THURSDAY, AUGUST 5, 1897.

## *ORE DEPOSITS.*

*A Treatise on Ore Deposits.* By J. Arthur Phillips, F.R.S. Second edition, rewritten and greatly enlarged by Henry Louis, M.A. Pp. xxii + 943. 128 illustrations, and index. (London: Macmillan and Co., 1896.)

PROFESSOR LOUIS has attempted no easy task in endeavouring to bring up to date Phillips' well-known work on ore deposits. Since the first edition appeared in 1884, mineral discoveries have been numerous and important. I need only refer to the gold of the Rand and Mount Morgan, the lead and silver of Broken Hill, the copper, silver and lead of Tasmania, the nickel of Sudbury, and the iron of Minnesota. Besides these comparatively new finds, one has to recollect the progress which has been made in developing some of the older mining districts. The years which have elapsed since 1884 have likewise been fruitful in bringing forth descriptions of ore deposits: scores of official memoirs, and hundreds of papers in the *Transactions* of scientific and technical societies at home and abroad, have been written about them; so that Prof. Louis must have had great difficulty in deciding what to ingraft upon the old stock without making his book too bulky. As it is, the second edition is half as big again as the first.

I cannot agree with Prof. Louis in excluding the minerals yielding aluminium and magnesium from the term "metalliferous ores," on the ground that by popular usage this term is applied solely to the ores of the heavy metals. We must march with the times. There was a day when hæmatite was not used for producing metallic iron; what we now call the ores of nickel and cobalt were long regarded as worthless and injurious substances. Zinc blende has only comparatively lately entered into the category of ores, and later still the carbonate of manganese. My opinion is that bauxite should rather be received with welcome into what Prof. Louis seems to consider as an aristocratic circle, than hustled out as an intruder because its metal happens to be light. After all, the ratio between the specific gravities of iron and aluminium does not differ very greatly from the ratio between the specific gravities of platinum and iron. The scientific man should rather try to lead the populace aright than cringe to misconceptions.

The book is divided into two parts: the first deals with mineral deposits generally, whilst the second is devoted to a description of the ore deposits of the various countries of the entire globe.

As several objections can be made to Phillips' classification of ore deposits, Prof. Louis very wisely abandons it; but, when he endeavours to find a substitute, he meets with many difficulties. Finally, he introduces a provisional arrangement in which origin, instead of form, is taken as the basis of the classification: giving new names to old faces, he subdivides all ore deposits into two kinds—"symphytic" and "epactic."

The former term is applied to deposits which are contemporaneous with the enclosing rocks, whilst the latter includes those which have been formed subsequently. Whether it is necessary to coin two more words with which to puzzle the unfortunate miner appears doubtful. The epactic deposits are next separated into two groups, and we eventually have three main classes, our old friends: (1) Beds; (2*a*) Veins; (2*b*) Masses.

The author of a large work dealing with all parts of the world can scarcely expect to escape some errors; nor is it easy to be quite up to date.

Prof. Louis has not corrected Phillips' erroneous description of the treatment of the lead-bearing sandstone at Mechernich, Rhenish Prussia. The book says that the little nodules of galena are separated below ground, and alone are sent to the surface, whilst the waste sand left behind is employed for filling the exhausted workings. As a matter of fact the nodules are extracted by a true dressing process above ground, more particularly by the aid of a special concentrator—the "Heberwäsche."

Phillips has likewise led Prof. Louis into error by saying that the Freiberg School of Mines was founded in 1702. The first project for the School was made at the end of 1765, and the lectures began at Easter 1766.

Following my usual custom, I cannot help tilting at the employment of unnecessary provincial terms in books upon mining. If "flucan" simply means "clay," why not stick to the word understood by all English-speaking persons, and consign to oblivion the Cornish term, which serves no useful purpose? On the other hand, if a provincialism is to be adopted on account of its brevity or general convenience, it should not be altered. I refer, as I have often done before, to the expression "country rock." By "country" the Cornishman means "surrounding rock"; to say "country rock" is tautology. Further, is it advisable to introduce into technical literature such a term as "lode formation," in the sense in which it is employed by ignorant persons or unthinking mining engineers? In the first place, the term "formation," as applied to lodes, already has a definite meaning attached to it. It denotes a group of mineral veins having certain characteristics, leading one to believe that they have a common origin. The so-called "lode formations" of Western Australia are veins of a special kind which can be described without bringing in the puzzling word "formation." The introduction of any new terms should be scrutinised by writers with the greatest jealousy, for the science of ore deposits is quite obscure enough already without being further darkened by a vague terminology.

Following Phillips, Prof. Louis retains the word "huel" in speaking of various Cornish mines, though the mining companies invariably write the word "wheal." Even if one admits that "huel" may be the more correct spelling, it is far too late now to think of enforcing it. It is not convenient for the student to have the name of a mine, "Wheal Mary Ann," for instance, inserted under the letter "h" instead of the letter "w," as he may be quite ignorant of the old way of writing the word.

Slight errors in the spelling of names of persons and places are a little too common. It may be hoped that the prefix "Sir" to Dr. Selwyn's name is simply the shadow of a coming event, and that it will cease to be a mistake long before the present edition is exhausted.

With regard to his own country, Prof. Louis is a little behind the times. He says that mining is "extensively" carried on in Anglesey and in the south-west of Ireland. Taking the official statistics for 1895, one finds that only thirteen persons were employed underground in Anglesey, and thirty-two in the county of Cork. These figures scarcely justify the adverb used by Prof. Louis. Herodsfoot mine is spoken of as if it were still at work, though it was abandoned fully ten years ago; in fact, Cornwall has ceased for some time to be a lead-producing county.

No doubt it is easier to pick a few holes in a work of this kind than to write it, and it must not be supposed from my criticisms that I in any way undervalue the great amount of care and labour which Prof. Louis has bestowed upon his work. The new book is a very valuable addition to technical literature, in spite of the want of a sufficient number of plates, which was likewise a fault of the first edition; possibly Prof. Louis may have had to bow to restrictions imposed upon him by his publishers. In any future edition it would be better to sacrifice some of the letterpress, if by so doing more figures could be introduced. C. L. N. F.

## *THE RESISTANCE OF THE AIR.*

*Experiments made with the Bashforth Chronograph to find the Resistance of the Air to the Motion of Projectiles.* By Francis Bashforth, B.D. (Cambridge: at the University Press, 1895.)

IF Mr. Bashforth could have struck a bargain with the Government similar to that made by James Watt with the Cornish miners, his royalties on the gunpowder saved annually by the use of his Ballistic Tables would have rivalled the claims contested in some recent lawsuits.

By a few well-designed experiments with his Electro-Ballistic Chronograph, initiated now more than thirty years ago, when he took up the appointment of Professor of Mathematics to the Advanced Class of Artillery Officers, he was able to determine the resistance of the air at all useful velocities to the projectiles fired from the muzzle-loading guns then in vogue, to which a return had been made by our military authorities.

In accordance with the proverb—*Ὅτι οὐκ ἔστι προφήτης ἄτιμος, εἰ μὴ ἐν τῇ πατρίδι αὐτοῦ, καὶ ἐν τοῖς συγγενέσι καὶ ἐν τῇ οἰκίᾳ αὐτοῦ*—these experiments attracted great attention in naval and continental expert circles, everywhere except at Woolwich; they still remain to this day the only actual determinations of the Resistance of the Air with which we have to work in Artillery; and the Ballistic Tables of Mr. Bashforth, based upon these experiments, are to be found in all naval and foreign treatises on the Theory of Artillery.

When the Bashforth Chronograph revealed the unsteadiness of shooting of our guns, the manufacturers of ammunition and guns felt insulted, and wanted to throw the blame on the imperfections of the instrument, as if their manufactures were not absolutely perfect; now, however, in recent Range Tables, the manufacturers have to submit to the indignity of 50 per cent. zones, showing the degree of scattering of their weapon at the various ranges.

Although the breech-loading system has been finally adopted, and although the experimental side of Electricity may be said to have been re-created since Mr. Bashforth began to experiment, so far no new experiments have been carried out or sanctioned for finding the modification of the Resistance of the Air due to changes of shape in modern projectiles, and to the superior steadiness in flight obtainable with the breech-loading system.

Gunpowder, or cordite, costing annually many thousands of pounds, is blazed away at proof, merely to inspect brands of powder by determining a muzzle velocity, by shooting between two electric screens. If only one more screen, but the more the better, could be introduced, much useful information could be gained at the same time of the Resistance of the Air.

The Boulengé Chronograph, employed at proof, is not adapted for more than two screens; but superior Chronographs are now in the field, with which it is possible to read any number of screen records.

Every new Chronograph claims to record at least a millionth of a second; but Mr. Bashforth did not attempt to go behind the fourth decimal, knowing that the accuracy of any experiment is only that of its most inaccurate part; in this case the screens, in which the breaking of a wire might take place within the limits of a foot, according to the manner in which the wire is struck by the head or shoulder of the projectile; this alone is sufficient to account for a discrepancy enough to render the fourth decimal almost nugatory.

By averaging the results, however, of as many rounds as possible, Mr. Bashforth has arrived at the normal Resistance of the Air, from which individual shots may vary as much as 10 per cent. or more; and allowing for difference of shape, smoothness, and steadiness of projectile, the continental experiments of Krupp and others amply confirm Mr. Bashforth's results.

Inflation in the manufacture of warlike stores is at the present moment unprecedented; and yet very little careful examination takes place, of what improvements are possible as the result of scientific inquiry, carried out leisurely on a small scale. Our ministers vote millions for warlike stores, and still, as in the days of General Peel's report, they scrutinise with the greatest care a small vote, technically called ineffective, which serves to prevent these millions from being money thrown away.

Provided with Bashforth's Tables, and a knowledge of how to use them, which need not appreciably alter his weight or the height of his centre of gravity in the saddle, the artillery officer of the future might dispense with half the useless weight of ammunition he drags about in the field; or, at least, the same weight might be refashioned for a heavier gun, employing curved fire.

The superior ballistic coefficient of the larger projectile soon enables it to overtake the puny projectile of the rival pop-gun; but to secure these advantages, good range-finding is an indispensable accessory; the weight of the most efficient range-finder need not, however, exceed that of a single round, which might otherwise be expended as a trial shot.

Mr. Bashforth writes occasionally with bitterness; but he has been the victim of our curious official scientific etiquette, which disparages a new idea when submitted, and afterwards appropriates the results without acknowledgment when the idea has proved a success.

No doubt this etiquette is inspired from the highest quarters, but Mr. Bashforth was not the man to take such treatment lying down; he did not rest till he had extracted a written minute, acknowledging that his experiments had been adopted officially. But a bad mark has been put against him for his audacity; for while other inventors have been rewarded, we have yet to learn that Mr. Bashforth has received any acknowledgment from our own Government, either of a tangible or complimentary nature.

## *TEACHING THE TEACHERS.*

*Thirty Years of Teaching.* By L. C. Miall, F.R.S., Professor of Biology in the Yorkshire College. Pp. viii + 250. (London: Macmillan and Co., Ltd., 1897.)

A FRIENDLY criticism of schoolmasters and their ways, written by a professor of biology, is a book of special value. Biology is a subject not usually taught in schools, and students taking it up at college are not in the condition which the schoolmaster is fond of describing as "thoroughly well-grounded in the elementary parts of the subject," and the scientific professor as "crammed with a multitude of imperfectly understood facts." The professor of biology therefore, in forming an opinion upon the previous training of his pupils, thinks more of the mental habits which they have formed than of the knowledge which they have acquired.

Prof. Miall is singularly fortunate in his suggestions upon the teaching of special subjects:—that geography should be taught mainly by means of map drawing; that text-books should be used merely as works of reference; that lessons in arithmetic and geometry should include practical work in measurement; that in teaching modern languages the written or spoken language should be made the basis, and instruction in grammar founded upon it; that mastery of English does not come by grammar and analysis, but by observation and practice; that true science consists in a scientific habit of mind, and not in a knowledge of scientific facts; that the present system of teaching classics to boys who leave school at sixteen, is laying a costly foundation for a structure which will never be built. These are truths which schoolmasters may or may not believe, but which very few of them follow in practice, influenced as they are chiefly by the demands of examinations, but also in part by the large numbers in their classes, and by the inertia of human nature. All that Prof. Miall says upon the method of teaching of every subject is well worthy the careful attention of every schoolmaster. Prof. Miall, too, shows a keen insight when he speaks of the true value of examinations, while the statement that the University local examinations were once a great step forward, but that they have now (like other human institutions) outlived their usefulness, and become rather a hindrance than a help, is one which may mark an epoch in the history of middle-class education.

In one point, however, we find the experience of the professor somewhat at variance with that of the schoolmaster. Prof. Miall appears entirely to overlook the moral elements of boyhood: he tells us, for instance, that boys will work at a subject in proportion to their interest in it; this is probably the case with students, but it is conspicuously not the case with schoolboys. The chief factor in causing the industry of a schoolboy is his sense of duty; the industrious boy is the one who has a strong sense of duty, the idle boy is he in whom the sense of duty has not been aroused; the main thing in which boys always will be interested is not their lesson, but each other. Again, Prof. Miall would abolish home-work for younger boys, and commence it with boys over fourteen; but, in this case, how much home-work would he get done? We venture to say that if boys had not formed the habit of doing a regular hour's evening work by the time they were twelve, they would never begin at all; the object of setting home-work to young boys is not to replace teaching, but to assist in forming regular and industrious habits. A few other instances might be given in which the experience gained by observing students would only lead to failure when applied to the teaching of schoolboys; and we doubt whether any boys could be taught by *class* lessons to read, write, and speak French by the age of fourteen.

Prof. Miall gives us some striking remarks upon the absurdity of extreme precision when based upon loose data, and some interesting biographies occupy the final chapters of his book; on the whole, we can thoroughly recommend "Thirty Years of Teaching" for the perusal of every schoolmaster and every parent in the country.

## *OUR BOOK SHELF.*

*Abhandlungen zur Physiologie der Gesichtsempfindungen.* Edited by J. von Kries. Vol. i. Pp. vi + 198. (Hamburg and Leipzig: Leopold Voss, 1897.)

THE five papers in this volume are contributions from the Freiburg Physiological Institute, reprinted from the *Zeitschrift für Psychologie und Physiologie der Sinnesorgane.* Dr. von Kries is the author of three papers dealing with the functions of the retinal elements, subjective effects produced by light of short duration, and colour vision, in the course of which a number of observations on the visual effects of different parts of the spectrum on different colour-blind individuals are recorded. Two other papers included in the collection are on the influence of light-intensity and adaptation on the vision of green-blind subjects, by Drs. J. v. Kries and W. Nagel; and on the influence of the yellow-spot—the part of the retina which lies directly in the axis of vision—upon colour appreciation, by Dr. Breuer.

*Cuirassés et Projectiles de Marine.* By E. Vallier. Pp. 188. (Paris: Gauthier-Villars et Fils; Masson et C^ie^, 1897.)

*Les Huiles minérales; Pétrole, Schiste, Lignite.* By François Miron. Pp. 198. (Same publishers.)

BOTH these volumes appear in the Encyclopédie scientifique des Aide-Mémoire series. M. Vallier's volume deals with the various kinds of armour-plates used upon men-of-war of different nations, and the projectiles employed for attacking these ironclads. In M. Miron's book, the extraction, composition, use, and analysis of mineral oils is described.

*Botanische Wanderungen in Brasilien.* By Prof. Dr. W. Detmer. Pp. vi + 188. (Leipzig: Veit and Co., 1897.)

IT is not given to many of us to realise our heart's desire, yet this is what Prof. Detmer did when he made a journey to Brazil. The impressions received from the start to the home-coming are set down in this little book, and the whole make an interesting narrative. The journey taken was through the States of Bahia, Rio de Janeiro, Minas Geraes, San Paulo, and Espirito Santo.

## 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.*]

### Röntgen Ray Theory.

THE most important question whether Röntgen rays are to be considered as falling into the domain of light, or whether they are something else, has occupied so many minds, that the literature on this subject has grown enormously; but is it settled yet whether they are transversal waves of very small wave-length, or longitudinal waves, or vortex motion of the ether, or longitudinal impulses, or due to electromagnetic dispersion, or radiant matter?

Having read so many theories, with so many supporters and opposers, one gets puzzled what to believe.

Not a small part of the confusion is caused by the number of contradictory experiments; and no wonder at this, the question being, in fact, of the utmost complication; that is to say, not only as to the nature of the rays themselves, but also the way in which to get them.

Being engaged in the study of silent electric discharges, it was rather in our line to consider the question of kathodic, as well as of our anodiç, discharges. Thus we arrived at a conclusion about the nature of Röntgen rays, which explains a great deal, though we must confess that certain assumptions have to be made.

This is not an attempt to explain this most difficult problem, but to suggest an hypothesis that is most nearly in accordance with experiments, so far as they go.

We consider Röntgen rays to be nothing but discharged kathode rays, and will now test this hypothesis by seeing how it will explain some of the most striking experiments and facts.

As a matter of fact, kathode rays are deflected by a magnet; they obey the law of attraction of a current by a magnet; *i.e.* they behave as a current or stream of negatively charged particles.

These negative particles impinge upon the glass wall o. the tube, which, as is well known, possesses a strong positive (external) charge.

Is there anything strange in the idea that those particles may lose their charge when in contact with the positive charged wall, and proceed on their way as *discharged* particles?

These *discharged* particles cannot, and are not, attracted by a magnet; why should they be? Hence the essential difference between kathode and Röntgen rays is explained.

Röntgen rays would thus discharge a negatively or positively charged body, as a matter of course, since any electrified body, struck by neutral particles, always loses its charge.

Now let us consider some important details: in the first place, with regard to the focus tube. Some people do not accept the property of being reflected as possessed by the Röntgen rays. It seems to us that the experiments of Tesla with his T-tube, allowing him to take simultaneously a sciagraph from reflected and from rays that have passed through different plates of metal, are conclusive in this respect; but they prove that the total amount of reflexion varies not very much for the least and for the best reflectors; the maximum result obtained with zinc (platinum does not seem to have been tested) was only 3 per cent.

Probably platinum will give a higher percentage of reflected rays; but even then it does not sufficiently account for the large difference of efficiency of the ordinary and the focus tube.

According to our theory the real cause of the high efficiency of the focus tube lies in the fact that the kathode rays strike on an actual anode, instead of upon an anode by induction.

This, so far, is not new. To quote Lodge, in an article written some time ago: "Hence, undoubtedly the X-rays do not start from the kathode, or from anything attached to it, but do start from a surface upon which the kathode rays strike, whether it be an actual anode or only an anti-kathodic surface; best, however, if it be an actual anode."

Röntgen and Rowland had discovered the same thing. According to our theory, it is evident that the negatively charged particles can lose their charge sooner and more completely when they strike an actual anode, than when they strike an anode by induction (of greater surface, and thus of smaller density).

Everybody will agree with Prof. Peckham, where he says that the discharge-tube is a resonator for its coil, and when the coil and tube are properly attuned the maximum effect is obtained. We should say, when the discharging capacity of the surface struck by the kathode rays can keep time with the vibrations of the intermittent current or stream of charged particles, the most intense Röntgen rays will be obtained.

If discharging—or, better to say, neutralising—of the waves of negative particles from the kathode be not synchronous with their impinging upon the focus or wall, they cannot lose all their charge, and will proceed either as particles with a minute negative charge, or with a minute positive charge, or perhaps mixed with neutral parts.

The result will be feebler Röntgen rays, and, according to the preponderance of the one or the other particles, these rays will discharge an electrified body, and give it *charge* according to its own charge.

