THURSDAY, APRIL 4, 1889.

A "PRACTICAL MAN" ON ELECTRICAL UNITS.

AT the last meeting of the British Association an energetic attempt was made to prove that the progress of the human race has been chiefly due to the "practical man," and this teaching was quickly caught up and explained to mean that the triumphs of industry have been achieved without the help of workers in the field of pure science. We have before us a periodical which is instructive reading when viewed in the light of the discussion on this subject. It is a recently issued number of the Transactions of an Institute connected with one of the most important of our national industries. Among other provincial organizations it holds high rank. Meetings are held at frequent intervals, papers are read, and not only are they printed at length, but the discussions by which they are followed are also given in full. Two papers were recently communicated to this Society on the application of electricity to the industry with which its members are chiefly connected. One of them was by a gentleman who, according to the Chairman, represented an important firm "who have done more of this class of work in this country than anyone else," and he added that on this account "anything that he may say will carry great weight and give information to us unattainable otherwise, and I am sure we shall therefore appreciate the more, the trouble he has been at in coming here." The whole of the last number of the Transactions of the Institute—forty-one pages in all—is filled with the discussion that followed the speech in which this passage occurred.

We give these details because we wish to make it clear from the outset that the thing which we are about to discuss was not done in a corner. No circumstance which could add to the formality and importance of the occasion was wanting. A full account of the whole is published, and is matter for public comment.

Let us now see how the gentleman thus introduced and who calls himself a practical man utilized this opportunity. In the course of his remarks he discussed the theory of the electric motor, and then proceeded to say that he was frequently asked

"why we measure electrical quantities in volts, amperes, and watts, rather than in foot-pounds. Well, the main reason is that the practical units, the *volt*, *ampere*, and *ohm*, are so easy to measure and so simply connected by the equation

$$C = \frac{E}{R},$$

in which C = the ampere, the unit of current;
E = the volt, " " pressure;
R = the ohm, " " resistance.

Now, the product of one *ampere* and one *volt* = one *watt*, and 746 *watts* = one horse-power.

"In the mechanical units 33,000 foot-pounds = one horse-power per minute; and if we are doing electrical work at the rate of 746 watts per minute, we are doing 33,000 foot-pounds per minute. The electrical unit of work is then related to the mechanical unit by the ratio $\frac{746}{33,000}$, or one *watt* is equal to 4·4 foot-pounds.

"We could thus measure all electrical quantities in foot-pounds if it were desirable, but it is far more convenient to measure the volts and amperes, and then estimate the horse-power. If anyone, however, wishes to express electrical quantities in foot-pounds, he will now be able to do so; but bear in mind that the electrical horse-power is equal to the mechanical brake horse-power."

It is hardly necessary to point out the blunders with which every sentence of this passage teems, but it is necessary that they should be made evident to "practical men." To talk of measuring a volt or an ampere in foot-pounds is as ridiculous as to propose to measure miles or gallons in seconds; and yet, when the inquirer asks why it is not done, he is told that "all electrical quantities could be measured in foot-pounds if it were desirable."

In the equation C = E/R, the symbols, when applied to the practical system of units, do not represent the ampere, volt, or ohm, but certain numbers of amperes, volts, and ohms.

The "horse-power per minute" is an old friend; it has as much meaning as the statement that a man has walked four miles per hour per second. Of course, the equally absurd phrase "watts per minute" follows.

Next, we find the "electrical unit of *work*" confounded with the unit of power, and the statement that their ratio is 746/33000; the truth being that the ratio between the watt and the horse-power is 1 : 746. As the first of these fractions is nearly seventeen times greater than the second, this sentence, in so far as it means anything, makes the watt seventeen times too great.

Lastly, like a cockney, who, having put in unnecessary *h*'s, proceeds to redress the balance by leaving them out where ordinary mortals insert them, the speaker, having liberally distributed "per minute" where it makes nonsense, proceeds to leave it out where it is absolutely necessary. The statement that "1 *watt* is equal to 4·4 foot-pounds" is wrong, (1) because the ratio of 33,000 to 746 is 44, and not 4·4; (2) because it is necessary to add "per minute" after "foot-pounds." The "practical man" of this type apparently thinks that it is quite unimportant whether a machine does 44 foot-pounds of work in a second, or in a minute, or in all eternity.

In drawing attention to this extraordinary series of statements, it is necessary to point out that nobody desires to interfere with such engineers provided they confine themselves to fulfilling the useful function of putting together machines they do not understand, and learning by bitter experience, but, be it well understood, at other people's expense, practical wrinkles which may no doubt often be of great service.

When, however, they pose as electricians, go down to the provinces as instructors of the ignorant, and print their opinions on "Sir William Thompson's" (*sic*) address to the Institute of Electrical Engineers, the matter becomes serious.

The speech from which we have quoted was characterized as "clear and explicit." A "junior member of the Institute enjoyed this lecture more than any we have had since I have joined," and thought that "we have had a very instructive meeting."

Thus these guileless representatives of a great British industry sit agape while enjoying phrases which have as much meaning to them, and to the speaker, as "that blessed word Mesopotamia" to a village crone. But, surely, there must be a limit to their endurance. Even from the point of view of the most practical of "practical men" it must be a serious thing to find, in one line containing eleven words and ten figures, the ratio of the watt to the horse-power made seventeen times larger than it really is, a decimal point misplaced so that the watt is made ten times less than it really is, the vulgar fraction from which the ratio is deduced in decimals inverted, and the essential statement as to the time in which the work is done omitted in a calculation of power.

If men are to be "practical" and nothing else, they must at all events be accurate. If they are to use formulæ which they do not understand, they must at all events know how to use them correctly. If the representative of a great firm—in explaining the answer to a question, which is not sprung upon him unawares, but which he himself puts forward as one which he has been "frequently" asked, and to which, therefore, he volunteers a reply—can in addition to employing language which makes all his statements nonsense, turn a fraction upside down, misplace a decimal, and, finally, pass the report of his speech for press with these blunders uncorrected, how can outsiders avoid suspecting that similar mistakes may be not infrequent in calculations upon which specifications and contracts are based, and on which very "practical" questions of success or failure, and of pounds, shillings, and pence, depend?

And now for the application of all this. The speaker gave as his reason for using volts and ohms that they are "so easy to measure, and so simply connected." Do the supporters of the "practical man" think that this easy measurement, this simple connection, came by accident? Do they think that this system which they find so useful could have been elaborated by men who, when it has been before the world for years, cannot open their mouths or put pen to paper, without showing in every sentence that they are absolutely ignorant of the fundamental conceptions on which the whole system is based, and equally incapable of using it correctly?

In the course of the evening the same speaker claimed for himself and for practical men that "we don't want to know what [electricity] is, but what it will do." He, and such as he, have yet to learn that what electricity has done is mainly the outcome of the work of men who did want to know what it is, being certain that if they knew that they could make it do more than under any other conditions.

They elaborated a system of units which our authority finds *easy* and *simple*, by the aid of investigations which even now require a good knowledge of mathematics and physics on the part of those who would really understand them, and which at the date of their original performance were masterpieces which only intellects of a very high order and knowledge of a very wide grasp could have achieved. Among them were numbered some engineers, but these ranked among them not because they were practical men who did not "want to know what electricity is," but because they had risen above such wretched cant, and had become not only "practical" but scientific.

The mischief done by the Bath meeting is not yet ended. It may or may not be a good joke to discuss whether Sir William Thomson is or is not an engineer. But the views then expounded are, all over the country, leading so-called "electrical engineers," who are ignorant of all that concerns what is, by their own confession, the easy part of their subject, to fling their cheap sneers at men who do "want to know what electricity is," who have made it possible to use and measure electrical quantities, and who have directly or indirectly created the very trades by which their detractors earn their daily bread.

"We don't want to know" will be the ruin of British industry, unless its leaders use their influence to crush the spirit indicated by this expression. In the March number of the *Fortnightly Review*, Lord Carnarvon relates that chairs, of which the various parts are fastened by glue, as is the custom in this country, will not hold together in the warmer climate of Australia. English makers did not know, perhaps did not "want to know, this. "The Austrian manufacturers, on the other hand, had discovered the cause of the defect, and, by a very simple alteration in the fastening, had practically driven out of a large part of the country our home-made furniture." "Wherever I went," says Lord Carnarvon, "I observed that, as a matter of fact, German, and not English, furniture was in use." And so, while the columns of every newspaper are full of the unity of the Empire, and of the unemployed, another tie between mother-country and colony is broken, another outlet for British industry is closed, because our manufacturers do not know what the Austrian discovers for himself.

It is all of a piece with this that in England, in the year of grace 1889, an electrical engineer, who is, as we gather from the Chairman's statement, no tyro or underling, but who was welcomed at an important meeting as a worthy exponent of the views of a well-known firm, was not ashamed to tell his hearers that he does not "want to know what electricity is," and that he could "measure all electrical quantities in foot-pounds."

THE CEPHALOPODA.

Catalogue of the Fossil Cephalopoda in the British Museum (Natural History), Cromwell Road, S.W. Part I., containing part of the Sub-order Nautiloidea, consisting of the Families Orthoceratitidæ, Endoceratidæ, Actinoceratidæ, Gomphoceratidæ, Ascoceratidæ, Poterioceratidæ, Cyrtoceratidæ, and Supplement. By Arthur H. Foord, F.G.S. Pp. xxxii. and 344, and Fifty-one Woodcuts. (London: Printed by Order of the Trustees, 1888.)

JUST as heraldry in the Middle Ages formed a necessary part of the education of every knight and noble, without which it would have been impossible to trace the connection of the great families whose genealogy was symbolized on banner, shield, and crest, so palæontology is essential to the biologist, if he would successfully trace the connection of the living forms around him with their remoter progenitors whose records must be sought for in rocks of Palæozoic age.

Of such high lineage are the Cephalopoda, whose ancient life-history Mr. A. H. Foord has essayed to write in the carefully-prepared volume before us. There is evidently a fascination about the nautilus and cuttle-fish family, which seems specially to attract the attention of naturalists. The living animals of the cuttles and squids are remarkably vivacious, as well as cosmopolitan, whilst the Octopus, or "devil-fish," has been invested with quite supernatural powers and intelligence. As to the gigantic calamaries of the North Atlantic, they almost realize in size De Montfort's fancy sketch of the "colossal poulpe" seizing a three-masted ship in its arms; or the fabled "Kraken," described by Dr. Paullinus and the Bishop of Bergen in the last century as a beast so huge that a regiment of soldiers could conveniently manœuvre on its back!

Striking and varied as are the animals of living Cephalopods, their shells, both recent and fossil, are of immense interest, indeed they are unsurpassed for elegance and variety of form by any of the Molluscan sub-kingdom; and as we have seldom the other parts left to us—especially in the older rocks—save the shell alone, it becomes necessary to study these structures with increased attention, and strive to elicit from them all that is possible of the past life-history of their inhabitants, and thus, by the light which they afford us, to trace the origin of the allied living forms.

The labours of naturalists during the past fifty years have tended to eliminate certain groups formerly classed with the Mollusca, and thus to define more clearly the characters of this great phylum of the Cœlomata as now recognized. The first group to be removed from the Mollusca was that of the Cirripedia by the labours of J. V. Thompson in 1830. In 1866, Kowalewsky showed that the Tunicata had affinities with the Vertebrata, and that their agreement with the Mollusca was only superficial. In 1844, H. Milne-Edwards had placed the Polyzoa with the Brachiopoda and Tunicata in a large group, the "Molluscoidea"; but the investigations of Cardwell, in 1882, showed that the Polyzoa and Brachiopoda had only a delusive agreement with the Mollusca, and must be removed from that phylum also.

There now remain, according to Prof. Lankester, only two great branches of the Molluscan phylum; namely, (1) the GLOSSOPHORA (characterized by possessing an odontophore), embracing the Gasteropoda, the Scaphopoda, and the Cephalopoda; and (2) the LIPOCEPHALA (= *Acephala*, Cuvier), including all the Lamellibranchiata (mussels, oysters, cockles, clams, &c.), without any definite head.

The question that interests us most to-day is, To which of the Mollusca belongs the honour of representing the primitive type from which all the varied forms we now recognize have arisen? Leaving out of consideration the earlier Brachiopoda, as having been already excluded from the Mollusca, we find in the older Palæozoic rocks that the Pteropoda, Heteropoda, Nautiloidea, and Lamellibranchiata (or Lipocephala) appear almost contemporaneously. But the Pteropoda (represented by *Theca* and *Conularia*), and the Cephalopoda (by *Orthoceras sericeum*, and at least three other species), begin in the Tremadoc rocks; Lamellibranchs (such as *Palæarca* and *Ctenodonta*) in the Arenig; whilst Gasteropods of several well-marked genera (*Murchisonia*, *Pleurotomaria*, *Euomphalus*, *Trochus*, &c.), with *Bellerophon* and the strange *Maclurea*, are found in the Bala series. If, as appears from the views of Prof. Lankester, we are to regard the *Lipocephala* as degenerated forms of *Glossophora*, they must have begun very much earlier indeed to have become so differentiated as we find them in the Arenig group. Nor do the Gasteropoda of the Bala series present the appearance of primitive forms (unless it be the genus *Maclurea*), for we find mollusks with turreted, turbinate, and discoidal shells, already defined as distinct generic types.

That the Pteropoda preceded the higher Cephalopoda in time seems pretty certain, and that both of these preceded the Gasteropoda seems established; but of the priority of the latter over the Lamellibranchiata there is no evidence.

As Prof. Lankester, in his recent classification of the Mollusca, places the Pteropoda with the Cephalopoda as Branch A. *Pteropoda*, Branch B. *Siphonopoda*, we must be content, for the present, to consider that the Cephalopoda represent the most ancient type of Mollusca, and that the shells of the little Pteropod, *Theca*, are the earliest representatives which we at present know.

In *Orthoceras* we become acquainted with the first and simplest form of camerated Cephalopod shell. They were straight shells, with plain suture-lines marking the septa, the siphuncle varied in position, the septa being concave towards the aperture; the initial chamber was conical, with a cicatrix, the body-chamber large, and its aperture simple (pp. 1–128). Nearly 200 species of these straight simple shells are described by Mr. Foord, ranging from the Tremadoc shales of Portmadoc to the Trias of St. Cassian. That they were external shells is proved by their surface-ornamentation, consisting of transverse and longitudinal ridges, and fine decussating striæ, with occasional colour-bands and markings rarely preserved.

In *Endoceras* (pp. 129–63) the internal structure of the shell is varied by the undulating character of the septa, which bend downwards, and overlap the neck of the preceding septum, forming a complete shelly siphuncular tube. The siphuncle, moreover, is eccentric in position, and often half the diameter of the shell. Within this wide siphuncle a series of funnel-shaped conical sheaths (endosiphons) have been observed, of the nature of which we are at present left in doubt. *Piloceras* has also an unusually large siphuncle, within which a series of invaginated sheaths, similar to *Endoceras*, occur.

The genus *Actinoceras* presents other peculiarities in the structure of the shell. Within the siphuncle, which is very large, a slender tube passes down the centre, called the endosiphon (pp. 164–99). The siphuncle expands between each septum into a broad bead-like dilatation, perforated around its periphery by a series of minute shelly radiating tubuli given off from the endosiphon. It has been suggested by Owen that these were connected with the vascular system of the animal, and were designed to convey nutrition to the lining membrane of the septal chambers. No such complex siphon and endosiphon exists in the shell of any living Cephalopod; nevertheless, it is difficult to accept for them the interpretation here suggested, unless we would invest the shells of these ancient Mollusca with a structure akin to that of the skeletons of the Vertebrata!

The huge siphuncles of *Huronia*—allied to *Actinoceras*—have been described by Stokes, Bigsby, Hall, and

Woodward (pp. 199-207). The septa and shell-wall are thin, and but rarely preserved. These weathered tubes were frequently noticed by Dr. Bigsby standing out in relief from the limestone cliffs of Drummond Island on Lake Huron, and were as large as the vertebra of a man, and not unlike them in shape, and over 6 feet in length.

In the family of the *Gomphoceratidæ* (pp. 211-45), we meet with shells ranging from nearly straight, through varying degrees of curvature, to one in which a complete whorl is attained. The aperture of the shell in this family is so contracted that it is obvious the animal could not have withdrawn its head into its body-chamber as does the living *Nautilus*. The mouth of the shell is T-shaped, and reminds one of the mouth of the shell in some land-snails, like *Helix globulosa* and *Auricula scarabæus*, which are so guarded by tooth-like projections from the margin of the aperture as seemingly to preclude the animal from ever retreating into its shell, or emerging from it if withdrawn.

The *Ascoceratidæ* (p. 246) have the test of a sac-like form, the body-chamber extending to the lower end of the dilated portion of the shell, while the septa adhere to the dorsal wall, and bend upwards with their convex side towards the mouth of the shell. The apex was unknown, the shell being always found truncated, but Barrande, first, in Bohemia, and subsequently Lindström in Sweden, have discovered the earlier apical portion of the shell of *Ascoceras*, which was nearly straight, and with the septa normal, as in *Orthoceras* (p. 335).

The *Cyrtoceratidæ* (pp. 262-318) are more regular in their growth than the preceding; the shell is more or less curved, and tapers rapidly, or more slowly, according to the species or variety examined. The siphuncle is small, and varies in position in the different species, being external, internal, or sub-central; they range from the Carboniferous to the Tremadoc series, and are well represented in the Devonian of Gerolstein, Eifel, by large and handsome forms.

In his introduction the author discusses many points of great interest relating to the class, as, for example, the classification, the structure of the shell, the range in time, and the distribution of the group.

Seventeen genera and 403 species are described, but there yet remain the *Lituitidæ*, *Trochoceratidæ*, *Nautilidæ*, and *Bactritidæ*, to complete the NAUTILOIDEA; while the AMMONOIDEA and the DIBRANCHIATA will be treated of still later on.

Mr. Foord writes:—

"The classification of the Nautiloidea adopted in this volume will be found to differ in some of its details from systems hitherto employed, the more recent writings of Noetling, Zittel, Mojsisovics, and Hyatt having furnished the basis of the changes introduced. The arrangement of the groups described in the following pages is primarily zoological, secondarily stratigraphical, each genus being dealt with separately, from its appearance to its extinction."

The author passes in review the various systems of classification of the Cephalopoda proposed by Prof. Hyatt, Dr. Paul Fischer, Barrande, and other writers on this group.

"Hyatt considers that the generic terms *Cyrtoceras*, *Gyroceras*, *Lituites*, *Nautilus*, are merely 'descriptive terms for the different stages in the development of an individual, and also the different stages in the development or evolution of the adult forms in time. In other words, each of these genera, as now used, includes representatives of all the different genetic series of Tetrabranchs, which are either young shells in the corresponding stage of growth, or adult shells in the corresponding stage of evolution.' He finds 'that genetic affinities on a large scale are best exhibited by the siphuncle, particularly by the funnels of the septa, which are more invariable than any other part of the shell.'

