr. W. G. Hoyle, Keeper of the Museum. The book should be useful in directing attention to the most important specimens, and explaining their character. Its value would be greatly increased, however, by the addition of an index.

"A BIBLIOGRAPHY of Vertebrate Embryology," by Mr. C. S. Minot, has been published by the Boston Society of Natural History. The titles are grouped into subjects, and the subjects are alphabetically arranged, so there is no difficulty in finding the original source of any paper, the title of which is known. An index of authors is also given to facilitate reference. Biological investigators will find the bibliography of great assistance.

THE volume of selections from the philosophical and poetical works of Miss Constance C. W. Naden, compiled by the Misses E. and E. Hughes, and published by Messrs. Bickers and Son, is one of the daintiest that we have seen for some time. The selections from her essay on induction and deduction contain some remarkably fine expressions, and many other parts of the book are of great interest.

MR. P. ANDERSON GRAHAM'S "All the Year with Nature" (Smith, Elder, and Co.) contains a number of reprints of articles originally contributed to various magazines. The author has a chatty style, and his heterogeneous collection will serve to while away an hour or two. The connection of many of the articles with the seasons is not very apparent, and some of the statements are not scientifically accurate, but, taken as a whole, the book is well worth reading.

PART I. of the sixth edition of Prof. Michael Foster's well-known "Text-Book of Physiology" has been published by Messrs. Macmillan and Co. It comprises the first book of the original volume, and deals with the blood, the tissues of movement, and the vascular mechanism. A number of important modifications have been made in the section devoted to the phenomena and mechanism of the heart-beat, but with this exception few changes have been introduced. Since the publication of the first edition, seventeen years ago, Prof. Foster's work has been recognised to be the best of its kind, and the issue of a new edition shows that it retains its position as a physiological "classic."

THE fourth volume of Alembic Club Reprints, published by Mr. W. F. Clay, Edinburgh, is before us. It deals with the foundations of the molecular theory of gases, and comprises papers and extracts of papers by Dalton, Gay-Lussac, and Avogadro. There is no better way of studying the development of an idea than by reading such reprints as those issued under the auspices of the Alembic Club, for they enable the student to see the many difficulties that have to be overcome before a theory crystallises into shape. We therefore welcome this last addition to an excellent series of books.

THE Cambridge University Press has published the first volume of the series of manuals of biological science edited by Mr. Arthur E. Shipley. The book to which we refer is "Elementary Palæontology," by Mr. Henry Woods. In it the author gives a concise account of invertebrate palæontology, chiefly considered from a stratigraphical point of view. The plan of the book is excellent, the zoological features of each group being first described, then the genera of importance geologically are classified, and with this knowledge the student is able to understand the following section dealing with the distribution of the group. Instead of giving archaic illustrations of genera, Mr. Woods includes figures required to explain structure and terminology. The student will benefit by this change. Remarkable pictures of perfectly preserved fossils may suit the popular mind, but the student must study fossils in collections, and he needs more detailed instruction concerning their characteristics. As an introduction to the study of palæontology, Mr. Woods' book is worthy of high praise.

NOTES from the Marine Biological Station, Plymouth.—The past week has been one of the stormiest of the year. Work outside the Sound was quite impossible in our small boats, and even within the harbour was attended with difficulties. The captures included specimens of the Actinian *Cylista viduata*, and of the Dorididæ *Platydoris planata*, *Ægirus punctilucens* and the scarlet *Rostanga coccinea* upon the red incrusting sponge on which it feeds. From several hauls of *Antedon rosacea* a small number of the Polychæte commensal *Myzostomum* were obtained. The dogfish in the aquarium (*Scyllium catulus* and *canicula*) have begun to breed.

THE additions to the Zoological Society's Gardens during the past week include a Mozambique Monkey (*Cercopithecus pygerythrus*, ♂) from East Africa, presented by Mr. Bayes; a Rhesus Monkey (*Macacus rhesus*, ♀) from India, presented by Mr. C. E. Morres; two —— Jerboas (*Alectaga jaculus*) from Persia, presented by Capt. R. A. Ogilby, F.Z.S.; a Tuatera Lizard (*Sphenodon punctatus*) from New Zealand, presented by Mr. Chas. Smith; a Lion (*Felis leo*, ♀) from West Africa, an American Bison (*Bison americanus*, ♀) from North America, deposited; a Cunning Bassaris (*Bassaris astuta*) from Mexico, purchased.

OUR ASTRONOMICAL COLUMN.

MECHANICAL THEORY OF COMETS. — Prof. J. M. Schaeberle, in the *Astronomical Journal*, No. 306, communicates a "preliminary note on a mechanical theory of comets," this being "a strictly logical consequence of the mechanical theory of the corona." The principles which serve as a basis may be said very briefly to be the following. Any given solar eruption gives rise to both prominences and streams. The ejective force being the same, the mass of a given volume of coronal stream is less than that of a prominence. Assuming mean density of coronal stream to be one-seventh of that of accompanying prominence, the same explosive force which during the last eclipse sent prominences to a height of 80,000 miles, will send coronal matter forming the streams to an infinite distance. Coronal streams extend far, then, into space. The densest portion of the stream is located at the point of minimum velocity, and the coronal streams visible in the last total eclipse were, Prof. Schaeberle says, according to his photographs, apparently most dense in the higher regions, proving that the matter was in rapid movement. The mechanical theory of comets supposes coronal streams to issue from the sun at all angles. These streams will penetrate far into space (some crossing one another). The atmosphere of a comet on striking these streams will in projection be in the form of luminous, nearly concentric, arcs, the greatest brilliancy being near the most advanced part of each stratum. More than one coronal stream will produce in the comet multiple tails, the angles between the tails being a function of the velocities of motion, and the inclinations of the streams. An examination of the cases where a tail is turned towards the sun is explained by a coronal stream, having a less velocity than that of the receding comet, thus producing such a phenomenon the moment the stream is entered. Prof. Schaeberle, at the conclusion of the papers, refers to a satisfactory explanation of the "Gegenschein," and also to a plausible explanation of the Aurora, both based on the coronal streams.

THE NEW STAR IN NORMA.—When the announcement of the discovery of this star by Mrs. Fleming reached Prof. F. C. Kapteyn, a search was made by him through his manuscript of his photographic Durchmusterung for this region, with the result (*Astronomische Nachrichten*, No. 3196) that he found the following star, which is "wohl fast ohne Zweifel" identical with Mrs. Fleming's. Its position is

Phot. Mag.	R.A. 1875·0	Decln. 1875·0
9·2	15h. 21m. 0.5s.	−50° 9′·7

The plates which he had that contained this part of the heavens were taken in 1887, on June 25, July 25, and August 2; and in 1890, on April 29 and May 2. An examination of these showed that on the first three dates the star was not visible, but the last two distinctly indicate it as a star a whole magnitude brighter than the faintest star on the plate. A comparison of its brightness with the following three stars in its neighbourhood was made.

	Mag. in Phot. Diam.	R.A. 1875·0. h. m. s.	Decln. 1875·0. ° ′
(*a*)	9·2	15 20 51·0	−50 21·4
(*b*)	9·1	20 59·0	21·5
(*c*)	9·1	21 35·0	9·4

The results show that Mrs. Fleming's star is brighter than (*a*), scarcely dimmer than (*b*), and a little dimmer than (*c*). Its magnitude then in July and August, 1887, could not have been more than 9·2.

THE NATAL OBSERVATORY.—Mr. Nevill, the Government Astronomer for Natal, has to work under great difficulties. The grant of £800 per annum, made by the Natal Government to the Observatory, is certainly not enough to keep the establishment efficient. When the Observatory was first erected it was a substantially built, rectangular red brick edifice, carrying a light wooden upper structure, which formed equatorial and transit rooms, but there was only one room below, and this had to serve the double purpose of a computing room by day and sleeping room by night. Mr. Nevill has asked the Government to give him more accommodation, but his application has not been granted, the plea being shortness of funds; so he has had extra rooms built entirely at his own expense, and even now the four assistants of the Observatory work in a room which is nothing more than an enclosed verandah. The principal points under investigation at the Observatory are: the parallactic inequality in the motion of the moon, the lunar diameter, the effects of irradiation and its variations upon the moon's apparent semi-diameter, and lunar libration.

MAGNITUDE AND POSITION OF T AURIGÆ.—The current *Comptes Rendus* (November 13) contains a number of observations of Nova Aurigæ, made by M. Bigourdan, at the Paris Observatory. The star's magnitude was compared with that of neighbouring stars on October 10 and 12, and on November 8, 11, and 12. The observations show that from the middle of October to the 8th inst. the light diminished very definitely, and afterwards increased, but on the date of the last observation it had not attained the magnitude observed on October 10. In 1892 M. Bigourdan micrometrically measured the position of the Nova with respect to a neighbouring star, and a repetition of his measurements, after an interval of eighteen months, shows that no change of position has taken place.

PERIOD OF JUPITER'S FIFTH SATELLITE.—Prof. E. E. Barnard's new measures (*Astronomy and Astro-Physics* for November) for the times of elongation of the fifth satellite give a period

$$P = 11\text{h. } 57\text{m. } 22{\cdot}56\text{s.}$$

The value obtained from his last year's value was

$$P = 11\text{h. } 57\text{m. } 23{\cdot}06\text{s.}$$

While Mr. A. Marth, from the same observations, derived a period of

$$P = 11\text{h. } 57\text{m. } 21{\cdot}88\text{s.}$$

The new determination falls, as will be noticed, nearly midway between the two values quoted, and covers a period of 743 revolutions of the satellite.

GEOGRAPHICAL NOTES.

THE fate of the Bjorling exploring expedition, concerning the safety of which much anxiety has been felt in Sweden, has now been ascertained. Messrs. Bjorling and Kalstennius, two young Swedish naturalists, hired a small schooner, the *Ripple*, at St. John's, in June, 1892, and set out for a collecting trip along the west Greenland coast, accompanied by a crew of three men. After leaving the Danish settlements on the west coast last summer, no further news was received from the expedition, and the captains of the whaling vessels at work in Davis Strait this summer were specially requested to look for traces of the *Ripple* and her party. Captain McKay, of the Dundee whaler *Aurora*, who returned last week, reports that he visited the Carey Islands at the entrance to Smith's Sound on June 17 this year, and found there the wreck of the *Ripple*, a number of documents, and the body of one of the ill-fated crew. One of the papers written by Bjorling on August 17, 1892, on which day he had visited the Carey Islands to get provisions from the cache left by Sir George Nares, stated that on leaving the schooner ran aground, and the party had to land. A later note, dated October 12, shows that they attempted to reach Foulke's fjord to winter there, but after reaching Northumberland Island circumstances compelled their return. At the date of writing Bjorling intended to start immediately to endeavour to reach the Eskimo settlements at Cape Faraday or Clarence Head in Ellesmere Land, with the hope of returning to Carey Islands by July 1, 1893, to meet any whaler. In case of not finding a vessel he intended to push on to the Danish settlement. On receiving this news Captain McKay at once headed for Ellesmere Land, but the ice closed in, and he had to turn back. As the provisions would only last until January 1, it is to be feared that the whole party has perished, unless they were successful in reaching the Eskimo. If they did so, and were subsequently able to make their way to the Danish settlements, there may still be hope, but no news can be received until next summer.

THE *Times* announces that the Peruvian Government has awarded a gold medal to Mr. Clements R. Markham, F.R.S., President of the Royal Geographical Society, for the great services he has rendered to Peru in elucidating its geography, and in giving expression "with upright impartiality" to the facts of its history.

MR. W. H. COZENS-HARDY, who has just returned from a summer spent in exploration on the borders of Montenegro and Albania, has succeeded in making a number of observations of high geographical value. He has been able to lay down on a map for the first time the present frontiers of the principality, and from his knowledge of Slavonic languages and the free access accorded to him to the Montenegrin archives, he can also give a most interesting account of the past changes in the boundaries, furnishing, in fact, a chapter in the historical geography of the Balkan peninsula.

