THURSDAY, JUNE 13, 1895.

MASKELYNE'S CRYSTALLOGRAPHY.

Crystallography, a Treatise on the Morphology of Crystals. By N. Story-Maskelyne, M.A., F.R.S., Professor of Mineralogy, Oxford. 521 pp. and xii. pp., 398 figures, 8 plates, 8vo. (Clarendon Press, 1895.)

AFTER wandering in the desert for considerably more than forty years, the English student of crystallography is at length brought within sight of the promised land; it is true that guides have been offered to him in the interval, but they have spoken in strange tongues, and have sometimes been mere dust-clouds of unnecessary formulæ and notations, calculated rather to bewilder than to lead.

The long-expected treatise of Prof. Maskelyne will be found to fully justify the anticipations with which it has been awaited; those who desire to study crystals and crystallography are no longer confronted by the want of an authoritative handbook, and need no longer lose themselves among the works of foreign authors. The English books hitherto available are few in number. The remarkable "Treatise" and "Tract" of the late Prof. Miller established, in the most rigid manner, a mathematical basis for the science, and must always remain standard works—masterpieces of precision. These two books contain, in a few pages, all that is essential; but being condensed into a bald sequence of theorems, they appeal almost exclusively to the mathematician. Mr. Gurney's little introduction to the subject, and the textbook of the late Prof. G. H. Williams, are excellent stimulants to the beginner, but will not suffice for the more advanced student; the present work supplies most completely what was wanted.

It is easy to state what is required from the practical point of view in a text-book on the morphology of crystals: the learner desires to know what are the forms of crystals, and how they differ from other figures; he must be told how they are determined and described, and for educational purposes it is especially important that the geometrical relations should be established by simple methods of proof from intelligible principles.

All this the present volume satisfactorily accomplishes. A crystal is considered to be, for morphological purposes, a complex of planes which obey a simple geometrical law—that known as the law of rational indices, and the early part of the book is consequently devoted to the investigation of such a complex, and shows, further, how it is denoted and represented; this involves a series of propositions relating to axes and indices, to stereographic projection, and to the relations of zones. The idea of symmetry superimposed on such a geometrical complex is considered in the two following chapters, and the six systems, having thus been established, are considered in detail in chapter vii.

Although this treatise will certainly not prove attractive to readers who are totally unfamiliar with mathematical methods and conceptions, yet it succeeds in giving simple and elegant proofs (many of them new) of all the necessary theorems without introducing any advanced mathematics. At the same time the book is far from being a geometrical study. The eighth and ninth chapters, comprising more than one hundred pages, are devoted to the practical methods employed in the goniometrical measurement and calculation of angles, and to the manner in which crystals are depicted by projections and perspective drawings; further, each crystalline type is represented by copious examples from minerals and chemical products, and frequent references will be found to the bearing of certain physical investigations upon the points discussed. Such a complete treatment, for example, as is here given of the twinning of diamond, quartz, and felspar is infinitely more satisfactory than the meagre sketch usually found in text-books, whether of crystallography or mineralogy.

But the book contains far more than is indicated above; it is, at least so far as regards certain aspects of the subject, a really philosophic treatise, of which the originality and peculiar interest will be best appreciated by a reader who refers to the discussion of crystalloid symmetry contained in the fifth and sixth chapters. Here the nomenclature is to a large extent new, although some of the terms have become familiar in Mr. Gurney's little book, where they are mentioned as due to Prof. Maskelyne. Many of them are invaluable aids to precision; haplo- and diplo-hedral, meta- and anti-strophic, holo- and hemi-systematic, for example, are terms which avoid much circumlocution, introduce clear conceptions, and once used can scarcely be dispensed with.

The chapters dealing with symmetry must have been familiar to Prof. Maskelyne's pupils many years ago, at a time when the importance of this subject was by no means recognised; to him is undoubtedly due the credit of first in this country directing to crystal symmetry the consideration which it deserves, which, moreover, it failed to receive in the methods of Miller. In the present book symmetry is of cardinal importance; the systems are deduced from a discussion of the possible forms which may be assumed by the systematic triangle, *i.e.* the triangle formed by the intersection of a sphere with three adjacent planes of symmetry; the mero-symmetrical divisions of the systems are then considered as resulting from the possible "presence or absence of certain faces consequent upon the abeyance of the actual symmetrical character of planes which are otherwise potentially planes of symmetry"; in other words, the symmetry of the system is regarded as a complete type latent in the hemihedral and tetartohedral crystals, and exercising a symmetrical influence by virtue of the axes of symmetry, which are themselves the result of dormant planes of symmetry.

Now in recent years new methods of treating crystallography, also mainly from the point of view of symmetry, have been developed in other countries; to avoid criticising the present treatise in the light of the newer teaching, would be to shirk a responsibility obviously imposed upon a conscientious review.

One method frames a theory of crystal structure which shall accord with the observed homogeneity of crystals, finds in how many ways such structures may be symmetrical, and so deduces the systems; such is the course pursued in Mallard's magnificent treatise upon the basis of Bravais' theory of structure, and a similar method might be based upon a more extended theory, such as that of Sohncke or that of Fedorow and Schönflies, and would lead to all known varieties of crystal symmetry. Such a deductive method is not, however, one which has ever commended itself to scientific teachers in this country, and it is not one which can be logically adopted in a book dealing solely with the morphology of crystals.

The second method is the one introduced by Gadolin; it inquires in how many ways a figure obeying the law of rational indices can be symmetrical according to the number and distribution of its planes and axes of symmetry, and it leads satisfactorily to all the known varieties of crystals. It was employed by Liebisch, and has been carried to its utmost extreme in the new edition of Groth's "Physikalische Krystallographie," where the systems are geometrically little more than artificial groups constructed by synthesis of the various types, the conception of mero-symmetry being completely abandoned. Prof. Maskelyne treats of planes before axes of symmetry, and regards the latter as begotten by the former; accordingly he is compelled to introduce the idea of mero-symmetry as a second empirical law, whereas the method of Gadolin requires the one law of rational indices alone. In the opinion of the present writer, Gadolin's is the most, indeed the only, logical process. It must, however, be confessed that the method of Prof. Maskelyne possesses a simplicity which is important from the educational point of view, and might alone be sufficient justification for its use; that he has considered and rejected other possible courses is clear from the discussion on p. 171, which leads to the following suggestive remark: "It is, however, evident that the whole treatment of crystallographic symmetry on the assumption of planes and axes of symmetry, actual or potential, represents a geometrical abstraction; an abstraction that needs for its development and due explanation a complete science of position applied to the molecular mass-centres."

In the preface it is stated that the greater part of the present treatise has long been in print; this being the case, the earlier part must inevitably be somewhat out of touch with recent discovery, and since there is no list of errata, statements which are not, like the geometrical propositions, unassailable, must be received with due caution. Thus milk-sugar is stated to be orthorhombic, it has recently been proved mono-symmetric; the whole of § 314 should now be cancelled. Again, § 140 must be read in the light of § 266. Cuprite is described both as holo-symmetrical and as hemi-symmetrical; but the intelligent reader will find the most important of such contradictions implicitly corrected in a table of crystalline types, with authentic examples, given on p. 502. This table is introductory to eight useful plates which deserve special attention, since they represent all the varieties of merohedra and their relations, and render the previous descriptions easily intelligible.

The appearance of this book is an interesting event in the history of crystallography. The volume stands as a striking and permanent record of the original manner in which this science has for many years been treated by the Oxford Professor in lectures, of which the substance is now for the first time made public. By those who have had the privilege of personal acquaintance with his teaching, it will be welcomed as the familiar echo of a style of exposition singularly adapted to kindle enthusiasm for an abstruse subject, and by the scientific public, as an authoritative treatise on a science of which the growing importance is continually becoming more fully recognised.

H. A. Miers.

THE STUDY OF STEREOCHEMISTRY.

Stéréochimie. Exposé des théories de Le Bel et Van't Hoff. Par E. G. Monod, avec une préface de M. C. Friedel. (Paris: Gauthier-Villars et Fils, 1895.)

This is a small book of 162 pages which gives a clear account of the fundamental ideas upon which is founded the modern doctrine of chemistry in space, which sprang, as every one knows, out of Pasteur's classical researches on the relation between optical activity and crystalline form. Much fault need not be found with this book because it contains rather dogmatic statements of debatable propositions, but we venture to think the treatment of the subject too sketchy and superficial to afford much real help to the student.

M. Monod's little book relates only to the stereochemistry of carbon, and the isomerism of nitrogen compounds is not referred to. Now this department of theoretical chemistry is one which should be entered by the student at a comparatively advanced stage of his progress, when he is already familiar with the more important facts upon which the theory is based. It seems doubtful, therefore, whether so scanty an outline as this will supply what is wanted by students at this stage. They will desire to be told not only that a certain number of groupings are possible with a stated number of carbon atoms, which is usually obvious enough, but they will require to be told something of the secondary hypotheses with which the fundamental idea has become encrusted. For example, the union between two carbon atoms joined by a single bond is shown (p. 17) to be "mobile," that is, each carbon is supposed to be able to rotate, together with its attached radicles, round the axis joining the two carbons; but the student is left at that point to wonder why it should rotate at all. It is only much later (p. 63), in connection with the isomerism of fumaric and maleic acids, that reference is made to the doctrine of attractions between the radicles associated with carbon atoms adjacent to each other. In this case it is not justifiable to say that the attraction of CO_2H for H is *evidently* greater than that of CO_2H for CO_2H or H for H. There is nothing *evident* about the statement, which is almost purely hypothetical, such evidence as does exist tending almost as much one way as the other.

Throughout the book the conventional tetrahedron is the symbol used, and we have not been able to find any account of Wunderlich's hypothesis as to the configuration and union of carbon atoms, nor of Baeyer's strain theory in the formation of closed chains, nor of any other explanation of the way in which two carbons may unite by double or triple bonds, and the consequences of such union.

The most interesting part of the book is the brief fourth section, which relates to the researches and hypotheses of Guye as to the relation between the rotatory power of the substance and the masses of the radicles attached to an asymmetric carbon in the molecule of an optically active compound.

One word more. The short preface by Prof. Friedel explains, as follows, the object of the book: "La branche de la science chimique à laquelle on a donné le nom de stéréochimie ou chimie dans l'espace est de date toute récente. Elle a été créé par MM. A. Le Bel et Van't Hoff: . . . A l'étranger les publications d'ensemble faites pour répandre ces notions ne manquent pas. Il n'en est pas de même en France," &c. This seems strange, while close by, rue S. André des Arts, may be had Meyerhoffer's edition of Van't Hoff's celebrated "Dix années dans l'histoire d'une théorie," a book of infinitely greater interest than the volume before us.

A practice has grown up of late years of inserting into text-books by obscure authors little prefaces by better-known men, containing nothing in particular in the way of information, and in which the laudatory expressions are not always quite justified by the character of the book. So long as "puffing" is regarded as allowable, there is no very clear reason why it should not be permitted in connection with books; but the sort of preface referred to, has rather too strong a family likeness to the "certificate" so often found on the label of hair-restorers and packets of cocoa, to the virtues of which these writers of prefaces would probably in most cases shrink from testifying.

OUR BOOK SHELF.

The Telephone Systems of the Continent of Europe. By A. R. Bennett. (London: Longmans, 1895.)

WITH what object was this book written? The introduction is a violent diatribe against the telephone powers that be in England; and yet by his titles, the author seems to have been nursed in their service. Moreover, England and Germany with their 162,000 telephones, rank next to the United States, and possess more telephones than all the rest of Europe put together. In fact, next to Scandinavia and Switzerland, England ranks above Germany in telephonic development—the rest of Europe being "nowhere." Why, therefore, this wailing and gnashing of teeth? Why should England and Germany alone in Europe excite his wrath? Is it that they will not adopt at home his views of low rates and, perhaps, no profits, and did his apparent rough treatment in Berlin prejudice his judgment of German ways? The book is full of statistics of the growth and development of the business in different European countries—except England. It indicates the public uses to which telephones can be applied, but it contains little that is scientific or practical. Its facts are fleeting, and its *raison d'être* is not evident.

The development of telephony in Sweden is very remarkable. The difficulty of locomotion, and the long dark days in winter, may account for much of it. In a population of 4,824,000 there are 26,201 telephones in use. This means one telephone to 184 inhabitants. In the United States there is one telephone to 270 inhabitants.

In Switzerland it is even more developed than in Sweden. The difficulties of locomotion and internal communication, the isolation of valleys, that gold mine to the country—the great summer tourist traffic—and hotel life, may account for this, but the author attributes its success solely to its cheapness. In fact it is too cheap, for it does not pay, and this state of things is not conducive to future prosperity.

The great development of telephony in the United States, where there are 232,140 subscribers in spite of very high rates, does not support the views of the author.

The annual charge in Switzerland was originally 150 francs per annum for an unlimited local service, and an additional 25 francs per annum to cover trunk or inter-urban service. It was soon found necessary to charge 20 centimes per talk of five minutes on trunk lines. Since 1890 the local charges have been 80 francs per annum with 800 free talks, and 5 centimes per extra talk, and the trunk charges per three minutes, 30 centimes for any distance up to 50 kil., 50c. to 100 kil., and above 100 kil. 75c. From January 1, 1896, it will be a very practical and sensible tariff, viz. an initial annual charge of 40 francs and a uniform charge of 5 centimes for all local talks, the trunk charges remaining unchanged. The number of talks per annnm per subscriber during 1894 was—local 504 and trunk 85, but the trunk traffic in many places far exceeds the local. In Affoltern, for instance, during 1894, there were only 105 local talks, while the trunk talks amounted to 8167 (*Journal Telegraphique*, May 25, 1895). There were at the end of 1894, 18,814 subscribers in Switzerland. This means one telephone to 147 inhabitants.

A word is wanted badly to express a telephonic conversation or talk analogous to "telegram." The author's "telephonogram" is lengthy. "Phonogram" is in use in connection with the phonograph. "Telelogue" has been proposed, but has not met with general approval.

The Elements of Health. By Louis C. Parkes, M.D. D.P.H. (London: J. and A. Churchill, 1895.)

THE author of this manual states in the preface that his "main idea has been to give some simple yet practical information on the preservation of individual or personal health." It is impossible to say, with any degree of certainty, who is to be accorded the distinction of having originated such an "idea." Certainly Hippocrates undertook the writing of treatises on hygiene, and even he was only following in the footsteps of others. This preliminary remark mainly arises out of the fact that when another manual of hygiene appears, one's natural impulse is to turn to the preface, in order to see if the author has any new motive to suggest for its appearance; for the fact is, there is, at present, a superabundance of such works. Dr. Parkes' manual, good as it is, contains practically nothing that cannot be found in any of the other dozen or more elementary treatises dealing with the same subject; and to those who are familiar with the same author's work upon "Hygiene and Public Health," it will be sufficient to state that the present volume under review is practically that work popularised and very much abridged.

The illustrations are excellent; and it is a positive relief to find that they show a little freshness in their treatment, and are something more than the stock figures that appear in so many similar publications.

Dr. Parkes occupies a deservedly high position among sanitarians, and it goes without saying that his teachings are sound. There are only two points which call for adverse criticism. The table on page 168 needs revision; the author is well aware that the fat in butter does not average 88 per cent.; indeed, on a subsequent page (196) he himself puts it down at 83 per cent.; and his statement that it is "doubtful if alum (in bread), unless present in considerable quantity, is able to influence health adversely," is also open to criticism. In the first place, it is doubtful whether, if such be the case, it is prudent to make so loose a statement in what is designed to be a popular work for the lay reader. There is little doubt that the hydrate of alumina, which results from the use of baking powders containing alum, is soluble in the hydrochloric acid of the gastric juice, and there are many good reasons for regarding such addition as very undesirable; it would, moreover, probably prove harmful when present in what may be held to constitute less than a *considerable quantity*.

LETTERS TO THE EDITOR.

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

Hypnotised Lizards.

SEVERAL communications relating to the so-called "death-feigning instinct" of certain reptiles have appeared in the columns of NATURE during the last few months. The following observations bearing on this question may be of sufficient interest to justify publication. They refer to a species of lizard of the genus Stellio (identified in Tristram's "Fauna and Flora of Palestine" as *S. cordylina*), which is extremely common in these parts. When one of these lizards is captured, it makes a few vigorous efforts to escape, and then, if held firmly, falls into a limp, motionless state, which might easily lead an inexperienced person to think it dead. A very little examination, however, shows that the animal is not dead, but in a trance-like condition. Gentle respiratory movements are visible just behind the shoulders, and sometimes show a rising and falling rhythm with short intervals of complete rest; the eyes may remain wide open, but are commonly half-closed, and the lids wink slowly from time to time spontaneously or by reflex action; the mouth is almost always open—sometimes wide, sometimes but little—and in either case the jaw is quite rigid, and if closed by force is apt to reopen when the pressure is withdrawn; the limbs lie extended and semi-flaccid, with some approach to a cataleptic condition, *i.e.* if bent, or stretched into positions not too strained, they maintain such positions when let go; and the same is true of the trunk and tail. If, now, the lizard be laid down gently on the floor or on a table, it will lie perfectly still and seemingly unconscious for some minutes (unless roused by a sudden jar or loud noise), the eyes preserving throughout a peculiarly vacant, expressionless aspect, quite suggestive of death. While in this state the lizard may be put into a variety of positions without eliciting any sign of consciousness, and will lie as quietly on its back as in the natural position; and I have without difficulty made one maintain various grotesque postures, such as standing erect with one hand resting on the edge of a book, like a preacher behind a pulpit; bending sharply around, and seizing the tail with the claws of one fore-foot; cocking the tail over the back, scorpion fashion, &c.

Although some reflex actions are maintained (*e.g.* winking, as above mentioned), there is a considerable degree of cutaneous anæsthesia, as shown by the fact that a pin may be run through a fold of skin without fully rousing the animal, a sluggish, feeble wriggle being the sole result.

This trance state (obviously akin to some phases of hypnotism) lasts, as before stated, for several minutes. I have on several occasions timed it, the lizard being laid on its back, and myself concealed or standing quite still at a distance, and in each instance recovery seemed to come suddenly after about five minutes (sometimes a few seconds less, sometimes more), the animal showing no sign of consciousness until by one brusque effort it turned over into the normal position; this done, it lies quite still, but evidently awake and observant, for a few moments more, and then scuttles off in a hurry.

I find that the readiest way of inducing the trance is to take the lizard's head between my finger and thumb, making gentle pressure upon the angles of the jaw and upon the tympanic membranes; but similar pressure on the sides of the trunk, just behind the forelimbs, is just about as effective.

Such are the facts: and it seems to me that, so far as the animal in question is concerned, they lend no support whatever to the hypothesis of voluntary or conscious death-feigning; but, on the contrary, are perfectly consistent with the view that such phenomena belong to the same class as the various manifestations of hypnotism, &c., with which we are all more or less familiar in the human subject.

Supposing, however, for the sake of argument, that we have to do here with a true instinct, and not, as I believe, with a mere neurosis—an incidental reaction of the higher nervous centres—what possible purpose could such an instinct serve? The natural enemies of these lizards are foxes, jackals, martens, birds of prey, and snakes. Can any one believe that any one of these animals, having captured a lizard, would be in the least inclined to let it go because it lay motionless and apparently dead in the captor's grasp? Or will it be argued that the trance condition is a special gift "in mercy" to the victim, to mitigate or abolish the pain of death? If the last be the true explanation, one is tempted to ask why such tenderness is shown to a favoured few of the victims in nature's wondrous system, while the majority (*pace* Dr. A. R. Wallace) are left in possession of consciousness and sensibility more or less acute until they have sustained enough mechanical injury to kill or stun them.