"He next discusses the embryonic relations of the structure of the septa and of the siphuncle, and mentions the difference between the Nautiloids and the Ammonites exemplified in these structures, the one commencing with a globular initial chamber ('protoconch' of Owen), the other with a conical initial chamber and a cicatrix. He remarks that generally among the Palæozoic Cephalopod types much greater differences exist, in regard to the septa, the position of the siphuncle, and so on, than among the Mesozoic forms, thus indicating that the evolution of forms was quicker in the Palæozoic epoch than at subsequent periods, and from these circumstances he concludes that 'types are evolved more quickly, and exhibit greater structural differences between genetic groups of the same stock, while near the point of origin, than they do subsequently.' It must not be forgotten, however, that the Palæozoic epoch was of much longer duration than the Neozoic. 'In the smaller divisions (families and genera) of Hyatt's scheme of classification, an important place is assigned to the characters of the sutures for distinguishing the different groups. In some groups, however (notably the Orthoceratidæ), the less stable characters presented by the ornamentation of the shell are for a like purpose employed.'

"While there can be no question as to the value of Prof. Hyatt's work, and the thoroughness of research which he has brought to bear upon the class he has with so much boldness and originality attempted to re-classify, the extremely revolutionary nature of the changes he has proposed in the minor divisions of his system (involving the wide separation of many forms hitherto associated together) challenges the inquiry as to whether our knowledge of the developmental history of the Cephalopoda is not as yet far too imperfect to justify such a radical departure from existing systems. The suppression of the familiar names *Cyrtoceras* and *Gyroceras* seems quite unnecessary, and seeing that the names *Orthoceras* and *Nautilus* are retained, in a restricted sense, in Hyatt's scheme, there seems to be no good reason why the two former should not have been similarly used" (Introduction, p. vii.).

Mr. Foord discusses at some length the nature of the camerated structure of the Cephalopod shell, and the question as to whether the *camera* should be called "air-chambers" or "water-chambers"; he concludes to avoid the difficulty by calling them "septal chambers." Bearing in mind the fact that each sealed-up chamber of the shell is but the partitioned-off lower portion of the animal's body-chamber, it is obvious that it must, at the moment of separation, contain the same medium as that which envelopes the animal.

In the case of the living *Nautilus*, dredged by the *Challenger* off Matuku Island, in 320 fathoms,[1] it seems improbable that the "septal chambers" could have been full of gas when the animal was crawling upon the sea-bed at a depth at which the pressure would be equal to about 750 pounds on each square inch of surface, or fifty-three times greater than at the sea-level. Any such

[1] See "Notes of a Naturalist on the *Challenger*," by H. N. Moseley, M.A., F.R.S. (p. 297).

inclosed gas-filled chambers would have sufficed, by their buoyancy, to bring the shell and animal at once to the surface.

That the living Nautilus, noticed by Prof. Moseley, should have been unable to sink in the tub of sea-water in which it was placed on the deck of the *Challenger*, and that this inability was due, as he observes, "to some expansion of gas in the interior, occasioned by the diminished pressure," is equally certain; but Moseley does not state that the expanded gases were in the shell-chambers; had such been the case, the gas, in order to expand, must have ruptured the rigid shell-wall. But gases are, no doubt, evolved within the crop and alimentary canal of the animal, and these, by their expansion, on coming to the surface, would suffice to produce the effect observed by Moseley. "The living specimen," he says, "seemed crippled, and unable to dive, no doubt because it had been brought up so suddenly to the surface from the depths" (*op. cit.*, p. 298). Exactly similar effects were observed in fishes with "swim-bladders." These "come up" (says Moseley), "in the deep-sea dredge, in a horribly distorted condition, with their eyes forced out of their heads, their body tense and expanded, and often all their scales forced off" (*op. cit.*, p. 580).

Mr. Foord quotes an observation by Dr. Woodward, "that many dozens of specimens of newly-imported shells of *Nautilus*, examined by him at the Docks, were, when shaken, all found to contain fluid within their chambers, just as in the camerated shell of the Water Spondylus (*S. varians*)" (Introduction, p. xiii.). We cordially indorse Mr. Foord's remark that "it is much to be regretted that recent opportunities of setting this question (of the contents of the septal chambers) at rest should apparently have been neglected."

What we would strongly insist upon is, that, it being admitted on all hands that the Cephalopoda are, in every respect, Glossophorous Mollusks, their shells must, in a similar manner, be found to conform to the ordinary Molluscan type. The striking regularity of their septal chambers has usually hindered a comparison with those of other camerated Molluscan shells; but in *Caprinella*, the camerated interior of the "water-chambers" is quite equal in regularity and symmetry with that of the Cephalopoda, and many of the *Hippuritidæ* show not only septa, but a pseudo-siphuncle, reminding one still more of the chambered Nautilus.

Space does not permit a longer notice of Mr. Foord's excellent "Catalogue"; it is a most valuable addition to the now really fine series of descriptive Catalogues issued by the Trustees of the British Museum of Natural History. We hope soon to welcome the appearance of the second part of this useful work, when we may be tempted to reopen the question of shell-growths and shell-structures.

SANITARY SCIENCE.

Transactions of the Sanitary Institute of Great Britain. Vol. IX. (1887–88.)

THIS volume is largely composed of the papers and addresses read at the Congress of the Institute at Bolton. The authors are chiefly men well known in their different professions, whose contributions are not only of much scientific interest, but carry weight with the public. Sanitation is a science in which the "faddist" delights to dabble, but his effusions have but little representation in the volume before us. The majority of the workers appear to be content to record steady advances in knowledge, or to make practical suggestions for administrative reform, rather than to air brilliant theories, popular with the public for their novelty, but greatly wanting in substantial proof. To force premature conclusions in sanitation, as in other older sciences, is to retard true progress; and on the whole it may be said that sanitary reformers, however earnest, are content to preach the doctrine of pure air, earth, and water, which is as old as Hippocrates.

In the department of practice, improved sanitary administration is urged on all hands. Such reforms take largely the shape of what has been called "interference with the liberty of the subject"; an interference, however, which is not unnecessary or uncalled for, but is imperative in health matters where the act or default of one individual may imperil the lives of many. Those who on this plea found their objections to such measures as compulsory vaccination, compulsory notification of infectious disease, isolation of infectious disease in hospitals, better supervision of building operations, dairies, and cow-sheds, and stricter enforcement of nuisances clauses and sanitary regulations generally, appear to forget that civilized existence depends upon the observance of mutual obligations, and that society could not exist if every individual were free to exercise his liberty of action at the expense of the community. Little is thought of the restrictions that already exist, and to which all law-abiding citizens cheerfully adhere, but when, with advancing knowledge of cause and effect in disease, certain measures are pointed to as being necessary to avoid unwholesome conditions, or to prevent the dissemination of epidemics, an outcry is raised which is too often not only illogical but insincere.

In the section of chemistry, meteorology, and geology some valuable papers are contributed on the application of bacteriology as a means of obtaining evidence as to the purity of water-supplies, and of ascertaining the degree of contamination of the air of buildings and sewers. As a science, bacteriology is still in its infancy, but already its teachings are producing a most profound effect in the domains of medicine and hygiene. The recognition of the bacterial and fungoid organisms as the principal factors in the processes of fermentation and putrefaction of organic substances, and the discovery that certain specific microbes are the actual agents provocative of certain contagious diseases, have secured a basis on which can be founded rational measures for the prevention and alleviation of disease and for the control of insanitary conditions. Hygiene was a science practised with most beneficial results before the discoveries of Pasteur, Koch, and Tyndall, but many of its teachings and precepts were at that time empirical, although founded more or less on experience and observation. With the more definite knowledge of disease causation and dissemination now arrived at, it is most satisfactory to find that the measures of sanitary reform and improvement which have marked the latter half of the present century are almost entirely in accord.

At the close of the Congress addresses were given to the working classes of the town of Bolton. The Sanitary Institute appears to fully recognize the desirability of bringing home to the minds of all classes of the community the importance of healthy homes and temperate living; and the lesson it should aim at inculcating is that if the public sanitary authorities can do much for the working man, he can do for himself by his own efforts—if he only knew how—very much more to improve his own surroundings, and bring up his family in health and comfort.

The annual visits of the Institute, for the purpose of holding Congresses, to the large towns of this country, must rank as one of the most influential means of popularizing sanitary science. The proceedings of the Congress are watched with interest by persons whose attention it is not easy otherwise to engage; whilst the exhibition of sanitary appliances brings under their notice the latest improvements in domestic and municipal sanitation, and is an incentive to manufacturers to turn out none but the most approved articles.

It also appears that the Institute holds examinations and grants certificates to local surveyors and inspectors of nuisances to sanitary authorities. These examinations should prove of the greatest use to the public sanitary service, for the certificate is a guarantee that the holder possesses sound knowledge on sanitary subjects; and there can in future be no reason why Local Boards should appoint incompetent and ignorant officials, when certificated candidates for office present themselves.

GLEANINGS IN SCIENCE.

Gleanings in Science. By Gerald Molloy, D.D., D.Sc. (London: Macmillan and Co., 1888.)

THIS is a pleasantly-written book, containing ten popular lectures which have, from time to time, been delivered by the author to popular audiences. They were, with one exception, delivered under the auspices of the Royal Dublin Society. The lectures are not offered as containing anything new, but simply as an attempt to popularize some of the most important of modern developments in physical science.

The author has, in our opinion, been thoroughly successful. In a short space he has been able to put before his hearers and his readers, in a satisfactory and thoroughly intelligible manner, a great variety of most important scientific facts and principles. The subjects of the lectures are well chosen.

First, we have two lectures on "Latent Heat," in which the fundamental discoveries of Black are explained and the great results which have flowed from them are recounted. Three lectures on electrical subjects follow. Then we have two lectures on "The Sun as a Storehouse of Energy." Two lectures on the Electric Light enable the author to lay before his readers in a popular but scientific manner the principles of dynamo-machines and the modes of producing the light from the electric current. The transformations of energy which take place during production of the light are also explained. The last lecture is on the Glaciers of the Alps.

The illustrations, which take the place of the experiments of the original lectures, are for the most part suggestive and satisfactory. The lecture on glaciers contains three or four striking, if somewhat sensational, woodcuts by Mr. Whymper, whose Alpine sketches are well known.

A book of the kind we have described is scarcely likely to be absolutely free from blemishes, particularly of style; and there are two or three to which we would call the attention of the author. One of them, at any rate, can be easily amended in another edition. It is the habit of saying "indefinitely small" when he means either infinitely small or extremely small. This use of the word indefinite he shares with some, sometimes indefinite, writers on certain branches of mathematics and dynamics. But indefinite does not in pure nor in common language mean infinite; much less can "indefinitely small" be put for "extremely small," as in speaking of the length of the path of a molecule. There is nothing indefinite about the path of a molecule any more than about the path of a billiard ball. Again, there is nothing "inconceivable" about the velocity of the radiant energy of the sun (p. 191). It is measurable and well known. The pruning down of some exaggerated language would make the wonders of science all the more wonderful.

A lover of Nature is apt to get into difficulties if he invokes her aid too frequently in descriptions of the physics of our universe. When we read of Nature dealing out the sun's energy *to man* with the prodigality of a spendthrift, we are apt to think of the much more prodigal way in which the sun's energy is poured out all round. The energy which comes in our direction is a very small proportion of the whole. There are too many references to personified Nature in these lectures.

The parts which seem to us least satisfactory are the early part of the lecture on storing of electrical energy, and the classification of the forms of energy contained in the next lecture. The enumeration of various forms of energy available to man is very imperfect. For example, energy of chemical separation is omitted from the list, and yet dynamite, gunpowder, and the gas-engine, are surely worthy of mention. Dynamite is manufactured as electrical energy is; but an engine worked, say, by native petroleum, would be far more efficient than the *earth currents* which Nature provides, but which, as our author properly remarks, are more of a trouble than a pleasure. In another place "bags of oxygen and hydrogen" are referred to as examples of stored-up heat. This seems a little far-fetched, to say the least of it. Just about the same place a rod of "chalk" is used to produce the lime-light.

Perhaps, however, it may seem somewhat ungracious to prolong the list of minor corrections. The book is most readable, and is deserving of praise throughout.

OUR BOOK SHELF.

The Gamekeeper's Manual; being an Epitome of the Game Laws of England and Scotland, and of the Gun Licences and Wild Birds Acts. By Alexander Porter, Chief Constable of Roxburghshire. Second Edition. Pp. 120. (Edinburgh: Douglas, 1889.)

ASSUMING, and we are not in a position to affirm or deny the assumption, that the legal points laid down in this little book are sound, it will certainly fulfil its author's intention, which, he tells us, is "to place within the reach of all connected with the protection of game a knowledge of the law which it is their duty to administer," and we may hope, also in his words, that it may contribute "to a more effective suppression of a form of lawlessness which leads to so many crimes of a more serious nature." Seeing that it is a second edition, that the author is the head of the constabulary of an important Border county, and that in compiling it he has had the assistance of other chief constables, Mr. Porter is probably right on all points that have been decided. Within his prescribed limits he traces his subject, as it seems to us, very well, though tersely enough, beginning with the earliest records of both Kingdoms, and ending with the silly (but well-meant) Sand Grouse Act which closed the labours of the last session of Parliament—an admirable instance of locking the stable-door after the steed was stolen. However, his treatment is so purely from an executive aspect that comments upon the book are hardly suited to these pages. Yet we may remark that the game law question, which, not so many years ago, was a party cry, has through certain modifications of opinion ceased to occupy that position. There are now few reasonable men who do not perceive that if our ever-increasing population is to continue the enjoyment of the delicacies which "game" (using the word in a wide and not a legal sense) affords, some sort of preservation of such "game" is a necessity. The practical extermination, in many large districts, of *Lepus timidus* since the passing of the Act of 1880 (43 and 44 Vict. cap. 47), leading to the almost prohibitive price of hares in our markets, has fully shown this. The way in which the principle of preservation should be applied is, of course, quite another thing. All men nowadays agree in condemning the savagery of the early laws, though many are apt to forget that it was only of a piece with the savagery of other contemporary laws; but until within the last twenty years few enactments, whether in Great Britain or Ireland, grasped the scientific truth which has been at the bottom of the most recent legislation (the Ground Game Act excepted), and should be at the bottom of all—namely, take care of the parents, and you may leave the offspring to take care of themselves.

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.*]

The Meteoric Theory of Nebulæ, &c.

MY last letter in NATURE of March 7 (p. 436) substantially contended that there must be a certain limit to the rate of translatory motion possible to meteoric masses, beyond which they would be rapidly resolved into vapour and consequently be unable to maintain the weight of the heaped-up material constituting the nebula (supposed to be formed of meteorites). It appears tolerably evident that there must be such a limit as to size to which the system can apply, if a period anything like "a few thousand years" mentioned[1] by Prof. G. H. Darwin is to be accorded to this stage of evolution. The doubt was expressed whether the size (or mass) of the original solar nebula was not past that limit, in view of the rate of translatory motion required, viz. 5½ kilometres per second in the mean.

That the theory is in principle true, appears but little open to doubt. For when masses fall in a confused manner to a centre, the kinetic movement is naturally and inevitably produced, and corrected so as to be maintained symmetrically in all directions by the masses themselves. The main question seems to be, What is the limiting mass of the future sun to which such a system can apply in its completeness, so as to allow anything like permanence enough for the forcible tendency of the energy to equalize itself to extend to a notable radial distance (to settle down into a sort of temporary kinetic equilibrium, that is)? It is conceivable that in the case of an eventual very large sun, the system of meteorites may be heaped up to a certain height, and then collapse at or near the nucleus, where the density is greatest, and consequently the impacts most numerous. Such a partial collapse would give rise to violent oscillation in the nebula, and perhaps assist the throwing off of rings (query). The fact that meteorites are exceptionally the only celestial masses that we can handle and analyze in our laboratories, gives a basis of certainty to inquiries about them, which lends a special interest to theories as to the part played by them in Nature.

[1] Abstract of Mr. G. H. Darwin's paper "On the Mechanical Conditions of a Swarm of Meteorites," appeared in NATURE, November 22 and 29, 1888 (pp. 81 and 105). The complete paper is in the Philosophical Transactions, vol. clxxx., 1889.

In my last letter I avoided the use of the term "elasticity," considering purely the physical conditions which constitute its basis. Prof. G. H. Darwin remarks on this point:—"It may, however, I think, be shown that the very greatness of the velocities will impart what virtually amounts to an elasticity of a high order of perfection" (NATURE, p. 82). "Ordinary elasticity must be nearly inoperative" (p. 107). To produce this exceptional degree of elasticity, the expansive action of the gas generated by the high velocity of impact is relied on, which is regarded to act as "a violent[1] explosive introduced between the two stones." No doubt the volatilized gases due to the collision expand and assist rebound; but in estimating its value we must consider if the chilling of the gas, in doing the work, is at all the equivalent of the heating. If the gas is left in an incandescent state (as is probable, from spectroscopic evidence also), that represents so much spent work, so much[2] imperfect elasticity. The gas is not confined as in a gun, and so cannot exert its expansive effect to full advantage.

Meteorites constituting a nebula having a mean velocity of 5½ kilometres per second represent an energy about eighty times that of a cannonade. Taking the velocity of an ordinary projectile at 2000 feet per second, that of the meteorites (5½ kilometres) is 18,000 feet per second, about. Meteorites normally composed of materials imperfectly welded together, cannot stand the same knocking about as steel projectiles. If it were imagined that the latter could have a velocity of 18,000 feet per second imparted to them, they would doubtless leave a luminous track in the atmosphere from friction, and the energy of their mutual collision at this speed may be imagined. It must be remembered, however, that the collisions are generally oblique or glancing.

No doubt in a nebula a metallic rain flying in all directions like the meteorites would accompany the motion. This rain of metal we may suppose under favourable accidents to collide together to form nuclei in various parts of the nebula; and an incipient nucleus constituting a sort of shelter would collect more rain (or hot metal), and so constitute a new meteorite—much as the occasionally dissociated lumps or molecular clusters of a compound gas reunite in another part of the gas, so that the mean state of aggregation remains the same.

The mean interval between encounters depends, of course, on the mean size of the meteorites. It appears from the data afforded that thirty days or a month would be the average interval between collisions (at about one-third radius of the supposed original solar nebula), for meteorites of mean mass equal to 3¼ kilogrammes, which would possess a volume of about half a cubic decimetre.[3] So that a nucleus, which we may suppose at rest or only slowly moving—as it is, on the average, struck equally on all sides by the metallic rain—would have, as a mean, a month to collect or grow before the chance of being disturbed by a collision in a nebula under the conditions named. In this way, doubtless, nuclei could be formed, and the meteorites renewed.

