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THURSDAY, MARCH 2, 1871

THE SMALL-POX EPIDEMIC

THE present epidemic of Small-pox in London is the most destructive, we are told, that has occurred in London during the present century. This is a very painful disclosure, when it has been almost demonstrated that, of all contagious and epidemic diseases, it is the one over which man has the most control. It is a well-known fact that when persons have once had the small-pox they seldom or never take it again, and that the disease known as cow-pox is a modified form of small-pox, and that persons who have had this modified form of small-pox are as little liable to take this disease as those who have had the small-pox itself. This was the great discovery of Jenner, and the practice of vaccination has more than realised the hopes of its discoverer and his friends. Where vaccination has been carried out with energy, and communities by wise laws or individual action have seen that every child is duly vaccinated, there small-pox has not spread. It appears that where communities are all properly vaccinated, there, even if an isolated case of small-pox does occur, it has no pabulum to feed on, and it does not spread. It is only when the small-pox contagion is communicated to unvaccinated persons that the disease is set up, and has sufficient vitality to spread through a community. Forty-five millions of persons died in Europe from small-pox in the century preceding the introduction of vaccination, whilst it is calculated that it has not killed more than two millions of persons in Europe since the introduction of vaccination. In London, during the last century, one death in every fourteen was due to small-pox. Up to the present time in this century not more than one-fiftieth of the persons who have died in London have died of this disease Greater differences than even this have been observed in. some of the cities and towns of the Continent of Europe. At Trieste the deaths from small-pox have been seventy-five times less than before vaccination; in Moravia, twenty-one times less; in Silesia, twenty-nine times less; in Westphalia, twenty-five times less, and in Berlin, nineteen times less.* These instances might be indefinitely increased, but we are anxious to show to what extent this disease is really controllable.

Supposing even that it is not demonstrated that small-pox can be eradicated by vaccination alone, carefully collected statistics show that when small-pox is taken by the vaccinated, it is much less fatal than among the unvaccinated. In the epidemic now prevailing in the metropolis, it is found that not more than six per cent. of persons who have been vaccinated die of small-pox, whilst about thirty-six per cent. of those who have not been vaccinated die. This is the proportion of deaths observed at the Small-pox Hospital from 1836 to 1851, so that it will be observed that the small-pox has neither lost nor increased in malignity.

There is a question which we ought to allude to here, and that is, Does vaccination lose its protective power? The best observers are of opinion that when vaccination has been properly performed, and the system brought thoroughly under the influence of the cow-pox, a person has no more liability to take the small-pox than if he had had the small-pox itself. But unfortunately, from various causes, vaccination is either improperly performed, or the disease is only imperfectly developed, and in such cases it is desirable that re-vaccination should be effected. It is not, however, possible to say by looking at an arm, whether the operation has been properly performed, or the disease has perfectly developed. Under these circumstances it is no doubt desirable that every person should be re-vaccinated at least once in his life. The best time for the performance of this operation, where persons have been vaccinated in infancy, is between the twelfth and twentieth year. But in times of epidemic every person in the household should be re-vaccinated who has not been so before.

Thus much with regard to our knowledge of one means of averting this disease. That this means has not been adopted, arises partly from the ignorance of our population, partly from the perversity of our vestries and boards of guardians, and partly from the feebleness of our legislators.

The ignorance of our population of the means of preventing ordinary diseases is astounding, and if left to themselves with regard to vaccination, they will do nothing, from sheer ignorance of the nature of small-pox or the nature of its great antidote. Our coroners' courts bear testimony to this, where poor people have excused themselves for not having their children vaccinated, by not being aware of its value or of the measures to be adopted to get the operation performed. Surely this is one of the subjects to be embraced by the "relations of man to the universe," recommended by Professor Huxley in his programme of the education of children under the new School Board.

The perversity of vestries and boards of guardians has much to do with the present unvaccinated state of London. This epidemic has not come on London unanticipated. The medical officers of health in many districts warned their vestries of the coming evil, but, unfortunately, from our wholly exceptional legislation in sanitary matters, the power of looking after small-pox and vaccination is given to the Poor Law Board. The vestries have therefore thrown on the guardians the burden of providing for an attack of small-pox; and in many instances little or nothing has been done, either in the way of looking up the unvaccinated, putting in force the vaccination laws, or treating the first cases of small-pox with those precautions which the fearful nature of the disease imperatively demands.

With a bundle of Acts of Parliament passed at different times with different objects, and giving authority variously to the Privy Council, the Poor Law Board, the Home Secretary, and other bodies, it is no wonder that our Government acts feebly in sanitary matters. There have been no vigorous attempts on the part of the Government or the Legislature to meet the present outburst of small-pox. If cows and sheep had been attacked, it is probable that something new would have been done. The old machinery has, it is true, been kept in motion. There is the Vaccination Act, which threatens every person with a fine who does not have

* Further facts of this kind are recorded in Dr. Ballard's Essay on Vaccination, 1868.

his child vaccinated, but this law is regularly set at defiance. Besides this, even if boards of guardians and vestries are disposed to carry it out, they have no means of finding out unvaccinated children. There is, it is true, the Registration Act, but that Act does not make registration compulsory, and in some districts of London it has been stated that twenty-five per cent. of the population are not registered. There is no machinery yet set on foot to enable the inspectors of vaccination to lay hold of these children. Then there is the emigration of families from one parish to another. There is no plan of action for discovering these unvaccinated children in these families. It is vain for one parish to look after its vaccination, if other parishes do not. This is an Imperial question, and ought not to be left to Boards of Guardians.

But what can we do at once, so as, if possible, to avert the further course of the epidemic? We can hardly hope, from past experience, that the epidemic will cease at present, if nothing further be done. The measures which a knowledge of the nature of the disease would suggest are as follows:

1. There should be a vigorous attempt made at once to secure the vaccination and re-vaccination of all persons who have not yet undergone these operations. It is of no use to wait till such persons seek this for themselves. They must be found out, and found out at once. At the rate at which inspectors are now finding out the unvaccinated, the epidemic will have spent its force, and little, if any, life will be saved. But if the Legislature would at once interfere, and insist on a house-to-house visitation with a body of men armed with vaccine virus, and ability and power to vaccinate, the whole of London might be visited, and every inhabitant inspected or vaccinated, in the course of the next month. These agents must be medical men or medical students, who should be paid so much a day for their trouble. It is this question of paying that constantly hampers vestries and boards of guardians. They would rather see any number of people sick than pay to prevent their sickness. The Government must do it, and do it at once, or it will not be done at all. A few hundreds of pounds will do it, and it will save thousands that the small-pox would cost.

2. Vigorous efforts should be made to stamp out each case of small-pox where it occurs. Every case of the disease should be reported to the sanitary authority of the district, on pain of fine and imprisonment. The person affected should be either removed to a hospital or isolated. If the latter, the isolation should be complete. Satisfactory evidence of the isolation should be given to the medical officer of health, and unless he is satisfied with the means taken, some method of punishing the erring parties should be devised. Contagious diseases of all kinds may be thus arrested. It is the difficulty of discovering the first cases that makes the spread of contagious disease so rapid and extensive.

3. Disinfection should be insisted on. This subject requires more thought and attention than it has yet received. All possible means by which the poison can be conveyed from one person to another should be prevented. The poison of small-pox retains its vivaciousness or reproductive power more tenaciously, apparently, than any other animal poison. It can be conveyed in clothes, paper, thread, string, everything that it is possible to use in the sick room. The doctor may take it to his patients, the lawyer to his clients, or the clergyman to his congregation if he has been visiting the sick. The Levitical laws against leprosy would be hardly too severe to prevent the spread of small-pox. Rules of the most stringent kind ought to be laid down for the guidance of nurses and all persons entering the sick room. Above all, in every district where small-pox prevails, there should be a disinfecting apparatus. This should be an oven not heated by gas, but by a stove. The oven should be long enough to receive beds and all kinds of bed-clothes and wearing apparel. These things should be conveyed to the store in a covered van, which could at once be placed in the oven without opening it to remove its contents. Filthy rags and beds of straw and shavings should be burned in the stove. Such an apparatus is at present at work in the parish of St. Giles. It should be forthwith erected in every district in London. Even when the small-pox has killed its utmost, such ovens will be useful for a future war with the demon of contagion in some other form.

Will some philanthropic member of the House of Commons draw up a Bill embracing these suggestions, and get it passed into law as quickly as possible, so as to save the lives of some thousands of our population, and the faces and purses of many thousands more?

E. LANKESTER

GÜNTHER'S CATALOGUE OF FISHES

Catalogue of Fishes in the British Museum. By Albert Günther, M.A., M.D., LL.D., F.R.S., F.Z.S., &c. Eight volumes. (London, 1859-1870.)

THE recent issue of the eighth and last volume of Dr. Günther's Catalogue of Fishes brings to a completion one of the most laborious and important zoological works of the present epoch. For Dr. Günther's Catalogue is not a mere catalogue in the ordinary sense of the word, but rather a more or less complete history of all the known members of the class of fishes. Not merely the higher divisions of the class, but the genera and species are all fully characterised, and to each species is appended a list of the specimens of it contained in the British Museum. References to other species, either doubtful or not yet acquired by the national collection, are also added. When it is stated that our national collection of fishes now contains 29,275 specimens, some idea may be formed of the labour that has been involved in naming, arranging, cataloguing, and describing such a vast mass of materials. Each of these specimens has to be carefully examined, in many cases internally as well as externally, and to be compared with its brethren of the same and allied species, before it can be satisfactorily determined. Let our readers go through this process in the case of one fish, and they will be able to form some sort of idea of the amount of toil involved in repeating this experiment some thirty thousand times over. Dr. Günther has in fact expended about thirteen years of unremitting labour on this great work, and has had the good fortune to bring it to a felicitous conclusion. No general account of fishes has been published since Lacépede and Schneider's edition of Bloch about the beginning of the present century, as the celebrated "Histoire Naturelle des

Poissons" of Cuvier and Valenciennes, like too many other French publications, has never been finished.

When Dr. Günther commenced his labours in 1859, his work was apparently intended to be confined to the Acanthopterygian order of fishes, and the volumes were entitled accordingly. On the completion of this great order with the third volume in 1861, the scope and title of the publication were extended so as to embrace the whole class of fishes. The fourth volume, issued in 1862, was devoted to the Pharyngognathi and Anacanthini of Müller, whose system Dr. Günther has generally followed. Here, however, he has made a slight change in the nomenclature used by the great German Anatomist, considering that the structure of the fins is of more importance than that of the pharyngeal bones, and therefore changing Müller's name, "Pharyngognathi acanthopteri" into "Acanthopterygii pharyngognathi."

In the fifth volume of his work, issued in 1864, Dr. Günther commenced the order Physostomi, treating first of the Siluroids and allied forms, which were formerly associated with the Salmonidæ; in the sixth, published two years later, the Salmonidæ themselves were handled. This group, Dr. Günther informs us, both on its own account and from the large amount of literature involved in its investigation, offers such great difficulties to the ichthyologist, that "as much patience and time are required for the investigation of a single species of it as in the case of other fishes for that of a whole family." The ordinary method followed by naturalists in distinguishing and determining species is here utterly inadequate; and Dr. Günther does not hesitate to assert that "no one, however experienced in the study of other families of fishes, will be able to find his way through this labyrinth of variations without long preliminary study, and without a good collection for constant comparison. Sometimes forms are met with so peculiarly and so constantly characterised, that no ichthyologist who has seen them will deny them specific rank; but in numerous other cases one is much tempted to ask whether we have not to deal with a family which, being one of the most recent creation, is composed of forms not yet specifically differentiated."

Dr. Günther's preliminary remarks (vol. vii. p. 3), before he commences the discussion of the true *Salmones*, well merit perusal by any naturalist engaged on the differentiation of species. In the Salmonidæ, characters such as the proportion of one part of the body to another, and the number of fin rays, which in other groups of fishes are generally employed for the separation of species, fail entirely, and another set of characters has to be relied upon. To add to the confusion, some of the species at least *interbreed*, and "it is probable, although at present not yet confirmed by direct observation, that such hybrids mix again with one of the parent species, thereby producing an offspring more or less similar to the pure breed." The difficulties thus added to the correct determination of the Salmonidæ, may be easily understood.

The seventh volume of Dr. Günther's work, published in 1868, continues the history of the order Physostomi, and is devoted mainly to the extensive families Cyprinidæ and Clupeidæ, and to smaller groups nearly allied to them. In the eighth and last volume, published last year, Dr. Günther concludes the Physostomi with the eels and their allies, and then treats of the Lophobranchii and Plectognathi, which form the two last orders of the Teleostian subclass of fishes. The small subclass *Dipnoi*, embracing only the two Lepidosirens, comes next, and to them is appended a short notice of the recent discovery of the new Australian Mud-fish, which Mr. Krefft has referred to the Agassizian genus *Ceratodus*.* Dr. Günther states his inability at that period to determine whether it "should be referred to the Dipnoans or to the Ganoids, or should form the type of a separate subclass." But it is well known that he has since received perfect examples of this wonderful fish, and has in preparation a memoir which will, no doubt, put at rest all questions upon its structure and its position in the natural series.