W. T. VAN DYCK,

Beyrout, Syria, May 16.

Stridulating Organ in a Spider.

IT is exactly twenty years now since I described to Geoffry Nevill the sound made by our large "Bhaluk Mokra" (or Bear Spider). I noticed that Wood Mason, who sat opposite me, appeared to be highly amused, but he said nothing.

Next morning when he joined Nevill and me at table, Mason was in high glee, and said, "I've found out all about your wonderful spider. I thought yesterday you were telling Nevill a stiff yarn for amusement, but as it wasn't your usual custom, I unbottled a lot of the big spiders, and found the stridulating apparatus."

He there and then made me recite all over again, and promise to write out, what he quoted in the *Trans. Ent. Soc.*, 1877, and give him a sketch, which is plate vii.; a previous notice of it all appearing in our *Proc. As. Soc.*, Bengal, 1876, and *Ann. and Mag. Nat. Hist.*

It was in the cold season of 1869–70 that I captured the specimen, and noticed the stridulating phenomena. The sound can be heard easily at ten or twelve yards, and is like pouring small shot on a plate.

I should not have mentioned the above, were it not that my report of "sound-producing Ants" seems to have been overlooked. If I mistake not, Sir John Lubbock looks on them as a silent group; but it is ten or twelve years now since I drew attention to the sounds made, and gave a small "Morse" diagram of the same, either in NATURE or the *English Mechanic*, one kind of ant giving a series of triple sounds, another kind a set of five or six, gradually decreasing.

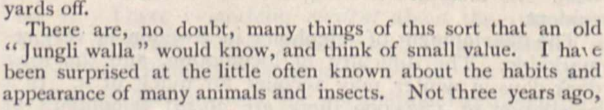
I described how the sounds were made by rasping the horny tip of the last abdominal segment on any resonant material, such as thin dry bark, dry leaves, &c.

I am not aware if the tolerably loud percussive "tok-tok" of the Mahsir (*Barbes Macrocep*) is known. I described it to a friend in England in 1879, and saw it quoted in the *Daily Telegraph* (about August to October) soon after.

While on this subject, I may mention that we have a rather rare butterfly here, which is dark in colour, some three inches across, a very hard flyer, and when darting about (generally after sunset), in a shady avenue, makes a series of taps, sounding like "tip, tip, tip."

Three or four of these butterflies generally fly together. I have not seen one alone; and though I have often enough tried to catch one, never secured a specimen. The sound, I presume, is made by striking the anterior margins of wings together; and if standing still, one can hear the "tip, tip" six or seven yards off.

There are, no doubt, many things of this sort that an old "Jungli walla" would know, and think of small value. I have been surprised at the little often known about the habits and appearance of many animals and insects. Not three years ago,



a well-known naturalist was quite interested in my description of the "happy family" one often finds in the holes, a little above water level, in our clay banks of small rivers, at low water during cold season; fish of several kinds, and crabs (three and four inches across) living together in the hole under water as a "colony." But for these tolerably deep holes, the otters would leave no fish in the smaller rivers.

S. E. PEAL.

Sibsagar, Asam, May 9.

The Migrations of the Lemmings.

ALTHOUGH I have dwelt among the Lemmings for many years, and paid great attention to their migrations, I have thought it might be more satisfactory to my readers to record the result of an interview with a captive member of the tribe, as recorded by the aid of a phonograph, assisted by a certain legitimate amount of amplification which the poverty of the language necessitates. This, however, I am convinced is what my little prisoner intended to say. "I am amused by the reasons men give for our sudden appearances and inexplicable migrations. But, although I do not see why I should enlighten you on either of these points, especially as you would probably only stick the harder to your own opinion, I will venture to ask whether you think we cross wide lakes, the opposite shores of which are quite invisible to us, in order to find the food which we thus abandon; indeed, though I fear I am somewhat letting what you call the cat out of the bag by saying it, I have often wondered why I myself did not wander along the green shores of Heimdalsvand and down the valley amid sweet grasses and clover, instead of swimming across to barren Valders, and getting caught by you for my pains. But, after all, it is no worse than when my friends the swallows leave their flies, and even their families, and start on their travels, when the impulse seizes them, whilst the former are still plentiful, and the latter not yet ungrateful. So I feel indignant at the suggestion that we travel because we are overcrowded and underfed at home. I admit that our temper as a race is somewhat short; it has been impaired by incessant bullying. Dogs, wolves, and lynxes eat us wholesale; and the reindeer disgustingly declare that we are a mere bag of succulent *saur-kraut*. Shadows annoy us, and you men have even invoked spiritual weapons to aid your carnal implements of destruction. But let me seriously advise you not to fling about inappropriate epithets; our customs are at least as good as your own, and probably somewhat older, for we too have had an ancestry, and *noblesse oblige*. Enough; let me out; I want to get on." W. DUPPA-CROTCH.

Richmond, Surrey.

Boltzmann's Minimum Theorem.

THERE is a point of great interest, in connection with Mr. Burbury's letter in your issue of May 30, on which he has not touched.

The expression obtained in the Boltzmann theorem for the value of $\frac{dH}{dt}$ depends on the assumption that the actual distribution is at every instant absolutely identical with the most probable distribution. This we know cannot be exactly true. Therefore the value of $\frac{dH}{dt}$ in Boltzmann's theorem is not identified with the *most probable* value of $\frac{dH}{dt}$. It is, for instance, quite possible, in the absence of proof to the contrary, that no matter in what way the actual distribution differs from the most probable one, the actual $\frac{dH}{dt}$ may be numerically smaller than the value corresponding to the most probable distribution.

In that case Boltzmann's theorem would give the maximum rate of subsidence instead of the most probable rate. Can Mr. Burbury or Dr. Boltzmann throw any light on this question?

EDWD. P. CULVERWELL.

Trinity College, Dublin, June 1.

THE CAMBRIDGE NATURAL HISTORY.[1]

ALTHOUGH the third in the series, this volume is the first of the long-promised "Cambridge Natural History" to appear, and as such excites additional interest because it affords some clue to the probable style of the remainder—probable, since "complete uniformity of treatment has not," we are told, "been aimed at. It is worthy of remark that, contrary to what obtains in most popular works on natural history, the Invertebrates are to receive their fair share of attention, and to extend over nearly seven of the ten volumes projected. It is almost a Cambridge work in a double sense, for with the exception of Prof. Herdman, who is to write on the "Ascidians and *Amphioxus*," and Mr. F. E. Beddard, who will undertake two such widely separated subjects as "Earthworms and Leeches" and "Mammals," all the contributors are connected with that University.

"'The Cambridge Natural History' is intended," the publishers announce, "in the first instance for those who have not had any special scientific training, and who are not necessarily acquainted with scientific language. At the same time an attempt is made not only to combine popular treatment with the latest results of modern scientific research, but to make the volumes useful to those who may be regarded as serious students in the various subjects. Certain parts have the character of a work of reference."

By this standard, then, the present volume must be judged; and on opening its leaves and turning over its pages, with their abundance of new and beautiful illustrations, it is at once manifest that artist and engraver, printer and publisher, have vied with each other to produce a work worthy of the conception.

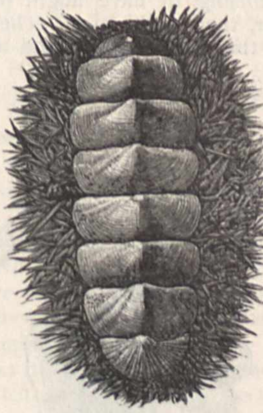


FIG. 1.—*Chiton Spinosus*, Brug.

The major portion, or, to be precise, 459 pages of the whole, is devoted to the Mollusca. It is no fault of the author's if it has to be admitted that a treatise on this branch of natural history, at once popular and scientific, still remains to be written. Mr. Cooke, who is responsible for this section, save for a casual passage or phrase here and there, has produced a most readable work; but the burden laid on his shoulders is greater than one man can bear nowadays, for no single individual can be a specialist in all the numerous branches of the subject; and yet nothing short of special knowledge in every ramification is adequate for the production of a text-book. The co-operation of specialists is yearly becoming more and more of a necessity in compiling manuals if good work is to be achieved, and in our opinion the system of minute subdivision, adopted for example in the "Standard Natural History," which was published some years ago in America, is the only wise one.

It is not, therefore, any matter for wonder that Mr. Cooke has had to resort largely to compilation, with the inevitable result that facts here presented in one form of phraseology, would, with a more intimate personal knowledge, have been differently expressed. Thus, for example, when speaking of barriers to distribution, we are told that "ranges of inferior altitude, such as the Pyrenees, the Carpathians, and the Alleghanies, may be turned in flank as well as scaled," and when he wrote, "The Mediterranean offers no effectual barrier"—the author evidently did not take into consideration the altered distribution of sea and land in the Mediterranean region during Pleistocene

[1] "The Cambridge Natural History." Edited by S. F. Harmer, M.A., and A. E. Shipley, M.A. Vol. iii. "Molluscs." By the Rev. A. H. Cooke, M.A. "Brachiopods" (Recent). By A. E. Shipley, M.A. "Brachiopods" (Fossil). By F. R. C. Reed, M.A. Pp. xii. 535; 334 Figures in text, and 3 Maps. 8vo. (London: Macmillan and Co., 1895.)

times. Whilst in this respect the work, for a text-book, suffers unavoidably from too much of the "study," it, on the other hand, would have been better if an extension of time had been allowed the author in which to weld his mass of interesting and valuable material into a more homogeneous whole.

The method of treatment of the subject, differing as it does in many respects from that of the ordinary handbook, will best be gathered from a brief recapitulation of the order in which the main points are taken.

Prefixed is a scheme of the classification adopted ; and concerning this it will be more convenient to speak later on. The opening pages are devoted to a brief introductory statement defining the relationship of the Mollusca to the rest of the animal kingdom, and sketching their classification so far as the principal groups are concerned. Only one phylogenetic table is submitted, and that, unfortunately, the misleading one dividing the Mollusca into the utterly unnatural groups of Glossophora and Aglossa. On the other hand, Mr. Cooke is cordially to be congratulated on refusing to have aught to do with that mythical monster, the "archi-" or "schematic-mollusc." A discussion on the origin of the land and fresh-water mollusca follows, and leads up to chapter ii., which deals with the habits and economy of the non-marine forms. Enemies of the mollusca, means of defence, parasitic mollusca, commensalism, and variation occupy the next chapter. Field malacologists especially will appreciate the bionomical facts and fancies here carefully gathered together from innumerable minor sources, and presented in available form ; indeed, were it not from lack of space, we would gladly quote largely out of this, the most interesting portion of the work from a popular point of view. In the succeeding four chapters (v.–viii.), the anatomy, or rather the comparative anatomy, and embryology of the several classes are dealt with. The shell and the designation of its parts come next. Distribution (in space) forms the theme of the three subsequent chapters, and here the non-marine have preference by two to one over the marine mollusca. Three maps accompany and "illustrate" this section, by obviating the necessity of referring to an atlas. Finally, there is the systematic portion, in which a brief description is given of the principal characters of each family with its distribution in time, and a list of the more important genera composing it.

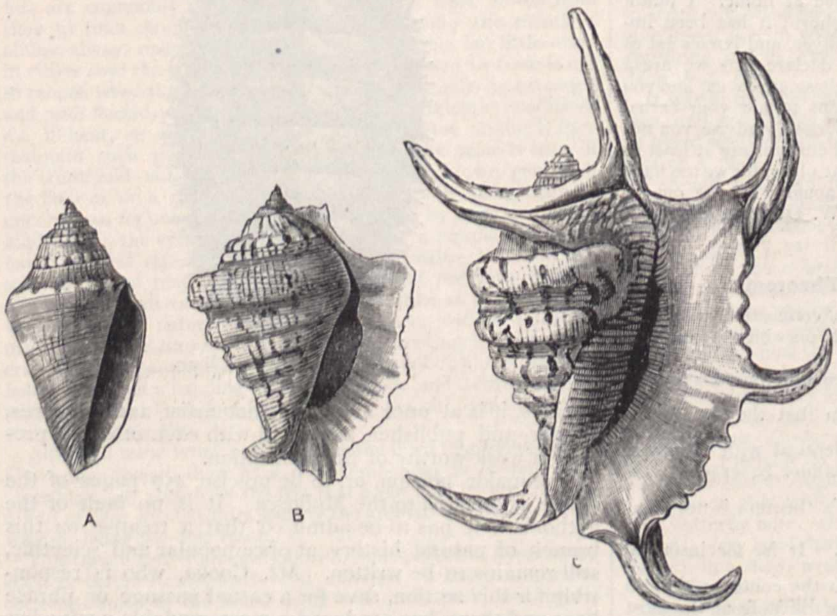


FIG. 2.—Three stages in the growth of *Pterocera rugosa*, Sow.

In great part, therefore, the present work reverses the method adopted in most modern text-books, wherein it is customary to describe the animals first and discuss their habits afterwards ; the writer has, in fact, followed the arrangement adopted in the preliminary chapters of Woodward's "Manual," rather than that in Fischer's. This system of inversion also obtains in the anatomical portion; reproduction, usually reserved for the last, being put first apparently, with some idea of starting at the beginning of the molluscan career. The principle may undoubtedly possess advantages, but it also has its drawbacks. For example, the nomenclature of the parts, or topography, of the shell is not given till the close of chapter ix., whilst many of the terms there first defined have previously been freely employed, and that although the student is theoretically not expected to be acquainted with anything beforehand.

This is a detail which the editors should have attended to, for wherein their utility if not to assist by bringing a fresh and impartial eye to bear on the work they supervise, since however able a writer may be, he is naturally apt when engrossed with his task to overlook such minutiæ. So, too, they should have noted that the "classes" have in the opening pages, by a slip of the pen, been called "orders." They might also, though it is not fair to charge it to their account, have observed that whilst at p. 14 *Dreissensia* and *Mytilopsis* are spoken of as "scarcely modified *Mytili*," these two genera are in the systematic part correctly referred to a totally different order from *Mytilus*. The author himself, however, must be held responsible for having overlooked Dr. Carpenter's retraction of his theory of shell formation in the later editions of "The Microscope," and for such other oversights as referring the well-known and beautiful *Choanopoma hystrix*, from Cuba, to the genus *Cylindrella*, or describing *Strombus* as "frugivorous."

Although on so vexed a question as classification the greatest latitude seems allowable, yet certain points in the one here adopted call for remark. For instance, the Amphineura are retained as an order of Gastropoda (Mr. Cooke prefers the older and, we think, less correct spelling of Gasteropoda) in contradistinction to the opinion of recent authorities such as Pelseneer, Simroth, &c. Moreover, by-the-by, why is Pilsbry's classification of the Chitons passed over for an older and less complete one? What to do with the Pteropoda, Mr. Cooke was apparently in doubt when he began his book (pp. 6, 7), but in the systematic part at the close, their affinities with the Tectibranchiate Opisthobranchs is duly pointed out. At the same time, though their two main divisions, Thecosomata and Gymnosomata, are most closely allied to the Bulloidea and the Aplysioidea respectively, the Pteropoda are here for convenience sake retained as a group by themselves of equal rank with the Tectibranchiata as a whole. This, if not exactly logical, is comprehensible, but not so the separation of these two sections by the interposition of the Ascoglossa and Nudibranchiata.

About the Heteropoda, on the other hand, our author has no scruples, and though they retained their independence to a later date than the Pteropoda, they are referred without comment, albeit correctly, to the Prosobranchiata, and even, following Lang, to the Tænioglossa.

The term Platypoda, founded to include all the Pectinibranchiate Prosobranchs except the Heteropoda, is here restricted and made to apply, without reason given, to the Tænioglossa other than the Heteropoda.

Those interesting and somewhat anomalous genera *Siphonaria* and *Gadinia*, Mr. Cooke, in accordance with the conclusions lately arrived at by Köhler, Haller and Plate, places with the Tectibranchiata, creating for them the sectional name of Siphonarioidea. Pelseneer, we may incidentally remark, in his "Recherches sur divers Opisthobranches," which has only just been published, objects to this conclusion of his German *confrères*, and seemingly on very good grounds.

The Brachiopoda, which are incorporated at the end of the volume, are subdivided into "recent" and "fossil." The former (pp. 463–88) have been undertaken by one of the editors, Mr. A. E. Shipley; the latter (pp. 491–512), by Mr. F. R. Cowper Reed.

Mr. Shipley's chapter is a compact little summary, pithily written, and whilst not erring on the side of popularity, ought to be readily followed by any average student or reader.

It consists almost entirely of anatomical description, embryology, &c., for in "habits" the Brachiopoda are extremely deficient, preceded by a short sketch of the historical bibliography of the group, and followed by a few notes on their distribution, with a synopsis of their classification by Davidson.

Mr. Shipley concludes that the affinities of the Brachiopoda "seem to be perhaps more closely with the

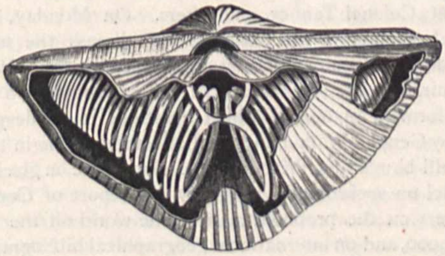


FIG. 3.—*Spirifer striata*, Carboniferous Limestone.

Gephyrea, and with *Phoronis*, than with any of the other claimants" which have from time to time been advanced.

Mr. Reed, on the other hand, by the nature of his subject, is reduced to a description of the shell, especially emphasising such features as indicate anatomical structure and to a classification or "Synopsis of Families." The latter closely approximates the classification employed by Zittel in his "Handbuch," and hence can hardly be said to embody the very latest researches. Schuchert's classification should, we think, at least, have been referred to. Some allusion, too, ought to have been made to *Trematobolus*, which its discoverer, Mr. G. F. Matthew, describes as possessing articulate valves, though it is allied to the Obolidæ. Mr. Reed's descriptive writing must be accorded equal praise with that of Mr. Shipley for clearness of style.

Through the kindness of the editors and the publishers, we are enabled to reproduce some of the illustrations in the text. These of themselves should serve to distinguish "The Cambridge Natural History" from most of its competitors for popular favour, with their plentiful reproduction of ancient blocks, now, alas, too familiar to the eye, and by no means always joys for ever.

NOTES.

THE Ladies' soirée of the Royal Society took place yesterday evening, at the time NATURE went to press.

AN unknown donor has given to the University of the City of New York, funds for a central building, on University Heights, for a library, museum, and hall, so arranged that all may be turned into a library capable of holding 1,000,000 volumes. The gift will amount to 250,000 dollars, being the largest ever received in the sixty-six years of the existence of the University. The only condition is that the name of the donor shall never be revealed.

ARRANGEMENTS are being made by the Marine Biological Association for a series of dredging and trawling expeditions during July, August, and September, to investigate the fauna and flora of the outlying grounds between the Eddystone Rocks and Start Point. In order to make the results as complete as possible it is extremely desirable that the investigation of each group should be carried out by a competent naturalist. Zoologists and botanists who are willing to take part in these expeditions, or to assist in working out the material collected, are requested to communicate with the Director, the Laboratory, Plymouth.

THE summer meeting of the Institution of Naval Architects was opened at Paris on Tuesday, when Lord Brassey delivered his presidential address, and several papers were read and discussed. In the afternoon the members of the Institution visited the Paris Observatory and the Arts et Métiers, and a banquet was given at the Hôtel Continental in the evening. After the close of the meeting, we shall give a report of the proceedings.