The question of the applicability of the theory would seem to

[1] The entire passage is: "It must necessarily be obscure as to how a small mass of solid matter *can* take up a very large amount of energy in a small fraction of a second, but spectroscopic evidence seems to show that it does so; and if so, we have virtually what is a violent explosive introduced between the two stones" (NATURE, November 22, 1888, p. 82).

[2] It will be seen afterwards that this radiation must not be considered as absolutely lost, but in great part radiated to another region of the nebula of meteorites.

[3] For meteorites of 3¼ tonnes (about half a cubic metre in volume), the interval between collisions would be 300 days, nearly a year; and the mean length of path 130,000,000 kilometres, nearly the sun's distance. Multiplying the mass by a thousand (or the diameter by 10) increases the interval between collisions and the length of path ten times. At this rate we should get to the stellar distances and stellar movements, regarding the visible universe as a nebula, if a merely speculative remark may be allowed.

be one of degree rather than of principle. For, even if its application for the full time required for that stage of evolution, in the case of so large a nebula as the original solar one, were considered doubtful, there is no objection apparently on that head to be adduced in the case of smaller nebulæ, or comets (the smallest class of nebulæ); in which latter case there is some support to the theory afforded by experiment and observation.

In regard to the question of "elasticity," the only resource, in my view, is to abandon this idea, in the ordinary sense of that term (which conveys the idea of retention of form), and suppose that there may be complete disintegration by the collisions at times,[1] welding or fusion together at others; so that the mean degree of aggregation remains constant so long as the translatory motion remains constant. This idea of the existence of a number of possible mean states of aggregation of matter between the extremes of complete integration into one mass, and complete disintegration into molecules (the states being dependent on the rate of translatory motion), was thrown out by me in NATURE (vol. xix. p. 461);[2] also, further, in the *Philosophical Magazine*, August 1879, p. 153.

According to this view, it is implied that "cohesion," as a central force, can play the same part as "chemical action" under translatory motion, and produce fluctuations about a mean state of aggregation—just as in a compound gas, for example, even at normal temperature, the small lumps of matter which move as wholes in the motion of translation are disintegrated now and then, and integrated elsewhere, the *mean* state (only) of the gas remaining unchanged.

The above, with the exception of the last two paragraphs, was written before the appearance of Prof. G. H. Darwin's reply (March 14, p. 460), to some criticism I ventured to offer (March 7, p. 436) on certain points of his theory in my first letter. I would make a few remarks in addition here.

I am not quite able to agree with the view of M. E. Minary, expressed in a paper brought before the French Academy on February 18, of which an abstract appeared in NATURE of February 28, p. 432, and is referred to in Prof. Darwin's letter. The chief passage, as given in the abstract, is as follows:—

"The gases being perfectly elastic bodies, and in the upper atmospheric regions in an extremely rarefied state, heat cannot be produced by the shock of bodies endowed with great velocity and impinging on perfectly elastic molecules capable of receiving the motion and acquiring the velocity of these bodies: in this case the movement is communicated, not dissipated or transformed into heat."

This view is opposed, I venture to think, to the deduction we may draw from such an experiment as that with the "fire syringe," where the air in a cylinder is inflamed by suddenly compressing a piston. What is the flame here observed through the tube of glass due to? It is due, of course, to the *vibrations* of the molecules of air, which break the ether up into waves, and so affect the eye. From the fact that the air molecules are "perfectly elastic," it becomes impossible for a moving body to impinge against them violently without throwing the molecules into energetic vibration, which is the physical basis of "radiant heat." The same must occur on a greater scale, as it appears, when the air is compressed by a flying meteorite, although I accept M. Cornu's suggestion (quoted in the abstract, p. 432) that the *luminosity* observed may partly be of electric origin.

In Joule's "Scientific Papers," vol. ii., experiments in association with Sir William Thomson are described, of whirling thermometers through the air (attached to a lathe). The experiments, which were numerous, gave 163·7 as the velocity in feet per second; on the average, equivalent to a rise of temperature of 1° C. (vol. ii. p. 316). Whirling a thermo-electric junction attached to a reflecting galvanometer was tried with consistently the same result as the thermometer (p. 310). These experiments were made partly[3] with the view to test the theory of the heating of meteorites. The temperature was found to be as the square of the velocity. The law of Clausius, that the translatory motion and the vibratory motion (which latter motion alone affects the eye and senses as radiant heat) of the molecules of an ordinary gas are proportional to each other, has—as Prof. Darwin allows—been experimentally verified through[1] a considerable range of temperature. To my mind it appears obvious that these two forms of motion (translatory and vibratory) must be interconvertible and mutually sustain each other. When the gas, for example, is exposed to the pulsations of ether waves (radiant heat), this vibratory motion is first taken up by the molecules, but part of it is converted into translatory motion, as proved by the rise of pressure. If, on the other hand, the translatory motion of the molecules of gas be augmented, part of this is instantly, as we know, converted into vibratory motion, the source of radiant heat. S. TOLVER PRESTON.

Paris, March.

[1] A special property of iron, which may have importance here.

[2] It would seem even curious to my mind, if there were no intermediate state between these two extremes of complete integration and complete disintegration, by a varying rate of translatory motion (or energy). It may be observed that the radiant energy set free so abundantly at a collision is not lost, but radiated in great part to another region in the nebula (and there absorbed).

[3] Also see vol. i. pp. 399, 536. The temperature was found to be independent of the form and size of the thermo-electric function, and was assumed as evidently independent of the density of the air. No doubt the more air there is the more there is to heat. But it nevertheless seems plain, I think, that the temperature of a meteorite must rise higher in dense air than in excessively rarefied air. How is this to be explained? Each individual molecule of air in striking the moving meteorite, is thrown into violent vibration, and this (temperature) is independent of the number of air molecules evidently. But in dense air, no doubt, the heat accumulates much faster than it can be radiated away, and so the temperature of the meteorite attains a final *maximum*, which is greater the denser the air is. The temperature may probably be appreciably constant within certain limits of variation of density; but it appears obvious that in excessively rare air the temperature developed must be less, owing to rapid dissipation in space by radiation.

[1] The ratio of Maxwell referred to in my last letter, and which Prof. Darwin remarks in a footnote (p. 460) was stated inaccurately by me—was not, I may explain, the one quoted by him from Maxwell's "Theory of Heat." It is contained in Maxwell's paper "On the Dynamical Evidence of the Molecular Constitution of Bodies" (NATURE, vol. xi. pp. 357 and 374). After alluding to the law of Clausius, Maxwell remarks:—"In 1860 I investigated the ratio of the two parts of the energy on the hypothesis that the molecules are elastic bodies of invariable form. I found, to my great surprise, that, whatever the shape of the molecules, provided they are not perfectly smooth and spherical, the ratio of the two parts of the energy must be always the same, the two parts being in fact equal" (p. 375).

The Molecular Formulæ of Aluminium Compounds.

IN a letter to NATURE, December 27, 1888 (p. 198), I gave a tabulated statement of the numerous vapour-density determinations of halogen, and a few other compounds of aluminium and the allied metals, and pointed out what appear to me to be the legitimate conclusions to be derived from the experiments, regarding the molecular formulæ of these compounds.

Since then two interesting articles have appeared in NATURE (pp. 447 and 495), in which accounts are given of determinations of the vapour-densities of aluminium acetyl acetonate, $Al(C_5H_7O_2)_3$, and aluminium methide, $[Al(CH_3)_3]_n$. The simple formula given for the first compound has been proved to be correct, at any rate for the conditions under which the experiments were made. The results given for aluminium methide are: calculated for $Al_2(CH_3)_6$, 4·98; calculated for $Al(CH_3)_3$, 2·49; observed (10° above the boiling-point under atmospheric pressure), 3·92.

What I ask permission to call attention to and to criticize is the conclusion drawn by the author of these articles from the experimental results. Speaking generally, the conclusion may be stated in this way: Molecules of the formula MR_3 do exist, *therefore* molecules with the double formula M_2R_6 do not. I confess that I am wholly unable to appreciate the force of the argument. Must we take the existence of the molecule NO_2 as a proof of the non-existence of N_2O_4?

That I have not stated the argument unfairly may be shown by quotations from the article (p. 495). Speaking of aluminium acetyl acetonate the author says: "It is supremely satisfactory that in this case the density, at a temperature only 45° above the boiling-point, was found to actually correspond precisely with that required by the triad formula, *precluding again the possibility of the existence of molecules of the type* Al_2R_6." And previously, after giving the results for aluminium methide, notwithstanding the fact that the observed density 3·92, obtained by Quincke, corresponds rather more closely to the higher than the lower formula, the author remarks: "*Hence it can no longer be doubted that molecules of the double formula are incapable of existence.*" The italics in both cases are mine. The conclusion I should draw from all the experiments with aluminium compounds is this: The experiments of Deville and Troost, Friedel and Crafts, and Louise and Roux, prove conclusively that molecules of the higher formula Al_2R_6 are capable of existence; the results obtained by Nilson and Pettersson, and by Buckton and Odling, point also to the existence of molecules of the lower formula, but further proof was certainly needed, and this has now been afforded by the valuable experiments of M. Alphonse Combes with aluminium acetyl acetonate.

In conclusion, it appears to me that the close relationship of the metals aluminium, gallium, indium, iron, and chromium, makes it probable that molecules of both formulæ M_2R_6 and MR_3 may be capable of existence in the case of all these metals, even though the conditions necessary for the stability of the higher molecules have not yet been attained for indium and chromium chlorides. That the conditions have been attained in the case of the chlorides of aluminium, gallium, and iron, appears to me to admit of no doubt. SYDNEY YOUNG.

University College, Bristol, March 23.

Luminous Night-Clouds.

IN accordance with my anticipations (see *Astr. Nachr.*, 2885), luminous night-clouds have now been seen at the southern point of South America. Herr Stubenrauch, meteorological observer at Punta-Arenas, writes to me that he twice saw the phenomenon in December 1888. According to the same observer, it was noticed several years ago by a naval officer in the Beagle Channel, rather to the south of Punta-Arenas. The description given by Herr Stubenrauch leaves no room for doubt that the phenomenon is identical with that observed in Europe.

Sternwarte, Berlin, March 28. O. JESSE.

Zodiacal Light Observations.

COULD any of your readers furnish me with a list of observations of zodiacal light, or refer me to any record where such a list can be found? I applied to the Meteorological Office some years ago, and found that no such list was to be obtained. All I require is the precise time of the display, the place of observation, and any other mathematical observations concerning the angle of inclination, &c., which may be relied on.

W. DONISTHORPE.

32 Pembridge Villas, W., March 30.

Vapour, or Meteoritic Particle.

ABOUT 6.30 p.m. on Friday last, while out in the country, I observed a large meteor falling slowly and almost vertically in the north-north-east. After it had disappeared, half-way between the zenith and the horizon, I noticed, in the strong twilight, a faint phosphorescent trail, which did not disappear, but changed to a straw-colour, like a streak of vapour illuminated by the setting sun. Gradually the shaft expanded in the centre and contracted at the ends, until it assumed the shape of a balloon. Continuing to change, it flattened out horizontally like a foot-ball, and apparently about half the size; and it looked a little brighter as it became more condensed. This process occupied quite half an hour, and the object remained like a nebulous patch lit up by the sun's rays, until that orb was well below the horizon, finally disappearing about 7.15 p.m. That part of the sky was perfectly clear—there were no clouds except the usual dense bank on the horizon. F. B.

Rugby, March 27.

The Satellite of Procyon.

WITH reference to Mr. Barr's letter on p. 510, I would mention that Mr. Burnham has lately observed Procyon with the 36-inch refractor at the Lick Observatory, and finds no trace of any close companion with powers up to 3300. H. SADLER.

March 29.

RECENT RESEARCHES ON THE RARE EARTHS AS INTERPRETED BY THE SPECTROSCOPE.[1]

IF I name the spectroscope as the most important scientific invention of the latter half of this century, I shall not fear to be accused of exaggeration. Photography has rendered vast services in recording astronomical and biological phenomena, and it even supplies us with indirect means of studying ray vibrations to which the human retina does not respond. The electro-acoustic devices of Edison and his co-workers permit almost magical communication between human beings. Ruhmkorff's coil and the Geissler tube have rendered notable service in physical investigations; and the electric lamp promises to aid in exploring the internal parts of living animals as well as in studying the organic forms of the deep sea. But in the spectroscope we possess a power that enables us to peer into the very heart of Nature. In the extent of its grasp and the varied character of its applicability it surpasses the telescope, and at least rivals the microscope. It enables the astronomer to defy immeasurable distance, and to study the physical condition and the chemical composition of the sun and the stars as if they were within touch, and even to ascertain the direction of their movements.

Without attempting to discuss the import of the results thus gained—which would lead us too far—I may point out that they overthrow a dogma concerning the classification of the sciences. It has been said that the simpler and more general sciences lend both doctrines and methods to the more complex and less general sciences, and that the latter give nothing in return. But we now see chemistry endowing astronomy with an original and fruitful method of research.

Turning to the very opposite extremity of the scientific hierarchy, we find that to the biologist the spectroscope is of value in studying the relations of animal and vegetable fluids, and even of certain tissues. But this wonderful instrument is clearly destined to play its chief part in what is called terrestrial chemistry—the field where it has won the most signal triumphs.

It must be remarked, despite this vast range of applicability—a range sweeping through the whole universe and embracing all the four elements of antiquity—and despite the astonishing results already achieved and the prospect of greater revelations to come, that the spectroscope is still inadequately appreciated by professed men of science, and in consequence is to a great extent ignored by the "educated and intelligent public." In urging its more thorough recognition, I do not advocate the formation of Spectroscopic Societies for the fragmentary study of everything that can be observed with a spectroscope. But I recommend researching chemists to appeal to this instrument wherever requisite and possible.

An elaborate spectroscopic study of the basic constituents of rare minerals from different localities would be of great value, and I would suggest that on all possible occasions meteorites should be submitted to careful spectroscopic analysis.

I do not propose to discuss all the splendid achievements of the spectroscope in chemistry; nor its applications in ordinary analysis, qualitative and quantitative; nor the conduct of technical operations, such as the Bessemer process. I confine myself to the light thrown by the spectroscope upon the nature and the relations of our *elements*, real or supposed.

Though systematically employed by few experimentalists, the spectroscope has already led to the discovery of several hitherto unknown elements. In the early days of spectrum analysis, attention was mainly concentrated on the flame spectra: that is, the bodies in question were vaporized and rendered luminous by the action of a flame, such as that of the Bunsen burner or of the oxyhydrogen jet. This procedure in the hands of Bunsen and Kirchhoff gave us cæsium and rubidium; afterwards, in my own hands, thallium; and in those of Reich and Richter, indium.

Then followed the production and examination of spark spectra. The spark produced by means of the induction coil, especially when its energy is reinforced by the intercalation of a Leyden jar, volatilizes and renders luminous minute portions of matter, solid, liquid, or gaseous, which may then be examined by the spectroscope. In this

[1] Abstract of Address delivered on Thursday, March 28, at the annual general meeting of the Chemical Society, by the President, Mr. W. Crookes, F.R.S.

manner gallium was discovered, in 1875, by Lecoq de Boisbaudran. In consequence of the sharpness and the well-marked character of these spark spectra, they are relied on by chemists as certain proof of the identity of any two elements which yield identical spectra.

Next was introduced the systematic study of the absorption spectra seen when a beam of light is passed through certain transparent solids or through solutions of various substances. One of the earliest observers in this branch of spectroscopy was Dr. Gladstone, who, in 1858, read before this Society a paper on the absorption of light by various metallic salts, and gave the first description of the absorption spectrum of didymium. This branch of spectroscopy has proved not less fruitful in the recognition of new metallic elements.

In the investigation of the rare earths my principal object has been to separate the true from the undemonstrated and spurious, verifying the true, rejecting the spurious, and reducing as far as possible the number of the doubtful. In the following table I have given a list of the so-called "rare elements," with which for the last seven or eight years I have been specially occupied.

TABLE.

	Atomic Weight of Metal and Formula of Oxide.	Gives Spectrum by	Component Meta-Elements according to Crookes (1836).	Component Meta-Elements according to Nilson and Krüss (1887).
Didymium	Neodymium—140·3, Nd_2O_3 Praseodymium—143·6, Pr_2O_3 Unnamed.	Absorption.	Dα λ = 475	Diα Diβ Diγ Diδ Diε Diη Diθ Diι Diχ
Decipium				
Samarium	150·12, Sm_2O_3.	Absorption and Phosphorescence.	Sδ Gε Gγ Gθ	Smα Smβ
Lanthanum	138, La_2O_3.	Phosphorescence.	? !	
Erbium	166, Er_2O_3.	Absorption and Phosphorescence.	λ 550 λ 493	Erα Erβ
Philippium	45—48, PpO.	Phosphorescence.		
Holmium		Absorption.		Xα Xβ Xγ Xδ
Thulium	170·7, Tm_2O_3.	Absorption.		Tmα Tmβ
Dysprosium		Absorption.	λ 457 (1888)—448	Xζ Xε Xη
Yttrium	88·9, Yt_2O_3.	Phosphorescence.	Gα Gβ Gδ Gζ Gη	Zα, Tb, Yt — Lecoq de Boisbaudran.
Terbium	124·7, Tb_2O_3.			
Gadolinium (Yα)		Phosphorescence.	Gβ Gζ	
Ytterbium	173·01, Yb_2O_3.	Phosphorescence.		
Scandium	44·03, Sc_2O_3.			

Column 1 gives the names by which they are commonly known. Column 2 gives their atomic weights, &c. Column 3 shows in what manner they come under the domain of spectroscopy; and columns 4 and 5 notify the components or meta-elements into which some of these bodies have been decomposed in 1886 by myself, and in 1887 by Krüss and Nilson. In the first column I have exercised a judicial leniency in retaining candidates, for the sake possibly of old associations, when strict justice would have disestablished them. Thus, it may be doubted whether decipium, philippium, or gadolinium should have been retained. But since doubts have been cast on the integrity of nearly all the occupants of this column, the line should not be drawn too strictly.

At first spectroscopic examination was applied directly to substances, natural or artificial, which had not undergone any special preparation. The idea next occurred of attempting to split up substances supposed to be simple into heterogeneous constituents before appealing to the spectroscope. The refined chemical processes used for this operation may be summarized under the name of fractionation, whether they be fractional precipitations, crystallizations, or decompositions. The essential principles of this process were so fully discussed on the last occasion when I had the honour of addressing you that I need not further allude to them.

THE DIDYMIUM GROUP.

A combination of such delicate and prolonged chemical processes with spectroscopic examination applied to bodies showing absorption spectra soon led to important discoveries. When in that year the didymium from samarskite was examined by M. Delafontaine (*Comptes rendus*, vol. lxxxvii. p. 632 ; *Chemical News*, vol. xxxviii. p. 223), he found it to differ somewhat from ordinary didymium as extracted from cerite and gadolinite, and by a series of chemical fractionations he succeeded in separating from it an earth which he called decipium, giving at least three absorption bands, one having a wave-length of 416 ($1/\lambda^2$ 578) ; another narrower and stronger, at wave-length 478 ($1/\lambda^2$ 438), and a very faint "minimum of transmission" near the limit of the blue and green. Nine months later, M. Lecoq de Boisbaudran (*Comptes rendus*, vol. lxxxix. p. 212 ; *Chemical News*, vol. xl. p. 99) announced the discovery of samarium as a constituent of the didymium from samarskite.