THE Arctic skipper Hans Johannens-en, of Hammerfest, Norway, has heard from old Yakutsks that from the highest points of the northern shores of the New Siberian Islands a lofty land has been discerned to the north-west, at a distance of about fifteen nautical miles. He thinks, therefore, that should Nansen not steer too close to the coast, this new land might be seen from the masthead. And should the state of the ice be at all favourable, Nansen will, in all probability, attempt to take up his winter quarters there instead of the New Siberian Islands.

FROM a recent number of the *Kölnische Zeitung* we learn the somewhat remarkable fact that Cologne is the largest city in Germany, taking account of the area it covers, Berlin coming only fourth in order. In Cologne, however, only eight per cent.

of the area is built upon, the remainder being streets and open spaces.

THE Paris Geographical Society has awarded the grand prize for geographical research to M. Maistre, for his great journey from the Congo to the Shari.

FLAME.[1]

THE subject on which I have the honour to address you this evening is, I am aware, one of the most hackneyed among the topics that have served for popular scientific lectures. I can only hope that it has not quite lost its charm. The chemist is often twitted with having to deal with mere dead soulless things, which at the best only set themselves into angular and unpalpitating crystals. There may be a certain amount of truth in this, but in flames we surely have phenomena of some liveliness. Our flame must be fed; it has its anatomy and varied symmetry; it is vigorous, mobile, and fleeting. I do not wish to make extravagant claims, but I do think that one may be excused for feeling almost as much interest in the study of flame as, for example, in the contemplation of the somewhat torpid evolutions of an amœba or the circulation of water in a sponge. To our guileless ancestors, at any rate, flame was a phenomenon of the rarest mystery; unable as they were to discriminate between the material and the immaterial, unable to track the solid or liquid fuel to its gaseous end, this radiant nothingness called flame became to them one of the primary inscrutable, irresolvable things of Nature—an all-devouring element, often of peculiarly divine significance.

The essential nature of flame appears to have been discovered at the beginning of the seventeenth century by the Belgian, Van Helmont. This remarkable man is well known to chemists as one of the acutest and least superstitious of the whole band of alchemists. He was somewhat speculative in the domain of physiology, but in chemistry Van Helmont made discoveries of fundamental importance. From our immediate point of view, one of the most important things he did was to sweep away the mystery that had so long attached to the gaseous state of matter. In so far as he distinguished between different gases obtained from different sources, he may be said to have been the first to bring aëriform matter within the range of substantial things that might be submitted to experimental investigation. It was in consequence of this that he was led to the discovery of the nature of flame. I will quote the important passage from his writings.

"But the flame itself, which is nothing but a kindled smoke, being enclosed in a glass in the very instant perisheth into nothing.

"The flame indeed is the kindled and enlightened smoke of a fat exhalation; be it so; but as the flame is such and true fire it is not another matter, being kindled and not yet kindled, neither doth it differ from itself; but that light being united in its centre, hath come upon a fat exhalation which is the same as to be inflamed.

"Let two candles be placed which have first burned awhile, one indeed being lower than the other by a span; but let the other be of a little crooked situation; then let the flame of the lower candle be blown out; whose smoke, as soon as it shall touch the flame of the upper candle, behold the ascending smoke is enlightened, is burnt up into a smoky or sooty gas, and the flame descendeth by the smoke even unto the smoking candle. Surely there is there, the producing of a new being, to wit, of fire, of a flame, or of a connexed light; yet there is not a procreation of some new matter or substance.

"For the fire is a positive artificial death but not a privative one, being more than an accident and less than a substance."

We can best understand the meaning of this somewhat oracular statement by repeating Van Helmont's experiment. We take a bundle of lighted tapers so as to get a large flame, we hold over "in a little crooked situation" another lighted taper, and now blow out the lower flame. We note the ascending column of smoke, and observe that when it touches the upper flame it ignites, and the flame descends several inches through the smoke to the bundle of tapers. Flame therefore, says Van Helmont, is burning smoke; it is not a new substance nor a mere chance occurrence, but the incandescence of a vapour or smoke that already existed.

Van Helmont only recognised in a vague way the important part played by the atmosphere in the phenomenon. This was realised much more perfectly soon afterwards by Hooke, who speaks of "that transient shining body which we call flame," as "nothing but a mixture of air and volatile sulphureous parts of dissoluble or combustible bodies which are acting upon each other whilst they ascend," an action so violent, he says, "that it imparts such a motion or pulse to the diaphanous parts of the air" as was requisite to produce light.

Without entering further into early historical details I may say that it was only towards the end of last century that the essential chemistry of the phenomenon was fully expounded by the great Lavoisier. He showed that, as Hooke had surmised, flame is the region in which combination attended by the evolution of light takes place between the components of a gaseous substance and the oxygen of the air.

The next step in the history of our knowledge of flame brings us to the memorable researches of Humphry Davy, whose name more than that of any other man is associated with this subject. Of Davy's work I shall have more to say presently; but at this moment I will only make one allusion to it, an allusion which will provide us with a proper starting-point this evening. It is interesting to note that Davy's discoveries concerning flame were the consequence and not the cause of the discovery of the miners' safety-lamp. In this case practical application preceded purely scientific discovery.

I need not describe the safety-lamp to you in Nottingham, where it has recently received such important improvements at the hands of Prof. Clowes. When the lamp is placed in an explosive mixture, you know what happens—the explosive mixture burns with a quiet flame within the lamp, but the flame cannot pass through the wire gauze to ignite the mixture outside the lamp. I can demonstrate this by means of this large gas-burner, which is primarily a Bunsen burner, that is, a burner which by means of holes at the base of the tube draws in sufficient air to enable the gas to burn with a practically non-luminous flame. If I turn on the gas and apply a light to the top of the burner, you observe that I get a flash and a small explosion within the tube, but no continuous flame. The fact is that the mixture of gas and air within the tube is highly explosive. Placing a gauze cap over the burner and applying a light, I now get a steady flame. The explosive mixture made in the tube passes through the gauze and is inflamed, or, if you like, exploded; but the explosion cannot pass through the gauze, because the metallic wires withdraw the heat so rapidly that the mixture below it never reaches the temperature of ignition. Above the gauze we have the continuous flame.

"These results are best explained," says Davy, "by considering the nature of the flame of combustible bodies, which in all cases must be considered as the combination of an *explosive mixture* of inflammable gas or vapour and air; for it cannot be regarded as a mere combustion at the surface of contact of the inflammable matter."

Davy, then, regarded flame as being essentially the same as explosion; it was, in fact, a kind of tethered explosion.

Since Davy's time we have learned much about the nature of gaseous explosions, and we now know that such explosions, when fully developed, proceed with enormous rapidity and are of great violence, incapable of arrest by such simple means as we have just used. Still there is not much to correct in what I have said. I think I cannot do better than show you the transition of flame into explosion by an experiment which was first shown by Prof. Dixon in the lecture which he gave at the meeting of the British Association in Manchester in 1887.

The apparatus before you consists simply of a Bunsen burner surmounted by a long glass tube. If I turn the gas on and light it I obtain at the top of the glass tube a steady flame. The mixture ascending the tube can scarcely be called explosive at present, but if I alter the proportions of gas and air suitably it becomes distinctly explosive. Observe what happens when this is the case. The flame can no longer keep at the top of the glass tube; it passes within it, and descends with uniform velocity till at a certain point it flickers and then shoots down almost instantaneously to the bottom. This sequence of events is exhibited in all cases when flame develops into explosion. We are concerned only with the first phase, viz. that of comparatively slow inflammation and a flame, we may say, is a gaseous explosion brought to anchor in the period of incubation.

There is one other point connected with explosion that we must note on account of its important bearing on the chemistry of flame. When we are dealing with explosive mixtures of gas and air, we find practically that the composition of the

[1] An evening discourse to the British Association at the Nottingham meeting, September 15, 1893, by Prof. Arthur Smithells.

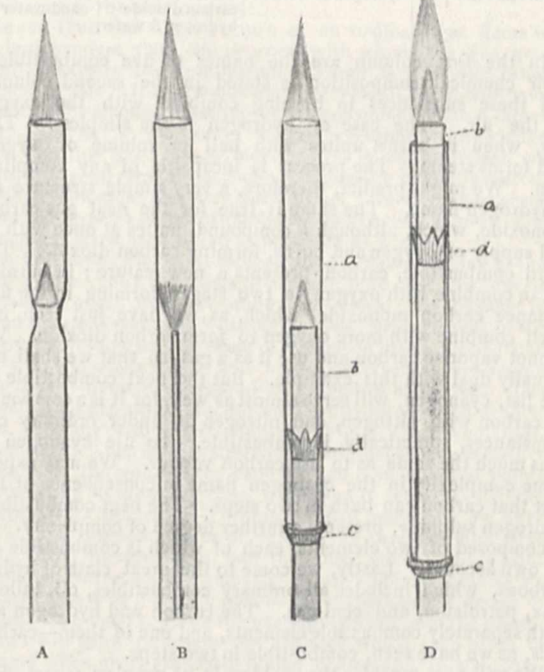
mixture may vary considerably and still retain its explosive properties. There is, of course, a certain mixture which presents the greatest explosive power; a further quantity of the combustible gas or of the air will diminish the explosibility, but not entirely destroy it till a large excess is used. With hydrogen, for example, two and a half times the volume of air (which contains exactly the oxygen requisite to combine with the hydrogen and produce water) is the right quantity for the maximum explosive effect, but we still get explosion when we have much more than two and a half times as much air as hydrogen, or when, on the other hand, we have much less. In one case there will be oxygen left uncombined, in the other case hydrogen. I dwell upon this in order that we may be prepared to find the same thing in flames, in order that we may not be surprised to find combustion taking place in mixtures where either gas or air is in excess of the quantity actually required for the purpose of chemical combination. Bearing this in mind, let us revert to the experiment that I have just shown. It consists, you remember, in mixing air with gas before burning it, to such an extent that the flame strikes down the tube. On a close examination we find that this is not quite a correct statement, for when I regulate the air with nicety you see that it is only part of the flame that strikes down the tube. There remains all the while at the top of the tube another part of the flame which is not mobile. With a little care I can adjust the proportion of air and gas so that the part of the flame which is mobile shall move up and down the tube like a piston. All the while you see the pale steady flame at the top of the tube. When in this critical condition a little more air determines the descent of the movable part of the flame, a little less sends it to the top.

Let us now turn to the explanation of this phenomenon. It is clear, in the first place, that coal-gas and air form an explosive mixture long before there is enough air to burn all the gas. For it is only part of the flame that descends the tube, and there is enough gas passing through this part to form a second flame as soon as it reaches the outside air at the top of the tube. There is, as a matter of fact, only about two-thirds as much air entering the tube at the bottom as would be necessary to burn the whole quantity of gas. We see, in the next place, that the explosibility varies greatly according to the proportions of gas and air. For what is the cause of the descending flame? It is simply that we have an explosive mixture in process of inflammation. The inflammation is tending downwards; opposed to it is the movement of the explosive mixture upwards. If the upward movement of the unburned mixture is more rapid than the downward tendency of the inflammation, the flame cannot descend. We can only make it descend by making the downward tendency greater. This we do by adding more air, and making the mixture more explosive. We see that we can balance these two opposite velocities with the greatest nicety by a careful adjustment of the proportions of the explosive mixture.

In order to ascertain what proportion of gas is being burnt in this movable flame, and what is the chemical character of the products there formed, it is necessary to keep the two parts of the flame separate, and to take out some of the gases from the intervening space.