THE annual meeting of the Société des Amis des Sciences was held at Paris last week. The Society was founded by Thénard in 1857, for the purpose of affording assistance to men of science or their families. It numbers more than two thousand members or subscribers, and since its foundation has distributed nearly £50,000 to deserving investigators. Grants are only made to persons who have had papers or memoirs presented to the Academy of Sciences, or who have published papers of equal merit to those approved by the Academy. The Society lays stress on the fact that the grants must not be regarded as charitable doles, but as rewards for services to science, and of a similar nature to the pensions which a grateful country gives to its servants. The awards are therefore publicly announced, and are looked upon as honours for meritorious work.

THE Committee of the American Public Health Association, appointed to determine the possibility of establishing co-operative investigation into the bacteriology of water supplies, have made arrangements for a conference of bacteriologists to be held on June 21 and 22, in the Academy of Medicine, New York city. The conference will consider how to obtain increased exactitude in the details of bacteriological research, and establish standard methods. The conference will, in fact, attempt to establish some common ground-plan for systematic work in bacteriology in general, and in the bacteriology of water supplies in particular. The Bacteriological Departments of many State and Provincial and Municipal Boards of Health will be represented at the conference, as also the principal universities of the United States and Canada.

IN the eyes of the law, the Royal Agricultural Society is not a scientific institution which can claim exemption from local rates. It was decided in the Queen's Bench Division on Tuesday, that, as the funds of the Society are not exclusively applied to the purposes of science, but are used to promote "the comfort and welfare of labourers," the Society does not come within the statute under which exemption from rates is claimed.

COLONEL J. WATERHOUSE has been elected President of Photographic Society of India for the current year.

THE summer meeting of the Geological Society of America will take place at Springfield, Massachusetts, on August and 28.

WE learn from *Science* that nearly a thousand dollars have been subscribed in the United States towards the memorial to Helmholtz.

THIS year's conversazione of the Institution of Electrical Engineers will be held in the Galleries of the Royal Institute of Painters in Water Colours, on Wednesday, July 3.

THE third International Congress of Physiology will be held at Berne, from September 9 to September 13. An exhibition of physiological apparatus will be held at the same time. Those who desire to become members, or to read papers, should communicate with Prof. H. Kronecker, Berne, before August 1. The subscription is ten francs.

SEVERAL clearly marked earthquake disturbances have been felt at Florence during the past week. A strong shock, followed by two slighter shocks, was felt there at 1.36 a.m. on Thursday last. The shocks have done no damage in Florence, nor, so far as can be learned, in the surrounding country. The earthquake was most violent at Pontassieve, Rignano, and San Casciano.

SIR SAMUEL WILSON, whose death is announced, was greatly interested in science and education. Among other generous acts, he presented £30,000 to Melbourne University in 1875. He was Vice-President of the Melbourne International Exhibition of 1880, and a Royal Commissioner for the Fisheries Exhibition.

AMONG the recent appointments abroad we notice the following:—Dr. Celakovsky to be Professor of Pharmacology in the Bohemian University at Prague; Dr. Rohde to be assistant in the Zoological Institute at Breslau; Dr. F. Trendelenburg, Professor of Surgery in Bonn University, to succeed the late Prof. Thiersch at Leipzig; Prof. J. v. Kries to be the late Prof. Ludwig's successor at Leipzig; Dr. F. Schütt, of Kiel, to Greifswald University as Professor of Botany, and Director of the Botanical Gardens and Museum; Dr. v. Knorre to the new chair of Electro-chemistry in the Technical High School at Charlottenburg; Prof. A. Overbeck, of Greifswald, to be Professor of Physics in the University of Tübingen; Dr. Hermann Struve to be Professor of Astronomy in Königsberg University; Prof. E. Koken to be Professor of Geology and Mineralogy in Tübingen; Prof. R. Brauns to be Professor of Geology and Mineralogy in Giessen; Dr. T. Smith to be Professor of Applied Zoology in Harvard University.

IN all parts of the British Islands, and especially over England, the weather has continued persistently dry; in the neighbourhood of London the total fall during the first eleven days of June did not exceed half a tenth inch, and the aggregate fall since the beginning of May, a period of six weeks, was but little over half an inch. The *Weekly Weather Report* of the 8th inst. showed that the amount of rainfall since the beginning of the year was below the average in all districts, except the north-east of England. In the west of Scotland the deficiency amounted to 10·4 inches. High summer temperatures have occurred during the past week in many parts of the country, the shade readings having reached 84° in the east of Scotland, and 83° in the south of England. In London, readings of 80° were recorded both on Saturday and Sunday last.

THE Whitsuntide party at the Port Erin Biological Station included the following naturalists:—Mr. F. W. Gamble (Owens College), Mr. W. I. Beaumont (Cambridge), Dr. H. O. Forbes (Liverpool Museum), Mr. A. Leicester (Southport), and Prof. R. Boyce, Mr. A. Scott and Prof. Herdman, from Liverpool. Dredging, tow-netting, shore collecting, and laboratory work were carried on much as usual. Amongst the more noteworthy animals obtained were *Polygordius* sp., *Sarcobotrylloides* sp., *Embletonia pulchra*, *Elysia viridis*, and *Cynthia morus*. The tow-nets contained some fish eggs, but fewer than at Easter. Diatoms and gelatinous Algæ were nearly absent; Copepoda and larval forms were present in great abundance. Prof. R. Boyce and Prof. Herdman have commenced an investigation on the effect of surrounding conditions upon oysters, and their connection with disease. A number of oysters have been laid down in different parts of Port Erin bay and on the shore, and others are being experimentally treated with various fluids and food matters in the aquarium. Mr. W. I. Beaumont stays on for some weeks at the laboratory studying the Nemertines of the district, and the Rev. T. S. Lea goes to Port Erin shortly to assist Prof. Herdman in working out the detailed "zoning" of the shore and the distribution of the littoral animals.

THE general arrangements for the sixth International Geographical Congress, to be opened in London towards the end of next month, are made known in a circular just distributed. The Congress promises to be truly international, for delegates have been appointed to represent Governments and Geographical Societies in all parts of the world. The provisional programme of the meetings is as follows:—The Congress will be opened on Friday, July 26, at 9 p.m., when short addresses of welcome will be delivered by H.R.H. the Duke of York, Honorary President, and by Mr. Clements R. Markham, President. On the following day, Mr. Markham will deliver his inaugural address, after which the Congress will meet in two sections to discuss papers on geographical education, by Profs. Levasseur and Lehmann, and others; and on mathematical geography, especially the use of photography in surveying, by Colonel Laussedat, Colonel Tanner, and others. On Monday, July 29, a general meeting of the Congress will discuss the subject of Arctic and Antarctic exploration, introduced by Prof. Neumayer and Admiral A. H. Markham. In the afternoon two sections will be formed, in one of which questions in geodesy will be treated by General Walker and M. Lallemand, while in the other papers will be read by Prince Roland Bonaparte on glaciers, and M. Martel on spelæology. On July 30, report of Committees and papers on the proposed map of the world on the scale of 1 : 1,000,000, and on international geographical bibliography, will be presented at the general meeting, and two sections will then deal with oceanography, and with the orthography of place names. On Wednesday, July 31, Sir John Kirk will initiate a discussion on Europeans in Africa in the general meeting, and in the afternoon the sections will consider applied geography (commercial geography) and limnology, the latter to be introduced by Prof. Forel. The general meeting on August 1 will deal with the terminology of land forms, and in the afternoon cartography and other subjects will be treated. On Friday, August 2, the forenoon will be devoted to papers by Baron Nordenskiöld, Prof. Hermann Wagner, and others, on the history of maps; and all the remaining papers will be taken in the afternoon. On August 3 the votes proposed for consideration will probably be discussed, the date and place of meeting of the next Congress considered, and the President will deliver his concluding address. After the close of the Congress, a series of excursions will be organised to places of geological and geographical interest.

THE Rev. O. Fisher contributes a short paper on the age of the earth to the *Geological Magazine* for June. Arguing in favour of a comparatively thin crust and a liquid substratum, he urges that the continual laving of the bottom of the crust by the molten rock will retard the cooling of the crust, and will produce an effect on the temperature-gradient at the surface similar to that to which Prof. Perry has recently drawn attention (NATURE, vol. li. pp. 224-227). If this be the case, then, no trustworthy estimate of the earth's age, based on the present temperature-gradient at the surface, has yet been made.

DR. M. CINELLI has recently compiled a valuable list of the records of the Vicentini microseismograph at Siena between

July 15 and October 31, 1894 (*Atti, R. Acc. dei Fisiocritici*, v., 1895). An examination of the traces corresponding to seismic movements shows that they exhibit different kinds of oscillations, some short, others long, in period. When the earthquakes occur in neighbouring districts, the disturbance of the instrument is brief and the vibrations are rapid. But, with distant earthquakes, the disturbances last for much longer intervals. They begin with rapid oscillations which generally present several maxima, so as to appear as if distributed in groups; while towards the end, either alone or in company with the former, succeed much slower oscillations, which perhaps correspond to undulatory movements of the earth's crust.

DR. HOEBER has been making some interesting experiments to ascertain whether the presence of water-weeds affects the vitality of anthrax germs in water. For this purpose he constructed small fresh-water aquaria, each of which contained about eight litres of ordinary river Main water, some river sand, and a supply of water-weeds, and in addition about 200,000 anthrax microbes. These aquaria were only submitted to diffused light, and were kept at 10° and 19° C., respectively. Dr. Hoeber presumably worked with anthrax bacilli only, but special precautions were not taken to ensure the absence of spores; no anthrax germs, however, could be found after three days at the lower, and four days at the higher temperature, respectively. In his second report to the Royal Society, Prof. Percy Frankland states that *sporiferous* anthrax retained their vitality in ordinary river Thames water for upwards of seven months without losing their virulence; but when exposed to sunshine they were destroyed after eighty-four hours. On the other hand, when using *anthrax bacilli free from spores*, as derived from the dead body of an animal, the same authority (*Proceedings* Royal Society, 1894, p. 549) states that in sterilised river Thames and loch water they were destroyed in about five days at 5° C., and in fourteen days at 13° C.; but that at 19° C. they multiplied enormously, and were present in large numbers on the forty-second day. This different behaviour was found to be due to the bacilli having formed spores in the water at the higher temperature. The danger of anthrax germs gaining access to water depends, therefore, upon the temperature of the water and the presence or absence of spores in the morbific material. Judging by Dr. Hoeber's experiments, it would appear that the presence of water-weeds and the competition of water-bacteria may offer obstacles to the vitality of anthrax bacilli in water.

A SALE of much interest recently took place at the dispersal of the herd of white polled cattle belonging to Mr. R. E. Lofft, of Troston Hall, Bury St. Edmunds. The herd, which comprised twenty cows and heifers and five bulls, represented the old "monks' cattle," descended from the oldest historic breed of cattle in the British Isles—the polled white, with black or red points on the ears, muzzle, rims of eyes, and hoofs. Under the wave of improvement which set in with the work of Bakewell, of Dishley, more than a century ago, the old hornless white breeds no longer enjoyed the pride of place, and Mr. Lofft's herd really embodied an attempt to restore this ancient breed to something like its former position. It is probable that these cattle were originally selected by the monks, who in their day were the leaders of agriculture. Being hornless, the animals would be more easily domesticated, and less adapted to purposes of sport, such as the chase and bull-baiting. After the dissolution of the monasteries, these cattle were dispersed over the country, and mostly became merged in the common local varieties. A few, however, were kept pure, and at the beginning of this century there were two herds in Suffolk, which quite escaped the notice of the late Rev. John Storer, the historian of the breed. It is satisfactory to know that some of Mr. Lofft's quaint cattle were purchased by Mr. Assheton-Smith, of Vaynol Park, Carnarvonshire, where he has a herd of black-eared and black-muzzled white horned cattle, and is now going to keep some of the polled type.

PROF. L. DE MARCHI, the author of an Italian hand-book of Meteorology, has contributed an important essay on the theory of cyclones to the *Pubblicazioni* of the Milan Observatory. The discussion consists of 42 small folio pages and 15 plates, and while giving a general account of recent researches, treats the subject chiefly from a mathematical point of view. The following is a brief summary of the principal results arrived at:—The changes in the shape and path of a cyclone, as well as all the principal dynamic phenomena that accompany it, may be deduced from the equations of the horizontal motion, if account is taken of the distribution of temperature round the cyclone, both as regards that which previously existed in the mass of air subsequently occupied by the cyclone, and that drawn into the same area by the vertical movements produced by the earth's rotation. Therefore in some cases, if not always, it is useless to have recourse to external causes, and particularly to the general circulation of the atmosphere, to explain the persistence, change of form, or the motion of a cyclone. The general circulation may be the determining cause of a cyclone at a given point; its propagation, or the successive transference of cyclonic conditions to contiguous masses of air, is determined and maintained, at least in some cases, by the disturbances of thermic equilibrium caused by the sun at the surface of the earth, and induced by the earth's rotation.

THE old and fascinating problem concerning the manner in which the ether moves with or through matter has been attacked by Herr L. Zehnder, who contributes an interesting paper on the subject to *Wiedemann's Annalen*. He endeavoured to decide whether the ether is pushed along by atoms or bodies, or whether it passes through them without resistance, or, finally, whether only a portion of the ether adheres to the particles of bodies, and this portion only is carried along. The apparatus used consisted of a cast-iron cylinder in which a piston moved air-tight. A narrow tube led out from one end of the cylinder, doubled back upon itself, and returned by the other end. Now if the cylinder was exhausted of air, and the piston pushed the ether before it, the latter would stream through the narrow tube with a velocity greater than that of the piston in the ratio of the sectional areas of the cylinder and the tube. This ratio was 560, and exhaustion was carried to 1-40,000th of an atmosphere. To test the motion of the ether, a beam from a brilliant sodium flame was passed through two thick parallel glass plates, the second one being silvered at the back. This plate, by its two reflecting surfaces, split the beam into two, each of which travelled through one portion of the narrow tube. The two beams, reflected near the cylinder by a rectangular prism, were recombined by the same thick plate and returned along the way they had travelled, being finally reflected into the reading telescope by the first plate. Interference fringes were thus produced in the field of view, the motion of which would have indicated a motion of the ether. But no such motion was observed when the tubes were thoroughly exhausted, so that it must be concluded that the ether passes freely through solid bodies. The corollary to this conclusion, that there is a relative motion between the earth and the luminiferous ether, though investigated by the author by means of a new and ingenious apparatus on the Rosskopf, near Freiburg, could not be proved.

THE thirteenth part of Kerner and Oliver's "Natural History of Plants," just published by Messrs. Blackie and Son, refers chiefly to the production and characteristics of plant hybrids.

THE June *Journal* of the Chemical Society contains, in addition to papers read before the Society, and abstracts of other papers, a description of the life-work of the late Prof. J. C. G. de Marignac, by Prof. P. T. Clève, together with a portrait of that lamented chemist.

MR. C. L. PRINCE has sent us details of observations made made by him at Crowborough Hill, Sussex, during the great frost of January and February last. In his report, he contrasts the period with other periods of severe cold which have occurred during the present century.

THE Guide-books to Middlesex and Hertfordshire, published by Messrs. Iliffe and Son, will direct the tourist's steps aright, and afford him instruction upon points of more or less historical interest, but they furnish very little information with regard to the counties from a scientific point of view.

WE have received a "Guide to the Bristol Museum," by Mr. Edward Wilson, the Curator. The Museum contains a large number of valuable objects, and geology is very well represented. With this guide to assist them, students of science must find the collections more helpful than they used to be.

THE Lumleian Lectures on certain points in the ætiology of disease, delivered by Dr. P. H. Pye-Smith, F.R.S., before the Royal College of Physicians in 1892, and the Harveian Oration, delivered before the College in the following year, have been published in volume form by Messrs. J. and A. Churchill. The volume also contains a memoir of the life and works of Harvey.

THE fact that the report of the Marlborough College Natural History Society for the year 1894 runs into one hundred and fifty pages, may be taken as an indication that the Society is in a satisfactory condition. The report contains summaries of lectures delivered during the year, a description of the College museum and the collections in it, notes and observations, and accounts of the work of sections; it is altogether a creditable production.

PUBLISHERS' catalogues are frequently of great assistance to librarians and bibliographers. A catalogue lately issued by W. Engelmann, of Leipzig, belongs to that class of useful publications. It contains descriptions of all the books, memoirs, and periodicals published by Engelmann from the foundation of the firm to February of this year. The books are arranged alphabetically according to the authors' names, and are also classified into subjects. There is, therefore, no difficulty in finding a volume of which the author or the title is known.

THE annual report of the Zoological Society of Philadelphia shows that, but for grants made by the City Councils, the Gardens would have had to be closed, the receipts from admissions having been too low to meet expenses. We notice that, in addition to nearly three thousand free admission tickets to charitable institutions, donors, &c., the Society issued fifty thousand tickets to the Board of Education, for the admission of pupils of the elementary schools. The collection of animals now comprises 251 mammals, 416 birds, and 245 reptiles and amphibians, or a total of 912 specimens.

THE new editions received during the past few days include the second part of Dr. Michael Foster's standard "Text-book of Physiology" (Macmillan), dealing with the tissues of chemical action and their respective mechanisms, and with nutrition. The work, which is now in its sixth edition, has been brought into line with the present state of physiological knowledge. Messrs. J. and A. Churchill have published a sixth revised edition of "A Treatise on Practical Chemistry," by Prof. Frank Clowes; and Messrs. Smith, Elder, and Co. have published a fourth edition of Marshall and Hurst's "Junior Course of Practical Zoology."

THERE are only four papers in the June number of *Science Progress*, but each of them is an important contribution to scientific literature. Prof. Marshall Ward describes the growth of knowledge concerning the fixation of free nitrogen by plants. He briefly states the aspects of the question, and gives references to the most important papers upon it. A valuable paper on the ratio of the specific heats of gases is contributed by Mr J. W. Capstick; it affords interesting reading in connection with the recent discussion in these columns of points arising from the kinetic theory of gases, and also with reference to the atomicity of argon. Mr. J. W. Rodger concludes his most useful statement of the progress made in physical chemistry during 1894. The papers are classified in such a manner that it is easy to find what was done in every branch of the subject. The fourth paper is by Mr. J. E. S. Moore, and has for its subject "The Protoplastid Body and the Metaplastid Cell."

THE current *Journal* of the Anthropological Institute (No. 4) contains the presidential address delivered by Prof. A. Macalister, F.R.S., in January last. The Institute by no means possesses a membership in proportion to the importance of the subjects fostered by it. "When we consider," remarks Prof. Macalister, "the wide-reaching importance of the myriad of practical problems with which we as anthropologists are concerned, and the useful work which the Institute has done in the past, it is scarcely conceivable that our membership of 362 should be taken as representing the number of persons to whom these matters are interesting. And further, it is little short of a national disgrace that in the largest empire of the world, within whose bounds there are nearly as many separate peoples, and tribes and kindreds and tongues as in all the other nations put together, there is no Imperial department whose function should be to collect and classify the facts of the physical, psychical, and ethical histories of our fellow subjects."