Still didymium was not reduced to its ultimate simplicity. In 1885, Dr. Auer von Welsbach (*Monatsh. Chem.*, vi. 477), by fractionally crystallizing the mixed nitrates of ammonium, didymium, and lanthanum, showed it was thus possible to cleave didymium in a certain direction and separate it into two other bodies, one giving green salts and the other pink salts. Each of these has a characteristic absorption spectrum, the sum of the two sets of bands approximating to the old didymium spectrum. These bodies the discoverer has named respectively praseodymium and neodymium. The neodymium spectrum, according to Dr. Auer, consists of the whole of the bands in the red, with part of the large one in the yellow ; it then misses all the green and blue, and takes in the second line in the violet. The spectrum of praseodymium takes the other part of the yellow band and all the green and blue, except the second blue, which belongs to neodymium. Subtracting these two spectra from the old didymium spectrum, there are still two bands left at λ 462 and 475 ($1/\lambda^2$ 465 and 443). Assuming that the argument from absorption spectra is a legitimate one—and all recent research tends to show that if not quite trustworthy it is at all events a weighty one—the inference I draw from these results is that the old didymium still contains a third body distinct from neo- and praseo-dymium, to which one or both of these extra bands is due.

I must venture to lay especial emphasis on the words *in a certain direction*. Didymium in my own laboratory has undergone other cleavages, and I have not yet decided whether we shall have to recognize further decompositions of neodymium and praseodymium, or whether the original didymium is capable of being resolved differently according to the manner in which it is treated. Keeping the band in the orange always of the same strength, in many of the fractions of didymium from different sources the other bands of neo- and praseodymium are seen to vary from very strong almost to obliteration (*Chemical News*, vol. liv. p. 27). In this way I have worked on the spectra of didymium from allanite, cerite, euxenite, fluocerite, gadolinite, hielmite, samarskite, yttrotitanite, &c., and the further I carry the examination the more the conclusion is forced upon me that didymium must not be regarded as compounded of two elements only, but rather as an aggregation of many closely allied bodies. Later researches of Messrs. Krüss and Nilson have led them to the same conclusion.

By examining the absorption spectra of solutions of rare earths obtained from widely different sources, MM. Krüss and Nilson (*Berichte der deutsch. chem. Gesellschaft*, vol. xx. Part 12, p. 2134 ; and *Chem. News*, vol. lvi. pp. 74, 85, 135, 145, 154, 165, 172) came to the conclusion that the elements giving absorption spectra, and known as didymium, samarium, holmium, thulium, erbium, and dysprosium, were not homogeneous, but that each one contained almost as many separate components as it produced bands of absorption.

They have discovered that in didymium obtained from some minerals one of the fainter lines of the normal didymium spectrum is strong, while others usually stronger are almost or quite absent ; results to which I shall presently refer will show that this cannot be explained by dilution or concentration. In this way, by examining a great number of minerals, they found anomalies occurred in the case of almost each of the old didymium lines, and therefore decided, as above mentioned, that it is a compound body, capable of resolution into at least nine separate components.

Identical arguments are brought forward to prove that each of the other so-called elements, samarium, erbium, holmium, thulium, dysprosium, &c., are compounds of many closely allied bodies.

Messrs. Krüss and Nilson, I believe, are pushing their investigation with the object of isolating the separate components of these different earths. They, however, question the possibility of resolving the erbia and didymia earths into their several ultimate constituents by a fractionated decomposition of the nitrates. In fact, they assert that by means of the methods of separation at present known it would be almost impossible to completely isolate any single constituent of the mixed earths. They therefore propose, as I had previously done,[1] a method by which we may certainly arrive nearer to the mark and dispense with much tedious fractionation. If we examine the minerals which contain these rare earths, we find they occur in very different states of mixture or combination. Sometimes many of the constituents which we wish to separate are conjointly present, and sometimes but few. The desired differentiation, in fact, has already been commenced by Nature. Krüss and Nilson, therefore, whichever ingredient they wish to separate, propose to operate upon a mineral which contains that ingredient as far as possible in a state of isolation. In other words, they will take advantage of the work that Nature has already begun, and endeavour by refined chemical means to put the last finishing touches to her work. Thus they will be able to work with smaller quantities of primary material,—no small consideration in the case of some minerals,—and to obtain results in a shorter time. How widely the composition of one and the same mineral, as judged by our searching physical tests, may vary, will be seen from the following instances. Fergusonite from Arendal shows six of the bands of holmium, fergusonite from Ytterby four, and that from Hitterö only three. Moreover, the ingredient provisionally called X*a* is to be found in the fergusonite from Ytterby, but not in that of Arendal and Hitterö.

The foundation for thus firmly declaring what I had previously ventured to infer, is the striking differences in the spectra given by several specimens of one earth, say didymium, when obtained from different sources.

We are anxiously waiting the results of this investigation, but although the paper quoted was published in July 1887, no further communication has come from these illustrious workers.

Chemists recently have stated, as proof of the existence of new elements, the fact that certain bands of absorption as seen in various fractions "follow the same variations

[1] Address to the Chemical Section of the British Association, Birmingham Meeting, *Chem. News*, vol. liv. p. 123. "On the Fractionation of Yttria," *Chem. News*, vol. liv. p. 157. *Proc. Roy. Soc.*, vol. xl. (1886), p. 505.

of intensity." Before deciding the question whether didymium is a homogeneous whole, or whether an argument in favour of its heterogeneity can be based on the fact that the absorption spectra of didymium from different minerals differ *inter se*, it was necessary to ascertain if the absorption bands seen in its solutions, whatever the thickness of the layer, whether dilute or concentrated, followed the same variations, and also to ascertain the nature of these variations. To contribute to this inquiry I examined the absorption spectrum of a solution of neutral didymium nitrate containing 1 part by weight of metal in 10 of water, as seen through a series of cells from 1 mm. to 25 mm. in thickness. For this work I used a new form of binocular spectroscope, fitted with a mechanical tracing arrangement, so that each spectrum can be automatically mapped on paper strips. At the bottom, at 25 mm. thickness, all the known bands are visible, and they become fainter and die out in order, some of them remaining visible almost to the end. For instance, almost as long as the deep line in the blue part of the spectrum at $1/\lambda^2$ 507 can be distinguished, it is possible to see the group of three very narrow ones next to it. Two or three other less characteristic bands can be seen only when there is a very considerable depth of liquid; thus, the group in the red at about $1/\lambda^2$ 255 cannot be seen distinctly through less than 20 mm. of this strength of solution.

Having ascertained in this series how the spectra varied in appearance with different thicknesses of the same solution (strength 1 of Di in 10 of water), I repeated the experiments, keeping the thickness of layer of solution constant, and diluting the standard solution of didymium so that the rays of light passed through the same quantity of metal as in the former series. The results in each case were practically identical; the differences being too slight to be detected in my apparatus. The spectrum exhibited, for instance, by 1 mm. of the standard solution of didymium is found to be identical with the spectrum shown by the same solution diluted twenty times and viewed through a 20 mm. cell.

There are at least two points in these researches that I must touch, since they illustrate the necessity of great caution in drawing conclusions from an examination of absorption spectra. Messrs. Paul Kiesewetter and Krüss (*Berichte der deutsch. chem. Gesellschaft*, vol. xxi., 2310; *Chem. News*, vol. lviii. pp 75, 91) have recently published a paper on this subject. They have examined gadolinite, and find that some of the constituents of didymium and samarium are absent, notably the group of lines in the green to which I have already referred. In my own laboratory I have worked for the last two years almost exclusively upon the earths from gadolinite—of which I obtained a large quantity from Fahlun—and there is not the shadow of a doubt that in my gadolinite earths the lines reported absent by Kiesewetter and Krüss are present in abundance.

Some hitherto unexplained condition doubtless rendered these lines invisible to Messrs. Kiesewetter and Krüss—perhaps the presence of some other earths, or some condition of concentration or acidity. In the light of this knowledge I do not see how we can take the results of Messrs. Krüss and Nilson or my own as final.

Owing to its complicated nature, Kiesewetter and Krüss consider gadolinite an unfavourable source of didymium for these investigations, and recommend that a large quantity of earth from keilhauite should be systematically worked up, for the reason that keilhauite didymium is more simple in constitution.

The Erbium Group.

It is known that a certain oxide, ten years ago called erbia, and regarded as belonging to a simple elementary body, has been resolved by the investigations of Delafontaine, Marignac, Soret, Nilson, Clève, Brauner, and others into at least six distinct earths—three of them, scandia, ytterbia, and terbia, giving no absorption spectra, whilst others, erbia (new), holmia, and thulia, give absorption spectra.

The first to announce that erbium was not a simple body was Delafontaine, who in 1878 (*Comptes rendus*, vol. lxxxvii. p. 556; *Chemical News*, vol. xxxviii. p. 202), published an account of philippium, a yellow oxide characterized by a strong band in the violet, λ 400 to 405, a broad black absorption band in the indigo-blue, about λ 450, two rather fine bands in the green, and one in the red.

The history of philippium is curious, and I may perhaps be allowed to give it in some detail. A year after Delafontaine's discovery, Soret (*Comptes rendus*, vol. lxxxix. p. 521; *Chemical News*, vol. xl. p. 147) published a paper in which he declared that philippia was identical with his earth X. The next month, in a note on erbia, Clève (*Comptes rendus*, vol. lxxxix. p. 708; *Chemical News*, vol. xl. p. 224) said he could not identify Soret's X with Delafontaine's philippia, as the latter was characterized by an absorption band in the blue which occupied the same place as one of the erbia bands. In February 1880 (*Comptes rendus*, vol. xc. p. 221; *Chemical News*, vol. xli. p. 72), Delafontaine returned to the subject, enumerating ten new earths in gadolinite and samarskite, viz., mosandra, philippia, ytterbia, decipia, scandia, holmia, thulia, samaria, and two others unnamed. He said that the properties of philippia were those of Soret's X and of Clève's holmia, and proposed that the name "holmia," being a duplicate name for an already known earth, should be discarded in favour of philippia. In July 1880 (*Comptes rendus*, vol. xci. p. 328; *Chemical News*, vol. xlii. p. 185), Clève repeated his former statement that philippia was not the same body as Soret's X or holmia. Delafontaine next withdrew all he had said about the absorption spectrum of philippium, and decided that it had no absorption spectrum (*Archives de Genève*, [3], 999, p. 15). Finally, Roscoe (Journ. Chem. Soc., vol. xii. p. 277), in an elaborate chemical examination of the earth-metals in samarskite, proved that philippia was a mixture of yttria and terbia. From a prolonged chemical study of these earths I have since come to a similar conclusion; but a spectroscopic examination of the earth left on igniting some specially purified crystals of "philippium formate" tested in the radiant-matter tube, has shown me that in the separation of Delafontaine's philippium the yttria undergoes a partial fractionation, and three of its components or meta-elements, $G\zeta$, $G\delta$, and $G\beta$, are present in great abundance, while others, $G\alpha$ and $G\eta$, are almost if not quite absent.

Shortly after the announcement of philippium, Soret (*Comptes rendus*, vol. lxxxvi. p. 1062) described an earth which he provisionally called X. This was soon found to be identical with an earth subsequently discovered by Clève (*Comptes rendus*, vol. lxxxix. p. 479; *Chemical News*, vol. xl. p. 125), and called by him holmia. Soret admitted the identity, and agreed to adopt Clève's name of holmia. The absorption spectrum of X consists of a very strong band in the extreme red, λ 804, two characteristic bands in the orange and green, λ 640 and 536, besides fainter lines in the more refrangible part of the spectrum.

Simultaneously with the discovery of holmia, Clève announced the existence of a second earth from erbia, which he called thulia. Its absorption spectrum consists of a very strong band in the red, λ 684, and one in the blue, λ 464·5.

In 1886 (*Comptes rendus*, cii. 1003, 1005), Lecoq de Boisbaudran showed by fractional precipitation of Soret's X, and by spectroscopic examination of the simple fractions, that this X, or holmium, consisted of at least two elements, one of which he named dysprosium, retaining the name of holmium for the residue left after deducting dysprosium. The absorption spectrum of dysprosium contains the two bands λ 753 and λ 451·5, the residual holmium having a spectrum consisting of the remaining two bands, λ 640 and 536.

Phosphorescence Spectra.

I will now deal with phosphorescence spectra. Not a few chemists and physicists, conspicuous among whom is Ed. Becquerel, have carefully studied the phenomena of phosphorescence. Phosphorescence may be excited by elevation of temperature, by mechanical action, by electricity, and by exposure to the rays of the sun, and the light thus given off, for example in the case of fluor-spar, has been examined by means of the spectroscope. In my own spectroscopic research I have dealt with the phosphorescence occasioned by the impact of the molecules of radiant matter upon certain phosphorescent bodies, or what I have ventured to call molecular bombardment.

It is not necessary for me to describe the mode of procedure further than to say that the substance under examination is placed in a very high vacuum—a vacuum which varies in degree in the case of certain earths. In such a vacuum, when submitted to the action of the induction current, substances phosphoresce very differently from what they do when treated similarly at the ordinary pressure of the atmosphere. Under such circumstances the spectroscopic examination of matter affords what I have called the radiant matter test. The number of substances which are thus phosphorescent is very considerable. Glass of different kinds, according to its composition, phosphoresces with various colours. Phenakite (glucinium silicate) phosphoresces blue; spodumene (aluminium and lithium silicate) gives off a rich golden-yellow light; whilst the emerald phosphoresces crimson, and the diamond, being exceptionally sensitive and brilliant, throws off a bright greenish white light.

The ruby, one of the minerals I examined earliest in this manner, glows with a rich brilliant red tone, quite independent, as regards its depth and intensity, of the colour of the stone as seen by daylight; the pale, almost colourless specimens, and the highly-prized variety of the true "pigeon's blood," all phosphoresce with substantially the same colour.

This method of observing the constitution of the rare earths, duly aided by delicate and prolonged chemical processes, has permitted us to push our investigations further than had previously seemed practicable. It enables us to determine whether we have reached the end of our investigations—a consummation which had hitherto been vainly sought. It has enabled us to prove that yttrium, samarium, &c., are not simple, homogeneous bodies. But what of the constituents into which they have been thus resolved? Suppose we refine them down until each displays merely one spectral band—what then? Is each one of such bodies, barely differentiated from its neighbours chemically or physically, entitled to rank as an element? If so, as I pointed out in the address which I had the honour to deliver before you in March last, we shall have to deal with further perplexing questions, arising in part from the relation of such elements to the periodic system. In a discussion of the elements, not as yet published, Dr. Wundt maintains that their possible number cannot exceed seventy-nine. But I myself see no definite and sufficient reason for limitation to this number. If these bodies are not elementary, possessing as they do the properties commonly regarded as characteristic of an element, we must be prepared to show why not?

Whatever rank may ultimately be assigned to these substances, they must, for convenience sake, have names as soon as our knowledge of their properties is in a sufficiently advanced state to allow of their removal from the suspense account.

The Yttrium Group.

Yttrium—the old yttrium—proves now to be not a simple element, but a highly complex substance. I have come to the conclusion that it may be split up certainly into five and probably into six constituents. If we take these constituents in the order of their approximate basicity—the chemical analogue of refrangibility—the lowest of these constituents gives a deep blue band, $G\alpha$; then follows a strong citron band, $G\delta$, which increases in sharpness until it may be called a line; then a red band, $G\zeta$: then a deeper red band, $G\eta$; and lastly a close pair of greenish blue lines, $G\beta$. Following these are frequently seen $G\epsilon$, $G\gamma$, and $G\theta$, the yellow, green, and red components of samarium.

A possible explanation of the existence and nature of the new bodies into which "old yttrium" has been split up, and of parallel cases which will doubtless be found on closer examination, is this. Our notions of a chemical element must be enlarged; hitherto the elemental molecule has been regarded as an aggregate of two or more atoms, and no account has been taken of the manner in which these atoms have been agglomerated. The structure of a chemical element is certainly more complicated than has hitherto been supposed. We may reasonably suspect that between the molecules we are accustomed to deal with in chemical reactions, and the conponent or ultimate atoms, there intervene sub-molecules, sub-aggregates of atoms, or meta-elements, differing from each other according to the position they occupy in the very complex structure known as "old yttrium."

The arguments in favour of the different theories are as yet not unequally balanced. But the assumption of compound molecules will perhaps account for the facts, and thus legitimate itself as a good working hypothesis, whilst it does not seem so bold an alternative as the assumption of eight or nine new elements.

I have just mentioned that the earth heretofore called yttria, and supposed to be simple, has been split up into a number of simpler bodies. Now these constituents of the old yttria are not *impurities* in yttria, any more than praseodymium and neodymium are impurities in didymium. They proceed from a real splitting up of the yttrium molecule into its components, and when this process is completed the "old yttria" has disappeared. If these newly-discovered components on further examination should be found worthy to take the rank of elements, I think, as first discoverer, I am entitled, by the custom prevailing among men of science, to name them. For the present, and until their investigation is more advanced, I designate them by provisional symbols. One of the most distinct characteristics of "old yttria" is its very definite spark spectrum. To which of its components this spark spectrum belongs I am not yet able to say. It is possible the particular component to which the spark spectrum is due yields no phosphorescent spectrum. It is also possible that the spark spectrum, like "old yttria," may prove to be compound, and then the well-known lines it contains will have to be shared between two or more of the newly-discovered bodies.

I wish emphatically to re-state that at present no single component of old yttria can lawfully lay claim to what may be called the paternal name; and it seems to me that in the present state of the question no one is entitled to call one of the new bodies "yttria," and to characterize the remainder as impurities.

Interference of Phosphorescence Spectra.

A recent discovery of some beautiful spectra given by the rare earths when their pure oxides are highly calcined, shows the remarkable changes produced in the spectra of these earths when two or more are observed in combination. It has likewise opened to me a wide field of investigation in the nature of the elements themselves. Alumina is especially active in inducing new spectra when mixed with rare earths. I have given more than a twelvemonth to the exclusive study of alumina phosphorescence, and still the research is incomplete. But I have obtained some remarkable results. A moderate amount of fractionation has enabled me to penetrate below the surface of the red glow common to crude alumina, and to see traces of a most complicated sharp line spectrum. By pushing one particular process of fractionation to a considerable extent I have obtained evidence of a body which is the cause of some of these lines. The spectrum, described by me in 1887 (*Chem. News*, vol. lvi. pp. 62, 72), is one of great beauty. The new body is probably one of the rare elements or meta-elements closely connected with decipia, for I have reproduced the spectrum very fairly by adding decipia to alumina. Before arriving at definite conclusions much time must be devoted to the subject. Certain it is that this new earth is not yttria, erbia, samaria, didymia, lanthana, holmia, thulia, gadolinia, or ytterbia, the spectrum of each of these when mixed with alumina being very beautiful, but differing entirely from the decipia-alumina spectrum.