Borgmann found that a negatively charged plate, when exposed to Röntgen rays, lost its charge to become positively charged; when the plate was positively charged, it lost part of its charge.

Righi found just the contrary; a positively charged plate lost its charge to end with a negative charge.

Porter, and nearly all other experimenters, found in all cases a complete loss.

We could explain these differences by admitting that neither Borgmann nor Righi had pure Röntgen rays, but had them mixed with positive and negative rays respectively.

We consider Porter's $X_1$ $X_2$ $X_3$ rays, and Lenard's gamut of rays of more and less magnetic deflexibility, as Röntgen rays of less and more purity, *i.e.* neutrality.

The more perfectly the negative charge has been taken away by the anode—without, however, imparting a positive charge instead—the more intense the Röntgen rays will be, and the stronger penetrating power they will possess.

It is hardly necessary to say that it must be very difficult to obtain perfectly neutral rays; this end will obviously only be attained when the whole system of generating—current, frequency, self-induction, capacity, vacuum, size and form of tube, and all the rest—be in true harmony with one another; and to realise this, means no small thing to do in practice.

We do not venture to say that our theory explains everything, but we do think it explains much; we do not ignore the fact that it is difficult to understand how the Crookes' radiant matter could pass through the glass wall as discharged matter, but the theory of ether motion also presents difficulties. Experiments on the Lenard rays passing as charged particles through aluminium, are described in NATURE of May 27 (p. 93).

Why should the etheric disturbance in the air answer so closely to the vacuum in the tube? Why should the rays, if they be ether vibrations, which in any case must be of so short a wave-length that the well-known properties of light do not show, make any difference whether they are obliged to pass through one or the other metal or material? If the intermolecular space be of any influence to them, one should expect refraction in those materials that show greater resistance to the rays passing.

The strongest proof for our theory is Lafay's experiment, where he found that Röntgen rays, passed through a negatively charged leaf of silver, can again be deflected by a magnet, and in the same direction as the kathode rays in the tube; and when the leaf was positively charged, in the opposite direction.

That means that neutral, non-deflectible rays, after recharging, become again sensitive to the magnet; the deflection being absolutely in accordance with electromagnetic laws of attraction and repulsion.

Unfortunately this experiment, repeated by Lodge, has not been confirmed by him; but it is easy to understand that recharging, just like discharging, is no simple thing to accomplish.

A. VOSMAER.
F. L. ORTT.

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### Some Further Experiments on the X-Rays.

MESSRS. A. VOSMAER AND F. L. ORTT, in a paper kindly sent to me by the editor of NATURE, have arrived at the conclusion that the X-rays are more or less perfectly discharged particles. Others—Sir W. Crookes, for example—have suggested the material nature of the rays, and the only novel point in the paper referred to seems to me to be that the rays owe their greater or less penetrative power to the fact that they are particles less or more free from electric charges. If this were the case, it seems scarcely possible that the particles could pass as freely through, and in the neighbourhood of, a conductor when charged as when uncharged. If the particles were completely without charge, it is true they should be equally affected by a conductor first charged positively, and then with an equal negative charge. On the other hand, if the particles were positively charged, they would experience stronger attraction to a conductor negatively electrified than to the same conductor equally positively charged; and the same, *mutatis mutandis*, may be said if they are negatively charged.

The effect one might expect is that the uncharged particles would at first be attracted by a charged conductor, and then repelled from it, if they acquired part of its charge, in which case the photographic image of the uncharged conductor produced by the X-rays would be modified in intensity if not in form [1]—probably in both.

If, as Messrs. Vosmaer and Ortt suppose, the "rays" are diselectrified by striking against the charged anode inside the tube, it is difficult to see why they should not be re-charged, and therefore act like other charged particles, if they strike against an electrified conductor *outside* the tube, especially if the potential of the external conductor be as great or greater than the potential of the internal. Indeed, the authors of the paper admit this; and if it is true, one might reasonably expect some such action as I have sought for.

I therefore thought it would be, at any rate, worth trying experiments to see if the X-ray photograph of a conductor, such as of a small plate of aluminium (with carefully rounded edges), differs according to (1) whether it be charged or not, and (2) whether it be charged positively or negatively.

According to the paper the X-ray particles are to be considered free from charge when they completely discharge a charged insulated plate, without afterwards imparting to it a charge, and the focus tube I used in all these experiments was one which gave rays of this description.

In the first set of experiments two small squares (A and B) with rounded edges, cut from the same piece of sheet aluminium, and one-thirtieth of an inch in thickness, were arranged in the same horizontal plane beneath the focus tube placed symmetrically with respect to the anode of the tube, so that the line joining the centres of the squares was in a direction at right angles to the line joining the centre of the anode and the centre of the kathode mirror. Below these small squares, and resting on a thick block of paraffin, was placed the photographic plate (all the plates used belonged to the same batch—the Ilford special rapid, and all the plates of each set of experiments were developed together in the same dish). The tube was worked by a large coil, giving six-inch sparks, and A and B were electrified when necessary by wires from the poles of a Wimshurst machine with leydens giving seven-inch sparks between the nobs when used in the ordinary way. The duration of each exposure was timed by a stop-watch in each case, and was as nearly as possible the same for each set of experiments.

A blank experiment in each set, in which the plate, wrapped in dark paper (the same number of folds in every case), was exposed to the radiation from A and B without the Röntgen rays, proved that no photographic effect was produced by their electrification by the Wimshurst.

Exposures were then made as follows:—

(1) A and B both earthed by a wire soldered to a gas-pipe.
(2) A positively, B negatively electrified.
(3) A negatively, B positively electrified.
(4) A positively, B to earth.
(5) A negatively, B to earth.
(6) A and B both earthed.

Development showed that the electrification of A and B was without effect, either absolute or comparative.

Since in the above experiments sparks passed between A and B when their difference of potential exceeded an amount far less than that which could be given by the Wimshurst, and it seemed possible that a stronger charge might still yield some indication of a difference, one of the aluminium squares was removed, and the other shifted till it was immediately beneath the anode, and a second set of experiments was made in a rather different way. In each pair of exposures the same plate was used, each half of the plate being protected, whilst the other half was exposed by a thick slab of plate glass—proved by experiment to allow no developable action of the X-rays to pass through it during the time of exposure used. The experiments were as follows:—

(1) A blank experiment without the X-rays in which one-half of the plate was exposed, first to A charged positively to the full power of the Wimshurst, and then the other half to A charged negatively in the same way. The result showed there was no developable action.

(2) 1st half X-rays only, then an interval of rest (the same interval being allowed between every experiment), then the 2nd half to the X-rays only; this was done to see how the emission of the tube varied.

(3) 1st half A positively charged, 2nd half X-rays only: A to earth.

(4) 1st A negatively. 2nd A to earth.
(5) 1st A positively. 2nd A negatively.
(6) 1st A insulated. 2nd A to earth.
(7) Same as (2).
(8) Same as (1).

The whole of this series was repeated, using the contents of one box of Edwards's isochromatic plates. Development showed no action which could be attributed to the electrification of A.

In a third series of experiments A was connected by a wire first to the kathode loop and then to the anode loop of the focus tube, and radiographs were taken comparing the effects of this treatment with that of earthing A; but these, too, gave no indication of any increase or decrease of the X-rays reaching the plate, nor of any re-distribution of the rays.

In a fourth set the photographic plate was placed on an ordinary discharging table, and brush discharges, and afterwards thick sparks were passed between the poles of the discharger, and the radiographs developed; but they showed no traces whatever of any effect of the sparks.

I, therefore, conclude that the radiograph of a conductor (though it is true I have only tried aluminium and brass) is not sensibly altered by even powerful electrification, nor are the rays altered in force or direction in passing through air in the neighbourhood of a powerfully-charged conductor, nor even through air which is being subjected to a powerful disruptive discharge.

This seems to me to make it more difficult to believe that the X-rays are due to particles, whether totally or partly devoid of charges of positive or negative electricity. T. C. PORTER.

Eton College, July 5.

### Primitive Methods of Drilling.

IN NATURE for June 10 (p. 140) there was an abstract of a monograph upon drills, dealing, among others, with those used in ancient Egypt. May I be permitted to point out that the object shown in Fig. 3 is not, as the author suggests, a drill bow, but a censer.

Again, the meaning of the sign *sam* (explained by Mr. McGuire as a disc drill), when used as a phonetic hieroglyph, is perfectly certain. It means "joining" or "union," and when accompanied by the lotus and papyrus (called "strings" by the author), "union of Upper and Lower Egypt." The figures accompanying it are most certainly gods, and not captives. At the same time, the sign *sam* occurs as a determinative to the word *casanet*, usually translated chisel, but which may well mean a drill such as Mr. McGuire indicates.

Constantinople, July 10. FRANZ CALICE.

### Meteor of July 29.

IN case it may be of any interest to you, I beg to inform you that at 7.45 p.m., yesterday (Thursday, July 29), when standing $0^\circ\ 29'\ 40''$ W., by $51^\circ\ 10'\ 12''$ N. (Sparelands or Willinghurst on 1-inch Ordnance Map, Sheet 285, near Cranleigh) I saw a meteor fall in a direction bearing 46° east of north, as near as I could tell by a bearing subsequently taken. Its appearance was that of a falling magnesium star rocket. It did not appear to explode, but left a long trail of fragments.

Willinghurst, Guildford, July 30. J. V. RAMSDEN.

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[1] The alteration in form, and to a certain extent intensity, would depend partly on the velocity with which the particles were travelling. I do not remember reading of any determinations of the velocity of propagation of the X-rays; but if this remains very high over great distances, as it seems to do, it would appear very unlikely that the rays consist of material particles.

## *THE APPROACHING TOTAL ECLIPSE OF THE SUN.*[1]

### III.

### *The Work proposed for the Indian Eclipse.*

AMONG the work proposed to be carried on during the eclipse of 1896, it may be well imagined that the employment of the prismatic camera occupied a large place, but, unfortunately, the weather allowed no observation to be made by those of 6 inches and 9 inches aperture I took out to Kiö; the instrument employed in Brazil in 1893 was, however, again successful in the hands of Mr. Shackleton in Novaya Zemlya.

With regard to the work I propose to attempt in India, the following extracts from a letter I was called upon to write some time ago, still express my views. These are based upon the results obtained with the prismatic cameras in 1893 and 1896, which, although they are not yet fully worked out, in my opinion far transcend in importance any observations made on the eclipsed sun since 1868:—

"(1) I propose in 1898 to use a prismatic camera with double the present dispersion, although the dispersion employed by me in 1893 and 1896 was, I believe, far beyond anything obtained before.

"The facts are as follows. With the 6-inch prismatic camera used in 1893, the photographed spectrum was 3·1 inches long from D to K. With the 9-inch, which it was proposed to employ in 1896, the corresponding length of spectrum was 3·9 inches; while with the 3-inch prismatic camera actually used in Novaya Zemlya, the spectrum was 2·9 inches long from D to K.

"I believe the next highest dispersion obtained before was by Captain Hills in 1893, and by Dr. Schuster in 1886. Data are not available for exactly comparing the dimensions of the spectra then photographed with those stated above, but they were certainly considerably smaller in both cases.

"The imperative necessity for this increased dispersion may be gathered from the following facts concerning the spectrum of iron which I have best studied, and on which I have thousands of unpublished observations to compare with an eclipse spectrum when we can get one on a sufficient scale.

"Taking Rowland's lines, it may be generally stated that, on an average, one occurs at every 6/10 of a tenth-metre, the unit of measurement generally employed in such matters. With the dispersion in my photographs—the greatest so far obtained, as I have explained—we do not feel ourselves justified in assuming a greater accuracy than 5/10 of a unit. Evidently then, so far as this line of work alone is concerned, we can make no definite statements as to the presence or absence of iron lines in the eclipse photographs.

"So far as I am aware, no observations with the slit spectroscope will enable us to determine with any kind of exactness the relative composition of the successive layers of the sun's gaseous envelope. The difficulty chiefly arises, as I pointed out in 1882, from the fact that we have to deal with the projection of a sphere surrounded by vapours, and not with a section.

"On the other hand, the photographs taken with the prismatic cameras in 1893, and during the last eclipse, show clearly that there are essential differences in the composition of the envelopes at different levels, and the limits of various layers are indicated; but the dispersion is too small to enable us to define the chemical origins of the layers with sufficient certainty. A full statement of the evidence upon this point is included in the report on the results obtained with the prismatic cameras in 1893, which is now in the press.[2]

"(2) The prismatic camera has enabled us to photograph radiations at many different wave-lengths in the spectrum of the corona, differentiating them absolutely from the radiations of the chromosphere and prominences. This is a gigantic advance. But, in the prismatic camera photographs, the indications, except in the case of the 1474 ring, and two or three others, are very dim.

"It is important, therefore, to employ an integrating spectroscope of large dimensions to attempt to get stronger indications of these radiations *by utilising the greater area of the corona*, which of course the prismatic camera cannot do.

"(3) I may say, roughly, that in the (still unpublished) spectrum of the chromosphere obtained in 1893 and 1896, we deal with less than 10 per cent. of the Fraunhofer lines. It is of the first importance, then, to search for the others. I certainly saw some of them in 1882, but a very special inquiry is necessary. This I therefore include in my programme. These lines are certain to be dim, otherwise we should have photographed them already. The tendency of the observations of 1893 and 1896 is to show that they will be found in all probability above the chromospheric layer we have photographed, and associated with the coronal layers, of which we have photographed a few of the brightest radiations.

"The thicknesses of the chromospheric layers have been:—

| | | 1893. | 1896. |
|---|---|---|---|
| H and K ... ... | less than | 5000 miles. | 5000 miles. |
| G ... ... ... | | 3000 ,, | |
| 4471 ... ... | ,, | 3000 ,, | |
| Strontium line 4077 | | 500 ,, | |
| Iron triplet | ,, | 500 ,, | |
| Shortest arcs of Fe, &c. | ,, | 500 ,, | 90 miles. |

"Beyond the dark moon, both in 1893 and 1896, we have indications of luminosity in the prismatic camera photographs, but no final statement can be made as to its origin.

"This gives us the spectrum of a part of the solar atmosphere at a great height:—

1893 ... 22,500 miles to 600,000 miles.
1896 ... 14,000 ,, to a height not yet determined.

"This, therefore, indicates a region, some 10,000 of miles in thickness, to be also explored, and the blank in the photographic evidence so far obtained suggests that eye observations must be employed.

"It will be seen from the above statement that the three parts of the proposed inquiry are all strictly connected, and that to employ any one of them without the others would greatly weaken the attack."

It will have been gathered that the chief object of the above observations is to determine the chemical and physical conditions of that part of the sun's atmosphere just above the photosphere, and therefore including the chromosphere.

Why so much importance is attached to such observations during eclipses, is that ordinary daily observations on the uneclipsed sun, although they carry us far, do not carry us far enough.

The results once obtained are not limited to the sun, they find their application in the study of every star in the heavens; it is, indeed, now recognised that observations of eclipses, such as those made in 1882, 1893, and 1896, provide us with a series of facts with which to approach the question of the absorption phenomena presented by the stars, and the whole question of the classification of stars depends almost absolutely upon their absorption phenomena.

In many of these bodies the atmosphere may be millions of miles high; in each star the chemical substances in the hottest and coolest portions may be vastly different; the region, therefore, in which the absorption takes place which, spectroscopically, enables us to discriminate star from star, must be accurately known before we can obtain the greatest amount of information from our inquiries.

I may say that for some time I was of opinion that in the sun many of the darkest lines indicated absorptions high up in the atmosphere, for the reason that the bright continuous spectrum of the lower levels might have an important effect upon line absorption phenomena by superposing radiation, and so diminishing the initial absorption. The observations of the eclipse of 1893, however, indicate that this opinion is probably only strictly true when the strata of the sun's atmosphere not too high above the photosphere are considered.

If we are justified in arguing from a star with a photosphere as well developed as that of the sun to one in which it is in all probability much less marked in con-

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[1] Continued from page 178.

[2] This has since been published, and I shall refer to it later.

sequence of a much higher temperature, then we must consider that the absorptions which define the various star groups are more conditioned by the temperatures of the hottest regions merely than by the thickness of the absorbing atmospheres, or by the densities of the various vapours. Another consideration to be borne in mind is that if the atmospheres are in part composed of condensable vapours, and not entirely of gases permanent at all stellar temperatures, condensation must always be going on outside in the region of lowest temperature.

Hence it is important to consider the conditions of that part of the sun's atmosphere where it is known beyond all question that certain, but not all, of the absorptions which produce the Fraunhofer lines take place.

In my paper on the eclipse of 1893 (*Phil. Trans.*, 1896, vol. clxxxvii. A, p. 603), I referred at length to this point. The matter is so important that I do not hesitate to quote in the present connection what I then said :

> "As a result of solar spectroscopic observations, combined with laboratory work, Dr. Frankland and myself came to the conclusion, in 1869, that at least in one particular, Kirchhoff's theory of the solar constitution required modification. In that year we wrote as follows (*Roy. Soc. Proc.*, vol. xvii. p. 88) :—
>
> "'May not these facts indicate that the absorption to which the reversal of the spectrum and the Fraunhofer lines are due takes place in the photosphere itself, or extremely near to it, instead of in an extensive outer absorbing atmosphere?'
>
> "In an early observation of a prominence on April 17, 1870, I found hundreds of the Fraunhofer lines bright at the base, and remarked that a 'more convincing proof of the theory of the solar constitution put forward by Dr. Frankland and myself could scarcely have been furnished' (*Roy. Soc. Proc.*, vol. xviii. p. 358).
>
> "During the eclipse of 1870, at the moment of disappearance of the sun, a similar reversal of lines was noticed ; we had, to quote Prof. Young, 'a sudden reversal into brightness and colour of the countless dark lines of the spectrum at the commencement of totality.' On these observations was based the view that there was a region some 2″ high above the photosphere, which reversed for us *all* the lines visible in the solar spectrum ; and on this ground the name 'reversing layer' was given to it.
>
> "Continued observations, however, led me, in 1873, to abandon the view that the absorption phenomena of the solar spectrum are produced by any such thin stratum, and convinced me that the absorption took place at various levels above the photosphere. I need not give the evidence here ; it is set forth in my 'Chemistry of the Sun' (chap. xxii. pp. 303–309). On the latter hypothesis the different vapours exist normally at different distances above the photosphere, according to their powers of resisting the dissociating effects of heat (*Roy. Soc. Proc.*, vol. xxxiv. p. 292).
>
> "My observations during the eclipse of 1882, in the seven minutes preceding totality, to my mind set the matter at rest. 'We begin with one short and brilliant line constantly seen in prominences, never seen in spots. Next another line appears, also constantly seen in prominences ; and now, for the first time, a *longer* and thinner line appears, occasionally noted as widened in spots ; while, last of all, we get, very long, very delicate relatively, two lines constantly seen widened in spots, and another line, not seen in the spark, and never yet recorded as widened in spots' (*ibid.*, vol. xxxiv. p. 297).
>
> "Similar observations in the same part of the spectrum were made by Prof. Turner in 1886 (*Phil. Trans.*, 1889, vol. clxxx. A, p. 391). His observations were made under less favourable conditions than those in Egypt, and in the absence of statements as to the relative lengths of the lines observed, it is impossible to utilise them fully.
>
> "This is one of the most important points in solar physics, but there is not yet a consensus of opinion upon it. Prof. Young and others, apparently, still hold to the view first announced by Dr. Frankland and myself in the infancy of the observations, that the Fraunhofer absorption takes place in a thin stratum, lying close to the photosphere."