The Ganoids (of which, formerly multitudinous, subclass Dr. Günther only recognises *six* existing species) and the Chondropterygians, or Sharks and Rays, follow next in order, and the eighth volume concludes with the two undoubtedly lowest forms of the class of fishes—the Lampreys and the Lancelet, an *invertebrated* vertebrate.

The total number of specimens of fishes in the collection of the British Museum at the period of the close of this great work was, as we have already said, 29,275. These are referred by Dr. Günther to 5,177 species. Besides these, 1,666 other species are recognised as valid, of which the national collection has not yet obtained examples, and 1,682 more are referred to as doubtful. "Assuming, then," says Dr. Günther, "that about one half of the latter will ultimately be admitted into the system, and that since the publication of the volumes of this work, about 1000 species have been described elsewhere, we may put the total "number of fishes at present known as about 9000."

In the preface to the last volume, Dr. Günther, besides giving us a general *résumé* of the extent of the collection under his charge, enters very fully into several other questions which are well worthy of attention, particularly at the present moment, when the relations between Government and Science are undergoing investigation by a Royal Commission. Whilst expeditions, fitted out by Austria, Prussia, and Italy, are despatched round the globe, accompanied by a staff of naturalists, and bringing back large collections of fishes to the national museums, our navy, it appears, is almost inert on this subject. Except from the Magellan Straits Surveying Expedition, to which Dr. Cunningham was attached as naturalist, no contribution from our Admiralty, which has so many ships always afloat, has reached the ichthyological department of the British Museum of late years. Yet it cannot be doubted that a very few words of encouragement from my Lords of the Admiralty would induce some of the many naval officers whose time must hang heavy on their hands at foreign stations, to turn their attention to collecting the common objects of the element on which they pass their lives. We assume, of course, that the expense of attaching a competent naturalist to any foreign expedition would be so great that "*my Lords would not feel justified in incurring it!*" Yet even the economical government of the United States thinks differently, and "each exploring American expedition was and still is accompanied by collectors, employed solely for the benefit of public museums."

* See NATURE, vol. ii. p. 106.

We should also, did not space fail us, like to call special attention to Dr. Günther's remarks on the importance of the study of the class of pikes as regards the elucidation of some of the most perplexing problems of Biology. "No other class of vertebrates," says our author, "is of equal importance to the geologist and palæontologist: the materials for comparing the living with past creations being so numerous and so diversified that we cannot help thinking that the relation of the various epochs to one another will be solved in the fields of ichthyology. Although fishes are mostly hidden by the elements in which they live, so that the knife of the anatomist generally first reveals new facts connected with their life, we have sufficient evidence to show that the phenomena of life are more varied in their different groups than in any other of the higher vertebrata, and that their study will form a solid basis for the solution of those general biological questions which, perhaps rather prematurely, agitate the minds of many zoologists."

OUR BOOK SHELF

On the Relations between Chemical Change, Heat, and Force. By the Rev. H. Highton, M.A.

We should not have noticed this paper, though it has been sent to us for review, had it not been marked as "Reprinted from the *Quarterly Journal of Science.*" This is its sole claim on our attention.

To put the contents in their simplest form before the reader, we may at once say that the Rev. Mr. Highton is a Perpetual-motionist. Not, perhaps, consciously—rather the reverse—but he belongs *in fact* to that singular class, though he would probably deny the charge with indignation. A short extract or two will, however, be sufficient to prove it to the satisfaction of any one acquainted with modern physics. Take the following:—

"Does there not, then, exist a power in nature for force to multiply force—even in the same way as life is multiplied by life through successive generations, and one living being may in due time become a thousand without losing its own vital energy?"

"We cannot . . . . produce heat without at the same time producing virtually in some shape or other an equivalent of cold."

"Cannot skill, mere skill, produce a less or greater disturbance and restoration of equilibrium, and so more or less force?"

If these extracts, which are perhaps not the richest which a careful search may discover, be not sufficient for the reader's amusement, we refer him to the original work. If they be not sufficient to prove to him the justice of our remarks, we refer him at once to some good scientific text-book, for he will have amply proved his need of instruction.

It would be an insult to our instructed readers to suppose that the fallacies of this paper require to be exposed *seriatim* for their benefit. *Ex uno disce omnes.* One will be given presently. But before giving it, we must strongly protest against the way in which many of the early, and some even of the later, discoveries of Joule are ignored throughout, while the attempts made to verify them by inferior experimenters, are put forth as original researches. When, however, Joule *does* happen to be referred to, the description of his experiments is wonderful indeed. Here is an example:

"He (Joule) churned various liquids in a calorimeter, and measured the increase of temperature. But in this kind of motion, as, perhaps, in all cases of friction, there is a pulling exertion of force, as well as one of pushing. Behind the arms of the paddle-wheel in the churn the liquid is pulled, and is pushed before them."

Comment on this sort of thing would be thrown away. When men like Helmholtz, Rankine, and Thomson vouch for the accuracy of a proposition, the world may well be indifferent to the criticisms of a Heath or a Highton. The grand founders of a rapidly progressing science cannot turn aside from their labours to answer frivolous objections. And it is strange and sad, indeed, that such excellent journals as the *Philosophical Magazine*, the *Quarterly Journal of Science*, and the *Chemical News* should diminish the space at their disposal for facts, by affording facilities for the dissemination of palpable nonsense and error.

As regards Electro-dynamics, Mr. Highton follows in the track of several better-known men, and is thus to a certain extent relatively excusable for his blunders, though they are quite as grave as those he commits with reference to the general theory of Conservation of Energy. The subject is by no means a very easy one, and it would certainly be somewhat hard to explain in a thoroughly popular manner the causes of his error. His difficulties, however (so far as we have had patience to investigate them), are such as have been met and overcome long ago by Joule, Faraday, and Thomson. (See, especially, Thomson, *British Association Reports*, 1852.) They are due, in great part, to his having confined his attention to the *zinc* alone of a galvanic cell.

Our Feathered Companions. Conversations of a Father with his Children about Sea birds, Song birds, and other feathered tribes that live in or visit the British Isles, their habits, &c. By the Rev. Thomas Jackson, M.A. (London: S. W. Partridge and Co.)

Dogs and their Doings. By the Rev. F. O. Morris, B.A. (London: S. W. Partridge and Co.)

The first of the books of which we give the titles above, is one we cannot take up without pleasure, because of the memories that its numerous illustrations bring to those who are shut up amongst bricks and mortar, of some wild sea-shore, sweet and tender woodland, or moor with gorse and fern; all of which are the homes of birds who would rather be free from the companionship of man than seek it, and which we love all the more for their wild freedom. "Our Feathered Companions" is full of information about birds, telling many well-known things about their habits and lives, of which children will never tire as long as there are children. With regard to the children in this book, we wish our author had drawn them from nature; we do not often meet with little boys who quote Greek out of school, and should be sorry if little girls were always moralising about birds being useful to man; we cannot read the narrative without feeling that their lives must have some other purpose beyond this, and we are glad that our children should be reminded by some of the little poems in the book, that they should love birds for their beauty, and learn all they can about them, rather than kill them to gratify a selfish desire for possession. The pictures of sea-birds are almost all charming, and there are many more of our favourite birds which, with a few exceptions, are very good.

"Dogs and their Doings" appears to be rather a compilation of old stories of wonderfully wise and clever dogs than the result of fresh observation,—the most extraordinary is of one which could pronounce words, and of another which understood the use of money; there are others which show wonderful affection and faithful memory for years after death. The book will be intensely interesting to children who love animals, they will recognise the tricks and sagacity of some of their friends in the dogs described; for dogs who know Sunday, and show great evidence of memory and reasoning powers, are to be met with in almost every family where they are cherished and loved. Some of the numerous illustrations are by Harrison Weir, others after Landseer; they are very attractive, though few of them are new.

Both volumes are got up in that attractive style with which we are familiar from Mr. Partridge, and make very pretty books for presents to young people.

Class Book of Inorganic Chemistry. By D. Morris, B.A. (London: George Philip and Son, 1870.)

THIS text-book is specially designed for pupils preparing for the Oxford and Cambridge Middle-class examinations and for the matriculation examination of the University of London. How far it will answer its purpose will be seen best by the number of students who use it as their guide and succeed in passing such examinations. Written for a special purpose, it is exempt from the criticism to which a general text-book would be subject, and we shall therefore amply point out how the book fails, in our opinion, to accomplish its purpose.

Nothing is more important for a student than that the definitions he learns should be clear and precise; yet those in this work are almost uniformly bad. Absolute *weight*, for example, is defined as "the amount of weighable matter in a body," which might have been simplified by saying, "weight is weight." Again, *solution* is defined as the "perfect union of a solid with a fluid," thus making the hydration of caustic lime an example of solution, and excluding the solution of gases in liquids.

It would be unjust to the author to state which nomenclature he has adopted; he has shown his impartiality by adopting nearly every one proposed; "potass," "potash," "potassium hydrate," and "caustic potash," are used indifferently; "sulphate of potassium" and "dipotassic sulphate" are used as synonyms for the substance having the formula $HKSO_4$. But the most objectionable feature in the book is a certain looseness of expression, leading to positive errors, which is certain to perplex the student. Examples of this abound; we need only quote a few. The solution of ammonia in water, we are told, "has all the properties of the gas;" again, "strontia forms with water a hydrate which has all the properties of baryta water," and "lime-water has all the properties of solutions of potash and soda."

Several statements occur in the book which are so absurd that they can only be traced to careless revision; but although they will scarcely mislead the merest tyro in the study of chemistry, they are none the less objectionable. We thought it very absurd to read that "combustion in air and in oxygen is exactly the same thing," and we thought it more absurd to read that "ammonium and sodium are distinguished by the smell of ammonia on the addition of caustic potash," but we only arrived at the climax of absurdity when we read that "nitrogen increases the volume of the atmosphere; and in this way provision is made for winds and other things useful for man's well being." Surely after this the text-books will cease to tell us that nitrogen is a very inactive substance.

It is only fair to say that the latter portions of the book are tolerably free from such errors as we have noticed; and the chapters on the heavier metals are the best in the book. Questions selected from the examination papers of Oxford, Cambridge, and London Universities, and the Science and Art Department, are given in several places, and the book concludes with tables for the analysis of simple salts. It is doubtful if a work of the size and scope of the one before us was at all needed, when there are so many excellent small manuals; but if such a one is really required, it must be much more carefully compiled and edited before it can be either useful or instructive.

F. J.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Measurement of Mass and Force

"W. M. W." seems to regard units of mass and force as things which are to be talked and philosophised about, rather than to be actually used for the measurement of concrete qualities. It is true that the force of gravity on a standard pound at a specified locality is a definite unit of force; but instead of specifying a locality to which reference is by common consent to be made (and without which consent we shall have as many different units as there are localities), he says, "The assumption of a hypothetical force of gravity not dependent on latitude seems to stand on the same footing as the employment of a mean solar day."

The average length of the apparent solar day is the same at all places on the earth, and is called a mean solar day. I am curious to see "W. M. W.'s" definition of a hypothetical force of gravity not dependent on latitude. It appears to me to be such stuff as dreams are made of.

As your readers are probably nearly tired of this discussion, I propose to conclude it on my side by the following summary of the whole question:—

Mass and *quantity of matter* are generally identified, and were identified by Newton in the opening paragraph of the Principia; but I am disposed to agree with "W. M. W." so far as to allow that the comparison of quantity of dissimilar kinds of matter is to a certain extent conventional, though there is, perhaps, no other convention which has so strong a basis of reason. If it is the case that all kinds of matter consist of one elementary material differently arranged, this convention must of necessity be no longer a convention, but a simple statement of fact, and portions of dissimilar matter which have equal masses, whether as measured by the inertia test or the gravitation test, consist of equal quantities of the elementary material. The rigorous agreement which is known to hold between these two tests, as applied to the comparison of different kinds of matter, may even be quoted as an argument for the existence of such a common material.

Whatever we may think of this convention or assumption (call it which we will), the phrase *a pound of matter* is legitimate and involves no assumption. A pound of sugar is a pound of matter, and a pound of lead is a pound of matter, independent of all hypothesis.

All authorities are agreed that, in treating kinetical problems, it is expedient to express masses and forces in such units as to satisfy the relation.

$$\text{Force} = \text{Mass} \times \text{Acceleration},$$

which, for a body, falling freely in vacuo, becomes

$$\text{Gravitating force} = \text{Mass} \times g.$$

Now comes the point of divergence, the main question being this: shall we (I.) measure force by reference to a standard of mass, or shall we (II.) measure mass by reference to a certain (or uncertain) standard of force. I showed in my last letter (p. 228) that the pound is practically, in going from place to place, a standard of mass, not of force, and that masses in different localities can be more directly compared than forces. It is surely reasonable to measure force by reference to a universally accessible standard of mass, rather than to measure mass by reference to a standard of force which, on the earth generally, is difficult of identification. I therefore uphold the following system:—

I. Take a pound of matter as the unit of mass; from which it follows that the gravitating force of a pound of matter has the numerical value g, and the unit of force is $\frac{1}{g}$ of the force with which a pound of matter gravitates.

