THURSDAY, APRIL 13, 1876

A RESEARCH FUND FOR THE CHEMICAL SOCIETY

AN offer has lately been made to the Chemical Society, which has for its object the establishing of a fund to be applied in aiding the development of scientific chemistry. The offer comes from Dr. Longstaff, who proposes giving the society the sum of 1,000*l*., on condition that at least an equal amount be raised and invested together with his gift, in approved security. To make this offer known to the Fellows of the Chemical Society, the following circular has been sent to them, and already the major part of the required 1,000*l*. has, in donations varying from 1*l*. to 100*l*., been raised.

Chemical Society,
Burlington House, Piccadilly, W., March, 1876

Dear Sir,

I am instructed by the President and Council of the Chemical Society, to request your consideration of a matter, which they hope will elicit your interest and active co-operation.

The advancement of Chemical Science, which constitutes the special object of the Chemical Society, may be promoted chiefly in two ways :—

1. By facilitating the early acquirement by students of Chemistry of a knowledge of the results of chemical research carried on in this and other countries.

2. By affording direct assistance to workers in chemical science, with a view to encourage and facilitate their labours in experimental research.

The Chemical Society has sought from the time of its foundation to aid in the first of these objects by the publication of original papers communicated to the Society ; and, during the last five years, a special guarantee fund and liberal aid from the British Association for the Advancement of Science have enabled the Society to establish, on a firm footing, the publication of monthly abstracts of original papers published in this country and abroad, on chemistry and allied branches of science.

The Chemical Society has also recently endeavoured, as far as its funds would permit, to afford assistance to chemists undertaking original investigations, by the extension of its library, and occasionally by grants of small sums of money, when pecuniary aid was applied for. The limited resources of the society have, however, restricted the number and amount of these grants within very narrow limits. In 1872, Mr. T. Hyde Hills placed at the disposal of the Chemical Society the sum of 10*l*. as the nucleus of a fund for promoting original research ; and offered under certain conditions, made with the object of securing the co-operation of others, to contribute a like sum annually. This attempt of Mr. Hills to form a research fund was not, at the time, seconded. The Council have, however, recently received from Dr. G. D. Longstaff, one of the original members, the generous offer to place at the disposal of the society, the sum of one thousand pounds (1,000*l*.) towards establishing a permanent fund for promoting the advancement of Chemical Science, on the condition that not less than an equal amount be subscribed for the same purpose.

As the President and Council feel that such a fund would add much to the usefulness of their Society, and enable them to encourage still further the prosecution of Chemical Science, they are most desirous to secure to the Society the benefit of this munificent offer of Dr. Longstaff; and have therefore instructed me to ask your co-operation towards the attainment of this object. I remain, Yours obediently,

WILLIAM J. RUSSELL, *Treasurer*.

Considering the use to be made of this fund, and the very large number of persons interested either in the scientific development of chemistry, or else in its many lucrative applications, it is not unreasonable to expect that a sum far larger than this 2,000*l*. will be raised.

The feeling among those most interested and most active in raising this fund, is that there should be, in the first place, a permanent fund, the dispensers of which should have large discretionary power as to how the income is to be spent, so long as it be strictly used for the advancement of scientific chemistry, and secondly, that this fund should be aided by annual subscriptions. It is hoped that the invested capital may from time to time be largely increased by gifts ; possibly as the real character and object of such a fund as this becomes better understood and more widely known, it may receive legacies and bequests such as now are devoted to charitable or religious purposes.

The direct income arising from the money which is to be invested in accordance with Dr. Longstaff's conditions cannot amount to any large sum ; it is therefore hoped, and not without reason, that many will aid the fund by annual subscription, and while some may probably limit their subscriptions to a definite number of years, others may be willing to subscribe as long as they are assured that good and useful work is being done with the money thus raised.

A fund of this compound character has certain advantages ; the invested capital gives permanency and keeps alive the interest in it, the subscriptions help most materially towards forming an income which will more nearly approach the requirements of the case, and there are many who would rather that what they give should be used for immediate requirements than that it should be funded for the benefit of unknown and possibly never-to-exist requirements in time to come.

One point has been urged against such a fund as this ; it is that we should look rather to the State to aid research than to private generosity, and that every private fund of this kind tends to relieve the Government of some of its obligations, whereas all scientific bodies ought rather to increase such obligations, not lessen them. On the other hand the State aid, if it is to be really efficient, must be such that it will at least to some extent open out a career for those willing and able to devote themselves successfully to original investigations ; while from this fund no more could be expected than that it should afford a means by which some of the best known chemists of the day, as represented by the governing body of the Chemical Society, should have within their power the means of aiding particular investigations, publishing possibly important scientific tables or other data, and making important acquisitions of books or even instruments if such be specially required by the fellows of their Society in the prosecution of scientific investigation. Undoubtedly it is beyond all question that the sphere of usefulness of the Chemical Society will be much extended by the establishment of this research fund. W. J. RUSSELL

TAIT'S "RECENT ADVANCES IN PHYSICAL SCIENCE"

Recent Advances in Physical Science. By P. G. Tait, M.A., formerly Fellow of St. Peter's College, Cambridge, Professor of Natural Philosophy in the University of Edinburgh. (London : Macmillan and Co., 1876.)

THESE lectures, we are told in the Preface, were given in the Spring of 1874, at the desire of a number of the friends of the author—mainly professional men—who wished to obtain in this way a notion of the chief advances made in Natural Philosophy since their student days. The demand, therefore, which these lectures are intended to meet, is that of men who, though they have received a liberal education, in which the element of science has not been neglected, are too deeply engaged in their professional work to keep themselves abreast of contemporary science by regular study, but who are yet able to avoid falling behind by occasionally availing themselves of an hour with a scientific friend.

In lectures of this kind, therefore, we are not to look for the elaborate exposition and reiterated inculcation by which the facts and methods of science are impressed upon the minds of beginners. Still less are we to expect the forcible language and striking illustrations by which those who are past hope of being even beginners may be prevented from becoming conscious of intellectual exhaustion before the hour has elapsed. We are rather to listen to one who has climbed high on the hard and slippery peaks of science as he points out the grand features of the prospect to those who stand on a lower level but yet on the same solid foundation with himself.

In his own words he has "to point out a few of the principal peaks which we have to ascend, and of the more formidable abysses which we have to avoid."

As safety must come before success, it may be well to observe that the most formidable of all these abysses is that of *à priori* physics. The study of this branch of science as it is to be found in the works of Hegel, Herbart, and others, seems to furnish an unfailing source of recreation to those who are engaged in the less amusing researches of experimental physics. In our modern examinations those candidates who try to conceal their ignorance by sending up what appears to them to be the most plausible answer to a question, often help to relieve by their felicitous absurdities the tedious labours of the examiner. We have only to imagine that instead of the weary examiner we have the vigorous man of science, and instead of the timorous candidate, some great philosopher before whose inner vision the whole world of being and not-being, in its apparent contradiction and fundamental indifference, lies open; and we shall then have some faint idea of the mode in which the writings of these philosophers may be destined to contribute to the merriment, if not to the happiness of the coming race.

We are glad to find, however, that in spite of the contempt which Prof. Tait pours upon the *à priori* physics of non-experimental philosophers, he admits that there is a true science of metaphysics which discusses the fundamental ideas of all science and knowledge, not by shutting out all the facts of experience, but by calling in all the evidence obtainable from the whole circle of the science.

In fact one of the greatest benefits which the advance of science has conferred on the world at large is that words and phrases have been gradually introduced into ordinary language which are consistent with true scientific ideas, and that these have displaced words and phrases which implied false ideas about nature, so that each generation, as it learns its mother tongue, finds it better adapted to express what really exists, and less suggestive of what is not.

We have only to read the expositions of science in the seventeenth century to see that they are addressed to students whose minds were imbued with prejudices and superstitions which are now known only to archæologists. Those who have the good fortune to be born in these latter times can hardly realise the reasons why certain natural phenomena rather than others were dignified or stigmatised with the name of paradoxes.

The man of science, if he confines himself to writing scientific books, can influence only the professed students of science, but if he can find an audience among men of business and men of action, who desire to keep up their scientific knowledge, he will at the same time help them to keep up a scientific habit of thought and expression. In this way a course of lectures like those of Prof. Tait may do something towards infusing a scientific spirit into the affairs and phraseology of business life, and since it is of the essence of science to speak of things as they are, the business phrases which satisfy this condition will gradually but surely displace those which describe things as they are not.

The subjects discussed in these lectures are the conservation, transformation, and dissipation of energy, spectrum analysis, the conduction of heat, and the structure of matter.

The experiments by which the lectures are illustrated are many of them new, and all of them well described. We may take the following as an example of Prof. Tait's method of illustrating the connexion of radiation with absorption:—

"I can show to a few at a time, but not in a marked way at a distance, the same phenomenon [as in Stewart's experiment with pottery], by taking a piece of platinum foil and writing letters upon it with ink. When it is once heated there is a deposit, on the surface of the otherwise polished platinum foil, of oxide of iron which tarnishes the surface and makes it absorb considerably more light than a polished reflecting surface will do. We should expect, then, when this is heated (as I now heat it in a powerful but very slightly luminous flame), and becomes in turn the source of light, to see bright letters on a dark ground. The difference of brightness is not so marked in this case as in the last, but still those who are nearest to me will see the phenomenon distinctly enough.

"But you will see another phenomenon still more startling on looking at the back of the heated foil instead of the front of it. You see faint traces of bright letters on the dark ground when I turn the inked side to you, but when I turn the other side you see dark letters on a bright ground. Now, the reason why on the one side we have bright letters on a dark ground, while the other side of the same piece of metal shows dark letters on a white ground, is still more confirmatory of the result of Balfour Stewart's experiment, which I have just stated, because these letters appear dark while at present cold, because they are absorbing more than the rest of the polished surface. They appear brighter than the polished surface when heated, because they radiate more; but just because they radiate more they must become colder—must be kept permanently colder than the rest of the foil, and therefore the parts at the back of the foil, behind those which are radiating most, remain permanently colder. This is made evident when we look at the side which is without any difference of surface, as we then see, by the relative amounts of brightness, a marked distinction between the parts which are hotter and those which are colder. This is a still more complete proof of Stewart's proposition."

We may also notice Prof. Tait's exposition of his views with respect to the nature of comets and the origin of their light. He considers these bodies as consisting of swarms of distinct meteorites, which are illuminated partly by the light of the sun, but which also give out a light of their own arising from the numerous and violent collisions which are always taking place, especially near the nucleus where the swarm is densest. The most remarkable fact about this light is that discovered by Huggins, namely, that its spectrum is identical with that of a hydrocarbon.

We are sorry, however, not to find in this volume any exposition of that theory of Prof. Tait's concerning the development and manifestation of the tails of comets to which Sir W. Thomson referred in his presidential address to the British Association in 1871 as "Tait's beautiful sea-bird analogy." A "tactic arrangement" of brickbats extending over millions of miles would perhaps account for what at present appears from a dynamical point of view most paradoxical in the behaviour of comets' tails, but the dynamical explanation of this tactic arrangement itself seems still to remain as a desideratum of the theory.

J. CLERK MAXWELL

DR. BALL ON SCREWS

The Theory of Screws; a Study in the Dynamics of a Rigid Body. By R. S. Ball, LL.D., F.R.S., Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland. (Dublin: Hodges, Foster, and Co., 1876.)

DR. BALL has published several papers of late years on the subject here treated. The present volume contains the substance of these papers re-cast, with additional matter, and with a greatly improved terminology.

It has been shown by the combined labours of Poinsot, Chasles, and Möbius, that there is a perfect mathematical identity between the composition of forces and couples on the one hand, and of rotations and translations on the other. Every small movement of a rigid body consists of rotation round a definite line combined with sliding along it, in other words, consists of a *twist* on a definite screw, and every system of forces applied to a rigid body is reducible to a force along a definite line, together with a couple round it. The force is the analogue of the rotation, the couple is the analogue of the translation, and the combined action of the force and couple is called by Dr. Ball a *wrench* on a screw. In each case the screw consists of the definite line (fixed in position but unlimited in length), associated with a definite length, called the *pitch*, namely, the quotient of the translation by the rotation, or of the couple by the force. The *amplitude* of a twist is the magnitude of the rotation; the *intensity* of a wrench is the magnitude of the force. A twist, or its analogue a wrench, is defined by six numbers, one of which may be the amplitude or intensity, and the other five will then be common to all twists and wrenches on the same screw. A screw is therefore defined by five numbers.

When a body twists while acted on by a wrench, the work done by the latter is the continued product of the amplitude of the twist, the intensity of the wrench, and a third factor called the *virtual co-efficient* of the two screws. The virtual co-efficient is a symmetrical function of the two screws, and when it vanishes the two screws are called *reciprocal.* In other words two screws are said to be reciprocal if a wrench on one of them does no work in virtue of a twist on the other.

Taking six screws at random in space, we can express any twist as the resultant of six twists, one on each of these screws, and the amplitudes of the six components are called the *screw co-ordinates* of the resultant. Wrenches can be expressed in the same way, *intensity* being substituted for *amplitude.*

If we take five screws and combine arbitrary twists upon them, we obtain an infinite number of resultant twists on an infinite number of resultant screws. These resultant screws constitute a *screw-complex* of the 5th order; and in like manner we may have complexes of lower orders down to the 2nd.

Given a complex of the 5th order, one definite screw can be found which is reciprocal to it, that is to say, which is reciprocal to every screw contained in the complex. One practical application of this theorem is, that if a rigid body has freedom of the 5th order (or one degree of constraint), one definite screw can be found such that a wrench upon it can do no work on the body. Given any complex of the nth order, there is one definite complex of the $6 - n$th order that is reciprocal to it, in the sense that every screw of the one complex is reciprocal to every screw of the other.

In general problems on the dynamics of a rigid body, it is usually advantageous to select the six screws of reference, so that each of them shall be reciprocal to the other five. Such screws are called *co-reciprocals.*

For example, in discussing the action of wrenches on a body which has freedom of the 4th order (two degrees of constraint), four of the co-reciprocals should be selected from the complex which defines the freedom, and the other two will then of necessity define that other complex of the 2nd order which is reciprocal to it and includes every wrench that can be exerted by the constraints. When an applied wrench is resolved into its components on these six co-reciprocals, the first four components determine the movement of the body, the other two being completely destroyed by the reaction of the constraints.

It is not to be imagined that a wrench applied to a free body tends in general to make it twist on the same screw on which the wrench lies. If, however, we take six screws coinciding two and two with the principal axes at the centre of mass, and having pitches $\pm a$, $\pm b$, $\pm c$, where a, b, c denote the radii of gyration round these axes, these six screws will possess the property in question—an impulsive wrench on any one of them produces an instantaneous twist on the same. Dr. Ball calls these the six *principal screws of inertia* for a free body, and he further shows that a constrained body has a smaller number of such screws, the defect from six being equal to the number of degrees of constraint.

Again, he shows that if a body has freedom of the nth order, and has a position of stable equilibrium under the action of forces which have a potential, n screws (called *harmonic* screws) can be found possessing the following property, viz., that the body can execute small oscillations on any one of them, and will execute such oscillations if it receive an arbitrary displacement and initial twist-velocity upon it. Also that any possible small oscillations of the body can be resolved into n independent oscillations on the harmonic screws.

We have said enough to indicate the richness of the ground which Dr. Ball has broken, and we would commend his treatise to the careful attention of mathematicians.

We could wish that a sharper line had been drawn between real and imaginary solutions, and also between results that are only true for screws of finite pitch and those that are true without this restriction. We also think that the convention which Dr. Ball proposes (footnote, p. 11) for removing ambiguity from the expression for the virtual coefficient, is defective, partly because he has overlooked the fact that the virtual coefficient of two screws is essentially signless until positive as distinguished from negative directions have been arbitrarily selected along them. When this selection has been made, the virtual coefficient is the value of the quaternion expression $-pS\alpha\beta + S\alpha\beta\gamma$, where p denotes the algebraical sum of the pitches, α and β unit-vectors parallel to the two selected positive directions, and γ the vector perpendicular from the screw α to the screw β. To express the same thing unambiguously without quaternions would require such a long specification as would weary the patience of our readers.