I next proceeded to discuss the numerous photographs obtained during the eclipse, and I gave a map showing that there was only the slightest relation between the intensities of the lines common to the Fraunhofer and the eclipse spectrum, and, further, that only a few of the Fraunhofer lines are represented at all. Not only this, but in the eclipse photographs there are many bright lines not represented at all among the Fraunhofer lines.

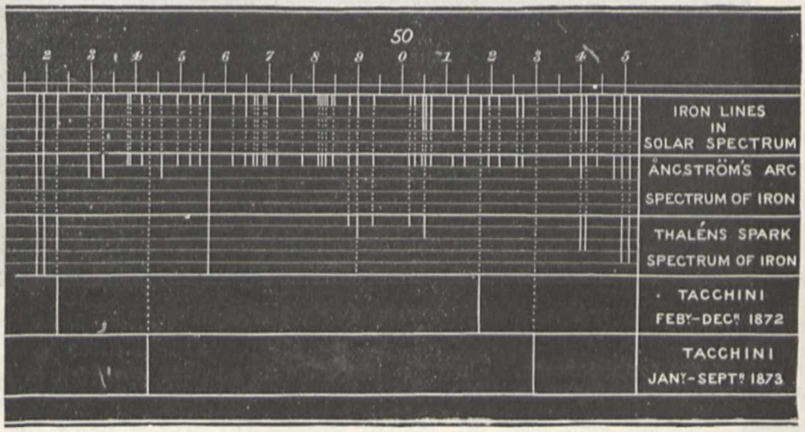

FIG. 10.—Tacchini's observations of the iron lines at 4924·1 and 5018·6 on the spectrum of the quiet chromosphere in 1872. It will be observed that new prominence lines were recorded when the iron lines disappeared.

The chromosphere, then, is certainly not the origin of the Fraunhofer lines, either as regards intensity or number. From the eye observations made since 1868, I pointed out many years ago that there is evidence that the quiescent chromosphere spectrum indicates a higher temperature than that at which much of the most valid absorption takes place ; in other words, that the majority of the lines associated with lower temperature are produced above the level of the chromosphere ; while the eclipse photographs of 1893 and 1896 afford evidence by the greater length of some of the lower temperature lines that we need not locate the region which produces them at any great height above the chromosphere.

The solar evidence, then, is that most of the line absorption is produced in, and not very far above, the chromosphere. This is a conclusion we are bound to accept in a discussion of the origin of stellar absorption in the absence of evidence to the contrary. We have no right to assume that the absorption will be produced at the top of the atmosphere in one star, and in the bottom in another, when the atmospheres are once relatively quiescent.

Quite recent work has very greatly strengthened these conclusions in regard to the sun. The conclusion with regard to the high temperature of the quiescent chromosphere depended chiefly upon the Italian observations and upon investigations communicated by myself to the Royal Society in 1879 (*Roy. Soc. Proc.*, 1879, vol. xxx. p. 22), and 1881 (*ibid.*, 1881, vol. xxxii. p. 204), on the effect of high-tension electricity on the line spectra of metals (Fig. 10).

These investigations consisted in noting (1) the lines brightened in passing a spark in a flame charged with metallic vapours, and (2) the lines brightened on passing from the arc to the spark. It was found, in the case of iron, that two lines in the visible spectrum at 4924·1 and 5018·6, on Rowland's scale, were greatly enhanced in brightness with higher temperatures, and were very important in solar phenomena. Thus the line at 4924·1 was at times the only representative of iron in the chromosphere; upwards of 1000 lines in the visible spectrum of iron gave no sign.

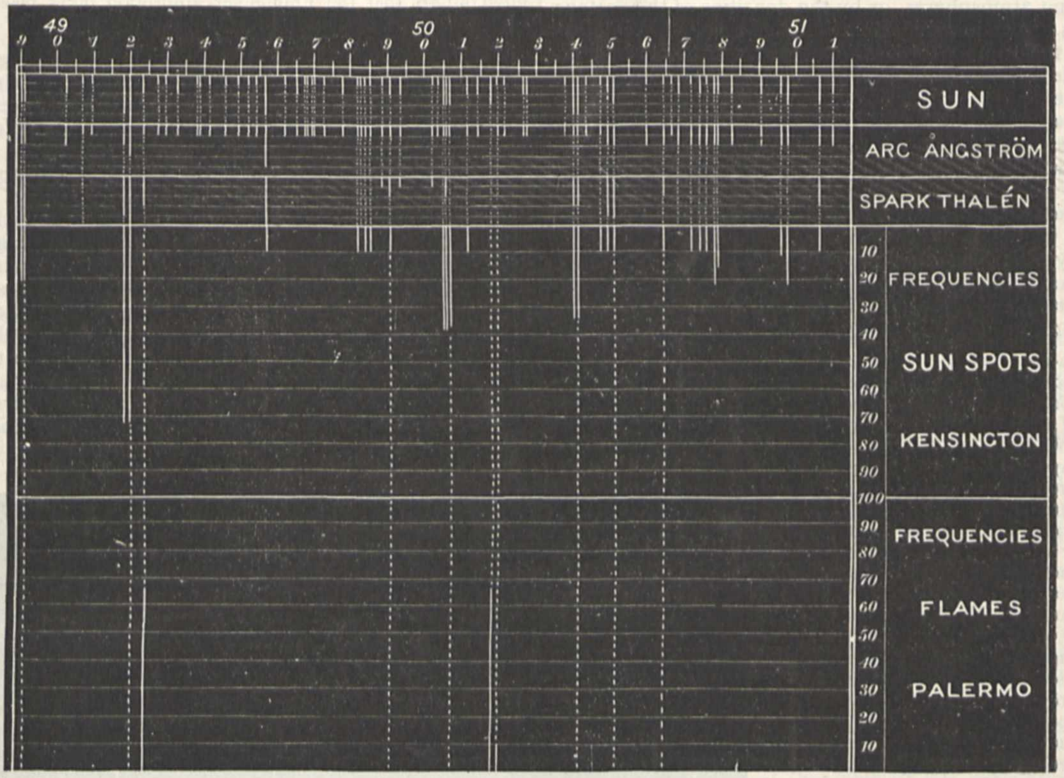

FIG. 11.—Iron spot-lines seen at Kensington confronted with iron prominence lines seen at Palermo.

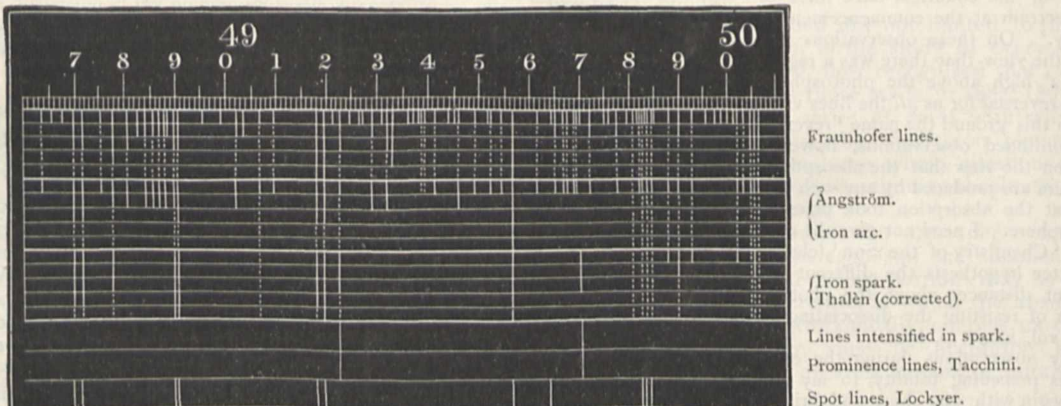

FIG. 12.—The line seen during the Solar Eclipse in 1882, showing that the prominence line at 4924·1 was seen short and bright seven minutes before totality, and some of the spot lines were not seen till two minutes before totality, and then they were observed long and dim.

Spots are conceded to be cooler phenomena than those observed in other parts of the photosphere, so we should not expect to find the hotter lines so frequent in them. Fig. 11 gives the facts. It is a comparison of the iron lines observed in spots, and in the quiet chromosphere during the same time in 1872, the spots being observed at Kensington, the chromosphere at Palermo. It will be seen that none of the lines seen in the chromosphere were seen in the spots, and *vice versâ*.

It was natural to suppose that the iron vapour producing the cooler lines was higher up than that responsible for the enhanced line at 4924·1. Hence a crucial observation was planned for the eclipse of 1882. If the vapour were higher it should be dimmer, and its lines should, if seen at all, be seen long and faint very near the beginning of totality, while the hotter line, being produced by vapours relatively low and at a higher temperature, should be seen short and bright some time before the beginning of totality.

Fig. 12 will show how absolutely the prediction was verified by the event.

J. NORMAN LOCKYER.

(*To be continued.*)

## ON LUNAR AND SOLAR PERIODICITIES OF EARTHQUAKES.

THE investigation of small periodical changes is rendered difficult chiefly by the doubt which so often exists, whether the results obtained by the ordinary methods are due to accident or prove some real periodically acting cause. Attention need only be drawn to the many calculations which have been made to trace the sunspot period in terrestrial phenomena, such as rainfall or temperature, to show that widely different conclusions may be drawn from the same evidence according as greater or smaller value is attached to the element of chance. I have been engaged for some time to apply the theory of probability to investigations of this nature, with a view if possible to being able in every instance to assign a definite number to the probability that any periodicity which may be found in the record of some physical phenomenon is of an accidental nature. In a paper recently communicated to the Royal Society, I have applied the results obtained to the periodicities of earthquakes. Mr. Knott (*Proc. Roy. Soc.*, vol. lx. p. 457) has recently published some investigations, conducted with skill and labour, which in his judgment were favourable to the existence of a true lunar influence on earthquakes. The theory of probability, however, does not support that view. The number of earthquakes treated by Mr. Knott is 7427, and Fourier's series is applied to determine the amplitudes of the changes which have periods coincident with the lunar day, the half-day and the third or fourth part of a lunar day. The method employed would always give some results whether a true periodicity existed or not, and I have calculated what the average amplitudes would be if earthquakes were distributed quite at random. These amplitudes depend, of course, on the number of events taken into the calculation, and are found to vary inversely as the square root of that number. The following table will show how the amplitudes found by Mr. Knott compare with those calculated by the theory of probability. $C_1$ in the table refers to the lunar day, while $C_2$, $C_3$ and $C_4$ refer to its submultiples.

| Coefficients | $C_1$ | $C_2$ | $C_3$ | $C_4$ |
|---|---|---|---|---|
| Expectancy for the coefficients | 19·3 | 15·7 | 10·6 | 5·2 |
| By the theory of probability... | 10·3 | 17·9 | 10·9 | 3·97 |

As it may further be shown that cases will frequently occur where the amplitudes found are equal to twice the expectancy, the table may be considered as conclusive that if a lunar effect exists, it must be so small that it is quite hidden by accidental effects. For the present, at any rate, the evidence is against such a lunar influence. A discussion of the periods, coincident with the lunar months, leads to the same conclusion.

It is otherwise with the annual and daily periods, which have recently been discussed by Mr. Davison. Here the amplitudes found are decidedly too large to be due to accident; and we may therefore say, with a degree of probability amounting practically to certainty, that there is a yearly period giving a maximum of earthquakes in December, and a daily period giving a maximum some time between ten o'clock in the morning and noon.

ARTHUR SCHUSTER.

## NOTES.

A SMALL committee has been appointed by the Treasury "to consider and report upon the desirability of establishing a National Physical Laboratory for the testing and verification of instruments for physical investigation, for the construction and preservation of standards of measurement, and for the systematic determination of physical constants and numerical data useful for scientific and industrial purposes, and to report whether the work of such an institution, if established, could be associated with any testing or standardising work already performed wholly or partly at the public cost." The following will be the members of the committee:—The Lord Rayleigh, F.R.S. (chairman), Sir Courtenay Boyle, K.C.B., Sir Andrew Noble, K.C.B., F.R.S., Sir John Wolfe Barry, K.C.B., F.R.S., Prof. W. C. Roberts-Austen, C.B., F.R.S., Mr. Robert Chalmers, of the Treasury, Prof. A. W. Rücker, F.R.S., Mr. Alexander Siemens, Dr. T. E. Thorpe, F.R.S.

AT a meeting of the Royal College of Physicians of London, on Thursday last, the College, on the recommendation of the Council, awarded the Moxon gold medal to Sir Samuel Wilks, F.R.S., the President of the College, for having especially distinguished himself by observation and research in clinical medicine, and the Baly medal to Prof. Schäfer, F.R.S., for having especially distinguished himself in the science of physiology. The Harveian oration will be delivered by Sir William Roberts on October 18, St. Luke's Day. Dr. E. Markham Skerritt, of Bristol, will give the Bradshaw lecture on November 4; and Dr. F. Pavy will deliver a special lecture, supplementary to the Croonian lectures, delivered in 1894, on November 11. The following were announced as lecturers for next year:—Goulstonian lectures, Dr. John Rose Bradford; Lumleian lectures, Sir Richard Douglas-Powell; Croonian lectures, Dr. Sidney Martin.

A PARLIAMENTARY paper has just been issued giving an additional Civil Service Estimate, amounting to 10,000*l.*, for Art and Science buildings of Great Britain. The total original net estimate for 1897-98 was 26,000*l.*, and this has been increased to 36,000*l.* The British Museum and the Science and Art Department buildings receive 5000*l.* each for new works, alterations, and additions. The increased grant to the Science and Art Department is on account of the cost of carrying out certain urgent works and services at South Kensington, designed to give effect to the recommendations made in the first report from the Select Committee on the Museums of the Department. It is proposed to remove the more dangerous buildings on the east side of Exhibition Road (including the "boilers" and the electric lighting plant), to displace the occupants of the official residences, and reconstruct the entrance to the galleries on the west side of Exhibition Road.

THE Weights and Measures Bill (Metric System), the scope of which has already been described (p. 275), was read a third time and passed in the House of Lords on Friday last.

WE notice with regret that the Hon. Ralph Abercromby, who did so much for the advancement of meteorological science, died at Sydney, New South Wales, on June 21, at fifty-four years of age.

It is announced in *Die Natur* that Prof. Rudolf Leuckart, the Nestor of German zoologists, has been made a knight of the Order pour le mérite in science and art by the German Emperor. The same distinction has been conferred upon Prof. Karl Neumann, professor of mathematics at Leipzig.

Mr. S. P. Langley, Secretary of the Smithsonian Institution, was present at the meeting of the Paris Academy of Sciences on Monday. In the course of a few remarks upon his experiments in mechanical flight, which he made at the invitation of the President, Mr. Langley said he had greatly enlarged the distance which his aeroplanes would run, without much altering his apparatus. He will shortly publish a detailed account of all he has done upon the subject.

What is believed to be the largest Land Tortoise now living in the world has been lately deposited in the Zoological Society's Gardens by Mr. Walter Rothschild. It is about 4 feet 7 inches in total length, and 2 feet 10 inches in breadth, and weighs about 5 cwt. As regards weight, however, the animal is in poor condition and will probably "put on flesh" when carefully fed. This tortoise belongs to the species called *Testudo daudini* by Duméril and Bibron, which is figured on the fifth plate in Dr. Günther's well-known memoir on Gigantic Land Tortoises. It was originally brought from the Aldabra Islands in the Indian Ocean, though it is said to have been kept elsewhere in captivity for the last 150 years.

The King of Siam, who arrived in this country on Friday last, paid a visit to the Kew Gardens on Sunday morning, his son, Lord Harris, and several members of the Royal suite accompanying him. The party was received by Mr. Thiselton-Dyer, and afterwards spent some time in the inspection of the various houses and conservatories. His Majesty throughout displayed the keenest interest in the ferns, orchids, and rich variety of Oriental and tropical plants. Many of these he recognised and admired, and noted from time to time that rare specimens of other places were in his own country grown in the open air. He compared the vast and varied collection of Kew with the famous gardens at Buitenzorg, in Java, with which he is familiar, and told Mr. Thiselton-Dyer that his visit was only a preliminary one, and that he should look forward to another occasion when he might inspect the treasures of Kew at greater leisure. After expressing his thanks to the director the King left the gardens. The King and his son and some members of the suite also paid a visit to Greenwich Observatory on Sunday.

The annual meeting of the Iron and Steel Institute was opened at Cardiff on Tuesday.

At a recent meeting of the Council of the Australasian Association for the Advancement of Science, letters were read which had been received from the Royal Society of Tasmania, the Royal Geographical Society (Melbourne branch), and the Medical Society of Queensland, suggesting certain means for permanently recording the services to science of the late Baron von Müller, and, in view of the importance of the subject, it was resolved to defer its consideration till the next meeting. The acceptance of the secretaryship of the Section of Economic Science and Agriculture, by Mr. R. R. Garran, was communicated; also that of Mr. H. C. Kent, of the Section of Engineering and Architecture; and that of Dr. F. Tidswell, of the Section of Sanitary Science and Hygiene. Drs. J. W. Springthorpe (Melbourne), D. Hardie (Queensland), and J. Ashburton Thompson forwarded their acceptances of the office of vice-president of this last Section. On the motion of Mr. G. H. Knibbs, it was resolved that Profs. Liversidge, Threlfall, Haswell, Mr. H. C. Russell, and Mr. Deane be deputed to form a preliminary committee to consider the question of appointing a reception committee to arrange for evening lectures, conversazione, a concert, excursions for scientific and other purposes, visits to works, garden parties, conferences, &c., and for the entertainment of members during the week of session. A list of papers promised was laid on the table, and the Secretary reported that the promises of scientific contributions to the various Sections were coming in most satisfactorily.

For some little time past there has been a local agitation in favour of opening Kew Gardens to the public in the mornings of week-days. Two deputations waited upon Mr. Akers-Douglas, First Commissioner of Works and Buildings, on Friday last, to urge the desirability of the earlier opening of the gardens. Mr. Akers-Douglas, however, while promising to give the views of the deputations careful consideration, held out no hope of their request being complied with. He said the *raison d'être* of the existence of Kew Gardens was the valuable scientific work it did, and he could not be expected to do anything in the way of extending the hours during which the gardens were open to the general public if it would interfere with that work. The financial question did not weigh with him at all, for if he were convinced that the interests of science would not suffer by the earlier opening he should endeavour to persuade the Treasury to grant any extra money required. The sole question for consideration was whether the interests of science could be combined with the desire of the people for the earlier opening, and he regretted to say that the scientific men whose opinions he had obtained were entirely opposed to the proposal. From a scientific point of view the experiment had not been a success in Edinburgh, and they had no reason to anticipate any better result at Kew.

The current number of the *Annales de l'Institut Pasteur* contains two very important communications on the etiology of yellow fever, the one by Dr. Sanarelli of Montevideo, and the other by Dr. Havelburg of Rio de Janeiro. Some months ago Dr. Sanarelli gave a lecture in which he reviewed his researches on this subject, a description of which appeared in our issue of July 15; his present communication is a detailed account, occupying eighty pages and elaborately illustrated, of his investigations, and he purposes to continue their recital in a second memoir at a later stage of his inquiries. Dr. Havelburg has worked quite independently of Sanarelli, and has also succeeded in isolating a micro-organism which he holds responsible for yellow fever. He, moreover, places this microbe in the group of coli and typhoid bacilli, and regards it as a form intermediate between these microbes and those associated with hæmorrhagic septicæmia, to which it also bears some resemblance. Cultures of both Sanarelli's and Havelburg's yellow fever microbes have been forwarded by their respective discoverers to the Pasteur Institute in Paris, and are being there submitted to a careful examination to establish their identity or difference. Havelburg mentions that he was able to immunise a dog against yellow fever infection so that its serum protected guinea-pigs from this disease. He hopes to extend his researches in this direction.