This is very easily done. The flame descends, we have seen, because its rate of inflammation is greater than the rate of ascent of the combustible mixture. If now we can make this rate of ascent more rapid at one part of the tube than it is anywhere else, we may expect to stop the descent of the flame at that point and keep it there. We can do this simply by choking the passage, for just as a river must flow rapidly where its banks are close, so must the stream of gas rush more rapidly where the tube is choked than either below or above, where there is a wide passage. If, then, I replace the plain glass tube by one that has a constriction in one part, and if I cause the inner cone of the flame to descend as before, it stops, as you see, at the constriction, and will remain there any length of time. Its rate of descent is greater than the rate of ascent of the gas where the tube is wide, but not so great as that where it is narrowed by the constriction. We have now got the two cones of flame widely separated. In this state of things we can, if we choose, draw off the gases from the space between the two cones by putting in a bent glass tube and aspirating. We could then analyse these gases and see what has happened in the first cone. (Fig. 1, A.)

I will now show you another method in which the two cones can be separated. It is based on the same principles as the one just used. I have here a two-coned flame burning at the top of a glass tube. I shall let the air supply be liberal, but not quite sufficient to cause the descent of the inner cone. The rate of ascent of the gas is now just a trifle greater than the rate of descent of the flame. If now I retard the rate of ascent of the gas, the balance will be disturbed and the inner cone will descend. I can easily do this by laying an obstacle *along* the stream of gas, for at the end of it there will be no more current than you would find over the stern of a boat anchored in mid stream. I take this obstacle, then, in the form of a glass rod fixed centrally along the current of gas; I push it up until it touches the tip of the inner cone, and then pull it down again. You observe what has happened. The cone has followed the rod into the tube, and remains attached to it. You will notice, too, that the cone is inverted. That is easily understood. It is only at the tip of the rod that the current is slowed down; there only is the rate of ascent of the stream less than the rate of inflammation. The tendency in every other part of the stream is for the cone to go to the top; hence the inversion. (Fig. 1, B.)



A B C D

FIG. 1.—Methods of separating the two cones of an air coal-gas flame.

We can get a still more convenient apparatus by a modification of the first method. Instead of choking the bore of the single tube by a constriction, we may use two tubes of different diameter, one sliding within the other. This apparatus is shown in Fig. 1, C; *a* is the wider tube, *b* the narrower one. The two tubes are connected by an india-rubber collar (*c*), and kept steady by the brass guide (*d*). The outer tube can be slid up and down the inner one as desired. If we place this apparatus over a Bunsen burner and turn on the gas, we shall have a tolerably rapid upward current in the inner tube, but as soon as the gas emerges into the wider one its velocity will of course diminish. The consequence is that if we now light the gas and gradually increase the air supply, the inner cone will descend until it reaches the orifice of the narrower tube; but at that point, meeting with the rapid stream, its progress is arrested, and it remains perched on the end of the tube. By sliding the tubes we can thus separate the cones any desired distance, or we can bring their orifices level and restore the original flame. Lastly, we can reverse the experiment, for we can begin with a two-coned flame burning at the protruding end of the narrower tube, and by sliding up the wider tube detach the outer cone and carry it upwards. (Fig. 1, D.)

Having now learnt the relation of flame to explosion, having discovered that flames have separable regions of combustion, and having armed ourselves with an appliance for dissecting the flame, we may proceed to discuss the main question.

I do not intend this evening to enter seriously into chemical details, but there are one or two simple points to which I must draw your attention. Flame, we see, is a region in which chemical changes are taking place with the evolution of light. It is to be expected, therefore, that the character of a flame, its structure and appearance, will vary according to the chemical changes that give it birth; and we should naturally anticipate that the more complex the chemical changes the more complex would be the flame. The kind of complexity to which I refer is illustrated by the diagram.

Name	Composition	Products: Partial Combustion	Products: Complete Combustion
Hydrogen		water	water
Carbon monoxide	carbon and oxygen	carbon dioxide	carbon dioxide
Carbon		carbon monoxide	carbon dioxide
Cyanogen	carbon and nitrogen	carbon monoxide and nitrogen	carbon dioxide and nitrogen
Hydrogen sulphide	hydrogen & sulphur	(?)	water and sulphur dioxide
Hydrocarbons	hydrogen & carbon	carbon monoxide carbon dioxide hydrogen & water	carbon dioxide and water

In the first column are the names of five combustibles; their chemical composition is stated in the second column. All these substances in burning combine with the oxygen of the air. The case of hydrogen is the simplest. This gas, when it burns, unites with half its volume of oxygen, and forms steam. The process is incapable of any complication. We might predict, therefore, a very simple structure for a hydrogen flame. The same is true for the next gas carbon monoxide, which, although a compound, unites at once with its full supply of oxygen and burns, forming carbon dioxide. The third combustible, carbon, presents a new feature; in burning it can combine with oxygen in two stages, forming in the first instance carbon monoxide, which, as we have just seen, can itself combine with more oxygen to form carbon dioxide. We cannot vaporise carbon and use it as a gas, so that we shall not actually deal with this example. But the next combustible on the list, cyanogen, will serve almost as well, for it is a compound of carbon with nitrogen, and nitrogen is, under ordinary circumstances, practically incombustible. To use cyanogen is thus much the same as to use carbon vapour. We may expect some complexity in the cyanogen flame in consequence of the fact that carbon can burn in two steps. The next combustible, hydrogen sulphide, presents a further degree of complexity. It is composed of two elements, each of which is combustible on its own account. Lastly, we come to the great class of hydrocarbons, which includes all ordinary combustibles, oil, tallow, wax, petroleum, and coal-gas. The carbon and hydrogen are both separately combustible elements, and one of them—carbon—is, as we have seen, combustible in two steps.

We will now consider the problem in its simplest aspect. For this purpose I choose the gas carbon monoxide. I should choose hydrogen were it not for the fact that its flame is almost invisible. We will allow a stream of carbon monoxide to issue from the circular orifice of this glass tube. Lighting the gas we get a blue flame. On examining this flame closely we perceive that it is simply a hollow conical sheath of pretty uniform character. I need scarcely demonstrate that it is hollow, but I may do so in a moment by using Prof. Thorpe's simple device of thrusting a match-head into the centre of the flame—a pin passing through the stick of the match, and its ends resting on the tube. The match-head is now thrust well up inside the flame, and you observe that it remains there sufficiently long without burning, to make it quite clear that there is no combustion within the cone. The conical form of the flame is easily explained. As the stream of gas issues from the tube the outside portions become mixed with the air and burn. The inner layers must successively travel further upwards, like the successive tubes of a telescope, before they can get enough air to burn, and in this way we arrive at the conical form.

There still remains one thing to account for, and that is the luminosity and colour of the flame. The questions here involved are perhaps the most interesting of all, but they are complicated, and I will not say more than a few words about them. The most obvious answer to the question, "Why is the flame luminous?" is to say that the heat developed during the chemical combination raises the product of combustion to a temperature at which it glows—a "blue heat" in the present case. Now if we put a thermometric instrument into the carbonic oxide flame, it does not register at any point as high a temperature as 1500° C., but if we take carbon dioxide and heat it in a tube by external heating to 1500° C. we get no signs of luminosity whatever. On these grounds several eminent investigators have been led to abandon the simple explanation above given, and to say that the luminosity of a carbon monoxide flame must depend not on the heat of chemical combination, but on something in the nature of electrical discharges between the combining substances, which discharges produce the disturbances of the ether perceptible as light. This view seems to be fraught with a fundamental error. The temperature registered by any instrument introduced into a flame is an *average* temperature, uncorrected for losses by conduction. It is not the temperature of the newly-formed gas, but of the mixture of that and the unburned gases. If we had a very small instrument which we could apply to the particles of newly-formed gas, we should undoubtedly find them at a very much higher temperature than any indicated by the ordinary thermometric apparatus, and it is not unlikely that the temperature would be several thousand degrees, approximating indeed to the temperature at which we arrive by calculation from the heat of combustion of the gas and the heat capacity of the product. We cannot say that the flame is luminous from some other cause than simple hotness, for we have no means of seeing whether carbonic acid glows when raised by external heating to a temperature of several thousand degrees.

At the same time one cannot help remarking on the similarity between such a flame as that of carbon monoxide and the appearance presented by an attenuated gas when submitted to the

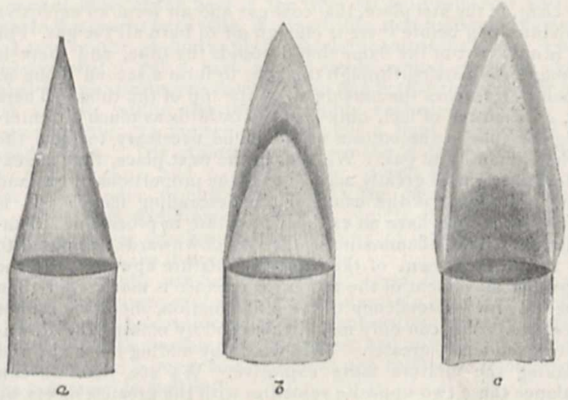


FIG. 2.—Typical Flames. (*a*) Carbon monoxide, single coned; (*b*) Cyanogen, two coned; (*c*) Small coal-gas flame.

electrical discharge in a Geissler tube. I have here such a tube, containing carbon dioxide, and I have placed a mask over it, so that we see a long triangular piece of it. When I pass the discharge you see it lights up and presents an appearance strikingly like that of our conical flame of carbon monoxide. There may be a close relationship between the phenomena, but we cannot affirm it yet. No doubt we shall soon learn a good deal more about both phenomena.

We have now done with the simplest kind of flame. We see that it consists of a single conical sheath of combustion, at every point of which the same chemical change is taking place, and every point of which in consequence has the same appearance.

We pass to the cyanogen flame. This flame is one of remarkable beauty; it consists, as you see, of two distinct parts: one a rose or peach-blossom coloured cone, surrounded by a paler cone, which is bright blue where it is near the inner cone, and shading off to a kind of greenish grey. What is the cause of this double structure? It might be that part of the gas is burning round the orifice, the rest further out in the second cone; but a similarity of the chemical processes in the two parts of the flame is here rendered improbable by the difference in colour. The only satisfactory way of answering the question is to separate the cones, and analyse the gases in the intervening space. This we can easily do in the cone-separating apparatus.

I now form the flame at the top of our cone-separating apparatus, and supply a certain amount of air along with the cyanogen. You observe the rose-coloured cone contracts somewhat. The gas burning there now gets its air supply easily, and has not to wander outwards. If I still further increase the air supply, and make the ascending mixture explosive, you see the inner cone begins to descend into the tube, and passes down until its progress is checked at the narrow tube, where the up-rush of gas is more rapid. We have now got the cyanogen flame dissected, and by taking out a sample of the gases from this interconal space and analysing it, we shall find what chemical change has taken place in the inner rose-coloured cone. The analysis shows that what takes place is the combustion of the carbon of the cyanogen to form carbon monoxide almost exclusively; the carbon monoxide then ascends, and when it meets with more oxygen in the outer air, burns in a second cone to form carbon dioxide.

Reverting then to the flame of the pure unmixed gas burning at the top of a tube, we see that the gas and air will interpenetrate. When there is just enough oxygen to burn the gas to carbon monoxide, we get the rose-coloured cone, and outside it, where this carbon monoxide gets more air, we have a second cone. The two-coned structure corresponds then to two chemical stages of combustion.

Now we might go further and anticipate that if we supplied a very large quantity of air to the cyanogen, as in a blowpipe, the two-coned structure would disappear, for the carbon should be burnt up at once to the ultimate product, carbon dioxide. We can easily try this. I will separate the two cones again in our apparatus, and increase the air supply still further. When I do so you observe that the second cone gradually fades away, and now the whole of the combustion is taking place at the end of the inner tube. Though this is so, the flame is not quite a simple cone. It is, as you see, surrounded by a greenish halo. This halo is due, I believe, as Prof. Dixon has suggested, to the fact that the nitrogen of the cyanogen is not, strictly speaking, incombustible. This has been proved by Mr. Crookes in his beautiful air flame, and besides, the greenish halo is frequently noticeable in cases of combustion where oxides of nitrogen are present.