Two years ago the American Philosophical Society, of which Benjamin Franklin was the first President, held a meeting, at Philadelphia, in commemoration of the 150th anniversary of its foundation. The meeting was attended by delegates representing learned societies and institutions in most parts of the world, and was completely successful. The volume containing full reports of the proceedings has only lately appeared, but the delay in its publication is probably due to the many addresses, memoirs, and plates contained in it; for the printing of the communications, and the preparation of nearly sixty plates, necessarily takes time when the work is so carefully done as it seems to be in the volume before us. Among the addresses is one by Dr. Roberts (the delegate of the Royal Astronomical Society), entitled "Illustrations of Progress made during Recent Years in Astronomical Science." This address is illustrated by thirteen plates representing some of Dr. Roberts' classical photographs. A richly illustrated paper on Tertiary Tipulidæ, by Dr. S. H. Scudder, has already been noticed in these columns (vol. 50, p. 111). Seven plates illustrate Dr. A. S. Packard's "Study of the Transformation and Anatomy of *Lagoa crispata*, a Bombycine Moth," and sixteen embellish a paper by Prof. A. Hyatt on "The Phylogeny of an Acquired Characteristic." Limits of space prevent us from referring to the many other papers. Suffice it to say that the volume is a worthy memorial of a remarkable meeting.

THE *Zeitschrift für Anorganische Chemie* gives a very complete account of the synthesis of metallic ores by crystallisation from solution in the appropriate molten metal, by Friedrich Roessler. The work includes the production of crystalline sulphides and selenides of such metals as lead, bismuth, and silver, and of arsenides, antimonides, and bismuthides of platinum, palladium, and gold. The production of silver bismuth ulphide will serve to illustrate the method followed. Twenty grams of bismuth were melted in a covered crucible, and two grams of silver sulphide were added. By solution of the slowly cooled product in nitric acid of specific gravity 1·1, there remained small dark crystals intermixed with silver-white crystals. The latter consisted of a bismuth-silver alloy, and, in time, dissolved in the acid. On drying, the remaining dark crystals were found to possess a steel-blue lustre. They formed pretty groups of octahedra (figure given in the paper) attached in rows. Analyses proved their composition to be well represented by the formula, $AgBiS_2$ or $Ag_2S + Bi_2S_3$.

IN the current number of the *Comptes rendus*, M. Clève gives the results of a determination of the density of the new gas helium by M. Langlet. The gas, extracted from cleveite, was freed from hydrogen by passage over red-hot copper oxide, and from nitrogen by metallic magnesium. It contained no argon. The density was found to be notably less than the number given by Prof. Ramsay, being 0·139 (air = 1) or 2·02 (hydrogen = 1). The determination of the specific heat of the gas has been taken in hand by the same investigators; their results will be awaited with much interest.

THE additions to the Zoological Society's Gardens during the past week include two Rhesus Monkeys (*Macacus rhesus*, ♂ ♀), from India, presented, respectively, by Sir Henry W. Peek, and Mr. R. Edmeades; a Patas Monkey (*Cercopithecus* (*ruber*, ♀), from West Africa, presented by Mr. C. H. Armitage; a Campbell's Monkey (*Cercopithecus campbelli*, ♂), from West Africa, presented by Miss L. Panther; a Herring Gull (*Larus argentatus*), British, presented by Mr. J. T. Gorvin; three Ocellated Skinks (*Sepsocellatus*), a —— Skink (*Chalcides sepoides*), a Defenceless Lizard (*Agama inermis*), two Diademed Snakes (*Zamenis diadema*), two —— Snakes (*Cœlopeltis moilensis*), four Egyptian Eryx (*Eryx jaculus*), two Cerastes Vipers (*Vipera cerastes*), two Egyptian Cobras (*Naia haje*), from Lower Egypt, presented by Dr. John Anderson, F.R.S.; a White-crowned Monkey (*Cercocebus æthiops*), a White-necked Stork (*Dissura episcopus*), from West Africa; two White Pelicans (*Pelecanus onocrotalus*), from North Africa; a Barraband's Parrakeet (*Polytelis barrabandi*), from New South Wales; three Hamadryads (*Ophiophagus elaps*), from India; fifty —— Tree Frogs (*Hyla* ——), from America, deposited; a Red Deer (*Cervus elaphus*), an Argus Pheasant (*Argus giganteus*), three Ruddy-headed Geese (*Bernicla rubidiceps*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

COMET 1892 V. (BARNARD).—The orbit of this comet, which had been discussed by Mr. J. G. Porter (*Astronomical Journal*, No. 310), has recently been made the subject of a further investigation by M. J. Coniel; but the elements, resting as they do on a very few observations, still remain uncertain to a considerable extent. The comet was discovered, photographically, on October 12, 1892, and regular observations do not extend beyond November 22, 1892, about six weeks only from the date of discovery; but an isolated observation made at Nice on December 8, not taken into the discussion by Mr. Porter, induced M. J. Coniel to reopen the inquiry, with the hope of making a better determination of the period. Mr. Porter's orbit represented this observation within the errors − 0·05s. and − 12″·9, in R.A. and Declination respectively; and considering the difficulty of the observation, such a discrepancy is not more than might be anticipated, and consequently does not suggest the possibility of decided improvement. The feature of the more recent discussion is to show that the observations can be equally well satisfied with elements in which the mean daily motion is altered by some 26″. This is not quite the twentieth part of the whole motion, therefore the period is uncertain to its twentieth part, or about 0·3 of a year.

M. Coniel judges that the uncertainty in the mean motion may extend to 25″ on either side of his result, and that the period may lie between 6·23 and 6·84 years. The consequence of this uncertainty is that the search for the comet at future returns must be greatly extended. In 1899, the sweeping ephemeris must be based on a mean motion corresponding to 6·65–6·84 years. In 1905, the comet may be visible if the period lies between 6·34–6·55 years; while in 1911, and even 1912, the return may be expected with still other and possible values of the mean motion. No near approach to Jupiter will take place during this interval, but the situation of the orbit is such that the comet can approach both Jupiter and Mars. Confining attention to the most probable period (6·52 years), the comet approaches the orbit of Jupiter within 0·07R, in Hel. Long. 207°, and that of Mars within 0·012R, in Hel. Long. 28°. This interest in the comet's path is still further increased by the speculation, due to M. Schulhof, that this comet formed originally a part of Wolf's comet, from which it possibly separated in 1815.

MEASUREMENT OF RADIAL VELOCITIES.—The methods at present employed for the measurement of the movements of the heavenly bodies towards or away from the earth usually involve the use of a comparison spectrum, whether the observations be made by eye or by photography. In special cases, however, other methods are employed, as, for example, the use of telluric lines by Dunér in the measurement of the sun's rotation. It has not, however, yet been considered practicable to utilise the objective prism for the work, on account of the difficulty of obtaining reference spectra. A new method, which has the great advantage of being applicable to spectra photographed with or without slits, has recently been suggested by M. Orbinsky, of Odessa. (*Astr. Nach.* 3289.) The principle of the method is based on the fact that the displacements of lines are different at different wave-lengths, so that the distance between two lines in a spectrum depends upon the velocity of the source of light; the higher the velocity, the greater or less will be the distance between any two lines in the spectrum, according as the source of light is approaching or receding, and providing the dispersion be sufficient, it may be possible to measure the velocities by this means. Obviously the measurements are much more delicate than the direct measures of the displacements. In practice it is proposed to employ reference stars, the velocities of which have been determined in the ordinary way by photographic comparison spectra of hydrogen or iron. One of these being photographed on the same plate as the star under investigation, the results will give the velocity relatively to the comparison star, and hence the absolute velocity. The instruments employed should give the greatest possible range of wave-lengths, and it will be specially advantageous to obtain as great a difference of dispersion as possible between the extreme ends of the spectrum. It is shown by actual figures that the measurements are quite practicable, both in the case of the Potsdam spectrograph and in the objective prism employed at Harvard College. It is in the case of the latter class of instruments that the method seems most likely to be of practical value.

TWO REMARKABLE BINARY STARS.—Apart from the binary stars which can only be recognised as such by the aid of the spectroscope, the two binary stars of shortest periods at present known are κ Pegasi and δ Equulei. The orbits of these have been redetermined by Dr. See, using all available observations, many of which are due to the industry of Profs. Burnham and Barnard. The elements deduced are as follows (*Astr. Nach.* 3285, 3290):—

	κ Pegasi.	δ Equulei.
P	11·42 years	11·45 years
T	1896·03	1892·8
e	0·49	0·14
a	0″·4216	0″·452
ι	81°·2	79°·05
☊	116°·25	22°·2
λ	89°·2	0°·0
n	−31°·5236	−31°·441

Prof. Burnham has repeatedly called attention to the importance of systematic observations of rapid binaries with large telescopes, so that we should in a few years get good orbits, which in the case of most binaries would require the observations of centuries.

It will be seen that there is still a great gap between the telescopic and spectroscopic binaries, but it is quite possible that as the powers of both instruments are increased the gap may be gradually shortened from both sides.

THE SUN'S PLACE IN NATURE.[1]

VI.

We come now to the third new point of view. Many apparent stars are really centres of nebulæ, *i.e.* of meteoritic swarms.

In that very simple statement we have perhaps the very greatest and the most fundamental change which has been suggested by the new hypothesis. I am quite certain that all of you who have read text-books of astronomy will be perfectly familiar with the statement that all stars are distant suns. I have written that myself several times, but I now know that it is not true. Some stars, instead of being distant suns like our sun, a condensed mass of gas with a crust gradually forming on it, and a thick atmosphere over it, are simply the brighter condensations, the central condensations of nebulæ, whether they be like that of Andromeda, or planetary nebulæ, or such a nebula as that of Orion. You see the idea is perfectly new and completely different from the old one, which taught us that all stars were suns. Shortly after I made this assertion, photography came to our aid, and I am so fortunate as to be able to

FIG. 24.—Nebula round η Argus (Dr. Gill).

prove to you the absolute truth of it by an appeal to Nature herself; that is, I refer for demonstration to autobiographical records with which the heavens themselves have supplied us. Among the finest and most wonderful of the nebulæ is one which, unfortunately, we do not see here, because it is in the southern hemisphere; it is that surrounding the star η in a wonderful constellation, Argo, which it is quite worth while to go south to see, were there no other reasons. From the photograph you see that there is such an intimate connection, such an obvious relation, between star and nebula, that it is impossible for us to imagine for one moment that they are not most closely and intimately connected.

I will now bring before you another case which we can, all of us, see, so far as a certain part of the phenomena is concerned, and especially at this time of the year. I refer to those "stars," the six Pleiads, which you will remember once lost a sister, that one sees in the constellation of the Bull. Here they are, photographed by Dr. Roberts. You see they are not stars; they are nebulæ. What we see in this photograph (see Fig. 25), in the case of each so-called "star," is obvious; we see the centre of condensation, and more than that, it is not a simple condensation, but there are stream-lines going in all directions, and the maximum luminosity, where we locate the "star," is just at the place where, according to this photograph, the greatest number of these streams cut each other, and where, therefore, we should get the greatest possible number of collisions per second of time. The main point demonstrated by this photograph, then, is that we are not dealing with stars anything like our sun; we are simply dealing with nebulous condensations. I can show you the spectra of the brighter parts of these condensations, and you will see that they resemble the spectra of ordinary stars. Broad dark lines of hydrogen are represented in every one; hence, although we are dealing not with a star like the sun, but a meteoric condensation—a place of intersection of streams of nebulous matter—we get a spectrum such as is generally associated with the spectrum of a star. And for this there is very good reason.

Here an interesting point comes in. Suppose that we wished to observe spectroscopically what was going on in these condensations, and that I allow the image of one of them to fall on the slit of the spectroscope, so that we have the condensation at the centre, and the ends of the slit of the spectroscope beyond the condensation. At the centre, where the slit crosses the condensation, of course we should have the spectrum which you have already seen on the screen, a spectrum indicating that there is something there which gives us a continuous spectrum, *i.e.* one rich in all the colours of the rainbow; but that some of the light is absorbed here and there in consequence of the surrounding atmosphere of hydrogen gas. So much for the centre. Next consider what will happen when I observe, for instance, this or that part of the nebula where the condensation is absent; we shall not get absorption phenomena, but we shall get radiation phenomena, and therefore a long bright line representing the radiation of hydrogen over a large area, and at the middle of it the ordinary spectrum of a star. Prof. Campbell, at the Lick Observatory, has recently subjected another star to a similar treatment, and you will see (Fig. 26) what he has found. By putting the slit of the spectroscope upon the image of the star, he finds that he gets the spectrum from one end to the other; but you see that at the place occupied by one of the hydrogen lines he gets a much longer image of the slit, showing that he had to deal there with a star immersed in something which was competent to give a spectrum of hydrogen. What was that something? You can understand perfectly well that, if one of the Pleiads had been examined in the same way, it would be quite possible that we should get just such an appearance as Prof. Campbell was fortunate enough to obtain. This raises an interesting question, in which astronomic thought has been going up and down now for the last fourteen or fifteen years, and I think I can show you exactly how the matter lies. The diameter of the sun is very nearly a million miles. Now, suppose that the diameter of the solar atmosphere was ten million miles; then if we were by any means whatever to spectroscopically examine the image of the sun under such conditions that all the light coming from these different regions could enter the slit of the spectroscope at the same time, and give us, added together, the whole light, we should be able to determine practically what we might be able to see under these conditions by some such considerations as these:—

Diameter of the sun, one million miles.

Diameter of the sun's atmosphere, ten million miles.

We should therefore get the light from the sun in the ratio of 1 to 99 of the light from the atmosphere. Now suppose that there is any chemical connection between the absorption in the light of the sun and the radiation in the light of the sun's atmosphere, if we sweep the slit of the spectroscope along the edge of the sun, the part of the spectrum which writes for us what is going on in the solar photosphere, gives us the spectrum crossed by dark lines; the effect of the atmosphere is to absorb the light of the more distant sun at which we look, and the result of the absorption is to give us dark lines.

But when we look at the atmosphere which is resting on the edge of the sun, and look at it where there is no brighter sun behind, absorption no longer comes into play, and we get bright lines. This is what happens when we look at the solar atmosphere above the sun's edge and the solar atmosphere between us and the sun. So long as we are telling the story of the sun, we get

[1] Revised from shorthand notes of a course of Lectures to Working Men at the Museum of Practical Geology during November and December, 1894. (Continued from page 14.)

the dark lines; so long as we are telling the story of the sun's atmosphere, we get bright lines.

We found that the area from which the sunlight comes to us is represented by 1, whereas the area from which the atmospheric light comes to us is represented by 99; so that if the light of the atmosphere is very much dimmer than the light of the central sun, in consequence of its enormous area we may get some light from it intermingled with the light of the sun itself in our spectroscopes.

Therefore, when we look at the complete spectrum, we may lose the dark hydrogen lines in the spectrum of the star, and we may get bright lines instead of dark ones for every line in the spectrum of a star which is filled up by the absorption of a substance the line of which may be seen bright in the spectrum of that star's atmosphere. Thus there is the possibility that when we have to deal with bright lines in the spectrum of an apparent star, we may be dealing with the atmosphere of the star. You will at once see that; if we are dealing with a pure meteoric agglomeration, then of course we shall get that appearance beyond all possible question.

FIG. 25.—The Pleiades (Dr. Roberts).

Now, let me give you one or two cases showing you how this thing works out. The strongest case would be that we should get the bright hydrogen lines putting out the dark hydrogen lines, so that if we got a class of stars without any dark hydrogen lines, we should be justified in supposing that those stars had an enormous atmosphere of hydrogen, and that the fainter bright lines from the larger area just cancelled the effect of the other light from the very much smaller area. Another way that we might expect this thing to work would be that we should not get the bright hydrogen lines entirely putting out the dark hydrogen lines, but that we should get a thinner line in the centre of a broader dark one. Now, that really happens in several stars in the heavens.

I give in Fig. 27 untouched photographs of a star in Orion, and a star in Cassiopeiæ. The latter is very like the star in Orion, because all the absorption lines are common to

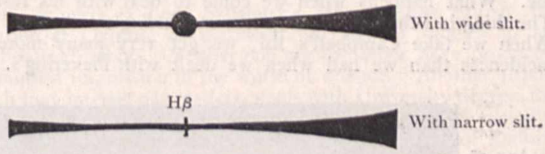


FIG. 26.—Prof. Campbell's observation of the F line of hydrogen in the spectrum of a bright line star.

the two stars; but I may point out to you that we get a bright hydrogen line running down the centre of the dark ones. We may have such an effect produced either by a star having an enormous atmosphere, or by the star with which we are dealing being simply the central condensation of an enormous nebula.

I am bound to say that when I began this work in 1876, I was under the impression that such phenomena were due only to the effects of the atmosphere. But one lives and learns, and since then I have come to the conclusion that that explanation is not the best one, and that when we get such phenomena as those you now see on the screen, we have really to deal with the central condensations of nebulous swarms. I do not hesitate to bring these facts before you, because it is particularly in this connection of thought and experiment and comparison that whatever progress which is now being made in astronomical science is being secured.

Associated with this view we have the statement that stars with bright lines are closely associated with nebulæ, as evidenced by their structure. You will see that there is one method which enables us to compare the bright lines in stars like γ Cassiopeiæ with the nebulæ, as it gives us an opportunity of determining whether or not the bright lines seen in the so-called bright-line stars are or are not the same as the bright lines seen in nebulæ. In the first inquiry in this direction, which consisted of a statistical statement of the number of times certain lines were seen in the spectra, both of nebulæ and of bright-line stars, it was stated that nine lines were coincident, and that and other work done about that time was of such a very trenchant nature that Prof. Pickering, who is one of our very highest authorities in all these matters, accepted at once the grouping together of stars having bright lines in their spectra with the nebulæ. That, you see, was another very definite step in advance indeed.

I can show you a map giving you the evidence of this kind which has been brought into court. We have in it the lines seen in the spectrum of the nebula of Orion, and the longer the line is the stronger it is in the photograph. Then we have underneath the lines recorded in the Orion stars, in the bright line stars, and in the planetary nebulæ; and if you will cast your eyes down these chief lines, you will see that there is a considerable number of lines common to all these bodies.

That is the kind of evidence on which we have been compelled to rely to answer the question: Is there any chemical relationship, and therefore physical relationship, between the bright line stars and the nebula of Orion? And you see the evidence is very strongly in favour of an affirmative statement. Not only does Prof. Pickering accept it, but Prof. Keeler also confirms it. He says the spectra of the planetary nebulæ have a remarkable resemblance to the bright line stars.

But even more fortunate for us than all this is the fact that Prof. Campbell has just finished a most important and laborious study of these stars at the Lick Observatory, and has observed all the lines in the spectra of a much greater number of stars than was available when I began the inquiry; his measurements are very much more accurate than any that were possible then to me. What happens when we come to deal with his results? The thing is a thousand times more convincing than it ever was. When we take Campbell's list, we get very many more coincidences than we had when we dealt with Pickering's. So

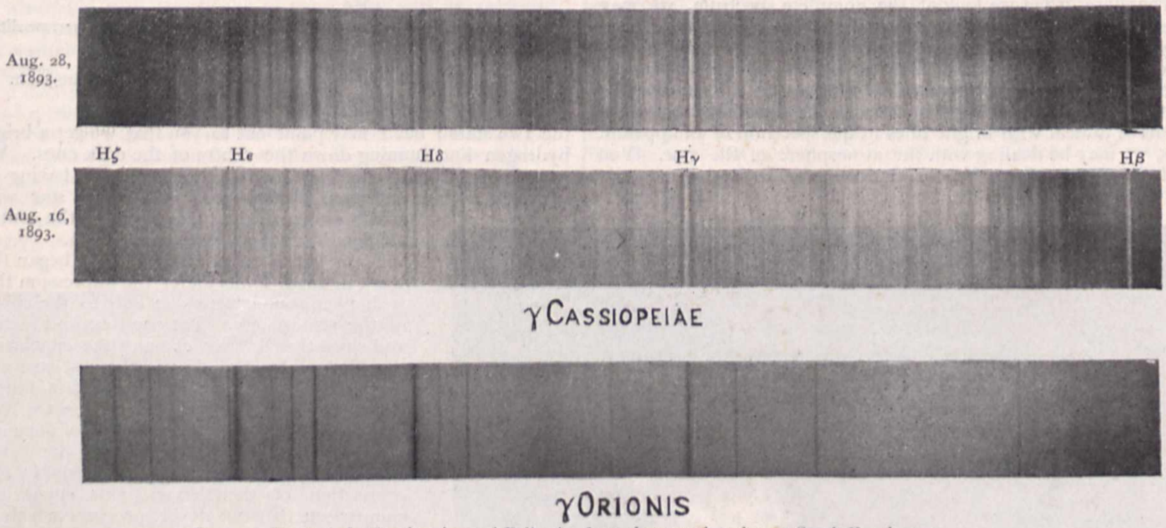


FIG. 27.—Spectra of γ Cassiopeiae and Bellatrix, from photographs taken at South Kensington.

that, the further we go in this inquiry, the greater is the number of coincidences. I told you that in the first inquiry there were nine coincidences observed; now we get nineteen coincidences out of thirty-three. We are therefore justified in saying that the more these phenomena are observed, the more closely associated are they seen to be.