Prof. T. R. Fraser, F.R.S., whose experiments on immunisation against serpents' venom, and the treatment of snake-bite with antivenene, have been fully described in these columns (vol. liii. p. 569, 1896), has made another contribution to this subject. His experiments showed, among other things, that, when introduced into the stomach of an animal, serpents' venom produces no obvious injury, even when the quantity is so large as to be sufficient to kill 1000 animals of the same species and weight if the venom were injected under the skin. An investigation of this remarkable fact has now proved that the cause is to be found in the bile, which has such a decided influence upon serpents' venom that it is sufficient in itself to account for the innocuousness of stomach administration. It is shown that the bile of venomous serpents is able, when mixed with the venom of serpents, to prevent lethal doses of the latter from producing death; and that the bile is indeed so powerful an agent in doing this, that a quantity actually smaller than the quantity of venom may be sufficient for the purpose. It need scarcely be added that the doses of bile thus shown to be sufficient represent only minute portions of the bile stored in the gall-bladder of a serpent, and that a serpent, therefore, has at its disposal enough of bile to prevent injury from venom introduced into the stomach in quantities many times greater than the minimum lethal dose. The bile of other animals, such as the ox, rabbit, and guinea-pig, also possesses this anti-venomous property, but in a smaller degree than that of venomous serpents. Prof. Fraser has isolated the antidotal constituent from the bile of venomous serpents, and an experiment with this substance not only supplies strong confirmation of the evidence that bile is able to render serpents' venom inert, but also suggests that from bile there may be produced an antidote for snake-poisoning, which, in its antidotal value, is at least equal to the most powerful antivenene or antivenomous serum as yet obtained from the blood of immunised animals.

THE Porthcawl Urban District Council required a water supply; and to the majority of the members it appeared that a water diviner was the most competent man to inform them how to obtain their desire. A specialist, for whom water had strong attractions, was thereupon engaged, though more than one half of the ratepayers opposed this action, thus showing that they were more enlightened than their elect. However, a total expenditure of about 800*l.* was made in connection with the water supply, and apparently upon the recommendations of the water diviner. Nothing might have been known of this scheme outside Porthcawl but for the inquiry which the Local Government Board holds in the case of such expenditure. As the result of the inquiry, however, the water finder's fees have been disallowed on the ground that they had been illegally paid to a person supposed to be possessed of supernatural powers, and upon whose advice several hundred pounds had been spent, which action involved waste of public money. The whole of the expenditure was therefore disallowed, less 250*l.* allowed as a loan for experimental purposes. The Council has thus to pay dearly for its experience.

THE *Annales* of the Central Meteorological Office of France for 1895 contain a valuable discussion of the rainfall of Western Europe, by M. A. Angot. Next to temperature, rainfall is the most important meteorological element, but owing to the difficulty that the subject presents, it has only been seriously attacked in a few cases, the principal exceptions in Europe being for the British Islands, Russia, and the Iberian Peninsula. The influence of topographical conditions and the varying amount of rainfall make it necessary to deal with a great many stations, and a considerable number of years. It is also essential that the data should be for the same years, otherwise the rainfall of a relatively dry period at one station may be compared with a relatively wet one at another station. M. Angot has discussed the monthly and seasonal values at 275 stations for the thirty years 1861–1890. At many of the stations a longer series was available, but at some it has been necessary to reduce a shorter period to the longer one by use of coefficients. The values are given both in a tabular form and upon charts, and a short discussion for each month explains the principal features of the rainfall. Space will not allow us to enter into any of the interesting details shown by the monthly and seasonal charts. The yearly chart shows that the dryest regions, where the rainfall is less than 20 inches, are few in number and small in extent. Outside the Iberian Peninsula there are only five such regions: the south-east coast of Sweden, two in Bohemia, and two in France. In Spain there are several large areas where the fall does not reach that amount, the minimum value being 11·3 inches at Salamanca. Smaller values are recorded in Russia, but this is outside the area of the charts. The greatest amounts are found in Scotland, Cumberland, the southern slopes of the Alps, and the mountainous region of Upper Austria, where the falls exceed 78 inches.

A CORRESPONDENT sends us an extract from a Melbourne paper, *The Age*, for March 13, on the subject of weather forecasting in Australia, which, in the light of Mr. Eliot's recent article in these columns, is of great interest. The following is a brief summary of the communication in question:—Since Christmas the extraordinary weather that has been experienced in Australia has created more than usual interest in meteorological affairs. There have been three falls of snow in the Victorian Alps within the last three months, and at the time of writing (March) Mounts Bogong and Feathertop were covered with snow to a depth of six feet. Mr. Baracchi stated recently that the bad weather of the last few days was caused by the "backing of the Antarctic depression, which has been arrested in its eastward course by suction at the equator." He may be correct, for he has "carefully, as we have seen, kept the data on which his opinion is founded to himself, but it seems somewhat unusual and unlikely." Mr. Russell, of New South Wales, attributes the cold weather to an extensive high pressure of great energy which has come in almost a direct line from West Australia. The writer of the article previously suggested that a probable cause of the unreasonable weather was the unwonted number of icebergs and ice-fields in the Southern Ocean, for almost every ship that hailed from the Cape passed through hundreds of miles of ice.

IN some years the pack-ice and icebergs from Victoria Land drift in vast quantities more northwards than in others, and the cause of their abundance this year is, according to the U.S. Hydrographic Department, the great volcanic disturbances which have broken up the polar sea and set it adrift. Mr. Russell's explanation is that we owe their presence to the long continuance of southerly and south-westerly winds; but this explanation does not account for the *cause* of such winds. It is, however, "only reasonable to conclude that there is an intimate association between the cold weather and the icebergs." Usually the air blows over a large expanse of open ocean, which warms it before it approaches the coast of Australia; but in the present year they were chilled by blowing over vast fields of ice and icebergs. It would be interesting, however, to examine carefully the meteorological observations made on ships during their voyages, and it is suggested that "our meteorologists would act wisely were they to imitate the example of the American authorities, who offer inducements to shipmasters for such information."

MR. THOS. MEEHAN records ("Contributions to the Life-Histories of Plants," xii., *Proc. Acad. N.S. Philadelphia*) a case of cleistogamy in an umbelliferous plant, *Cryptænia canadensis*. This species, like so many others of the same order, produces two kinds of flowers in the same inflorescence. The outer florets are male, and possess showy corollas, whilst the inner flowers are hermaphrodite, possessing both stamens and pistil concealed in a small fugacious corolla. Fertilisation takes place in the unopened buds, and thus the process may be legitimately regarded as one of cleistogamy, although the degeneration of floral structure, usually a concomitant feature, is relatively little apparent. There can exist but little doubt that the (assumed) function of the outer sterile showy florets as serving to attract insect visitors proved to be imperfectly discharged, and that a ruinous situation has only been saved by a recourse to self-fertilisation, which is undergoing evolution along cleistogamic lines.

THE U.S. Department of Agriculture, Division of Botany, Washington, has just issued as one of the series of "Contributions from the U.S. National Herbarium," "Notes on the Plants used by the Klamath Indians of Oregon," by F. V. Coville. The author, while engaged in a botanical survey of the plains of south-eastern Oregon in the summer of 1896, spent three days at Fort Klamath and the Klamath Indian Agency, from whence he was enabled to secure information as to the principal plants used by the Klamath Indians. The notes are now issued with a view to their use by others in securing fuller and more varied data about the aboriginal uses of plants by this tribe. The author hopes, when the necessary material has been collected, to prepare a comprehensive paper on the subject. The present list includes a few plants which give suggestions of usefulness in the arts and industries of others besides the Indians, among which may be mentioned the yellow pine-lichen, which produces a beautiful canary-yellow dye; the Rocky mountain flax, which furnishes a strong, fine fibre; and several of the tuberous-rooted perennials of the parsley family, which make palatable and nutritious foods.

WE have upon our table the following new editions:—"Elemente der Geologie," by Dr. Hermann Credner (Leipzig: Wilhelm Engelmann.) This is an eighth revised edition of a work which first appeared twenty-five years ago. It is a bulky volume of eight hundred pages, illustrated by more than six hundred woodcuts. As a handbook of general geology the work has found considerable favour in Germany.—"Physikalisches Praktikum," by Eilhard Wiedemann and Hermann Ebert. Third revised and enlarged edition (Brunswick: F. Vieweg and Son.) In this work the principles of physics are described with special reference to physico-chemical methods. The volume should be in the hands of every student of physical chemistry familiar with the German language. It is a book both for physical and chemical laboratories, and it contains a course of practical work which should be completed by every student before he begins the study of chemistry.—Messrs. Macmillan and Co. have published the first volume of the sixth edition, revised and enlarged, of "The Elementary Part of a Treatise on the Dynamics of a System of Rigid Bodies," by Dr. E. J. Routh, F.R.S. The dynamical principles of the subject are given in this first volume, together with the more elementary applications, the more difficult theories and problems being reserved for the second volume. Many additions and improvements have been made in the work.—Messrs. Hodder and Stoughton have published a cheap edition (twenty-fifth thousand) of the late Dr. Drummond's "Lowell Lectures on the Ascent of Man."

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*, ♀) from India, presented by Mr. J. Fleming; a Macaque Monkey (*Macacus cynomolgus*, ♂) from India, presented by Mr. G. H. Cheverton; a Wood Brocket (*Cariacus nemorivagus*, ♀) from Buenos Ayres, presented by Mr. C. Passingham; a Purplish Death Adder (*Pseudechis porphyriacus*), a Brown Death Adder (*Diemenia textilis*), a Shielded Death Adder (*Notechis scutatus*) from Australia, presented by Mr. E. H. Bostock; two Grooved Tortoises (*Testudo calcarata*) from South Africa, deposited; a Green-billed Toucan (*Ramphastos discolorus*) from Guiana, purchased; three Bar-tailed Pheasants (*Phasianus reevesi*), three Amherst Pheasants (*Thaumalea amherstiæ*) bred in the Gardens.

## OUR ASTRONOMICAL COLUMN.

THE PERIODIC COMET D'ARREST.—M. Gustave Leveau publishes in the *Astronomischen Nachrichten* (No. 3434) a more accurate ephemeris for D'Arrest's comet than has yet appeared. It may be mentioned that the comet has not only been observed by Mr. Perrine at the Lick Observatory, but also at the observatories of Algiers and Toulouse. By employing these observations the following ephemeris has been computed.

*Ephemeris for Paris Mean Midnight.*

| 1897 | | α<br>h. m. s. | | δ<br>° ′ |
|---|---|---|---|---|
| Aug. 5 | ... | 3 35 13 | ... | +5 43·8 |
| „ 7 | ... | 39 10 | ... | 34·4 |
| „ 9 | ... | 43 0 | ... | 24·3 |
| „ 11 | ... | 46 43 | ... | 13·5 |
| „ 13 | ... | 50 18 | ... | 2·0 |
| „ 15 | ... | 53 46 | ... | 4 49·9 |
| „ 17 | ... | 3 57 5 | ... | 4 37·1 |

NATAL OBSERVATORY REPORT.—Mr. Nevill, the Government Astronomer, in his report to the Colonial Secretary for the year 1896, points out that although he has been able with his small staff to cope with the ordinary routine work of the observatory, there is still a great amount of useful and important work which has accumulated, and which awaits printing and publishing. Among these is a valuable contribution to the theory of the moon, which consists of the reduction of all the observations of the moon to a uniform basis and their comparison with the portion of Hansen's tables, whose theoretical accuracy is undoubted. From the difference between these two results must be determined the approximate value and probable period and character of all the terms of long period and their associated terms of shorter period, which are indicated by the observations and shown by the comparison to be required to be added to the known portion of the complete theory in order to enable it to properly represent the motion of the moon. A second piece of work, which is far advanced, consists of a simple and powerful method of calculating from theory the values of such terms as those mentioned above, and to apply them to the computation of the exact values of all the terms shown to exist by the comparison between theory and observation. It may be mentioned that both these investigations have been in progress for some years, and now that they are so near completion their publication should not be delayed. The report contains a detailed account of all the meteorological observations made at the observatory for the past year, in addition to tables of "mean values" for the past eleven years.

CHRONOMETERS.—Those who are specially interested in the performance of chronometers will find Prof. Raoul Gautier's report on the "Concours International de réglage pour chronomètres de poche de haute précision," which was presented last year, a very interesting pamphlet. The object of this competition was to examine in every respect the performance of chronometers sent by standard makers, and to award prizes to those which behaved best under the given conditions. The marks were divided into four groups: 100 points for mean daily error, this must not *exceed* ± 0·75s.; 100 for mean error of position, which must not exceed ± 2·5s.; 100 for error of compensation, which must be under ± 0·20s.; and 50 for "reprise de marche," which was not allowed to exceed 5·00s. Of the 142 chronometers examined 12 exceeded 256·7 points, while 32 obtained over 233·3. The most highly rewarded "series of three chronometers" obtained in the mean 271·5 points, one of these, No. 298,225, having received 284·7 points. For further details regarding the actual errors measured, we must refer the reader to the original pamphlet, where he will find all the required information minutely discussed and tabulated.

NEW DETERMINATION OF PRECESSIONAL MOTION.—The *Astronomical Journal* (vol. xvii. No. 21) contains a new determination of the constant of precession which was undertaken by Prof. Simon Newcomb at the request of a conference held in Paris in 1896. This conference was brought together to discuss a report on what was to be considered the best system of fundamental stars to be adopted for international use. Prof. Simon Newcomb was chosen to represent this part of the inquiry, and his work having now been accomplished, he publishes, previous to his official communication, a brief abstract of the results at which he has arrived. The discussion, he tells us, was beset with difficulties, the most troublesome being the parallactic motion of the stars arising from the solar motion. Another difficulty was the fact that the proper motions of the stars did not follow the normal or exponential law of error on which the method of least squares and the practice of taking them are based. Prof. Newcomb divided the work into four parts, employing what he calls the statistical method, the method of individual stars, the method of zones and regions, and a method of which the parallactic motion is eliminated. The results may be briefly summed up in the following table.

| | Struve-Peters. | New. | Diff. |
|---|---|---|---|
| | " | " | " |
| General precession | 5025·24 ... | 5024·53 ... | – 0·71 |
| Luni-solar ,, ... | 5038·23 ... | 5036·84 ... | – 1·39 |
| Value of 100 m. ... | 4607·65 ... | 4607·11 ... | – 0·54 |
| ,, 100 n. ... | 2005·64 ... | 2005·11 ... | – 0·53 |

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## *DIAMONDS.*[1]

IT seems but the other day I saw London in a blaze of illumination to celebrate Her Majesty's happy accession to the throne. As in a few days the whole empire will be celebrating the Diamond Jubilee of our Queen, who will then have reigned over her multitudinous subjects for sixty years, what more suitable topic can I bring before you than that of diamonds! One often hears the question asked: "Why *Diamond* Jubilee?" I suppose it is a symbol intended to give a faint notion of the pure brilliancy and durability of the Queen's reign; and in thus associating Her Majesty with the precious diamond, to convey an idea of those noble qualities, public and private, which have earned for her the love, fealty, and reverence of her subjects.

From the earliest times the diamond has occupied men's minds. It has been a perennial puzzle—one of the riddles of creation. The philosopher Steffans is accredited with the dictum that, "Diamond is quartz which has arrived at self-consciousness!" and an eminent geologist has parodied this metaphysical definition, saying, "Quartz is diamond which has become insane!"

Prof. Maskelyne, in a lecture "On Diamonds," thirty-seven years ago, in this very theatre, said: "The diamond is a substance which transcends all others in certain properties to which it is indebted for its usefulness in the arts and its beauty as an ornament. Thus, on the one hand, it is the hardest substance found in nature or fashioned by art. Its reflecting power and refractive energy, on the other hand, exceed those of all other colourless bodies, while it yields to none in the perfection of its pellucidity"; but he was constrained to add, "The formation of the diamond is an unsolved problem."

Recently the subject has attracted many men of science. The development of electricity, with the introduction of the electric furnace, has facilitated research, and I think I am justified in saying that if the diamond problem is not actually solved, it is certainly no longer insoluble.

### GRAPHITE.

Intermediate between soft carbon and diamond come the graphites. The name graphite is given to a variety of carbon, generally crystalline, which in an oxidising mixture of chlorate of potassium and nitric acid forms graphitic acid easy to recognise. Graphites are of varying densities, from 2·0 to 3·0, and generally of crystalline aspect. Graphite and diamond pass insensibly into one another. Hard graphite and soft diamond are near the same specific gravity. The difference appears to be one of pressure at the time of formation.

Some forms of graphite exhibit a remarkable property, by which it is possible to ascertain approximately the temperature at which graphites were formed, or to which they have subsequently been exposed. Graphites are divided into "sprouting" and "non-sprouting." When obtained by simple elevation of temperature in the arc or the electric furnace they do not sprout; but when they are formed by dissolving carbon in a metal at a high temperature, and then allowing the graphite to separate out on cooling, the sprouting variety is formed. One of the best varieties is that which can be separated from platinum in ebullition in a carbon crucible. The phenomenon of sprouting is easily shown. Place a few grains in a test-tube and heat it to about 170° C., when it increases enormously in bulk and fills the tube with a light form of amorphous carbon.

The resistance of graphite to oxidising agents is greater the higher the temperature to which it has previously been exposed, Graphites, which are easily attacked by a mixture of fuming nitric acid and potassium chlorate, are rendered more resistant by strong heat in the electric furnace.

I will now briefly survey the chief chemical and physical characteristics of the diamond, showing you by the way a few experiments that bear upon the subject.

### COMBUSTION OF THE DIAMOND.

When heated in air or oxygen to a temperature varying from 760° to 875° C., according to its hardness, the diamond burns with production of carbonic acid. It leaves an extremely light ash, sometimes retaining the shape of the crystal, consisting of iron, lime, magnesia, silica, and titanium. In boart and carbonado the amount of ash sometimes rises to 4 per cent., but in clear crystallised diamonds it is seldom higher than 0·05 per cent. By far the largest constituent of the ash is iron.

The following table shows the temperatures of combustion in oxygen of different kinds of carbon:—

| | °C. |
|---|---|
| Condensed vapour of carbon ... ... .. | 650 |
| Carbon from sugar, heated in an electrical furnace ... ... ... ... ... ... | 660 |
| Artificial graphites, generally ... ... ... | 660 |
| Graphite from ordinary cast-iron ... ... ... | 670 |
| Carbon from blue ground, of an ochrey colour ... | 690 |
| ,, ,, ,, very hard and black .. | 710 |
| Diamond, soft Brazilian ... ... ... ... | 760 |
| ,, hard Kimberley ... ... | 780 |
| Boart from Brazil ... ... ... ... ... | 790 |
| ,, from Kimberley ... ... ... ... | 790 |
| ,, very hard, impossible to cut ... ... | 900 |

At the risk of repeating an experiment shown so well at this table by Prof. Dewar, I will heat a diamond to a high temperature in the oxyhydrogen blowpipe and then suddenly throw it in a vessel of liquid oxygen. Notice the brilliant light of its combustion. I want you more especially to observe the white opaque deposit forming in the liquid oxygen. This deposit is solid carbonic acid produced by the combustion of the carbon. I will lead it through baryta water, and you will see a white precipitate of barium carbonate. With a little more care than is possible in a lecture I could perform this experiment quantitatively, leading the carbonic acid and oxygen, as they assume the gaseous state, through baryta water, weighing the carbonate so formed, and showing that one gramme of diamond would yield 3·666 grammes of carbonic acid—the theoretical proportion for pure carbon.