Keeping to our list we ought next to deal with the combustible hydrogen sulphide or sulphuretted hydrogen. This gas, you remember, is composed of two separately combustible elements, each burning in one stage. The flame is, as you might expect, two-coned, but I will not dwell upon this case—partly because it is not yet fully worked out, and partly because any prolonged experimenting with this gas would, I feel sure, be resented even by the most indulgent audience.

I am obliged, therefore, to pass to compounds of carbon and hydrogen, in which there are not only two combustible elements, but one of them, as we have seen, combustible in two chemical stages. Here we have an almost unlimited choice of materials, for we come amongst the combustibles which ordinarily supply us with light. I shall, for the sake of convenience, use coal-gas. This is really a very complex combustible, consisting one half of hydrogen, the other half of at least a dozen different compounds of carbon and hydrogen. But experience has shown that the chemical phenomena attending its combustion are quite of the same character as those to be observed with a single compound of hydrogen and carbon.

It will, I imagine, be scarcely necessary for me to point out the various parts which are to be seen in the flame of a candle or of coal-gas. There is on the diagram (Fig. 2, *c*) before you the picture of a somewhat small coal-gas flame, produced at a circular orifice. It is, of course, enormously enlarged in the diagram. Four distinct parts are to be recognised. First, the central and darkest part; this contains the unburnt gas, just as we saw in the case of the carbon monoxide flame. Perhaps it is wrong to speak of this at all as part of the flame, for it is really a region of no flame. At the base of the flame are two blue strips embracing the lower portion of the flame. This appearance you will understand results from the mode in which we view the flame. The strips are really due to a sheath which goes right round the flame like an uninterrupted calyx. It looks bright where we view it edgewise. When we look through, as in the middle of the diagram, it is very pale indeed. Next we have to notice the bright yellow patch, so bright in the reality as to mask the other parts. Though it looks bright and dense, it is merely a hollow sheath. Lastly, there is surrounding the whole flame a pale mantle of flame of very slight luminosity, and of an almost indescribable tint, which perhaps we may call lilac. These parts are discernible in all ordinary flames. They do not always occupy the same relative space. In the flame given by a good gas-burner the yellow part is made by intention as large as possible; in the flame of a piece of string or a spirit-lamp you will see the outer investing mantle very distinctly developed.

If we are to understand flame, then, we must find an intelligible explanation of the existence of these distinct parts of its anatomy. One important point we can settle at once. An ordinary flame owes its differentiated structure to the slowness with which it gets the oxygen necessary for combustion. If there is an immediate and sufficient supply of air, the characteristic structure disappears. This we can secure by making the stream of gas sufficiently rapid. I have here a steel cylinder containing coal-gas at very high pressure. If I allow the gas to escape slowly, we get a flame in which we should find the ordinary parts. But if now I allow the gas to issue rapidly, the admixture with air is so rapid, and, as you see, we have a pale flame quite undifferentiated in structure. We reach the same result by introducing a strong current of air into an ordinary flame, as in the blast blow-pipe. The flame, you see, is then homogeneous, as in the previous case.

We see then that the structure of an ordinary gas flame is largely dependent upon the slowness with which the gas gets the air necessary for combustion. There is still one other evidence of this. It is obvious that a very small flame will have a much better chance of getting its oxygen quickly than a larger flame. It is, I am sure, within everyone's knowledge that a very tiny gas flame is blue, and, as a matter of fact, we can learn a great deal about flame structure by carefully watching the development of a very small flame. I am going to show you on the screen a series of photographs of actual flames. The photographs have been tinted as faithfully as possible.

The first slide (Fig. 3, *a*) shows a tiny gas flame burning at the end of a glass tube; it consists of a bright blue cone surrounded by a fainter one. Both are quite continuous. By putting in another slide, and using the "dissolving view" arrangement of the lantern, I will show you the effect of turning on the gas. The flame (Fig. 3, *b*) you see is larger, and now is observed a third region in the flame—namely, a patch of bright yellow at the tip. The original cones are still there, but are slightly interrupted at their apices. Turning on more gas, the flame (Fig. 3, *c*) again enlarges, the yellow patch increases in size, and the original cones are further broken into. But you see the yellow patch is indented at points corresponding to the inner cone, which, as it seems, is striving to maintain its integrity. Turning on still more gas, we have now a great preponderance of yellow, the original blue cone is reduced to mere vestiges, and the outer cone forms a faint surrounding to the whole flame (Fig. 3, *d*). This is flame as we ordinarily know it. I wish now to show you another series of changes. We must suppose the gas supply fixed, and the photographs I will show represent the changes which take place in the flame when air is gradually added beforehand to the coal-gas. The supply of coal-gas is, I repeat, the same in all cases. The first change seen is, you will notice, that the yellow patch diminishes in extent (Fig. 3, *e*). If I add more air it diminishes still more, and the inner cone is growing in distinctness (Fig. 3, *f*). If I add a trifle more air, the yellow patch disappears altogether, and we have now complete and distinct inner and outer cones (Fig. 3, *g*). I think you will admit that these two sets of photographs show a close correspondence, and you can see it more plainly if I throw them on to the screen in a group. There is really nothing surprising in this similarity. The smallest gas flame has obviously the best chance of getting air, and when it gets enough it burns in a two-coned flame. The same effect is reached by adding air to the gas before it is burned. If we have a larger gas flame it has, of course, less chance of getting its oxygen rapidly, and we see that in whatever way we starve the flame of oxygen, we lose the simple structure, and come upon the yellow patch.

Now, when we come to inquire into the chemical changes occurring in such a flame, we may, I think, feel confident that the chemical actions which determine the existence of the blue cone and the outer cone are the same, whether these cones are complete, as in a small flame, or fragmentary, as in a larger one.

If that is so we can soon make progress, for, as I have shown you, we can easily separate these cones and find what is going on in each. I again use the cone-separating apparatus. First we have an ordinary luminous gas flame at the top of the outer tube. I pass in air, the flame loses luminosity, and rapidly becomes an ordinary two-coned Bunsen flame. I push the air supply further; the inner cone enters the tube, and descends until it rests on the end of the inner tube. The two cones of a hydrocarbon flame are thus widely separated; we can aspirate a sample of the gases, and see what changes have taken place in the first region of combustion. The result is one that we might await with curiosity, for we have now a competition. There are both carbon and hydrogen to burn, and not enough oxygen to burn both; the question is, which will have the preference? I think I may say that the off-hand opinion of any chemist who has not had his attention drawn specially to this point would be that the hydrogen would easily have the preference. But, as a matter of fact, this question was settled long ago by Dalton, and in the opposite sense, and in the present case analysis would confirm this conclusion. If we analysed the gases we should find that all the carbon is burnt in the first cone, whilst a considerable part of the hydrogen passes through unburnt. The change is not quite so simple as these words might apply, as you will see from the actual figures of analysis.

ANALYSIS OF INTER CONAL GASES FROM A COAL GAS AIR FLAME.

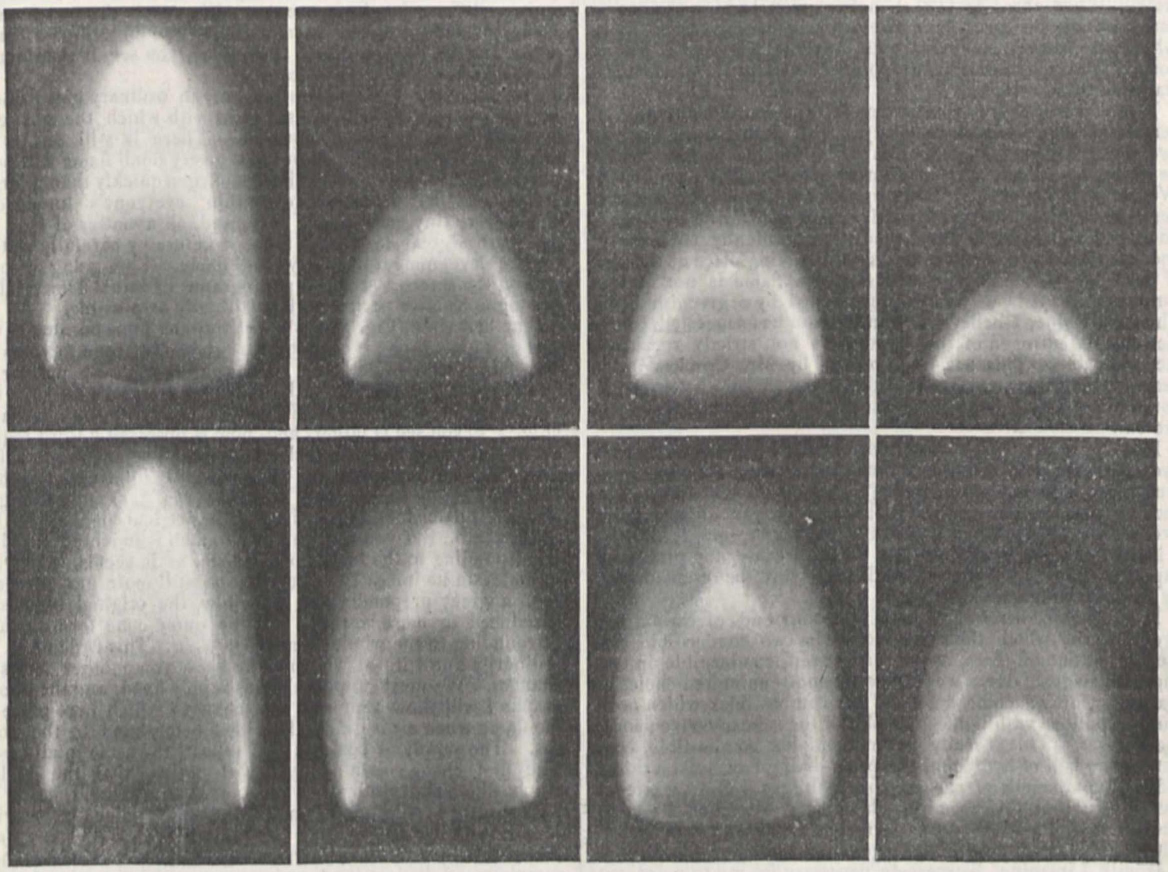
Carbon monoxide	8·7	17·9 combustible gases.
Hydrogen	9·2	
Carbon dioxide	4·1	20·1 burnt gases.
Water	16·0	
Nitrogen	62·0	
	100·0	
Amount of air used	78·5	Oxygen 16·5; Nitrogen 62·0
Amount of air still needed	42·9	Oxygen 9·0; Nitrogen 33·9

About two-thirds of the carbon is burnt to form carbon monoxide, one-third to form carbon dioxide, whilst rather less than two-thirds of the hydrogen is burnt, and more than one-third remains altogether unburnt. We need not dwell on the details, especially as the analysis of the gases was made after they had cooled. The four gases—carbon monoxide, carbon dioxide, steam, and hydrogen—act upon one, as a matter of fact, while they are cooling down, and the distribution of the oxygen that we find in our analysis of the cold gas is not precisely that which exists in the gases as they just leave the inner cone. We shall only draw a general inference, and it is one that has been recently verified in a very complete manner by Prof. Dixon and his pupils. This inference is simply that the carbon in the inner cone is for the most part burnt to carbon monoxide, and that the hydrogen to a considerable extent is set free. So much then for the inner cone. The outer cone is due simply to the burning of the carbon monoxide and hydrogen which escape from the inner cone. When they meet with oxygen in the free air their combustion is completed. We are now in possession of the explanation of the two-coned gas air flame. Applying this to the tiny gas flame to which no air has been previously added, we see that the inner cone will be formed where the air has penetrated the gas sufficiently to produce such a gaseous mixture as we had in the lower cone of our separator. The gases coming from this burn further out when they meet with more air, and form a second cone.