This system was first proposed by Gauss, for the comparison of magnetic forces in different parts of the earth. It is the system which has been always taught by Sir William Thomson, and is adopted in Thomson and Tait's Natural Philosophy, Tait and Steele's Dynamics (except the first edition), the Reports of the Brit. Assoc. Committee on Electrical Standards (see especially the Report for 1863), Balfour Stewart's Elementary Physics, and the later editions of Atkinson's Ganot.

If we adopt the other alternative, we are driven to choose between certain subordinate alternatives, of which, with "W M. W.'s" help, I may enumerate three,

II. (1.) Localise your pound of force by specifying the standard locality of reference where the imperial pound gravitates with unit force. For this unit to be generally adopted, there must be general agreement as to the locality of reference, or what amounts nearly to the same thing, general agreement as to a definite reference-value of g. If we make this 32·2, the unit of force, at a locality where g has any other value, will be $\frac{32.2}{g}$ of the local gravitating force of the imperial pound, and the unit of mass will be the mass of 32·2 imperial pounds, so that the units will be a definite multiple of those employed in I.

This system is sound because (though in a roundabout way) it acknowledges the pound as a universal standard of mass, which is everywhere to be denoted by the same number, while it denotes the gravitating forces of pounds in different latitudes by different numbers.

If we take as unit of force the gravitating force of half an ounce at a place where g is 32, we obtain clumsy definition of the Gaussian unit of force, and our unit of mass becomes the pound.

(2.) File or load your pounds so as to make a pound at the pole gravitate with the same force as another and larger pound at the equator. This is the only way of making the pound a direct *standard of force* to the inhabitants of the world generally. The unit of mass will then be g times the mass of one of these filed or loaded pounds, and will everywhere represent the same mass.

(3.) Let every man adopt the local gravitating force of an imperial pound at the place where he happens to be, as his unit of force, and the mass of g imperial pounds as his unit of mass. This system gives a rough and ready unit of force, which is frequently adopted for rough purposes by all physicists; but experimental results stated in terms of it must be accompanied by a statement of the local value of g, to make them comparable with those obtained at other places. In dealing with masses in cases where forces are merely subsidiary, as in buying and selling goods, no one would recommend the adoption of the unit of mass which this system gives. In cases where forces and masses have to be considered in conjunction, this system has no advantage over I. in point of simplicity, and has the disadvantage of requiring us to express both forces and masses in terms of changeable units which tend to confusion of ideas. It is altogether inapplicable to astronomy, and is not even competent to express the mass of the earth; for it would make the mass of a cubic foot of matter at the earth's centre many millions of times greater than the mass of a cubic foot of gold or platinum at the earth's surface.

Writers who, without special explanation, express forces in pounds or grammes, and say that the mass of a body is numerically equal to its weight divided by g, must be classed as adopting this system; for though such expressions are ambiguous in themselves, this is the sense in which they will usually be received and applied. It is indeed the system which was almost universally taught until the publication of Thomson and Tait's Natural Philosophy.

I am not quite clear as to the particular system which "W. M. W." elects to adopt. He began (p. 145) by siding with Deschanel, who seems to adopt II. (3). In answer to my first letter (p. 167), he stated (p. 187) that "if a true pound, as determined at London, were carried to the North Pole, it would weigh more than a pound." If this be not an adoption of II. (2), it amounts to saying that at the North Pole a pound does not weigh a pound. In the second sentence of his last letter he adopts II. (1) without, however, distinctly committing himself to a definite locality of reference; and as long as this point is left open, every man will make the locality where he happens to be the locality of reference, so that II. (1) in this indefinite form degenerates into II. (3). In the same letter he says, "As a philosophical theory, I am perfectly ready to admit that the standard pound is most appropriately considered as a standard of mass, but the employment of this standard in text-book for the use of beginners seems calculated to lead to confusion." It rather appears to me that the refusal to accept this real standard of mass leads usually to a confused mixture of the systems II. (1), II. (2), II. (3); and I have shown in this letter that II. (1), which is the best of the three, does, in fact, make the pound a standard of mass.

I would earnestly commend to all teachers of dynamics the practice of Sir W. Thomson in strictly abstaining from the use of the word *weight* in all definitions and specifications relating to mass and force, as its ambiguous use does more than anything else to confuse these subjects. The weight of a body may and most frequently does mean its mass stated in pounds, or it may mean the force with which it gravitates. In the one sense its weight is the same whatever place it is carried to; in the other sense it varies from place to place.

I would also recommend for imitation Sir W. Thomson's practice of discarding the term *accelerating force*, which has been used to denote what ought to be called *force per unit of mass* or *intensity of force*, as distinguished from amount of force. Forces should always be expressed in terms of comparable units. As long as the learned recognise two non-comparable measures of force, they can hardly blame the unlearned for recognising a third and confounding force with energy.

The phrase *absolute force* (of a centre) is for the same reason objectionable. *Strength* of a centre is a better designation, and magneticians already speak in this sense of the strength of a magnetic pole. It would be a great advantage to have a short and handy name for some unit of force properly so called, and I venture to propose that the unit of force defined in I. be called a *kinit*. It may be formally defined as *that force which, acting on an avoirdupois pound of matter for a second, generates a velocity of a foot per second.* If we substitute gramme for pound, and metre for foot, we obtain a different unit which must be called by a different name, and of which 138¼ make one kinit. This numerical relation remains true, even if one of the forces compared be at the earth and the other at the moon. If any etymologist objects to making a word derived from Greek end in *it*, he may adopt the convenient fiction that it is an abbreviation of *kinetic unit*.

J. D. EVFRETT

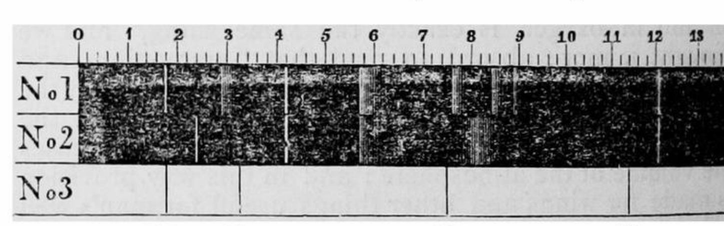
Belfast

The Spectrum of the Aurora

In the *Philosophical Magazine* for February there is a paper by F. Zöllner, on the Aurora Spectrum, in which he points out that, since the light of the aurora is as faint as that of the faintest vacuum-tube capable of being spectroscopically examined, while the mass of incandescent gas is almost infinitely greater, its temperature must be exceedingly low comparatively. He therefore supposes "that the spectrum of the aurora borealis does not correspond with any known spectrum of the atmospheric gases, only because, though a spectrum of our atmosphere, it is one of another order, and one which we cannot yet produce artificially."

I do not know if any other observer has noted the apparent coincidence in position of several of the auroral bands with those of a spectrum which I have occasionally obtained from air at low pressure, and with a feeble discharge. It is sometimes exhibited with great brilliancy by ordinary "lumière" tubes, and is, I believe, in part at least, the spectrum described by Wüllner (*Phil. Mag.*, June 1869), as a new spectrum of oxygen. I have certainly obtained it very vividly in pure electrolysed oxygen, with a feeble discharge; but some perplexing observations make me rather doubtful of its origin.

In the annexed sketch No. 1 represents the spectrum above



Hα Nα Hβ Hγ

mentioned, No. 2 that of the aurora, while No. 3 gives the lines of H and Nα for comparison.

I first noticed the coincidence of the yellowish-green line at 41 with the principal line of the aurora in Jan. 1870, and since that time have frequently repeated the observation; once with a spectroscope with a 60° bisulphide prism, and magnifying power of about six times. With this instrument the lines of both spectra appeared nebulous, but perfectly coincident. When the light from the vacuum tube is strong, this band appears shaded off gradually on the most refrangible side, but when it is no brighter than the aurora this is not noticeable.

The faint lines shown in the sketch were never bright enough to be compared with the same accuracy as the bright line, but repeated observations convince me that the positions given cannot be far wrong. Their relative brightness is very variable.

The red line, which was so brilliant in the auroral displays of last October, is very rarely visible, and does not coincide with any line which I have observed. The first red line of the tube spectrum is Hα, due to a trace of residual moisture.

I think it best to publish these imperfect results without more delay, in the hope that they may be corrected or confirmed by other observers, as I am unable at present to continue the research.

HENRY R. PROCTER

Royal College of Chemistry, Feb. 7

Resemblances of Plants *inter se*

MR. BENNETT, in his very interesting review of Mr. Mivart's "Genesis of Species" (NATURE, No. 66), refers to the close resemblance of an African Euphorbia and a South American Cactus" as an instance of "imitation," and to "the extraordinary resemblance of certain Conifers to flowerless plants" as "opposed to the theory of Natural Selection."

Neither example seems to be well chosen. In every case of supposed "mimicry" or "imitation," the question first arises, whether the resemblance between different organisms be or be not referable to similar conditions of life. Nobody will think a dolphin "imitates" a fish, nor does a climbing Tamus "imitate" a Humulus. These are cases of similar adaptations, but wholly unconnected with anything like mimicry in its true sense. An aquatic mode of life, or climbing habits, will necessitate certain peculiarities of structure, and the influence of an epidermis precluding evaporation will be the same in a Euphorbia as in a Cactus. The consecutive similarities of detail may without straining be referred to Mr. Darwin's "Correlations of Development."

There is still less cause for wonder in the resemblance of Conifers to flowerless plants, since it has been long ago shown by Hofmeister ("Vergleichende Untersuchungen," 1851, cf. Sachs, Lehrbuch der Botanik, 1868, pp. 310 and 384) that there is a close affinity between Lycopodiaceæ and Conifers, in spite of the chief boundary line of our system running between them. The resemblances of these respective orders therefore are scarcely more "extraordinary" than the resemblance of a Juncus to a grass, nor can I comprehend their bearing against the theory of Natural Selection.

Mr. Bennett deals rather strongly with botanists in denying to them, with rare exceptions, "a philosophic spirit." I cannot think this severe criticism to be applicable to the labours of Du Bary, Hofmeister, Sachs, Nägeli, Schwendener, Pringsheim, and other contemporaries of ours (confining myself to my own country), labours alike distinguished by comprehensiveness of generalisation and accuracy of detail.

Did Mr. Bentham really come over to "the evolutionists" "only within the last twelve months"? I should consider that he expressed his full assent to the ideas of Mr. Darwin already in his addresses of 1868 and 1869.

D. W.

Frankfort-on-the-Main, Feb. 21

The Genesis of Species

IF this note should meet the eye of the writer on Darwinism in the *North British Review* for June 1867, I should feel greatly obliged if he would explain the following passage, quoted by Mr. Mivart in p. 57 of his "Genesis of Species":—"The advantage is utterly outbalanced by numerical inferiority. A million creatures are born; ten thousand survive to produce offspring. One of the million has twice as good a chance as any other of surviving; but the chances are fifty to one against the gifted individual being one of the hundred survivors."

Is it an assumption or the statement of a fact that "one of the million has twice as good a chance as any other of surviving?" and how are "the hundred survivors" arrived at?

STUDENS

Fertilisation of the Hazel

IN NATURE for April 7, 1870 (vol. i. p. 583), Mr. Marcus Hartog stated, as the result of his observations, that the male catkins and female flowers of the hazel are not simultaneously developed on the same twig, and that therefore a kind of quasi-cross-fertilisation necessarily takes place. Although convinced at the time that my observation did not tally with Mr. Hartog's, it was then too late in the season to submit my impression to a practical test. During the past week I have closely observed the hazel bushes in flower, and have found on every bush which has come under my notice, the female and male flowers in a perfect state of development on the same ultimate twigs, in close proximity to one another, the stigmas being frequently loaded with pollen-grains, apparently from the neighbouring catkin; at all events there appears no provision of nature specially to promote fertilisation from other bushes. We see in fact here a confirmation of the general law suggested in my paper in the first number of NATURE, on "The Fertilisation of Winter Flowering Plants," that when plants flower in the depth of winter, and at a time when no or few insects are about, self-fertilisation is the rule rather than the exception, or in the case of unisexual flowers, as near an approach to self-fertilisation as is possible under the circumstances.

ALFRED W. BENNETT

Feb. 27

Sanitary Tests

DR. LANKESTER recently pointed out in NATURE that the existence and spread of fever were mainly owing to popular ignorance and the neglect of physiological laws. Since the article referred to was written, a greater plague has followed the fever then prevailing; and we are now told that Ireland is much better off than England—vaccination being there almost universal.

This state of things seems to me but a symptom of a more deeply-rooted evil, viz., the general neglect of physiological and sanitary science among the *higher* orders as well as the lower.

In how many large schools are the laws affecting the human body and the relation of man's frame to the destructive and constructive elements which surround us systematically taught? Are there any?

We talk of the gross ignorance and filthy habits of the poor; but both their ignorance and their filthiness are often a part of the very conditions of their existence. Have *we* been faithful stewards in this great matter?

Allow me to draw attention to the fact that, though Government has greatly facilitated and cheapened the acquisition of other branches of science, hygiene seems entirely omitted from the programme. But knowledge must descend from the higher to the lower levels. Ought not the heads of all public schools to undergo a compulsory examination in this subject? I could adduce instances where the greatest mischief has resulted from ignorance in such high quarters.