M. de Boisbaudran's Reversion Spectra.

Another modification of the phosphorescence process is afforded by the "reversion spectra" of M. Lecoq de Boisbaudran.

The following is the description of this process by M. Lecoq de Boisbaudran read before the Academy of Sciences on June 8, 1885:—"When the electric spectrum of a solution *with a metallic base* is produced, it is customary to make the outside platinum wire (whence the induction spark strikes) positive, the liquid consequently forming the negative pole. If the direction of the current is reversed, the metallic rays (due to the free metal or to one of its compounds) are scarcely or not at all visible; at all events, so long as the exterior platinum wire now forming the negative pole is not coated with a deposit."

M. de Boisbaudran continues:—"Having again taken up last year my researches on the rare earths belonging to the didymium and yttrium family, I had occasion to observe with many of my preparations the formation of spectrum bands, nebulous, but sometimes tolerably brilliant, having their origin in a thin layer of a beautiful green colour, which appeared at the surface of the liquid (a solution of a chloride) when it was rendered positive."

M. de Boisbaudran further adds:—"The production of my reversion spectrum appears to be analogous physically with the formation of the phosphorescence spectra obtained by Mr. Crookes at the negative pole in his high vacuum tubes containing certain compounds of yttria. The conditions of the two experiments are, however, practically speaking, very different."

By this method M. de Boisbaudran has discovered phosphorescent spectra, which he considers due to the presence of two earths, one of which, supposed to be new, he has provisionally named $Z\alpha$, and another, also thought at first to be new, and therefore called $Z\beta$, but since admitted by him to be terbia (*Comptes rendus*, vol. cviii. p. 167, January 28, 1889). In the hands of so skilful an experimentalist as my accomplished friend, this method may give trustworthy indications, but the test is really beyond the range of practical analysis, owing to the difficulty of eliciting the phenomena. Unless the strength of the spark, the concentration and acidity of the solution, and the dispersive and magnifying power of the spectroscope bear a certain proportion to each other, the observer is likely to fail in seeing a spectrum even in solutions of earths which contain considerable quantities of $Z\alpha$ and terbia.

The Phosphorescence of Alumina.

I now wish to draw attention to some recent researches on the phosphorescence spectrum given by alumina. So far back as 1859, Becquerel examined in his phosphoroscope pure alumina carefully prepared, and described it as glowing with a splendid red colour. He rendered his specimens phosphorescent by exposure to the sun, and made no use of the induction spark. As described by Becquerel (*Annales de Chemie et de Physique*, vol. lvii., 1859, p. 50), the spectrum of the red light emitted from alumina agrees with that of the ruby when submitted to the radiant matter test. It displays one intensely red line a little below the fixed line B in the spectrum, having a wave-length of about 689·5. There is a continuous spectrum beginning at about B and a few fainter lines beyond it, but in comparison with this red line the faint ones are so dim that they may be neglected. My latest observations in the vacuum tube prove this line to be double, the distance apart of the components being about half the distance separating the D lines (Roy. Soc. Proc., vol. xlii. p. 26, December 30, 1886), their respective wave-lengths being 694·2 and 693·7 ($1/\lambda^2$ 207·5 and 207·8).

The red phosphorescence of this alumina is exceedingly characteristic. M. de Boisbaudran (*Comptes rendus*, vol. ciii. p. 1107; vol. civ. pp. 330, 478, 554, 824) contends, however, that this red phosphorescence is due, not to the alumina itself, but to an accompanying trace of chromium, 1/1100 part of chromium being sufficient to give a splendid red phosphorescence, whilst even 1 part of chromic oxide in 10,000 will produce a very distinct rose colour. In testing this view I have purified alumina most carefully, so as to secure the absence of chromium, and on examining it in the radiant-matter tube I have still obtained the characteristic phosphorescence and spectrum. I have then added to my purified alumina chromium in known varying proportions, but without finding any increase in the intensity of the phosphorescence. I fractionated my purified alumina by different methods, and found that the substance which forms the crimson line becomes concentrated towards one end of the fractionations, whilst chromium concentrates at the other end. I have suggested four possible explanations of the phenomena—

(1) The crimson line belongs to alumina, but it is liable to be masked or extinguished by some other earth, which accumulates towards one end of the fractionations.

(2) The crimson line is not due to alumina, but to the presence of an accompanying earth which accumulates towards the other end of the fractionations.

(3) The crimson line belongs to alumina, but its development requires certain precautions to be taken in the duration and intensity of the ignition, and absolute freedom from alkaline and other bodies carried down by precipitated alumina, and difficult of removal by washing.

(4) The earth alumina is a compound molecule, one only of its component sub-molecules giving the crimson one. If this hypothesis is correct, alumina must admit of being split up in a manner analogous to yttria.

Conclusions.

During the course of the investigations—whose results are briefly summarized in the foregoing pages,—I have repeatedly had recourse to the balance, to ascertain how the atomic weights of the earths under treatment were varying. An atomic weight determination is valuable in telling when a stable molecular grouping is arrived at. During a fractionation, the atomic weight of the earth slowly rises or falls until it becomes stationary, after which no further fractionation of that lot *by the same process* makes it vary. Usually a result of this kind has been relied on as proof that the elementary stage has been reached. This constancy of atomic weight, however, only proves that the original body has been split up by the fractionating process into two molecular groupings capable of resisting further decomposition by that identical process; but these groupings are not unlikely to break up when a different fractionating process is brought to bear on them, as I found in the separation of didymium and samarium when using dilute ammonia as the fractionating precipitant. In my paper on "Radiant Matter Spectroscopy" I said ("Part II., Samarium," Phil. Trans. Roy. Soc., Part 2, p. 129, June 18, 1885):—"After a time a balance seems to be established between the affinities at work, when the earth would appear in the same proportion in the precipitate and the solution. At this stage they were thrown down by ammonia, and the precipitated earths set aside to be worked up by the fusion of their anhydrous nitrates so as to alter the ratio between them, when fractionation by ammonia could be again employed."

It is obvious that when the balance of affinities here spoken of was reached, the atomic weight of the mixture under treatment would have become constant, and no further fractionation would have caused the atomic weight to alter.

Atomic weight determinations are valuable in telling when the fractionating operation in use has effected all the separation it can: at this point it becomes constant. The true inference is, not that a new earth has been obtained, but simply that the fractionating operation requires changing for another, which will cleave the group of meta-elements in a different direction.

Meantime, I have kept strictly in view the question, What is an element, and how shall it be recognized when met?

On this subject I beg to submit the following considerations, which, primarily referring to didymium, may at any moment apply to other cases:—

Neodymium and praseodymium are simply the products into which didymium is split up by one particular method of attack.

It must be remembered that a single operation, be it crystallization, precipitation, fusion, partial solution, &c., can only separate a mixture of several bodies into two parts, just as the addition of a reagent only divides a mixture into two portions, a precipitate and a solution, and these divisions will be effected on different lines according to the reagent employed. We add, *e.g.*, ammonia to a mixture, and at once get a separation into two parts. Or we add, say, oxalic acid to the same original solution, and we then split up the mixture into two other parts differently arranged.

Thus by crystallizing didymium nitrate (in Auer's way) we divide the components into two parts. By fusing didymium nitrate we divide its components in a different way; but so long as different methods of attack split up a body differently, it is evident that we have not yet got down to "bed rock."

Further, a compound molecule may easily act as an element. Take the case of didymium, which is certainly a compound, whether the products of Auer's operation be final or not. Didymium has a definite atomic weight; it has well-defined salts, and has been subjected to the closest scrutiny by some of the ablest chemists in the world. I refer particularly to Clève's classical memoir. Still the compound molecule known as didymium was too firmly held together to act otherwise than as an element, and as a seeming element it emerged from every trial. The simple operations to which it had been submitted in the preparation of its salts, and in its purification from other compound molecules, such as samarium and lanthanum, were not sufficient to split it up further. But subjected to a new method of attack it decomposes at once.

We have, in fact, a certain number of reagents, operations, processes, &c., in use. If a body resists all these, and behaves otherwise as a simple substance, we are apt to take it at its own valuation and to call it an element. But for all that it may, as we see, be compound, and as soon as a new and appropriate method of attack is devised, we find it can be split up with comparative ease. Still, we must never forget that, however complex, it can hardly be resolved into more than two parts at one operation.

From considerations above laid down I do not feel in a position to recognize neodymium and praseodymium as elements. We need some criterion for an element which shall appeal to our reason more clearly than the old untrustworthy characteristic of having not as yet been decomposed; and to this point I must beg to call the special attention of my colleagues. It may be that whatever body gives only one absorption band is an element, but we cannot conversely say that an element may be known by its giving only one absorption band, since most of our elements give no bands at all!

Until these important and difficult questions can be decided, I have preferred to open what may be figuratively called a suspense account, wherein, as I have previously suggested, we may provisionally enter all these doubtful bodies as "meta-elements."

But these meta-elements may have more than a mere provisional value. Besides compounds, we have hitherto recognized merely ultimate atoms or the aggregations of such atoms into simple molecules. But it becomes more and more probable that between the atom and the compound we have a gradation of molecules of different ranks, which, as we have seen, may pass for simple elementary bodies. It might be the easier plan, so soon as a constituent of these earths can be found to be chemically and spectroscopically distinguishable from its next of kin, to give it a name and to claim for it elemental rank; but it seems to me the duty of a man of science to treat every subject, not in the manner which may earn for him the greatest temporary κῦδος, but in that which will be of most service to science.

If the study of the rare earths leads us to clearer views on the nature of the elements, neither my colleagues nor myself will, I am sure, regret the months spend in tedious and apparently wearisome fractionations. No one can be more conscious than myself how much ground is yet uncovered, and how many radical questions have received but very inadequate answers. But we can only work on, "unresting, unhasting," trusting that in the end our work will throw some white light upon this deeply interesting department of chemical physics.

NOTES.

A MOST important and profoundly interesting letter from Mr. Stanley to the Chairman of the Emin Pasha Relief Committee has been published this week. It is dated Bungangeta Island, Ituri River, or Aruwimi River, August 28, 1888, and records the adventures of the Expedition from June 28, 1887, until the time when the letter was written. There is no more stirring tale even in the long and romantic history of African exploration. On April 29, 1888, Mr. Stanley met Emin Pasha on the shores of the Albert Nyanza Lake, and it would be impossible to over-rate the courage, energy, and resource manifested by the great traveller in grappling with the terrible difficulties which had stood in his way. Having spent some time with Emin, Mr. Stanley returned to the Aruwimi River, and reorganized what remained of Major Barttelot's force. When the letter was despatched he was on the point of starting again for the Albert Nyanza, and we may have to wait some time for further intelligence. Next week we hope to give an account of the geographical results of the Expedition, so far as they are now known.

The Executive Committee of the International Exhibition of Geographical, Commercial, and Industrial Botany, to be held at Antwerp in 1890, has decided to celebrate on this occasion the three hundredth anniversary of the invention of the microscope. It proposes to organize what it calls a retrospective exhibition of the microscope, and an exhibition of instruments produced by living makers. Conferences relating to all important questions connected with the microscope will also be held. The Exhibition ought to be remarkably interesting, and will no doubt be a great success.

We are glad to hear that the Congress of the United States has recently provided for the establishment of a Zoological Park in the City of Washington, and has appropriated money for the purchase of a tract of land of not less than one hundred acres in extent, immediately adjacent to that city. The proposed site for the park is the valley of Rock Creek, a small river emptying into the Potomac at Washington. This is said to be one of the most picturesque sites ever devoted to such a purpose, having several rocky cliffs of considerable extent, groves of pine, oak, beech, and other trees, and several little streams running down the steep sides of the valley into the river. Part of the land is under cultivation, but much of it is virgin forest, so that its natural advantages have been preserved by singular good fortune in spite of the neighbourhood of the growing city. Amongst the animals to be kept here will, no doubt, be a herd of the buffalo (*Bison americanus*), now nearly extinct in the Western prairies.

The half-yearly general meeting of the Scottish Meteorological Society was held in the hall of the Royal Scottish Society of Arts, Edinburgh, on Monday, April 1. Next week we shall give some account of the proceedings. We are glad to learn from the Report of the Council that the application of the Society for a grant from the surplus fund of the Association of the Edinburgh International Exhibition of 1886 has been acceded to, the Association having granted the handsome donation of £1000 towards the completion of the Ben Nevis Observatory by the establishment of the Low Level Observatory at Fort William.

A frightful hurricane, which raged for nearly two days, broke over the Samoan Islands on the night of March 16. Of seven foreign war-vessels caught by the hurricane at Apia, only H.M.S. *Calliope* contrived to make the open sea. The German and American squadrons were destroyed, and many lives lost.

The meeting of the French Meteorological Society on the 5th of March was chiefly occupied by an analysis of the report of the Krakatão Committee, by M. L. Teisserenc de Bort. The Abbé Maze presented an apparatus for rotating a thermometer fixed in a framework, and intended to take the place of the sling-thermometer in cases in which the latter was inconvenient. The Society has received a circular from the Minister of Public Instruction, asking for a list of old manuscript observations, with a view to the preparation of a catalogue for publication.

Vapour-density determinations of bismuth, arsenic, and thallium have been successfully carried out at extraordinarily high temperatures in the laboratory of the University of Göttingen by Dr. Biltz and Prof. Victor Meyer. The highest temperature hitherto attained in former experiments by Dr. Mensching and Prof. Meyer lay somewhere between 1400° and 1500° C. Now, thanks to a suggestion of Prof. Nilson, of Stockholm, means have been found of raising the temperature of the Perrot gas furnace, in which the well-known Victor Meyer porcelain density apparatus was heated, to a white heat of 1650°–1750°. Hence there are now from 200 to 300 more degrees of temperature at which density determinations are possible, and it may naturally be expected that substances which were only partially vaporized at 1450° may yield definite results at 1750°. The temperatures were determined by means of a glazed porcelain air-thermometer, which was decisively proved to be impermeable to the furnace gases. In order to further strengthen that portion of the porcelain apparatus placed in the furnace, it was surrounded by an outer casing of platinum. The volatilizations were effected in atmospheres of nitrogen, all traces of oxygen being rigorously excluded. Bismuth was found in the former experiment to be only partially vaporized at 1450°. At the higher temperatures now available it has been found to be rapidly and completely volatilized, and density determinations have been readily carried out. The values obtained in two experiments at 1600°–1700° are 11·98 and 10·12 (air = 1). If the bismuth molecule in the gaseous state consists of the normal two atoms, Bi_2, its vapour-density should be 14·4; if it contains only one atom, Bi_1, the density becomes 7·2. The values obtained, which are considerably less than that required for the normal molecular condition, show that this condition is impossible, and bismuth therefore resembles mercury, cadmium, and zinc, in containing only one atom to the molecule. In the case of arsenic the results agree very well with the assumption of a two-atom molecule, As_2. At 1714° the density found was 5·45; and at 1736°, 5·37. As_2 requires 5·20. Hence the four-atom molecule of arsenic at lower temperatures becomes dissociated about 1750° into the normal molecule consisting of two atoms. Thallium appears to be at once normal. At 1636° the value obtained was 16·11; and at 1728°, 14·25.) The ordinary molecule Tl_2 corresponds to 14·17. The metal, however, is still difficultly vaporizable even at this tremendous temperature. Another very interesting result was obtained in case of cuprous chloride, which even at 1700° gave densities almost exactly corresponding to the formula Cu_2Cl_2. Sulphur, iodine, and mercury also gave results confirming the stability of molecules consisting of two atoms of sulphur and single-atom molecules of iodine and mercury.

Mr. C. G. Hall, of Dover, sends to the new number of the *Entomologist's Monthly Magazine* the following note, written by the late moth-collector, Mr. H. J. Harding:—"On a beautiful evening at the end of June 1852, in the locality of Darenth Wood, I had just pinned my first insects taken at sugar, when I heard a strange sound behind me, and, on looking round, observed what I thought was a beetle flying round a sallow bush; when in my net, it again repeated the sound, but what was my surprise upon finding it a Lepidopterous insect. I had now got it between my thumb and finger to give it an entomological pinch, when it again produced the sound; the deadly pin was now presented, and, with the aid of my lantern, I found it was a common *Halias prasinana*. But it was a fact new to me: I I had never, during thirty years entomologizing, heard of such a thing before. The sound was as if you passed a pin sharply along three or four teeth of a comb. I suppose it was a love song to charm his lady."

Recently there have been some valuable "finds" of antiquities belonging to the Iron Age in Norway. At Nötterö, on the Christiania Fjord, there were found in a mound some bones, an iron pot with handles, a sword 2 feet 6 inches long, the handle having knobs of a yellow metal, an anvil, and a pair of smith's tongs. The mound was no doubt at one time situated close to the sea; it is now some 300 yards inland. At Laurvig a large number of similar articles were discovered in two mounds.

In a Report, just received, Mr. R. L. Jack, Government Geologist, Queensland, gives a valuable account of the geology of the Russell River, which he lately visited with Mr. Christie Palmerston. They were accompanied by eight "aboriginal boys," and Mr. Jack incidentally presents a vivid description of these attendants, who (with one exception) had been, eighteen months before, "absolute savages." "I observed with some interest," says Mr. Jack, "Mr. Palmerston's method of 'working' his boys. Its essential elements seemed to be giving them time, feeding them well, and keeping them in good humour by allowing for their propensities to hunt or play, and by making fun with them. It is fortunately unnecessary to be a Joe Miller to keep the boys amused, as anything which would amuse children will serve the purpose as well as the most delicate jokes. Mr. Palmerston's method is evidently successful, as I never saw more contented, willing, useful, and well-bred young men of any nationality."

DR. FRANZ BOAS contributes to the Proceedings of the U.S. National Museum an interesting paper on the houses of the Kwakiutl Indians, British Columbia. In these houses, the uprights are always carved according to the crest of the gens of the house-owner. The Indians of the present time make various combinations of the emblems of the gentes of both parents of the house-owner, and this is the reason for the great variety of forms. Besides this, legends referring to certain ancestors are illustrated in the emblems, and thus it happens that seemingly the ancient styles are not strictly adhered to.

AT a recent meeting of the Swedish Anthropological Society, Prof. G. Storm read a paper on his researches relating to the Lapps. The speaker held that this race had settled in Northern Scandinavia as far back as the Stone Age, and had not begun to move southwards until the Middle Ages. These southward movements had occurred periodically. At the end of the fifteenth century the Lapps had reached the sixty-fourth degree of latitude, but were now found much further south. The subject was of interest, because of the general belief that the Scandinavians had driven the Lapps northwards. In common with others, Prof. Storm was of opinion that the Lapps belonged to the Finnish-Ugrian race.