The last thing we have to explain in the ordinary gas flame is the production of the yellow luminous patch, which, from the illuminating point of view, is the most important feature of all.

Now I need scarcely remind you that the general opinion is

d *c* *b* *a*



d *e* *f* *g*

FIG. 3.—*a*, *b*, *c*, *d*, flames with successively increasing quantities of coal-gas. *d*, *e*, *f*, *g*, flames with fixed supply of coal-gas and successively increasing quantities of air.

that this yellow patch in the flame is due to glowing carbon in a solid and very finely-divided state. The very familiar fact that a cold object introduced into the yellow part becomes coated with a black solid deposit, composed almost wholly of solid carbon, confirms this view. That this carbon or soot is solid in the flame, is shown by the fact that it is deposited as a solid even when a highly-heated object is placed in the flame, and there are other proofs—some of them very pretty—which I cannot show for lack of time and of a means of magnifying.

This explanation is due to Davy, and constitutes his most celebrated discovery on the subject of flame. He describes it in the following words:—

"When a wire-gauze safe-lamp is made to burn in a very explosive mixture of coal-gas and air, the light is feeble and of a pale colour, whereas the flame of a current of coal-gas burnt in the atmosphere, as is well known by the phenomena of the gas-lights, is extremely brilliant. . . . In reflecting on the circumstances of the two species of combustion, I was led to imagine that the cause of the superiority of the light of the *stream* of coal-gas might be due to the *decomposition* of a part of the gas towards the interior of the flame where the air was in smallest quantity, and the deposition of solid charcoal which, first by its *ignition*, and afterwards by its *combustion*, increased in a high degree the intensity of the light; and a few experiments soon convinced me that this was the true solution of the problem.

"I held a piece of wire-gauze of about 900 apertures to the square inch over a stream of coal-gas issuing from a small pipe, and inflamed the gas above the wire-gauze which was almost in contact with the orifice of the pipe, when it burned with its usual bright light. On raising the wire-gauze so as to cause the gas to be mixed with more air before it inflamed, the light became feebler, and at a certain distance the flame assumed the precise character of that of an explosive mixture burning within the lamp, but though the light was so feeble in this last case, the heat was greater than when the light was much more vivid, and a piece of wire of platinum held in this feeble blue flame became instantly white hot.

"On reversing the experiment by inflaming a stream of coal-gas and passing a piece of wire-gauze gradually from the summit of the flame to the orifice of the pipe, the result was still more instructive, for it was found that the apex of the flame intercepted by the wire-gauze afforded no solid charcoal, but in passing it downwards solid charcoal was given off in considerable quantities, and prevented from burning by the cooling agency of the wire-gauze; and at the bottom of the flame, where the gas burnt blue in its immediate contact with the atmosphere, charcoal ceased to be deposited in visible quantities."

Only one attempt has been made to disturb the conclusion here drawn by Davy. In 1868 Prof. Edward Frankland, to whom we are indebted for many important discoveries respecting flame, came to the conclusion that the light-giving agency in flames was not solid carbon, but certain complex vaporous compounds of carbon and hydrogen. I regret very much that time will not admit of my detailing the evidence in favour of this view, or the counter evidence by means of which most chemists have been persuaded that Davy's explanation was, after all, the correct one. It is, however, right to remark that Prof. Frankland not only adheres to his own view, but promises to adduce further evidence in its favour.

Let us for the present, at any rate, stick to the opinion of the majority, and admit that the bright light of ordinary flames is due to incandescent particles of solid carbon. The next question is, How does this carbon become separated?

This question is dealt with by Davy, but in language of some ambiguity. He says, "I was led to imagine" . . . . that it "might be due to the *decomposition* of a part of the gas towards the interior of the flame where the air was in smallest quantity, and the deposition of solid charcoal which first by its *ignition*, and afterwards by its *combustion* increased in a high degree the intensity of the light."

Whatever these words may have been intended to mean, or whatever interpretation is the fair one, it is certain that Davy's explanation was soon presented as if it implied lack of air to be the chief cause of carbon separation. As there was a large quantity of hydrocarbon, and only a small amount of oxygen in the central parts of flame, the hydrogen, it was said, being the more inflammable element, will seize upon this oxygen and leave the carbon uncombined. The fact that this version was given by Faraday lends some countenance to the belief that it was a fair representation of Davy's view.

Now this doctrine was really incompatible with facts known, though apparently not widely known, at the time. I have already referred to the fact that Dalton at the beginning of the century showed that when a hydrocarbon is exploded with a supply of oxygen insufficient to burn both the hydrogen and the carbon, it is the carbon, and not the hydrogen, which has the preference. If, therefore, we follow Davy in regarding flame as a tethered explosion, we cannot explain the separation of carbon as being due to the preferential combustion of the hydrogen. This fact was clearly pointed out by Kersten in 1861, but notwithstanding this, and other investigations tending to the same conclusion, the old view has somehow kept its ground down to the present day.

We must now turn to the alternative explanation. It is supplied by the words, and, I think, by the intention, of Davy. He says that the carbon separation might be due to the *decomposition* of the gas towards the interior of the flame. If this decomposition be not due to chemical action, it must be due to heat; and certain it is that hydrocarbons when strongly heated do decompose, and do deposit carbon. Here is a result of this action occurring on the large scale. This gas-carbon, as it is called, is deposited in gas-retorts owing to the action of intense heat on the hydrocarbons of the gas.

In another place Davy says: "I have shown in the paper referred to in the introduction, that the light of common flames depends almost entirely upon the deposition, ignition, and combustion of solid charcoal, but to produce this deposition from gaseous substances demands a high temperature."

This explanation of carbon separation in flames seems perfectly adequate and free from objection. There is, as we have seen, surrounding all ordinary hydrocarbon flames a shell of almost non-luminous combustion. The gas which passes upwards within this shell must be highly heated, and in the absence of air will be decomposed so as to deposit solid carbon. This carbon is intensely heated, and glows, and as it reaches the air will burn to form carbon dioxide. The fact that the upper parts of flame are the most luminous in itself indicates that the more we roast the gas the more do we separate the carbon; and there are other proofs, which I cannot stop to explain.

We have now got pretty well to the end of the explanation of the structure of ordinary luminous flames, and I will show you an experiment which epitomises the explanations that have been given.

We turn once more to the cone-separating apparatus, and use as fuel a substance particularly rich in carbon. This substance, benzene, is a liquid, so I shall have to vaporise it by means of a current of air. When I apply a light to this current of air strongly impregnated with benzene, we get, as you see, a very bright flame. This flame exhibits the usual structure. This is one extreme. Now I will reduce the amount of benzene vapour very rapidly without altering the air, and we shall get the other extreme, that is, a scarcely luminous flame consisting of one single cone. The whole of the combustion is now taking place in a single cone of flame. If I still further reduce the benzene, this flame enlarges slightly and becomes paler. There is now excess of air. A little less benzene still, and you see the flame rises from its perch and disappears; we have got past the limits within which combustion is possible. Let us next move in the other direction, and gradually increase the supply of benzene to the single cone. It becomes smaller and brighter as we proceed up to a certain point. At length we have evidently got more benzene than there is air to burn, and now appears the second cone at the top of the tube. By sliding the tubes we can unite the flame and make a Bunsen flame. Separating the cones again, let us add still more benzene. The result is very remarkable. The two cones remain intact, but stretching between them are thin luminous streaks of glowing carbon. The excess of benzene is being decomposed by the heat, so that the carbon separates and glows. The more benzene I add the broader do these streaks become, until eventually the inner cone ascends, the luminous streaks coalesce, and we have the ordinary luminous hydrocarbon flame.

I have now put before you the considerations and methods which will serve, I believe, for the elucidation of all problems of flame *structure*. I am not aware, at any rate, of any flame which does not accord with the general principles which I have explained to you.

There are many other flame problems besides that which relate to mere structure. Of these one of the most interesting concerns the colouration of flame. I will refer to it for a moment only to show how closely that question is connected with the points we have been discussing. I have here a gas flame to which I feed air until its yellow luminosity has disappeared. If I add to the air supply the fine spray of a dissolved copper salt, the flame assumes a green tint characteristic of the metal. This green tint seems to belong to the whole flame, but if we dissect it by the apparatus already so often used, we find that the green tint is developed only in the outer cone. It is due, in fact, to oxide of copper, which can only exist on the outside of the flame. Similar peculiarities are noticed with some other coloured flames, and it is hoped that their study, which leads us into the domain of spectrum analysis, will yield some interesting information on points which are at present very obscure.

I have directed your attention this evening to terrestrial flames of small dimensions, but in conclusion I should like to remind you that at one time there were probably quite other flames upon this earth. The globe we inhabit is in the process of cooling and of oxidation; at one time we believe, in fact we know, that it was incandescent. If we take a chemical retrospect and imagine as we recede in time our present cool earth becoming hotter, we may follow out some interesting changes. We should soon reach a temperature too high for the persistence of liquid water; our oceans would be evaporated and surround the globe as an envelope of steam. In remoter times and at higher temperatures this steam could not exist even as steam, but would be dissociated into hydrogen and oxygen. At that time, too, many of the elements now existing as oxides in the solid crust of the earth would be floating in a gaseous state in the vast atmosphere. Let us stop our retrospect at this point, and look towards the present with a cooling earth. At a certain point chemical combination must have begun in the fringe of the ancient atmosphere, and it must have been the scene of colossal chemical activities, the hydrogen and vaporous metals flashing into their oxides. On gravitating to hotter regions, these combinations may have been again undone, the elements sent again into circulation. How long such a period may have lasted we need scarcely stop to ask. If the retrospect is reasonable, it is enough. It is interesting to think how such an earth as we have pictured must have resembled the sun as we know it at the present day.

There was formerly a chemical theory of the sun, which ascribed both its heat and light to the act of chemical combination. That theory has long since been refuted and discarded, and with it ordinary laboratory chemistry banished from that luminary as altogether unsuited to its high temperature. There is cause, I think, to ask if this is quite warrantable. We know extremely little of chemistry at high temperatures, but if the sun could be shown to have its reasonable share of oxygen, we might well ask if its surface phenomena were not largely ascribable to ordinary chemical activities and of the nature of flames. It is certainly remarkable, when we consider the unity of plan in which heavenly bodies are seen more and more to move and have their being, that the sun should not exhibit the possession of its fair share of that element—oxygen—which has ruled the chemistry of the earth throughout all geological time and long precedent ages of its evolution. But this is ground which the terrestrial chemist must tread with care. He still has many unsolved problems lurking in the flame of a common candle, and flame, wherever we find it, is still a mystery.

"The power of *Fire* or *Flame*," says Carlyle, "which we designate by some trivial chemical name, thereby hiding from ourselves the essential character of wonder that dwells in it as in all things was with the old northmen Loke, a most swift subtle *Demon* of the brood of the Jötuns. The savages of the Landrones Islands too (say some Spanish voyagers) thought Fire, which they never had seen before, was a devil or god, that bit you sharply when you touched it, and that lived upon dry wood. From us, too," adds Carlyle, "no Chemistry, if it had not stupidity to help it, would hide that Flame is a wonder."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—On Monday, the 20th inst., Prof. E. B. Poulton, the President of the Ashmolean Society, gave a *conversazione* in the University Museum, which was numerously attended by members of the city and University, who were specially invited to meet the Local Executive Committee of the British Association. The features of the entertainment were: an interesting lecture on features in the past history of science in Oxford, by Mr. Falconer Madan; physical experiments, by Prof. Clifton and Mr. J. Walker; exhibits of various entomological specimens from the Hope Collection; glass-blowing, by Herr Zitzmann; living animals and museum preparations, by Dr. Benham and Mr. Goodrich; physiological exhibits, by Messrs. Pembrey, Gordon, and Howard; and many other exhibitions which cannot be noticed for want of space.