Let us take the case of one of the brightest stars of this class in Argo, the spectrum of a star which my friend Respighi and myself

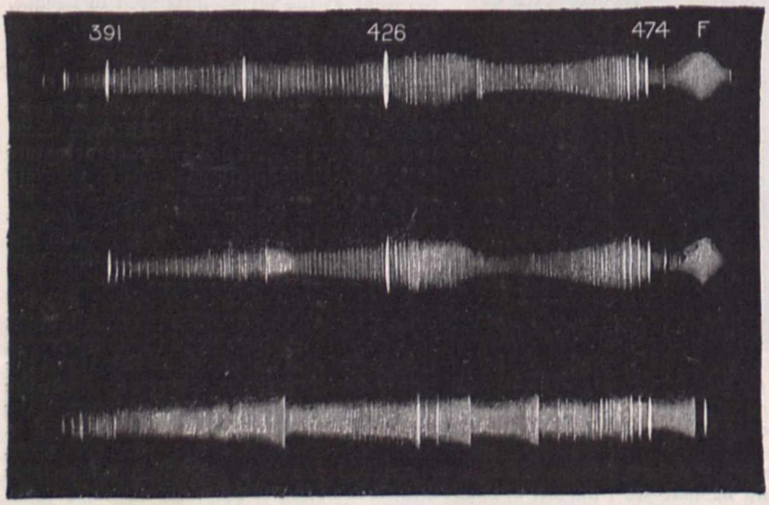


FIG. 28.—Spectrum of carbon at different temperatures.

were the first to see on a very hot night in Madras in 1871, a beautiful spectrum with many bright lines. Now, here these bright lines are indicated in the diagram, and we find by attempting to study their real positions that some of them are due to carbon, and some of them to iron, and some of them to sodium. Prof. Campbell has recently included the study of this star in his work at Lick, and everything that he has done there seems to confirm the idea. The great question is the question of carbon. You know the importance of carbon in a star like this, because we have had carbon differentiating comets from nebulæ, and finally the discovery of carbon in the nebulæ.

I have some apparatus here to show you, which illustrates what one has to do in studying the spectrum of carbon; we must not only deal with it in its ordinary form, and observe the spectrum as seen in the Bunsen flame, and so on, but we must get different compounds of carbon, and expose them to different temperatures and different pressures. That has been done by myself and others; during the last twenty years I suppose I have made thousands of observations on the spectrum of carbon in different forms and conditions.

Fig. 28 shows a series of photographs of the same carbon compound in the same tube, taken under different conditions; you will see that there is a very considerable difference in the intensity of the same bands, as the pressure of the gas has been changed; the particular part of one of the bands which you see enhanced seems to be playing a rôle of considerable importance in the spectra of some of these stars.

This is shown merely as an indication of the kind of minute work which is absolutely essential to determine what is happening in the chemical elements in these bodies.

J. NORMAN LOCKYER.

(*To be continued.*)

THE MANAGEMENT OF EPPING FOREST.

AS a sequel to the continued agitation in the newspapers about Epping Forest, a deputation was received by the Committee at their meeting on Tuesday last at the Guildhall. The object of the deputation was to present the following memorial:—

"Your memorialists have heard with grave concern that your Committee have been urged to put a stop to all further removals of trees in Epping Forest for a period of years. The undersigned have examined the area in question and are of opinion that such a resolution, if sanctioned by your Committee, would be productive of undoubted injury to the Forest, especially as regards those portions of Loughton, Epping, Waltham and Sewardstone Manors which are covered with a dense growth of pollarded trees.

"Those who have approached you with the request to which we have referred do not appear to have apprehended the altered conditions which were brought about by the arrest of pollarding enacted in 1878.

"Many of these pollards, whether single trees or groups, are capable of picturesque development, but only under healthy conditions and with adequate space. To leave them all to grow together—several hundreds to the acre—will lead to mutual destruction, while the continuous overhead shade destroys the undergrowth and the varied vegetation which constitutes the chief charm of a forest and the hope of its reproduction in the future.

"The evils we have indicated are already sufficiently manifest, and it must be obvious to all competent observers that, unless timely steps are taken, a few years' further growth must produce a singularly monotonous, artificial, and unhealthy result.

"Some of us have been familiar with the Forest for many years, and can certify to the great improvement and the increase of natural growth which has already resulted from the operations of your Committee, now continued for many years."

The following signatures were attached:—The Earl of Gainsborough, Viscount Powerscourt, Lords Northbourne, Rayleigh (Lord Lieutenant of Essex) and Walsingham, Sir John Lubbock, Sir W. H. Flower, Right Hon. J. Bryce (President of the Board of Trade), Right Hon. G. Shaw Lefevre (President Local Government Board), Mr. Justice Wills, Sir Robert Hunter (Solicitor to the Post Office), Prof. G. S. Boulger, Mr. Horace T. Brown, Mr. F. Chancellor (Mayor of Chelmsford), Mr. W. Cole (Secretary to the Essex Field Club), Dr. M. C. Cooke, Prof. J. B. Farmer, Prof. W. R. Fisher (Royal Indian Engineering College), Mr. W. Forbes (Agent to the Duke of Richmond and Gordon), Mr. F. Carruthers Gould, Mr. J. E. Harting, Mr. T. V. Holmes, Mr. David Howard (President of the Essex Field Club), Mr. Andrew Johnston (Chairman of the Essex County Council), Mr. H. Joslin (High Sheriff of Essex), Mr. T. Kemble, Colonel Lockwood, M.P., Dr. Maxwell Masters, Prof. R. Meldola, Mr. Briton Riviere, R.A., Prof. E. B. Poulton, Mr. A. Savill, Prof. Stewart, Mr. W. White (Curator of the Ruskin Museum).

The following memorial, bearing the signatures of about forty residents in the Forest district, was at the same time presented:—

"We, the undersigned, being residents in the Forest parishes, beg to state that we have witnessed with satisfaction a great improvement in the aspect of the Forest directly due to the removal, during the past sixteen years, of inferior stems, and to the consequent advance in beauty of those that remain, as well as the encouragement of healthy young growth. We are certain that it will be an irreparable misfortune if the careful thinning which has been hitherto carried out is not steadily continued.

"We further beg to assure the Committee that in our opinion the operations in Hawk Wood, so far from being excessive, still fall short of what is required for the healthy growth of oak trees.

"In Monk Wood there is already a marked improvement following on your removal, eighteen months ago, of a proportion of the poorest pollarded trees. The same is true, even in a more marked degree, of Lord's Bushes. We believe that, if the gentlemen who have appeared as critics of your management were to judge of it by the appearance of the portions thinned three or four years after thinning, instead of immediately after, when they necessarily have a bare and unattractive effect, they would themselves be of a different opinion.

"In conclusion, we beg to assure you that the view that the action of the Committee has been destructive is not entertained by those living on the spot who are most qualified to judge."

The deputations were formally introduced by the Chairman of the Essex Council, and the first memorial was presented by Prof. Meldola. The Committee was addressed also by Sir Robert Hunter, Prof. Boulger, and Mr. F. C. Gould. After these representations the public may safely disregard all future expressions of irresponsible and unskilled opinions in the press. The Chairman of the Committee assured the deputation that their policy would not be influenced by such criticisms.

SCIENCE IN THE MAGAZINES.

MR. HERBERT SPENCER'S second article on "Professional Institutions" appears in the *Contemporary*. The article deals with the intimate relation between the priest and the medicine-man of early societies, and shows how the physician was originated from the priest. Many proofs are given that medical treatment was long associated with priestly functions, and that the uncultured mind still believes in some of the methods of the primitive medicine-man. Mr. Spencer has also an article in the *Fortnightly*, in which he exhibits the insecure base upon which Mr. Balfour has laid his "Foundations of Belief," and describes that distinguished author's dialectic efforts, as well as Lord Salisbury's address to the British Association at Oxford, as sacrificial offerings of effigies to an apotheosised public. Neither one nor the other have produced the faintest impression in the world of science. Another article which may interest our readers, deals with University degrees for women, the writer comparing the action of Göttingen, in recently granting a degree to Miss Chisholm, with the policy of Oxford and Cambridge Universities as to women students.

In a superbly illustrated paper, entitled "The Discovery of Glacier Bay," that veteran explorer Mr. John Muir gives, in the *Century*, an account of his journey to the now famous Glacier Bay of Alaska, in 1879. The great public library in Boston is described in the same magazine: its artistic aspects by Mrs. S. Van Rensselaer, and its ideals and working conditions by Mr. Lindsay Swift.

That fluent writer Eha, the author of "A Naturalist on the Prowl" and other equally attractive works, contributes a short paper, entitled "Voices of the Indian Night," to the *Sunday Magazine*. Ethnologists may be interested in an article by Miss A. Spinner in the *National*, on beliefs concerning "Duppies" prevalent in the West Indies. A "Duppy" is not simply the negro equivalent for a ghost, but is regarded as the shadow of the departed.

There are two popularly-written papers in *Longman's*, one, of a Selbornian character, by Mr. H. G. Hutchinson, and another concerned with the natural processes involved in the evolution of soil in general, and golf-links in particular, by Dr. Edward Blake.

Science Gossip has among its articles one on explosions in electric light mains, by Mr. J. A. Wanklyn and Mr. W. J. Cooper, and some suggestions with reference to the work of a scientific society, by the Rev. H. N. Hutchinson. *Chambers's Journal* contains short papers on soluble paper, Scottish goldfields, forest dwarfs of the Congo, and the habits and tastes of Lepidoptera. *Scribner* has some common-sense remarks, by Dr. J. W. Roosevelt, on cycling from a physiological point of view.

We have received, in addition to the magazines named in the foregoing, *Humanitarian* and *Good Words*, but no articles in them call for comment here.

ARGON.[1]

IT is some three or four years since I had the honour of lecturing here one Friday evening upon the densities of oxygen and hydrogen gases, and upon the conclusions that might be drawn from the results. It is not necessary, therefore, that I should trouble you to-night with any detail as to the method by which gases can be accurately weighed. I must take that as known, merely mentioning that it is substantially the same as is used by all investigators nowadays, and introduced more than fifty years ago by Regnault. It was not until after that lecture that I turned my attention to nitrogen; and in the first instance I employed a method of preparing the gas which originated with Mr. Vernon Harcourt, of Oxford. In this method the oxygen of ordinary atmospheric air is got rid of with the aid of ammonia. Air is bubbled through liquid ammonia, and then passed through a red-hot tube. In its passage the oxygen of the air combines with the hydrogen of the ammonia, all the oxygen being in that way burnt up and converted into water. The excess of ammonia is subsequently absorbed with acid, and the water by ordinary desiccating agents. That method is very convenient; and, when I had obtained a few concordant results by means of it, I thought that the work was complete, and that the weight of nitrogen was satisfactorily determined. But then I reflected that it is always advisable to employ more than one method, and that the method that I had used—Mr. Vernon Harcourt's method—was not that which had been used by any of those who had preceded me in weighing nitrogen. The usual method consists in absorbing the oxygen of air by means of red-hot copper; and I thought that I ought at least to give that method

[1] A discourse delivered at the Royal Institution on Friday, April 5, by the Right Hon. Lord Rayleigh, F.R.S.

a trial, fully expecting to obtain forthwith a value in harmony with that already afforded by the ammonia method. The result, however, proved otherwise. The gas obtained by the copper method, as I may call it, proved to be one-thousandth part heavier than that obtained by the ammonia method; and, on repetition, that difference was only brought out more clearly. This was about three years ago. Then, in order, if possible, to get further light upon a discrepancy which puzzled me very much, and which, at that time, I regarded only with disgust and impatience, I published a letter in NATURE inviting criticisms from chemists who might be interested in such questions. I obtained various useful suggestions, but none going to the root of the matter. Several persons who wrote to me privately were inclined to think that the explanation was to be sought in a partial dissociation of the nitrogen derived from ammonia. For, before going further, I ought to explain that, in the nitrogen obtained by the ammonia method, some—about a seventh part—is derived from the ammonia, the larger part, however, being derived as usual from the atmosphere. If the chemically derived nitrogen were partly dissociated into its component atoms, then the lightness of the gas so prepared would be explained.

The next step in the inquiry was, if possible, to exaggerate the discrepancy. One's instinct at first is to try to get rid of a discrepancy, but I believe that experience shows such an endeavour to be a mistake. What one ought to do is to magnify a small discrepancy with a view to finding out the explanation; and, as it appeared in the present case that the root of the discrepancy lay in the fact that part of the nitrogen prepared by the ammonia method was nitrogen out of ammonia, although the greater part remained of common origin in both cases, the application of the principle suggested a trial of the weight of nitrogen obtained wholly from ammonia. This could easily be done by substituting pure oxygen for atmospheric air in the ammonia method, so that the whole, instead of only a part, of the nitrogen collected should be derived from the ammonia itself. The discrepancy was at once magnified some five times. The nitrogen so obtained from ammonia proved to be about one-half per cent. lighter than nitrogen obtained in the ordinary way from the atmosphere, and which I may call for brevity "atmospheric" nitrogen.

That result stood out pretty sharply from the first; but it was necessary to confirm it by comparison with nitrogen chemically derived in other ways. The table before you gives a summary of such results, the numbers being the weights in grams actually contained under standard conditions in the globe employed.

ATMOSPHERIC NITROGEN.

By hot copper (1892)	2·3103
By hot iron (1893)	2·3100
By ferrous hydrate (1894)	2·3102
Mean	2·3102

CHEMICAL NITROGEN.

From nitric oxide	2·3001
From nitrous oxide	2·2990
From ammonium nitrite purified at a read heat	2·2987
From urea	2·2985
From ammonium nitrite purified in the cold	2·2987
Mean	2·2990

The difference is about 11 milligrams, or about one-half per cent.; and it was sufficient to prove conclusively that the two kinds of nitrogen—the chemically derived nitrogen and the atmospheric nitrogen—differed in weight, and therefore, of course, in quality, for some reason hitherto unknown.

I need not spend time in explaining the various precautions that were necessary in order to establish surely that conclusion. One had to be on one's guard against impurities, especially against the presence of hydrogen, which might seriously lighten any gas in which it was contained. I believe, however, that the precautions taken were sufficient to exclude all questions of that sort, and the result, which I published about this time last year, stood sharply out, that the nitrogen obtained from chemical sources was different from the nitrogen obtained from the air.

Well, that difference, admitting it to be established, was sufficient to show that some hitherto unknown gas is involved in the matter. It might be that the new gas was dissociated nitrogen, contained in that which was too light, the chemical nitrogen—and at first that was the explanation to which I leaned; but certain experiments went a long way to discourage such a supposition. In the first place, chemical evidence—and in this matter I am greatly dependent upon the kindness of chemical friends—tends to show that, even if ordinary nitrogen could be dissociated at all into its component atoms, such atoms would not be likely to enjoy any very long continued existence. Even ozone goes slowly back to the more normal state of oxygen; and it was thought that dissociated nitrogen would have even a greater tendency to revert to the normal condition. The experiment suggested by that remark was as follows—to keep chemical nitrogen—the too light nitrogen which might be supposed to contain dissociated molecules—for a good while, and to examine whether it changed in density. Of course it would be useless to shut up gas in a globe and weigh it, and then, after an interval, to weigh it again, for there would be no opportunity for any change of weight to occur, even although the gas within the globe had undergone some chemical alteration. It is necessary to re-establish the standard conditions of temperature and pressure which are always understood when we speak of filling a globe with gas, for I need hardly say that filling a globe with gas is but a figure of speech. Everything depends upon the temperature and pressure at which you work. However, that obvious point being borne in mind, it was proved by experiment that the gas did not change in weight by standing for eight months—a result tending to show that the abnormal lightness was not the consequence of dissociation.

Further experiments were tried upon the action of the silent electric discharge—both upon the atmospheric nitrogen and upon the chemically derived nitrogen—but neither of them seemed to be sensibly affected by such treatment; so that, altogether, the balance of evidence seemed to incline against the hypothesis of abnormal lightness in the chemically derived nitrogen being due to dissociation, and to suggest strongly, as almost the only possible alternative, that there must be in atmospheric nitrogen some constituent heavier than true nitrogen.

At that point the question arose, What was the evidence that all the so-called nitrogen of the atmosphere was of one quality? And I remember—I think it was about this time last year, or a little earlier—putting the question to my colleague Prof. Dewar. His answer was that he doubted whether anything material had been done upon the matter since the time of Cavendish, and that I had better refer to Cavendish's original paper. That advice I quickly followed, and I was rather surprised to find that Cavendish had himself put this question quite as sharply as I could put it. Translated from the old-fashioned phraseology connected with the theory of phlogiston, his question was whether the inert ingredient of the air is really all of one kind; whether all the nitrogen of the air is really the same as the nitrogen of nitre. Cavendish not only asked himself this question, but he endeavoured to answer it by an appeal to experiment.

I should like to show you Cavendish's experiment in something like its original form. He inverted a U tube filled with mercury, the legs standing in two separate mercury cups. He then passed up, so as to stand above the mercury, a mixture of nitrogen, or of air, and oxygen; and he caused an electric current from a frictional electrical machine like the one I have before me to pass from the mercury in the one leg to the mercury in the other, giving sparks across the intervening column of air. I do not propose to use a frictional machine to-night, but I will substitute for it one giving electricity of the same quality of the construction introduced by Mr. Wimshurst, of which we have a fine specimen in the Institution. It stands just outside the door of the theatre, and will supply an electric current along insulated wires, leading to the mercury cups; and, if we are successful, we shall cause sparks to pass through the small length of air included above the columns of mercury. There they are; and after a little time you will notice that the mercury rises, indicating that the gas is sensibly absorbed under the influence of the sparks and of a piece of potash floating on the mercury. It was by that means that Cavendish established his great discovery of the nature of the inert ingredient in the atmosphere, which we now call nitrogen; and, as I have said, Cavendish himself proposed the question, as distinctly as we can do, Is this inert ingredient all of one kind? and he proceeded to test that question. He found, after days and weeks of protracted experiment, that, for the most part, the nitrogen of the atmosphere absorbed in this manner, was converted into nitrous acid; but that there was a small residue remaining after prolonged treatment with sparks, and a final absorption of the residual oxygen. That residue amounted to about $\frac{1}{120}$ part of the nitrogen taken; and Cavendish draws the conclusion that if there be more than one inert ingredient in the atmosphere, at any rate the second ingredient is not contained to a greater extent than $\frac{1}{120}$ part.