The value of Dr. Ball's book is enhanced by an appendix containing a very clear and interesting *résumé* of the literature of the subject, from Poinsot downwards. We may supplement this list by a reference to §§ 200, 201 of Thomson and Tait's "Treatise on Natural Philosophy," where one degree of constraint is shown to be reducible to the condition that "every longitudinal motion of a certain axis must be accompanied by a definite proportion of rotation about it." This comes very near to the indication of the one reciprocal screw by which such constraint may be defined.

J. D. E.

OUR BOOK SHELF

Mittheilungen aus dem k. Zoologischen Museum zu Dresden. Herausgegeben mit Unterstützung der Generaldirection der k. Sammlungen für Kunst und Wissenschaft. Von Dr. A. B. Meyer, Director des königl. zoologischen Museums. 1 Heft mit Tafel i.-iv. (Dresden: Verlag von R. v. Zahn, 1875.)

THERE can be no question that the establishment of a journal in connection with a scientific institution is one of the very best methods of promoting the interests of the latter and obtaining for it more extended support. While the institution remains in one place, its journal travels about the world, makes its most recent acquisitions known to its supporters and correspondents, and encourages them to promote its welfare by further donations. Such being the case, Dr. Meyer has acted most wisely in endeavouring to resuscitate the somewhat decayed zoological branch of the Royal Museum of Dresden, by starting the present periodical. Dr. Meyer's recent travels and discoveries in the Eastern Archipelago have brought him much and deserved credit, to which, no doubt, he partly owes his present appointment. They have likewise supplied him with abundant materials for contributing valuable memoirs to his journal. Not unnaturally, therefore, the first number of the new periodical commences with papers containing the results of some of his own researches in the Eastern Islands. The first of them contains an account of a new Bird of Paradise, not actually discovered by Dr. Meyer himself, but by one of his correspondents since his return to Europe. *Diphyllodes Gulielmi III.*, as this splendid bird is named, in honour of the King of Holland, is said to be from the little known Papuan island of Waygiou, and vies in brilliancy of plumage and elaborate excess of feathered ornaments with the finest species of this gorgeous family. Descriptions of other novelties in the class of birds discovered by Dr. Meyer himself, together with additional notes on little known species, complete this interesting memoir. Another paper by Herr Kirsch contains descriptions of new beetles from Malacca, from a large collection sent by Herr Eichhorn to the Royal Museum, and a third, which will be of special interest to our anthropological friends, is devoted to an account of 135 Papuan skulls obtained in New Guinea and Mysore by Dr. Meyer himself. We observe that a second part of the *Mittheilungen* is announced for publication early in the present year, so that we may expect shortly to have an opportunity of bringing further labours of Dr. Meyer and his assistants to the knowledge of our readers.

Table of British Sedimentary and Fossiliferous Strata. By Henry William Bristow, F.R.S., F.G.S., Director of the Geological Survey of England and Wales. The Description of Life Groups and Distribution by R. Etheridge, F.R.S. Second Edition, revised. (London: Edward Stanford.)

THIS is an admirable and evidently very carefully prepared table, which is well suited for the use of students, science classes, and schools. In it Mr. Bristow has managed to embody a vast amount of information, which could only be obtained and verified by the consultation and comparison of a great number of maps and documents; and for this service all engaged in teaching the science of geology are greatly indebted to him. The foreign equivalents of the British rocks are only given in such cases as that of the Trias, in which our own series is incomplete. Mr. Etheridge's contribution to the work consists in a very valuable palæontological digest, in which the order of succession of the different forms of plants and animals is clearly described. The only points which seem to call for critical remark in this excellent work is the use of the term *Laurentian* for the so-called "Fundamental Gneiss" of Scotland, and the manner in which the name of Cambrian is employed. There is absolutely no evidence whatever whereby the geologist is able to correlate the azoic rocks on the opposite sides of the Atlantic, and therefore the application of the term Laurentian to any British formation would scarcely appear to be justifiable. In the long-vexed question as to the boundary between the Silurian and Cambrian systems, we regret to find Mr. Bristow adopting the extreme views of the late Sir Roderick Murchison, and confining the name of Cambrian to a few almost unfossiliferous rocks quite at the base of the series. The line of division at the top of the Tremadoc slates, which was adopted both by Lyell and Phillips, has the advantage of making the British Cambrian system, as now defined by Hicks, very closely agree with the "Primordial" of Barrande; and we hope that in a future edition of this table, which we doubt not will soon be called for, the author may see his way to the adoption of it.

Catechism of Chemistry. New edition by Robert James Mann, M.D., &c. (London: Edward Stanford, 1876.)

PERHAPS no better illustration of the truth of Pope's line—

"For fools rush in where angels fear to tread"—

can be cited than the method employed in the manufacture of many of the so-called scientific elementary textbooks, a series of which *must* be put forth by every publisher. The "Catechism" before us is a typical example of a book constructed on this most pernicious method. The author appears to have learned a little chemistry from works which were in vogue a quarter of a century ago, and to have tacked on to this knowledge a smattering of the more recent development of the science. Being thus completely furnished, he has entered the arena. The result is altogether what might have been expected. Inaccuracy of statement, meaningless definitions, sins of omission and of commission, abound. If anyone wishes to learn the rudiments of chemistry, let him eschew this catechism as he would poison.

M. M. P. M.

Summer Holidays in Brittany. By Thomas J. Hutchinson. With Map and Illustrations. (London: Sampson Low and Co., 1876.)

MR. HUTCHINSON is well known, among other things, for his researches among Peruvian antiquities, and therefore, to his tour in Brittany he brought a trained observation. He has managed to write a very pleasant book on rather a worn subject, a book which is likely to give its readers a desire to follow the author's example. Indeed it might form a very useful guide-book for the district traversed by Mr. Hutchinson, and would have the advantage of being much more pleasant reading than guide-books generally are. Mr. Hutchinson evidently made good use of his time when in Brittany, and to those who have not read much on the subject the book will furnish a great deal of information on the nature of the country, the characteristics and manners and customs of the people, the antiquities, historic and prehistoric, the ecclesiastical and political history, and many other interesting points. A very good map and some fair illustrations add to the value of the book.

LETTERS TO THE EDITOR

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

Colour of Flowers Grown in the Dark

IN NATURE, vol. xiii. p. 348, Mr. Thiselton Dyer gives an extract from Sachs's "Text-book of Botany" to the effect that no change is produced in the colour of flowers by growing them in the dark. This led to a letter from Mr. J. C. Costerus (vol. xiii. p. 427), calling attention to the results obtained by Askenasy, published in 1876, who found that some kinds were changed in colour, and some not changed. In the autumn of 1873 I made a number of experiments on this subject, and published a short account of them in the same year in the *Quarterly Journal of Science*, vol. iii. p. 474. I came to the same conclusion as Askenasy has come to, and was also able to establish some important generalisations. I will only mention a few special instances. I was unable to cause any change in the colour of the common Orange Lily (*Lilium aurantiacum*), whereas I found that a very considerable change was produced in the case of *Erysimum Peroffskianum* by only a moderate degree of darkness. This may perhaps be owing to the fact that the orange tint of these two kinds of flowers is due to entirely different substances. That of the lily is due to what I have called orange xanthophyll, whereas that of the other flower is due to a much less stable compound, giving an entirely different spectrum, met with also in the orange marigold, and therefore named by me *Calendula xanthine.* Comparing together the mixed colouring matters found in an equal weight of the petals, I found that the amount of the *Calendula xanthine* was only half as great in the petals grown in the dark, whereas the more yellow constituents were reduced only to three-quarters, so that the general colour was more yellow. I found that a similar change could be produced in the case of the marigold. If shaded when the flowers are somewhat grown, the total colour may be very considerably reduced without there being any material change in the ratios of the different colouring matters, whereas when grown in the dark from a very small bud, the ratios are changed, as in the case of *Erysimum.* Growing flowers in the dark seems to stop the normal development to a greater or less extent according to the nature of the colouring-matters, the effect being the greatest in the case of those substances which are the most easily decomposed. We thus find what appears at first sight to be a very unlikely result, viz., that those constituents which, when dissolved out from the petals, are the most easily discolorised by exposure to light, are formed in relatively greater amount when the flowers are grown in the light, which is easily explained if we assume that a higher vital power, depending on the presence of light, is necessary to overcome the more powerful chemical affinities of the less stable compounds.

H. C. SORBY

The Ash Seed Screw

MR. STEPHEN WILSON remarks, in his note on this subject (NATURE, vol. xiii., p. 428), "Why the seed generally becomes twisted as it dries is a very interesting question. But what seems to me the most remarkable fact about this phenomon is, that in every case, and in all trees alike, the thread of the screw is in one direction." He also alludes to the uniformity in the direction of torsion in the awns of two species of oat. Torsion of all kinds occurring in plants is usually assumed to be due to unequal longitudinal tension (see Sach's "Handbook," p. 770). In a paper read before the Linnean Society, March 16, I pointed out that the uniformity in the *direction* of the torsion cannot be thus accounted for; and a totally different explanation was given of the twisting and untwisting of the awns of certain fruits (*Avena elatior* among the number) when they are dried and moistened. It was shown that the power of torsion resides in the individual cells of which the awn is constructed, and tha it is by their combined action that the awn, as a whole, becomes twisted in drying. It appeared to me extremely probable that the same explanation would hold good for the twisted wing of the ash fruit. I therefore boiled one in nitric acid and chlorate of potassium, by which means the woody tissue is separated into its constituent cells. These were then teazed out on a slip of glass and thoroughly dried over a lamp, and it was found that many of them had become twisted on their axes; and, which is important, that they were all *twisted in the same direction as the fruit itself.* This artificial drying represents the natural drying process which occurs during the ripening of the fruit. In both cases contraction and consequent torsion result from the loss of water, but in the natural process the cells not being free to twist independently, are compelled to combine in producing that torsion of the whole fruit which we are considering. It is interesting to find the same principle holding good in the case of the ash screw as in that of the awns of various Gramineæ and Geraniaceæ, and the twisted tails of the achenes in *Anemone montana.* Moreover, I strongly suspect that the principle of the torsion of an organ being dependent on the twisting of its constituent cells is capable of wider extension, so as to embrace the torsion of the stems of twining plants, &c. This subject I hope immediately to investigate.

FRANCIS DARWIN

P.S.—The samara of the sycamore is a more efficient parachute than that of the ash, but the wing has no appreciable twist, and there is no uniformity in the direction of rotation assumed as the fruits fall to the ground.

Down, Beckenham, April 4

The Animal of Millepora

IT is a remarkable fact that during all the discussions on the late L. Agassiz's statements regarding the animal of Millepora some very careful drawings of it have been in the possession of Major-General Nelson, R.E. They were done by himself during his residence at Bermuda at the time when he was writing that communication to the Geological Society on the reefs and general structure of the islands which has made *Lieut. Nelson, R.E.*, a name of mark.

In common with most naturalists, I had expected that soon after the *Challenger* reached Bermuda, we should have had a satisfactory description of this very interesting polyp, so that the truth, or the contrary, of Agassiz's description could be tested. But it was not until July in last year that any communication relating to the subject was sent off from the *Challenger*, the paper being read on Nov. 25, 1875, at the Royal Society. Mr. Moseley noticed therein that the examination of Millepora is beset with serious difficulties, he, however, states that there are large and small polyps, and that both kinds have tentacles, and "they appear to be four in number, and compound." He observes: "they are simply retracted by means of muscular fibres which are arranged round the base of the cylindrical stomach radially, and that no mesenteries have been seen." Mr. Moseley was naturally dissatisfied with these poor results, and hoped to do better things at Hawaii. In the meantime I became aware of the value of the drawings I have already alluded to, and as I am at work on several subjects with Gen. Nelson, I sent a communication and the drawings to the "Annals and Magazine of Natural History" before the evening of April 6, when Mr. Moseley's paper on Millepora was heard by me at the Royal Society. It is a satisfaction for me to be able to state that Gen. Nelson's drawings prove that Agassiz saw a part of a polype, and that Mr. Moseley's beautiful delineations, far in advance of all, testify to the correctness of my fellow-worker. I do not credit the hydroid nature of the polyp now, any more than I did when writing the reports on the British fossil corals, and I believe Millepora to be an Actinozoan.

P. MARTIN DUNCAN

The Use of the Words "Weight" and "Mass"

THE relations between weight and mass, gravity and acceleration, are so well defined in all good treatises on dynamics, that it appears superfluous to dwell on these questions. But as it has been stated by Prof. Barrett, vol. xiii., p. 385, that the C. G. S. system of units has been introduced into the course of Mechanics in this College, I may be permitted to say that the system actually employed is not that referred to by your correspondent. I generally employ the kilogramme, metre, and second, and sometimes the foot, pound, and second, to measure a dynam or unit of force. The dynamometers alluded to as about to be exhibited at the Loan Exhibition of Scientific Instruments at South Kensington are suitable to the former system, and I use them for the measurement of dynams in kilogrammetres. One of these dynamometers is graduated for every 200 grammes up to 100 kilogrammes, the other for every 100 grammes up to 10 kilogrammes, and they cannot be depended on for results within the tenth of a kilogramme. Spring dynamometers, though suitable for the large units employed in mechanics, are totally unfit for measuring units on the C. G. S. system. I concur with Prof. Everett, in his book on this system, when he says :—"A spring balance, it is true, gives a direct measurement of force, but its indications are too rough for purposes of accuracy" (p. 8). Spring dynamometers are therefore unsuited to a system where the units are measured by $\frac{1}{981}$ of a gramme, or about $\frac{1}{63}$ of a grain, as in the C. G. S. system.

HENRY HENNESSY

Royal College of Science for Ireland, Dublin

The Physical Constitution of Steam

I BELIEVE the following remarks on the physical constitution of steam are in some degree original, in form at least, though perhaps not in substance.

Dr. Andrews has shown by his experimental researches on carbonic acid, that at a temperature above 31° C. or 88° F. the difference between the gaseous and the liquid states no longer exists. I quote the following brief statement from an admirable paper on the subject, by Prof. James Thomson, in the "Proceedings of the Belfast Natural History and Philosophical Society," 1872 ;—he is speaking of the case in which a given quantity of carbonic acid, of which part is in the gaseous and part in the liquid state, is kept at constant volume, while the temperature, and consequently the pressure, are gradually increased :—

"As the temperature and pressure are augmented, the gaseous part is always increasing in density, and the liquid part is diminishing in density, till at last the two come to have the same density with one another, and then they are perfectly alike in every respect, all distinction between them having vanished. At this stage the temperature is 31° C., and the pressure is about seventy-five atmospheres. Above this temperature of 31° no change of pressure can cause gasification or liquefaction ; and above this pressure of about seventy-five atmospheres, no change of temperature can cause gasification or liquefaction."

This temperature of 31° C. is called by Dr. Andrews the critical temperature for carbonic acid. Above its critical temperature, although carbonic acid may have the density either of a gas or of a liquid, the two states are not sharply separated from each other as a liquid is from its vapour, but graduate into each other insensibly. It is believed that every gas and vapour has its own critical temperature. Those of the permanent gases are believed to be so low as to be unattainable by any known process. That of water or steam, on the contrary, is probably too high to be observed in a glass tube, and consequently too high to be directly observed at all : for the only known test of the critical temperature being attained, consists in the disappearance of the visible boundary surface between the liquid and the vapour or gas. My purpose is to show how the critical temperature for steam may be approximately estimated with a great degree of probability.

The fact that the latent heat of steam diminishes as the temperature increases, formerly seemed to me one of the strangest of all facts ; but the above-mentioned properties of carbonic acid, and no doubt of all gases and vapours, make it quite intelligible.