Some crystals of diamonds have their surfaces beautifully marked with equilateral triangles, interlaced and of varying sizes. Under the microscope these markings appear as shallow depressions sharply cut out of the surrounding surface, and these depressions were supposed by Gustav Rose to indicate the probability that the diamonds at some previous time had been exposed to incipient combustion. Rose also noted that striations appeared on the surfaces of diamonds burnt before the blowpipe. This experiment I have repeated on a clear smooth diamond, and have satisfied myself that during combustion in the field of a microscope, before the blowpipe, the surface becomes etched with markings very different in character from those naturally inscribed on crystals. The artificial striæ are cubical and closer massed, looking as if the diamond during combustion had been dissected into rectangular flakes, while the markings natural to crystals appear as if produced by the crystallising force as they were being built up.

I exhibit on a diagram a form of graphite from the Kimberley blue ground (reproduced from M. Moissan's work), which in its flaky crystalline appearance strangely resembles the surface of a diamond whose internal structure has been partially dissected and bared by combustion. It looks as if this piece of graphite was ready to separate out of its solvent as diamond, but owing to some insufficient factor it retained its graphitic form.

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[1] A lecture delivered at the Royal Institution, June 11, by William Crookes, F.R.S.

## PHYSICS OF THE DIAMOND.

The specific gravity of the diamond is from 3·514 to 3·518. For comparison, I give in tabular form the specific gravities of the different varieties of carbon:—

| | |
|---|---|
| Amorphous carbon ... ... | 1·45 to 1·70 |
| Graphite ... ... ... | 2·11 ,, 3·0 |
| Hard gas coke | 2·356 |
| Boart ... ... ... | 3·47 ,, 3·49 |
| Carbonado ... ... ... | 3·50 |
| Diamond ... ... ... | 3·514 ,, 3·518 |

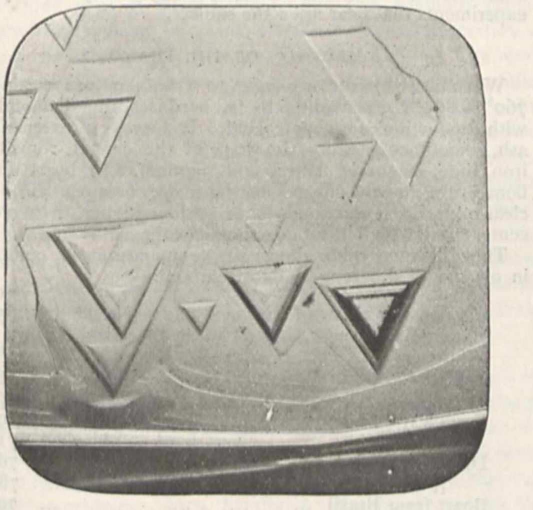

FIG. 1.—Triangular markings on natural face of a diamond.

The following table gives the specific gravities of the minerals found on the sorting tables. I have also included the specific gravities of two useful liquids:—

| | Specific gravity. |
|---|---|
| Hard graphite ... ... ... | 2·5 |
| Quartzite and granite ... ... | 2·6 |
| Beryl ... ... ... ... | 2·7 |
| Mica ... ... ... ... | 2·8 |
| Hornblende ... ... ... | 3·0 |
| METHYLENE IODIDE ... ... | 3·3 |
| DIAMOND... ... ... ... | 3·5 |
| THALLIUM LEAD ACETATE ... ... | 3·6 |
| Garnet ... ... ... ... | 3·7 |
| Corundum ... ... ... | 3·9 |
| Zircon ... ... ... ... | 4·4 |
| Barytes ... ... ... ... | 4·5 |
| Chrome and titanic iron ore ... | 4·7 |
| Magnetite... ... ... ... | 5·0 |

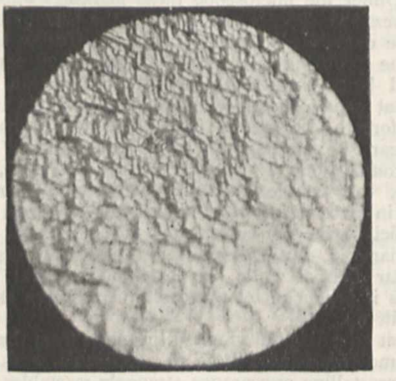

FIG. 2.—Artificial markings on face of a diamond, produced by partial combustion.

This table shows that if I throw the whole mixture of minerals into methylene iodide, the hornblende and all above that mineral will rise to the surface; while the diamond and all minerals below will sink to the bottom. If I now take these heavy minerals, and throw them into thallium lead acetate, they will all sink, except the diamond, which floats and can be skimmed off.

The diamond belongs to the isometric system of crystallography. It frequently occurs with curved faces and edges. Twin crystals (macles) are not uncommon. Having no double refraction it should not act on polarised light. But, as is well known, if a transparent body which does not so act is submitted to strain of an irregular character it becomes doubly refracting, and in the polariscope reveals the existence of the strain by brilliant colours arranged in a more or less defined pattern, according to the state of tension in which the crystal exists. Under polarised light I have examined many hundred diamond crystals, and with few exceptions all show the presence of internal tension. On rotating the polariser, the black cross, which is most frequently seen, revolves round a particular point in the inside of the crystal, and on examining this point with a high power, we see sometimes a slight flaw, more rarely a minute cavity. The cavity is filled with gas at an enormous pressure, and the strain is set up in the stone by the effort of the gas to escape.

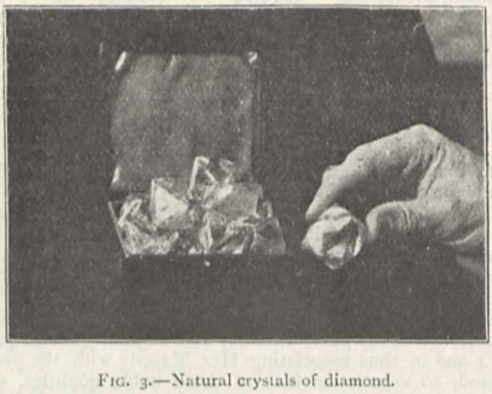

FIG. 3.—Natural crystals of diamond.

It is not uncommon for a diamond to explode soon after it reaches the surface, and some have been known to burst in the pockets of the miners or when held in the warm hand. Large crystals are more liable to burst than smaller pieces. Valuable stones have been destroyed in this way, and it is whispered that cunning dealers are not averse to allowing responsible clients to handle or carry in their warm pockets large crystals fresh from the mine. By way of safeguard against explosion, some dealers embed large diamonds in raw potato to ensure safe transit to England.

In the substance of many diamonds we find enclosed black uncrystallised particles of graphite. There also occur what may be considered intermediate forms between the well-crystallised diamond and graphite. These are "boart" and "carbonado." Boart is an imperfectly crystallised diamond, having no clear portions; therefore it is useless for gems. Boart is frequently found in spherical globules, and may be of all colours. It is so hard that it is used in rock-drilling, and when crushed it is employed for cutting and polishing other stones. Carbonado is the Brazilian term for a still less perfectly crystallised form of carbon. It is equally hard, and occurs in

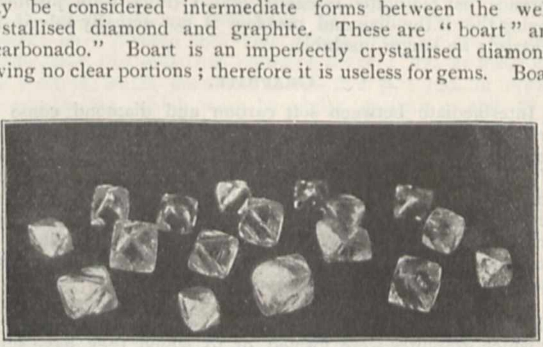

FIG. 4.—Natural crystals of diamond.

porous masses, and in massive black pebbles, sometimes weighing a couple or more ounces.

Diamonds vary considerably in hardness, and even different parts of the same crystal are decidedly different in their resistance to cutting and grinding. The famous Koh-i-noor, when cut into its present form, showed a notable variation in hardness. In cutting one of the facets near a yellow flaw, the crystal became harder and harder the further it was cut into, until, after working the mill for six hours at the usual speed of 2400 revolutions a minute, little impression was made. The speed was accordingly increased to more than 3000, when the work slowly proceeded. Other portions of the stone were found to be comparatively soft, and became harder as the outside was cut away.

Beautifully white diamonds have been found at Inverel, New South Wales, and from the rich yield of the mine and the white colour of the stones, great things were expected. A parcel of many hundred carats came to England, when it was found they were so hard as to be practically unworkable as gems, and I believe they were ultimately sold for rock-boring purposes.

I will illustrate the intense hardness of the diamond by an experiment. I place a diamond on the flattened apex of a conical block of steel, and on the diamond I bring down a second cone of steel. With the electric lantern I will project an image of the diamond and steel faces on the screen, and force them together by hydraulic power. Unless I happen to have selected a diamond with a flaw, I shall squeeze the stone right into the steel blocks without injuring it in the slightest degree.

But it is not the hardness of the diamond so much as its optical qualities that make it so highly prized. It is one of the most refracting substances in nature, and it also has the highest reflecting properties. In the cutting of diamonds advantage is taken of these qualities. When cut as a brilliant the facets on the lower side are inclined so that light falls on them at an angle of 24° 13′, at which angle all the incident light is totally reflected. A well-cut diamond should appear opaque by transmitted light, except at a small spot in the middle where the table and culet are opposite. All the light falling on the front of the stone is reflected from the facets, and the light passing into the diamond is reflected from the interior surfaces and refracted into colours when it passes out into the air, giving rise to the lightnings and coruscations for which the diamond is supreme above all other gems.

The following table gives the refractive indices of diamonds and other bodies:—

### REFRACTIVE INDICES FOR THE D LINE.

| | |
|---|---|
| Chromate of lead ... ... ... | 2·50–2·97 |
| Diamond ... ... ... ... | 2·47–2·75 |
| Phosphorus ... ... ... | 2·22 |
| Sulphur ... ... ... ... | 2·12 |
| Ruby ... ... ... .. | 1·78 |
| Thallium glass ... ... ... | 1·75 |
| Iceland spar ... ... ... | 1·65 |
| Topaz ... ... ... .. | 1·61 |
| Beryl ... ... ... ... | 1·60 |
| Emerald ... ... ... ... | 1·59 |
| Flint glass ... ... ... | 1·58 |
| Quartz ... ... ... ... | 1·55 |
| Canada balsam ... ... ... | 1·53 |
| Crown glass ... ... ... | 1·53 |
| Fluor-spar ... ... ... | 1·44 |
| Ice ... ... ... ... | 1·31 |

According to Dr. Gladstone, the specific refractive energy, $\frac{\mu - 1}{d}$, will be for the D line 0·404, and the refraction equivalent, $P\frac{\mu - 1}{d}$, will be 4·82.

After exposure for some time to the sun many diamonds glow in a dark room. Some diamonds are fluorescent, appearing milky in sunlight. In a vacuum, exposed to a high-tension current of electricity, diamonds phosphoresce of different colours, most South African diamonds shining with a bluish light. Diamonds from other localities emit bright blue, apricot, pale blue, red, yellowish green, orange, and pale green light. The most phosphorescent diamonds are those which are fluorescent in the sun. One beautiful green diamond in my collection, when phosphorescing in a good vacuum, gives almost as much light as a candle, and you can easily read by its rays. The light is pale green, tending to white.

### CONVERSION OF DIAMOND INTO GRAPHITE.

I will now draw your attention to a strange property of the diamond, which at first sight might seem to argue against the great permanence and unalterability of this stone. It has been ascertained that the cause of phosphorescence is in some way connected with the hammering of the gaseous molecules, violently driven from the negative pole, on to the surface of the body under examination; and so great is the energy of the bombardment, that impinging on a piece of platinum, or even iridium, the metal will actually melt. When the diamond is thus bombarded in a radiant matter tube the result is startling. It not only phosphoresces, but assumes a brown colour, and when the action is long continued becomes almost black.

I will project a diamond on the screen and bombard it with radiant matter before your eyes. Some diamonds visibly darken in a few minutes, while others, more leisurely in their ways, require an hour.

This blackening is only superficial, but no ordinary means of cleaning will remove the discolouration. Ordinary oxidising reagents have little or no effect in restoring the colour. The black stain on the diamond is due to a form of graphite which is very resistant to oxidation. It is not necessary to expose the diamond in a vacuum to electrical excitement in order to produce a change.

I have already signified that there are various degrees of refractoriness to chemical reagents among the different forms of graphite. Some dissolve in strong nitric acid; other forms of graphite require a mixture of highly concentrated nitric acid and potassium chlorate to attack them, and even with this intensely powerful agent some graphites resist longer than others. M. Moissan has shown that the power of resistance to nitric acid and potassium chlorate is in proportion to the temperature at which the graphite was formed, and with tolerable certainty we can estimate this temperature by the resistance of the specimen of graphite to this reagent.

The superficial dark coating on a diamond after exposure to molecular bombardment I have proved to be graphite (*Chemical News*, vol. lxxiv., p. 39, July 1896), and M. Moissan (*Comptes rendus*, cxxiv. p. 653) has shown that this graphite, on account of its great resistance to oxidising reagents, cannot have been formed at a lower temperature than 3600° C.

It is therefore manifest that the bombarding molecules, carrying with them an electric charge, and striking the diamond with enormous velocity, raise the superficial layer to the temperature of the electric arc, and turn it into graphite, whilst the mass of diamond and its conductivity to heat are sufficient to keep down the general temperature to such a point that the tube appears scarcely more than warm to the touch.

A similar action occurs with silver, the superficial layers of which can be raised to a red heat without the whole mass becoming more than warm (*Proc. Roy. Soc.*, vol. l. p. 99, June 1891).

This conversion of diamond into graphite is, I believe, a pure effect of heat. In 1880 (*Proceedings of the Royal Institution*) Friday Evening Meeting, January 16, 1880) Prof. Dewar, in this theatre, placed a crystal of diamond in a carbon tube, through which a current of hydrogen was maintained. The tube was heated from the outside by an electric arc, and in a few minutes the diamond was converted into graphite. I will now show you that a clear crystal of diamond, heated in the electric arc (temperature 3600° C.) is converted into graphite, and this graphite is most refractory.

The diamond is remarkable in another respect. It is extremely transparent to the Röntgen rays, whereas highly refracting glass, used in imitation diamonds, is almost perfectly opaque to the rays. I exposed over a photographic plate to the X-rays for a few seconds the large Delhi diamond, of a fine pink colour, weighing 31½ carats, a black diamond weighing 23 carats, together with an imitation in glass of the pink diamond lent me by Mr. Streeter; also a flat triangular crystal of diamond of pure water, and a piece of glass of the same shape and size. On development, the impression where the diamond obscured the rays was found to be strong, showing that most rays passed through, while the glass was practically opaque. By this means imitation diamonds and some other false gems can readily be detected and distinguished from the true gems. It would take a good observer to distinguish my pure triangular diamond from the adjacent glass imitation.

### Genesis of the Diamond.

Speculations as to the probable origin of the diamond have been greatly forwarded by patient research, and particularly by improved means of obtaining high temperatures. Thanks to the success of Prof. Moissan, whose name will always be associated with the artificial production of diamonds, we are able to-day to manufacture diamonds in our laboratories—minutely microscopic, it is true—all the same veritable diamonds, with crystalline form and appearance, colour, hardness, and action on light the same as the natural gem.

Until recent years carbon was considered absolutely non-volatile and infusible; but the enormous temperatures at the disposal of experimentalists—by the introduction of electricity—show that, instead of breaking rules, carbon obeys the same laws that govern other bodies. It volatilises at the ordinary pressure at a temperature of about 3600° C., and passes from the solid to the gaseous state without liquefying. It has been found that other bodies which volatilise without liquefying at the ordinary pressure will easily liquefy if pressure is added to temperature. Thus, arsenic liquefies under the action of heat if the pressure is increased; it naturally follows that if along with the requisite temperature sufficient pressure is applied, liquefaction of carbon will be likely to take place, when on cooling it will crystallise. But carbon at high temperatures is a most energetic chemical agent, and if it can get hold of oxygen from the atmosphere or any compound containing it, it will oxidise and fly off in the form of carbonic acid. Heat and pressure, therefore, are of no avail unless the carbon can be kept inert.

It has long been known that iron when melted dissolves carbon, and on cooling liberates it in the form of graphite. Moissan discovered that several other metals have similar properties, especially silver; but iron is the best solvent for carbon. The quantity of carbon entering into solution increases with the temperature, and on cooling in ordinary circumstances it is largely deposited as crystalline graphite.

Prof. Dewar has made a calculation as to the critical pressure of carbon—that is, the lowest pressure at which carbon can be got to assume the liquid state at its critical temperature, that is the highest temperature at which liquefaction is possible. He starts from the vaporising or boiling point of carbon, which, from the experiments of Violle and others on the electric arc, is about 3600° C., or 3874° Absolute. The critical point of a substance on the average is 1·5 times its absolute boiling point. Therefore the critical point of carbon is 5811° Ab., or, say, 5800° Ab. But the absolute critical temperature divided by the critical pressure is for elements never less than 2·5. Then—

$$\frac{5800^\circ \text{ A.}}{\text{PCr}} = 2\cdot5, \text{ or PCr} = \frac{5800^\circ \text{ A.}}{2\cdot5}, \text{ or } 2320 \text{ atmospheres.}$$

The result is that the critical pressure of carbon is about 2300 atmospheres, or, say, 15 tons on the square inch. The highest critical pressure recorded is that of water, amounting to 195 atmospheres, and the lowest that of hydrogen, about 20 atmospheres. In other words, the critical pressure of water is ten times that of hydrogen, and the critical pressure of carbon is ten times that of water.

Now, 15 tons on the square inch is not a difficult pressure to obtain in a closed vessel. In their researches on the gases from fired gunpowder and cordite, Sir Frederick Abel and Sir Andrew Noble obtained in closed steel cylinders, pressures as great as 95 tons to the square inch, and temperatures as high as 4000° C. Here, then, if the observations are correct, we have sufficient temperature and enough pressure to liquefy carbon; and if the temperature could only be allowed to act for a sufficient time on the carbon, there is little doubt that the artificial formation of diamonds would soon pass from the microscopic stage to a scale more likely to satisfy the requirements of science, industry, and personal decoration.

### Artificial Manufacture of the Diamond.

I will now proceed to manufacture a diamond before your eyes—don't think I yet have a talisman that will make me rich beyond the dreams of avarice! Hitherto the results have been very microscopic, and are chiefly of scientific interest in showing us nature's workshop, and how we may ultimately hope to vie with her in the manufacture of diamonds. Unfortunately, the operations of separating the diamond from the iron and other bodies with which it is associated are somewhat prolonged—nearly a fortnight being required to detach it from the iron, graphite, and other matters in which it is embedded. I can, however, show the different stages of the operations, and project on the screen diamonds made in this manner.

In Paris, recently, I saw the operation carried out by M. Moissan, the discoverer of this method of making carbon separate out in the transparent crystalline form, and I can show you the operations straight, as it were, from the inventor's laboratory. I am also indebted to the Directors of the Notting Hill Electric Lighting Co. and to the General Manager, Mr. Schultz, for enabling me to perform several operations at their central station, where currents of 500 amperes and 100 volts were placed at my disposal.