I must not wait too long over the experiment. Mr. Gordon tells me that a certain amount of contraction has already occurred; and if we project the U upon the screen, we shall be able to verify the fact. It is only a question of time for the greater part of the gas to be taken up, as we have proved by preliminary experiments.

In what I have to say from this point onwards, I must be understood as speaking as much on behalf of Prof. Ramsay as for myself. At the first, the work which we did was to a certain extent independent. Afterwards we worked in concert, and all that we have published in our joint names must be regarded as being equally the work of both of us. But, of course, Prof. Ramsay must not be held responsible for any chemical blunder into which I may stumble to-night.

By his work and by mine the heavier ingredient in atmospheric nitrogen which was the origin of the discrepancy in the densities has been isolated, and we have given it the name of "argon." For this purpose we may use the original method of Cavendish, with the advantages of modern appliances. We can procure more powerful electric sparks than any which Cavendish could command by the use of the ordinary Ruhmkorff coil stimulated by a battery of Grove cells; and it is possible so to obtain evidence of the existence of argon. The oxidation of nitrogen by that method goes on pretty quickly. If you put some ordinary air, or, better still, a mixture of air and oxygen, in a tube in which electric sparks are made to pass for a certain time, then in looking through the tube you observe the well-known reddish-orange fumes of the oxides of nitrogen. I will not take up time in going through the experiment, but will merely exhibit a tube already prepared (image on screen).

One can work more efficiently by employing the alternate currents from dynamo machines which are now at our command. In this Institution we have the advantage of a public supply; and if I pass alternate currents originating in Deptford through this Ruhmkorff coil, which acts as what is now called a "high potential transformer," and allow sparks from the secondary to pass in an inverted test tube between platinum points, we shall be able to show in a comparatively short time a pretty rapid absorption of the gases. The electric current is led into the working chamber through bent glass tubes containing mercury, and provided at their inner extremities with platinum points. In this arrangement we avoid the risk, which would otherwise be serious, of a fracture just when we least desired it. I now start the sparks by switching on the Ruhmkorff to the alternate current supply; and, if you will take note of the level of the liquid representing the quantity of mixed gases included, I think you will see after, perhaps, a quarter of an hour that the liquid has very appreciably risen, owing to the union of the nitrogen and the oxygen gases under the influence of the electrical discharge, and subsequent absorption of the resulting compound by the alkaline liquid with which the gas space is enclosed.

By means of this little apparatus, which is very convenient for operations upon a moderate scale, such as for analyses of "nitrogen" for the amount of argon that it may contain, we are able to get an absorption of about 80 cubic centimetres per hour) or about 4 inches along this test tube, when all is going well. In order, however, to obtain the isolation of argon on any considerable scale by means of the oxygen method, we must employ an apparatus still more enlarged. The isolation of argon requires the removal of nitrogen, and, indeed, of very large quantities of nitrogen, for, as it appears, the proportion of argon contained in atmospheric nitrogen is only about 1 per cent., so that for every litre of argon that you wish to get you must eat up some hundred litres of nitrogen. That, however, can be done upon an adequate scale by calling to our aid the powerful electric discharge now obtainable by means of the alternate current supply and high potential transformers.

In what I have done upon this subject I have had the advantage of the advice of Mr. Crookes, who some years ago drew special attention to the electric discharge or flame, and showed that many of its properties depended upon the fact that it had the power of causing, upon a very considerable scale, a combination of the nitrogen and the oxygen of the air in which it was made.

I had first thought of showing in the lecture room the actual apparatus which I have employed for the concentration of argon but the difficulty is that, as the apparatus has to be used, the working parts are almost invisible, and I came to the conclusion that it would really be more instructive as well as more convenient to show the parts isolated, a very little effort of imagination being then all that is required in order to reconstruct in the mind the actual arrangements employed.

First, as to the electric arc or flame itself. We have here a transformer made by Pike and Harris. It is not the one that I have used in practice; but it is convenient for certain purposes, and it can be connected by means of a switch with the alternate currents of 100 volts furnished by the Supply Company. The platinum terminals that you see here are modelled exactly upon the plan of those which have been employed in practice. I may say a word or two on the question of mounting. The terminals require to be very massive on account of the heat evolved. In this case they consist of platinum wire doubled upon itself six times. The platinums are continued by iron wires going through glass tubes, and attached at the ends to the copper leads. For better security, the tubes themselves are stopped at the lower ends with corks and charged with water, the advantage being that, when the whole arrangement is fitted by means of an indiarubber stopper into a closed vessel, you have a witness that, as long as the water remains in position, no leak can have occurred through the insulating tubes conveying the electrodes.

Now, if we switch on the current and approximate the points sufficiently, we get the electric flame. There you have it. It is, at present, showing a certain amount of soda. That in time would burn off. After the arc has once been struck, the platinums can be separated; and then you have two tongues of fire ascending almost independently of one another, but meeting above. Under the influence of such a flame, the oxygen and the nitrogen of the air combine at a reasonable rate, and in this way the nitrogen is got rid of. It is now only a question of boxing up the gas in a closed space, where the argon concentrated by the combustion of the nitrogen can be collected. But there are difficulties to be encountered here. One cannot well use anything but a glass vessel. There is hardly any metal available that will withstand the action of strong caustic alkali and of the nitrous fumes resulting from the flame. One is practically limited to glass. The glass vessel employed is a large flask with a single neck, about half full of caustic alkali. The electrodes are carried through the neck by means of an indiarubber bung provided also with tubes for leading in the gas. The electric flame is situated at a distance of only about half an inch above the caustic alkali. In that way an efficient circulation is established; the hot gases as they rise from the flame strike the top, and then as they come round again in the course of the circulation they pass sufficiently close to the caustic alkali to ensure an adequate removal of the nitrous fumes.

There is another point to be mentioned. It is necessary to keep the vessel cool; otherwise the heat would soon rise to such a point that there would be excessive generation of steam, and then the operation would come to a standstill. In order to meet this difficulty the upper part of the vessel is provided with a water-jacket, in which a circulation can be established. No doubt the glass is severely treated, but it seems to stand it in a fairly amiable manner.

By means of an arrangement of this kind, taking nearly three-horse power from the electric supply, it is possible to consume nitrogen at a reasonable rate. The transformers actually used are the "Hedgehog" transformers of Mr. Swinburne, intended to transform from 100 volts to 2400 volts. By Mr. Swinburne's advice I have used two such, the fine wires being in series so as to accumulate the electrical potential and the thick wires in parallel. The rate at which the mixed gases are absorbed is about seven litres per hour; and the apparatus, when once fairly started, works very well as a rule, going for many hours without attention. At times the arc has a trick of going out, and it then requires to be restarted by approximating the platinums. We have already worked fourteen hours on end, and by the aid of one or two automatic appliances it would, I think, be possible to continue operations day and night.

The gases, air and oxygen in about equal proportions, are mixed in a large gasholder, and are fed in automatically as required. The argon gradually accumulates; and when it is desired to stop operations the supply of nitrogen is cut off, and only pure oxygen allowed admittance. In this way the remaining nitrogen is consumed, so that, finally, the working vessel is charged with a mixture of argon and oxygen only, from which the oxygen is removed by ordinary well-known chemical methods. I may mention that at the close of the operation, when the nitrogen is all gone, the arc changes its appearance, and becomes of a brilliant blue colour.

I have said enough about this method, and I must now pass on to the alternative method which has been very successful in Prof. Ramsay's hands—that of absorbing nitrogen by means of red-hot magnesium. By the kindness of Prof. Ramsay and Mr. Matthews, his assistant, we have here the full scale apparatus before us almost exactly as they use it. On the left there is a reservoir of nitrogen derived from air by the simple removal of oxygen. The gas is then dried. Here it is bubbled through sulphuric acid. It then passes through a long tube made of hard glass and charged with magnesium in the form of thin turnings. During the passage of the gas over the magnesium at a bright red heat, the nitrogen is absorbed in a greater degree, and the gas which finally passes through is immensely richer in argon than that which first enters the hot tube. At the present time you see a tolerably rapid bubbling on the left, indicative of the flow of atmospheric nitrogen into the combustion furnace; whereas, on the right, the outflow is very much slower. Care must be taken to prevent the heat rising to such a point as to soften the glass. The concentrated argon is collected in a second gas-holder, and afterwards submitted to further treatment. The apparatus employed by Prof. Ramsay in the subsequent treatment is exhibited in the diagram, and is very effective for its purpose; but I am afraid that the details of it would not readily be followed from any explanation that I could give in the time at my disposal. The principle consists in the circulation of the mixture of nitrogen and argon over hot magnesium, the gas being made to pass round and round until the nitrogen is effectively removed from it. At the end that operation, as in the case of the oxygen method, proceeds somewhat slowly. When the greater part of the nitrogen is gone, the remainder seems to be unwilling to follow, and it requires somewhat protracted treatment in order to be sure that the nitrogen has wholly disappeared. When I say "wholly disappeared," that, perhaps, would be too much to say in any case. What we can say is that the spectrum test is adequate to show the presence, or at any rate to show the addition, of about 1½ per cent. of nitrogen to argon as pure as we can get it; so that it is fair to argue that any nitrogen at that stage remaining in the argon is only a small fraction of 1½ per cent.

I should have liked at this point to be able to give advice as to which of the two methods—the oxygen method or the magnesium method—is the easier and the more to be recommended; but I confess that I am quite at a loss to do so. One difficulty in the comparison arises from the fact that they have been in different hands. As far as I can estimate, the quantities of nitrogen eaten up in a given time are not very different. In that respect, perhaps, the magnesium method has some advantage; but, on the other hand, it may be said that the magnesium process requires a much closer supervision, so that, perhaps, fourteen hours of the oxygen method may not unfairly compare with eight hours or so of the magnesium method. In practice a great deal would depend upon whether in any particular laboratory alternate currents are available from a public supply. If the alternate currents are at hand, I think it may probably be the case that the oxygen method is the easier; but, otherwise, the magnesium method would, probably, be preferred, especially by chemists who are familiar with operations conducted in red-hot tubes.

I have here another experiment illustrative of the reaction between magnesium and nitrogen. Two rods of that metal are suitably mounted in an atmosphere of nitrogen, so arranged that we can bring them into contact and cause an electric arc to form between them. Under the action of the heat of the electric arc the nitrogen will combine with the magnesium; and if we had time to carry out the experiment we could demonstrate a rapid absorption of nitrogen by this method. When the experiment was first tried, I had hoped that it might be possible, by the aid of electricity, to start the action so effectively that the magnesium would continue to burn independently under its own developed heat in the atmosphere of nitrogen. Possibly, on a larger scale, something of this sort might succeed, but I bring it forward here only as an illustration. We turn on the electric current, and bring the magnesiums together. You see a brilliant green light, indicating the vaporisation of the magnesium. Under the influence of the heat the magnesium burns, and there is collected in the glass vessel a certain amount of brownish-looking powder which consists mainly of the nitride of magnesium. Of course, if there is any oxygen present it has the preference, and the ordinary white oxide of magnesium is formed.

The gas thus isolated is proved to be inert by the very fact of its isolation. It refuses to combine under circumstances in which nitrogen, itself always considered very inert, does combine—both in the case of the oxygen treatment and in the case of the magnesium treatment; and these facts are, perhaps, almost enough to justify the name which we have suggested for it. But, in addition to this, it has been proved to be inert under a considerable variety of other conditions such as might have been expected to tempt it into combination. I will not recapitulate all the experiments which have been tried, almost entirely by Prof. Ramsay, to induce the gas to combine. Hitherto, in our hands, it has not done so; and I may mention that recently, since the publication of the abstract of our paper read before the Royal Society, argon has been submitted to the action of titanium at a red heat, titanium being a metal having a great affinity for nitrogen, and that argon has resisted the temptation to which nitrogen succumbs. We never have asserted, and we do not now assert, that argon can under no circumstances be got to combine. That would, indeed, be a rash assertion for any one to venture upon; and only within the last few weeks there has been a most interesting announcement by M. Berthelot, of Paris, that, under the action of the silent electric discharge, argon can be absorbed when treated in contact with the vapour of benzine. Such a statement, coming from so great an authority, commands our attention; and if we accept the conclusions, as I suppose we must do, it will follow that argon has, under those circumstances, combined.

Argon is rather freely soluble in water. That is a thing that troubled us at first in trying to isolate the gas; because, when one was dealing with very small quantities, it seemed to be always disappearing. In trying to accumulate it we made no progress. After a sufficient quantity had been prepared, special experiments were made on the solubility of argon in water. It has been found that argon, prepared both by the magnesium method and by the oxygen method, has about the same solubility in water as oxygen—some two-and-a-half times the solubility of nitrogen. This suggests, what has been verified by experiment, that the dissolved gases of water should contain a larger proportion of argon than does atmospheric nitrogen. I have here an apparatus of a somewhat rough description, which I have employed in experiments of this kind. The boiler employed consists of an old oil-can. The water is supplied to it and drawn from it by coaxial tubes of metal. The incoming cold water flows through the outer annulus between the two tubes. The outgoing hot water passes through the inner tube, which ends in the interior of the vessel at a higher level. By means of this arrangement the heat of the water which has done its work is passed on to the incoming water not yet in operation, and in that way a limited amount of heat is made to bring up to the boil a very much larger quantity of water than would otherwise be possible, the greater part of the dissolved gases being liberated at the same time. These are collected in the ordinary way. What you see in this flask is dissolved air collected out of water in the course of the last three or four hours. Such gas, when treated as if it were atmospheric nitrogen, that is to say after removal of the oxygen and minor impurities, is found to be decidedly heavier than atmospheric nitrogen to such an extent as to indicate that the proportion of argon contained is about double. It is obvious, therefore, that the dissolved gases of water form a convenient source of argon, by which some of the labour of separation from air is obviated. During the last few weeks I have been supplied from Manchester by Mr. Macdougall, who has interested himself in this matter, with a quantity of dissolved gases obtained from the condensing water of his steam engine.

As to the spectrum, we have been indebted from the first to Mr. Crookes, and he has been good enough to-night to bring some tubes which he will operate, and which will show you at all events the light of the electric discharge in argon. I cannot show you the spectrum of argon, for unfortunately the amount of light from a vacuum tube is not sufficient for the projection of its spectrum. Under some circumstances the light is red, and under other circumstances it is blue. Of course when these lights are examined with the spectroscope—and they have been examined by Mr. Crookes with great care—the differences in the colour of the light translate themselves into different groups of spectrum lines. We have before us Mr. Crookes' map, showing the two spectra upon a very large scale. The upper is the spectrum of the blue light; the lower is the spectrum of the red light; and it will be seen that they differ very greatly. Some lines are common to both; but a great many lines are seen only in the red, and others are seen only in the blue. It is astonishing to notice what trifling changes in the conditions of the discharge bring about such extensive alterations in the spectrum.

One question of great importance, upon which the spectrum throws light is, Is the argon derived by the oxygen method really the same as the argon derived by the magnesium method? By Mr. Crookes' kindness I have had an opportunity of examining the spectra of the two gases side by side, and such examination as I could make revealed no difference whatever in the two spectra, from which, I suppose, we may conclude either that the gases are absolutely the same, or, if they are not the same, that at any rate the ingredients by which they differ cannot be present in more than a small proportion in either of them.

My own observations upon the spectrum have been made principally at atmospheric pressure. In the ordinary process of sparking, the pressure is atmospheric; and, if we wish to look at the spectrum, we have nothing more to do than to include a jar in the circuit, and put a direct-vision prism to the eye. At my request, Prof. Schuster examined some tubes containing argon at atmospheric pressure prepared by the oxygen method, and I have here a diagram of a characteristic group. He also placed upon the sketch some of the lines of zinc, which were very convenient as directing one exactly where to look. (See Fig. 1.)

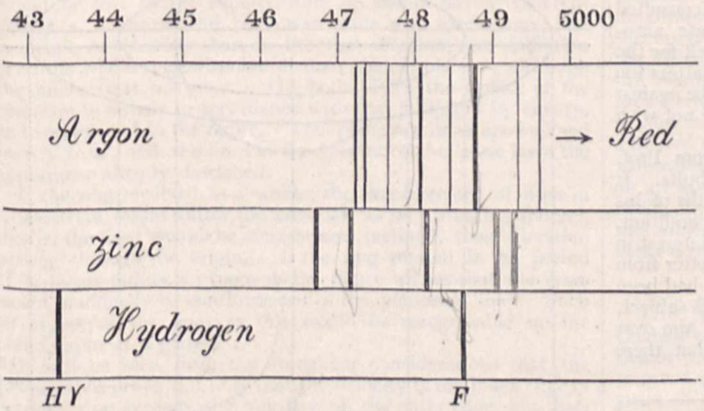


FIG. 1.

Within the last few days, Mr. Crookes has charged a radiometer with argon. When held in the light from the electric lamp, the vanes revolve rapidly. Argon is anomalous in many respects, but not, you see, in this.

Next, as to the density of argon. Prof. Ramsay has made numerous and careful observations upon the density of the gas prepared by the magnesium method, and he finds a density of about 19·9 as compared with hydrogen. Equally satisfactory observations upon the gas derived by the oxygen method have not yet been made, but there is no reason to suppose that the density is different, such numbers as 19·7 having been obtained.

One of the most interesting matters in connection with argon, however, is what is known as the ratio of the specific heats. I must not stay to elaborate the questions involved, but it will be known to many who hear me that the velocity of sound in a gas depends upon the ratio of two specific heats—the specific heat of the gas measured at constant pressure, and the specific heat measured at constant volume. If we know the density of a gas, and also the velocity of sound in it, we are in a position to infer this ratio of specific heats; and by means of this method, Prof. Ramsay has determined the ratio in the case of argon, arriving at the very remarkable result that the ratio of specific heats is represented by the number 1·65, approaching very closely to the theoretical limit, 1·67. The number 1·67 would indicate that the gas has no energy except energy of translation of its molecules. If there is any other energy than that, it would show itself by this number dropping below 1·67. Ordinary gases, oxygen, nitrogen, hydrogen, &c., do drop below, giving the number 1·4. Other gases drop lower still. If the ratio of specific heats is 1·65, practically 1·67, we may infer then that the whole energy of motion is translational; and from that it would seem to follow by arguments which, however, I must not stop to elaborate, that the gas must be of the kind called by chemists monatomic.

I had intended to say something of the operation of determining the ratio of specific heats, but time will not allow. The result is, no doubt, very awkward. Indeed I have seen some indications that the anomalous properties of argon are brought as a kind of accusation against us. But we had the very best intentions in the matter. The facts were too much for us; and all we can do now is to apologise for ourselves and for the gas. Several questions may be asked, upon which I should like to say a word or two, if you will allow me to detain you a little longer. The first question (I do not know whether I need ask it) is, Have we got hold of a new gas at all? I had thought that that might be passed over, but only this morning I read in a technical journal the suggestion that argon was our old friend nitrous oxide. Nitrous oxide has roughly the density of argon; but that, as far as I can see, is the only point of resemblance between them.

Well, supposing that there is a new gas, which I will not stop to discuss, because I think the spectrum alone would be enough to prove it, the next question that may be asked is, Is it in the atmosphere? This matter naturally engaged our earnest attention at an early stage of the inquiry. I will only indicate in a few words the arguments which seem to us to show that the answer must be in the affirmative.