Aigburth, Liverpool, Feb. 25 — SAMUEL BARBER

Morell's Geometry

IN my ignorance of the fact that there are two Morells of very different calibres in the literary world, I find, to my great regret, that I have done serious injustice to the "widely-known" writer "on philosophy and grammar" and scholarly Mr. J. *D.* Morell, by attributing to him the authorship of the work on the "Essentials of Geometry," whose appearance I was compelled to notice so unfavourably in last week's NATURE. Pray give immediate publicity to this confession of error on the part of

March 1 — THE REVIEWER

Algaroba

AT p. 313 of NATURE (February 16) is a paragraph on the use of the Algaroba in the province of Catamarca, Argentine Republic, in which it appears to me the writer has confounded two or more plants. Algaroba is a name applied to *Ceratonia siliqua*, to several species of *Prosopis* in South America, and to *Hymenæa Courbaril* in Panama. In Brazil the last-named plant in called Jatai, from which, I presume, the writer of the paragagh in question has obtained his "*Hymenæa Courbaril-Jetaiba.*" The sweet fleshy pods of *Prosopis dulcis*, a tree widely spread over Southern and Central America, are used for feeding cattle, and several other species are employed for a like purpose in different parts of the tropics; it is therefore more than probable that *P. dulcis* is meant instead of *Hymenæa Courbaril.* Moreover, the pods of this latter are very thick and woody, and would not be easily "pounded in a wooden mortar;" and the tree cannot be well described as growing "to a height of forty feet, with wide-spread branches, and a rather slender stem," when we know that it frequently attains a hundred feet in height, and sixty feet in circumference.

JOHN R. JACKSON

Kew, Feb. 25

The Exeter Museum

THE paragraph in NATURE of the 23rd ult., respecting the Royal Albert Museum at Exeter, has excited my curiosity. Will you be so good as to mention an instance or two in proof that the museum "has done much towards attracting attention to the value of scientific knowledge in the West of England?"

It is difficult for a stranger to see how the case of the Plympton Grammar School can be ascribed to its influence.

Feb. 25 INQUIRER

Aurora Australis

[Extract from a letter received from Captain H. P. Wright, Ship *Gasfarth*]

Madras, Dec. 5.

I ENCLOSE you an account of the Aurora Australis as we saw it in the South, and I might also state that Mr. Pogson (Madras Observatory) says that the magnetic disturbance in these two days, 14th and 25th October, was so great that his instruments would not register the amount.

(Signed) H. P. WRIGHT

"October 24, 1870.—New moon at 6 P.M., lat. 42° S., long. 39° E.; at 7.30, as the twilight began to fade in the sky, we observed a bright rosy light at first resembling the reflection from a very red fire in the southern heavens. It extended from W. to E., and was visible from 8° to 50° high, being brightest at about 35° or 40°. Bright stars of the first and second magnitude shone through it. This cloud of crimson light had nearly all faded away by 9 P.M., first in the south-western direction, and so on gradually to the south-eastward. It may have been blown along by the wind, which was N.W. by W., but I did not think so. Other light clouds were passing; the sky below was its usual colour, and the stars shining very low down. As soon as this had passed away, there came a yellowish white, or milky white, light in the southern sky, and, as it were, taking the place of the crimson light. I should guess it to be about equal to $\frac{1}{3}$ of the moon's light, and showing a little bank of clouds of a dark-grey colour some 4° or 5° above the horizon underneath. This continued until 10.40 P.M., when it suddenly assumed a grander appearance. There was one long line of the brightest crimson some 8° or 9° broad, reaching up from south towards north, and some 70° high, fading into the normal colour of sky; this rose up a little to the west of the Southern Cross, on from this to the eastward was a great cloud or clouds of this bright crimson light, the bright star Canopus &c. showing through with a deep yellow light, and, passing over all, cumulus clouds carried somewhat quickly by the brisk breeze then blowing. To give, perhaps, a better estimate of the yellowish-white light, we could as long as it lasted only see the stars in the Southern Cross indistinctly. By midnight, or a little after, it all passed away, and we had lightning to the S.W. in the middle watch. The following night was very rainy, but the strong crimson and white light could still be discerned."

Aurora by Daylight

IN NATURE of Dec. 15, a correspondent asks the question, "Can Aurora be seen in daylight?" I answer, yes, beyond a doubt. In the autumn of last year (I cannot give the date nearer than that it was early in Ooctober) my eye was attracted by an unusual motion, in what at the first glance appeared to be a light fleecy cloud, but was in reality a broad ribbon of Aurora of a yellowish white colour, which changed its form and position with the peculiar streaming motion of the Aurora, sometimes almost fading entirely and again recovering its comparative distinctness.

It was about four o'clock in the afternoon when my attention was drawn to it, and I watched until late in the evening, and saw it as the dusk came on, supported by fainter streamers of light, which stole out as the darkness increased, and almost imperceptibly grew into one of those magnificent auroral displays so frequently seen here.

The Aurora, as I first saw it, was about N.W. by N., and I should say 30° above the horizon, and the sky was beautifully clear and free from clouds.

Will any of your correspondents inform me if the intensity of auroral light, as proved by its visibility in daylight, teaches us anything more than is at present known of the Aurora? And I should be much obliged to anyone who will inform me if the spectrum of lightning has ever been obtained, and if so, how it compares with the spectrum of the Aurora.

W. G. THOMPSON

Matapediac, Province of Quebec, Feb. 4

Tigers at Bay

IN NATURE for Feb. 2, p. 275, a doubt is expressed as to whether a tiger when in danger will ever take to a tree. An anecdote related in vol. 2, p. 112 of De Beauvoir's "Voyage Round the World," seems to settle the question:—"Attacked and conquered by the buffalo, the tiger bounded some thirty feet into the air into a cocoa-nut tree. Some twenty natives were in an elevated position amongst the branches of this tree; in one and the same moment they let themselves fall like ripe fruit from a tree that is shaken." G. E. D.

Furzewell House, Torquay

Dr. Donkin's Natural History of the Diatomaceæ

THE reviewer of the above-named work (see NATURE, vol. iii., p. 210) describes the plates as inferior to those in the Synopsis. With this opinion I fancy many will differ; as correct representations of the species described, they are far in advance of those in the Synopsis, for example, compare the figure of *Navicula tumens* in the latter work with *Navicula rostrata* in Dr. Donkin's; with the exception of the outline, the figure in the Synopsis does not resemble that species, and is inferior to Ehrenberg's in the Microgeologie. Also compare *N. Hebes*, *N. palpebralis*, *N. subsalina*, *N. latiuscula*, *N. alpina*, with the corresponding forms in the Synopsis, and I think the superior fidelity of Dr. Donkin's illustrations will be conceded.

Many of the forms in the Synopsis must have been drawn from memory, they are so glaringly inaccurate, *e.g.*, *Amphipleura pellucida* is represented with marginal punctæ, *Nitzschia bilobata* with indistinct distant striæ. The marginal dots on *A. pellucida* existed only in the delineator's imagination; and, as every student of the Diatomaceæ knows, *N. bilobata* has close but distinct striæ.

I agree with "W." that the synonymy might have been more extended. It is, however, next to impossible to identify from figures or descriptions the forms intended by the early observers.

The desirability of giving habitats in full is questionable; three or four localities are sufficient, as with very few exceptions the same species would be found (the localities being similar) in any part of the United Kingdom.

In conclusion, I would remark that the following species are not Ehrenberg's, as stated by "W.," but Gregory's:—*Navicula Smithii* var. *fusca*; *N. Smithii* var. *suborbicularis*; *N. Smithii* var. *nitescens*, and *N. latissima*.

I know the works of Ehrenberg, Kutzing, Rabenhorst, Grunow, and Greville, but who is Cleeve? K.

PROPOSED OBSERVATIONS OF VENUS

THE following circular has just been issued by the Observing Astronomical Society:—

The committee of the society have decided to undertake a series of systematic observations of the planet Venus, during one complete revolution, for the purpose of obtaining results that shall lead to our becoming better acquainted with the markings which are visible on her surface, and a correct knowledge of their form and permanency.

In common with other observers it has been to them a matter of regret that although this beautiful object approaches nearer to us than does any other member of the solar system (our satellite excepted), yet that our knowledge of its superficial condition should be far less than of those planets less favourably situate. In most astronomical works the information concerning Venus is very meagre, whilst the drawings of her appearance exhibit, in the majority of cases, merely a blank crescent.

Yet, in turning to the ancient observations made of this planet, the committee have been struck by the large number recorded, many exhibiting well-defined markings, and when they considered the numerous observations of the same character made of late years, including several important ones from members of the society, it seemed evident that observation of this planet was not so difficult as is generally represented. It was seen further that if a proper discussion and analysis of all recorded observations were made, the result might be a large addition to our knowledge of the planet's surface.

The Committee, therefore, in inaugurating this important movement, divide the work to be done into three branches:—

1. The formation of a sub-committee of astronomical observers (including non-members of the society) for the purpose of continually observing Venus during one complete synodical revolution.

2. The collection of all ancient observations and drawings of the planet.

3. The collection of as much modern data as possible from existing observations, and from public and private records.

At the conclusion of the observations of the sub-committee, the results obtained, together with the ancient and modern observations collected, will be placed in the hands of a competent astronomer for complete analysis and discussion, when the results obtained will be published.

Those observers who are willing to join the "Venus Observation Sub-Committee" are requested to send their names and addresses to the hon. secretary of the Society, Ashley Road, Bristol, before March 10, stating the aperture and power of the instrument they intend to employ.

The observations will commence on March 20, previous to which a circular, containing full instructions, will be issued to every observer who has expressed his willingness to assist in the project.

PROFESSOR DE NOTARIS AND HIS NEW WORK ON MOSSES

IN the year 1838 Professor De Notaris, of the Universities of Turin and Genoa, published a Syllabus Muscorum, which he now calls *Un lavoro giovanile* (a juvenile work). Still it is a very useful manual for the young Italian bryologist, for whom it was first written, and to whom it gives not only the exact characters of each plant, but also their localities and the synonymy. From that time, year after year, he went on increasing and improving his work, till he had gathered together sufficient materials for a new work. It was not, however, till last year that he succeeded in persuading the bountiful municipality of Genoa to take his MS. in hand and publish it, which they effectually did at their own cost some months ago. The simple title of De Notaris' work* was mentioned by one English scientific periodical; but, except Dr. Braithwaite, the well-known contributor of the "Recent Additions to our Moss Flora," and Mr. C. J. Smith, who recently published a "Moss Flora of Sussex," accompanied by a clear and really instructive paper on the structure and reproduction of mosses,† I have hardly heard of any other English bryologist taking account of the newly edited *Atti della Universitá di Genoa*, the first of which, in large quarto, is entirely taken up by Professor De Notaris' standard work.

This painstaking and well-digested composition represents, as it were, the *résumé*, or, we might say, the quintessence of the long and seldom interrupted labours of the veteran bryologist, who, far from rejecting or disregarding the improvements that bryology has recently made, is quite willing to adopt them wherever he finds them of practical value, but not when, instead of the promised gold, they only give glittering or useless tinsel.

"I really find myself"—thus he writes in his preliminary comments—"in a sort of quandary (*specie di peritanza*) whenever I have to produce any ulterior, though obvious, re-adjustment, which might probably be attributed to some deplorable mania of upsetting and overthrowing the monumental edifice of the European bryology, or even to an ungracious instinct (*parmaloso istinto*) of seizing upon and appropriating the works of others. For, after all, we must acknowledge that the *Bryologia Europæa* (of Schimpfer) is the bible of every student of mosses."

Whilst admitting that any very small and apparently insignificant character may at times be useful to establish a natural group, De Notaris believes that such differential characters occasionally assumed, to complete the diagnosis of some genus or species, cannot be taken as absolute and invariable, and therefore he prefers to stick to the old Linnean canon: *Quæ in uno genere ad genus stabiliendum valent, minime idem in alio genere præstant.*

Thus pointing out the different forms of cells in the epicarp of *Bryum cæspitosum* and *B. erythrocarpum*, and seeing that it is not in the power of anyone to examine all the capsular membranes of each individual plant of these two species, he says: "We must not wonder if now and then an exception to the ruling character of the cells is to be found," observing, at the same time, that a pretty good character for distinguishing certain natural groups might also be found in the specific conditions of the *endochrome*. Another striking remark on this subject he makes, where he says that "the epithet *pachydermic*, applied to the cellule of the epicarps, might easily be a source of error, owing to either a stronger or weaker cohesion of the chromoplasm, in which case the cellules might well be pachydermic, and flocculently membranaceous at the same time."

He further observes upon the variable characters of the inflorescence that "*Mnium medium* is found both monœcious and polygamous, as well as *Bryum torquens* and *Bryum pendulum*. *Catoscopium* is not seldom to be found monœcious as well as diœcious. Even that form, he says, which I have distinguished under the name of *Bryum bimoideum*, may turn out at some time to be merely a diœcious form of *B. bimum*."