The first necessity is to select pure iron—free from sulphur, silicon, phosphorus, &c.—and to pack it in a carbon crucible with pure charcoal from sugar. Half a pound of this iron is then put into the body of the electric furnace, and a powerful arc formed close above it between carbon poles, utilising a current of 800 amperes at 40 volts pressure. The iron rapidly melts and saturates itself with carbon. After a few minutes' heating to a temperature above 4000° C.—a temperature at which the lime of the furnace melts like wax and volatilises in clouds—the current is stopped, and the dazzling fiery crucible is plunged beneath the surface of cold water, where it is held till it sinks below a red heat. As is well known, iron increases in volume at the moment of passing from the liquid to the solid state. The sudden cooling solidifies the outer layer of iron, and holds the inner molten mass in a tight grip. The expansion of the inner liquid on solidifying produces an enormous pressure, and under the stress of this pressure the dissolved carbon separates out in a transparent, dense, crystalline form—in fact, as diamond.

Now commences the tedious part of the process. The metallic ingot is attacked with hot nitro-hydrochloric acid until no more iron is dissolved. The bulky residue, consisting chiefly of graphite, together with translucent flakes of a chestnut-coloured carbon, black opaque carbon of a density of from 3·0 to 3·5, and hard as diamonds—black diamonds or carbonado, in fact, and a small portion of transparent colourless diamonds showing crystalline structure. Besides these, there may be carbide of silicon and corundum, arising from impurities in the materials employed.

The residue is first heated for some hours with strong sulphuric acid at the boiling point, with the cautious addition of powdered nitre. It is then well washed and allowed for two days to soak in strong hydrofluoric acid in the cold, then in boiling acid. After this treatment the soft graphite will disappear, and most, if not all, of the silicon compounds will be destroyed. Hot sulphuric acid is again applied to destroy the fluorides, and the residue, well washed, is repeatedly attacked with a mixture of the strongest nitric acid and powdered potassium chlorate, kept warm, but to avoid explosions not above 60° C. This ceremony must be repeated six or eight times, when all the hard graphite will gradually be dissolved, and little else left but graphitic oxide, diamond, and the harder carbonado and boart. The residue is fused for an hour in fluorhydrate of fluoride of potassium, then boiled out in water, and again heated in sulphuric acid. The well-washed grains which resist this energetic treatment are dried, carefully deposited on a slide, and examined under the microscope. Along with numerous pieces of black diamond are seen transparent colourless pieces, some amorphous, others with a crystalline appearance, as I have attempted to reproduce in diagrams. Although many fragments of crystals occur, it is remarkable that I have never seen a complete crystal. All appear broken up, as if on being liberated from the intense pressure under which they were formed they burst asunder. I have direct evidence of this phenomenon. A very fine piece of artificial diamond, carefully mounted by me on a microscopic slide, exploded during the night, and covered my slide with fragments. This bursting paroxysm is not unknown at the Kimberley mines.

On the screen I will project fragments of artificial diamond, some lent me by Prof. Roberts-Austen, others of my own make; while on the wall you will see drawings of diamonds copied from M. Moissan's book on the electric furnace. Unfortunately these specimens are all microscopic. The largest artificial diamond, so far, is less than one millimetre across.

Laboratory diamonds burn in the air before the blowpipe to carbonic acid; and in lustre, crystalline form, optical properties, density, and hardness they are identical with the natural stone.

Many circumstances point to the conclusion that the diamond of the chemist and the diamond of the mine are strangely akin as to origin. It is conclusively proved that the diamond has not been formed *in situ* in the blue ground. The diamond genesis must have taken place at great depths under enormous pressure. The explosion of large diamonds on coming to the surface shows extreme tension. More diamonds are found in fragments and splinters than in perfect crystals; and it is noteworthy that although many of these splinters and fragments are derived from the breaking up of a large crystal, yet in no instance have pieces been found which could be fitted together. Does not this fact point to the conclusion that the blue ground is not their true matrix? Nature does not make fragments of crystals. As the edges of the crystals are still sharp and unabraded the *locus* of formation cannot have been very distant from the present sites. There were probably many sites of crystallisation differing in place and time, or we should not see such distinctive characters in the gems from different mines, nor indeed in the diamonds from different parts of the same mine.

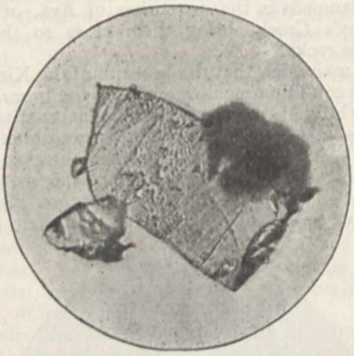

FIG. 5.—Diamond artificially crystallised from molten iron.

### THE MECHANISM OF THE DIAMANTIFEROUS PIPES.

How the great diamond pipes originally came into existence is not difficult to understand in the light of the foregoing facts. They certainly were not burst through in the ordinary manner of volcanic eruption; the surrounding and enclosing walls show no signs of igneous action, and are not shattered nor broken even when touching the "blue ground." These pipes after they were pierced were filled from below, and the diamonds formed at some previous epoch too remote to imagine were erupted with a mud volcano, together with all kinds of débris eroded from the adjacent rocks. The direction of flow is seen in the upturned edges of some of the strata of shale in the walls, although I was unable at great depths to see any upturning in most parts of the walls of the De Beers mine.

Let me again refer you to the section through the Kimberley mine. There are many such pipes in the immediate neighbourhood. It may be that each volcanic pipe is the vent for its own special laboratory—a laboratory buried at vastly greater depths than we have reached or are likely to reach—where the temperature is comparable with that of the electric furnace, where the pressure is fiercer than in our puny laboratories and the melting-point higher, where no oxygen is present, and where masses of carbon-saturated iron have taken centuries, perhaps thousands of years, to cool to the solidifying point. Such being the conditions, the wonder is, not that diamonds are found as big as one's fist, but that they are not found as big as one's head. The chemist arduously manufactures infinitesimal diamonds, valueless as ornamental gems; but nature, with unlimited temperature, inconceivable pressure, and gigantic material, to say nothing of measureless time, produces without stint the dazzling, radiant, beautiful crystals I am enabled to show you to-night.

The ferric origin of the diamond is corroborated in many ways. The country round Kimberley is remarkable for its ferruginous character, and iron-saturated soil is popularly regarded as one of the indications of the near presence of diamonds. Certain artificial diamonds present the appearance of an elongated drop. From Kimberley I have with me diamonds which have exactly the appearance of drops of liquid separated in a pasty condition and crystallised on cooling. At Kimberley, and in other parts of the world, diamonds have been found with little appearance of crystallisation, but with rounded forms similar to those which a liquid might assume if kept in the midst of another liquid with which it would not mix. Other drops of liquid carbon retained above their melting-point for sufficient time would coalesce with adjacent drops, and on slow cooling would separate in the form of large perfect crystals. Two drops, joining after incipient crystallisation, would assume the not uncommon form of interpenetrating twin crystals. Illustrations of these forms from Kimberley are here to-night. Other modified circumstances would produce diamonds presenting a confused mass of boarty crystals, rounded and amorphous masses, or a hard black form of carbonado.

Again, diamond crystals are almost invariably perfect on all sides. They show no irregular side or face by which they were attached to a support, as do artificial crystals of chemical salts; another proof that the diamond must have crystallised from a dense liquid.

When raised the diamond is in a state of enormous strain, as I have already shown by means of polarised light. Some diamonds exhibit cavities which the same test proves to contain gas at considerable pressure.

The ash left after burning a diamond invariably contains iron as its chief constituent; and the most common colours of diamonds, when not perfectly pellucid, show various shades of brown and yellow, from the palest "off colour" to almost black. These variations accord with the theory that the diamond has separated from molten iron, and also explains how it happens that stones from different mines, and even from different parts of the same mine, differ from each other. Along with carbon, molten iron dissolves other bodies which possess tinctorial powers. One batch of iron might contain an impurity colouring the stones blue, another lot would tend towards the formation of pink stones, another of green, and so on. Traces of cobalt, nickel, chromium, and manganese—all metals present in the blue ground—might produce all these colours.

An hypothesis, however, is of little value if it only elucidates one half of a problem. Let us see how far we can follow out the ferric hypothesis to explain the volcanic pipes. In the first place we must remember these so-called volcanic vents are admittedly not filled with the eruptive rocks, scoriaceous fragments, &c., constituting the ordinary contents of volcanic ducts. At Kimberley the pipes are filled with a geological plum-pudding of heterogeneous character—agreeing, however, in one particular. The appearance of shale and fragments of other rocks shows that the mélange has suffered no great heat in its present condition, and that it has been erupted from great depths by the agency of water vapour or some similar gas. How is this to be accounted for?

It must be borne in mind I start with the reasonable supposition that at a sufficient depth[1] there were masses of molten iron at great pressure and high temperature, holding carbon in solution, ready to crystallise out on cooling. In illustration I may cite the masses of erupted iron in Greenland. Far back in time the cooling from above caused cracks in superjacent strata through which water[2] found its way. Before reaching the iron the water would be converted into gas, and this gas would rapidly disintegrate and erode the channels through which it passed, grooving a passage more and more vertical in the endeavour to find the quickest vent to the surface. But steam in the presence of molten or even red-hot iron rapidly attacks it, oxidises the metal and liberates large volumes of hydrogen gas, together with less quantities of hydrocarbons[3] of all kinds—liquid, gaseous, and solid. Erosion commenced by steam would be continued by the other gases, and it would be no difficult task for pipes, large as any found in South Africa, to be scored out in this manner. Sir Andrew Noble has shown that when the screw-stopper of his steel cylinders in which gunpowder explodes under pressure is not absolutely perfect,

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[1] The requisite pressure of fifteen tons on the square inch would exist not many miles beneath the surface of the earth.

[2] There are abundant signs that a considerable portion of this part of Africa was once under water, and a fresh-water shell has been found in apparently undisturbed blue ground at Kimberley.

[3] The water sunk in wells close to the Kimberley mine is sometimes impregnated with paraffin, and Sir H. Roscoe extracted a solid hydrocarbon from the "blue ground."

gas finds its way out with a rush so overpowering as to score a wide channel in the metal; some of these stoppers and vents are on the table. To illustrate my argument Sir Andrew Noble has been kind enough to try a special experiment. Through a cylinder of granite is drilled a hole 0·2 inch diameter, the size of a small vent. This is made the stopper of an explosion chamber, in which a quantity of cordite is fired, the gases escaping through the granite vent. The pressure is about 1500 atmospheres, and the whole time of escape is less than half a second. Notice the erosion produced by the escaping gases and by the heat of friction, which have scored out a channel over half an inch diameter and melted the granite along their course. If steel and granite are thus vulnerable at comparatively moderate gaseous pressure, is it not easy to imagine the destructive upburst of hydrogen and water-gas grooving for itself a channel in the diabase and quartzite, tearing fragments from resisting rocks, covering the country with débris, and finally, at the subsidence of the great rush, filling the self-made pipe with a water-borne magma in which rocks, minerals, iron oxide, shale, petroleum, and diamonds are churned together in a veritable witch's cauldron! As the heat abated the water vapour would gradually give place to hot water, which forced through the magma would change some of the mineral fragments into the now existing forms.

Each outbreak would form a dome-shaped hill, but the eroding agency of water and ice would plane these eminences until all traces of the original pipes were lost.

Actions, such as I have described, need not have taken place simultaneously. As there must have been many molten masses of iron with variable contents of carbon, different kinds of colouring matter, solidifying with varying degrees of rapidity, and coming in contact with water at intervals throughout long periods of geological time—so must there have been many outbursts and upheavals, giving rise to pipes containing diamonds. And these diamonds, by sparseness of distribution, crystalline character, difference of tint, purity of colour, varying hardness, brittleness, and state of tension, would have impressed upon them, engraved by natural forces, the story of their origin—a story which future generations of scientific men may be able to interpret with greater precision than we can to-day.

Who knows but that at unknown depths in the earth's metallic core beneath the present pipes there are still masses of iron not yet disintegrated and oxidised by aqueous vapour—masses containing diamonds, unbroken, and in greater profusion than they exist in the present blue ground, inasmuch as they are enclosed in the matrix itself, undiluted by the numerous rock constituents which compose the bulk of the blue ground. If this be the case a careful magnetic survey of the country around Kimberley might prove of immense interest, scientific and practical. Observations, at carefully selected stations, of the three magnetic elements—the horizontal component of direction, the vertical component of direction, and the magnetic intensity—would soon show whether any large masses of iron exist within a certain distance of the surface. It has been calculated that a mass of iron 500 feet in diameter could be detected were it ten miles below the surface. A magnetic survey might also reveal other valuable diamantiferous pipes, which owing to the absence of surface indications would otherwise remain hidden.

### METEORIC DIAMONDS.

There is another diamond theory which appeals to the fancy. It is said that the diamond is a direct gift from heaven, conveyed to earth in meteoric showers. The suggestion, I believe, was first broached by A. Meydenbauer (*Chemical News*, vol. lxi. p. 209, 1890), who says:—"The diamond can only be of cosmic origin, having fallen as a meteorite at later periods of the earth's formation. The available localities of the diamond contain the residues of not very compact meteoric masses which may, perhaps, have fallen in historic ages, and which have penetrated more or less deeply, according to the more or less resistant character of the surface where they fell. Their remains are crumbling away on exposure to the air and sun, and the rain has long ago washed away all prominent masses. The enclosed diamonds have remained scattered in the river beds, while the fine light matrix has been swept away."

According to this hypothesis, the so-called volcanic pipes are simply holes bored in the solid earth by the impact of monstrous meteors—the larger masses boring the holes, while the smaller masses, disintegrating in their fall, distributed diamonds broadcast. Bizarre as such a theory may appear, I am bound to say there are many circumstances which show that the notion of the heavens raining diamonds is not impossible.

In 1846 a meteorite fell in Hungary (the "Ava meteorite") which was found to contain graphite in the cubic crystalline system. G. Rose thought this cubic graphite was produced by the transformation of a diamond. Long after this prediction was verified by Weinschenk, who found transparent crystals in the Ava meteorite. Mr. Fletcher has found in two meteoric irons—one from Youndegin, East Australia, and one from Crosby's Creek, United States—crystals absolutely similar to those in the Ava meteorite.

In 1886 a meteorite falling in Russia contained, besides other constituents, about 1 per cent. of carbon in light grey grains, having the hardness of diamond, and burning in oxygen to carbonic acid.

Daubrée says the resemblance is manifest between the diamantiferous earth of South Africa and the Ava meteorite, of which the stony substance consists almost entirely of peridot. Peridot being the inseparable companion of meteoric iron, the presence of diamonds in the meteorites of Ava, of Youndegin, and of Crosby's Creek, bring them close to the terrestrial diamantiferous rocks.

Hudleston maintains that the bronzite of the Kimberley blue ground is in a condition much resembling the bronzite grains of meteorites; whilst Maskelyne says that the bronzite crystals of Dutoitspan resemble closely those of the bronzite of the meteor of Breitenbach, but are less rich in crystallographic planes.

But the most striking confirmation of the meteoric theory comes from Arizona. Here, on a broad open plain, over an area about five miles diameter, were scattered one or two thousand masses of metallic iron, the fragments varying in weight from half a ton to a fraction of an ounce. There is little doubt these masses formed part of a meteoric shower, although no record exists as to when the fall took place. Curiously enough, near the centre, where most of the meteorites have been found, is a crater with raised edges three-quarters of a mile in diameter and about 600 feet deep, bearing exactly the appearance which would be produced had a mighty mass of iron or falling star struck the ground, scattered in all directions, and buried itself deep under the surface. Altogether ten tons of this iron have already been collected, and specimens of the Canyon Diablo meteorite are in most collectors' cabinets.

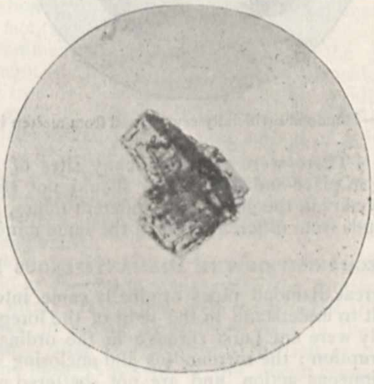

FIG. 6.—Diamond from the Canyon Diablo meteorite.

An ardent mineralogist, the late Dr. Foote, in cutting a section of this meteorite, found the tools were injured by something vastly harder than metallic iron, and an emery-wheel used in grinding the iron had been ruined. He examined the specimen chemically, and soon after announced to the scientific world that the Canyon Diablo meteorite contained black and transparent diamonds. This startling discovery was afterwards verified by Profs. Friedel and Moissan, who found that the Canyon Diablo meteorite contained the three varieties of carbon—diamond (transparent and black), graphite, and amorphous carbon. Since this revelation, the search for diamonds in meteorites has occupied the attention of chemists all over the world.

I am enabled to show you photographs of true diamonds I have myself extracted from pieces of the Canyon Diablo meteorite, five pounds of which I have dissolved in acids for this purpose—an act of vandalism in the cause of science for which I hope mineralogists will forgive me. A very fine slab of the meteorite, weighing about seven pounds, which has escaped the solvent, is on the table before you.

Here, then, we have absolute proof of the truth of the meteoric theory. Under atmospheric influences the iron would rapidly oxidise and rust away, colouring the adjacent soil with red oxide of iron. The meteoric diamonds would be unaffected, and would be left on the surface of the soil to be found by explorers when oxidation had removed the last proof of their celestial origin. That there are still lumps of iron left at Arizona is merely due to the extreme dryness of the climate and the comparatively short time that the iron has been on our planet. We are here witnesses to the course of an event which may have happened in geologic times anywhere on the earth's surface.

Although in Arizona diamonds have fallen from above, confounding all our usual notions, this descent of precious stones seems what is called a freak of nature rather than a normal occurrence. To the modern student of science there is no great difference between the composition of our earth and that of extra-terrestrial masses. The mineral peridot is a constant extra-terrestrial visitor, present in most meteorites. And yet no one doubts that peridot is also a true constituent of rocks formed on this earth. The spectroscope reveals that the elementary composition of the stars and the earth are pretty much the same; so does the examination of meteorites. Indeed, not only are the self-same elements present in meteorites, but they are combined in the same way to form the same minerals as in the crust of the earth.

This identity between terrestrial and extra-terrestrial rocks recalls the masses of nickeliferous iron of Ovifak. Accompanied with graphite, they form part of the colossal eruptions which have covered a portion of Greenland. They are so like meteorites that at first they were considered to be meteorites till their terrestrial origin was proved. They contain as much as 1·1 per cent. of free carbon.

It is certain from observations I made at Kimberley, corroborated by the experience gained in the laboratory, that iron at a high temperature and under great pressure will act as the long-sought solvent for carbon, and will allow it to crystallise out in the form of diamond—conditions existent at great depths below the surface of the earth. But it is also certain, from the evidence afforded by the Arizona and other meteorites, that similar conditions have likewise existed among bodies in space, and that a meteorite, freighted with its rich contents, on more than one occasion has fallen as a star from the sky. In short, in a physical sense, heaven is but another name for earth, or earth for heaven.

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## *THE INSTITUTION OF MECHANICAL ENGINEERS.*

THE Institution of Mechanical Engineers was founded in 1847, and the present year is therefore its jubilee. As it came into existence as a Birmingham Society, and for the first thirty years of its career had its offices in that city, the removal to London being made in 1877, it was appropriate the jubilee meeting should be held there.