In the first place, if argon be not in the atmosphere, the original discrepancy of densities which formed the starting point of the investigation remains unexplained, and the discovery of the new gas has been made upon a false clue. Passing over that, we have the evidence from the blank experiments, in which nitrogen originally derived from chemical sources is treated either with oxygen or with magnesium, exactly as atmospheric nitrogen is treated. If we use atmospheric nitrogen, we get a certain proportion of argon, about 1 per cent. If we treat chemical nitrogen in the same way, we get, I will not say absolutely nothing, but a mere fraction of what we should get had atmospheric nitrogen been the subject. You may ask, why do we get any fraction at all from chemical nitrogen? It is not difficult to explain the small residue, because in the manipulation of the gases large quantities of water are used; and, as I have already explained, water dissolves argon somewhat freely. In the processes of manipulation some of the argon will come out of solution, and it remains after all the nitrogen has been consumed.

Another wholly distinct argument is founded upon the method of diffusion introduced by Graham. Graham showed that if you pass gas along porous tubes you alter the composition, if the gas is a mixture. The lighter constituents go more readily through the pores than do the heavier ones. The experiment takes this form. A number of tobacco pipes—eight in the actual arrangement—are joined together in series with indiarubber junctions, and they are put in a space in which a vacuum can be made, so that the space outside the porous pipes is vacuous, or approximately so. Through the pipes ordinary air is led. One end may be regarded as open to the atmosphere. The other end is connected with an aspirator so arranged that the gas collected is only some 2 per cent of that which leaks through the porosities. The case is like that of an Australian river drying up almost to nothing in the course of its flow. Well, if we treat air in that way, collecting only the small residue which is less willing than the remainder to penetrate the porous walls, and then prepare "nitrogen" from it by removal of oxygen and moisture, we obtain a gas heavier than atmospheric nitrogen, a result which proves that the ordinary nitrogen of the atmosphere is not a single body, but is capable of being divided into parts by so simple an agent as the tobacco pipe.

If it be admitted that the gas is in the atmosphere, the further question arises as to its nature.

At this point I would wish to say a word of explanation. Neither in our original announcement at Oxford, nor at any time since, until January 31, did we utter a word suggesting that argon was an element; and it was only after the experiments upon the specific heats that we thought that we had sufficient to go upon in order to make any such suggestion in public. I will not insist that that observation is absolutely conclusive. It is certainly strong evidence. But the subject is difficult, and one that has given rise to some difference of opinion among physicists. At any rate this property distinguishes argon very sharply from all the ordinary gases.

One question which occurred to us at the earliest stage of the inquiry, as soon as we knew that the density was not very different from 21, was the question of whether, possibly, argon could be a more condensed form of nitrogen, denoted chemically by the symbol N_3. There seem to be several difficulties in the way of this supposition. Would such a constitution be consistent with the ratio of specific heats (1·65)? That seems extremely doubtful. Another question is, Can the density be really as high as 21, the number required on the supposition of N_3? As to this matter, Prof. Ramsay has repeated his measurements of density, and he finds that he cannot get even so high as 20. To suppose that the density of argon is really 21, and that it appears to be 20 in consequence of nitrogen still mixed with it, would be to suppose a contamination with nitrogen out of all proportion to what is probable. It would mean some 14 per cent. of nitrogen, whereas it seems that from $1\frac{1}{2}$ to 2 per cent. is easily enough detected by the spectroscope. Another question that may be asked is, Would N_3 require so much cooling to condense it as argon requires?

There is one matter on which I would like to say a word—the question as to what N_3 would be like if we had it? There seems to be a great discrepancy of opinions. Some high authorities, among whom must be included, I see, the celebrated Mendeléef, consider that N_3 would be an exceptionally stable body; but most of the chemists with whom I have consulted are of opinion that N_3 would be explosive, or, at any rate, absolutely unstable. That is a question which may be left for the future to decide. We must not attempt to put these matters too positively. The balance of evidence still seems to be against the supposition that argon is N_3, but for my part I do not wish to dogmatise.

A few weeks ago we had an eloquent lecture from Prof. Rücker on the life and work of the illustrious Helmholtz. It will be known to many that during the last few months of his life Helmholtz lay prostrate in a semi-paralysed condition, forgetful of many things, but still retaining a keen interest in science. Some little while after his death we had a letter from his widow, in which she described how interested he had been in our preliminary announcement at Oxford upon this subject, and how he desired the account of it to be read to him over again. He added the remark, "I always thought that there must be something more in the atmosphere."

A SPECTROSCOPIC PROOF OF THE METEORIC CONSTITUTION OF SATURN'S RINGS.[1]

THE hypothesis that the rings of Saturn are composed of an immense multitude of comparatively small bodies, revolving around Saturn in circular orbits, has been firmly established since the publication of Maxwell's classical paper in 1859. The grounds on which the hypothesis is based are too well known to require special mention. All the observed phenomena of the rings are naturally and completely explained by it, and mathematical investigation shows that a solid or fluid ring could not exist under the circumstances in which the actual ring is placed.

The spectroscopic proof which Prof. Keeler has recently obtained of the meteoric constitution of the ring, is of interest because it is the first *direct* proof of the correctness of the accepted hypothesis, and because it illustrates in a very beautiful manner the fruitfulness of Doppler's principle, and the value of the spectroscope as an instrument for the measurement of celestial motions.

Since the relative velocities of different parts of the ring would be essentially different under the two hypotheses of rigid structure and meteoric constitution, it is possible to distinguish between these hypotheses by measuring the motion of different parts of the ring in the line of sight. The only difficulty is to find a method so delicate that the very small differences of velocity in question may not be masked by instrumental errors. Success in visual observations of the spectrum is hardly to be expected.

[1] Abridged from a paper, by Prof. James E. Keeler, in the *Astrophysical Journal* for May.

After a number of attempts, Prof. Keeler obtained two fine photographs of the lower spectrum of Saturn on April 9 and 10 of the present year. The exposure in each case was two hours, and the image of the planet was kept very accurately central on the slit-plate. After the exposure the spectrum of the Moon was photographed on each side of the spectrum of Saturn, and nearly in contact with it. Each part of the lunar spectrum has a width of about one millimetre, which is also nearly the extreme width of the planetary spectrum. On both sides of the spectrum of the ball of the planet are the narrow spectra of the ansæ of the ring. The length of the spectrum from *b* to D is 23 millimetres.

These photographs not only show very clearly the relative displacement of the lines in the spectrum of the ring, due to the opposite motions of the ansæ, but exhibit another peculiarity, which is of special importance in connection with the subject of the present paper. The planetary lines are strongly inclined, in consequence of the rotation of the ball, but the lines in the spectra of the ansæ do not follow the direction of the lines in the central spectrum; they are nearly parallel to the lines of the comparison spectrum, and, in fact, as compared with the lines of the ball, have a slight tendency to incline in the opposite direction. Hence the outer ends of these lines are less displaced than the inner ends. Now it is evident that if the ring rotated as a whole, the velocity of the outer edge would exceed that of the inner edge, and the lines of the ansæ would be inclined in the same direction as those of the ball of the planet. If, on the other hand, the ring is an aggregation of satellites revolving around Saturn, the velocity would be greatest at the inner edge, and the inclination of lines in the spectra of the ansæ would be reversed. The photographs are therefore a direct proof of the approximate correctness of the latter supposition.

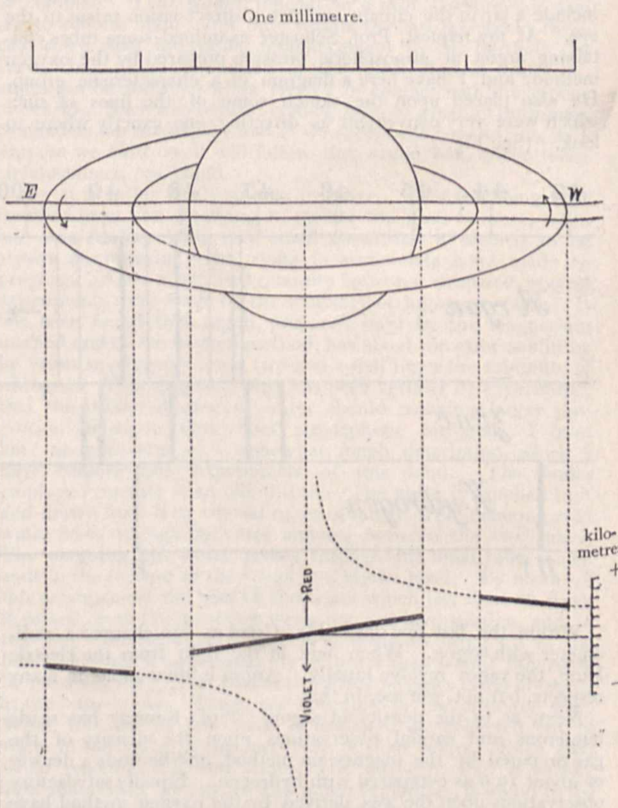


Fig. 1.

It is interesting to determine the form of a line in the spectrum of Saturn when the slit is in the major axis of the ring, on the assumption that the planet rotates as a solid body, and that the ring is a swarm of particles revolving in circular orbits according to Kepler's third law. At present the motion of the system as a whole is neglected. The upper part of Fig. 1 represents the image of Saturn on the slit of the spectroscope (the scale above it applies to the instrument used at Allegheny), and the narrow horizontal line in the lower part of the figure represents an undisplaced line in the spectrum, or solar line.

By Doppler's principle, the displacement of any point on this line is proportional to the velocity in the line of sight. The inclination of the planetary line to the solar line can be expressed by a simple formula. It is also possible to determine the form of a line in the spectrum of the ring, regarded as a collection of satellites, by the application of Kepler's third law. With the computed motions of different parts of the system, the dotted curves in the figure were plotted. For the ordinates, however, twice the calculated values were taken, since the displacement of a line, due to motion in the line of sight, is doubled in the case of a body which shines by reflected and not by inherent light, provided (as in this case) the Sun and the Earth are in sensibly the same direction from the body. The planetary line is drawn to the same scale, and the heavy lines in the figure represent accurately the aspect of a line in the spectrum of Saturn, with the slit in the axis of the ring, as photographed with a spectroscope having about three times the dispersion of the instrument used by Prof. Keeler.

The width of slit used is also represented in the figure.

If the whole system has a motion in the line of sight, the lines in the figure will be displaced towards the top or the bottom, as the case may be, but their relative positions will not be altered.

It is evident that in making a photograph of this kind the image must be kept very accurately in the same position on the slit-plate, as otherwise the form of the lines shown in the figure would be lost by the superposition of points having different velocities. The second plate was made with special care, and as the air was steadier than on the first occasion, the definition is on the whole somewhat better than that of plate 1, although the difference is not great. On both plates the aspect of the spectrum is closely in accordance with that indicated by theory, and represented in the figure. The planetary lines are inclined from 3° to 4°, and the lines in the spectra of the ansæ have the appearance already described.

If the ring revolved as a whole, the displacement of lines in its spectrum would follow the same law as for a rotating sphere; that is, the lines would be straight and inclined, their direction passing through the origin. If the ring rotated in the period of its mean radius, a glance at the figure shows that the lines would practically be continuations of the planetary lines. Such an aspect of the lines as this would be recognisable on the photographs at a glance.

It will be seen from the foregoing considerations that the photographs prove not only that the velocity of the inner edge of Saturn's ring exceeds the velocity of the outer edge, but that, within the limits of error of the method, the relative velocities at different parts are such as to satisfy Kepler's third law.

Besides (1) the proof of the meteoric constitution of the rings, explained above, each line of the photographs gives (2) the period of rotation of the planet, (3) the mean period of the rings, (4) the motion of the whole system in the line of sight. Prof. Keeler has measured a number of lines on each plate, and compared the results with the computed values of the corresponding quantities.

The results for (2) and (3) from both photographs are:

(2) Velocity of limb $= 10{\cdot}3 \pm 0{\cdot}4$ kilometres,
(3) Mean velocity of ring $= 18{\cdot}0 \pm 0{\cdot}3$ kilometres;

the computed values being 10·29 and 18·78 kilometres respectively.

Prof. Keeler has not yet determined from his photographs the motion of the whole system in the line of sight.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Mr. T. J. I. Bromwich, Scholar of St. John's College, is the Senior Wrangler of the year. There are thirty Wranglers, of whom St. John's furnishes ten, and Trinity six. One lady only is among the Wranglers, namely Miss N. A. L. Thring, of Newnham, who is placed twenty-third in the list.

The Tyson Medal for Astronomy is awarded to Mr. A. Y. G. Campbell, of Trinity.

Sir Edward Maunde Thompson, K.C.B., has been appointed the first Sandars Reader in Bibliography for the year 1895-6.

The Board of Managers of the Arnold Gerstenberg Studentship give notice that a Studentship on this Foundation will be offered for competition in 1896. The competition will be open to men and women who have obtained honours in Part I. or Part II. of the Natural Sciences Tripos, and whose first term of residence was not earlier than the Michaelmas term of 1890. The Studentship will be awarded to the writer of the best essay on one of the six subjects printed below. The essays must be sent before October 1, 1896, to Dr. Sidgwick, Newnham College, Cambridge. The Studentship will be of the value of nearly £90. It will be tenable for one year only, but subject to no conditions of tenure.

Subjects:—"A statement of the physicist's 'working conceptions' of Matter and Motion, together with a discussion of the philosophical questions to which they give rise." "A criticism of the diverse views that have prevailed from the time of Newton onwards as to the conceivability or otherwise of *Actio in distans.*" "A critical examination of the doctrines of J. S. Mill concerning the ground of Induction and the Methods of Inductive Inquiry." "The limits and relations of mechanical and teleological explanations of natural phenomena." "A brief historical account and a critical examination of the views which make the phenomena of life dependent on the existence of a special vital principle." "Natural Selection considered as a special example of the general principle of Evolution."

WITH the view of encouraging University Extension students to take up systematic courses of study, the Local Examinations and Lectures Syndics have remodelled their scheme of Local Lectures Certificates, and have made several other changes of importance. The certificates are now arranged so as to form successive steps in a ladder of continuous work, beginning with the Terminal Certificate for one term's work passing through the Sessional Certificate for a year's work to the Vice-Chancellor's Certificate of Systematic Study for four years' work. There is also an Affiliation Certificate obtainable only at centres affiliated to the University. This certificate is accepted by the Education Department as qualifying a person to be recognised as an assistant teacher. This system is thus adapted to the needs of persons who merely desire a general acquaintance with the subjects taught, as well as to students who are anxious to make a more thorough study of them.

THE Technical Education Board of the London County Council will proceed in July next to award five of its valuable Senior County Scholarships. These scholarships, which are reserved as a rule for young men and women under nineteen years of age, are intended to enable promising and deserving students, who would otherwise be unable to afford the expense, to go through a three years' course at a University or at a Technical Institute of University rank. They are limited to those candidates whose parents are in receipt of not more than £400 a year. The scholarships not only give free tuition, but also a money payment of £60 during each of the years that the scholarship is tenable. They are primarily intended to encourage the pursuit of some branch of science, art, or technology, but they may also be awarded for the promotion of studies in modern languages or other branches of education. In making the award, the Board takes mainly into account the record of each candidate's past career and distinctions, and the evidence as to ability, industry, and good character which the candidate is able to supply. At the same time it reserves the right to apply any examination test that it may think fit. Full particulars may be obtained from the Secretary of the Board, at 13 Spring Gardens, S.W. Candidates should send in their names not later than June 29.

THE summer assembly of the National Home-Reading Union will be held at Leamington Spa, from Saturday, June 29, to Monday, July 8. Lectures will be given by Major Leonard Darwin, M.P., on "The National and International Advantages of the Study of Geography"; Sir Robert Ball, on "Comets"; Mr. H. Yule Oldham, on "The Discovery of America"; Mr. J. E. Marr on "The Geology of the District"; Mr. G. F. Scott Elliot, on "Interesting Problems in Botany, suggested by the Flora of the District." There will also be a conference on "The Wider Education," at which the chair will be taken by Dr. Hill, Master of Downing College, Cambridge. Addresses will be given by Miss Mondy, Dr. R. D. Roberts, a representative of the Oxford Delegacy for University Extension, Mr. T.

C. Horsfall, Mr. J. E. Flower (Secretary Recreative Evening Schools Association), and other speakers. Excursions will be made to a number of places in the district, and Profs. W. Ridgeway and T. McKenny Hughes, Mr. J. G. Marr, Mr. Scott Elliot, and others, will accompany the excursions for the purpose of explaining the archæology, geology, and botany of the places visited.

MR. C. J. FORTH, Mathematical Master at Bolton Grammar School, has been appointed Lecturer in Mathematics at the Plymouth Technical Schools.

THE textile department of the Yorkshire College at Leeds has just been added to by the opening of a museum which is to contain a complete collection of woven samples and models of weaving machinery. The building has cost the Clothworkers' Company £3000, and they will, to the extent of £1200, defray the cost of equipping the museum. The opening ceremony was performed by Mr. Sidney Wilson, Master of the Clothworkers, assisted by Mr. J. E. Horne, his senior warden, and other members and officials. Twenty years ago the Clothworkers established the textile department of the college at the cost of £34,000, and they make an annual grant to it of £2500.

SOCIETIES AND ACADEMIES.

LONDON.

Chemical Society, May 16.—Mr. A. G. Vernon Harcourt, President, in the chair.—The following papers were read:—Kjeldahl's method for the determination of nitrogen, by B. Dyer. The author describes an exhaustive series of experiments made with the various modifications of Kjeldahl's process in order to ascertain their applicability to organic nitrogen compounds of different types.—Note on liquation in crystalline standard gold, by T. K. Rose.—Preparation of the active lactic acids, and the rotation of their metallic salts in solution, by T. Purdie and J. W. Walker. The optical activity of the metallic lactates in aqueous solution is in the opposite sense to that of the active acid from which they are derived; cryoscopic determinations made with the lithium and strontium lactates show that the racemic form is resolved into the two active ones in aqueous solution.—Derivatives of succinyl and phthalyl dithiocarbimides, by A. E. Dixon and R. E. Doran. On heating succinyl or phthalyl chlorides with lead thiocyanate and dry benzene, succinyl or phthalyl dithiocarbimide, respectively, is formed; a number of derivatives of these two substances are described.—The action of nitrous acid on dibromaniline, $C_6H_3BrBr.NH_2$ = 1 : 4 : 2, by R. Meldola and E. R. Andrews. The authors were unsuccessful in preparing a diazoxide from dibromaniline under the conditions which yield these compounds in the naphthalene series; in the present case a diazoamido-derivative, $C_6H_3Br_2.N_2.NH.C_6H_3Br_2$, was obtained.—A new modification of benzilosazone, by H. Ingle and H. H. Mann. The unstable α-benzilosazone, corresponding to the known β-isomeride, is obtained, together with dibenzaldiphenylhydrotetrazone by the action of iodine on a mixture of benzalphenylhydrazone and sodium ethoxide.—Affinity of weak bases, by J. Walker and E. Aston.—Substitution derivatives of urea and thiourea, by A. E. Dixon. The properties of a number of substituted ureas are described.—Note on some reactions of ammonium salts, by W. R. E. Hodgkinson and N. E. Bellairs. Fused ammonium nitrate and sulphate are readily attacked by many metals with evolution of ammonia; other products, such as hydrogen and sulphites, also result in certain cases.