The *latent heat* is defined as the heat given out when steam is condensed into water of its own temperature. The *total heat* is defined as the heat given out when steam is condensed into water at zero Centigrade, and is the sum of the latent heat and the temperature. According to Regnault, the relation between temperature and total heat is expressed by the formula—

$$\lambda = 606{\cdot}5 + {\cdot}305\,t.$$

λ being the total heat and t the temperature. This has been ascertained to be true from 0° to 230°, and if it is true for all temperatures, at a temperature of 872·7 the total heat and the temperature would be the same, and the latent heat would vanish : 872·7 is consequently the critical temperature which is deducible from the above formula for water.

Old Forge, Dunmurry, Co. Antrim

JOSEPH JOHN MURPHY

Coloured Solar Halos

IN the interesting letters of Drs. Schuster and Frankland I note some remarks on the rarity of the apparition of complete halos about the sun in this country. Had I read these remarks some six years ago I should have passed them by without surprise. It so happened, however, that my attention was drawn to the subject of halos, coronas, &c., by Kämtz's "Lehrbuch der Meteorologie" about the year 1869, and I at once began to examine the sky near the sun every fine day, and note down any appearance of halos, fringes, &c. When I began, my impression was that I should rarely see the solar halo seeing that it had escaped me for several years. However, I soon found out my mistake, and the subjoined list, compiled from the observatory note-books, gives the number seen each month in 1874 and 1875. No doubt, several escaped my vigilance in some months and a few in others. The figures, at any rate, show that the phenomenon is by no means rare.

Solar Halos in 1874 *and* 1875.

	1874.	1875.		1874.	1875.
Jan.	0	2	July	3	4
Feb.	1	2	Aug.[1]	1	—
March	1	2	Sept.	2	4
April	11	4	Oct.	0	2
May	8	1	Nov.	3	0
June	11	2	Dec.	1	0

	Jan.	Feb.	March.
1876	1	0	8

Had I been quite sure that Dr. Schuster's remarks referred to the ordinary solar halo, this letter would have reached you a week ago or more. I will only add that the halos I have observed are nearly always complete (when the sun is high enough), that they are often very bright and most striking phenomena, and that the radius is usually about 22°.

Bermerside, Halifax, April 6

JOSEPH GLEDHILL

"The Effect of the Sun's Rotation and the Moon's Revolution on the Earth's Magnetism"

THE above was the title of an article by J. Allan Broun, published in NATURE, vol. xiii. p. 328.

The establishment of these facts, if they have not already been published, will aid amateur investigators very much in arriving at satisfactory conclusions in regard to the phenomenon above referred to.

First, from the revolutions of the sun, do the positively defined edges of sunspots always reappear at the same moment, and are their relative positions with regard to each other ever the same through a series of years, giving them a fixed and positive character, both as to position and time of revolution ?

Second : At what times, with regard to the position and area of the largest and most numerous sunspots, and whether they are hidden by the revolution of the sun, or face the earth, is the

[1] I was from home most of this month in both years.

greatest effect observable in the differences of the earth's magnetism?

Third: Are the sun-spots caused by clouds or curtains outside, and hiding the apparent surface of the sun, or are they deep cavities through the same? STEPHEN W. ALLEN

Boston, U.S.A., March 27

Metachromism

I REFRAIN from replying to Mr. Petrie's second letter (p. 426) until after the appearance of the article to which Mr. Costerus refers (p. 427) on "Organic Colour Change."

In defence of Miller, however, I would just add that on p. 298, vol. ii. (fifth edition), occur the following words:—"The sodic dioxide, Na_2O_2, obtained by igniting sodium in oxygen is of a *pure white colour*." WM. ACKROYD

Royal Coll. of Chemistry, South Ken ington, April 3

Dr. Klein on the Small-pox of Sheep

I WRITE this note in order to inform you that, my attention having been directed to some alleged fallacies in some of my observations regarding the small-pox of sheep, I am at present engaged in reinvestigating the subject. E. KLEIN

The Brown Institution, April 11

OUR ASTRONOMICAL COLUMN

OLBERS' SUPPOSED VARIABLE STAR NEAR 53 VIRGINIS.—The only comet detected in the year 1796 was found by Olbers in Virgo on the night of March 31. On the following evening, at 8h. 55m., apparent time at Bremen, it was over a star of the seventh magnitude south—following 53 Virginis, and the light of the star was remarked to be little affected by the intervention of the comet. On March 1, 1797, desiring to fix more exactly the place of this star, Olbers found in its position one of only the tenth or eleventh magnitude, whereas in April previous, according to Schroeter, who appears to have compared the comet with it early on the morning of April 2, it was the brightest star in the immediate neighbourhood of 53 Virginis, and hence in Olbers' judgment "a seventh magnitude at least." Writing to Bode in March 1797 he directs attention to this star, as perhaps a more remarkable variable star than even χ Cygni. The circumstances preclude suspicion of a similar phenomenon to that described by Piazzi when the great comet of 1811 passed over his star XX. 197.

From the positions of the supposed variable, and its neighbours given by Olbers (who also appends a diagram), it is evident that his star, which followed 53 Virginis 30′ 55″ in R.A., 20′ 45″ to the south, is No. 12,728 of Oeltzen's Argelander, a star observed 1851, April 24, and noted of the eighth magnitude. For 1876·0 its place is in R.A. 13h. 7m. 32s., and N.P.D. 105° 53′·7.

Approximate mean places for 1797, March 1, of several stars with which Olbers compared the one in question were:—

	Olbers' Magnitude.	R.A. ° ′	N.P.D. ° ′
(*c*)	9	194 40	106 6
(*a*)	7	194 44	105 24
(*d*)	11	196 15	105 11

The star (*c*) is Lalande 24,421, called by him 8½; (*d*) is L. 24,597, noted 9; but the star (*a*) is not found either in Lalande or Argelander. Its position in Olbers' diagram corresponds to the place above assigned. What is its present magnitude, or is there some mistake about its position?

On April 1, 1796, the supposed variable was considerably brighter than the star (*a*), according to Olbers; in March 1797, much fainter than (*c*) and only slightly brighter than (*d*); he remarked no change in March, April, and May. Bode says, on April 24 and May 12 and 20 of the same year he saw it as a 9·10. In March 1855 it was fully eighth magnitude or 7·7.

THE APRIL METEORS.—As the moon will be absent during the nights of the 19th and 20th of the present month, a watch may be advantageously kept for meteors which are supposed to move in the path of the first comet of 1861 discovered by Mr. Thatcher, of New York, on April 4. At the descending node this comet makes a remarkably close approach to the earth's annual track, the definitive orbit calculated by Prof. Oppölzer, showing that at this point the distance between the two orbits is only 0·00232 of the earth's mean distance from the sun, or 214,000 miles; less, therefore, than the moon's mean distance from the earth. The elements are elliptical with a revolution of 415 years, and this form of orbit we may assume with much probability to have been occasioned by a near approach of the comet to the earth at some distant epoch. The descending node is passed 22½ days before perihelion passage, and to bring the comet into closest possible proximity to our globe, it is necessary that the perihelion point should be passed on May 12. Had this been the case in 1861, the comet would have occupied the following positions on its descent towards the plane of the ecliptic:—

	R.A. °	N.P.D. °	Distance from earth.
March 24·0	269·2	57·4	0·700
April 1·0	270·3	57·0	0·494
„ 9·0	271·3	57·1	0·281
„ 17·0	285·2	59·4	0·065

The true dimensions of the orbit of this comet will be defined by the following numbers, which are in units of the earth's mean distance from the sun.

Semi-axis major	55·676
Semi-axis minor	10·083
Semi-parameter	1·826
Perihelion distance	0·921
Aphelion distance	110·430

PROF. HUXLEY'S LECTURES ON THE EVIDENCE AS TO THE ORIGIN OF EXISTING VERTEBRATE ANIMALS [1]

IV.

THE crocodiles form the highest group of existing reptiles; they are higher than lizards as a steam-vessel is higher than a sailing-ship; for, while built essentially on the same lines, and exhibiting altogether the same fundamental structure, they are in some respects peculiarly modified, and that always in the direction of greater complexity.

Besides the characters of the skull mentioned in the last lecture, they are distinguished from lizards by having a four-chambered heart, one in which the separation of the ventricle into two distinct cavities is completed, so that, in the heart itself, the blood from the lungs is kept separate from that returned from the body generally. A mixture, however, takes place subsequently, through an aperture between the two aortæ, one of which springs from each ventricle.

Crocodiles are found in Central America, India, Africa, and Australia. Of the many species, the greater number are short-snouted; the fish-eating Gavial of the Ganges, on the other hand, has an extremely long and narrow snout.

All the existing crocodiles are fresh-water or estuarine animals, but, fortunately, this was not the case with the ancient forms, many of which were exclusively marine, seeming, so to say, to take the place, in the sea of their own epoch, of our porpoises and dolphins.

Besides Tertiary species, crocodiles are found in the Chalk, Oolite, Lias, and Trias often in the best possible state of preservation; they therefore extend back to the very commencement of the Mesozoic epoch.

[1] A course of six lectures to working men, delivered in the theatre of the Royal School of Mines. Lecture IV., March 20. Continued from p. 430.

If we had specimens of all known forms of crocodiles, recent and extinct, and set to work to classify them according to their degrees of likeness and unlikeness, we should find that they naturally fell into three series.

In the first of these it would be found that the skull had all the characters mentioned at the end of the last lecture, the posterior nares being small apertures opening into the cavity of the mouth behind the pterygoid bones; the vertebræ would be concave in front and convex behind; the two bones composing the shoulder-girdle, the shoulder-blade or scapula and the coracoid, would be similar in shape, both being long and narrow; in the hip-girdle, the haunch-bone or ilium would be much cut away in front and excavated below, the ischium and pubis being both long blade-like bones; and there would be seven or eight longitudinal rows of bony plates on the back.

In the second set we should find the posterior nares to be much larger and placed farther forwards, immediately behind the palatine bones, the pterygoids not uniting as in the first group. The vertebræ would be slightly hollowed out at each end. In the shoulder and hip girdles there would be no important difference from the first group, with which also the more minute structure of the limbs would correspond closely. A difference would, however, be found in the fact of there being not more than two rows of plates on the back.

In the third series, we should notice certain very striking changes. The posterior nares would be actually as far forward as in a lizard; neither the palatine nor the pterygoids uniting in the floor of the mouth; the vertebræ would be completely amphicœlous or biconcave; the coracoid no longer long and narrow, but expanded and rounded like that of a lizard; the ilium more elongated and without the notch on its lower edge; and the ischium considerably broadened. As in the preceding group, the rows of bony plates on the back would not exceed two.

Thus we should find that the second group held an exactly intermediate place between the first and third, and that the third set, in every respect in which it differed from the normal crocodilian structure, approached to that of lizards.

It is a very interesting point to see how these three groups appear in time. We should find that in the first are included all the Recent and Tertiary forms, and that there are no indications of the type below the later Cretaceous.

The second group would be found to extend from the older Cretaceous down to the Lias; moreover, a careful examination would show that there were lesser modifications among the individual species of a very instructive nature; those from the Wealden, for instance, would be seen to have the posterior nares farther back (*i.e.*, nearer the typical crocodilian position) than those of the middle Mesozoic, and these again than those of the Lias.

The third group would contain exclusively Triassic forms, such as the dragon-like *Belodon* and the *Stagonolepis* of the Elgin sandstones. In this latter formation the fossils are in a very curious condition; after the sand accumulated round the bodies of the Triasssic animals had hardened, water, percolating through the porous rock, completely dissolved out the bones, leaving nothing but cavities. Thus we have only the remains of remains to deal with, but casts taken from the cavities enable us to make out, with perfect certainty, even important characters, although there may be hardly a bone left.

We see then, that our third set of forms is the oldest, our first the youngest, and the study of crocodilian remains seem to show that that has happened in the history of crocodiles, which should have happened, if the theory of evolution be true. Anatomical characters show that crocodiles are a modification of the lacertian type, and to this type the Triassic species, from which we are certainly justified in supposing that existing forms are descended, exhibit a marked approximation.

Still we are very far from knowing the whole story: it is certainly allowable to assume that our third group of crocodilian forms was evolved from a common stock with lizards, but this is as far as the facts of the case will take us at present.

There seems, at first sight, to be something unnatural in speaking of birds and reptiles together, for no two animals can be, to all appearance, more unlike. The wonderfully constructed feathers of the one group, compared with the scutes and scales of the other, the cold blood of the reptile contrasted with the hot fluid which circulates through the vessels of a bird and raises its body several degrees above our own in temperature; the dumbness and general sluggishness of the reptile as compared with the vocal powers and the rapid flight of birds; all these compel us to say, and justly so, that nothing can be more different than the character of the two classes.

Even when we go more into details, similar differences are apparent. The bird has a small head, set on a long flexible neck, and provided with a horny beak in lieu of teeth; its bones are hollow and full of air; its breast-bone, instead of being a small plate of cartilage, is a huge bony plate, usually provided with a large keel for the attachment of the powerful muscles of flight; the fore-limb is of no use in progression on the ground, and, the body having to be supported entirely by the hind limbs, the femora are placed parallel to the long axis of the body, instead of almost at right angles to it as in a reptile, so that the body is well raised from the ground, and a gait the very opposite of a reptile's sprawling waddle is the result.

The scapula and coracoid are not so very different from the corresponding bones in the lower class; the humerus, ulna, and radius, can also be perfectly well identified, but the modification of the distal division of the limb—the part answering to the reptile's fore-paw or to our own hand—is very great. First come two small bones answering to carpals, then three longer ones all united together, which represent the metacarpus, and are followed by the rudiments of the phalanges of the three corresponding digits. In the ostrich two of these three "fingers" are terminated by claws, the use of which it is rather hard to divine, unless the bird uses them for scratching itself, an operation in which a very large portion of the activity of the lower animals is taken up.

The haunch-bone, or ilium, is of enormous size, and extends a long way in front of, as well as behind, the acetabulum; in correspondence with this, a great number of vertebræ are fused together to form a sacrum of sufficient size for the attachment of the ilia and the support of the weight of the body. The ischium and pubis are long slender bones, and the latter, as well as the former, is bent back, so that they both come to lie nearly parallel with the vertebral column.

To allow of the femur taking up its position parallel with the axis of the body, its well-finished globular head is set on at right-angles to the shaft; moreover, its further end has a characteristic notch for the reception of the upper extremity of the fibula. The shin-bone is provided with a large and very characteristic crest for the attachment of the strong muscles of the anterior part of the thigh; its lower extremity is pulley-shaped, and, in a young bird, the pulley-like end, continued into a tongue of bone running up the back of the tibia, can be separated as a perfectly distinct ossification; its shaft also is so twisted that its two ends come to lie in different planes.

Following upon the tibia comes a bone with an easily separable piece at its upper end, and showing signs of a longitudinal division into three separate bones; this is the tarso-metatarsus, and represents the metatarsals and all the tarsals except one—the astragalus—which is represented by the pulley of the tibia. As a rule there are four toes, three of which are turned forwards and articulate with the tarso-metatarsus, while the fourth, the representative of our hallux or great toe, is turned backwards and articulates with a small distinct bone.

The heart has four perfectly distinct chambers, so that the pure blood from the lungs, and the impure blood from the rest of the body, are kept quite separate. There is a single aorta which turns to the right side after leaving the heart.

(*To be continued.*)

ON SAFETY MATCHES

THE fact has been known during some years past that the so-called safety matches, which are warranted to ignite "only on the box," can be fired by being rubbed on glass, and as Mr. Preece recently pointed out (NATURE, vol. xiii. p. 208), on ebonite. I find that they can be ignited by friction against ivory (I used an ivory paper-knife), steel (a steel spatula, somewhat worn), zinc, copper, marble with the polish worn off, and a freshly-cleaved surface of slate.

The match (or two matches together, for the sake of strength) should be held near the tipped end, and then be rubbed with strong friction, and with a long sweep upon the solid surface. From two to twelve such sweeps may be required before the match ignites, and the result seems to be due to the conversion of mechanical work into heat sufficient to fire the paste at the end of the match, which, I suppose, consists mainly of potassic chlorate and sulphide of antimony.

After a few rubs the match begins to crackle, and then suddenly bursts into flame. A similar result may be obtained by grinding the chlorate in a mortar with a little sulphur or sulphide.