The meeting commenced on Tuesday of last week, July 27, and was brought to a close on the following Friday. There were two sittings for the reading and discussion of papers held on the Tuesday and Wednesday mornings, the President, Mr. Mr. E. Windsor Richards, occupying the chair. There were five papers on the agenda, but time was only found for the reading of the following three:—

"Some points in cycle construction," by F. J. Osmond, of Birmingham.

"The City of Birmingham Corporation Waterworks," by Henry Davey, of London.

"High-speed self-lubricating engines," by Mr. Alfred Morcom, of Birmingham.

The President also read an address, in which he gave particulars of the founding and early history of the Institution, together with short biographical notices of its past-presidents, from George Stephenson, who was the first, down to the present day.

Mr. Osmond, in his paper, discussed a few of the points to be observed in designing a successful bicycle. The principal causes of inefficiency, he said, were want of rigidity and undue friction. Of these two he considered the former the most important, and it is in this particular that cycles differ far more than in friction. The cause of loss is twofold. Firstly, the work done in springing the frame out of shape at each stroke of the foot is not spent in driving at the end of the down-stroke, but only in lifting the foot at the beginning of the up-stroke. Secondly, the springing of the frame causes a general condition of instability, due partly to the alteration of the balance through lateral movement of the pedals, and partly to the wheels being forced out of line, thereby causing the machine to swerve from side to side instead of running a true course. Purchasers of bicycles would do well to remember these facts. The rage for lightness is so great, that the makers, who have to follow the fashion, often cut material down to a point where there is only just enough metal to support the rider's weight under the varying conditions of running, the factor of safety being perilously small. As to rigidity, that is often abandoned altogether, or at any rate is only considered so far as it does not add to weight. Considering that the average purchaser only tests the machine by spinning the wheels and pedals to see if they run easily, one cannot wonder at this abandonment of a vital principle by the maker; but perhaps after the warning of Mr. Osmond, himself a noted manufacturer of cycles, sounder principles may prevail. In well-constructed machines friction is mainly due to the chain, and it is said that no more than 1 per cent. of the total power exerted by the rider has been lost. Even allowing a much higher factor than this, and doubtless it is too small, it will be seen to what perfection ball bearings have enabled the cycle maker to produce his machines. Mr. Osmond thinks that a mechanical efficiency of 95 per cent. would be nearer the truth, and this would be somewhat lower than the best record with which we are acquainted for the steam engine. The factor of safety for the bicycle frame is about 1½, and if this is to be taken as including the ordinary conditions of riding, Mr. Osmond considers it true; but he states that a well-built frame will carry at least ten times its natural load without injury. The difference is due to the fact that the front part of the frame is exposed to shocks which must cause bending stresses near the head. If the two front tubes are arranged so that their axes intersect vertically above the axle of the front wheel, the stresses are only pure tension and compression so long as the force acting through the front axle is purely vertical. Such conditions are naturally not present when the wheel meets an obstacle, and bending stresses are therefore introduced. Other details of construction were discussed in the paper, and were illustrated by numerous wall diagrams. The discussion on this paper was confined to the suggestion by one speaker, Mr. Sharp, of Birmingham, that the weakening effect of brazing together the members of frames might be overcome by making a mechanical joint in which a hollow plug of suitable formation should be inserted in the ends of two tubes to be connected, the plug being corrugated on the outside, the idea being that the tube ends of the tubes containing the plug should be pressed into the corrugations. The joint would seem difficult to make, and one would fear that even if tightly made in the first instance it would be likely to work loose in time; but we are assured by the inventor that the device has given most promising results in actual practice. If these promises can be confirmed, the invention is of considerable value, as the brazing of steel undoubtedly causes deterioration of the metal.

Mr. Davey's paper gave an historical and general account of the Birmingham water works, together with cost of pumping, &c. This contribution led to practically no discussion.

Mr. Alfred Morcom's paper was far the most important of the three, and indeed was an excellent example of what a contribution to the proceedings of this Institution should be. The author is managing director of the firm of G. E. Belbis and Company, who have for some time past devoted their resources largely to the construction of what are generally known as high-speed engines, for which of late there has been a large demand owing to the spread of electric generation for lighting and power purposes. The engines of this firm differ from those largely manufactured for like purposes in the fact that the cylinders are double-acting, steam being taken on both sides of the piston. For very high speeds of rotation it has been often said to be necessary, in order to give smooth running, that there should be no reversal of stress on the working parts; steam, therefore, has generally been admitted only above the piston, so that the stresses on the connecting-rods were always those of compression. With such an arrangement naturally a given cylinder only does half the work that can be obtained from a double-acting cylinder of the same capacity, and this leads to additional weight and space being required for the single-acting engine. For this reason it was the common practice, and still is to a large extent, to run the necessarily quickly rotating dynamo belt-gearing from a large engine making moderate revolutions, and occupying much space; but for a considerable time past the high-speed, single-acting engine, coupled direct, has been a formidable rival. The high-speed double-acting engine has also been growing in favour of late, and, as has been stated, undoubtedly has advantages. The dynamo-electric machine has certainly done one good thing—it has raised the standard of stationary engine design and manufacture enormously, just as the torpedo boat did for marine engineering. The chief features dealt with by Mr. Morcom in his paper were lubrication and vibration, the two great difficulties to be met in quick-turning engines. To effectually lubricate bearings a force-pump is employed, which continuously injects oil at pressure into the space between the shaft or journal and the bearing. The reciprocation of pressure of the shaft on the bearing assists the circulation of the lubricant for the following reason: when strain is above the piston, and the connecting rod is in compression, the journal will be pressing on the bottom brass—we put out of consideration any tendency of the shaft to bend—and, as a journal can never be an absolutely tight fit in its bearing, there will be a space between the top bearing and the shaft. Into this space oil is at once forced by the pressure-pump, and when the stress is reversed the film of oil remains during the whole of the up-stroke, because there is not time to squeeze it out from between the rubbing surfaces before the pressure is again released. The same thing, of course, applies to the bottom brass, and in this way there is always a liquid film of oil between the journal or shaft and the brass or bearing, and the two, therefore, never come in contact. Observed data support the latter view, as the wear on journals has been found to be inappreciable after considerable running; but perhaps the best testimony is that Prof. Kennedy, in an exhaustive test of one of these engines, found the mechanical efficiency of the machine to be 96·3 per cent. It will be seen that in this matter of distributing the oil on the bearing surfaces the double-acting engine has an advantage over the single-acting engine, where the pressure is always in one direction, and is never released while the engine is running, although it may be relaxed. In regard to vibration so much has been done lately, especially by the builders of torpedo craft, that not much is left to add. It may be said that Mr. Morcom is fully alive to the need for providing against the disturbance "due to couples produced by the changing momentum in the several lines of moving parts," and that occasioned by the obliquity of movement of the connecting-rod. He refers to Mr. Yarrow's admirable experiments, and considers the effect of crank angle and multiple cylinders. We have not, however, space to go into these problems, and must refer our readers to the original paper.

A long and interesting discussion followed the reading of the paper.

There were several excursions to neighbouring towns, where works were visited, speeches made, and luncheons eaten after the manner of meetings of this kind. One of the trips which attracted a great deal of interest was that to Coventry, where the much-discussed "motor-mills" where "horseless carriages" are made in such profusion, according to certain glowing accounts, were to be inspected. This establishment is said to be "the largest and best organised for the purpose in the country." To judge by what was seen in regard to work in progress, there need not be much fear that the country will be flooded by horseless carriages for some time to come yet.

## *A TROUBLESOME AQUATIC PLANT.*

FOR several years past an aquatic plant known as the water hyacinth has been developing to such an enormous extent in the St. Johns River, Florida, as to cause serious apprehension in that region regarding its possible obstruction to navigation. About two years ago the War Department was asked to investigate the matter, and did so. In answer to urgent requests for exact information on the subject, the Department of Agriculture, on January 25, directed one of its agents, Mr. Herbert J. Webber, an assistant in the Division of Vegetable Physiology and Pathology, to visit the region and prepare a report covering the following points: (1) Historical notes regarding the plant, including its habitat, manner of growth, propagation, and anatomical and physiological characters; (2) an account of its introduction and spread in Florida; (3) the present distribution of the plant in the State, and its effect on navigation and commerce; and (4) possibilities of exterminating it. Mr. Webber's report has now been issued from the Government Printing Office, Washington, and is very exhaustive. The plant is mostly limited in its growth to sluggish fresh-water streams, lakes, &c., and the character of the water appears to have much to do with its growth. It can endure only a small percentage of salt, and is killed when it floats down into the sea-water. It is normally propagated by seeds and by stolons. Its introduction into the St. Johns River took place about 1890, when a number of plants were thrown into it. They grew there luxuriantly, producing beautiful masses of flowers which rendered the river attractive. At this time no one suspected that the plant would become a nuisance, and it was introduced at various points to beautify the river. In a short time it interfered very materially with navigation, making it, in fact, both difficult and dangerous. Its effect has been most disastrous to those engaged in the lumber trade and in the fishing industry. It is feared that eradication is impracticable, but suggestions are made as to possible methods for keeping the evil in check. Of these the one most in favour with the author is the use of a light-draught stern-wheel steamer, having a double bow or outrigger, which, being forced into a mass of plants, would cause them to gather towards the middle of the boat, where an inclined carrier would pick them up and deposit them in front of rollers driven by machinery, which would force the water from them, thus greatly reducing their bulk. The crushed material could be delivered to barges alongside, to be deposited where no injury could again result, or a cremator could be arranged on a barge alongside of the boat, and so save additional handling.

## *THEORY AND PRACTICE.*[1]

I PROPOSE to speak to-day of the relative importance of theory and of practice in the arts; and especially, of course, in the art of medicine. It is said that Englishmen are falling behind other nations, and especially behind the German nation, in their perception of the value of theory in the practical arts. Now this is somewhat strange and inconceivable to us. Englishmen proudly feel in this year of the Greater Jubilee that their achievements in the conduct of life are not only great but incomparable. Not only has England become great as an empire, as the Roman Empire; it is great also in the achievements of the intellect: the land of Roger Bacon, of Francis Bacon, of Newton and Adams, of Berkeley, Locke and Hume, of Boyle, Priestley, Cavendish and Dalton, of Young and Faraday, of Harvey, Owen, and Darwin, need not be ashamed even before the brilliant nation of Descartes and Laplace, of Lavoisier and Cuvier, of Paré, Bichat, and Bernard. Nor will I forget to speak of our place in letters, wherein we acknowledge none as our masters; for it is of the gifts of imagination no less than of the gift of analysis that scientific theory is born. Can it be true, then, that with these endowments we are to fall behind in the practice of the arts because, as a nation, we have no due sense of the bearing of theory upon practice?

It cannot be doubted, I fear, that, in some departments of knowledge we are falling behind relatively if not absolutely; that we have failed to keep before ourselves a due sense of the value of theory, and have forgotten that, although in generalisation we should never lose our hold upon detail, nor lose our tact in converse with the manifold aspects of life, nor our memory of the devices whereby we must meet the incursions of contingencies often themselves incalculable, we shall nevertheless fall behind in the fight with reluctant nature if we do not incessantly revise our formulas in the light of progressive research on more and more general lines. We have perhaps forgotten that the work of Watt and Stephenson would have made little progress but for the great modern advances in thermodynamics in which, among others, are

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[1] Abstract of an address delivered at the combined meeting of the Cambridge and Huntingdon, the East Anglian, and the South Midland Branches of the British Medical Association at Cambridge, by Prof. T. Clifford Allbutt, F.R.S. Abridged from the *British Medical Journal*.

eminent the names of our own Joule and Thomson; we have perhaps forgotten that, brilliant as is the work of the modern electrician, his achievements depend upon the theoretical researches of disinterested students such as Faraday and Hertz; we have perhaps forgotten, as a contributor to NATURE said last year, that "Kekulé first gave definite form to Frankland's conception of valency, and his application of this idea to the study of the carbon compounds was nothing less than epoch making. Out of this conception grew the famous theory of cyclic compounds which has been prolific to an extent almost unparalleled in the history of pure science, and which on the practical side has made Germany what it is in the domain of organic chemical technology." In our own art, proud as we may be of Jenner and Lister, yet we have to remember that the recent advances in the theory of infection are due rather to the schools of Pasteur and Koch than to our own; while the scarcely less remarkable advance in the discovery and manufacture of new drugs is almost entirely of foreign growth.

How comes it, then, that if we are a people of contemplative as well as of practical gifts, we have so far forgotten ourselves as to fall behind—in many respects far behind—in discovery and, consequently, in practical success? Some of the reasons are not far to seek. The most important of all is, no doubt, that Englishmen, by their practical genius—that is, by their gifts of adventure, of restless energy, of perception of contingency and accident in daily life, and of a correlative readiness of resource—have achieved so much in the mastery over man and nature, that in their day of prosperity they have lost reverence for those qualities of the mind upon which the development of science and art must in the main depend. The system of practical rules and maxims which they have built up are now becoming obsolete, and need revision in the face of larger requirements; as the rules of the first steam engineers had to be revised in the face of the discovery of the mechanical equivalent of heat, a discovery not made by engineers, but given to them. That practical rules shall from time to time be brought up for revision, and be remodelled on the lines of advancing theoretical research, is essential to the continuance of our progress and of all successful practice.

Another reason of our defect is that our people are above all things efficient in the manual arts. Now the manual arts are less open to theoretical inadequacy than the chemical arts or the art of medicine; the complexity of the conditions is less, the incursions of incidental contingencies are fewer, and the necessary calculations are both simpler and in their effects more immediate and obvious. Hence we are tempted to assume that the ready calculations of the practical man are roughly sufficient for all other arts, as they are for the manual; and that they pay as promptly and obviously. But this is not the case; as we pass from the manual arts to the chemical, let us say, or to the physiological, we have to deal with causes of much greater complexity, contingencies are multiplied and are less and less easily foreseen; thus it is that even valid generalisations have to wait for the fulfilment, or practical fulfilment, of a number of secondary verifications before they can be utilised; meanwhile they are as useless to the practical man as, for example, the more general laws of meteorology are to the navigator. Now thus to wait means time and men's lives; both must be spent without reward until a considerable block of discovery is made, verified and applied. Research, then, in the chemical and biological sciences, at any rate, cannot be self-supporting, but must rest on large endowments. The American people and the people of Lancashire and Yorkshire now see this more or less darkly, and they are endowing research with a generous hand; but national aid—liberally given in the United States as well as in France and Germany—is refused in England, and will be refused until the nation, aroused by the urgency of pressing need, determines no longer to be governed by the clerks of the Treasury.

Another reason for our defect is the want of a bridge between the contemplative and the practical man. The man of abstract speculation, the experimenter absorbed in his single desire to wrest her secrets from nature begins at the opposite pole from him whose whole activity is absorbed in industrial adventure.

It is not that theory and practice are essentially opposed, but that the man of theory loves to move in the larger sphere of those more general laws which express the order of phenomena in their wider orbits, and therein to neglect those incidental and subordinate causes which, after all, are the main concerns of the journeyman. It is as though a mathematician would work at his problems, neglecting friction; a political economist at the secular aspects of industrial problems, without taking account of the passions of mankind; a hydrographer at the sweep of the great systems of oceanic currents, without taking account of the whirlpools, races, and under-currents which modify them on every coast and in every gulf; yet the journeyman is mainly concerned with the irregularities of which the abstract-thinker may be ignorant. While, therefore, the theorist may rightly reproach the practical man with the narrowness of his outlook, the practical man may usefully retort that a theory which only accounts for the larger recurrent cycles is imperfect, even as a theoretical statement; and that in respect of cases not only are the wider laws to be formulated, but also the smaller periods of those many contingencies and perturbations which, in a complete theoretical statement, have all to be reckoned with. If we forecast our weather on barometrical pressure alone, we shall be disappointed; a complete theory of meteorological cases must comprehend formulas for the phenomena of moisture, of temperature, of electricity, of oceanic currents, and so forth. A meteorologist, who pursued his studies in the Islands of the Blest, might become a very learned philosopher, but would be a very untrustworthy guide to the weather; and one who should work, say at physics or physiology, in a university where engineering and medicine are unknown, would likewise fall out of touch with the practical ends which are the ultimate purpose of all science. There is a casuistry in the arts, as there is in morals; particular instances are apt to elude general principles: and although it be true that, as with statistics,, by taking a sufficiently large number of instances we may eliminate incidental causes, yet this is precisely what the practical man must avoid.

We shall not, as practical men, whether in medicine or in morals, allow ourselves to be blinded by the light of the brilliant generalisations of the laboratory and the lecture-room, helpful as this light is, to the value of the empirical rules which have been painfully gathered together in the trials and errors of generations of men. As Oliver Wendell Holmes says, "Science is a first-rate piece of furniture for a man's upper chamber if he has common sense on the ground floor; but if a man has not got plenty of good common sense, the more science he has the worse for his patient." A sagacious man is not to be driven too readily out of his rules, absurd as they may seem.

On the other hand, while clinging to our rules until something better is demonstrated to us, we practical men must not forget day by day to submit our empirical laws to revision; we must bear in mind that empiricism is science without roots. We are right in our paradoxical tolerance of anomalies; we are right in our instinct that broad generalisations are too often impracticable in that they may not take account of incidents and contingencies, but this is not all.

In England the man of thought and the man of action have been too much apart; thus both have suffered. So long as our national work has been pioneering work, rough practical rules of thumb, applied with indomitable energy, have been irresistible; but as industrial pursuits become more complex, and the sciences concerned also more complex, rule of thumb is no longer wide enough or refined enough; methods based upon wider theoretical considerations have to be introduced.

We cannot stand still; we must advance, revolve, or degenerate; pathology must be renewed by the new meanings and new bearings of biology and by a comparative method, embracing in experience no less than the diseases of all living things. We must work out, or obtain from our fellows who work them out, the mechanical and chemical laws which underlie all life, and by a method of exclusion ascertain what the residuum is which may be peculiar to living matter. We shall not assume vitality as a principle, and grudgingly admit a little chemistry and physics to fill up a few holes in a threadbare principle. Medicine depends upon theoretical advance, not in physiology only, but in all sciences; and upon the practice of many, as upon practical optics in respect of our microscopes, upon mechanics in respect of our graphic machinery and so forth; as we change we change our circumstances, and circumstances reacting on us change us again; so that we depend upon a highly compound process of advance, and need theoretical reinforcement from all sides.

## UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

MR. WILLIAM ESSON, F.R.S., Deputy Savilian Professor of Geometry in the University of Oxford, has been appointed to succeed the late Prof. Sylvester as Savilian Professor.

DR. ARTHUR A. RAMBAUT, Royal Astronomer of Ireland, has been appointed Radcliffe Observer at Oxford, in succession to the late Dr. E. J. Stone, F.R.S.

THE John Lucas Walker Studentship at Cambridge, having an annual value of 250*l.* and tenable, under certain conditions, for three years, will be awarded in October next. The holder of the studentship is required to devote himself (or herself) to original research in pathology. Applications should be sent, before October 15, to Dr. A. A. Kanthack, Pathological Laboratory, Cambridge, to whom also requests for further information should be addressed.

THE following resolution, with reference to the London University Commission Bill, has been adopted by the Technical Education Board of the London County Council: "That the Board, being satisfied that the Bill now before Parliament makes adequate provision for inclusion within the scheme of the University of duly-qualified teachers and students in the polytechnic institutes (and other institutions aided by the Board), and that the special interests with which the Technical Education Board is concerned have been duly safeguarded, hereby approves the Bill and expresses its desire that it may be passed into law this session."