Zoological Society, May 21.—Lieut.-Colonel H. H. Godwin-Austen, F.R.S., Vice-President, in the chair.—Dr. R. Bowdler Sharpe gave an account of the ornithological collection made by Dr. Donaldson Smith during his recent expedition into Somaliland and Gallaland. The present series contained about 500 specimens, which were referred to 182 species. Of these twelve were considered to be new to science.—Mr. G. A. Boulenger, F.R.S., read a synopsis of the genera and species of apodal batrachians, and gave a description of a new genus and species proposed to be called *Bdellophis vittatus*.—Lieut.-Colonel H. H. Godwin-Austen, F.R.S., read a list of the land-molluscs of the Andaman and Nicobar groups of islands in the Bay of Bengal, and gave descriptions of some new species, together with a complete account of the distribution of all the species in the various islands of these two groups.—A communication was read from Dr. J. Anderson, F.R.S., containing the description of a new species of hedgehog from Somaliland, which he proposed to name *Erinaceus sclateri*.—A communication from Mr. R. Lydekker contained notes on the structure and habits of the sea-otter (*Latax lutris*).—A communication was read from Dr. B. C. A. Windle containing remarks on some double malformations observed amongst fishes.—Mr. F. E. Beddard, F.R.S., read a paper on the visceral and muscular anatomy of *Cryptoprocta*, dealing chiefly with the brain, alimentary canal, and muscles of this carnivore.

Geological Society, May 22.—Dr. Henry Woodward, F.R.S., President, in the chair.—On a human skull and limb-bones found in the palæolithic terrace-gravels at Galley Hill, Kent, by E. T. Newton, F.R.S. A human skull with lower jaw and parts of the limb-bones were obtained by Mr. R. Elliott from the high-terrace gravels at Galley Hill, in which numerous palæolithic implements have been found. The skull is extremely long and narrow, its breadth-index being about 64, it is hyperdolichocephalic; it is likewise much depressed, having a height-index of about 67. The small extent of the cranium in both height and width shows that it has undergone little or no post-mortem compression, although it has become somewhat twisted in drying. The supraciliary ridges are large, the forehead somewhat receding, the probole prominent, and the occiput flattened below. All the chief sutures are obliterated. Three lower molars and two premolars are in place and are well worn, the three molars being as nearly as possible equal in size. The limb-bones indicate an individual about 5 ft. 1 in. in height. These remains were compared with the fossil human relics which have been found in Britain and on the continent of Europe, as well as with the dolichocephalic races now living, and their relations to the "Spy," "River-bed," "Long-barrow," "Eskimo," and other types were pointed out. The gravels, in which these human bones were found, overlie the chalk at a height of about 90 feet above the Thames, and are about 10 feet thick. They form part of the high-terrace gravels extending from Dartford Heath to Northfleet, and their palæolithic age is shown by the numerous implements which have been found in them, as well as by the mammalian remains which have been met with in similar beds near by, although not at Galley Hill. The human bones were seen *in situ* by Mr. R. Elliott and Mr. Matthew Heys, both of whom speak positively as to the undisturbed condition of the 8 feet of gravel which overlay the bones when discovered.—Geological notes of a journey round the coast of Norway and into Northern Russia, by G. S. Boulger. The author accompanied the Jackson-Harmsworth Polar Expedition as far as Archangel, and returned by way of the River Dvina. His observations relate mainly to four points: the origin of the foliation of the Norwegian gneiss; the question of raised beaches on the north-western coast of Norway; the boulders and boulder-formation of Northern Russia; and the Trias of the Dvina valley. Between Christiansund and Tromsö the author was struck with the wide-sweeping folds of the foliation-planes of the gneissose rocks, which appeared to him more readily explicable on a theory of dynamo-metamorphism of rocks originally in part igneous, than by any process of diagenesis. He noted that the terraces observed in the transverse fjords would be perfectly explained by the formation of ice-dammed lakes, though the terraces of the Gulf of Onega seemed less dubious raised beaches than those of the north-west of Norway. He confirmed the views of previous writers that many of the boulders of the boulder-formation of Northern Russia were of Scandinavian origin. The beds on the Dvina consist of sands and loams, often coloured red, with bands of alabaster and anhydrite. The strata are horizontal or inclined at a low angle. North of Ustyug Veliki the strata are marked as Permian on the Russian maps, and those to the south as Trias, but the author saw no perceptible break in the succession.—On some Foraminifera of Rhætic Age, from Wedmore in Somerset, by Frederick Chapman. The author has examined six samples of clays and limestones collected from a quarry south-east of the village of Wedmore, which has yielded Megalosaurian remains. The microscopical details of the various clay-washings were given, and the great abundance of some forms of the acervuline foraminifer *Stacheia* was noticed. In a comparison made with the foraminiferal faunæ of the older and younger rocks respectively, the Rhætic fauna shows marked affinities with both the Upper Palæozoic and the Liassic facies. Twenty-six species of foraminifera, chiefly of arenaceous types, were described, nine of which are new forms.

PARIS.

Academy of Sciences, June 4.—M. Lœwy in the chair.—Notice on the works of M. Neumann, by M. J. Bertrand. Franz Neumann, correspondent of the Geometry Section, died at Königsberg on May 23 last. He will be chiefly remembered by his great memoir "On the theory of undulations," in which he considers luminous vibrations as occurring in the plane of polarisation. His great mathematical ability was especially shown by the general formulæ in which he expressed the results of Faraday's discoveries and Lenz's rules.—Volume of salts in their aqueous solutions, by M. Lecoq de Boisbaudran. The author compares the dilatometer and pyknometer methods, and describes a special form of dilatometer used in this work.—A contribution to the study of the acetylcyanacetic esters of the general formula,

$$C_nH_{2n+1}.CO.CH\begin{cases}CN\\CO_2R\end{cases}$$

or

$$C_nH_{2n+1}.CH:COH.CH\begin{cases}CN\\CO_2R\end{cases},$$

by M. A. Haller.—A projected Swedish exploration of Tierra del Fuego, by M. Daubrée. The Swedish Government is about to send out an expedition of three persons to explore the unknown parts of Tierra del Fuego, and the Argentine Government will assist by conveying the members of the exploring party to their destination and finding attendants. MM. Nordenskiöld, Dusen, and Ohlin will arrive at Buenos Ayres in September, and hope, during the Antarctic summer, to explore those parts of the island unvisited by the French expedition of 1882-1883. They aim particularly at gathering material for a comparison of the southern island with Northern Europe; for instance, the quaternary rocks of Tierra del Fuego will be compared with rocks of the same age in the boreal continents.—Report on the project of a balloon expedition to the Polar regions, by M. J. A. Andrée (Committee: MM. Faye, Daubrée, Blanchard). It is reported that the conditions for the success of such an expedition have been fully considered, the funds necessary have been raised, and the expedition will set out from Spitzbergen in July of the coming year. The conditions formulated by M. Andrée are: (1) The balloon must have an ascensional power sufficient to carry three persons, all the necessary instruments, food for four months, arms, a boat transformable into a sledge, and the ballast, in all about 3000 kilograms. (2) The balloon must have the quality of impermeability to such an extent that it can remain thirty days in the air. (3) It must be to a certain extent dirigable.—Memoirs presented: By M. A. Lucas, on the centrifugal and centripetal forces and on a new value of *g*; by M. Bonnal, an alcoholimeter allowing the simultaneous estimation of alcohol and extract in wines.—Observations of Charlois' planet BX, made with the Coudé equatorial at Algiers Observatory, by MM. Rambaud and Sy.—On the movement of a plane figure in its plane, by M. A. Pellet.—On a category of groups of substitutions associated with groups of which the order equals the degree, by M. R. Levavasseur.—On the density of helium (a letter from M. Clève to M. Berthelot). (See Notes).—On the reduction of nitrous oxide by metals in presence of water, by MM. Paul Sabatier and J. B. Senderens. The results fully confirm those formerly obtained. Dissolved nitrous oxide is reduced to the state of nitrogen by magnesium, zinc, iron, and even cadmium, with the simultaneous formation of a little ammonia.—Heat of formation of sodium acetylide, by M. de Forcrand.—On phthalyl chloride and phthalide, by M. Paul Rivals.—Conductibility of some β-ketonic esters, by M. J. Guinchant. The sodium salts of the *cyanomethinic* acids behave quite normally with regard to conductibility. These acids, as well as acetylacetone, obey Ostwald's law (K = const.) as far as can be expected with compounds containing an acid group and an ether function. Their chemical affinity deduced from thermochemical data agrees well with that obtained from their conductibilities. The values of K for homologous acids diminish as the molecular weight increases.—Estimation of volatile acids in wines, by M. E. Burcker.—Considerations on the chemical phenomena of ossification, by M. C. Chabrie.—On the flora of the coal deposits of Asia Minor, and the presence in this flora of the genus *Phyllotheca*, by M. R. Zeiller.—On the chlorosis of American vines and its treatment by sulphuric acid, by MM. Gastine and Degrully. The authors find treatment by ferrous sulphate and by sulphuric acid to yield identical results; it is concluded that the sulphuric acid is the active agent in overcoming chlorosis.—*Oidium albicans*, a general pathogenic agent. Pathogeny of morbid disorders, by MM. Charrin and Ostrowsky. In conclusion, the study of the general disease which determines inoculation by *Oidium albicans* reveals a series of processes peculiar to this fungus. Comparing these processes with those due to bacteria, some analogies, but more differences, are observed.

BERLIN.

Physiological Society, May 3.—Prof. H. Munk, President, in the chair.—After the President had dwelt on the loss physiology had suffered by the death of Prof. Ludwig, Prof. I. Munk spoke on Kjeldahl's method for determining nitrogen in organic substances as compared with Dumas' method. The former has largely supplanted the latter owing to the greater ease with which it may be carried out, but some chemists have found it less accurate than that of Dumas, notably when applied to casein. The speaker had recently repeated the analysis, and found the above statement confirmed as long as he used oxide of copper in Kjeldahl's process. But when he used oxide of mercury (Wilfarth) or potassium bichromate (Krüger), the two methods gave identical results for the nitrogen. He had also found Kjeldahl's method applicable to nitrogenous compounds with closed rings, such as pyridin, chinolin, &c. Prof. Gad developed Fick's hypothesis as to the two-fold nature of the chemical processes taking place in a contracting muscle, a hypothesis to which he gave his support on the basis of his experiments made together with Heymans (see NATURE, vol. xl. p. 288), on the influence of temperature on muscular contraction. He described several experiments on the production of heat in muscles contracting isotonically and isometrically, which can be most readily explained on the basis of Fick's hypothesis of two mutually interfering chemical processes.

May 17.—Prof. H. Munk, President, in the chair.—Dr. W. Cowl spoke on the action of diaphragms in microscopes, and explained a general improvement he had obtained by applying an iris-diaphragm to the ocular, capable of regulation from the outside.—Dr. Thierfelder gave an account of experiments made with Dr. Nutan on guinea-pigs.

Physical Society, May 10.—Prof. von Bezold, President, in the chair.—After election of officers, Prof. König spoke on experiments made in conjunction with Dr. Rubens on the distribution of energy in the spectrum of a triplex burner. The methods employed made it possible to measure the energy by means of a bolometer between W.L. 800 μ to W.L. 420 μ, and at the same time to measure the intensity of the light at the same part of the spectrum by means of a Lummer photometer. He dealt in great detail with the correction which is necessary on account of the fact that diffused light acts on the bolometer in addition to that of each given wave-length. The curve of energy thus obtained was so steep that it could only be recorded by logarithms; the energy of the extreme red was more than a thousand times as great as that of the blue. By comparing the relative intensities of the rays of a normal amylacetate flame with that of the above burner, the distribution of energy in the amylacetate flame was deduced by calculation, and in this case also the curve was very steep; the energy of the red end being 300 times that of the blue. The curve for the spectrum of the cloudless sky ascended from the red towards the blue end, whereas it was nearly horizontal for the light from a cloud.—Prof. Neesen exhibited two automatic mercurial air-pumps.

May 24.—Prof. du Bois Reymond, President, in the chair.—Prof. Neesen described an automatic mercurial valve added to his automatic pumps.—Prof. von Bezold spoke on a theory of terrestrial magnetism, based on the construction of the isonomals of terrestrial magnetic potentials. He explained the methods by which he had calculated the isonomals, and discussed the results observable on a chart of the same for the year 1880. The mean values of magnetic potential are simple functions of geographical latitude, and the isonomals have both their poles in the southern hemisphere. The determination of the potential and the construction of the lines of equilibrium is far simpler by Prof. von Bezold's method than by the employment of Gauss's formulæ, and will make it possible to attack a whole series of important problems concerning terrestrial magnetism. As soon as isonomal charts have been constructed for different periods it will be possible to draw conclusions as to the causes of magnetic disturbances.

NEW SOUTH WALES.

Linnean Society, April 24.—The President, Mr. Henry Deane, in the chair.—Description of a fly-catcher, presumably new, by C. W. de Vis. The name *Arses lorealis* was proposed for a fly-catcher from Cape York, with the lower surface entirely white in the male, ochreous in a band on the lower throat in the female, and with white lores in both sexes.—On the specific identity of the Peripatus, hitherto supposed to be *P. leuckarti*, Sänger, by J. J. Fletcher. It was shown by a translation of Sänger's paper (in Russian) descriptive of the Australian Peripatus, that *P. insignis*, Dendy, is a synonym of *P. leuckarti*. Various considerations point to the following classification of Australian Peripatus: *Peripatus leuckarti*, Säng. Australian Peripatus with 14 or 15 pairs of walking legs; without or with an accessory tooth at the base of the fang of the outer jaw blade, or with several (three in one case, indications of even more in another). Males with a pair of (accessory genital) pores between the genital papilla and the anus; with a white tubercle on each leg of the first pair only, or of the last pair only, or of all or only some of the pairs with the exception of the first. (1) *P. leuckarti*, Säng., var. *typica* (*P. leuckarti*, Säng.; *P. insignis*, Dendy). With 14 pairs of walking legs; no accessory tooth; New South Wales, Victoria, Tasmania. (2) *P. leuckarti*, Säng., var. *occidentalis*. With 15 pairs of walking legs; no accessory tooth; West Australia (Mr. A. M. Lea). (3) *P. leuckarti*, Säng., var. *orientalis* (*P. leuckarti*, Säng.) With 15 pairs of walking legs; with one or more accessory teeth; viviparous; Queensland, New South Wales. (4) The Victorian Peripatus described by Dr. Dendy as *P. oviparus*. Victoria and Tasmania (probably—for a specimen in the Macleay Museum).—Description of *Peripatus oviparus*, by Dr. A. Dendy. In the light of knowledge gained from the translation of Sänger's description of *P. leuckarti*, already referred to, and the consequent necessary revision of the nomenclature at present in use, the author dealt at length with the larger Victorian Peripatus, which he proposed to call *P. oviparus*. —Notes on the sub-family *Brachyscelinæ*, with descriptions of new species, by W. W. Froggatt. This paper comprised notes upon the classification and systematic position of the gall-making Coccids, some corrections in the earlier descriptions of *Brachyscelis Thorntoni*, together with descriptions of three new species proposed to be called *B. dipsaciformis*, *B. sessilis*, and *B. rosiformis*.—On a Fiddler Ray (*Trygonorhina fasciata*) with abnormal pectoral fins, by J. P. Hill. The specimen observed, a young male 26·9 cm. long, presented a striking appearance by reason of the anterior portion of each pectoral fin being separated from the head by a wide and deep notch. The significance of the abnormality was discussed at some length.

AMSTERDAM.

Royal Academy of Sciences, April 18.—Prof. Van de Sande Bakhuyzen in the chair.—Prof. MacGillavry gave a sketch of two methods employed by him to detect the adulteration of butter with less than one per cent. of oleo-margarine or with oils.—Prof. Pekelharing read a paper on the objections raised against his view as to the nature of the fibrine ferment, viz. that it is a compound of nucleoproteid and lime, more particularly on the objections brought forward by Halliburton, who, by his important and extensive investigations, has contributed so much to our knowledge in this department. The author had found (1) that artificial fibrine ferment, prepared by treating nucleoproteid first with lime-water and then with carbonic acid, became only partly soluble by being kept under alcohol for a long time, whereas when treated in the same manner as Schmidt's ferment, it yielded a powerful fibrino-plastic solution; (2) that magnesium sulphate-plasma remained liquid, not for want of nucleoproteid, but because it did not contain enough calcium salts. The magnesium sulphate prevented the combination of nucleoproteid and lime; but when the combination had once been brought about, $MgSO_4$ impeded the coagulation in a much smaller degree. Magnesium sulphate plasma was coagulated by artificial fibrine just as well as by ferment from blood serum; (3) that intravenous injection of Schmidt's and Hammerston's ferment had the same consequence as the injection of a small quantity of nucleoproteid, viz. Wooldridge's "negative phase," a retardation of the coagulation of the blood which was effused from the vessels. On the other hand, if a more concentrated solution of fibrine ferment, prepared by Ganger's method, was injected into a vein of a rabbit, the animal died of intravascular coagulation.—Prof. Schoute proved that the number of crystallographic forms of the regular system in a space of n dimensions is $2^n - 1$.—Prof. Kamerlingh Onnes communicated the results of investigations by Mr. A. Lebret in the Leyden laboratory: (1) compensation method of the observation of Hall's effect; (2) on the dissymmetry of Hall's effect in bismuth when the directions of the magnetic field are opposite to each other. In every plate there are two perpendicular directions of great importance. The primary electrodes being attached in accordance with these directions, there is no dissymmetry. When they are attached in a direction making an angle α with one of them, the Hall effect is given by $H \pm \frac{1}{2}(K_1 - K_2) \sin 2\alpha$. It is explained by a difference between the variations of resistance through magnetisation K_1 and K_2 in two perpendicular directions.

BOOKS AND SERIALS RECEIVED.

BOOKS.—Dairy Bacteriology: Dr. E. von Freudenreich, translated by Prof. J. R. A. Davis (Methuen).—Petrology for Students: A. Harker (Cambridge University Press).—A Text-Book of Zoogeography: F. E. Beddard (Cambridge University Press).—Hydrodynamics: Prof. H. Lamb (Cambridge University Press).—Museums Association. Report of Proceedings, &c., at the Fifth Annual General Meeting, held in Dublin, June 26 to 29, 1894 (Sheffield).—The Horticulturist's Rule-Book: L. H. Bailey, 3rd edition (Macmillan).—Agriculture: R. H. Wallace (Chambers).—Off the Mill: Bishop G. F. Browne (Smith, Elder).—Bibliotheca Geographica, Band 1 (Berlin, Kühl).

SERIALS.—Journal of the Anthropological Institute, May (K. Paul).—Bulletin of the American Mathematical Society, May (New York, Macmillan).—Proceedings of the Physical Society of London, June (Taylor).—Report of the Marlborough College Natural History Society, 1894 (Marlborough).—Journal of the Chemical Society, June (Gurney).—Geological Magazine, June (Dulau).—Phycological Memoirs, Part 3 (Dulau).—Ethnographische Beiträge zur Kenntnis des Karolinen Archipels, 3 Heft (Leiden, Trap).—Natural History of Plants, Part 13; Kerner and Oliver (Blackie).—American Journal of Science, June (New Haven).—Materials for a Flora of the Malayan Peninsula, No. 7: Dr. G. King (Calcutta).—Journal of the Asiatic Society of Bengal, Vol. lxiii. Part 2, No. 4 (Calcutta).—Ditto Vol. lxiv. Part 2, No. 1 (Calcutta).—Science Progress, June (Scientific Press, Ltd.).—Strand Magazine, June (Newnes).—Picture Magazine, June (Newnes).—Engineering Magazine, June (Tucker).

CONTENTS.