The readiness with which the match ignites by friction depends greatly on the nature of the surface. Lead is too soft, and tin too smooth. The metals produced by rolling have a sort of skin on the surface, over which the match glides without sufficient friction, but if the surface of zinc be rubbed with sand-paper or with a fine file, it becomes active in firing the match. I noticed that the polish of my ivory paper-knife became worn before it acted well. Nor is it very easy to fire the match on glass. A long sweep repeated about a dozen times with considerable pressure seems to be necessary. The two specimens of sheet copper used by me have a sort of grain which is favourable to the success of the experiment. The copper acted equally well whether the surface was dirty or cleaned with dilute sulphuric acid. After rubbing a match ten or twelve times on zinc, without effect, the same match rubbed on copper immediately took fire.

In the case of slate, lead, tin, and some other surfaces, the composition on the match acts as a polish, and thus renders it unfit for ignition. On the other hand, a finely-cut file removes the composition from the end of the match without igniting it.

I have no doubt that many other surfaces might be found on which the safety matches would ignite with greater or less difficulty. Notwithstanding this, the match is still a safety match, although it does not comply with the conditions asserted twice over on the box. It does not ignite readily on any of the surfaces pointed out except copper and marble, but it does ignite with wonderful facility when rubbed against the side of the box, an invention so ingenious that a few words of its history may not be out of place here.

About the year 1850 a gentleman entered the laboratory of King's College, London, and drew from his waistcoat pocket a fragment of a rough-looking red solid, and, placing it in the hands of Prof. Miller, asked him if he knew what it was. It was handed round among those present, but no one had the slightest idea as to its nature, when, to the astonishment of every one, the gentleman said, "It is phosphorus—amorphous phosphorus, discovered by me, Herr Schrötter, of Vienna."

Up to this time, and indeed for some years later, persons engaged in the manufacture of lucifers were subject to a terrible disease, known in the London hospitals as "the jaw disease;" necrosis of the lower jaw induced by constantly inhaling the fumes of phosphorus acid escaping from the phosphorus of the paste with which the matches were tipped.

Ordinary matches made with phosphorus were, during many years, dangerous contrivances. They were luminous in the dark, liable to ignition on a warm mantelpiece, poisonous; children have been killed by using them as playthings; and, moreover, they absorbed moisture, and became useless by age.

But the chief inducement in getting rid of ordinary phosphorus and substituting the new variety was to put an end, as far as possible, to the jaw disease. The red, or amorphous phosphorus, gave off no fumes, had no smell, was not poisonous, and the matches made with it were not luminous in the dark; they did not fire on a warm mantelpiece, did not contract damp, and would keep for any length of time. A manufacturer, in 1851, sent me several samples of matches made with red phosphorus. I found some of these matches the other day, and they were as active, after twenty-five years, as at first.

But here was a difficulty. When the red phosphorus is brought into contact with potassic chlorate a slight touch is sufficient to produce an explosion, in which the red phosphorus reassumes its ordinary condition. Many attempts were made to form a paste, and many accidents and some deaths occurred in consequence. Prizes and rewards were offered by manufacturers and others for a safe paste, or for some means of using the red instead of the ordinary phosphorus, but without success, so that the patent for the manufacture of red phosphorus, which was secured by Mr. Albright, of Birmingham, in 1851, threatened to be of but little value.

At length the happy idea occurred to a Swedish manufacturer not to attempt to make a paste at all with the red phosphorus, but to make the consumer bring the essential ingredients together in the act of igniting the match.

Mr. Preece's suggestion that the ignition of the matches is due to electricity, may be dismissed in the face of the following experiment:—Place a few grains of red phosphorus on a hard surface together with some powder or a crystal of potassic chlorate, when a gentle tap will cause them to burst into a flame. C. TOMLINSON

NOTES FROM SIBERIA

THE following Siberian notes are furnished me by a Polish gentleman resident at Irkutsk. The dates mentioned follow the Old System, as in Russia, and are twelve days behind our own dates. The letter is dated the 10th of February. My informant says:—

"Some time ago Mr. Czckanofski returned from his second expedition to the most northern parts of Siberia by the Olensk River. He went as far as its mouth, and the extraordinarily warm autumn gave him the opportunity to make very interesting explorations. Till the month of September there was no frost nor snow, and the sea not frozen. The same is reported by Mr. Neumann, who returned lately from the Behring Strait. It may be that these exceptional climatical conditions allowed also Mr. Nordenskjöld's entering the mouth of the Jenessei. The exploration in the Achinsk country of a cavern situated now some thirty fathoms above the bed of the river gave to Mr. Tskersky[1] a fine collection of well-conserved fossils of extinct species. Mr. Tskersky occupies himself now with the description of the Tunka Alps, which he believes to be the former boundary of Lake Baikal, as he found there the fossils of the crab and

1 The Curator of the Museum at Irkutsk.—G. F.

seals.[1] Dr. Dybowsky passed a year on the Ussour River, and brought a beautiful collection of birds, fish, and quadruped skeletons. His descriptions are sent to the Berlin Museum. Now he is occupied upon the Baikal with soundings and observations on the Baikal seals. He wishes to write a monograph on this particular species of seals. This is nearly all that was done last year, as far as expeditions are concerned, in this part of the world."

My correspondent refers me to the proceedings of the Siberian Geographical Society for further details. It is much to be regretted that this publication, as well as the excellent Calendar of Eastern Siberia are so little known, out of Asia. I further learn that earthquake shocks have been felt at Irkutsk on the 4th of September last, at 2.55 A.M., and a slight one on the 4th of January. The first one came from the east. A clock which was secured by screws almost an inch long, was left leaning on one side, and both of the screws drawn completely out of the wall. The Baikal district is the spot in Northern Asia which is most visited by earthquakes.

Since I am on the subject of Siberia, I may mention two facts of considerable interest which I learnt last year. I was told by inhabitants of Jenesseisk that in the regions to the north of that town the compass is of no use during an auroral display. It is not at all unlikely that this should be the case in a country where auroral effects are intense, and the horizontal component of the earth's magnetism is small. The other interesting fact is that Mr. Muller had reached Gauss' Siberian magnetic pole, where he found the needle vertical. This was shortly before I reached Irkutsk. His observations were to be published in the Proceedings of the Siberian Geographical Society. I do not know whether a translation has been published. GEORGE FORBES

Andersonian University, Glasgow, April 4

[1] Lake Baikal is remarkable, among other things, for the presence of these marine animals. The seals are grey, and have a very coarse fur. I took a photograph last summer of one which was in the Museum at Irkutsk. —G. F.

THE DUBLIN SOCIETIES

WE have recently referred in several articles to the efforts which are being made to introduce a more satisfactory organisation among the various scientific institutions in Dublin, which have hitherto been independent of each other. It appears now to be proposed not merely to unite museums, but to unite into one body the Royal Irish Academy and the Royal Dublin Society. This project would seem to have originated at a meeting which a deputation from the latter body had with Major Donnelly. It is evident that many difficulties would be removed and many advantages result from the amalgamation of these two societies. Of course the arrangements for such an amalgamation must be carried out entirely by the societies, though it would no doubt tend to forward such a scheme if the societies were assured of the approval of Government, and of such aid towards taking the necessary steps as the Government has in its power to give.

We understand further that there is some possibility of an amalgamation of the Royal Agricultural Society of Ireland with the Royal Dublin Society. It is most desirable that such an amalgamation should be effected, and that the agricultural shows should be removed from the present buildings beside Leinster House to the Phœnix Park.

From a letter which has been published in the Irish papers, it appears that these points have been submitted by Major Donnelly to Lord Sandon, who has informed him that the Government are prepared to aid the amalgamation and to give the necessary space in the Phœnix Park.

Should the amalgamation be effected, it would probably take the form of a new Society with a limited number of Fellows, ordinary members, and an Agricultural Section.

It is possible enough that some of the members of the Royal Irish Academy may object to the proposed change, on the score that they would thus lose caste. We cannot admit the validity of such an objection. The Academy has no doubt done good work, but it has a large number of members on its roll who are no more entitled to any scientific or literary distinction than the general body of the members of the Royal Dublin Society. If, however, the Academy consent to the proposed change, the Fellowship of the new Society would become a high and much-coveted honour, and the reputation of the whole body would be far higher than that of the separate societies is now. As to the objection that the large body of general members are unfit to select Fellows, we think that the Fellows may very well be entrusted with the selection of Fellows; the first Fellows under the new charter might be, say Fellows of Trinity College, Dublin, Professors and ex-Professors of a College or University, and others with similar positions, who should be empowered to choose their successors. There need be no difficulty, while acting with perfect fairness and openness, in choosing for the inner circle and also for the governing body the best men of the new society, men who would make a point of maintaining its honour and dignity. The Royal Irish Academy would thus become, under a new name, a select body of Fellows chosen for their scientific and literary merit; in time, indeed, this fellowship might come to be regarded as an honour little inferior to that of F.R.S.

The union of the societies would remove many difficulties as to ownership of property, and would place at their disposal a much larger amount of funds for scientific and literary work than they at present possess. Indeed, it appears to us that from the union on the proposed basis, nothing but good could result, great benefits to the members, and much greater advantages than at present exist for the promotion of science in Ireland. Since government has promised to aid the United Society as far as possible, we think it would be a pity if any petty spirit of local jealousy should raise obstructions to the accomplishment of a scheme which promises so well for the country.

GERMANY AND THE LOAN EXHIBITION

THE German Committee for the London Loan Exhibition of Scientific Apparatus has addressed a report to the Crown Prince and Crown Princess of Germany on the success of their efforts. It results from this document that 311 German exhibitors will be represented by 2,492 objects. The 19 classes will be represented as follows:—

1. Arithmetic by	6	exhibitors and	11	objects.
2. Geometry by	13	,, ,,	35	,,
3. Measurement by	59	,, ,,	126	,,
4. Kinematics by	10	,, ,,	308	,,
5. Molecular Physics by ...	23	,, ,,	33	,,
6. Sound by	10	,, ,,	24	,,
7. Light by	35	,, ,,	118	,,
8. Heat by	19	,, ,,	41	,,
10. Electricity by	32	,, ,,	194	,,
11. Astronomy by	25	,, ,,	78	,,
12. Applied Mechanics by ...	13	,, ,,	64	,,
13. Chemistry[1] by	32	,, ,,	389	,,
14. Meteorology by	18	,, ,,	49	,,
15. Geography by	29	,, ,,	110	,,
16. Geology by	22	,, ,,	118	,,
17. Mineralogy by	19	,, ,,	39	,,
18. Biology by	54	,, ,,	289	,,
19. Educational Collections by	5	,, ,,	441	,,

The space claimed by the exhibitors will be 109 square metres floor, 442 square metres repositories (tables, &c.), 299 square metres wall. Considering that two months had to suffice for bringing together this collection, that manu-

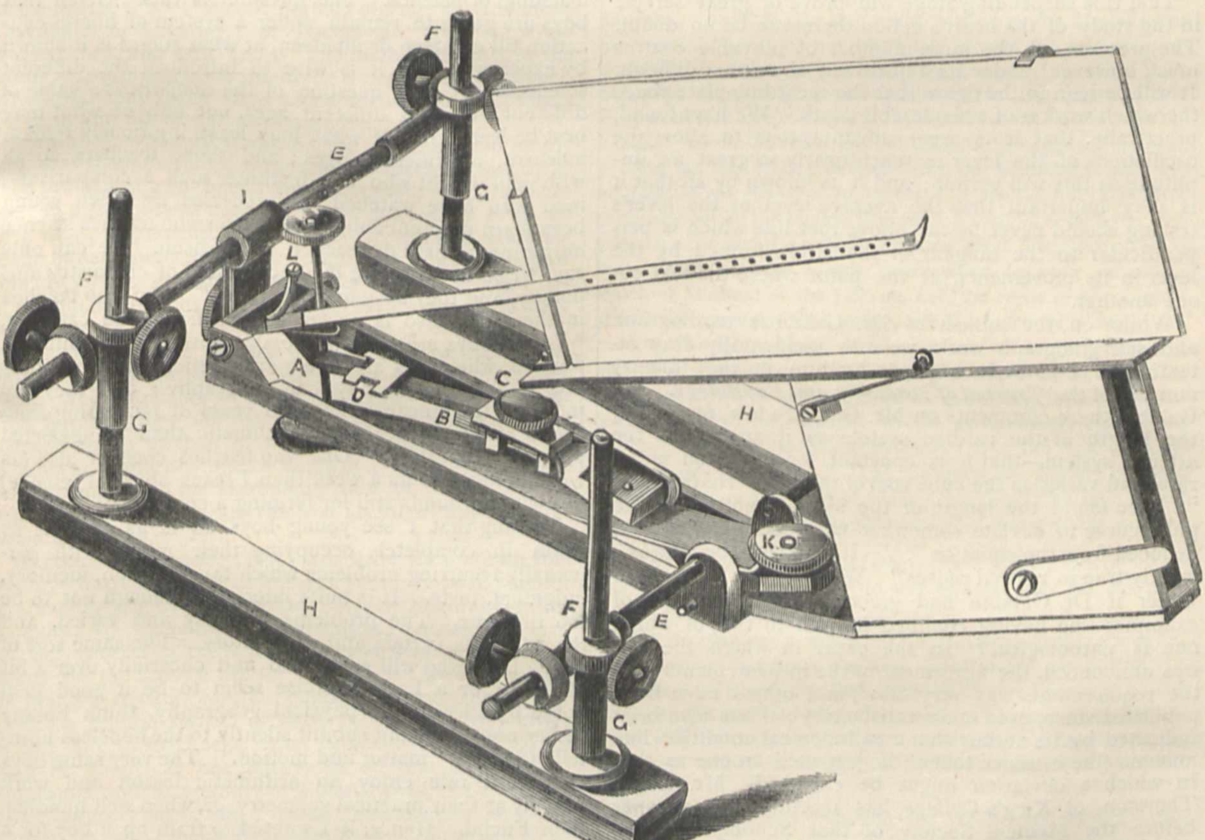
[1] Exclusive of the collective exhibition of the German Chemical Society, which will represent about 40 exhibitors with 300 objects.

facturers are beginning to feel indifferent with regard to exhibitions, that the Vienna Exhibition in the past and the Philadelphia Exhibition in the present years, have been absorbing their energies, the Committee think that they have reason to be contented with the results obtained. This view is strengthened by comparing the above numbers with those of 127 exhibitors only who represented German science at Vienna. The Committee express themselves greatly obliged for the assistance given by the Lord President of the Council of Education, the Duke of Richmond and Gordon, the Vice-President, Viscount Sandon, the Director of the South Kensington Museum, Mr. Cunliffe Owen, and to the Government and Officers of the German Empire and of Prussia, notably to the Ministers of Education, Dr. Falk, of Commerce, Dr. Achenbach, of War, General von Kamecke, of Marine, to the head of the General Staff, Count Moltke, to the Postmaster-General, Dr. Stephan, and also to the Royal Library, to the Royal Academy of Science, and to the German Chemical Society. The Committee conclude by claiming the assistance of the German Empire for the production of a systematic and critical report on the scientific treasures of all nations that will be exhibited in London.

ON A MODIFIED CARDIOGRAPH

DR. A. L. GALABIN, whose investigations with the sphygmograph and cardiograph we have had the opportunity of noticing on former occasions (*vide* NATURE, vol. xii. p. 275), has introduced a modification of the cardiograph, a woodcut drawing of which, through the kindness of the Council of the Royal Medico-Chirurgical Society, we are able to reproduce from their "Transactions."

The cardiograph of Marey is too well known to require description; suffice it to say that it depends for its action on the transmission through air-filled tubes of movements from one stretched elastic membrane to another. In it, therefore, errors originating in the tubes are introduced; and these, from practical experience, are found to be considerable. More than one physiologist has obtained far more satisfactory "cardiograms" by applying the sphygmograph, which was originally constructed by its inventor—M. Marey—for the purpose of recording the movements of the pulse at the wrist, upon the chest-wall, in the intercostal spaces. This instrument, when thus



applied, reproduces in a most faithful manner the movements of the chest-walls as there produced by the subjacent heart in action; and in the healthy subject any accessory apparatus is rarely needed for the satisfactory production of the tracings.