The Junior Scientific Club, whose proceedings have been hitherto published in a somewhat haphazard manner, have decided to issue a series of fortnightly numbers, each of which contains an account of the papers read at the previous meeting. The first of these was published on the 17th inst., and is in all respects a credit to its editor. It contains, besides abstracts of papers read by Messrs. M. H. Gordon, S. A. Simon, and W. J. Waterhouse, a syllabus of all the papers read before the club during the past year, an obituary, and notes on the distinctions gained during the past year, by present and former members of the club.

At a meeting of Convocation held on Tuesday last, Dr. Arthur Thomson, University Reader in Human Anatomy, was appointed Professor of Human Anatomy.

CAMBRIDGE.—Mr. M. R. James, of King's College, has been appointed Director of the Fitzwilliam Museum in succession to Prof. Middleton.

An election to an Isaac Newton studentship in astronomy, astronomical physics, and physical optics, will be held in the Lent Term 1894. The candidates must be B.A.s and under the age of twenty-five. The studentship is worth £200 a year for three years. Applications to be sent to the Vice-Chancellor by January 26, 1894.

A syndicate has been appointed for the purpose of obtaining specifications and tenders for the erection of the Sedgwick Memorial Museum of Geology, in accordance with the plan of Mr. T. G. Jackson.

An influential deputation waited upon the Chancellor of the Exchequer on Tuesday in order to place before him the necessity for continuing, and, if possible, increasing, the Parliamentary grant of £15,000, which was conceded to the University Colleges in 1889. Sir W. Harcourt said that though he was prepared to recommend the renewal of the grant, the present condition of public finances would not permit him to propose its increase.

SCIENTIFIC SERIALS.

American Journal of Science, November.—On New England and the Upper Mississippi basin in the glacial period, by James D. Dana. During the recent discussions concerning the unity or otherwise of the glacial epoch in North America, it has appeared that workers in the central and western portions have mostly advocated two glacial epochs, while New England geologists have been the chief advocates of unity. The author has not found any facts in New England geology that require for their explanation an appeal to two glacial epochs, but has found an explanation of the appearances which have led western geologists to that opinion. The cause of this sectional divergence is mainly meteorological. Even at the present time, the precipitation in the east is far above that of the west, and in the glacial epoch the difference must have been still greater, owing to the greater elevation of the east. The conditions of the ice-sheet in the interior being near the critical point, a small meteorological change, if long continued, might carry off the ice for scores or hundreds of miles from a southern limit, while the eastern border was all the time gaining in ice, or was making only a short retreat.—On the use of the name "Catskill," by John J. Stevenson. Mr. Darton's suggestion that the term Catskill should be applied to the whole of the Upper Devonian period is inappropriate, since Catskill has been shown to belong to an epoch only, whereas "Chemung" carries with it the conception of those physical and biological characteristics which mark the great closing period of the Devonian.—The finite elastic stress-strain function, by G. F. Becker. This is an investigation of finite stress and strain from a kinematical point of view, and of the function which satisfies the kinematical conditions consistent with the definition of an isotropic solid. The bearing of the theory upon finite sonorous vibrations is compared with the corresponding deductions from Hooke's incomplete law.—A larval form of Triarthrus, by C. E. Beecher. Since the discovery of antennæ and other appendages of this trilobite by Mr. W. D. Matthew, it has been possible, with the new material supplied to the Yale Museum, to trace its development back to the earlier stages. Larval specimens have been found in which the thorax is undeveloped and the cephalon predominates, while the other parts are not clearly differentiated. The larva is ovate in outline. The frontal margin is marked by a convex fold of the test. The axis is annulated. Near the lateral anterior margins are two slight elevations which may represent the palpebral lobes of the eyes.

American Journal of Mathematics, vol. xv. No. 4.—On toroidal functions, by A. B. Basset (pp. 287-302). The theory of these functions was first investigated by Prof. W. M. Hicks, in his discussion of the motion of circular vortex rings (*Phil. Trans.* 1881). The author considers that Prof. Hicks presented the subject in a somewhat complicated form, and the object of his own communication is to develop the subject, and to correct errors which he attributes to Prof. Hicks. The memoir appears to be on the lines of a communication Mr. Basset made to the London Mathematical Society (April 13, 1893).—Simple groups as far as order 660, by F. N. Cole (pp. 303-315). This is a continuation of a paper in vol. xiv., in which it was shown that the orders of simple groups between the limits 200 and 500 are restricted to two possibilities, 360 and 432. In the present memoir the order 432 is shown to be inadmissible, and the order 360 to furnish only one type of a simple group. Two other simple groups are shown to present themselves, of orders 504 and 660 respectively. The order 504 "seems hardly to have been recognised hitherto." It was a singular fact, pointed out at the November meeting of the London Mathematical Society, that this memoir anticipated some results in Prof. W. Burnside's notes on the theory of groups of finite order. The latter had evidently arrived at his result quite independently of Dr. Cole.—On the expansion of functions in infinite series, by W. H. Echols (pp. 316-320).—The elliptic inequalities in the lunar theory, by E. W. Brown (pp. 321-338). This is a resumption of the author's paper from p. 263.—On the multiplication of semi-convergent series, by F. Cajori (pp. 339-343). The writer's object is to extend results given by A. Voss, in the *Math. Ann.* (vol. xxiv. p. 44).—On certain ruled surfaces of the fourth order, by T. F. Holgate (pp. 344-386). An introductory section is historical, and refers to previous memoirs on the subject. The author considers those species of the surface of the fourth order which may be generated by two projective sheaves of planes of the second order. These admit of a trinodal quartic section, and are consequently of deficiency zero. The volume concludes with a note on the so-called quotient G/H in the theory of groups by Prof. Cayley, (pp. 387-8), and the index of contents.

SOCIETIES AND ACADEMIES.

LONDON.

Physical Society, November 10.—Prof. A. W. Rücker, F.R.S., President, in the chair.—A paper on the separation of three liquids by fractional distillation, by Prof. F. R. Barrell, G. L. Thomas, and Prof. Sydney Young, F.R.S., was read by Prof. Young. Accepting the results obtained by F. D. Brown in his experiments on the variation in the composition of the distillate from a mixture of two liquids, viz. that the relative quantities of the two substances in the vapour at any instant are proportional to the weights of the substances in the still, multiplied by the ratio of their vapour pressures, the authors write Brown's equation in the form $\frac{d\xi}{d\eta} = c\frac{\xi}{\eta}$, where ξ and η are the weights of the two liquids in the still, and c the ratio of their vapour pressures. Taking c as constant, the above equation is integrated, and from the resulting expressions curves are plotted showing the changes in composition that take place during the distillation. Assuming that a similar law holds for three liquids, A, B, and C, viz. $\frac{1}{a}\frac{d\xi}{\xi} = \frac{1}{b}\frac{d\eta}{\eta} = \frac{1}{c}\frac{d\zeta}{\zeta}$, the composition of the distillate at any instant is calculated. Taking $a = 4$, $b = 2$, and $c = 1$ (numbers nearly proportional to the vapour pressures of methyl, ethyl, and propyl acetates), numerous curves are plotted showing the progress of the separation at various stages of fractionation. These curves show distinctly that although fractions containing large proportions of the liquids A and C, of lowest and highest boiling points respectively, can be easily separated, the middle substance, B, is much more difficult to obtain in a state of purity. Consideration of these curves led the authors to see that by carrying out the fractionation in a particular way, it was possible to separate the mixture into two portions, one containing only A and B, and the other B and C. These mixtures of two liquids could then be fractionated in the usual manner. This process was carried out on a mixture of methyl, ethyl, and propyl acetates, the results of which are given in considerable detail in the paper. The remarkable agreement between the densities of the ethyl acetates obtained respectively from the mixtures A and B, and B and C, as well as the fact that the densities of the separated liquids were the same as before the mixing, show conclusively that the method employed was highly successful. Prof. Ramsay said the paper was a most valuable one, and would be a great aid to chemists. Distillations were usually carried out by mere "rule-of-thumb," with the result that absolutely pure liquids could rarely be obtained. The President inquired whether curves representing the progress of distillation could be constructed from the very complete experiments made, and so test the assumed law. Prof. Young thought this not possible from the numbers obtained. To test the law in this way would be very laborious.—A note on the generalisations of Van der Waals regarding "corresponding" temperatures, pressures, and volumes, was read by Prof. S. Young. In November 1891 the author read a paper on the same subject (*Phil. Mag.* February 1892), and gave the critical molecular volumes of some twelve substances as calculated by M. Mathias. Since then a few small errors have been found in the calculation, and the authors' corrected values are now given. The vapour pressures, molecular volumes and critical constants of ten esters (methyl formate, acetate, propionate, butyrate, and isobutyrate, ethyl formate, acetate and propionate, and propyle formate and acetate) have recently been determined (Trans. Chem. Soc. lxiii. p. 1191). In the present paper the absolute temperatures and volumes of the twelve substances are given in terms of their critical constants, and tables given showing, respectively, the ratios of boiling points (abs. temps.) at corresponding pressures, to absolute critical temperatures; the ratios of volumes of liquid at corresponding pressures to the critical volumes, and ratios of volumes of saturated vapours at corresponding pressures to critical volumes, for the halogen derivatives of benzene, carbon tetrachloride, stannic chloride, ether; methyl, ethyl, and propyl alcohols, and acetic acid; and the extreme values for the ten esters previously mentioned. Whilst showing fair agreement with each other, the differences between them exceed errors of experiment. The ratios also indicate that the substances can be arranged in four groups, thus tending to show that molecular weight and chemical constitution have some influence on the results. The differences found would probably result from the presence of complex molecules, such as are known to exist in acetic acid. If Van der Waals's generalisations were strictly true, the ratio $\frac{pv}{T}$ at the critical point should be constant for all substances, as also the ratio $\frac{D}{D'}$ of the actual to the theoretical density (for a perfect gas) at the critical point. On comparing these quantities only a rough approximation is found, but the grouping of the compounds is again well marked. Prof. Ramsay was not sure that the existence of complexes would alter the molecular volume in the liquid state, for liquids seem very compact. Experiments on the surface energy of liquids had proved that complex molecules do exist in the alcohols and acetic acid. Dr. Young's conclusions were therefore confirmed by experiments of an entirely different nature. Prof. Herschel was gratified to see Van der Waals's theory so well borne out in liquids, and hoped to see it extended to solids. The recent researches of Prof. Robert Austen on alloys seemed to point in this direction. Mr. Rogers said molecular complexes do exert an influence on the properties of substances, as had been shown by Prof. Thorpe's viscosity experiments. Van der Waals's generalisations should therefore be looked at from a chemical as well as a physical point of view. The President thought the number brought forward showed fair agreements, especially when it was remembered that Van der Waals took no account of complex molecules. Contrary to Prof. Ramsay, he would rather expect aggregation to affect the molecular volumes in the liquid state, for only about one-fifth the space was supposed to be occupied by matter. On the other hand, the relatively small contraction of liquids on cooling did not support this view.—An instrument for drawing conic sections was exhibited and described by Mr. J. Gillett. This consists of a spindle inclined to a plane board, and a tube fixed to the spindle at an angle. A pencil which passes through the tube traces out a cone in space as the spindle is turned, and on sliding the pencil through the tube, so as to keep its point against the plane, the point traces out a conic, the section of the cone made by the plane of the board. A circle, ellipse, parabola, or hyperbola, can be drawn according to the inclination of the spindle to the board. Prof. Henrici said a similar instrument had been described in an Arabian manuscript a thousand years old, and has been independently re invented by both a German and an Italian mathematician. He thought the fact of the angle between the spindle and the tube in Mr. Gillett's instrument not being adjustable was a disadvantage. Mr. Inwards and Prof. Herschell also took part in the discussion, to which Mr. Gillett replied.