The *peristome*, in his view, is but an exudation or, as it were, a hypertrophy in the evolution of the capsule, not an indispensable means for the ripening of the sporules. Hence the reason why he considers it, morphologically speaking, as an apparatus of secondary importance. Thus there are specimens of *Hypnum stellatum*, which bear their ordinary normal capsules, whilst there are others whose capsules are like those of the apothecia, with imperfect and nearly Leskeaseous peristoms.

Other important reflections are to be found in the same "Comments," such as the one relating to the *perichætial* leaves, which serve to distinguish a *Limnobium* from a *Hypnum* better than any other distinction taken from the habit of the stem or the arrangement of the leaves; and the other on the topographical distribution of mosses in Italy, where the great variegation of the "Bryological mantle" mainly depends upon the various constitution, elevation, or undulation of the soil; the heights and the valleys, naked, or well timbered and green with copse and grove, variously contributing to their infinitely varied diversifications.

Among the vagrant species of all regions he points out particularly the *Bryum alpinum*, which, "from the uppermost boundaries of the Alpine zone, spreads itself down to the very brackish pastures that scarcely raise themselves above the sea level in the islets of the Strait of Bonifacio." Again, that gem of mosses *Schistostega osmundacea* (Bridel's *Catoptridium smaragdinum*) which has been till late a *desideratum* in the Italian flora, De Notaris informs us he found himself whilst, in 1865, he was crossing the Morgan on his way to Mont Rosa.

J. GAGLIARDI

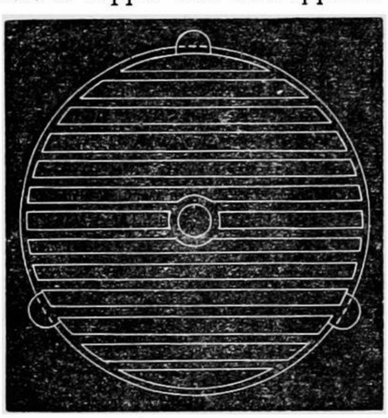
* "Epilogo del'a Briologia Italiana."

† "I have seen," writes Mr. Smith, "your friend's *Epilogo*, and am much struck with it. What a fine work, and brought out at the cost of the Municipality! When will such things occur in England?"

*A CONSTANT FORM OF DANIELL'S BATTERY**

GRAHAM'S discovery of the extreme slowness with which one liquid diffuses into another, and Fick's mathematical theory of diffusion, cannot fail to suggest that diffusion alone, without intervention of a porous cell or membrane, might be advantageously used for keeping the two liquids of a Daniell's battery separate. Hitherto, however, no galvanic element without some form of porous cell, membrane, or other porous solid for separator, has been found satisfactory in practice.

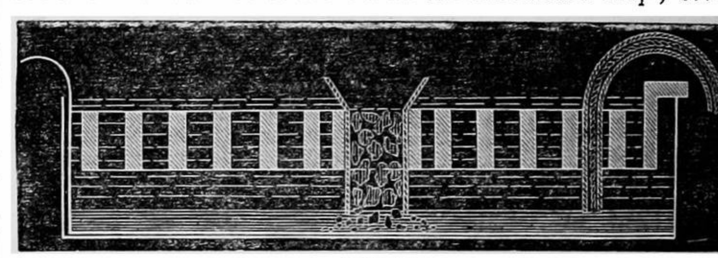
The first idea of dispensing with a porous cell, and keeping the two liquids separate by gravity, is due to Mr. C. F. Varley, who proposed to put the copper-plate in the bottom of a jar; resting on it a saturated solution of sulphate of copper; resting on this a less dense solution of sulphate of zinc; and immersed in the sulphate of zinc, the metal zinc-plate fixed near the top of the jar. But he tells me that batteries on this plan, called "gravity batteries," were carefully tried in the late Electric and International Telegraph Company's establishments, and found wanting in economy. The waste of zinc and of sulphate of copper was found to be more in them than in the ordinary porous-cell batteries. Daniell's batteries without porous cells have also been tried in France, and found unsatisfactory on account of the too free access of sulphate of copper to the zinc, which they permit. Still, Graham's and Fick's measurements leave no room to doubt but that the access of sulphate of copper to the zinc would be much less rapid if by true diffusion alone, than it cannot but be in any form of porous-cell battery with vertical plates of copper and zinc opposed to one another,



GROUND PLAN OF NEW BATTERY

as are the ordinary telegraphic Daniell's batteries which Mr. Varley finds superior to his own "gravity battery." The comparative failure of the latter, therefore, must have arisen from mixing by currents of the liquids. All that seems necessary, therefore, to make the gravity battery much superior instead of somewhat inferior to the porous-cell battery, is to secure that the lower part of the liquid shall always remain denser than the upper part. In seeking how to realise this condition, it first occurred to me to take advantage of the fact that saturated solution of sulphate of zinc is much denser than saturated solution of sulphate of copper. It seems† that, at 15° temperature, saturated aqueous solution of sulphate of copper is of 1·186 sp. gr., and contains in every 100 parts of water 33·1 parts of the crystalline salt; and that at 15° the saturated solution of sulphate of zinc is of sp. gr. 1·44, and contains in every 100 parts of water 140·5 parts of sulphate of zinc, both results being from Michel and Krafft's experiments.‡ Hence I made an element with the zinc below; next it saturated solution of sulphate of zinc, gradually diminishing to half strength through a few centimetres upwards; saturated sulphate of copper resting on this; and the copper-plate fixed above in the sulphate of copper solution. In the beginning, and for some time after, it is clear that the sulphate of copper can have no access to the zinc otherwise than by true diffusion. I have found this anticipation thoroughly realised in trials continued for several weeks; but the ultimate fate of such a battery is that the sulphate of zinc must penetrate through the whole liquid, and then it will be impossible to keep sulphate of copper separate in the upper part, because saturated solution of sulphate of zinc certainly becomes denser on the introduction of sulphate of copper to it. To escape this chaotic termination, I have introduced a siphon of glass with a piece of cotton wick along its length inside it, so placed as to draw off liquor very gradually from a level somewhat nearer the copper than the zinc; and a glass funnel, also provided with a core of cotton wick, by which water semisaturated with sulphate of zinc may be continually introduced at a somewhat lower level. A galvanic element thus arranged will undoubtedly continue remarkably constant for many months; but it has one defect, which prevents me from expecting permanence for years. The zinc being below, must sooner or later, according to the less or greater vertical dimensions of the cell, become covered with precipitated copper from the sulphate of copper which finds its way (however slowly) to the zinc. On the other hand, if the zinc be above, the greater part of the deposited copper falls off incoherently from the zinc through the liquid to the copper below, where it does no mischief, provided always that the zinc be not amalgamated, —a most important condition for permanent batteries, pointed out to me many years ago by Mr. Varley. Placing the zinc above has also the great practical advantage, that, even when after a very long time it becomes so much coated with metallic copper as to seriously injure the electrical effect, it may be removed, cleaned, and replaced, without otherwise disturbing the cell; whereas if the zinc be below, it cannot be cleaned without emptying the cell and mixing the solutions, which will entail a renewal of fresh separate solutions in setting up the cell again. I have therefore planned the following form of element, which cannot but last until the zinc is eaten away so much as to fall to pieces, and which must, I think, as long as it lasts, have a very satisfactory degree of constancy.

The cell is of glass, in order that the condition of the solutions and metals which it contains may be easily seen at any time. It is simply a cylindrical or rectangular jar with a flat bottom. It need not be more than ten centimetres deep; but it



SECTION

may be much deeper, with advantage in respect to permanence and ease of management, when very small internal resistance is not desired. A disc of thin sheet copper is laid at its bottom. A properly shaped mass of zinc is supported in the upper part of the jar. A glass tube (which for brevity will be called the charging-tube) of a centimetre or more internal diameter, ending in a wide saucer or funnel above, passes through the centre of the zinc, and is supported so as to rest with its lower open end about centimetre above the copper. A glass siphon with cotton-wick core is placed so as to draw liquid gradually from a level about a centimetre and a half above the copper. The jar is then filled with semisaturated sulphate of zinc solution. A copper wire or stout ribbon of copper coated with india-rubber or gutta percha passes vertically down through the liquid to the copper plate below, to which it is riveted or soldered to secure metallic communication. Another suitable electrode is kept in metallic communication with the zinc above. To put the cell in action, fragments of sulphate of copper, small enough to fall down through the charging tube, are placed in the funnel above. In the course of a very short time the whole liquid below the lower end of the charging tube becomes saturated with sulphate of copper, and the cell is ready for use. It may be kept always ready by occasionally (once a week for instance) pouring in enough of fresh water, or of water quarter saturated with sulphate of zinc at the top of the cell, to replace the liquid drawn off by the syphon from near the bottom. A cover may be advantageously added above, to prevent evaporation. When the cell is much used, so that zinc enough is dissolved, the liquid added above may be pure water; or if large internal resistance is not objected to, the liquid added may be pure water, whether the cell has been much used or not; but after an interval, during which the battery has not been much in use, the liquid added ought to be quarter saturated, or even stronger solution of sulphate of zinc, when it is desired to keep down the internal resistance. It is probable that one or more specific gravity beads kept constantly floating between top and bottom of the heterogeneous fluid will be found a useful adjunct, to guide in judging whether to fill up with pure water or with sulphate of zinc solu-

* From the Proceedings of the Royal Society.
† Storer's Dictionary of Solubilities of Chemical Substances. Cambridge, Massachusetts: Sever and Francis, 1864.
‡ Ann. Ch. et Phys. (3) vol. xli. pp. 478, 482, 1854.

tion. They may be kept in a place convenient for observation by caging them in a vertical glass tube perforated sufficiently to secure equal density in the horizontal layers of liquid to be tested by the floaters.

An extemporised cell on this plan was exhibited to the Royal Society, and its resistance (measured as an illustration of Mance's method, described in the first of his two previous communications) was found to be ·29 of an Ohm (that is to say, 290,000,000 centimetres per second). The copper and zinc plates of this cell, being circular, were about thirty centimetres in diameter, and the distance between them was about 7·5 centimetres. A Grove's cell, of such dimensions that forty in series would give an excellent electric light, was also measured for resistance, and found to be ·19 of an Ohm. Its intensity was found to be 1·8 times that of the new cell, which is the usual ratio of Grove's to Daniell's. Hence seventy-two of the new cells would have the intensity of forty of Grove's. But the resistance of the seventy-two in series would be 209 Ohms, as against 76 Ohms of the forty Grove's; hence, to get as powerful an electric light, threefold surface, or else diminished resistance by diminished distance of the plates, would be required. How much the resistance may be diminished by diminishing the distance rather than increasing the surface, it is impossible to deduce from experiments hitherto made.

Two or three cells, such as the one shown to the Royal Society, will be amply sufficient to drive a large ordinary turret-clock without a weight; and the expense of maintaining them will be very small in comparison with that of winding the clock. The prime cost of the heavy wheel-work will be avoided by the introduction of a comparatively inexpensive electro-magnetic engine. For electric bells, and all telegraphic testing and signalling on shore, the new form of battery will probably be found easier of management, less expensive, and more trustworthy, than any of the forms of battery hitherto used. For use at sea, it is probable that the sawdust Daniell's first introduced on board the *Agamemnon* in 1858, and ever since that time very much used both at sea and on shore, will still be found the most convenient form; but the new form is certainly better for all ordinary shore used.

The accompanying drawing represents a design suitable for the electric light, or other purposes, for which an interior resistance not exceeding $\frac{2}{10}$ of an Ohm is desired. The zinc is in the form of a grating, to prevent the lodgment of bubbles of hydrogen gas, which I find constantly, but very slowly, gathering upon the zincs of the cells I have tried, although the solutions used have no free acid, unless such as may come from the ordinary commercial sulphate of copper and commercial sulphate of zinc crystals which were used.

The principle which I have adopted for keeping the sulphate of copper from the zinc is to allow it no access to the zinc except by true diffusion. This principle would be violated if the whole mass of the liquid contiguous to the zinc is moved toward the zinc. Such a motion actually takes place in the second form of element (that which is represented in the drawing, and which is undoubtedly the better form of the two) every time the crystals of sulphate of copper are dropped into the charging-tube. As the crystals dissolve, the liquid again sinks, but not through the whole range through which it rose when the crystals were immersed. It sinks further as the sulphate of copper is electrically precipitated on the copper plate below in course of working the battery. Neglecting the volume of the metallic copper, we may say, with little error, that the whole residual rise is that corresponding to the volume of water of crystallisation of the crystals which have been introduced and used. It becomes, therefore, a question whether it may not become a valuable economy to use anhydrous sulphate of copper instead of the crystals; but at present we are practically confined to the "blue vitriol" crystals of commerce, and therefore the quantity of water added at the top of the cell from time to time must be, on the whole, at least equal to the quantity of water of crystallisation introduced below by the crystals. Unless a cover is added to prevent evaporation, the quantity of water added above must exceed the water of crystallisation introduced below by at least enough to supply what has evaporated. There ought to be a further excess, because a downward movement of the liquid from the zinc to the level from which the syphon draws is very desirable to retard the diffusion of sulphate of copper upwards to the zinc. Lastly, this downward movement is also of great value to carry away the sulphate of zinc as it is generated in the use of the battery. The quantity of water added above ought to be regulated so as to keep the liquid in contact with the zinc, a little less than half saturated with sulphate of zinc, as it seems, from the observations of various experimenters that, the resistance of water semisaturated with sulphate of zinc is considerably less than that of a saturated solution. A still more serious inconvenience than a somewhat increased resistance has been pointed out to me by Mr. Varley as a consequence of allowing sulphate of zinc to accumulate in the battery. Sulphate of zinc crystallises over the lip of the jar, and forms pendants like icicles outside, which act as capillary siphons, and carry off liquid. Mr. Varley tells me that this curious phenomenon is not unfrequently observed in telegraph batteries, and sometimes goes so far as to empty a cell and throw it altogether out of action. Even without this extreme result, the crystallisation of zinc about the mouth of the jar is very inconvenient and deleterious. It is of course altogether avoided by the plan I now propose.