In many pathological conditions, and in the healthy subject when the cardiac movements are more than ordinarily powerful, the movements of the heart are transmitted to the neighbouring ribs, on which the sphygmograph has to be supported, as well as to the more yielding intercostal tissues. Under these circumstances it is far better to employ, as supports for the instrument, more fixed points, which must, from the nature of the chest-wall, be at some distance from the centre of cardiac movement. Dr. Galabin's apparatus supplies us with the means needed. It is an expanded framework constructed in a manner which allows of its being firmly applied to a considerable expanse of the irregularly-shaped chest. From the drawing its principle can be best understood (See Figure).

In the middle of the figure the sphygmograph is seen. It differs from M. Marey's original in one or two minor details, which are decided improvements. The most important of these is that the brass bar A B, on which the knife-edge by means of which the recording lever is set in motion is fixed, can be varied in length; and this makes it possible to vary the magnifying power of the lever, because the distance of the knife-edge from its axle can be changed. Such an addition has always been a desideratum, even in the wrist sphygmograph. The screw, C, clamps the two component parts in any desired position. A second reserve knife-edge, D, can also be turned up to replace the ordinary one, A, when the cardiac action is extraordinarily forcible. By the screw, K, the compress-spring is fixed. L is the secondary spring, which prevents the recording lever from quitting the knife-edge; it can be thrown out of gear when not required.

The supporting bars are seen at H H; they replacing the side-lappets of the original instrument. On them are fixed uprights, G G G, on which again are attached by screw-clamps two transverse bars for the suspension of the sphygmograph. That to which the clock-work end is joined can only be moved upon the uprights with which it is connected. The other has an additional sliding-piece, I, that allows of the screw-pad portion being independently raised or lowered in a hinged manner.

That this suspending stage will prove of great service in the study of the heart's action there can be no doubt. The presence of the large number of movable centres must, however, render its adjustment somewhat difficult. It will be seen in the figure that the recording plate above the watch-work is of considerable depth. We have found, practically, that it is *never* advantageous to allow the oscillations of the lever to reach nearly so great an amplitude as this will permit; and it is known by all that it is very important that the average level of the lever's tracing should never be far above that line which is perpendicular to the tangent of the circle formed by the lever in its movements, at the point where the two cut one another.

Whilst on the subject of Dr. Galabin's cardiograph and sphygmograph work, we may incidentally draw attention to a point in a paper by him in the January number of the *Journal of Anatomy and Physiology*. Dr. Galabin there comments on Mr. Garrod's law respecting the length of the cardiac systole as it appears in the arterial system—that it is constant for any given pulse-rate, and varies as the cube root of the rate. He remarks, "I have found the length of the systolic portion of the pulse-curve to deviate somewhat considerably from that deduced from the equation. . . . It appears to be approximately true in normal pulses." Would it not have been better if Dr. Galabin had given a larger number of examples—he having confined himself to two, of which one is pathological? In the paper in which the law was announced, the agreement of the measurements with the requirements was very close, and others have been published since, even more satisfactory; it has also been indicated by its author that a pathological condition, like anæmia (the instance taken), is just such an one as that in which a deviation might be expected. Mr. Edgar Thurston, of King's College, has recently read a paper before the Medical Society of that School, which is quite in confirmation of the law as originally stated, from a considerable number of observations on *healthy* subjects.

PHYSICAL SCIENCE IN SCHOOLS

DR. WATTS quite puzzles me. I can see no contradiction between the passages from my essay of 1867 and my letter of 1876, which he silently places in juxtaposition. What I said in 1867 was (p. 261) that "science should be introduced into a school, beginning at the top and going downwards gradually to a point which will be indicated by experience." What I say in 1876 is that experience shows, as far as I can judge, that it is not generally wise to go down very far; that one soon comes to a point at which the loss in teaching science counterbalances the gain. I am quite as sure as ever I was of the value of science in schools, in its right place.

I think that those who advocate the teaching of science to young boys scarcely realise the difficulty of establishing their ground. Some, like Prof. Roscoe (p. 387), admit, when pressed, that it is a question which experience alone can decide, and that they have not had that experience. Liebig, to whom Mr. Gerstl refers (p. 431), was speaking of a different class of schools, in which boys must pick up some useful scientific facts early or not at all. Prof. Henslow's experience is of the same kind. Other philosophers, charmed with the bright intelligence of children when talked to by a Faraday or a Frankland, straightway pronounce an opinion on the relative value of science and classics and mathematics in the early part of a liberal education,—on somewhat insufficient grounds.

The question that this discussion began with was the merits or demerits of the Certificate Examination, in so far as it affected science in schools. That seems to be settled. We have drifted now into a different and most useful discussion on the results of experience in the early teaching of science. The question is this. Given that boys are going to remain under a system of liberal education till eighteen or nineteen, at what stages is it shown by experience that it is wise to introduce the different sciences? It is a question of the *comparative* value of different studies at different ages, not only of what may best be learnt, but of what may least injuriously remain unlearnt, at different ages; and those teachers speak with real weight who can institute such a comparison; men who have watched the processes by which young boys learn different subjects. A man who teaches science only cannot institute such a comparison. He can only say, "I *do* teach young boys something of chemistry and botany, and they *do* gain something." One who teaches mathematics also is so far better off that he can say, "Young boys are more (or less) attentive, active-minded, diligent, when they are doing arithmetic, than when they are at a lesson on physical geography; and they are more (or less) incapable in later years of recovering from the ill effects of neglected arithmetic than of neglected physical geography." One who teaches classics also (as I do for more hours a week than I teach physical science) has wider grounds still for forming a comparison.

Nothing that I see young boys do is as efficient as Latin in completely occupying their minds with perpetually recurring problems which tax attention, memory, judgment, taste. It is quite interesting enough not to be too tiresome. The problems are easy and varied, and the solutions certain and satisfactory. The same sort of young boys who will work hard and cheerfully over a bit of Cæsar or a Latin exercise seem to be a good deal bored by a lesson on physical geography, think botany rather nonsense, and submit silently to the hopeless unintelligibility of "matter and motion." The very same boys will as a rule enjoy an arithmetic lesson and work happily at their practical geometry, or, when well handled, their Euclid. Hence, if I wanted to train up a boy for a scientific career, I would not begin very early with science, but wait till he was thirteen or fourteen.

I admit that the experience of some others is against me. Mr. Tuckwell (p. 412) speaks warmly, and pronounces my opinion to mean nothing more than that I myself have failed to teach science to young boys. This is a mistake. It means that I have seen the work of others, here and elsewhere. It means not an absolute failure, but a comparative failure, as explained above. It means a summary of the opinions of a considerable number of other men. Mr. Wyles (p. 455) is against me, although he has "never been satisfied with his science teaching." Mr. West (vol. xiii. p. 48) is against me, and his opinion is a valuable one. But it must be remembered that it is not a personal question, which admits of the simple solution that Mr. Tuckwell can teach science and that Mr. Wilson cannot, but a general one: can science be taught to young boys by the rank and file of science teachers, who are, or will be when they are numerous, neither more nor less able and enthusiastic than the rank and file of classical teachers, very average sort of people? I do not doubt for a moment that my old pupil West can teach little boys science with great advantage, but I doubt very much whether there exist fifty Wests as schoolmasters at any one time in England; and to justify making his practice universal we want to be certain of finding five thousand or fifty thousand such men as teachers. Let it be remembered that very dry and dull men teach classics, and not very badly, while the same men would teach a science class nothing, or worse than nothing.

I wish Dr. Farrar, of Marlborough, would give us his opinion on this whole question. He has had unusual opportunities for forming an opinion and has, no doubt, used them; and I do not know to what conclusions he has arrived.

Mr. Gerstl's proposal to teach facts only—*facts* in italics—is truly fearful to me. I fancy an honest stupid man, like some I know, teaching conscientiously what he considers the *facts* of chemistry or botany, or mechanics; and selecting a book the counterpart of Page's "Advanced Text-book of Geology," or Nicolay's "Physical Geography," bristling with *facts*. The facts of botany, in the hands of most teachers, would be a dreary list I suspect. Mr. Gerstl may teach facts alone successfully, but could the rank and file of our profession do the same?

I will most willingly admit, on the contrary, and maintain, that there exists an early science teaching that is at once useful and well-timed: the excitement and gratification of disinterested curiosity about nature; it is to do for a class, if possible, what an intelligent and encyclopædic father would do for an intelligent child. But how difficult this is for bored and weary schoolmasters! It is so much easier to tell them to get up up pp. x to y in Oliver or Ansted.

One and only one English book do I know that might almost make a stupid man teach one science well; and that is Mrs. Kitchener's "A Year's Botany" (Rivington's). That happily does not teach facts only; but is the expression of the method of a first-rate teacher in such a form as to enable any one to follow it. And yet I tremble as I mention it, for fear some class of tinies shall be ordered to get it and learn the first six pages for their first lesson in botany.

To conclude, therefore, for I will write no more on this matter, what I advise is to interest young boys in science by conversation, by informal teaching, by Natural History Societies, by encouraging collections, aquariums, &c., but not, except in the case of having that rare thing, a genius for the science master (by which I mean a genius for being a master, not a genius for science), to make science a regular subject of class teaching in the lower forms; but to teach the other subjects *well*. Then to bring in science as compulsory on all, first as Physical Geography and Astronomy or Botany, then as Chemistry with laboratory work, and Physics; and after two or three years to let boys choose their own lines. Some will drop it, others will pursue it further. This is one opinion, in brief, on the right place of science in liberal education. Now let us hear what others have to say.

Rugby, April 8 JAMES M. WILSON

NOTES

As might have been expected, Lieut. Cameron met with an enthusiastic reception from a large and distinguished audience at the meeting of the Royal Geographical Society in St. James's Hall on Tuesday night. The hall was crowded, and the Duke of Edinburgh occupied the chair, surrounded by many eminent geographers. His Royal Highness introduced Lieut. Cameron in a few appropriate and appreciative words. The distinguished explorer gave a narrative of his journey from Zanzibar to the West Coast of Africa, going over ground which is no doubt already pretty familiar to our readers. Sir Henry Rawlinson gave a very clear summary of the work which Lieut. Cameron has accomplished. "He has not been a mere explorer," Sir Henry said, "one of those travellers who carry their eyes in their pockets. He always kept his eyes well about him, and the observations which he made, both of an astronomical and of a physical character, are of extraordinary value. The register of observations which he has brought home, and which are now being computed at the Observatory at Greenwich, promise to be of a most important character. They are astonishingly numerous, elaborate, and accurate, and I have great expectation that one consequence of computing those observations will be that we shall have a definite line laid down from one sea to the other across 20 degrees of longitude, which will serve as a fixed mathematical basis of all future geographical explorations of Equatorial Africa. Among the minor objects achieved by Lieutenant Cameron must be noticed his circumnavigation of the great lake Tanganyika and his discovery of the outlet whereby that lake discharges its waters into the great river Lualaba. Another very important matter is the identification as nearly as possible, not absolutely proved by mathematical demonstration, that the Lualaba is the Congo. One of the main objects of the expedition was to follow down the course of that river so as to prove or disprove the identity of the Lualaba and the Congo. Lieut. Cameron was not able, as he explained to you, to carry out that scheme in its entirety, but he collected sufficient information on the spot to render it a matter, not of positive certainty, but in the highest degree of probability, that the two rivers are one and the same. Another great discovery of his is the determination of a new river system between the valley which he followed of the Lolame, and the scene of Dr. Livingstone's discoveries. This valley, which consists of a large river running through a series of lakes, forms, as he fully believes, and as I also believe, the course of the true Lualaba. The observations which he has furnished respecting latitude, longitude, and elevation, amount to the extraordinary number of nearly 5,000; and he took as many as 130 or 140 lunar observations on one single spot." The Geographical Society has only done its duty in awarding to Lieut. Cameron "the blue riband of scientific geography," its principal gold medal of the year.

THE rules of the French Geographical Society strictly forbid the presentation of a prize to any explorer who has not published the narrative of his discoveries. For this reason the motion for granting a medal to Lieut. Cameron at the anniversary meeting this year, was lost. But in the report and the addresses delivered on that occasion, the admiration of the Society was emphatically expressed. The great medal for 1877 will be granted to Lieut. Cameron, we believe, if the necessary condition of publication shall have been complied with.

THERE was a large gathering last Wednesday evening at the Royal Society Conversazione, which passed off very successfully. One of the most attractive features of these meetings is the instruments and apparatus exhibited; in this respect last Wednesday's meeting was quite equal to any former one. A large proportion of the objects exhibited were connected with Mr. Crookes's recent experiments on light. Among these were the following:—(1) The Torsion Balance. (2) The Turbine Radiometer: (3) Radiometer with the vanes blacked on both sides, showing rotation in either direction according to the way the light falls on them. (4) Radiometer showing the very small amount of residual air which is present. (5) Radiometer showing rotation of the glass envelope when the vanes are held fixed in space. The radiometer carries a magnet on its arms, and is floated on water so as to be free to move. (6) Radiometer having inside it a platinum spiral. (7) Radiometer with one vane counterpoised by a mirror, showing method of keeping the steel point from falling off the cup. (8) Radiometer constructed of metal, showing reverse movement on cooling. (9) Bar Photometer, showing the method of balancing one light by another. (10) Heat Engine: A Turbine Radiometer, having ice below and hot air above; working by difference of temperature. Connected with this subject, Prof. Osborne Reynolds and Dr. Schuster exhibited various apparatus:—(1) Dr. Schuster's experiment, showing that the force discovered by Mr. Crookes reacts on the vessel in which the vacuum is. (2) An experiment, showing that apparently no part of the force is referable to radiation. (3) An instrument to show that the force acts in a direction perpendicular to the hot surface. (4) A photometer which measures the heating effect of light. Among other objects exhibited were:—A series of four Rheotomes, constructed and exhibited by Mr. Apps; Fossil Elephant Bones, found near London, exhibited by Prof. Tennant; New method of measuring the position of Absorption-Bands in Spectra, and Specimens of Pigments from Human Hair, illustrated by drawings, exhibited by Mr. H. C. Sorby, F.R.S.; New Form of Wave Apparatus, invented and exhibited by Mr. C. J. Woodward; Micro-Geometric Pen, and Medical Battery, with De La Rue's (modified) Chloride of Silver and Zinc Elements, exhibited by Tisley and Spiller; Dr. Siemens exhibited his Bathometer recently described in NATURE, and an Attraction Meter, an Instrument by which the attraction of Masses is demonstrated; Mr. Spottiswoode exhibited the largest pair of Nicol's Prisms yet made, and Prof. Tyndall Infusions exposed to Self-cleansed Air; Mr. J. Browning exhibited a large number of beautifully-constructed apparatus, and Mr. W. F. Stanley a Chronobarometer and Chronothermometer, new instruments for registering Atmospheric Temperature and Pressure; Edison's Electric Pen, exhibited by Mr. T. D. Clare, Altogether the objects exhibited were varied and of great interest.

AT a meeting of chairmen of sections for organising the conferences in connection with the approaching Loan Collection of Scientific Apparatus at South Kensington which was held on the 10th inst., it was resolved that the conferences should be held on the following dates:—Physics (including Astronomy), May 16, 19, and 24; Mechanics (including Pure and Applied Mathematics and Measurement), May 17, 22, and 25; Chemistry, May 18 and 23; Biology, May 26 and 29; Physical Geography, Geology, Mineralogy, and Meteorology, May 30, June 1 and 2. It is proposed that addresses should be delivered on special subjects, and that the more important instruments exhibited should be described and discussed.