THE Dundee and District Association for the Promotion of Technical and Commercial Education have issued an elaborate Report on education in Dundee and the neighbourhood. They bring together a mass of facts which afford "abundant and gratifying evidence of a growing appreciation of science studies." The Committee, however, point out that the increase of attendance at science classes is chiefly in the elementary stages of the different subjects. More advanced classes are small, or do not exist at all. Classes in the higher branches of mathematics, physics, chemistry, and engineering have been most abundantly provided for at the Dundee University College, and the Committee hope that a very large increase in the numbers attending these may in the immediate future be the result of the elementary teaching which is now so widely appreciated.

THE Royal University of Ireland has issued its Calendar for the year 1889. The papers set at the examinations in 1888 have already been published in a separate volume, and form a supplement to the Calendar.

PROF. ANGELO HEILPRIN has contributed to the Proceedings of the Academy of Natural Sciences of Philadelphia some valuable notes on the zoology of the Bermuda Islands. These notes are based on personal observations, and on collections made during a recent brief sojourn on the islands in company with a class of students from the Academy of Natural Sciences. Prof. Heilprin devoted much time to the study of the geological features of the Bermudas. The results of his work in this department he will embody in a future paper.

AT the annual meeting of the Asiatic Society of Bengal, on February 6, an address was delivered by the President, Colonel J. Waterhouse. This address has now been printed. It contains an interesting review of the progress of science and Oriental literature in India and its nearer border-lands during the year 1888.

WE have received Parts IV. and V. of "A Catalogue of the Moths of India," compiled by Mr. E. C. Cotes and Colonel C. Swinhoe. The subjects dealt with are Geometrites and Pyrales.

TWO works on palæontology are now being issued, in parts, in Germany: "Handbuch der Palæontologie," by Dr. A. Schenk and Prof. K. A. Zittel; and "Elemente der Palæontologie," by Dr. G. Steinmann and Dr. L. Döderlein. The former work is published by R. Oldenbourg, Munich and Leipzig; the latter by W. Engelmann, Leipzig.

ONE of the Johns Hopkins University Circulars, for March, contains the following morphological notes from the biological laboratory of the University: a preliminary abstract of researches by W. K. Brooks and F. H. Herrick on the life history of Stenopus, by W. K. Brooks; list of Actiniaria found at New Providence, Bahama Islands, by J. Playfair McMurrich; on the occurrence of an Edwardsia stage in the free-swimming embryos of a Hexactinian, by J. Playfair McMurrich; notes on the fate of the amphibian blastopore, by T. H. Morgan; on the anatomy and histology of *Cymbuliopsis calcœla*, by J. I. Peck; on a new phenomenon of cleavage in the ovum of the Cephalopod, by S. Watase; on the structure and development of the eyes of the Limulus, by S. Watase; notes on the embryology of *Mülleria agassizii*, Tel., a Holothurian common at Green Turtle Cay, Bahamas, by Charles L. Edwards; on the occasional presence of a mouth and anus in the Actinozoa, by Henry V. Wilson; on the breeding-seasons of marine animals in the Bahamas, by Henry V. Wilson; the multiplication of Bryophyllum, by B. W. Barton; notice of Dr. H. V. Wilson's paper on the development of *Manicina areolata*, by T. H. Morgan; report of Dr. Henry V. Wilson as Bruce Fellow of the Johns Hopkins University.

THE Royal Society of Victoria prints an alphabetical list of the genera and species of Sponges described by Mr. H. J. Carter, F.R.S., together with a number of his more important references to those of other authors, with an introductory notice, by Mr. Arthur Dendy, Demonstrator and Assistant Lecturer in Biology in the University of Melbourne.

ON Tuesday evening Prof. Raphael Meldola delivered an interesting lecture at the Royal Victoria Hall on "Insects in Disguise, and on Mimicry," to an audience of about 500 persons, chiefly working men. Many illustrations, lent by Mr. Poulton, were thrown upon the screen, and were much appreciated. The following lectures will also be delivered at the Royal Victoria Hall:—Tuesday, April 9, "Polarized Light," by Prof. Silvanus Thompson; Tuesday, April 16, "Electric Tram-cars," by Dr. Heming.

THE additions to the Zoological Society's Gardens during the past week include a Spanish Terrapin (*Clemmys leprosa*), South European, presented by Mr. F. T. Mason; two Tuatera Lizards (*Sphenodon punctatus*) from New Zealand, a Long-billed Butcher Bird (*Barita destructor*) from New Holland, deposited; a Common Otter (*Lutra vulgari* ♂), L.....h, two Black-necked Storks (*Xenorhynchus australis* ♂ ♀), from Malacca, a Teguexin Lizard (*Teius teguexin*) from South America, purchased; an Alleghany Snake (*Coluber alleghaniensis*) from North America, received in exchange.

OUR ASTRONOMICAL COLUMN.

THE ASTRONOMICAL SOCIETY OF THE PACIFIC.—The increased interest in astronomy which has been felt in California in consequence of the erection of the Lick Observatory, and perhaps even more widely from the recent solar eclipse, which was so well and widely observed in the State, has led to the formation of an Astronomical Society under the above title. The Society was organized at a meeting held on February 7, 1889, and Prof. Holden was appointed interim President, Messrs. Schæberle and Burckhalter Secretaries, and Mr. Molira Treasurer, until March 30, when a general meeting was to be held for the election of officers and Council, and other necessary business. The seat of the Society is to be in San Francisco, but only half the meetings are to be held there, the other half being proposed to be held at the Lick Observatory during the fine weather of the summer months. A circular setting forth the objects and regulations of the new Society has been widely circulated amongst those most likely to be desirous of joining it.

THE LATE W. E. TEMPEL.—We greatly regret to have to record the death, on March 16, of the Arcetri observer, William Ernest Tempel, so well known as one of the most keen-sighted and careful observers of comets and nebulæ. Herr Tempel, though his astronomical reputation was entirely associated with Marseilles or Italy, was of German extraction, having been born on December 4, 1821, at Nieder-Cunersdorf. His parents were poor, and when he grew up he followed the profession of lithographic artist. He settled down at Venice in 1859, after several wanderings, and here first began his astronomical observations with a 4-inch Steinheil, which he had purchased for himself. Here he discovered his first comet, and the famous Merope nebula. The following year he went to Marseilles, where he acted for some time as assistant at the Observatory, then under the direction of M. Valz. Here he discovered six minor planets, and ten comets, two of which proved to be of short period, and a third was rendered not less important from its being the comet connected with the Leonid meteors. The outbreak of the Franco-German war obliged him to leave France, and he returned to Italy. Giving up his lithography, and devoting himself wholly to astronomy, he acted for four years as Assistant at the Brera Observatory, Milan, and in 1875 he became Astronomer, and practically Director, of the new Observatory at Arcetri, Florence. Five more comets were discovered by him either at Brera or Arcetri, and at this latter place he took up the study of nebulæ, of which he made a great number of exceedingly fine drawings. In latter years his health obliged him to give up the work of observing, but he had already won for himself a distinct and honourable position in the history of the science. He was elected Foreign Associate of the Royal Astronomical Society in 1881, and received the Astronomical Prize of the Lyncean Academy in 1879, besides several from the Imperial Academy of Sciences of Vienna.

THE COMPANION OF SIRIUS.—Mr. Burnham, observing Sirius with the 36-inch telescope of the Lick Observatory during the past winter, finds the place of the companion as under:—

Date of Observation.	Distance.	Position Angle.
1888·97	5″·27	13°·9.

He was not able to see any other near companion.

COMET 1888 *e* (BARNARD, 1888 SEPTEMBER 2).—Herr A. Berberich gives in the *Astr. Nachr.*, No. 2883, the results of a more detailed computation of the orbit of this comet. He has formed nine normal places from observations extending from 1888 September 5 to 1889 February 17, and made at different Observatories, and deduces the following elements:—

$$T = 1889 \text{ January } 31{\cdot}256389.$$

$$\left.\begin{aligned} \omega &= 340^\circ\ 29'\ 22''{\cdot}71 \\ \Omega &= 357\ \ 25\ \ 35{\cdot}00 \\ \iota &= 166\ \ 22\ \ 12{\cdot}20 \end{aligned}\right\} \text{Mean Eq. } 1889{\cdot}0.$$

$$\log q = 0{\cdot}2587773.$$

The ephemeris shows that the theoretical brightness of the comet will undergo very little change for several months to come, and it will be almost of the same brightness as it was on the night of discovery by the time the anniversary of that date is reached.

Ephemeris for Berlin Midnight.

1889.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
April 6	23 33 55	0 6·7 S.	0·2997	0·4582	1·7
10	23 33 4	0 10·3 N.	0·3043	0·4548	1·7
14	23 32 4	0 27·0 N.	0·3090	0·4508	1·7
18	23 30 53	0 43·2 N.	0·3139	0·4459	1·7
22	23 29 28	0 58·8 N.	0·3189	0·4402	1·7
26	23 27 48	1 13·8 N.	0·3240	0·4337	1·8
30	23 25 51	1 28·1 N.	0·3292	0·4264	1·8

The brightness at discovery is taken as unity.

COMET 1888 *f* (BARNARD, 1888 OCTOBER 30).—The following ephemeris for Berlin midnight for this comet is by Dr. R. Spitaler, *Astr. Nachr.*, No. 2875:—

1889.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
April 6	9 21 22	37 14·3 N.	0·4830	0·4015	0·15
10	9 22 11	37 25·5 N.	0·4886	0·4171	0·14
14	9 23 22	37 33·4 N.	0·4941	0·4323	0·12
18	9 24 52	37 38·5 N.	0·4995	0·4472	0·11
22	9 26 42	37 41·0 N.	0·5049	0·4616	0·10

The brightness at discovery is taken as unity.

SATURN'S RING.—Mr. Keeler, writing to *Gould's Astronomical Journal* under date March 4, states that the very fine division on the outer ring of Saturn, detected with the 36-inch refractor early in 1888, has been recently seen again under specially favourable circumstances. The new division was about one-sixth of the breadth of ring A from its outer edge, and appeared as a distinct dark line of exceeding fineness. A dark shading extended inwards from the new division almost to the inner edge of the ring. The brightest part of the ring was the narrow strip lying outside the new division, and between it and the outer edge. The Lick observers consider this marking a permanent feature of the planet, but one which can only be observed with exceptional instrumental and atmospheric advantages. The shadow of the planet on the ring was carefully watched, but no deformation was detected.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *APRIL* 7–13.

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

At Greenwich on April 7

Sun rises, 5h. 23m.; souths, 12h. 2m. 4·7s.; sets, 18h. 41m.: right asc. on meridian, 1h. 5·9m.; decl. 7° 1′ N. Sidereal Time at Sunset, 7h. 46m.

Moon (at First Quarter on April 8, 14h.) rises, 9h. 17m.; souths, 17h. 34m.; sets, 1h. 53m.*: right asc. on meridian, 6h. 38·5m.; decl. 22° 30′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	5 8	11 2	16 56	0 5·5	1 57 S.
Venus	5 35	13 54	22 13	2 58·6	23 43 N.
Mars	5 58	13 14	20 30	2 18·0	13 49 N.
Jupiter	1 35	5 31	9 27	18 33·8	22 56 S.
Saturn	12 20	20 0	3 40*	9 5·1	17 55 N.
Uranus	18 46*	0 13	5 40	13 15·1	7 15 S.
Neptune..	7 5	14 50	22 35	3 54·5	18 40 N.

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

April.	h.	
9	13	Venus stationary.
10	13	Saturn in conjunction with and 1° 9′ south of the Moon.

Saturn, April 7.—Outer major axis of outer ring = 43″·1: outer minor axis of outer ring = 12″·3: southern surface visible.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·5	81 17 N.	Apr. 11,	3 34	*m*
R Lyncis	6 52·2	55 29 N.	,, 9,		*M*
U Boötis	14 49·2	18 9 N.	,, 10,		*M*
δ Libræ	14 55·1	8 5 S.	,, 9,	1 34	*m*
U Coronæ	15 13·7	32 3 N.	,, 11,	2 13	*m*
W Scorpii	16 5·3	19 51 S.	,, 10,		*M*
R Ursæ Minoris	16 31·5	72 30 N.	,, 11,		*m*
U Ophiuchi	17 10·9	1 20 N.	,, 10,	2 32	*m*
			and at intervals of	20 8	
R Scuti	18 41·6	5 50 S.	Apr. 8,		*m*
S Delphini	20 38·0	16 41 N.	,, 13,		*m*
T Vulpeculæ	20 46·8	27 50 N.	,, 10,	22 0	*m*
			,, 12,	0 0	*M*
W Cygni	21 31·9	44 53 N.	,, 8,		*m*
δ Cephei	22 25·1	57 51 N.	,, 12,	1 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near β Ursæ Majoris ...	164 ...	58 N. ...	April 9–12.
,, ζ ,, ,, ...	206 ...	57 N. ...	Slow; bright.
	249 ...	51 N. ...	April 9–12.

THE FORCES OF ELECTRIC OSCILLATIONS TREATED ACCORDING TO MAXWELL'S THEORY. BY DR. H. HERTZ.[1]

III.

The Interference Experiments.

IN order to ascertain the velocity of propagation of electric force in the equatorial plane, we brought it into interference with another wave advancing with corresponding constant velocity in a wire. The result was, that the successive interferences did not occur at equal distances, but followed more rapidly in the neighbourhood of the oscillator than at greater distances.

This behaviour was explained by the supposition that the total force could be decomposed into two parts, of which the one, the electrodynamic, travelled with the velocity of light, while the other, the electrostatic, travelled with a greater, perhaps an infinite, velocity.

According to our theory now, however, the force in question in the equatorial plane is—

$$Z = Elm^3 \left\{ -\frac{\sin(mr - nt)}{mr} - \frac{\cos(mr - nt)}{m^2r^2} + \frac{\sin(mr - nt)}{m^3r^3} \right\},$$

and this expression in no way splits up into two simple waves travelling with different velocities. If, then, the present theory is correct, the earlier explanation can only be an approximation to the truth.

We will investigate whether the present theory leads to a general explanation of the phenomenon.

First, we can write $Z = B \sin(nt - \delta_1)$, where the amplitude

$$B = \frac{El}{r^3}\sqrt{(1 - m^2r^2 + m^4r^4)},$$

and the phase δ_1 is determined by the equation—

$$\tan \delta_1 = \frac{\dfrac{\sin mr}{mr} + \dfrac{\cos mr}{m^2r^2} - \dfrac{\sin mr}{m^3r^3}}{\dfrac{\cos mr}{mr} - \dfrac{\sin mr}{m^2r^2} - \dfrac{\cos mr}{m^3r^3}};$$

which, after transformation, gives—

$$\delta_1 = mr - \tan^{-1}\frac{mr}{1 - m^2r^2}.$$

In Fig. 5, the quantity δ_1 is represented as a function of mr by the curve labelled δ_1. The length ab corresponds in the

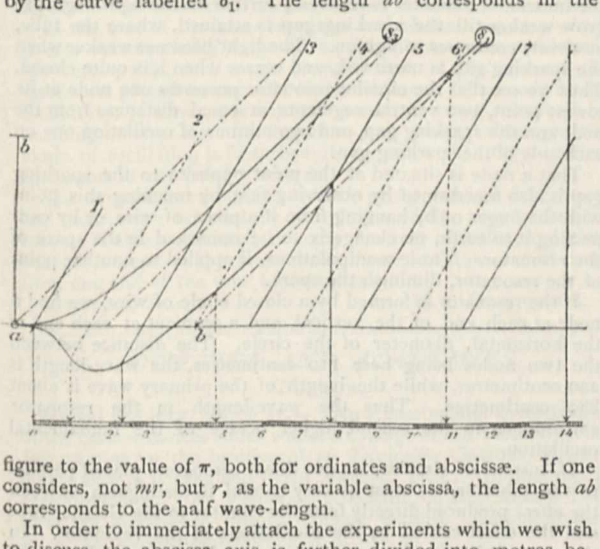

figure to the value of π, both for ordinates and abscissæ. If one considers, not mr, but r, as the variable abscissa, the length ab corresponds to the half wave-length.

In order to immediately attach the experiments which we wish to discuss, the abscissa axis is further divided into metres beneath the diagram. From the result of direct measurement (*Wied. Ann.*, xxxiv. p. 609, 1888), λ was equal to 4·8 metres, and thus the scale-length of a metre is determined. The zero of the scale is, however, not at the oscillator, but at a distance of 0·45 metre from it; for in this way the scale corresponds to the actual division of the base-line on which the position of the interferences was determined. One sees from the figure that the phase does not in general spread from the source, but its course is such that the waves arise at a distance of about $\frac{1}{2}\lambda$ from it, and give off thence a part to the conductor and a part out into space. At great distances, the phase is smaller by the value π than it would be if the waves had spread out with constant velocity from the source: the waves therefore behave, at great distances, as if they had traversed the first half wave-length with infinite speed.

The action, w, of the wire-waves at a definite position of the secondary conductor can now in any case be represented by the form $w = C \sin(nt - \delta_2)$; wherein the abbreviation—

$$\delta_2 = m_1 r + \delta = \frac{\pi r}{\lambda_1} + \delta$$

is used. λ_1 denotes the half wave-length of the waves in the wire, which in our experiment was 2·8 metres; δ, the phase of its action at the point $r = 0$, which we can arbitrarily change by adjustment of the length of the wire.

By this means we could change the amplitude, C, and give it such a magnitude that the action of the wave in the wire was approximately equal to that of the direct action. The phase of the interference depends, then, only on the difference of the phases δ_1 and δ_2. With the particular adjustment of the secondary circle to which our expression for w refers, both actions correspond (the interference has the sign +) if $\delta_1 - \delta_2$ is equal to zero, or a whole multiple of 2π; the actions disagree (the interference has the sign −) if $\delta_1 - \delta_2$ is equal to π, or any whole multiple of it. No interference occurs (the interference has the sign 0) if $\delta_1 - \delta_2$ is a whole multiple of $\frac{1}{2}\pi$.

We will now so determine δ that at the zero of the metre-divisions the phase of the interference has a definite value, δ_0, so that $\delta_1 = \delta_2 + \delta_0$.

The straight line 1 of our figure shall then represent the value of $\delta_2 + \delta_0$ as a function of the distance. The line is specially drawn with such a slant that for an increase of abscissa by $\lambda_1 = 2·8$ metres the ordinate increases by the value π, and is so arranged that it cuts the curve δ_1 in a point whose abscissa is the zero of the metre-divisions.

The lines 2, 3, 4, &c., represent further the course of the values $\delta_2 + \delta_0 - \frac{1}{2}\pi$, $\delta_2 + \delta_0 - \pi$, $\delta_2 + \delta_0 - \frac{3}{2}\pi$, &c. These lines are parallel to the line 1, and so drawn that they cut one and the same ordinate at distances of $\frac{1}{2}\pi$, and one and the same abscissa at distances of 1·4 metre.