In conclusion, then, the siphon-extractor must be arranged to carry off all the water of crystallisation of the sulphate of copper decomposed in the use of the cell, and enough of water besides to carry away as much sulphate of zinc as is formed in the use of the battery. Probably the most convenient mode of working the system in practice will be to use a glass capillary siphon, drawing quickly enough to carry off in a few hours as much water as is poured in each time at the top; and to place, as shown in the drawing, the discharging end of the siphon, so as to limit the discharge to level somewhat above the upper level of the zinc grating. It will no doubt be found convenient in practice to add measured amounts of sulphate of copper by the charging-tube each time, and at the same time to pour in a measured amount of water, with or without a small quantity of sulphate of zinc in solution.

As 100 parts by weight of sulphate of copper crystals contain, as nearly as may be, 36 parts of water, it may probably answer very well to put in, for every kilogramme of sulphate of copper, half a kilogramme of water. Experience (with the aid of specific-gravity beads) will no doubt render it very easy, as a perfectly methodical action involving very little labour, to keep the battery in good and constant action, according to the circumstances of each case.

When, as in laboratory work, or in arrangements for lecture-illustrations, there may be long intervals of time during which the battery is not used, it will be convenient to cease adding sulphate of copper when there is no immediate prospect of action being required, and to cease pouring in water when little or no colour of sulphate of copper is seen in the solution below. The battery is then in a state in which it may be left untouched for months or years. All that will be necessary to set it in action again will be to fill it up with water to replace what has evaporated in the interval, and stir the liquid in the upper part of the jar slightly, until the upper specific gravity-bead is floated to near the top by sulphate of zinc, and then to place a measured amount of sulphate of copper in the funnel at the top of the charging-tube.

W. THOMSON

NOTES

AT the Anniversary Meeting of the Geological Society, held on the 17th ult., the Wollaston Gold Medal was presented to Prof. Ramsay in recognition of his many researches in practical and in theoretical geology; and the balance of the proceeds of the Wollaston Donation Fund was given to Mr. Robert Etheridge in aid of the publication of his great stratigraphical "Catalogue of British Fossils." A report of the interesting proceedings on the occasion will be found in another column. Mr. Prestwich was re-elected President of the Society for the ensuing year.

A STATUTE was promulgated last week in Convocation at Oxford enlarging the powers of the delegates of "unattached students," whom it is proposed to allow to admit students after examining them in one of the subjects already permitted (*i.e.*, Classics and Mathematics), "together with some other subject recognised in the schools of the University" (*e.g.*, Physical or Natural Science).

We are indebted to *Harper's Weekly* for some of the interesting notes on science in America, which we are able to furnish this week, in advance of their publication there. This magazine provides its readers with an important and interesting summary in each issue of the progress of science on that side of the Atlantic.

DR. WILLIE KÜHNE, at present Professor of Physiology at Amsterdam. has been called to occupy the chair left vacant at Heidelberg by the removal of Helmholtz to Berlin.

IT is with great pleasure that we hear from the Abbé Moigno that the publication of *Les Mondes* will have recommenced before this, on March 1. Though the Abbé has escaped uninjured from the bombardment, a portion of his fine library was destroyed by a Prussian shell. We are still without files of any of the Paris papers.

IN reply to a question in the House of Commons on Friday night last, Mr. Ayrton said that considerable progress had been made in the preparation of the plans for the erection of the Natural History Museum at South Kensington, but until they had been completed it would be impossible for them to receive the sanction of the Government.

THE lectures of the present year for the Royal College of Physicians will be delivered at the College, Pall Mall East, at five o'clock on each of the following Wednesdays and Fridays Gulstonian lectures—Dr. Gee, March 3, 8, 10, "On the Heat of the Body;" Croonian lectures—Dr. Parkes, March 15, 17, 22, "On some points connected with the Elimination of Nitrogen from the Human Body;" Lumleian lectures—Dr. West, March 24, 29, 31, "On some disorders of the Nervous System in Childhood."

THE third course of Cantor Lectures for the Session of the Society of Arts will be delivered by Dr. T. S. Cobbold, F.R.S., and will treat of "Our Food-producing Ruminants, and on the Parasites which reside in them." The course will commence on Monday, the 17th of April, and will be continued on subsequent Monday evenings till completed. These lectures are open to members, who have also the privilege of admitting two friends to each lecture.

MR. ANDREW MURRAY will, during the following season, deliver a short course of lectures for the Royal Horticultural Society on Economic Entomology, especially in its relations to Horticulture and Forestry. Mr. Murray has been mainly instrumental in forming the Society's collection of Economic Entomology exhibited in the South Kensington Museum.

FLORIDA has, this winter, its usual complement of scientific visitors, who are engaged in prosecuting investigations upon its natural history. Mr. E. J. Maynard, of Massachusetts, is exploring the ornithology of the keys and the southern portion of the State; Mr. N. H. Bishop, of New Jersey, and Mr. George A. Boardman, of Maine, are at work with a similar object about Jacksonville. Professor Wyman, of Cambridge, is also making use of the opportunities of his third or fourth visit to the State in the critical examination of the ancient mounds and shell heaps which abound everywhere.

DR. PACKARD has lately announced the discovery, by Prof. Verrill, of a dipterous larva of the genus *Chironomus*, at a depth of 120 feet, in the vicinity of Eastport, Maine. He also describes. a mite, or *Acarus*, as occurring at a similar depth. He has not yet ascertained whether, like other species of the genus, the latter lives, in any of its stages, in the gills of the lamellibranchiate mollusca.

A RECENT communication to the State Department from the United States consul at St. Helena, states the fact that the white ants, which have effected a lodgment in the island, are rapidly destroying everything upon it. No wood but teak, and sometimes not even that, escapes their fangs; and numbers of houses in Jamestown have been fairly gutted by them—doors, window-sashes, floors, and roofs, all being eaten up, leaving nothing but the bare walls.

THE following is a list of the German learned men connected with the French Académie des Sciences, copied from the *Comptes Rendus*, although some of them have died since the investment of the city, but their names were not erased from the list for want of official notification:—Four Associates, MM. Ehrenberg, Liebig, Wöhler, Kummer: three mathematicians, Neumann, Weierstrass, Kronecker; one mechanician, Clausius; three astronomers, Hansen, Argelander, Peters; three physicists, Weber, Mayer, Kirchhoff; two chemists, Bunsen, Hofmann; three mineralogists, Naumann, Rose, Haidinger; four botanists, Mohl, Braun, Hofmeister, Pringsheim; one anatomist, Siebold; three surgeons, Virchow, Rokitansky, Sebert.

THE special correspondent of the *Times* gives the following account of the effects of the bombardment on the Jardin des Plantes:—No fewer than eighty-three shells had fallen within this comparatively limited area. On the night of January 8 and 9 four shells fell into the glass houses and shattered the greater part of them to atoms. A heap of glass fragments lying hard by testified to the destruction, but the effect of the shells was actually to pulverise the glass, so that it fell almost like dust over the gardens. The consequence was that nearly the whole of this most rare and valuable collection was exposed to one of the coldest nights of the year, and whole families of plants were killed by the frost. Some of the plants suffered the most singular effects from the concussion; the fibres were stripped bare, and the bark peeled off in many instances. All the Orchids, all the Clusiaceæ, the Cyclantheæ, the Pandaneæ were completely destroyed, either by the shells themselves or by the effects of the cold. The large palm-house was destroyed, and the tender tropical contents were exposed to that bitterly cold night; yet, singularly enough, although they have suffered severely, not one has yet died. All through the whole of the fortnight during which these gardens were subjected to this rain of shells, MM. Decaisne, Chevreuil, and Milne-Edwards, remained at their posts, unable to rest, and have since, at their own expense, repaired the damage done, trusting that whatever form of government France may choose, it will not repudiate its debts of honour. M. Decaisne is making out a list of his losses, a large proportion of which might possibly be supplied from Kew, while owners of private collections might also be glad to testify their sympathy and interest in the cause of science by contributing whatever they may be able to spare as soon as the amount and nature of the loss is ascertained. The animals fared better than the plants—not only have none of them been eaten by the population of Paris, as the latter fondly suppose, but although several shells burst among them, they have escaped uninjured. Of course, when food was so scarce for human beings, the monkeys and their companions were put upon short allowance. This fact, coupled with the extreme rigour of the season, increased the rate of mortality among them, and one elephant died, but was not eaten. The two elephants and the camel that were eaten belonged to the Jardin d' Acclimatation, and had been removed in the early stage of the siege from their ordinary home in the Bois de Boulogne, for safety, to the Jardin des Plantes, where, however, it would appear, it was not to be found. The birds screamed and the animals cowered, as the shells came rushing overhead and bursting near them, as they do when some terrific storm frightens them; latterly they seemed to become used to it.

WE have great pleasure in announcing that the Museum of Natural History at Strasburg has escaped the bombardment of the town. One shell entered one of the corridors and destroyed a small collection of chalk fossils, and a few fragments of a shell decapitated two or three birds. The concussion caused nearly all the glass in the cases to be broken. But the fine collections of mammals, of birds, and of fossils, the result of many years of labour of Prof. Schimper, are perfectly untouched. This has not been the case with some of the private collections in the town, and one, a collection of European Lepidoptera, belonging to the Taxidermist of the museum, was scattered into dust.

WE learn with great regret that the Société d' Acclimatation of Paris has thought it necessary to pass a resolution erasing from the list of its members all sovereigns and princes of German States engaged in the late war against France, "considérant que la manière dont le bombardement de Paris a été effectué par les armées allemandes constitue un acte contraire au droit des gens, ainsi qu'aux plus simples notions de l' humanité."

AMONG the experiences of the Mount Washington winter party may be mentioned an exposure to perhaps the greatest cold ever recorded in the annals of science. The temperature of 40° below zero was not in itself unusual; but to this was added a hurricane blowing at the rate of ninety miles an hour. The combination of such a wind with the temperature indicated would probably have been entirely unsupportable but for the means of protection enjoyed by the party in the dwelling which had been fitted up expressly for their accommodation.

PROF. J. YOUNG has recently brought the question of the education of the mining engineers before the Institution of Engineers for Scotland. His proposals for its amelioration are as follows:—"1. Great improvement in secondary schools, especially in teaching arithmetic, geometry, elements of natural philosophy. 2. The establishment in some large towns, such as Liverpool and Birmingham, of colleges on the Scottish model, or on that of Owens College—fairly well endowed—giving chiefly general scientific training, with a few special technical chairs. 3. The practical recognition of the value of scientific training by engineers who take pupils. (*a*) By giving free pupilships or valuable scholarships; (*b*) By admitting as pupils only those who have passed certain recognised examinations; (*c*) By co-operating with colleges as examiners; (*a*) By inserting in agreements with their pupils that during winter they shall attend certain classes; (*e*) By giving some privileges, in connection with engineering societies, to graduates."

WE regret to learn that the fine specimen of *Pandanus odoratissimus* in the Botanical Gardens at Glasnevin, near Dublin, has been completely destroyed by the attacks of a fungus, in all probability the same that has destroyed the Screw Pine in the Breslau Gardens, as referred to by Prof. Oliver in a late number of this journal. The Glasnevin plant was nearly fifty years old.

MESSRS. BELL AND DALDY will shortly publish "Outlines of Magnetism and Electricity for Public Schools and Science and Art Examinations," being notes of a course of lectures delivered at the Royal School of Naval Architecture, with an introduction on the First Principles of Physics, by W. F. Barrett.

WE learn from the *British Medical Journal* that Dr. Thorne Thorne, who has been very successfully engaged for several years as an occasional inspector under the Privy Council, and who more especially led to the exposure of the causes of the Terling epidemic, has been appointed to a permanent position under the Privy Council.

THE *British Medical Journal* states that the rumour that Dr. Liebreich, the distinguished ophthalmologist of Paris, was likely to be appointed ophthalmic surgeon and lecturer at St. Thomas's Hospital, is likely to be realised. The reputation of Dr. Liebreich is more than European, and his services to ophthalmoscopic science and practice are such that he may fairly claim to be considered as almost the founder of our present school of ophthalmoscopy. The services which he has rendered to science are cosmopolitan, and we feel assured that if, under the existing state of affairs in Paris, Dr. Liebreich elects to take up his residence in London, his services, reputation, and personal character will secure for him that welcome which the English profession has always been wont to extend to distinguished men of science of every nation, and which well becomes the members of a liberal profession in a country proud of its freedom and hospitality.