IN the House of Commons on Monday, Mr. Balfour announced with very great regret that he believed it to be impossible to take the London University Commission Bill in the course of the present Session. He felt to the full the objections as to leaving over for another year the settlement of a question in which a large number of independent educational bodies were concerned, and on which, speaking broadly, they had come to an agreement. But he recognised at the same time that there was serious opposition taken to it in its present shape, and the Government could not face the expenditure of time necessary to deal with the measure in the present Session. The Government must therefore postpone the Bill. He earnestly hoped that next year, at all events, the Government would be in a position to settle this long-standing and perplexing controversy.

THE circumstances and the legislative proceedings which resulted in the reconstitution of the French Universities were described in NATURE rather more than a year ago (vol. liv. p. 64, May 21, 1896). The Paris correspondent of the *Times* now reports that, under the law which has come into force, decrees have just been issued for the government of the Universities. Each University is to have a council, consisting of the rector, the heads of faculties, and two delegates of each faculty elected triennially by the professors. The council, subject in certain cases to the approval of the supreme education council, will have control over the teaching, discipline and property of the University. It will, however, have merely a consultative voice on the finances, and on the creation, abolition, or transformation of professorships, for the State will continue to pay the stipends. The maintenance of buildings, on the other hand, will fall on the University, and must be defrayed from students' fees or from endowments. The State takes the fees for examinations and State diplomas, but all other fees go to the University treasury. It will, therefore, be to the interest of each University to attract as many students as possible. The receipts of Paris University are estimated at 600,000 francs, and of Lyons at 130,000 francs, but Besançon and Clermont have at present only 700 francs or 800 francs, and will obviously have to solicit subsidies either from the State or from local bodies. Failing this, the smaller Universities are likely to succumb. One of the features of the new system is that a student will, as in Germany, be able to migrate from one University to another without lengthening his studies or delaying his degree.

HER Majesty's Commissioners for the Exhibition of 1851 have made the following appointments to Science Research Scholarships, for the year 1897, on the recommendation of the authorities of the respective Universities and Colleges. The scholarships are of the value of 150*l.* a year, and are ordinarily tenable for two years (subject to a satisfactory report at the end of the first year) in any University at home or abroad, or in some other institution approved of by the Commissioners. The scholars are to devote themselves exclusively to study and research in some branch of science, the extension of which is important to the industries of the country. A limited number of the scholarships are renewed for a third year where it appears that the renewal is likely to result directly in work of scientific importance.

| | Nominating institution. | Scholar |
|---|---|---|
| 1 | University of Edinburgh | Longfield Smith |
| 2 | University of Glasgow ... | James Muir |
| 3 | University of St. Andrews | Harry McDonald Kyle |
| 4 | University College, Dundee | Sydney A. Kay |
| 5 | Mason College, Birmingham | Gilbert Arden Shakespear |
| 6 | University College, Bristol | Chas. Henry Graham Sprankling |
| 7 | Yorkshire College, Leeds | Harold Albert Wilson |
| 8 | University College, Liverpool | William Augustus Caspari |
| 9 | University College, London | Percy Williams |
| 10 | Owens College, Manchester | John Henry Grindley |
| 11 | Durham College of Science, Newcastle-upon-Tyne | Robert Railton Hallaway |
| 12 | University College, Nottingham | Richard S. Willows (*conditional appointment*) |
| 13 | Firth College, Sheffield | Ernest Clark |
| 14 | University College of South Wales, Cardiff | Maria Dawson |
| 15 | Queen's College, Belfast | William Alexander Osborne |
| 16 | McGill University, Montreal | James Lester Willis Gill |
| 17 | Queen's University, Kingston, Ontario | Frederick John Pope |
| 18 | University of Sydney | Tom Percival Strickland |
| 19 | University of Melbourne | Walter Rosenhain |

The following scholarships, granted in 1896, have been continued for a second year on receipt of a satisfactory report of work done during the first year:—

| | Nominating institution | Scholar | Places of study |
|---|---|---|---|
| 1 | University of Glasgow | William Craig Henderson | Cavendish Laboratory, Cambridge |
| 2 | University of Aberdeen | Alexander Ogg | University of Göttingen |
| 3 | Mason College, Birmingham | Thomas Slater Price | University of Leipzig |
| 4 | University College, Bristol | Emily Comber Fortey | University College, Bristol, and Owens College |
| 5 | Yorkshire College, Leeds | Harry Medforth Dawson | University of Berlin |
| 6 | University College, London | Joseph Ernest Petavel | University College, London, and Davy-Faraday Laboratory |
| 7 | Owens College, Manchester | John Leathart Heinke | Owens College and University of Tübingen |
| 8 | Durham College of Science, Newcastle-upon-Tyne | John Armstrong Smythe | University of Göttingen |
| 9 | University College, Nottingham | George Blackford Bryan | Cavendish Laboratory, Cambridge |
| 10 | University College of Wales, Aberystwyth | Spencer William Richardson | Cavendish Laboratory, Cambridge |
| 11 | Queen's College, Galway | John Henry | Cavendish Laboratory, Cambridge |
| 12 | University of Toronto | Arthur Melville Scott | University of Göttingen |
| 13 | Dalhousie University, Halifax, Nova Scotia | Douglas McIntosh | Cornell University |
| 14 | University of New Zealand | John Angus Erskine | University of Berlin |

The following scholarships, granted in 1895, have been exceptionally renewed for a third year:—

| | Nominating institution | Scholar | Places of study |
|---|---|---|---|
| 1 | University of Glasgow | Walter Stewart | Universities of Glasgow and Berlin |
| 2 | McGill University, Montreal | Robert Owen King | McGill University and Harvard University |
| 3 | University of New Zealand | Ernest Rutherford | Cavendish Laboratory, Cambridge |

The Scholarships Committee consisted of Sir Henry Roscoe, chairman; Lord Rayleigh, Lord Kelvin, Lord Playfair, Mr. Mundella, Dr. William Garnett, and Sir J. Norman Lockyer.

## SCIENTIFIC SERIALS.

*Wiedemann's Annalen der Physik und Chemie*, No. 6.—Diffusion constants of some metals in mercury, by G. Meyer. A dilute zinc amalgam was poured into two glass tubes, one of which was closed by fusion at the bottom, while the other was provided with a fine net of platinum wire, which did not allow the amalgam to penetrate. The latter tube was inserted in a beaker containing $H_2SO_4$. The two tubes were joined by a siphon. On passing a current through, with a kathode of platinum immersed in the beaker, the lowest layer of amalgam next the net was deprived of zinc and reduced to the state of pure mercury. The concentration of the top layer was estimated by the E.M.F. between the two tubes, and the rate of fall of the concentration was used to determine the diffusion constant of the zinc in the mercury. It was found to be 0·087 in cm.-hours for zinc, 0·065 for cadmium, and 0·057 for lead.—Electric vibrations in the Lecher system, by R. Apt. The author investigates the influence of the primary exciter in a Lecher wire system upon the form and intensity of the oscillations. There are nodes at the bridges over the secondary wires and at the spark gap. A maximum of intensity is obtained when the divisions of the secondary circuit are in resonance amongst themselves, and with the two divisions into which the primary system is divided by the spark gap. If the spark passes in a gas, the intensity varies directly with the pressure.—Kathode and Röntgen rays, by J. Precht. Goldstein's "canal rays," produced by perforating the kathode, are kathode rays which are not deflected by the magnet. They are distinguished from Röntgen rays by the absence of photographic and fluorescent actions. Röntgen rays have a condensing effect upon water vapour, and they increase the resistance of a selenium cell by 32 per cent. A portion of the rays proceeding from discharge tubes is not a wave motion, since the extent of absorption of the Röntgen rays by paper depends upon the duration of the radiation. Perhaps this part of the action is of a purely electrical nature. Röntgen rays show distinct interference phenomena, and are therefore partly due to some kind of wave motion.—Measurements of the interference of direct X-rays and others reflected at grazing incidence gave wave-lengths of 370 to 830 $\mu\mu$. These are near the limits of the visible spectrum, and since these rays are practically invisible they are probably longitudinal for the most part.—Arc lamps with amalgam terminals, by E. Gumlich. Arons has constructed an arc lamp in which the electrodes consist of mercury. If amalgams could be used instead, they might be made to yield an intense spectrum due to the body combined with the mercury. The author took special precautions to avoid oxidation during the filling process, and constructed a successful cadmium amalgam lamp which gives a brilliant red line. To avoid loss of light due to the opaque deposit round the kathode, the electrodes are placed in side tubes, and the light is projected down the main tube by a mirror.

No. 7.—Damping effect of a magnetic field on rotating insulators, by W. Duane and W. Stewart. The phenomena of damping of such bodies as sulphur and paraffin when rotating in a magnetic field, described some time ago by Duane, are, after all, found to be due to traces of iron. This can only be proved, however, by distilling these bodies five times and noting the absence of the damping. The latter will persist even after all chemical reactions have failed to indicate the presence of iron. The damping test is fifty times more delicate than chemical analysis.—Conductivity of carbon for heat and electricity, by L. Cellier. In metals the electric and thermal conductivities have an approximately constant ratio, if the specific heat per unit volume is taken into account. Measurements made with graphite, retort carbon, and various kinds of arc light carbons show that there is no correspondingly simple relation in the case of carbon. Whilst in the case of metals the ratio between the two conductivities varies between 0·07 × $10^6$ and 0·12 × $10^6$, it varies between the limits 1·8 × $10^6$ and 53·72 × $10^6$ in the carbons studied. The relation referred to seems, therefore, only to hold good for metals.—Magnetic deflection of kathode rays and its dependence upon the discharge potential, by W. Kaufmann. The extent to which kathode rays are deflected by a magnetic field is usually considered to depend upon the gas, the degree of exhaustion, and the dimensions of the tube. The author claims to have shown that all these conditions are only of secondary importance, and owe their influence exclusively to the fact that they affect the discharge potential between the anode and the kathode. The magnetic deflectibility is inversely proportional to the square root of the difference of potentials.—Determination of the period of electric oscillations, by Margaret E. Maltby. This paper contains the description of a new method for determining the ratio $v$ between the electrostatic and electromagnetic units. It is based upon the principle of the Wheatstone bridge. The capacities of the two halves of an electrometer form two branches, and a known capacity and a known resistance form the third and fourth branches respectively. The mean value of three series of measurements was 3·015 × $10^{10}$, which differs from the best results extant by an amount well within the errors of observation.—A relation between the electrical, chemical, and geometrical properties of a crystal, by J. Beckenkamp. The genesis of the electrical poles is connected with the chemical structure of a crystal. This is evidenced by such facts as that in aragonite treated with HCl, and in baryta treated with $H_2SO_4$, the directions of greater solubility are opposed to the positive direction of the electrical lines of force.

## SOCIETIES AND ACADEMIES.

### EDINBURGH.

**Royal Society**, July 19.—Lord Kelvin in the chair.—The President presented to the successful Fellows the prizes awarded by the Council, and in a few words described the nature and value of the work done by each.—Mr. J. W. Inglis read an interesting popular account of his experience of Indian earthquakes during a residence in that country of nearly twenty-five years.—Dr. C. G. Knott read a paper on relations among various types of magnetic strains. The first note dealt with the relation between the elongation in iron or nickel in a magnetic field, and the twist produced in the same, when, in addition, an electric current was passed through the material in the direction of the magnetisation. Data recently obtained were used in testing a formula, given by the author in a previous paper (*Trans.* R.S.E., vol. xxxv., 1888), for the twist in a cylinder under longitudinal and circular magnetising process. The striking characteristics of the twist phenomenon were reproduced, *e.g.* the maximum twist in iron occurring in a field lower than the field for maximum elongation, and the maximum twist in nickel, although in this metal there is no maximum or minimum point in the elongation-curve. The second note gave an account of experiments elucidating the character of the strain in a nickel tube when magnetised. There was a small but measurable diminution of volume produced in the material of the tube, and a (comparatively) large apparent diminution of volume indicated in the outer dimensions of the tube, when it was plugged up at both ends. The elongation in the direction of magnetisation having also been measured, the data were used to calculate the radial displacements, usually outwards, of the inner and outer surfaces of the tube. In a tube of external radius 1·39 c.m. and internal radius ·477 c.m. these displacements, in a field of 200, of the corresponding surfaces were 9·6 and 1·9, and in a field of 500, 14·7 and 2·3, the unit being $10^{-6}$ c.m. The probable nature of the strain at different parts was considered.—A very interesting paper, giving an account of the expedition from Edinburgh to observe the total eclipse of the sun on August 8, 1896, was read by Prof. Copeland and Mr. Ramsay.—The President then adjourned the meeting till November.

### DUBLIN.

**Royal Dublin Society**, June 16.—Prof. W. J. Sollas, F.R.S., in the chair.—Mr. J. R. Wigham described a new method of conferring distinguishing characteristics upon illuminating buoys and beacons for harbours, estuaries, and rivers.—Mr. Richard J. Moss read a paper on the cause of the death of fish in the Flesk River and Killarney Lake during the recent bog-flow in the County of Kerry.—Mr. William Barlow read a paper on a mechanical cause of homogeneity of structure and symmetry geometrically investigated, with special application to crystals and to chemical combination. This paper was communicated by Prof. W. J. Sollas, F.R.S.—Prof. D. J. Cunningham, F.R.S., gave a lantern demonstration of the deep origins of certain of the cranial nerves in the chimpanzee and orang-utan.—The following paper was omitted from the list of those read at the meeting of May 19: A spectrographic analysis of iron meteorites, siderolites, and meteoric stones, by Prof. W. Noel Hartley, F.R.S., and Mr. Hugh Ramage.

### PARIS.

**Academy of Sciences**, July 26.—M. A. Chatin in the chair.—The gnomon of the Observatory and the old values of the toise: recovery of the Picard toise, by M. C. Wolf.—Establishment of a uniform state in a pipe of circular section, by M. J. Boussinesq.—On the composition of drainage water, by M. P. P. Dehérain. The formation of nitrate from the nitrogenous stock in the soil, by the action of organisms, is greatly accelerated by moisture; hence the advantage of irrigation where possible.—Researches on the state in which elements other than carbon occur in cast iron and in steel, by M. H. Ad. Carnot and Goutal. A continuation of a previous paper. Manganese combines as far as possible with the sulphur and silicon, any excess being simply dissolved in the iron. No compound of copper or nickel appears to be formed. Chromium is present in combination with both carbon and iron. Tungsten forms a definite compound, $Fe_3W$; molybdenum, $Fe_3Mo_2$.—On the explanation of an experimental result attributed to a magnetic deviation of the X-rays, by Sir G. G. Stokes. Some remarks on an observation of M. G. de Metz. The X-rays, as a mode of vibration of the ether, are not susceptible of deviation by a magnet; the kathode rays, on the other hand, consisting of a stream of electrified particles, are affected by the magnet. The kathode rays, moreover, are stopped by an air layer, and will only be able to affect a fluorescent screen as the vacuum is increased. The experiments of M. de Metz find a very simple explanation in these facts.—On the toxicity of the perspiration of a healthy man, by M. L. Arloing. Experimental results are given clearly showing the toxic action of normal perspiration.—Remarks on the preceding paper, by M. Berthelot.—On phthalic green, its preparation and constitution, by MM. A. Haller and A. Guyot. The formation of this colouring matter by the action of zinc chloride upon dimethylaniline and phthalyl chloride, is shown to depend upon the presence of phthalyl tetrachloride ($C_6H_5.CCl_3.COCl$) in the latter. Starting with this tetrachloride instead of the dichloride, yields of from 60 to 95 per cent. of the colouring matter are obtained, the constitution of which is different from that assigned to it by its discoverer (Otto Fischer).—On a generalisation of the problem of representation in three dimensions, by M. Émile Cotton.—The natural rotatory dispersion of quartz in the infra-red, by M. R. Dongier. The experimental method used gives results of a higher order of accuracy than any previously recorded; and no formula for the rotatory dispersion of quartz deduced from theoretical considerations will include both the visible spectrum and these results for the infra-red.—On the transformation of the X-rays by metals, by M. G. Sagnac. Different metals exert a selective absorption upon the X-rays. At the same time, the surface layer of the metal emits new rays which are transmitted through mica, aluminium, and black paper with much greater difficulty than the X-rays themselves.—On the veiled appearance of photographs taken with the X-rays, by M. P. Villard. The effect produced is not due to rays which have traversed all obstacles, since it is obtained under really opaque substances. The fluorescence of the surrounding air appears to be the source of the second image, and great difficulties are encountered for this reason in the radiography of a thorax.—Action of the X-rays upon the temperature of animals, by M. L. Lecercle. The cutaneous and rectal temperatures are both modified in the same direction by the X-rays, the temperature being at first lowered, but afterwards rising.—Researches on the nickel-steels. Expansions at high temperatures, the electrical resistance, by M. C. E. Guillaume. The results obtained for the variation of the electrical resistance with temperature show that this cannot be considered as a simple consequence of the expansion.—On the spectrum of the lines of carbon in fused salts, by M. A. de Gramont.—Relation between the polymerisation of liquid substances and their dissociating power upon electrolytes, by M. Paul Dutoit and Miss E. Aston. An experimental study of the electrical conductivities of some salts dissolved in propionitrile, acetone, methyl-ethyl-acetone, methyl-propyl-acetone, and nitroethane, all of which may be considered as polymerised liquids.—On a new group of amidines, by M. Fernand Muttelet.—On a method of estimating acetylene, generally applicable to hydrocarbons of the formula $R.C \vdots CH$, by M. Chavastelon. By the action of acetylenic hydrocarbons upon an aqueous or alcoholic solution of silver nitrate, two molecules of nitric acid are set free for each molecule of acetylene absorbed. The estimation of such hydrocarbons is thus reduced to a simple titration of an acid.—On the estimation of lime, aluminium, and iron in mineral phosphates, by M. L. Lindet.—On the absorption of oxygen in the decolorisation of wine, by M. J. Laborde.—Influences exercised by the pathological state of parents upon their descendants, by M. A. Charrin.—Bacteriological study of ambergris, by M. H. Beauregard.—The persistence of the activity of rennet at low or high temperatures, by MM. L Camus and E. Gley.—On a new form of the buccal apparatus of the Hymenoptera, by M. J. Pérez.—On a new Myxosphoridia of the family of Glugeidea, by M. Louis Léger.—On the carboniferous ground in the neighbourhood of Mâcon, by M. A. Vaffier.—On the marcasite of Pontpéan, and on the regular grouping of marcasite, pyrites, and galena, constituting pseudomorphs of pyrrhotine, by M. A. Lacroix.—On some new applications of the oscillating current in electric therapeutics, by M. le Dr. G. Apostoli.

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## PAMPHLETS AND SERIALS RECEIVED.

PAMPHLETS.—Royal Gardens, Kew: Hand-List of Tender Monocotyledons, excluding Orchideæ, cultivated in the Royal Gardens, Kew, 1897 (London).—The Mammoth Cave of Kentucky: Dr. Hovey Call (Louisville, Morton).—The Birds of Colorado: W. W. Cooke (Fort Collins, Col.).—Report of the International Meteorological Conference, Paris, 1896 (Eyre).

SERIALS.—Journal of the Royal Statistical Society, June (Stanford)—Journal of the Chemical Society, July (Gurney).—Morphologisches Jahrbuch, 25 Band, 2 Heft (Leipzig, Engelmann).—Bulletin of the Natural History Society of New Brunswick, Vol. xv. (St. John).—L'Anthropologie, May and June (Paris, Masson).—Notes from the Leyden Museum, January and April (Leiden).—Natural Science, August (Dent).—Zeitschrift für Physikalische Chemie, xxiii. Band, 3 Heft (Leipzig, Engelmann).

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