Project now the section of these straight lines with the curve δ_1 on the axis of abscissæ below, and one gets immediately those distances for which $\delta_1 = \delta_2 + \delta_0 + \frac{1}{2}\pi$, $\delta_2 + \delta_0 + \pi$, $\delta_2 + \delta_0 + \frac{3}{2}\pi$, &c., for which, therefore, the phase of the interference increases from the origin by $\frac{1}{2}\pi$, π, $\frac{3}{2}\pi$.

One obtains immediately from the figures the following:—

If the interference at the zero of the base-line possesses the sign +(−), it vanishes for the first time at a distance of about 1 metre; it attains the sign −(+) at about 2·3 metres, vanishes again at 4·8 metres, and reverts back to the sign +(−) at about 7·6 metres; it is again 0 at 14 metres, and from thence onward the signs recur at fairly regular intervals. If at the zero of the base-line the interference was zero, it will be zero again at about 2·3, 7·6, and 14 metres, while it has a prominently positive or negative character at about 1 metre, 4·8 metres, 11 metres distance from the zero. For mean phases mean values serve.

If one compares with this result of theory the result of experiment, especially those interferences which occurred with arrangements of 100, 250, 400, 550 centimetres of wire (*Wied. Ann.*, xxxiv. p. 563), one will find a correspondence as complete as can be at all expected.

I do not succeed quite so well in calculating back to the interferences of the second kind (*l.c.*, p. 565). To get these we used an arrangement of secondary circle by which the integral force of induction through the closed circuit came prominently into account. If we regard the dimensions of the latter as vanishingly small, the integral is proportional to the rate of change of the magnetic field normal to the plane of the circle, and hence to the expression—

$$\frac{dP}{dt} = AElm^2n^2 \left\{ -\frac{\cos(mr - nt)}{mr} + \frac{\sin(mr - nt)}{m^2r^2} \right\}.$$

[1] Translated and communicated by Dr. Oliver Lodge. Continued from p. 452.

Hence we get for the phase δ_3 this expression—

$$\tan \delta_3 = -\frac{\frac{\cos mr}{mr} - \frac{\sin mr}{m^2r^2}}{\frac{\sin mr}{mr} + \frac{\cos mr}{m^2r^2}}$$

or, after transformation—

$$\delta_3 = mr - \tan^{-1} mr.$$

The line δ_3 of our Fig. 5 represents this function. One sees that for this action the phase steadily increases direct from the origin. Those phenomena, therefore, which indicate a finite pace of propagation must make themselves apparent by interferences even close to the vibrator. So it shows itself in these experiments, and just herein consists the advantage which we derived from this kind of interference experiment. But the apparent velocity comes out greater in the neighbourhood of the vibrator than at a distance, and it is not to be denied that the phase of the interference must theoretically change less, but notably more quickly than was experimentally the case.

It appears to me probable that a more complete theory, one which does not consider both conductors as vanishingly small —perhaps, also, another estimate of the value of λ—would here afford a better correspondence.

It is of importance that even on Maxwell's theory the experiments cannot be explained without assuming a marked difference between the velocity of waves along wire and their velocity in free space.

(To be continued.)

NOTE ON THE USE OF GEISSLER'S TUBES FOR DETECTING ELECTRICAL OSCILLATIONS.

AT the suggestion of Prof. Lodge, I undertook to repeat in the Physical Laboratory of the University College, Liverpool, Hertz's celebrated experiments on electrical oscillations.

In performing these experiments, I was searching for means to make the effect of the electrical oscillations more easily observable, and I was induced to use for this purpose (1) Geissler's tubes, in order to strengthen the visible effect; and (2) the chemical action of the oscillating currents (paper soaked in solution of iodide of potash), in order to obtain a permanent trace of them.

For the present I will describe briefly the results of the use of Geissler's tubes.

In order to produce the electrical oscillations, I used a conductor consisting of two zinc plates, about 41·5 centimetres square, suspended in the same plane 55 centimetres apart; to each plate was fastened a No. 6 copper wire, which was finished off with a small brass knob. The two brass knobs were about 5 millimetres apart, and formed the *sparking gap*, as we shall call it. As receiver of the oscillations, I used, like Hertz, circles of No. 14 wire, 35 centimetres in radius.

After the example of Mr. F. T. Trouton (NATURE, February 21, p. 391), I will call the first conductor a *vibrator*, and the wire circles, or other receivers, resonators.

The vibrator was connected with a small coil, 20 centimetres long, supplied with an ordinary spring interrupter, and excited by four secondary cells.

If we connect one electrode of a convenient Geissler's tube with either side of the sparking gap of the resonator, currents pass through or into the tube, which lights up and so makes the effect of the electrical oscillations on the resonator visible even at a great distance.

Of the few tubes which were at my disposal, I found that the most convenient for this purpose was a small one with electrodes 8·5 centimetres apart, and filled with highly rarefied air. But spectral tubes 20 centimetres long and filled with hydrogen, oxygen, or nitrogen also gave good results.

With the first mentioned tube I perceived *a visible effect*, when the resonator was held horizontally in the plane containing the wires of the vibrator, and with the sparking gap turned towards it, at a distance of 4 metres from the vibrator. By this arrangement all the phenomena described by Hertz (*Wiedemann's Ann.*, xxxiv. p. 160, 1888) about the direction of the electrical lines of force can easily be shown.

A very instructive experiment is to show the directions of these lines by several resonators disposed round the vibrator. For this purpose I suggest the following apparatus:—

On a wooden frame mounted so as to be able to revolve on a vertical axis standing under the sparking gap of the vibrator are fastened several resonators, with their planes vertical and parallel respectively to the directions of the lines of force and the sparking gaps at the highest point. These resonators are supplied with Geissler's tubes. In this position of the resonators all the tubes will lighten up when the vibrator is working. But if the frame with the resonators moves round the vertical axis, the light of the tubes will become weaker, and, when the frame is turned 90°, the tubes will become quite dark; the planes of all the vibrators in this position being perpendicular to the directions of the lines of force. This change will occur inversely by turning the frame from 90° to 180°.

If, instead of one resonator, two are fastened to each point of the frame, one perpendicularly to the other, both being vertical, the changes in either of these will be contrary—that is to say, when the light in one set of the tubes becomes brighter it becomes weaker in the set perpendicular to it and *vice versâ*. Thus the strength of the light is, so to say, proportional to the magnitude of the components of the lines of force in the direction of the tubes.

If a disconnected Geissler's tube is held near the vibrator, it begins in a short time to light up, owing to oscillatory currents passing through it. The same effect is obtained if instead of holding the tube by the hand it reposes on an insulating body. This lighting occurs at all points near the vibrator, except about the sparking gap. The tube becomes quite dark if the hand or a conductor is interposed between it and the vibrator; on the contrary, the interposing of an insulating body causes no change in the tube. The tube becomes more sensitive if a portion of it is surrounded with tinfoil.

In this way the existence of electrical oscillations in space can be ascertained, and also the transparency of insulating bodies and the opacity of conductors for electrical oscillations can be demonstrated.

When the two electrodes of a Geissler's tube are connected with two different points of a resonator, the effect in the tube is produced by the difference of potential of the two points. If now we connect one point of the vibrator or the resonator with one electrode of the tube, the other electrode hanging free in the air or being earthed, we have an alternative current through the tube whenever the potential of the point connected with the electrode becomes different from zero, and thus the tube lights up. The effect is strengthened if one portion of the tube is surrounded by tinfoil. This is a very convenient arrangement for observing the form of the electrical oscillation in conductors.

If we investigate in this manner our circular resonator held vertically before the vibrator, with its plane parallel to it and the sparking gap upwards, we find that a tube hanging at the lower end of the vertical diameter of the circle, opposite to the sparking gap, remains quite dark, and lights up when moved to the right or to the left of this point. The light becomes brighter till the horizontal diameter is reached; further on the light begins to grow weaker till the sparking gap is attained, where the tube, however, continues to lighten. The light becomes weaker when the sparking gap is narrowed, and ceases when it is quite closed. Thus we see that the circular resonator possesses one node at its lowest point, two ventral segments at equal distances from the node and the sparking gap, and two minima of oscillation one on each side of the sparking gap.

That a node is situated at the point opposite to the sparking gap is also ascertained by observing that by touching this point with the finger or by hanging from it a piece of wire or by connecting it to earth, no change is to be remarked in the spark of the resonator. These manipulations, if applied to another point of the resonator, diminish the spark.

If the resonator is formed by a closed circle of wire, we find a node at each end of the vertical, and a segment at each end of the horizontal, diameter of the circle. The distance between the two nodes being here 110 centimetres, the wave-length is 220 centimetres, while the length of the primary wave is about 880 centimetres. Thus the wave-length in the resonator corresponds to the second higher octave of the fundamental oscillation.

If, instead of circular, we use linear resonators placed parallel to the vibrator, we must be very careful to distinguish between the effect produced directly from the vibrator in the Geissler tube and the effect caused by the oscillations of the resonator. In the case of the circular resonators, placed in the position above described, one need not trouble much about the direct effect of the vibrator, this being very small in the neighbourhood of the vibrator's sparking gap.

To prevent the direct action a small uninsulated metal screen can be placed between the vibrator and the tube, or the tube can be hung by a long and fine wire, in order to be removed from the sphere of the direct action of the vibrator. The best plan is, however, to surround the tube by wire gauze, which stops the direct action of the vibrator on the tube, and yet permits the tube to be observed. The absence of direct action can be ascertained in the different positions which the tube takes by insulating it from the resonator without changing its position, and noticing if it becomes quite dark.

If we place before the vibrator a resonator, consisting of a straight wire 220 centimetres long (I used copper wire No. 6), we find, by the tubes, that nodes exist in the middle and the two ends of the wire, consequently two segments at 55 centimetres from each end of the wire.

(The above-described circular resonator can be likened to a linear one which is curved to a circle and its two ends soldered together, thus the two nodes of the ends becoming one single node.)

If this straight wire is cut in the middle, a torrent of sparks passes between the separated ends, even if they are removed several millimetres apart. If then we examine each half of the wire, we find that it possesses a node in the middle and two segments, each at one end, but the node is not so well defined as in the case of the uncut wire; there is no single point the potential of which remains continually equal to zero, but a line in which the difference of potential from zero is a minimum. This complicated form of oscillation is produced by the fact that the forces acting in this resonator are not equal at all points or symmetrically distributed with respect to it, as in the case of the long resonator. The oscillations of the short resonator may be compared to those of a rod which is not firmly fixed by its middle. The state of these oscillations is not stable. If one or both ends of this resonator are touched by the finger, they become nodes, and a well-defined segment appears at the middle of the resonator. If the Geissler tube be connected with this middle point, it begins to light up when the ends of the resonator are touched, and ceases to light the moment the fingers are removed; the contrary takes place if the tube be connected with one end of the resonator. This phenomenon is analogous to the change of the form of the vibration of a rod when fixed by its middle or by its ends.

Quite similar is the mode of oscillation of a resonator 220 centimetres long disposed on one side of the vibrator; it possesses also a node at the middle not well defined, and a segment at each end.

To conclude, I will describe the mode of oscillation of a resonator, 110 centimetres long, disposed parallel and symmetrically to the vibrator. This resonator possesses one node in the middle, very clearly defined, and a segment at either end. This form of oscillation is the same as would occur in the long resonator if one-fourth of its length from each end were cut off.

In the case of the latter resonator and of the long one, which is also symmetrically disposed to the vibrator, the oscillations are very stable, and much stronger than in the case of the resonators placed on one side of the vibrator. The mode of the oscillations of these symmetrical resonators is not disturbed by touching them by the fingers at any point, although the mode of oscillation is disturbed if we touch the unsymmetrical resonators at any point whatever.

The experiments described must be performed in a dark room, and much care be used in the choice of the proper Geissler tubes. Tubes containing mercury are very sensitive, and they become more so if the mercury be allowed to flow several times from one end of the tube to the other.

Liverpool, March. E. J. DRAGOUMIS.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 21.—"An Experimental Investigation of the Circumstances under which a Change of the Velocity in the Propagation of the Ignition of an Explosive Gaseous Mixture takes place in Closed and Open Vessels. Part I. Chronographic Measurements." By Frederick J. Smith, M.A., Millard Lecturer on Mechanics, Trinity College, Oxford. Communicated by A. G. Vernon Harcourt, F.R.S.

It has been noticed by several investigators, viz. MM. Berthelot and Vielle, MM. Mallard and Le Chatelier, and Prof. H. B. Dixon, F.R.S., that explosive gaseous mixtures after ignition do not reach their maximum velocity of propagation at once, but that a certain maximum velocity is attained soon after initial ignition.

In order to investigate this period, which may be called the acceleration period of an explosion, chronographic measurements of a peculiar nature were found necessary.

It was at once evident that but little advance in this branch of the subject of explosions could be made unless exceedingly minute periods of time could be measured with certainty.

A new form of chronograph has been devised to meet as far as possible all the requirements of the case, by means of the instrument. The following results have been obtained:—

(1) The $\frac{1}{20000}$ of a second can be measured with ease, and periods of time differing from $\frac{1}{10}$ of a second to $\frac{1}{20000}$ of a second can be recorded on the same moving surface.

(2) The surface which receives the record moves at a velocity which is practically constant during the traverse of 5 cm.; also its velocity can be varied between wide limits.

(3) A large number of time records can be made side by side, all records being made in straight lines.

(4) Fractions of recorded vibrations of a fork can be subdivided by means of a micrometer microscope. This is not the case with vibrations recorded on a surface attached to a pendulum, where the velocity varies from zero up to a maximum at the middle of the swing.

The electro-magnetic styli, by means of which events are marked, are so constructed that their period of "latency" is almost absolutely constant, and their electro-magnets are so wound that no sparking takes place on breaking the circuit.

A moving surface is carried on a carriage, which is propelled by means of a falling weight, which after a certain velocity has been attained is removed: the surface then moves with a velocity which is found to be practically constant for the limits between which a time record is made.

The chronograph is used in conjunction with a steel tube in which the explosions take place. At even distances along the axis of the tube, conducting bridges, eight to ten in number, of Dutch metal insulated from the tube, are placed; each bridge is connected electrically with a recording stylus, so that as each bridge is broken by the explosion, a mark is made on the surface of the chronograph; these markings when duly interpreted provide data for constructing a curve, which indicates the rate at which the velocity of the explosion is changing during its propagation.

The rest of the paper treats of the methods by means of which the errors due to the use of electro-magnets in chronographic work have been dealt with and reduced as far as possible.

Chemical Society, March 7.—Mr. W. Crookes, F.R.S., in the chair.—The following papers were read:—The decomposition of carbon disulphide by shock; a lecture experiment, by Prof. T. E. Thorpe, F.R.S. The author, in studying the action of the fluid alloy of potassium and sodium on carbon disulphide, obtained a yellowish-brown solid substance which exploded with great violence when subjected to pressure or friction. If the explosion occurred in contact with carbon disulphide, that substance was resolved into its elements. A similar decomposition of carbon disulphide into carbon and sulphur can readily be effected by exploding a charge of 0·05 gramme of fulminate within a stout glass tube containing carbon disulphide vapour, and the experiment affords a good illustration for class purposes of the resolution of an endothermic compound into its elements by sudden shock.—The determination of the constitution of the heteronucleal $\alpha\beta$- and $\beta\beta$-di-derivatives of naphthalene, by Prof. H. E. Armstrong and Mr. W. P. Wynne. A preliminary note on the constitution of the three chloramidonaphthalenesulphonic acids obtained by sulphonating α-chloro-β-amidonaphthalene hydrochloride with weakly fuming sulphuric acid.—The action of chloroform and alcoholic potash on phenylhydrazine, by Dr. S. Ruhemann.

March 28.—Annual General Meeting.—The following is an abstract of the Annual Report, read by the President, Mr. W. Crookes, F.R.S.:—The pleasant duty again devolves on me to present to you the annual report on the state of the Chemical Society during the twelve months just past. The following statement shows the numerical position of the Society—

Number of Fellows (March 28, 1888)	1534
Present number of Fellows	1614
Increase	80

103 papers have been communicated to the Society this session. Our library continues to increase, and every year becomes richer in rare volumes and books of reference. The duplicate library for lending is also becoming increasingly useful. The expenditure under this head for the current year is £308 5*s.* 6*d.*