TWO natives of the Garrow Hills in Madras are to be trained as vaccinators to practise in their tribe, which suffers severely from small-pox. On the other hand the villages in Kunnool oppose the entrance of vaccinators by force, and hide their children in the jungle.

THE Siam papers report the fortunate news of the capture of an albino or white elephant. He had been brought to the capital in state, and will in due time succeed to the highest dignities of state, the chief white elephant ranking next the Queen, and the heir apparent coming next only to this elephant.

MR. W. KING, of the Indian Geological Survey, reports from Ballary, in Madras, that he doubts the reported discovery of coal in that district.

THE *Homeward Mail* states that the cold has been so intense at the Mullier in Scinde lately, that on January 24th icicles were found on the works connected with the viaduct in that place. It is hardly possible to believe this phenomenon were it not communicated by a reliable eye-witness.

A PAPER, read to the Academy of Sciences, Paris, during the siege, gives some very interesting information about the great cold experienced there, and its occurrence in former years. In the fifty years from 1816 to 1866, the average temperature of the month of December has been 3·54° Centigrade above zero, but December, 1870, gave a average of 1·07° C. *below zero*, thus showing how far below the average the cold of last year was. In the *Annales de la Société Météorologique*, vol. v., 1861, is a paper by M. Renou, "On the Periodicity of Great Cold." In this he shows that about every forty years there comes round a series of cold winters, in general five or six together, of which the central one is the coldest of all. His researches extend back to the fifteenth century, but to take recent times he notices the great frosts and cold winters which group themselves round the years 1709, 1748, 1789-90, and again in 1829-30. From these facts he predicted in 1860 that there would be a group of severe winters round the winter of 1870-71.

IN furtherance of the British Guiana Local Exhibition to be held in that Colony during the present year, the Committee of Correspondence has issued an address to the "Farmers and other Proprietors of the Soil," calling upon them energetically not only to aid the Exhibition by simply sending specimens of the products of the Colony to be seen, passed by, and forgotten, but to bear in mind how many useful products are lost to commerce through lack of continued exertions in the cultivation of the plants and the supply of the home market, and pointing out how that Exhibitions "can never be regarded as entirely satisfactory until additions are permanently made to the stock of commodities which are in daily use at home, as well as to the list of such as make the resources and the importance of the Colony known and esteemed in the markets of the world." British Guiana is a colony rich in natural productions, and it is to be hoped that the pamphlet will be read and digested by those to whom it is addressed.

ANOTHER tigress is recorded as dead in India, at the hands of Major Daires, of the Madras Presidency, and none too soon. She had, in seven years, killed above 140 persons in a few villages, so that many families had left, and a great part of the land had gone out of cultivation.

ON the 28th October two shocks of earthquake were felt at Shikarhera, in Upper Scinde. On the 1st November a severer shock was felt in the Tinnevelley district than ever had been known in the memory of man. The shocks were undulatory.

THE LONDON CONJOINT EXAMINING BOARDS

We reprint from the *British Medical Journal* the draft scheme of the committees of the Royal Colleges of Physicians and Surgeons, and the Society of Apothecaries, in the form in which it was presented to the College of Physicians at their Comitia and approved. In this form the scheme differs essentially from that of last year. This is no longer, as it now appears, a scheme for a minimum qualification for English practitioners, complete, unified, and preliminary in all respects to higher diplomas.

The following is the draft scheme :—

1. That one Board of Examiners, in the division of the United Kingdom, be appointed by the Royal College of Physicians of London, the Royal College of Surgeons of England, and the Society of Apothecaries of London, for the examination of candidates who desire to practise Medicine, Surgery, and Midwifery.

2. That candidates who shall have passed the several examinations of the Board be entitled, subject to the bye-laws of each institution, to the Licence of the Royal College of Physicians of London, the Diploma of Member of the Royal College of Surgeons of England, and the Certificate of the Society of Apothecaries of London.

3. That Examiners be appointed as follows ; viz. :

In Medicine, by the Royal College of Physicians and the Society of Apothecaries.

In Surgery, by the Royal College of Surgeons.

In Anatomy and Physiology, by the Royal College of Physicians and the Royal College of Surgeons.

In Midwifery, by the Royal College of Physicians, the Royal College of Surgeons, and the Society of Apothecaries.

In Materia Medica, Medical Botany and Pharmacy, Chemistry, and Forsenic Medicine, by the Royal College of Physicians and the Society of Apothecaries.

4. That the number of Examiners assigned to each subject be as follows, viz. : Anatomy and Physiology, not less than 8 ; Chemistry, Materia Medica, Medical Botany and Pharmacy, not less than 8 ; Medicine, not less than 10 ; Surgery, not less than 10 ; Forensic Medicine, not less than 4 ; Midwifery, not less than 6.

5. That the appointment of the Examiners in each subject be made by each of the three Corporations in the following proportion ; viz., Anatomy and Physiology—Royal College of Physicians, 3 ; the Royal College of Surgeons, 5. Chemistry, Materia Medica, Medical Botany and Pharmacy—the Royal College of Physicians, 4 ; the Society of Apothecaries, 5. Surgery—the Royal College of Surgeons, 10. Forensic Medicine—the Royal College of Physicians, 2 ; the Society of Apothecaries, 2. Midwifery—the Royal College of Physicians, 2 ; the Royal College of Surgeons, 2 ; the Society of Apothecaries, 2.

6. That there be two or more examinations on professional subjects, and that the fee payable for the examinations be thirty guineas, to be paid in two or more payments.

7. That one-half of the fees received for the examinations be appropriated to the payment of the examiners and the expenses of the examinations.

8. That the other half of the fees be divided amongst the three Corporations, upon the principle of giving to each Corporation a sum proportionate to that which each has respectively obtained from the grant of licences on the average of the last five years.

9. That the mode of division of the second half of the fees be subject to revision at the end of every three years.

ERNST HAECKEL ON THE MECHANICAL THEORY OF LIFE AND ON SPONTANEOUS GENERATION

In his recently published Biological Studies * Professor Haeckel, of Jena, has briefly stated his views on the question of Abiogenesis, which is now so largely occupying attention in England. He, having done more than any other observer to establish the Protoplasm theory by his discovery of organisms of the simplest conceivable structure—not even cellular (*i.e.*, not even possessing a differentiated central nucleus)—his remarks on the present condition of the Spontaneous Generation question must possess great weight. The philosophy of Monism, of which he is the exponent, as opposed to Dualism, rests on what he calls the Carbon theory and the Plastic theory. These theories are thus set forth in weighty, but clear sentences :—

1. The forms of organisms and of their organs result entirely from their life, and simply from the interaction of two physiological functions, Heredity and Adaptation.

2. Heredity is a part of the reproduction,—Adaptation, on the other hand, a part of the maintenance of the organism. These two physiological functions depend, as do all forms of vital activity, on the character of the physiological organ through which they come into play.

3. The physiological organs of the organism are either simple Plastids (Cytods or Cells), or they are parts of Plastids (*e.g.*, Nuclei of Cells, cilia of Protoplasm), or they are built up of numerous Plastids (the majority of organs).

In all these cases the forms and actions of the organs are to be traced back to the forms and actions of the individual Plastids.

4. Plastids are either simple Cytods (structureless bits of Protoplasm without nuclei) or Cells ; but since these last have originally arisen from Cytods by a differentiation of the inner "Nucleus" and the outer "Protoplasm," the forms and vital properties of all Plastids can be traced back to the simplest Cytods as their starting point.

5. The simplest Cytods, from which all other Plastids (Cytods and Cells) originally have arisen by Heredity and Adaptation, consist essentially and absolutely of nothing more than a bit of structureless Protoplasm—an albuminoid, nitrogenous Carbon-compound ; all other components of Plastids have been originally formed secondarily from Protoplasm (plasma-products).

6. The simplest independent organisms which we know, and which moreover can be conceived, the Monera, consist in fact while living of nothing else but the simplest Cytod, a structureless bit of Protoplasm : and since they exhibit all forms of vital activity (nutrition, reproduction, irritability, movement), these vital activities are here clearly bound on to structureless Protoplasm.

7. Protoplasm, or Germinal Matter (*Bildungsstoff*), also called Cell-substance or Primitive Slime (*Urschleim*), is therefore the single material basis (*materielle Grundlage*), to which without exception and absolutely all so-called "vital phenomena" are radically bound ; if the latter are regarded as the result of a peculiar Vital Force independent of the Protoplasm, then necessarily also must the physical and chemical properties of every inorganic natural body be regarded as the result of a peculiar force not bound up with its substance.

8. The Protoplasm of all Plastids is, like all other albuminoid or Protein-bodies, composed of four inseparable elements, Carbon, Oxygen, Hydrogen, and Nitrogen, to which often, though not always, a fifth element, namely, Sulphur, is added.

9. The forms and vital properties of Protoplasm are conditioned by the peculiar manner in which Carbon has

* A series of papers published originally in the *Jenaische Zeitschrift* during the year 1870, the author's Monograph of the Monera—for a translation of which see the Quart. Journal of Micr. Sci., 1869 ; also, the January No., 1871, where M. Haeckel's views on Coccoliths and Bathybius are noticed and illustrated in a plate.

combined itself so as to form a highly developed compound with the three or four other elements named. Compounds devoid of Carbon never exhibit those peculiar chemical and physical properties which exclusively belong to only a part of the compounds of Carbon (the so-called "organic compounds"); on this account modern chemistry has replaced the term "organic compounds" by the more significant term "Carbon-compounds."

10. Carbon, then, is that element, that indivisible fundamental substance, which, in virtue of its peculiar physical and chemical properties, stamps the various Carbon compounds with their peculiar organic character, and in chief fashions this Protoplasm, the "matter of life" (*Lebenstoff*), so that it becomes the material basis of all vital phenomena.

11. The peculiar properties which Protoplasm and the other component tissues and substances of the organism derived secondarily from it, exhibit, especially their viscid condition of aggregation, their continual change of matter (on the one hand their facile decomposition, on the other their facile power of assimilation) and their other "vital properties," are therefore simply and entirely brought about by the peculiar and complex manner in which Carbon under certain conditions can combine with the other elements.

12. The entire properties of the organism are, therefore, ultimately conditioned with equal necessity by the physical and chemical properties of Carbon, as are the entire properties of every salt and every inorganic compound conditioned by the physical and chemical properties of its component elements.

II.

We now pass on to the chapter in Haeckel's work headed "The Monera and Spontaneous Generation." Although, remarks Haeckel, Darwin himself states in his work that he has nothing to do with the origin of life, every thinking reader of the "Origin of Species" must ask himself whence came the simplest original living form? and no question has been more actively discussed, in consequence of Darwin's reform of the descent theory than that of spontaneous generation (*Urzeugung*). Abiogenesis (*Urzeugung*, which may best be translated as Archigenesis) is, in fact, a necessary and integral part of the universal evolution theory. It is the natural bridge which places in continuity Kant's and Laplace's theory of the mechanical origin of the universe and the earth, with Lamarck's and Darwin's theory of the mechanical origin of animal and vegetal forms. When we perceive that all inorganic nature, as well as the development of organic nature from an original parent organism, is explained by the continual working of one great law of evolution, we cannot admit in explanation of the one dark point in this great causal network a supernatural act of creation. We are logically bound to seek a natural link, and this link is Archigenesis (*Urzeugung*), *i.e.*, the origin of the simplest organisms from so-called lifeless inorganic materal. Till recently the question of Archigenesis has been treated by most naturalists in a most unphilosophical and superficial manner. Instead of examining the bearings of the question in all directions, and discussing duly its very complex nature, they have rushed into experiment, and obtained an answer without fairly putting the question. Because in highly artificial apparatus and under artificial conditions no organisms have been developed in certain fluids prepared for examination, the whole doctrine has been denied, and the totally unwarranted conclusion arrived at, "There is no Archigenesis." Such experiments as those of Pasteur and other very marvellous ones have really no value in this question, since they merely prove that in the particular case, under certain artificial and complex conditions, no organism is formed by Archigenesis. Positive contradiction of the hypothesis of Archigenesis is impossible. Positive proof there is not yet, since no one has yet seen any organism take origin, except by Parentage. But, thanks to our progress in biology during the last ten years, the question no longer presents the theoretical difficulties which it did. Before the discovery of those simplest of conceivable organisms, the Monera—it was necessary that from inorganic materials a Cell should be formed by Archigenesis—an organism presenting two chemically, physically, and morphological distinct portions, the inner Nucleus and the external Protoplasm. The formation of such a nucleated Plastid by Archigenesis is difficult to conceive. But now by the discovery of the Monera the matter assumes quite a different aspect. Such forms as *Protogenes* and *Protamœba* present no definite shapes, have no individual development, but grow and multiply by division. Their growth and nutrition is purely a physico-chemical process, just as the growth of a crystal, with this difference, that the viscid cohesion of Protoplasm entails intussusception of nutriment, whilst the crystal grows at its surface only. The same viscid state of cohesion explains the reproduction of such Monera—which we do not observe in crystals—the cohesive power of the Protoplasm under certain conditions of nutrition is no longer sufficient to hold the body together, and fission occurs. Thus the chief vital phenomena of Monera are traced to physico-chemical causes.