A MEMORIAL has been forwarded to the Prime Minister on the subject of University reform at Cambridge. It is signed by eighteen out of the thirty-four professors, and the Master of Trinity, eighty-three resident Fellows, twenty-nine University officers, lecturers, &c., have appended their names. The memorialists call the Prime Minister's attention to the following points contained in a memorial addressed to Mr. Gladstone three years ago:—"1. No fellowship should be tenable for life, except only when the original tenure is extended in consideration of services rendered to education, learning, or science, actively and directly, in connection with the University or the Colleges. 2. A permanent professional career should be as far as possible secured to resident educators and students, whether married or no. 3. Provision should be made for the association of the colleges, or of some of them, for educational purposes, so as to secure more efficient teaching, and to allow to the teachers more leisure for private study. 4. The pecuniary and other relations subsisting between the University and the Colleges should be revised, and, if necessary, a representative Board of University Finance should be organised." The memorialists then go on to express their conviction that, in the interest of science, learning, and education, the reforms specified are urgently required, and the hope that they will be distinctly recognised in any Bill that may be proposed in reference to this University.

WE learn from the *Times* that the following are the names of the fifteen candidates for the Fellowship of the Royal Society selected by the Council to be recommended to the Society for election. The day fixed for the election is June 1:—Captain Abney, H. E. Armstrong, Rev. W. B. Clarke, J. Croll, E. Dunkin, Prof. Erichsen, Dr. Ferrier, Colonel Lane-Fox, A. H. Garrod, R. B. Haward, C. Meldrum, E. J. Reed, Prof. Rutherford, R. Swinhoe, and Prof. Thorpe.

WE are much pleased to hear that Lord Walsingham has been appointed a trustee of the British Museum. Lord Walsingham is known to all entomologists as a most zealous collector of and authority upon Microlepidoptera. There being at present but one true biologist among the fifty trustees, the addition of a working naturalist will tend to place the department on a more satisfactory footing.

THE Committee of the German African Society has decided upon making another attempt to explore Central Africa from the West Coast, under the direction of the African traveller, Herr Mohr.

WE understand that Mr. Henry Whitely, jun., so well known for the natural history collections which he has made in Peru, who has recently returned to England, is again about to visit that country, and proposes on this occasion to explore the more northern portion of the Republic. His agent in this country is his father, who resides at 28, Wellington Street, Woolwich.

WE have received the prospectus of "A Monograph of the Cinnyridæ, or Family of Sun-birds," by Capt. G. E. Shelley, F.Z.S., the author of "A Handbook to the Birds of Egypt," &c. The work is to be issued in quarto-sized guinea parts, about twelve in number, each containing ten plates. The plates will be from the pencil of Mr. Keulemans; and the whole will be published as rapidly as their proper execution will permit. The author has, for some time past, been engaged in collecting Sun-birds, and has taken the opportunity of studying them mostly in a state of nature in both Western and Southern Africa.

IT is stated that Prof. Andrews of the Queen's College, Belfast, will probably be President of the British Association in 1877.

DR. JAMES RISDON BENNETT, F.R.S., has been elected President of the Royal College of Physicians, London.

THE April number of the *New Quarterly Magazine* contains, among other articles of general interest, a paper by the Hon. W. H. Drummond, author of "The Large Game of South Africa," on some incidents of African travel.

"ON opening his letters last week," the *British Medical Journal* states, "Prof. Huxley found in one of them a cheque for one thousand pounds, sent by Mr. Thomason, of Manchester, in the name of his lately deceased father, who was a great admirer of Prof. Huxley, and highly appreciated his great achievements in furtherance of our knowledge of the science of life."

WE have much satisfaction in noting that General Myer, Washington, U.S., has resolved to publish, in the *Bulletin of International Meteorological Observations*, the barometrical observations made at all stations 1,000 feet high and upwards, in two columns, one column giving, along with the height, the results reduced to 32° and corrected for instrumental errors only; and the other column giving the same reduced to sea-level. This mode of publishing the observations will, it is evident, furnish the materials for the discussion of important questions of an international character, which could not be attempted if the observations at the higher stations were published only as reduced to sea-level pressures.

IN No. 13 of the *Journal d'Hygiène*, Dr. de Pietra Santa urges with well-timed earnestness the importance to medical men of keeping steadily in view the two-fold function of climatology, which is, in the first place, to collect, by means of accurate instruments and simple methods, regular meteorological observations; and in the second place, to observe and study carefully the influence of these phenomena in their physiological and pathological relations. In the latter case the attention must be directed to types and sequences of weather which meteorologists have scarcely yet made subjects of investigation.

M. BALARD, whose death we announced last week, was born at Montpellier, Sept. 2, 1802. When quite young he manifested a strong passion for reading and study. He was early attracted to chemistry and physics, and while still young was made assistant *préparateur* and then *préparateur* in chemistry to the Faculty of Sciences. At the age of twenty-four years he discovered the element Bromine, and about 1833 was appointed Professor of Physics to the Montpellier School of Pharmacy and Professor of Chemistry to the Faculty of Sciences. He manifested great perseverance and energy in his researches on the utilisation of sea-water for obtaining various saline bodies, and it was while at Montpellier as professor that he made his fine experiments on hypochlorous acid and amylic alcohol. In 1843 he succeeded M. Thénard at the Sorbonne, and in 1846 he was, besides, appointed Superintendent of Lectures at the Upper Normal School. In both positions he acquired a high reputation for his solid instruction and his eminent qualities as a professor. In 1854 he was appointed Professor of General Chemistry at the Collége de France, a post which he held till his death. He shortly after quitted his position at the Sorbonne to become Inspector-General of Superior Education. In this capacity he never lost an opportunity of impressing upon teachers the great importance of introducing experimental science into schools; the want of apparatus he considered no difficulty, as for such simple experiments as are required in a school, the teacher, he thought, might easily devise his own apparatus. In 1846 he was made a member of the Academy of Sciences, and other well-deserved honours were awarded him. M. Balard's efforts and discoveries were mainly directed to the economic applications of science, and in this respect he has done much valuable work; and in the future his researches in the utilisation of sea-water may probably turn out to be of even greater practical value than they have hitherto been. M. Balard was a man who made many friends, was warm-hearted and benevolent, and was loved and respected by all who knew him. He has left no written work behind him, but his personal influence in the advance of science in France has been great.

MR. TORRENS has given notice that on April 24 he will ask the Prime Minister if the Government will give effect to the report of the Civil Service Commissioners recommending an improvement in the condition of the staff of the British Museum.

WE are glad to know that the idea has been broached in New Zealand and Australia, though in a very quiet way, of a union between the various Australian colonies for the prosecution of Antarctic exploration. The idea seems to have been suggested by the action of the mother-country in sending out the Arctic expedition, and we hope it may grow and take substantial shape. It seems to us that it would be a very proper and creditable thing for the Australian colonies to take up Antarctic exploration as their special department.

A CORRESPONDENT, Mr. F. Green, writing from Cannes, France, states that on the 8th instant, for the first time this year, he heard the Cuckoo in a valley amongst mountains sixteen miles to the westward of that place. The first time last year that he heard it in the same neighbourhood was on the 10th of April.

ON April 2 at 5.55 A.M., an earthquake was felt at Berne. Two movements took place from east to west. The duration at was two seconds; doors were opened, and church bells were rung by the shocks. In Neufchatel a strong detonation was heard; the oscillation was very strong in the lowest part of the city, and clocks struck the hour before the appointed time. Persons who were in the streets declared that warm wind was blowing for some seconds. A few hours afterwards a rain-spout occurred near Mainz, in Rhenish Hesse. A number of houses were struck by a thunderbolt and ignited, many others were flooded by the water falling from the mountains, and people drowned by an instantaneous flood.

THE additions to the Zoological Society's Gardens during the past week include two Chestnut-backed Colies (*Colius castanonotus*) from the River Daude, W. Africa, presented by Mr. Henry C. Tait; a Sclater's Muntjac (*Cervulus sclateri*) from China, presented by Mr. W. H. Medhurst; a Mandrill (*Cynocephalus mormon*), two Yellow Baboons (*Cynocephalus babouin*), a Sooty Mangabey (*Cercocebus fuliginosus*), a Monteiro's Galago (*Galago monteiri*), an African Civet Cat (*Viverra civetta*), a Servaline Cat (*Felis servalina*), a Banded Ichneumon (*Herpestes fasciatus*), a Senegal Touracou (*Corythaix persa*), an Angolan Vulture (*Gypohierax angolensis*), a Marabou Stork (*Leptoptilus crumeniferus*), three Broad-fronted Crocodiles (*Crocodilus frontatus*), from W. Africa, presented by Lieut. V. S. Cameron; two Secretary Vultures (*Serpentarius reptilivorus*), from S. Africa, deposited; three Wild Boars (*Sus scrofa*), born in the Gardens.

EXPERIMENTAL RESEARCHES ON THE EFFECTS OF ELECTRICAL INDUCTION, FOR THE PURPOSE OF RECTIFYING THE THEORY COMMONLY ADOPTED[1]

II.

THE physicist Munck, of Rosenschöld, in his memoir on electrical induction, and on the dissimulation of electricity,[2] concludes that the opposite electricity of the inductor ought to be regarded as bound, since it is connected with the same inductor and cannot be discharged by the induced body.

M. Riess continues to criticise Lichtenberg.[3] He unwittingly admits the existence of dissimulated electricity, since he says "that inductive electricity remains in part dissimulated." He afterwards says, "What has been published on the subject of *bound, latent, dissimulated* electricity has had a pernicious effect upon the science." But if I am not deceived, it is quite the opposite way, as will be seen from my experiments, by which all the objections urged by Riess against the new theory of electrical induction, published by Melloni and verified by me, are overthrown in the clearest possible manner.

Wullner says,[4] "The principal mistake made by Faraday, and on which his reasonings are based, is the hypothesis that induced electricity of the first kind has not the power of acting in an outward direction. It is true that the illustrious English physicist does not explicitly state this hypothesis; but without it his experiments lose all their value." Then according to Wullner, the absence of tension in induced electricity of the first kind is implicitly admitted by Faraday. We shall see that my experiments prove how little evidence there is of tension.

Verdet is not deceived[5] when he adduces the contradiction into which the physicists fall who deny that induced electricity of

1 An Exposition of the Two Theories of Electrical Induction. By M. Paul Volpicelli. Continued from p. 438.
2 "Pogg. Ann.," vol. 69, pp. 44 and 223.
3 "Pogg. Ann.," vol. 73, p. 371.
4 "Lehrbuch der Experimental Physik," 1st ed., vol. ii., p. 695. (Leipzig, 1863.)
5 "Ann. de Chem. et de Phys.," 3rd series, t. 42, p. 374, note 10.

the first kind is devoid of tension, when they treat of the experiment known as the induced cylinder. Yet the same physicists, Verdet says, admit this want of tension when they treat of the plate condenser, in the instrument known as Volta's condenser, as if these two experiments were not identical. It is clear, he says, that a similar restriction of the same hypothesis is not established, and that if there be dissimulated electricity upon two conducting discs placed near each other, it ought also to exist, although in a less proportion, on two cylindrical or spherical conductors, such as are ordinarily employed in experiments. All this is confirmed by De la Rive;[1] "the experiments of Melloni," he says, "appear to me to account for these anomalies in a satisfactory manner."

In Gehler's Vocabulary[2] we read that Munck, agreeing with Pfaff, did not admit the theory of Riess.

Prof. Tyndall thus expresses himself on the subject under discussion[3]:—"When an insulated conductor is under the influence of an electrified body, its repelled electricity is free; but its attracted electricity is held *captive* by the inducing electrified body. If for a moment we put the induced inductor into communication with the earth, its free electricity is dissipated; and if we remove to a distance the inducing electrified body, the *captive* electricity becomes *free*, and is distributed over the surface of the induced conductor." This manner of conceiving the phenomenon of electrical induction agrees perfectly with the new theory of Melloni, which, we maintain, satisfactorily explains the same phenomenon.

Finally, Melloni communicated to the Paris Academy of Sciences[4] (July 24, 1854) his ideas on electrical induction, and maintained, adducing all his reasons in support, that there was ground for amending the theory of induction commonly adopted, that it must be admitted that induced electricity of the first kind did not possess tension, and that the homonym of the inductor is found on every point of the induced body, including the extreme point nearest to the inductor.

After having given this brief but complete *résumé* of the various opinions which have been enunciated on the question, showing that there have never been wanting eminent physicists to maintain that induced electricity of the first kind is entirely devoid of tension, I shall now recount my own observations and experiments, by which, if I am not mistaken, I have proved the truth of Melloni's theory of electrical induction.

EXPERIMENTS.—The experiments I am about to describe should be made when the air is sufficiently dry, as then only are the results perfectly satisfactory.

First Experiment.—Upon the conducting cylinder, induced and insulated, the following five facts are proved:—1. On the same cylinder the two opposite electricities exist without neutralising each other. 2. If the extremity of the cylinder nearest to the inductor is put into communication with the earth, it is *only* the homonym of the inductor which is dissipated and not at all the opposite electricity. 3. Of the two kinds of electricity which are in the cylinder, the homonym of the influent *alone* is dissipated by contact with the air. 4. Points applied to the extremity of the cylinder nearest to the inductor allow only the homonym of the inductor to escape and not at all the opposite electricity. 5. Induced electricity of the first kind is not transferred from the induced body to the inductor, but the electricity of the inductor may certainly be transferred to the induced body.

These five experimental facts cannot be logically explained by the old theory of electrical induction, but only by the new, showing that induced electricity of the first kind does not possess tension, *i.e.*, that it is entirely dissimulated, and that induced electricity of the second kind, *i.e.*, the homonym of the inductor, is entirely free on all points of the induced object.

Second Experiment.[5]—In the communication referred to in the note are analysed the phases of divergence produced in the gold-leaf electrometers applied to the extremity of the insulated induced body nearest to the inductor. The same phases were obtained by means of two simultaneous inductions, the one principal, which came from the inductor, the other secondary, which came from the analyser.

On this ground it is concluded that these phases, when they are fairly interpreted, prove that the homonym of the inductor exists also on the extremity indicated, and that on this account the induced electricity has no tension. In the same experiment it was seen how these phases may be misleading, if we do not examine carefully the simultaneous effects of the two inductions indicated. The explanation of this experiment allows a much greater development than that of Melloni, published by M. Regnault in the "Comptes Rendus," t. 39, p. 177 (July 24, 1854).

Third Experiment.—When, into the inductive sphere of an electrified body, *a*, is introduced,[1] with the necessary precautions, another insulated body, *b*, the electricity of the inductor, *a*, always attracts and completely dissimulates the induced body, *b*, the opposite electrical condition, expelling the homologue, and rendering it completely *free*. But this is not all. There is another fact, which has not yet been indicated, viz., that if we bring close to or remove away from the inducing body, *a*, another body, *c*, then part of the dissimulated electricity in the induced *b* becomes *free* in the former case, while in the second case it increases in *b*, at the same time that the opposite condition is developed in it.

In the paper above cited all the experiments confirming this result are given, from which we conclude that the induced electricity of the first kind does not possess tension.

Fourth Experiment.—The first experiments made for the purpose of discovering if electrostatic induction can be effected in curved lines, are due to the illustrious Faraday, who, in one of his latest papers, says that by his experiments he believes he has established the possibility of this induction; perhaps the facts which I have discovered, and which are completely verified when the atmosphere is dry, may establish its certainty. These experiments, by which is proved the existence of curvilinear induction, will be found described in "Comptes Rendus," t. 43, p. 719.

Fifth Experiment.—This fifth experiment[2] contains an account of six considerations and of five experiments, by means of which it is shown that induced electricity of the first kind does not possess tension, and that the induced insulated cylinder shows at all points the existence of the homonym of the inductor.

Sixth Experiment.—In this[3] are described several experiments which show the existence of curvilinear induction, and prove at the same time that the separation of the gold-leaf electrometers, applied to the extremity of the induced body furthest from the inductor, is produced principally by this influence, and to a very small extent by the homonym of the inductor.

Seventh Experiment.—In this[4] is shown, in a different manner, that induced electricity of the first kind has no tension, and that on any point of the induced body there is always found the homonym of the inducing, not excepting the extremity nearest to the inductor, provided that the proper means is employed, as described in the communication referred to.