I must now ask your attention to an event of which none of you can be ignorant, which, though not exclusively relating to chemistry, bears closely upon it and upon the future of British science. I refer to the protest against the examination system in education which appeared in November last. That protest had long been in the air. For years past, men who take the trouble to observe and to reflect have come to the conclusion that competitive examination is injurious to the individual, injurious to the race, and that it starves original research at the root. They have convinced themselves that if we flag in scientific investigation, that if a large and increasing proportion of professorships and of leading positions in industrial establishments, both in the home kingdom and in the colonies, are filled by aliens, the fault lies mainly with our educational system. Men trained chiefly to pass examinations either in theoretical or practical departments cannot equal those who have been schooled in actual research, trained to accurately observe and draw correct inferences from facts. All the earlier protests were desultory, and calculated to produce no lasting impression; but the recent manifesto is the expression of the collective opinion of many earnest representative men and women. Hence it cannot be slighted as the mere outcry of a faction, a sect, a school, or an interest. A most satisfactory feature is the adhesion to the protest of men who formerly were in favour of competitive examination as the test for entrance into the civil or military service of the State. Prof. Max Müller, of Oxford, frankly admits he now considers competition to be a mistake, and avers that the failure springs not only from the manner in which the system has been worked, but is involved in its very nature. But if this protest is to avail it must be energetically followed up, for I must repeat what I have before declared, that the position of science in Britain is far from satisfactory. Though the number of articles devoted to research in German Transactions and journals exceeds those in our own publications, we must remember that the population of the German Empire is greater than that of the United Kingdom by at least one-fourth; further, that the *savants* of Russia, of the Austrian Empire, of Switzerland, of Holland, and Scandinavia, largely select German journals as their medium of publication. Not a few English and American scientific men follow the same course. Hence, as regards quantity, our share in the world's scientific work is more considerable than appears at the first glance. Further, I think that if deficient in quantity English research excels in quality. If we do less detail work we furnish a larger proportion of generalizations and laws than most of our rivals. As the discoverers of laws and generalizations, Black, Boyle, Dalton, Faraday, Graham, Joule, Newton, Wollaston, and Young are household words in the laboratory—yet none of these men were the products of the examination system. There is another evil against which I must strongly protest. I refer to the system of "sealed papers." Everyone knows that on the Continent, more especially in France, it is common for anyone who has, as he thinks, approached the solution of some important question, to deposit a sealed sketch of his incomplete results with the President or Secretary of some learned Society. The sketch may lie *perdu* for years, until the author requests it may be opened and read before the Society. The practice arose from a desire that the author's priority should be guaranteed against others who might lay claim to his ideas. But priority can be quite as effectually secured by a brief preliminary notice read before some Society or sent to some journal, the author thus reserving to himself the further investigation of the subject. Among men of honour such reservations are invariably respected. But the "sealed paper" system lends itself to something which borders unpleasantly upon fraud. Suppose an investigator takes up some question, sees that it admits of two or more solutions, or that various hypotheses present themselves to him as possible. To work out the matter conclusively might require much time and trouble. He therefore writes out each hypothesis, and incloses them separately in "sealed papers," duly numbered, carefully retaining copies. In process of time some other investigator, ignorant of what the first author has done, takes up the subject, and works out one of these hypotheses to demonstration. So soon as his supplementary memoir is before the world the first investigator requests that the "sealed paper" No. 2 or No. 3 be opened and read. The new theory, laboriously considered and worked out, is found to have been anticipated, and the man who has really done the work is robbed of much of his credit. The seeming anticipator says nothing about the contents of other "sealed papers," in which he has proposed totally different hypotheses: these he now leaves to oblivion. I think the Fellows of our Society will agree with me that a system which thus enables a man to reap the fruit of another man's experiments does not deserve to be naturalized in England. There is a further abuse to which attention may usefully be drawn. It sometimes happens a man of science will send an account of researches he has completed to two journals simultaneously, English or foreign, leaving each editor under the impression that he is the sole recipient of the communication. Or, still worse, a man reads a paper before our Society, and sends it to some foreign journal, so that it may figure in print before it appears in the Society's Transactions. To this subject I felt compelled to refer when I had the honour of addressing you last year. And you are now aware, your Council declines to publish any memoir which has previously appeared in a foreign journal, unless specially recommended by the Publication Committee and approved by the Council. The reasons for this resolution are not hard to seek. Not merely is the reputation of the Society, as the original channel of the researches in question, imperilled, or at least obscured, but a serious waste of time and labour is inflicted upon anyone who needs to read up the literature of the subject. We in England are by no means the only sinners in this respect. It often happens that memoirs which have been read before the Paris Academy of Sciences reappear as "original matter" in certain French journals. I cannot pass over a discovery made this season by Prof. Krüss concerning nickel and cobalt. As at first reported it seemed that these two metals might be eliminated from our text-books, and that two or three new substances would take their place. Had this been the case, it would undoubtedly have been one of the greatest steps in pure chemistry taken this century. It now appears that each of the two metals contains a common impurity, which Prof. Krüss has been the first to detect and isolate. Nickel and cobalt thus purified will still retain their individuality, though their accepted properties, physical and chemical, will need careful revision. In any case the discovery is most instructive, warning us how careful we should be to have firm ground under our feet. It is almost humiliating that two metals which have been subjected to infinite research and scrutiny should now be found to contain such a proportion of unsuspected impurity. You are aware that at the ballots for the election of Fellows half an hour or more of valuable time is spent in a manner which, to say the least, is not very interesting. An attempt has been made to save time by taking the ballot in the library, after the meeting, but so many Fellows leave before the end of the meeting that the number remaining has not been found sufficient to meet the requirement of the by-laws. Your Council have from time to time had this matter under discussion, and at their last meeting, on March 21, it was resolved "that in future the balloting for Fellows be conducted by means of papers." The best manner of carrying out this resolution will be a subject for future arrangement. A posthumous memoir on the compressibility of hydrogen, by the late Prof. Wroblewski, reminds us of the sad and untimely death of this meritorious and distinguished worker in physical chemistry. His death, as most of us doubtless are aware, was due to the frightful burns which he received from the overturning or explosion of a paraffin lamp. In the memoir in question Prof. Wroblewski treats of the compressibility of hydrogen at 99°, at 0°, at – 103°·5 (boiling-point of ethylene), and at – 182°·4 (boiling-point of oxygen), for pressures ranging from 1 to 70 atmospheres. From the results the following data were calculated: critical temperature – 240°; critical pressure, 13·3 atmospheres; critical volume, 0·00335. Hence it appears very doubtful whether M. Pictet or M. Cailletet really succeeded in liquefying hydrogen. Last year I had the pleasure to announce that one of our Fellows, Mr. Newlands, had received the "Davy Medal" of the Royal Society for his splendid discovery of the Periodic Law of the Chemical Elements. I may also be allowed to state that to me, your President, the Royal Society has likewise awarded the same distinction for my researches on the behaviour of substances under the influence of the electric discharge in a high vacuum, with especial reference to their spectroscopic reactions. Hence it has been suggested that I might not unprofitably claim your attention this evening for a history of the so-called rare earths, as they have been brought to light and discriminated by the aid of the spectroscope. [We print elsewhere Mr. Crookes's address on this subject.]

Linnean Society, March 21.—Mr. Carruthers, F.R.S., President, in the chair.—Mr. T. Christy exhibited the pod (36 inches in length) of an Apocynaceous plant received from Gaboon as *Strophanthus*, but believed to be allied to the *Holarrhena*.—Prof. Stewart, referring to the specimens of *Noctilio leporinus* exhibited at the last meeting of the Society, stated that he had examined the contents of the stomachs submitted to him by Mr. Harting, and had found without doubt fragments of fish, scales, and fin-rays, and a portion of the lower jaw of a small fish, proving the correctness of the assertions which had been made regarding the piscivorous habits of this bat.—Mr. W. B. Hemsley furnished a report on the botanical collections made on Christmas Island during the voyage of the *Egeria*. This included a complete list of the plants collected, with remarks on their general distribution, the author being of opinion that the flora of this island, which lies about 200 miles south of the western end of Java, was more nearly related to that of the Malayan Archipelago than to that of Australia. Mr. C. B. Clarke, commenting on the author's observations on the buttresses of trees, described some remarkable instances which he had seen of this singular mode of growth. Mr. J. G. Baker, referring to the Ferns which had been collected, noticed their affinities and distribution. Mr. R. A. Rolfe commented on three species of Orchids which had been brought home by this Expedition, all of which were new. Mr. Thiselton Dyer, referring to Mr. Lister's Report to the British Association on the zoological collections from this island, in which it was stated that the character of the avifauna was Australian, considered that this was not borne out by an examination of the flora, which was decidedly Malayan.—A paper was then read by Mr. R. A. Rolfe on the sexual forms of *Catasetum*, with special reference to the researches of Darwin and others. The purport of Darwin's paper (Journ. Linnean Soc., 1862) was to show that *Catasetum tridentatum* had been seen by Schomburgk to produce three different kinds of flowers, belonging to the same number of supposed genera, all on the same plant, and that the three represented respectively the male, female, and hermaphrodite states of the species. Mr. Rolfe showed that Schomburgk's remarks applied to two distinct species, *C. tridentatum* and *C. barbatum*, the females of which resembled each other so closely that they were thought to be one and the same—namely, *Monacanthus viridis*. Neither of these, however, belonged to the true plant of that name, which was really the female of another species—namely, *C. cernuum*, a fact hitherto unsuspected. The key of the situation was that the females of several species resemble each other very closely, and to three of them the name *Monacanthus viridis* had been applied.—After some critical remarks by the President and Mr. Bull, a paper by Mr. MacOwan was read, on some new Cape plants.

Geological Society, March 6.—W. T. Blanford, F.R.S., President, in the chair.—The following communications were read:—On the subdivisions of the Speeton Clay, by G. W. Lamplugh. Communicated by Mr. Clement Reid. The reading of this paper was followed by a discussion, in which Prof. Judd, Mr. Strahan, Prof. Blake, Mr. Hudleston, and Mr. Herries took part.—Notes on the geology of Madagascar, by the Rev. R. Baron. Communicated by the Director-General of the Geological Survey. With an appendix on some fossils from Madagascar, by Mr. R. Bullen Newton. The central highlands of Madagascar consist of gneiss and other crystalline rocks, the general strike of which is parallel with the main axis of the island, and also, roughly, with that of the crystalline rocks of the mainland. The gneiss is frequently hornblendic; its orthoclase is often pink; triclinic felspar often occurs in places; biotite is the most common mica, but muscovite is not uncommon; magnetite is generally present, often in considerable quantities. The gneiss is often decayed to great depths, forming a red soil, and the loosened rock is deeply eaten into by streams. The harder masses of gneiss, having resisted decay, stand out in blocks, and have been mistaken for travelled boulders of glacial origin. Other more or less crystalline rocks are mica-schists, chlorite-schists, crystalline limestone, quartzite (with which graphite is often associated), and clay-slate. Bosses of intrusive granite rise through the gneiss. That east of the capital contains porphyritic crystals of felspar which near the northern edge of the granite are arranged roughly in a linear direction; here also the granite contains angular fragments of gneiss. For the most part the granite of Madagascar is clearly intrusive, but this may not always be the case. The volcanic rocks are of much interest. The highest mountains, those lying to the south-west of the capital, consist, in their higher parts, of a mass of lava, for the most part basaltic, but with some sanidine-trachyte. The lava-streams are sometimes twenty-five miles long, and successive flows, up to 500 feet in thickness, are exposed by the valleys. From the great denudation which this area has undergone, and from the fact that no cones now remain, we may assume that this volcanic series is of some antiquity. Of the newer volcanic series there are numerous very perfect cones, dotting the surface of the gneiss in many places. No active volcano now exists in the island, but the occasional emission of carbonic acid gas, the occurrence of numerous hot springs and deposits of siliceous sinter, and the frequency of small earthquake-shocks, seem to show that volcanic forces are only dormant and not entirely extinct. The ashes generally lie most thickly on the side of the cone between north and west; this is accounted for by the prevalence of the south-east trade-winds. The volcanic areas are ranged roughly in a linear direction, corresponding with the longer axis of the island. Sedimentary rocks occur mainly on the western and southern sides of the island. The relations of these to each other have not yet been determined; but from the fossils (referred to the European standard) it seems that the following formations are represented: Eocene, Upper Cretaceous, Neocomian, Oxfordian, Lower Oolites, Lias. Possibly some of the slaty beds may turn out to be Silurian or Cambrian. The crystalline schists, &c., are probably, for the most part at least, Archæan. Recent deposits fringe the coasts, and are largely developed on the southern part of the island. East of the central line of watershed there is a long depression containing a wide alluvial deposit, probably an old lake-bed. Terraces fringe its sides in many places. The lagoons of the eastern coast are due to alluvial deposits. The paper concluded with some remarks on the geological antiquity of the island, its separation dating from early Pliocene times, if not earlier. This is the conclusion arrived at by Wallace from its fauna; the author's detailed researches into its flora, recently described before the Linnean Society, show that while about five-sixths of its *genera* of plants are also found elsewhere, chiefly in tropical countries, at least four-fifths of its *species* are peculiar to Madagascar. The appendix, drawn up by Mr. R. Bullen Newton, consisted of notes upon the fossils collected by the author, with tables, and descriptions of two new species—namely, *Astarte* (?) *Baroni* and *Sphæra madagascariensis*, both from deposits of Lower Oolitic age.—Notes on the petrographical characters of some rocks collected in Madagascar by the Rev. R. Baron, by Dr. F. H. Hatch. Some remarks on Mr. Baron's paper were made by the President, Dr. Geikie, Mr. H. B. Woodward, and Mr. Topley.

PARIS.

Academy of Sciences, March 25.—M. Des Cloizeaux, President, in the chair.—On the achromatism of interferences, by M. Mascart. The conclusions arrived at by Cornu and Stokes are here applied to the particular cases of interference fringes and of Newton's rings. In the phenomenon of W. Herschel's fringes the condition of achromatism is shown to be—

$$\frac{\cos^2 i}{\sin i} = \text{L} \frac{\sin \text{A}}{\cos r}.$$

—Remarks accompanying presentation of Prof. Karl Pearson's work, "The Electrical Researches of Barré de Saint-Venant" (Cambridge, 1889), by M. Boussinesq. The period from 1850 to 1886, covered by this important treatise, comprises the most remarkable researches, by the late M. de Saint-Venant, on torsion, flexion, live resistance, the distribution of elasticities in heterotrope bodies, plasticodynamics, &c. The work, which will be found of great service to English physicists, geometricians, and engineers, unfamiliar with the French language, forms the first part of the second volume of the series begun by Todhunter on the "History of the Theory of Elasticity."—On elliptical polarization by vitreous reflection, by M. A. Potier. Rejecting Cauchy's assumption of evanescent longitudinal waves, the author here develops a theory in which he takes as his starting-point the differential equations of the vibratory movement. The principle and results of this theory were already announced at the meeting of the French Association for the Advancement of the Sciences in 1872.—Researches on the cultivation of the potato, by M. Aimé Girard. The author here deals with the progressive development of the plant, and arrives at the general conclusion that the origin of the starch is to be sought in the leaves, where it is probably represented in its initial form by saccharose, or some analogous sugar. By its twofold decomposition this sugar becomes on the one hand the generator of the cellular tissue, on the other of the starch which is stored up in that tissue.—On the peroxides of cobalt and nickel, and on the volumetric analysis of these metals, by M. Adolphe Carnot. The action of potash combined with that of chlorine, bromine, iodine, or of an alkaline hypochlorite, yields in cobalt and nickel solutions certain black granular precipitates almost identical in appearance. Herrenschmidt, however, has pointed out that the peroxide of cobalt thus obtained has a brown colour, while the peroxide of nickel remains black under the microscope. M. Carnot here describes a series of experiments carried out for the purpose of determining the state of oxidation of the metals in these various precipitates. The general result is that the brown oxide obtained by precipitating cobalt with hydrogen dioxide and caustic potash at the boiling-point has the exact composition of the sesquioxide, Co_2O_3, and that the black oxide of nickel, precipitated by hypochlorite or by bromine and potash, is the sesquioxide, Ni_2O_3.—On the limits of the errors that may be committed in assaying fine gold, by M. Paul Charpentier. The figures here given are the result of about 300 assays executed by the author at the laboratory of the French Mint.—On the initial phase of electrolysis, by M. Piltschikoff. A protracted study of the phenomenon of retardation in the electrolytic process leads to the following results. The minimum electromotive force required to at once set up a visible electrolysis does not depend within certain limits on the nature of the salt, nor on the concentration of the solution (gold, zinc, sulphate of zinc; platinum, copper, sulphate of copper, nitrate of copper, gold, platinum or silver, &c.). The minimum does not depend perceptibly either on the heat of combination of the two metals, or on their contact electromotive force; but it depends essentially on the physical state of the cathode (negative pole), which may modify the resulting figures as much as 20 or even 25 per cent.—On the electric transport of salts in solution, by M. A. Chassy. The special case is here considered of a non-electrolyzed metallic salt, a salt of zinc, for instance, in a mixture of salts of copper and zinc.—On the glycol-ether of chloral, by M. de Forcrand. The author has prepared this compound,

$$CCl_3 . CH(OH).OC_2H_4OH,$$

in the crystallized state, by combining molecular proportions of chloral and glycol at the ordinary temperature. It is soluble in water, and melts at 42° C., which is also the melting-point of chloral ethylate, according to M. Berthelot.—Determination of the heats of combustion of metaldehyde, erythrite, aud tricarballylic acid, by M. Louguinine. These experiments have been carried out by means of the calorimetric apparatus under precisely the same conditions as those already published.—Papers were contributed by MM. J. Héricourt and Ch. Richet, on the varying toxic effects of the blood of the dog transfused into the rabbit; by M. V. Galtier, on the liability of sheep and other animals to contract infectious pneumo-enteritis, hitherto regarded as a disease peculiar to the pig; by M. Joannes Chatin, on the homologies of the inferior lobes in the brain of fishes; and by MM. Jules de Guerne and Jules Richard, on the fresh-water fauna of Greenland.

BERLIN.

Physical Society, March 8.—Prof. von Helmholtz, President, in the chair.—Dr. Rubens described the experiments which he had made on the selective reflection of light by metals. The method employed was as follows: the light emitted by an incandescent plate of zirconium was concentrated by a lens on to a mirror-surface of the metal under investigation, and the reflected rays were then allowed to fall into a spectroscope with flint-glass prism, whose ocular had been replaced by a bolometer. In this way the intensity of each part of the spectrum could be determined. The next step consisted in removing the mirror and putting the glowing zirconium in the place of the virtual image of the first source of light, in such a way that the rays of light, coming from the point previously occupied by the mirror, pursued the same course as in the first experiment. These rays were then allowed to fall into the spectroscope, and the intensity of each part of the spectrum thus formed by light which had undergone no change by reflection was measured by the bolometer. The intensity was determined at fifteen different points in the spectra, extending from near F in the blue into the ultra-red down to the wave-length 2μ. The changes produced in the light by reflection from the metals were represented by curves whose abscissæ corresponded to wave-lengths while their ordinates corresponded to the intensities of the several rays after reflection. The results thus obtained showed that silver possesses even for blue rays a very considerable reflexive power, which gradually increases and reaches its maximum in the red, at which maximum the intensity of the reflected light then remains constant even for rays of the greatest wave-length. Gold possesses a much smaller reflexive power for blue and green rays; the curve then rises very rapidly to a maximum in the yellow and falls again towards the red. Copper reflects the blue and green rays even less than gold does: its reflexive power then increases rapidly into the red, and then somewhat more slowly, until in the ultra-red it reaches a value equal to that for silver. Iron and nickel gave very similar curves, rising at first somewhat rapidly, but subsequently more slowly and continuously into the ultra-red, without however reaching the maximal values observed for copper or silver. On the basis of these experimental values for the reflexive power of the above five metals, the speaker had calculated their coefficients of extinction and refraction for red and blue light, making use of Cauchy's and Beer's formulæ. From this it was possible to deduce the dispersive powers of the metals, and to compare their indices of refraction with those which had been experimentally determined by Prof. Kundt: the agreement was in most cases very close.—Prof. Preyer gave an account of some letters of Robert Meyer which are shortly to be published. They were written in the years 1842 and 1844 to his friend Dr. William Griesinger. Prof. Preyer read out several characteristic passages from these letters, in which Meyer states how he arrived at his discovery of the conservation of energy, and from which his firm belief in the correctness of his theory is quite apparent. No less characteristic is the way in which Meyer takes pains to explain his theory to his medical friend, who was but little experienced in physical matters, and to put it before him in a way which he could easily understand.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Class-book of Geography (revised edition): C. B. Clarke (Macmillan).—A Treatise on Chemistry, vol. iii., Part 5: Roscoe and Schorlemmer (Macmillan).—The Principles of Empirical or Inductive Logic: J. Venn (Macmillan).—Borneo; Entdeckungsreisen und Untersuchungen; Gegenwärtiger Stand der Geologischen Kenntnisse; Verbreitung der Nutzbaren Mineralien: Dr. T. Posewitz (Berlin, Friedländer).—Tägliche Oscillation des Barometers: J. Hann (Wien).—Journal of Physiology, February (Cambridge).—Records of the Geological Survey of India, vol. xxii., Part 1 (Calcutta).

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