Geological Society, November 8.—W. H. Hudleston, F.R.S., President, in the chair.—The following communications were read :—The geology of Bathurst, New South Wales, by W. J. Clunies Ross. After sketching the physiography of the Bathurst district, the author described in detail its stratigraphy. The oldest sedimentary rocks are Silurian, but the floor on which they rest is unknown, and the author stated that it was probably fused up and incorporated in the granite, which is described in the paper. The Silurian rocks may have been folded before the granite was erupted ; in any case, the granite produced a zone of contact-metamorphism, whilst almost all the Silurian rocks may be considered to be examples of regional metamorphism, though the agents producing the metamorphism were least active to the east of Bathurst, where the Silurian limestones are very little altered. An anticlinal was probably produced at the time of the granitic intrusion. After a time there was subsidence, but at first it need not have been very extensive, since the Devonian conglomerates, sandstones, and shelly limestones were probably deposited in a comparatively shallow sea. They contain *Lepidodendron australe.* At Rydal they abut against the uplifted Silurian rocks of the Bathurst area. At the end of Devonian times there appears to have been a long interval, during which both Silurian and Devonian rocks were greatly denuded, and the granite exposed in places. The Upper Carboniferous and Permian rocks were deposited in the Lithgow district, but it is doubtful if they ever extended to Bathurst. There is nothing to show what happened in this region during Mesozoic and early Tertiary times. The Hawkesbury Sandstone (probably Triassic) may have approached nearer to Bathurst than it does now. In late Tertiary times stream-deposits were formed on the granitic rocks, and afterwards covered with thick basaltic lava-flows, which have since undergone much denudation. A discussion followed, in which the President, the Rev. H. H. Winwood, and Mr. J. E. Marr took part.—The geology of Matto Grosso (particularly of the region drained by the Upper Paraguay), by Dr. J. W. Evans. The district includes a portion of the Brazilian hill-country, and also of the low-lying plains to the south-west The rocks principally dealt with are unfossiliferous, and of unknown age, except that they appear to be older than the Devonian. They may be classified as follows :—(5) Matto Shales (relations not shown) ; (4) Rizama Sandstone (perhaps some unconformity) ; (3) Curumbá and Arara Limestones (very marked unconformity) ; (2) Cuyabá Slates (strong unconformity) ; (1) ancient crystalline rocks. The Devonian and later rocks were briefly described. The President, Mr. Spencer Moore, fellow-traveller with Dr. Evans in Matto Grosso, Mr. H. Bauerman, and Mr. R. D. Oldham spoke on the subject of the paper.—Notes on the occurrence of mammoth-remains in the Yukon district of Canada and in Alaska, by Dr. George M. Dawson, C.M.G., F.R.S. In this paper various recorded occurrences of mammoth-remains were noted and discussed. The remains are abundant in, if not strictly confined to, the limits of a great unglaciated area in the north-western part of the North American continent ; whilst within the area which was covered by the great ice-mass which the author has described as the Cordilleran glacier, remains of the mammoth are either entirely wanting or are very scarce. At the time of the existence of the mammoth the North American and Asiatic land was continuous ; for an elevation of the land sufficient to enable the mammoth to reach those islands of the Bering Sea, where these bones have been found, would result in the obliteration of Bering Straits. The bones occur, along the northern coast of Alaska, in a layer of clay resting on the somewhat impure "ground-ice formation" which gives indications of stratification ; and above the clay is a peaty layer. The author considered this "ground-ice" was formed as a deposit when more continental conditions prevailed, by snow-fall on a region without the slopes necessary to produce moving glaciers. The mammoth may be supposed to have passed between Asia and America at this time. At a later date, when Bering Straits were opened and the perennial accumulation of snow ceased on the lowlands, the clay was probably carried down from the highlands and deposited during the overflow of rivers. Over this land the mammoth roamed, and wherever local areas of decay of ice arose bogs would be produced which served a veritable sink traps. The author considered it probable that the accumulation of "ground-ice" was coincident with the second (and latest) epoch of maximum glaciation, which was followed by an important subsidence in British Columbia. In the discussion of the paper, Sir Henry Howorth remarked upon the long and careful survey of North-West America which has been made by the author, and upon the value of the conclusions which he has come to : firstly, in regard to the absence of ancient glaciation in Alaska and its borders ; secondly, in regard to the existence of a great glacier in the Cordilleras, whose products are quite independent of and have nothing to do with the Laurentian drift ; and thirdly, in regard to the distribution of the mammoth. It was a new fact to him, and one of great importance, that mammoth-remains had occurred in Unalashka and the Pribilof Islands in Bering Straits, proving that in the Mammoth age there was a land bridge here, as many inquirers had argued. It would be very interesting to have the western frontier defined where the mammoth-remains cease to be found. It would be very interesting to know how far south on the west of the Cordilleras the true mammoth, as distinguished from *Elephas Columbi*, has occurred. Regarding one conclusion of Dr. Dawson's, Sir Henry could not agree with him, namely, about the age of the strata of ice sometimes found under the mammoth-beds in Alaska as they have been found in Siberia. The speaker was of opinion that this ice had accumulated since the beds were laid down, and was not there when the mammoth roamed about in the forests where he and his companions lived. Humus and soil cannot accumulate upon ice except as a moraine, and there are no traces of moraines or of great surface-glaciation in Alaska and Siberia. Nor could either the flora or fauna of the memmoth age have survivid conditions consistent with the accumulation of these beds of ice almost immediately below the surface, or consistent with their presence there. The speaker considered that these beds were due to the filtration of water in the summer down to the point where there is a stratum of frozen soil, through which it cannot pass and where it consequently accumulates, freezes, raises the ground, and in the next season grows by the same process until a thick bed of ice has been formed. The evidence goes to show that the present is the coldest period known in recent geological times in Siberia and Alaska, and that the period of the mammoth and its companions was followed and not preceded by an Arctic climate where its remains occur. Dr. H. Woodward remarked that the most interesting point in Dr. Dawson's paper was the mention of the remains of mammoth on the Aleutian Islands, proving that this was the old high road for this and other mammals from Asia into North America in Pleistocene times.

Linnean Society, November 2.—Prof. Stewart, President, in the chair.—The secretary having read a list of the donations to the library since the last meeting, the President moved that the thanks of the society be given to the donors and to Lady Arthur Russell for the valuable collection of engraved portraits of naturalists which she has been so good as to present to the society in the name of her husband, the late Lord Arthur Russell, a motion which was passed unanimously. The President then referred to the improvement which had been carried out during the recess in the society's apartments by the introduction of the electric light, for which they were indebted to the liberality of the treasurer, Mr. Crisp, who on former occasions had shown himself so generous a benefactor, and moved that the hearty thanks of the society be given to Mr. Crisp for his munificent present. The resolution was carried by acclamation. Referring to the deaths of Fellows of the society which had occurred since the last meeting, the President alluded especially to the Rev. Leonard Blomefield, whose connection with the society, extending over seventy years, had recently been made the subject of a congratulatory address, to Mr. F. Pascoe, the distinguished entomologist, and to Mr. George Brook, whose lamented decease had caused the vacancy in the Council which they now had to fill. The ballot having then been taken for the election of a new councillor in the place of Mr. George Brook, deceased, Mr. Henry Seebohm was declared to have been elected.—Mr. George Murray exhibited and made remarks on a series of seaweeds mounted on lantern slides, some of which were new to Great Britain. He also showed some specially prepared tins which were recommended for collecting purposes, but which in the opinion of some present would be likely to become speedily useless from oxidisation. —Mr. Holmes showed some new British marine algæ, and made remarks on their affinities.—Dr. Prior exhibited the fully developed fruit of *Pyrus japonica* from Rogate, Sussex, seldom seen, although the plant is common, and alluded to its use as a conserve if it could be obtained in sufficient quantity.—Mr. Spencer Moore read a paper on the phanerogamic botany of an expedition to Mato Grosso, upon which he acted as botanist. Starting from Cuyaba, the expedition first visited the Chapada Plateau, to the east of that city, where many plants were collected. Thence a journey was made to the new settlement of Santa Cruz, on the Paraguay, about half-way between Villa Maria and Diamantino. The flora here is of mixed character, nearly 37 per cent. of the plants being common to tropical South America, upwards of 27 per cent. occurring in the N. Brazil Guiana province of Engler, with 20·5 per cent common to that province and the S. Brazilian, and only 13 per cent. of S. Brazilian types. From Santa Cruz a party penetrated through the primeval forest lying to the north, and reached the Serra de Sapirapuan. The forest flora is markedly Amazonian in character, nearly 50 per cent. of the plants being natives of Amazonia or of the neighbouring countries within the N. Brazil Guiana province, or related thereto, while the proportion of species common to tropical America falls to rather more than 28 per cent. the S. Brazilian element being present only to the extent of 9·5 per cent. Returning to Santa Cruz, the Rio Bracisto was partly explored, and the Paraguay ascended to the neighbourhood of Diamentino. The party then came down the Paraguay to the Corumba, where many plants of interest were found. The expedition was partly disbanded at Asuncion. Among the Amazonian plants found at Santa Cruz, or in the forest, may be mentioned *Randia Ruiziana*, *Bertiera guianensis*, the Loranthad *Oryctanthes ruficaulis*, *Cattleya superba*, *Epidendrum imatophyllum*, *Rodriguezia secunda*, &c. The collections comprise close upon 700 species, of which rather more than 200 were considered to be new, and referable to eight new genera. The southward extension of the Amazonian flora to a latitude well within the Paraguay River system was regarded as a noteworthy feature.—On behalf of Mr. G. M. Thomson, of Dunedin, N.Z., Mr. W. Percy Sladen read a paper on a new freshwater Schizopod from Tasmania, illustrating his remarks with graphic sketches on the blackboard to indicate its affinities and differences.

Entomological Society, November 8. — Henry John Elwes, President, in the chair.—Mr. F. Merrifield exhibited some low-temperature forms of *Vanessa atalanta*, artificially produced, which showed a great reduction in the area of the scarlet bands on the wings, and a great increase in the area of the white and bluish markings.—Prof. E. B. Poulton, F.R.S. described and illustrated, by means of a map, a simple method for showing the geographical distribution of insects in collections. Below the name-label of the genus, and of each species, were placed coloured slips of such a size as to be distinctly visible at a distance, and the colours, with one exception, corresponded with those made use of in the map at the beginning of vol. i. of Dr. A. R. Wallace's "Geographical Distribution of Animals." The exception referred to was the Palæarctic region, which was coloured blue, instead of pale brown as in the original. Framed maps of the same kind, and coloured in the same way as the one he exhibited, were to be placed in museums, so as to be readily seen from various groups of cabinets. In these maps the names of the regions, and numbers of the sub-regions, were distinctly printed, so that they could be read at a considerable distance. Prof. Poulton added that the method he had described was being gradually introduced into the Hope Collections at Oxford. Mr. McLachlan, F.R.S., stated that a somewhat similar plan to that described for showing the geographical distribution of insects has been adopted in the Brussels Museum by M. Preudhomme de Borre. Mr. W. F. H. Blandford, Dr. D. Sharp, F.R.S., Mr. C. J. Gahan, Mr. C. O. Waterhouse, Mr. Osbert Salvin, F.R.S., Prof. Poulton, and the President continued the discussion.—Dr. Sharp read the following extract from Dr. Livingstone's "Narrative of an Expedition to the Zambesi," and stated that he was indebted to Mr. Gahan for calling his attention to it:—"We tried to sleep one rainy night in a native hut, but could not because of attacks by the fighting battalions of a very small species of *Formica*, not more than one-sixteenth of an inch in length. It soon became obvious that they were under regular discipline, and even attempting to carry out the skilful plans and stratagem of some eminent leader Our hands and necks were the first objects of attack. Large bodies of these little pests were massed in silence round the point to be assaulted. We could hear the sharp, shrill word of command two or three times repeated, though until then we had not believed in the vocal power of an ant; the instant after we felt the storming hosts over head and neck."—Prof. Poulton read a paper entitled "On the sexes of larvæ emerging from the successively laid eggs of *Smerinthus populi*." Mr. Merrifield, Dr. Sharp, and the President took part in the discussion which ensued.—Mr. W. L. Distant communicated a paper entitled "On the Homopterous genus *Pyrops*, with descriptions of two new species."—The President read a paper, written by himself and Mr. J. Edwards, entitled "A revision of the genus *Œneis*," which he characterised as the most cold-loving genus of butterflies. He also exhibited his complete collection of species of this genus. A long discussion ensued, in which Prof. Poulton, Mr. McLachlan, Mr. Salvin, Mr. Bethune-Baker, the Rev. Dr. Walker, Mr. Kirby, Mr. Merrifield, Mr. Barrett, Mr. Blandford, Dr. Sharp, and Mr. Jacoby took part.