Eighth Experiment.—In this[5] are analysed the objections made by M. Riess to some of my experiments on electrical induction, and the doctrine of Melloni on this subject is confirmed by other facts. The conclusion is that the objections of M. Riess are not justifiable, and that if this eminent physicist had repeated the experiments which he criticises, he would have found them to be genuine, and would not have declared them to be inexplicable. It is now more than sixteen years since I made this reply to M. Riess, always communicating other experiments bearing on the same point; but, so far as I know, he has raised no other objections to the theory of Melloni.

Ninth Experiment.—In this communication[6] eight reasons are adduced to show that induced electricity has no tension. The same reasons do not hold good on the old theory, while Melloni's new theory of electrical induction explains them completely. This new theory does not entirely overturn the old, as some have mistakenly believed; the former only essentially modifies the latter in some of its parts.

Tenth Experiment.—This is a reply to the note of M. Gaugain, in which he observes that, notwithstanding the various experiments adduced by M. Volpicelli, it is still strongly maintained that induced electricity of the first kind possesses tension.[7]

Eleventh Experiment.—In this are advanced various observations, some on tension, both electro-statical and electro-dynamical, and others on electrical induction.[8]

Twelfth Experiment.—In this communication it is observed

1 "Archives des Sciences Phys. et. Nat. de Genève," t. 26, p. 323, note 1.
2 Vol ii. p. 131. (Leipzig, 1843.)
3 "Les Mondes," 2nd series, vol. xxiii. p. 566, § 79.
4 "Comptes Rendus," t. 38, p. 177.
5 Published in the "Comptes Rendus," t. 40, p. 246 (Jan. 29, 1855).

1 "Comptes Rendus," t. 41, p. 553 (Oct. 8, 1855).
2 Ibid., t. 44, p. 17 (May 4, 1857).
3 Ibid., t. 47, p. 623 (Oct. 18, 1858).
4 Ibid., t. 47, p. 664 (Oct. 23, 1858).
5 Ibid., t. 48, p. 1162 (June 27, 1859).
6 Ibid., t. 59, p. 570 (Sept. 26, 1864).
7 Ibid., t. 59, p. 962 (Dec. 5, 1864).
8 Ibid., t. 61, p. 548 (Oct. 2, 1865).

that continuous electrophoroi furnish, when carefully examined, a clear proof that induced electricity does not possess any tension. In fact, in the case of these machines, as in that of frictional machines, the conducting spikes of the prime conductor possess the two opposite electricities co-existent on the same points; this is easily shown by means of a very small proof-plane.[1]

Thirteenth Experiment.—It is shown, by means of Geissler's tubes submitted to the electrical influence, that induced electricity of the first kind has no tension.[2]

Fourteenth Experiment.—Here it is observed that the proof-plane, whether submitted or not to electrical induction, always receives by contact a charge greater than that which is free on the element which it has touched. Then the *coïbent*, always indispensable in the construction of the proof-plane, receives, by infiltration, a certain quantity of electricity, besides that obtained by communication with the metallic part. This infiltration or absorption varies not only with the nature of the *coïbent*, but also with its quantity, within certain limits. This communication [3] contains other observations on electrical induction, and it is concluded that on the extremity of the induced body nearest to the inductor, the two opposite electricities coexist, and that consequently induced electricity of the first kind has no tension. It is also concluded that the homonym of the inductor is always found on whatever point of the induced body this may be, and that the homonym indicated is the only one to be dissipated, because it alone is the only one endowed with tension.

Fifteenth Experiment.—In this is explained Nicholson's duplicator, which is satisfactory, since it is based on the want of tension in induced electricity of the first kind.[4]

Sixteenth Experiment.—In this is shown how we may shield from curvilinear induction the electroscope which hangs from the extremity of the induced body nearest to the inductor. From this experiment it is concluded that the divergence of the straws is due principally to curvilinear induction, and that induced electricity of the first kind does not possess tension.[5]

Seventeenth Experiment.—In this is analysed a little known electrostatic phenomenon; and from this analysis it follows that induced electricity of the first kind has no tension.[6]

Eighteenth Experiment.—It is shown mathematically that electric induction does not traverse conducting masses. It is afterwards observed that first the Florentine academicians and then Faraday admitted this truth. It is also observed that if we admit that induced electricity of the first kind possesses no tension, we arrive at the conclusion given below by means of experiment.[7]

CONCLUSION.—Upon an insulated conductor submitted to the electric influence—1. Induced electricity of the first kind does not possess tension. 2. It is found in greater quantity at the extremity of the induced body nearest to the inductor, and diminishes always as it approaches the other extremity. 3. Induced electricity of the second kind, *i.e.*, the homonym of the inductor, is found on every point of the induced body, not excepting the extremity nearest to the inductor; it continually increases in proportion as it approaches nearer to the other extremity, and is always free.

1 "Comptes Rendus," t. 67, p. 843 (Oct. 26, 1868).
2 Ibid., t. 69, p. 730 (Sept. 27, 1869).
3 Ibid., t. 74, p. 860 (March 25, 1872).
4 Ibid., t. 75, p. 257 (July 29, 1872).
5 Ibid., t. 76, p. 169 (Jan. 20, 1873)
6 Ibid., t. 76, p. 1296 (May 26, 1873).
7 Ibid., t. 78, p. 901 (March 30, 1874)'

SCIENTIFIC SERIALS

Mind—A Quarterly Review of Psychology and Philosophy. Edited by George Croom Robertson, M.A., Professor of Philosophy of Mind and Logic, in University College, London. Jan. 1876.—*Revue Philosophique de la France et de l'Etranger.* Dirigée par Th. Ribot. Première Année. Janvier 1876: Paris.—The growing importance of psychology has been asserted by the simultaneous appearance of a French and an English review, especially devoted to its interest. In scope and character the two publications are identical. One aim of the projectors of *Mind* seems to be to obtain a decision of the question: Is psychology a science? "Nothing less, in fact, is aimed at in the publication of *Mind.*" The first number opens with a lecture on "The Comparative Psychology of Man," read before the Anthropological Institute, by Mr. Herbert Spencer. It is one mass of valuable suggestions, and every reader will follow with interest the divisions and sub-divisions under which Mr. Spencer recommends that the subject should be studied.—Next follows under the title "Physiological Psychology in Germany," a rather lengthy account, by Mr. James Sully, of a work by Prof. Wundt of Leipzig. The other leading articles are in their order:—"Consistency and Real Inference," by Mr. John Venn; in which the comparative merits and defects of the conceptualist and material views of logic are considered. Towards the end of the article Mr. Venn refers to what he calls an irrelevant difficulty which sometimes puzzles the student of Mill. How, asks the student, can Mr. Mill, while professing to be an idealist, lay it down that logic has to do with the facts or things themselves, rather than with our ideas about them? We do not see that the consistency of Mr. Mill would be very conclusively vindicated even were it the fact that he did not allow his idealism to interfere with his logic more than does an idealistic astronomer allow his metaphysics to affect his astronomy. But does not Mr. Mill fall back on his idealism when in his discussion with Mr. Herbert Spencer as to the number of terms in the syllogism, he maintains that the things named in the premises and conclusion of a syllogism are our sensations or expectations of sensations, while Mr. Spencer holds the things spoken of to be so many separate objective entities? In an able and interesting paper, Mr. Henry Sidgwick discusses "The Theory of Evolution in its Application to Practice," and finds that when guidance is needed in ethics or politics the doctrine of evolution will not help us. A distinction between "Philosophy and Science," is next worked out by Mr. Shadworth H. Hodgson, in which he displays all his remarkable delicacy of thought. He finds the peculiar scope of philosophy to be "ultimate subjective analysis of the notions which to science are themselves ultimate." We doubt if Mr. Lewes would admit that this is not included in his conception of philosophy as embodied in the really great work on which he is now engaged. An excellent article on "Philosophy at Oxford," is contributed by the Rector of Lincoln College, which it is perhaps not too much to hope may bear some practical fruit. Coming last and occupying the place of the novel in the magazine, is "The Early Life of James Mill," to be continued, by Prof. Bain. In addition to being of intense interest to all who care for mental science, it has been eagerly read and discussed by many who have read nothing else in *Mind.* Short Critical Notices and Reports, and neat little Notes by the editor and others, complete the volume. We sincerely hope Prof. Robertson will be able to keep the *Review* up to the standard of this first number. To make it a commercial success will not be an easy task, for though philosophy has of late been a marketable commodity, that has been when distributed among many periodicals.

Of the *Revue Philosophique* we can say only a very few words. The first number opens with a most interesting and suggestive account, by M. Taine, of observations he made on the acquisition of language by a female child. His speculations about words entirely invented by the child and carrying with them natural meanings, as also his reasonings from the childhood of the individual to that of the race, are ingenious and plausible. He concludes with some excellent remarks on Max Müller's view that in *rational* language we find the distinctive characteristic of man. According to M. Taine the use of words, sounds carrying with them a vague general connotation, is, like the use of ornaments and the use of tools, in common with numerous other indications, an evidence that the stage human has been reached. The psychological condition of this superiority, he continues, will be found in a greater aptitude for general ideas, and its physiological condition in a larger and finer development of brain. In the second article the doctrine of final causes is ably discussed by M. P. Janet, from various points of view. He concludes, however, by maintaining a form of the doctrine, which, as far as we can see, without being able to serve any practical end, supposes a theory that lies wholly outside the boundaries of science. Mr. Herbert Spencer's lecture on "The Comparative Psychology of Man," of which we have already spoken, comes next. The remainder of the number is taken up with reviews of books.

THE *Journal of the Chemical Society* for February contains the following papers:—On the presence of liquid carbon dioxide in mineral cavities, by W. N. Hartley. The author's researches have been chiefly confined to quartz, evidence of the nature of the enclosed liquid being furnished by the specific gravity of the liquid as compared with water (which was also contained in the cavities), and by observing the critical temperature. The author is of opinion that the fluid-cavities of sapphires and rubies also contain carbon dioxide.—On certain bismuth compounds, by

M. M. Pattison Muir. The author has examined the trichloride, tribromide, and the so-called bismuthic acid.—On bismuthiferous tesseral pyrites, by W. Ramsay. The formula of this mineral appears to be $(Ni, Co, Fe)(As, Bi)_3$.—On the occurrence of native calcium chloride at Guy's Cliffe, Warwickshire, by John Spiller.—The decomposition of alcohol and its homologues by the joint action of aluminium and its halogen compounds, by Dr. J. H. Gladstone and Alfred Tribe. Aluminium and its iodide have no action upon methyl alcohol. Ethyl alcohol is energetically decomposed by a mixture of these substances, hydrogen gas being evolved in large quantities and a pasty residue being left, which the authors consider to be aluminic iodo-ethylate. Heated to 275° C. this residue fuses and undergoes decomposition thus:—

$$Al_2\left\{\begin{matrix}(C_2H_5O)_3\\ I_3\end{matrix}\right. = Al_2O_3 + 3\left\{\begin{matrix}C_2H_5\\ I\end{matrix}\right.$$

The authors have likewise obtained evidence of the existence of aluminic ethylate. Amylic alcohol is decomposed also by these substances. A mixture of the chloride with the metal has no action upon alcohol; the bromide has a decided action.—Ethyl-phenyl-acetylene, by T. M. Morgan. This substance has been obtained by the action of ethyl iodide upon the sodium compound of phenyl-acetylene the two substances being mixed with ether and heated in sealed tubes.—Narcotine, cotarnine, and hydrocotarnine, by G. H. Beckett and Dr. C. R. A. Wright. The authors have studied the action of water upon narcotine hydrochloride, the action of ethyl iodide on hydrocotarnine, narcotine, and cotarnine, and the action of acetic anhydride on all three of these bodies. Dr. Wright adds an appendix on the structural formulæ of narcotine and its derivatives.—Note on incense resin, by Dr. J. Stenhouse and C. E. Groves. This resin is the produce of *Icica heptaphylla*, Aubl., a native of British Guiana. The essential oil contains a hydrocarbon of the empirical formula C_5H_8, which the authors propose to call conimene. To the crystalline resin the authors assign the formula $C_{46}H_{76}O$, and propose the name *icacin*.—On certain sources of error in the ultimate analysis of organic substances containing nitrogen, by G. S. Johnson. These errors are: first, increase of weight by the absorption of oxygen by *nitrite* contained in the solution in the potash bulb owing to the passage of unreduced nitrous anhydride over the ignited copper. Secondly, the presence of occluded hydrogen in the metallic copper reduced in this gas, which is given off on the application of heat and reduces the surface film of oxide, producing water which adds to the weight of the chloride of calcium tube.

SOCIETIES AND ACADEMIES

London

Royal Society, March 16.—Preliminary Reports to Prof. Wyville Thomson, F.R.S., Director of the Civilian Scientific Staff. I. On the true Corals dredged by H.M.S. *Challenger* in deep water between the dates Dec. 30, 1870, and Aug. 31, 1875, by H. N. Moseley, Naturalist to the Expedition.

The author gives a list of the corals dredged in a depth of 50 fathoms and upwards, with notes on each. The whole is necessarily preliminary, on account of the impossibility of sufficient comparisons being made, and references obtained. The results embody great additions to our knowledge concerning the bathymetrical range of corals. Only one coral has been obtained from a greater depth than 1,600 fathoms; it is *Fungia symmetrica*. Only three other corals have been obtained at as great a depth as 1,500 fathoms. Only about twenty-seven genera of corals have as yet been proved to exist in a depth of 250 fathoms and upwards; of these a list is given, to which is added those obtained by the U.S. Coast Survey and the *Porcupine*, making forty-two genera in all. Of these twenty occur in the fossil state. No coral in any way allied to the Rugosa has been dredged by the *Challenger*.

II. On work done on board the *Challenger*, by Mr. John Murray, Naturalist to the Expedition. This Report includes the preliminary notice on oceanic deposits, describing specimens of the sea-bottoms obtained in the soundings, dredgings, and trawlings, over 300 in number, during the years 1873–1875, between England and Valparaiso. The deposits may be classed as follows:—

1. *Shore Deposits*—
   (*a*) Blue and green muds.—Met with near the shores of most of the great continents and islands.
   (*b*) Grey muds and sands.—Met with chiefly near oceanic islands of volcanic origin.
   (*c*) Red mud.—Met with on the eastern coast of South America.
   (*d*) Coral mud.—Met with near coral reefs.
2. *Globigerina Ooze.*—An abundant oceanic deposit not met with south of latitude 50° S.
3. *Radiolarian Ooze.*—An oceanic deposit met with only in the Western and Middle Pacific.
4. *Diatomaceous Ooze.*—An oceanic deposit met with only south of 50° S. latitude.
5. *Red and Grey Clays.*—The most abundant oceanic deposit.

The deepest sounding (4,475 fathoms) was a Radiolarian ooze.

In the early part of the cruise many attempts were made by all of the naturalists to detect the presence of free protoplasm in or on the bottoms from the soundings and dredgings, but with no definite result. It was undoubted, however, that some specimens of the bottom preserved in spirit assumed a very mobile or jelly-like aspect, and also that flocculent matter was often present.

At this point Mr. Buchanan determined that the flocculent matter was simply the amorphous sulphate of lime precipitated by spirit from the sea-water associated with the ooze. Subsequently a number of experiments were made, in conjunction with Mr. Buchanan, upon the behaviour of this amorphous precipitate when precipitated with different quantities of spirit, and when treated with colouring solutions. The precipitate was also examined alone and mixed up with some of the ooze. The ooze was examined at the same time, and in the same manner, but without having been treated with spirit. The results were shortly these:—

"When sea-water is treated with twice its volume of spirit or less, nearly the whole of the amorphous precipitate assumes the crystalline form in a short time.

"When treated with a great excess of spirit the precipitate remains amorphous, and assumes a gelatinous aspect.

"This gelatinous-like sulphate of lime colours with the carmine and iodine solutions, and when mixed with the ooze has, under the microscope, the appearances so minutely described by Haeckel.

"The ooze washed with distilled water, or taken just as it comes up, and treated in the same manner with colouring solution, does not show these appearances."

When it is remembered that the original describers worked with spirit-preserved specimens of the bottom, the inference seems fair that *Bathybius* and the amorphous sulphate of lime are identical, and that in placing it amongst living things the describers have committed an error.

A preliminary report on vertebrates is then given, containing a list of all the fishes taken in the trawl or dredge. New forms necessitate modifications in the definitions of some families, but it has not been found necessary to establish any new families. The deep-sea and oceanic forms belong to the families—*Stenoptychidæ*, *Macruridæ*, *Ophidiidæ*, *Scopellidæ*, *Stomiatidæ Pediculati*, *Halosauridæ*, *Notocanthi*, *Muraenidæ*, and *Trachinidæ*.

Of the Petrels and Penguins very extensive collections have been made, as skins and as spirit specimens. Two or three skeletons of very large specimens of the Sea-elephant have been preserved.

III. On observations made during the earlier part of the voyage, by the late Dr. R. von Willemöes-Suhm, naturalist to the Expedition. This report is on the Atlantic fauna only. Among the most interesting results obtained may be mentioned briefly the facts that shrimps in great depths are liable to be attacked by considerably large Gordiaceous worms; that a curious intermediate form between Priapulids and Sipunculids has been discovered; that relations of the famous Jurassic Eryonidæ are still living in the great depths, where they are (in the Pacific at least) by no means rare.

March 23.—"On the Force caused by the Communication of Heat between a Surface and a Gas; and on a New Photometer," by Prof. Osborne Reynolds, communicated by B. Stewart, F.R.S., Professor of Natural Philosophy in Owens College, Manchester.

This paper contains an account of an experimental investigation undertaken with a view to support, by absolute measurements, the theoretical arguments by which the author endeavoured to prove the existence of reactionary forces or "heat-reactions, whenever heat is communicated from a surface to a gas, and *vice versâ*, and the connection between these forces and the motion caused by heat and light falling on bodies *in vacuo*.

Having obtained one of the beautiful little "Light-Mills" constructed by Dr. Geissler, of Bonn, the author was in a position to make quantitative measurements of the effects produced, and of the force producing them.

In the first place, with regard to the sufficiency of the residual air to cause the motion. It was found that this air is, with the exception of the friction of the pivot, which is found to be so small as to be inappreciable, the sole cause of the resistance which the mill experiences, of the limit which is imposed on its speed for such intensity of light, and of the rapidity with which it comes to rest when the light is removed. The law of resistance, as determined by careful measurements, is found to agree perfectly with the resistance which highly rarefied air would offer to its motion; and this law is distinctly special in its character, being proportional to the velocity at low speeds, and gradually tending towards the square of the velocity as the speed increases.

Having established the fact that there is sufficient air in the mill (and Mr. Crookes's behaves in the same manner as this mill) to balance, by its resistance, the force which moves the mill, it is argued that all question as to the sufficiency of the air to cause the forces is removed. What the air can prevent it can cause.

As regards the possibility of the motion being in any way the direct result of radiation. This supposition the author had previously shown to be directly contradicted by the fundamental law of motion that action and reaction are equal. A cold body runs away from a hot body, while, if free to move, the hot body will run after the cold body, showing that the force does not act from body to body, but that each body propels itself through the surrounding medium in a direction opposite to its hottest side, the effect of one body on the other being due solely to the disturbance which it causes in the equilibrium of temperature.

Besides proving that the force acts between the vanes of the mill and the medium immediately surrounding them Dr. Schuster's experiments furnish a quantitative measure of the actual force. From this measure it is shown on theoretical grounds that the difference of temperature on the two sides of the vanes necessary to cause heat-reactions of this magnitude could not be less than 1°·7, while the probability is that it is considerably more.

In order to apply this test and see how far the actual difference of temperature in Dr. Schuster's experiments correspond with that deduced from the theory, a new photometer was devised by the author with an immediate view of measuring the difference of temperature caused by light on a black and a white surface.

Of two thin glass globes, 2½ inches in diameter, connected by a syphon tube ⅛ inch internal diameter, one was blackened with lamp-black on the inside over one hemisphere and the other was whitened with chalk in a similar manner, the two clean faces of the globes being turned in the same direction. Oil was put in the tube and the globes were otherwise sealed up. Any light which enters through the clean faces is received on the black and white surfaces, and the air in the globes expands in accordance with the difference of temperature which they attain, moving the oil in the tube. A motion of ½ an inch on the part of the oil shows a difference of 2°·2, in the temperature of the air within the globes.

The instrument so constructed is exceedingly delicate, and will show a difference in the intensity of light sufficient to make one revolution per minute difference in the speed of the mill.

Measured with this instrument, the difference of temperature caused by the light necessary to give the mill 240 revolutions per minute does not exceed 24°, and is probably less than this, which shows that the theoretical difference of heat necessary to cause the heat-reactions is well within the difference as actually measured, leaving an ample margin for error in the methods of approximation used in the calculation.

In concluding the paper the author claims to have set at rest the only point respecting the explanation of the motion caused by heat, which remained doubtful after he had discovered that, according to the kinetic theory, the communication of heat to a gas must cause a force reactionary on the surface, viz., whether this reaction was adequate in amount to cause the results seen to take place.

He adds a suggestion as to a new form of light-mill to have vanes inclined like the sails of a windmill, and not having one side white and the other black, like the light-mills at present constructed. Arguing that the forces act perpendicularly to the surface, and in a direction independent of that from which the light comes; so that such a mill would turn like a windmill with the full and not merely the differential effect of the light. Such a mill, he concludes, would furnish another test as to whether or not the force is directly referable to radiation.

Geological Society, March 22.—Prof. P. Martin Duncan, F.R.S., president, in the chair.—Frank Campion, Henry J. Gardiner, Henry Percy Holt, Lord Rosehill, Harold Underhill, Frederick Thomas Whitehead, and Thomas Wrightson were elected Fellows of the Society.—On the Triassic strata which are exposed in the cliff sections near Sidmouth, and a note on the occurrence of an Ossiferous zone containing bones of a *Labyrinthodon*, by H. J. Johnston Lavis. The author described the base of the cliffs east of Sidmouth as composed of the marl which is the uppermost subdivision of the Trias in South Devon, capped in Littlecomb Hill and Dunscomb Hill by greensand and chalk, and in Salcombe Hill by greensand alone. In the valley of the Sid it is largely exposed at the surface. Close to the mouth of the Sid the upper sandstone crops out beneath the marl, forming a cliff overhanging the river. To the west of Sidmouth there is a low projecting cliff, the Chit rock, formed also of the upper sandstone, and at the western end of this is a fault which has given the Chit rock an upthrow of at least 40 and perhaps of 80 feet, since it has no marl capping it, and in its lithological character it resembles the middle part of the Upper Sandstone. To this point the dip is to the east; but westward of the fault the dip is at first to the west for about half a mile, when the sandstone reappears with an easterly dip, having formed a synclinal curve. It is overlain by marl and greensand in Peake and High Peake Hills, which are capped with chalk gravels. West of High Peake Hill the sandstone forms the whole cliff. The author described the general characters presented by the Triassic beds in the section under notice, and mentioned the occurrence at about 10 feet from the top of the sandstone of a peculiar series of beds, composed of coarse sandstone, containing scattered nodules of marl from the size of a pea to that of a hen's egg, together with numerous fragments of bone, some of which, belonging to a species of *Labyrinthodon*, would be described by Prof. Seeley. The author mentioned that he had received from the Rev. S. H. Cooke some fragments of bone obtained by him twenty years ago from this same "Ossiferous zone." Mr. Whitaker's specimen of *Hyperodapedon* was also obtained from the upper sandstone.—On the posterior portion of a lower jaw of *Labyrinthodon* (*L. lavisi*) from the Trias of Sidmouth, by Mr. Harry Govier Seeley, F.L.S., Professor of Physical Geography in Bedford College, London. After referring to the doubtful position of the Labyrinthodontia in the system, and expressing his doubts as to the occurrence of the genus *Mastodonsaurus* in Britain, the author proceeded to describe in detail the posterior part of the right ramus of the lower jaw of a Labyrinthodont, obtained by Mr. Lavis from the ossiferous zone of the Trias near Sidmouth, the position of which was described by that gentleman in the preceding paper. The specimen, which is 13 inches long, and perfectly free from matrix, shows that the lower jaw in Labyrinthodonts not only contains articular, angular, and dentary elements, as hitherto supposed, but also separate splenial and surangular elements, and probably a distinct coronoid bone. These bones were described in detail, and the author remarked that although they are somewhat reptilian in aspect and arrangement, they are not very suggestive as to the affinities of *Labyrinthodon*. They surround a central hollow space, which no doubt received the primitive cartilage round which the bones were ossified; and the persistence of this character would seem to be a link rather with the lower than with the higher vertebrata. The jaw differs from the Batrachian mandible in possessing well-developed angular and surangular elements, and some reptiles, such as crocodiles and the marine Chelonia, present analogies in the perforations, the structure of the jaw, and the sculpture of the bones. In size the specimen is almost identical with that figured by Mr. Miall as belonging to *Labyrinthodon pachygnathus*, but the depths and outlines of the postarticular part of the jaw, and differences in the sculpture of the lateral subarticular ornament, furnish distinctive characters which lead the author to describe the present species as representing a new species, which he names, in honour of its discoverer, *Labyrinthodon Lavisi*. The author briefly noticed several other bones and fragments obtained by Mr. Lavis in the same locality, some of which probably belonged to the same skeleton.—On the discovery of *Melonites* in Britain, by Mr. Walter Keeping, communicated by Prof. T. M'Kenny Hughes. The author described a specimen from the carboniferous limestone of Derbyshire in the museum of the Geological Survey, which displays numerous plates belonging to the test of a large Echinoid, considered by him to be a new species of the genus *Melonites*, hitherto regarded as peculiar to America. The author proposed to call this species *Melonites Etheridgii*, and he described it as possessing a more or less spheroidal test, about seven inches in diameter, composed of very thick plates, arranged in five ambulacral and five interambulacral areas, all the plates being ornamented with minute tubercles for the support of spines. The interambulacral areas were probably about twice as broad as the ambulacral, and composed (at the equator) of about nine ranges of plates, the marginal ones pentagonal, the rest hexagonal, articulating with each other by faces varying from a right angle to one of 30°. The ambulacral areas were broad, each formed of two convex ribs separated by a meridional depression running from mouth to anus, and each rib (half-area) composed of six or seven ranges of irregular plates, each perforated by a pair of simple pores. The tubercles are minute, imperforate, without boss, and of two orders, the larger surrounded by a smooth areola, bounded by an elevated ring. The spines are small, tapering, coarsely sulcate, with a prominent collar round the articular end. A second specimen exists in the British Museum. The species differs strikingly from the North American *Melonites multiporus* in the characters of the ambulacral areas, which have 12–14 ranges of plates, and are divided by a meridional furrow in the new species, and only eight ranges of plates, with a median ridge formed of plates twice as large as the rest in *M. multiporus*.—Note on the phosphates of the Laurentian and Cambrian rocks of Canada, by Principal Dawson, F.R.S. The author described the mode of occurrence of phosphatic deposits in various localities in Canada. Dark phosphatic nodules, containing fragments of *Lingulæ*, abound in the Chazy formation at Allumette Island, Grenville, Hawkesbury, and Lochiel. Similar nodules occur in the Graptolite shales of the Quebec group at Point Levis, and in limestones and conglomerates of the Lower Potsdam at Riviere Ouelle, Kamouraska, and elsewhere on the lower St. Laurence; these deposits also contain small phosphatic tubes resembling *Serpulites*. The Acadian or Menevian group near St. John, New Brunswick, contains layers of calcareous sandstone blackened with phosphatic matter, consisting of shells and fragments of *Lingulæ*. The author described the general character of the phosphatic nodules examined by him at Kamouraska, and gave the results of analyses made of others from various localities, which furnished from 36·38 to 55·65 per cent. of phosphate of lime. A tube from Riviere Ouelle gave 67·53 per cent. The author accepted Dr. Hunt's view of the coprolitic nature of the nodules, and inclined to extend this interpretation to the tubes. The animals producing the coprolites could not be thought to be vegetable feeders; and he remarked that the animals inhabiting the primordial seas employed phosphate of lime in the formation of their hard parts, as had been shown to be the case with *Lingulæ*, *Conularia*, and the Crustaceans. The shells of the genus *Hyolithes* also contain a considerable portion of phosphate of lime. Hence the carnivorous animals of the Cambrian seas would probably produce phosphatic coprolites. With regard to the Laurentian apatite deposits, the author stated that they, to a great extent, form beds interstratified with the other members of the series, chiefly in the upper part of the Lower Laurentian above the *Eozoon* limestones. The mineral often forms compact beds with little foreign matter, sometimes several feet thick, but varying in this respect. Thin layers of apatite sometimes occur in the lines of bedding of the rock. Occasionally disseminated crystals are found throughout thick beds of limestone, and even in beds of magnetite. The veins of apatite are found in irregular fissures; and as they are found principally in the same parts of the seams which contain the beds, the author regarded them as of secondary origin. The Laurentian apatite presents a perfectly crystalline texture, and the containing strata are highly metamorphosed. The author's arguments in favour of its organic origin are derived from the supposed organic origin of the iron-ores of the Laurentian, from the existence of *Eozoon*, from the want of organic structure in the Silurian deposit described by Mr. D. C. Davies, and the presence of associated graphite in both cases, from the character of the Acadian linguliferous sandstone, which might by metamorphism furnish a pyroxenite rock with masses of apatite, like those of the Laurentian series, and from the prevalence of animals with phosphatic crusts in the Primordial age, and the probability that this occurred also in the earlier Laurentian. The position of the phosphatic deposits above the horizon of *Eozoon* is also adduced by the author as adding probability to the existence of organic agencies at the time of their formation.

PARIS

Academy of Sciences, April 3.—Vice-Admiral Paris in the chair.—The following papers were read:—On the displacement of lines in the spectra of stars, produced by their motion in space, letter from P. Secchi. The author tabulates a number of the observations made by Huggins, Vogel, and himself, and those at Greenwich Observatory, and shows there is considerable contradiction in the results. Might there not, he asked, be some cause of systematic error in the manner of observing or in the instruments? Comparing the dark line F of Sirius with the hydrogen line H β from a Geissler tube, he got always the same result—a shortening of the Sirius waves (contrary to Huggins), when the telescope was carried along by the clock-work, and the assistant was at the seeker to keep it on a fixed point, and corresponding to the slit of the spectroscope; but if the clock-work stopped, or the assistant deranged the position of the star, the bright line was displaced and came into coincidence with the star line. Dispensing with clock-work, the line was found to be on one side or the other according as the star was looked at on one side or the other of the axis of the telescope. A change was also had on turning the spectroscope 180° on its axis. P. Secchi merely points out these possible sources of illusion without trying to explain them.—Observations of sun-spots made at the Toulouse Observatory in 1874 and 1875, by M. Tisserand. In 1874, 237 spots were observed; in 1875 there were only 88. Of the 76 spots observed at least three times in 1874, 41 were in the boreal hemisphere, 35 in the austral, with a mean latitude (+ or −) of 10°·5. Of the 29 observed three times in 1875, 17 were in the boreal, 12 in the austral hemisphere; the mean latitude was 11°·7. M. Tisserand tabulates his observations with reference to diurnal rotation.—Testing for vinic alcohol in mixtures, and especially in presence of wood spirit, by MM. Alf. Riche and Ch. Bardy.—On the spermatia of the Ascomycetes, their nature and physiological *rôle*, by M. Max. Cornu. The spermatia were at first considered by M. Tulasne as fecundating corpuscles; and in support of this was their apparent refusal to germinate in the same conditions as three other sorts of spores. M. Cornu says he has obtained a very complete germination of spermatia in certain cases. Sometimes the action of pure water will suffice to bring them to vegetation, more often it is necessary to add nutritive elements. The facts observed refute the old theory of fecundation, and the simplification introduced into the number of reproductive organs gives a grand unity to the polymorphism of Ascomycetes.

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