Zoological Society, November 7.—Sir W. H. Flower, K.C.B., F.R.S., President, in the chair.—Mr. Sclater read some notes on the most interesting animals he had seen during a recent visit to the Zoological Gardens of Stuttgart, Frankfort, and Cologne.—An extract was read from a letter addressed to the Secretary by Mr. J. G. Millais, relating his endeavours to obtain specimens of the White Rhinoceros (*Rhinoceros simus*) in Mashunaland.—A communication was read from Babu Ram Bramha Sányál, describing a hybrid monkey of the genus *Semnopithecus*, born in the Zoological Gardens, Calcutta.—Mr. Tegetmeier exhibited a specimen of a hybrid grouse between the blackgame (*Tetrao tetrix*) and the red grouse (*Lagopus scoticus*). — Mr. Boulenger read a paper on a Nothosaurian reptile from the Trias of Lombardy, apparently referable to *Lariosaurus*. His description was based on a small, nearly perfect specimen from Mount Perledo, showing the ventral aspect, belonging to the Senckenberg Museum in Frankfort-on-Main, which had been entrusted to him by the directors of that institution, and was exhibited before the meeting. The author pointed out the presence of a series of minute teeth on the pterygoid bones, and of an entepicondylar (ulnar) foramen in the humerus. The number of phalanges was 2, 3, 4, 4, 3 in the manus, and 2, 3, 4, 5, 4 in the pes; the terminal phalanx was flattened and obtusely pointed, not claw-shaped. In discussing the affinities of this reptile the author stated that the *Lariosaurus* described by Diecke did not appear to be generically distinguishable from the *Neusticosaurus* of Seeley, which he referred to the *Lariosauridæ*, regarding that family as intermediate between the *Mesosauridæ* and the *Nothosauridæ*, though nearer the latter. The *Mesosauridæ*, in his opinion, formed one sub-order, the *Lariosauridæ* and *Nothosauridæ* together a second sub-order, of the order *Plesiosauria*.—Dr. A. Günther, F.R.S., read a second report on specimens of reptiles, batrachians, and fishes transmitted by Mr. H. H. Johnston, C.B., from British Central Africa. Dr. Günther also read descriptions of some new reptiles and fishes, of which specimens had been obtained on Lake Tanganyika by Mr. E. Coode-Hore.—Mr. Edgar A. Smith gave an account of a collection of land and freshwater shells transmitted by Mr. H. H. Johnston, C.B., from British Central Africa. The specimens in this collection, obtained by Mr. R. Crawshay from Lake Mweru, were almost all new to science.—Mr. Edgar A. Smith also read descriptions of two new species of shells of the genus *Ennea*.—A communication was read from Dr. Arthur G. Butler, containing an account of two collections of Lepidoptera sent by Mr. H. H. Johnston, C.B., from British Central Africa.—A communication was read from Mr. Edwyn C. Reed, containing a list of the Chilian Hymenoptera of the family *Oxyneridæ*, with descriptions of some new species.—A communication from Prof. Newton, F.R.S. contained the description of a new species of bird of the genus *Drepanis*, discovered by Mr. R. C. L. Perkins in the island of Molokai, Sandwich Islands.

PARIS.

Academy of Sciences, November 13.—M. Lœwy in the chair.—On the new star of 1892, T Aurigæ = 1953 Chandler, by G. Bigourdan (see our Astronomical Column).—Observations of the comets 1893 II. (Rordame) and 1893 *c* (Brooks, 1893, October 16), made at the Paris Observatory, by the same author. Observations of position are given, extending from November 6 to 8.—Elements of Brooks's comet, by M. Schulhof. The elements of this comet closely resemble those of the comet 1864 I.—Control of the trunnions of a meridian instrument by Fizeau's interferential method, by Maurice Hamy.—Measurement of the absorption of light by thin laminæ possessing metallic reflection, by M. Salvador Block.—Determination of the true atomic weight of hydrogen, by M. G. Hinrichs. Taking as abscissæ the weights of hydrogen employed by Keiser, Dittmar, and Morley, in their respective determinations of the atomic weight of hydrogen, and as coordinates the values found, the author has obtained a diagram which indicates that the values vary according to the weight of gas used in the experiments. In his opinion this proves that the ratio of H to O is absolutely as 1 is to 16.—On baryta emetic, by M. E. Maumené.—On the production of sucrose during the fermentation of barley, by M. L. Lindet. The experiments described indicate that sucrose and invert-sugar increase proportionally to the decrease of starch during the fermentation of barley.—On the nitrification of prairie lands, by MM. J. Dumont and J. Crochetelle. The following conclusions seem to be justified by the experiments: (1) Nitrification is forwarded in soils rich in humus by the addition of small quantities of potassium carbonate (2 or 3 parts per 1000); on the other hand, large quantities of the carbonate are hurtful. (2) Potassium sulphate is efficacious, and favours the production of nitrates when about seven or eight parts per thousand are used. (3) Chloride of potassium only exercises mediocre action. (4) Sodium carbonate does not appear to favour nitrification.—On the influence of mineral poisons on lactic fermentation, by MM. A. Chassevant and C. Richet. This paper is in continuation of a previous one. The authors divide the toxic action of metallic salts on lactic fermentation into two parts, terming the dose that retards the reproduction and pullulation of the ferment *antigénétique*, while that which arrests functional activity is called *antibiotique*. It appears that the antigentic dose may be as much as three times greater than the antibiotic dose, though for certain metals the two quantities are the same.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, NOVEMBER 23.

ROYAL SOCIETY, at 4.30.—On the Photographic Arc Spectrum of Electrolytic Iron: Prof. Lockyer, F.R.S.—Magnetic Observations in Senegambia: Prof. Thorpe, F.R.S., and P. L. Gray.—Alternate Current Electrolysis: Dr. Hopkinson, F.R.S., E. Wilson, and F. Lydall.—A Certain Class of Generating Functions in the Theory of Numbers: Major MacMahon, F.R.S.—On the Whirling and Vibration of Shafts: S. Dunkerley.—On Plane Cubics: Charlotte Angas Scott.
INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—The Electrical Transmission of Power from Niagara Falls: Prof. Geo. Forbes, F.R.S. (Discussion).
SANITARY INSTITUTE, at 8.—Metallic Dusts, Cutlery, Tool Making, and other Metal Trade: Dr. Sinclair White.

FRIDAY, NOVEMBER 24.

PHYSICAL SOCIETY, at 5.—The Magnetic Shielding of Concentric Spherical Shells: Prof. A. W. Rücker, F.R.S.—The Action of Electro-Magnetic Radiation on Films containing Metallic Powders: Prof. G. M. Minchin.
AMATEUR SCIENTIFIC SOCIETY, at 7.—Exhibition of Lantern Slides of Recent Photographs of Volcanoes: L. W. Fulcher.——At 8.—The Dawning of Life: J. Wilson Wiley.

SATURDAY, NOVEMBER 25.

ROYAL BOTANIC SOCIETY, at 3.45.

SUNDAY, NOVEMBER 26.

SUNDAY LECTURE SOCIETY, at 4.—Curiosities of Bird Life: Dr. R. Bowdler Sharpe.

MONDAY, NOVEMBER 27.

ROYAL GEOGRAPHICAL SOCIETY (at the University of London, Burlington Gardens, W.), at 8.30.—Antarctic Exploration: Dr. John Murray.

TUESDAY, NOVEMBER 28.

INSTITUTION OF CIVIL ENGINEERS, at 8.—The Tansa Works for the Water-Supply of Bombay: William J. B. Clerke.—The Baroda Water-Works: Jagannath Sadasewjee.—The Water-Supply of Jeypore, Rajputana: Colonel S. S. Jacob.—On the Design of Masonry Dams: Prof. Franz Kreuter. (Discussion.)

THURSDAY, NOVEMBER 30.

SANITARY INSTITUTE, at 8.—Textile Manufactures, Silk, Cotton, Woollen, and Linen Industries: Dr. J. T. Arlidge.

FRIDAY, DECEMBER 1.

GEOLOGISTS' ASSOCIATION, at 8.—Notes on a Discovery of Fossils at Little Stairs Point, Sandown Bay, Isle of Wight: Thos. Leighton.—Notes on the Sharks' Teeth from British Cretaceous Formations: A. Smith Woodward.—The Breaking-up of the Ice on the St. Mary River, Nova Scotia, and its Geological Lessons: Geoffrey F. Monckton.
INSTITUTION OF CIVIL ENGINEERS, at 7.30.—Forms of Tensile Test-Pieces: Leonard H. Appleby.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Alembic Club Reprints, No. 4:—Foundations of the Molecular Theory: J. Dalton, &c. (Edinburgh, Clay).—Practical Agricultural Chemistry: J. B. Coleman and F. T. Addyman (Longmans).—Methods of Practical Hygiene, 2 Vols.: Prof. Lehmann, translated by W. Crookes (K. Paul).—Suicide and Insanity: Dr. S. A. K. Strahan (Sonnenschein).—Mechanics of Hoisting Machinery: Dr. J. Weisbach and Prof. G. Herrmann, translated by K. P. Dahlstrom (Macmillan).—In the High Heavens: Sir R. S. Ball (Isbister).—Collected Mathematical Papers of Arthur Cayley, vol. vi. (Cambridge University Press).—Cancer, Sarcoma, and other Morbid Growths considered in Relation to the Sporozoa: J. J. Clarke (Baillière).—International Maritime Congress, London, 1893, Sections 1 to 4. Minutes of Proceedings and General Report (Unwin Brothers).—Report of the Commissioner of Education for the Year 1889-90, vols. 1 and 2 (Washington).—Royal Natural History, vol. 1, part 1: edited by R. Lydekker (Warne).—The Beauties of Nature: Sir J. Lubbock, 5th edition (Macmillan).

PAMPHLET.—Owens College Museum Hand-books—General Guide to the Contents of the Museum, 2nd edition (Manchester, Cornish).

SERIALS.—Bulletin Astronomique, October (Paris).—Meteorological Record, vol. xiii. No. 49 (Stanford).—Quarterly Journal of the Royal Meteorological Society, October (Stanford).—Journal of the College of Science, Imperial University, Japan, vol. 6, part 3 (Tokyo).—Journal of the Franklin Institute, November (Philadelphia).—Proceedings of the Aristotelian Society, vol 2, No. 2, Part 2 (Williams & Norgate).—Brain, Part 63 (Macmillan).—Boletin de la Sociedad Geográfica de Madrid, Tomo 35, Nos. 1, 2, 3 (Madrid).—Journal of Marine Zoology and Microscopy, No. 1 (Jersey, Sinel and Hornell).—Journal of the Polynesian Society, vol. 2, No. 3 (Wellington).

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