Since in our chemical laboratories, with our exceedingly limited and rough methods, we have succeeded in forming many Carbon-compounds, and have good reason to suppose that we may one day synthetically produce albuminoid bodies, is it not reasonable to suppose that in the great laboratory of Nature, similar but more complex chemical synthesis may go on, such as the formation from inorganic materials of albuminoids and of living protoplasm? If we are to call in a special creative act—superior to mere chemical synthesis—to account for the existence of Protoplasm because we have not succeeded in forming it artificially, so also must we postulate a peculiar creative act for a great variety of minerals, such as felspar, fluor spar, heavy spar, augite, &c., since we are equally unable to build up these inorganic bodies. In this way we should divide the whole world into a Natural and a Supernatural group. The former would contain such salts, gases, &c., as we can build up in the laboratory, also alcohol, acetic acid, &c. All these bodies have arisen by Archigenesis, *i.e.*, by natural, mechanical means, solely by the interaction of the inherent physical and chemical forces of their matter. The latter group would contain all minerals not yet formed in the laboratory, also all the complex Carbon-compounds. These bodies would be considered as arising by "Creation," that is, by supernatural means, through a mysterious creative force existing externally to the bodies.

To every philosophic naturalist such a view must appear as untenable as is every assumption of a "Creation." On the other hand the assumption of an Archigenesis for the first living beings from which all others have developed, is a logical postulate of the human intelligence.

It is not at all remarkable that as yet we have not observed the Archigenesis of Monera. Supposing it were taking place every day and every hour, it would be very difficult to observe. Very minute particles of Protoplasm are found in quantity, both in sea and fresh water, when carefully sought for. They are generally regarded as fragments of decomposing organisms. But what proof is there of this? and how could it be clearly proved that these particles have *not* arisen by Archigenesis? The first commencement of a Protoplasm-granule in a fluid would be as difficult to observe as the first commencement of a crystal in its mother-liquor. And not less difficult would be the observation of the gradual growth of such an excessively minute Protoplasm-granule into the larger protoplasmic masses of *Protamœba*, &c.

Bathybius seems to be of the greatest significance for the theory of Archigenesis. For if not through Archigenesis, whnce shall we derive this protoplasmic covering of the deepest sea-bottom?

Either the Monera were once for all, at the beginning of organic life on the earth, produced by Archigenesis—and hence—since Monera exist still to-day—they must have reproduced in a direct line unchanged for many million years; or, in the course of the earth's history, they have been produced by recurring acts of Archigenesis, and in this case there is no reason why this process should not occur at the present time. The latter view presents the fewest difficulties and exceptions to Prof. Haeckel. In any case the Monera still living at the present day point out to us the way to a correct understanding of the Origin of Life, and clear away the greatest difficulties which the hypothesis of Archigenesis previously presented. E. R. L.

SCIENTIFIC SERIALS

THE report of the meeting of Swiss Naturalists (*Schweizerische Naturforschende Gesellschaft*) in Solothurn on the 23-25 August, 1869, opens with an admirable presidential address on the progress of scientific investigation in Switzerland, by Prof. Lang. The reports of the sections contain numerous short notices of memoirs read, but among these are some of greater length and importance, such as a report by Dr. Hengi on his experiments in the culture of *Saturnia Yama-maya* and *S. mylitta*, a notice by M. F. Hermann on the use of Saussure's hair-hygrometer for scientific observations, a paper on creosote and other products by Prof. Völkel, a communication on fluorescence by Prof. Hagenbach, and a paper (printed in full) by Dr. von Fellenberg, containing analyses of some nephrites and jadeites, including a stone implement of the latter mineral from Möhringen-Steinberg, which he regarded as having been imported from the east. From his analyses of nephrites from different localities, it appears that they contain silica and magnesia in equal equivalents, but that the proportion of each of these to the lime varies as 7, $7\frac{1}{2}$, 8, 9 : 3. Hence he regards them as forming not a definite mineral species, but a group of silicates of magnesia and lime formed by the metamorphosis of rocks of similar composition. He does not consider the nephrites allied to the amphiboles and augites. The memoirs include a report by Prof. Pictet de la Rive on the present state of the question as to the limits of the Jurassic and Cretaceous periods, a notice of which has already appeared in NATURE; an account of the Miocene Flora of Spitzbergen, by Prof. Oswald Heer; and a report on the investigation and preservation of the erratic blocks in Switzerland, by MM. A. Favre and L. Soret. An amusing account of the festal doings of the Society concludes the volume.

FROM the Natural History Society of Stralsund (Neu-Vorpommern) and Rügen, we have received the second annual part of their *Mittheilungen* containing the proceedings of the society for the year 1869. It contains a life and list of the works of Prof. A. E. Legnitz, by Prof. von Feilitsch; a description of the Island of Gottland, with a notice of the birds inhabiting it, by M. L. Holtz, unfortunately disfigured by some very absurd misprints; a notice on the proper heat of plants, by Dr. J. Romer, containing details of experiments made with *Philodendron pinnatifidum* Schott, from which it appears that the proper heat of this plant is much higher than stated by C. H. Schulz; and a paper (illustrated) on the itch-mites of fowls by Prof. Finsterberg. In the last-mentioned paper the author describes a species of mite inhabiting the feet of fowls, for which he proposes the formation of a new genus, *Knemidokoptes* (recte *Cnemidocoptes*). He names the species *K. viviparus*, as it produces living young.

THE last part of the *Archives Néerlandaises des Sciences Exactes et Naturelles à Harlem* for 1870 contains the following papers:—J. A. Groshaus on the Specific Heats of Solids and Liquids, which is a continuation of his former papers published in the same journal, in which he confirms Kopp's results in obtaining a constant when the specific heat of every substance is multiplied with its atomic weight, the constant being 6·3 to 6·5.—C. K. Hoffmann and H. Weijenbergh, jr., on the position of Chiromys (The Aye-Aye of Madagascar) in the natural classification. This is an elaborate memoir which was crowned by the society in 1869, and treats of all the characters in detail, their final decision being to make the following classification: Mammalia, Order ii. Quadrumana, Sub-order ii. Prosimiæ, Families: 1, Lemurini; 2, Nyeticebini; 3, Macrotarsi; 4, Microtarsi. The fourth family is distinguished by the tarsal bones more or less elongated (*allongés*), and by difference in the dentary systems, especially in the character of the incisor teeth. In common with Macrotarsi they have the tail, long large eyes directed in front, and large ears, and they feed chiefly on insects. Of this family two genera are given; Microcebus Geoff. and Chiromys Cuv., two species of the first one found in Madagascar, and one of the second, the common Aye-Aye.—M. G. F. W. Baehr gives a note on the Results of the Mathematical Study of the Movements of the Eye.—M. H. H. von Zouteveen on the Petrified Forest of Cairo, and by the same author on the Synthesis of Sulphocyanate of Ammonium.—M. Van der Willigen on Holtz's Electrical Machine.—M. A. C. Oudemans, jr., on the Volumetric Estimation of Iron by Hyposulphite of Sodium.—M. H. Weijenbergh, jr., on Parthenogenesis among the Lepidoptera.—M. C. Ritsema on the Origin and Development of *Periphyllus testudo* v.d.H.; and lastly a report on the Purification of the Air of Hospitals by the Combustion of the Organic Germs, by MM. J. van Genns and L. H. von Baumhauer. This is the report of an investigation undertaken at the instance of the Dutch Government to determine if the apparatus devised by M. Woestyn, of Paris, completely destroy all vital properties in the germs. They report that the apparatus contains nothing new, and that it does not effect its purpose any better than the ordinary methods in use.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 19.—"Modification of Wheatstone's Bridge to find the Resistance of a Galvanometer Coil from a single deflection of its own needle," by Prof. Sir William Thomson, F.R.S. In any useful arrangement in which a galvanometer or electrometer and a galvanic element or battery are connected, through whatever trains or network of conductors, let the galvanometer and battery be interchanged. Another arrangement is obtained which will probably be useful for a very different, although reciprocally related object. Hence, as soon as I learned from Mr. Mance his admirable method of measuring the internal resistance of a galvanic element (that described in the first of his two preceding papers), it occurred to me that the reciprocal arrangement would afford a means of finding the resistance of a galvanometer-coil, from a single deflection of its own needle, by a galvanic element of unknown resistance. The resulting method proves to be of such extreme simplicity that it would be incredible that it had not occurred to any one before, were it not that I fail to find any trace of it published in books or papers; and that personal inquiries of the best informed electricians of this country have shown that, in this country at least, it is a novelty. It consists simply in making the galvanometer-coil one of the four conductors of a Wheatstone's bridge, and adjusting, as usual, to get the zero of current when the bridge contact is made, with only this difference, that the test of the zero is not by a galvanometer in the bridge showing no deflection, but by the galvanometer itself, the resistance of whose coil is to be measured, showing an unchanged deflection. Neither diagram nor further explanation is necessary to make this understood to any one who knows Wheatstone's bridge.

Zoological Society, February 21.—Mr. Osbert Salvin, F.Z.S., in the chair. The Secretary announced the birth of a young Hippopotamus in the Society's Gardens, which had taken place that day, being the first occurrence of this event in England, although this animal had previously bred in some of the Continental Gardens.—Mr. Sclater exhibited and made remarks upon the tusk of an Indian elephant, which appeared to have been attacked by parasites.—The Secretary exhibited, on behalf of Mr. E. Ward, F.Z.S., a remarkably fine series of heads and horns of sheep and other wild animals, which had been collected in Ladahk by Mr. George Landseer.—A communication was read from Dr. W. Peters, F.M.Z.S., containing a note on the *Tænia* from the Rhinoceros, in reference to a previous communication from Dr. Murie, to the society, upon the same subject.—A communication was read from Mr. J. H. Gurney, F.Z.S., containing remarks on certain species of Abyssinian birds.—A communication was read from Dr. J. Anderson, C.M.Z.S., containing notes on certain Indian reptiles belonging to the collection of the Indian Museum, Calcutta.—A second communication from Dr. Anderson contained the descriptions of eight new species of birds, recently collected by him during the Yunan expedition.—A paper was read by Mr. W. C. Atkinson, containing descriptions of some new species of Diurnal Lepidoptera, discovered in Yunan by Dr. Anderson during the same expedition.—The Secretary communicated a paper by Mr. Edward Bartlett, containing observations on the habits and distribution of the monkeys of Eastern Peru, as observed during a recent four years' sojourn on the Upper Amazons.

Geological Society, February 8.—Mr. Joseph Prestwich, F.R.S., president, in the chair. 1. "On the Punfield Formation," by Mr. John W. Judd. Those formations, which have been deposited under *fluvio-marine* conditions, and which yield at the same time marine, fresh-water, and terrestrial fossils, are of especial interest to the geologist, as they furnish him with a means of correlating the great fresh-water systems of strata with those of marine origin. At the bottom of the Wealden we have one such fluvio-marine series, the well-known Purbeck formation; at its summit is another, less known, but not less important, for which the name of "Punfield Formation" is now suggested. Some of the fossils of the latter were first brought under the notice of geologists by Mr. Godwin-Austen in 1850, and their peculiarities have since been the subject of remark by Prof. E. Forbes, Sir C. Lyell and others. The typical section of the beds is at Punfield Cove, in the Isle of Purbeck, where they are about 160 feet thick, and include several bands with marine shells. The lowest and most remarkable of these yields about forty well-defined species, many of which, as well as one of the genera, are quite new to this country. A section somewhat similar to that of Punfield is seen at Worborrow Bay. In the Isle of Wight, at Compton, Brixton, and Sanddown Bays, similar fluvio-marine beds are found at the top of the Wealden, and attain to a thickness of 230 feet. The marine bands here, however, yield but a very scanty fauna. Indications of the existence of beds of the same character and in a similar position are found in the district of the Weald. While the Purbeck formation exhibits the gradual passage of the marine Portlandian into the freshwater Wealden, the Punfield formation shows the transition of the latter into the marine Upper Neocomian (Lower Greensand). Thus we are led to conclude that the epoch of the English Wealden commenced before the close of the Jurassic period, lasted through the whole of the Tithonian and of the Lower and Middle Neocomian, and only came to a close at the commencement of the Upper Neocomian. In tracing the Cretaceous strata proper from east to west, they are found to undergo great modification, while the Neocomian and Wealden, which they overlap through unconformity, besides being greatly changed in character, thin out very rapidly. On stratigraphical and palæontological evidence, the Punfield formation is clearly referable to the upper part of the Middle Neocomian. Its fauna has remarkably close analogies with that of the great coal-bearing formation of eastern Spain, which is of vast thickness and great economic value. The claim of the Punfield beds, equally with the similarly situated Purbeck series, to rank as a distinct formation, is founded on the distinctness of their mineralogical characters, their great thickness, the fact of their yielding a considerable and very well characterised fauna, and of their being the equivalent of a highly important foreign series. The president remarked that the limited amount of fresh-water formations in this country was an obstacle to their correlation, and stated that Constant Prevost had endeavoured to correlate the secondary fresh-water and marine formations. Mr. Godwin-Austen remarked upon the thinning out of the Lower Greensand, especially in France; upon the imperfection of our knowledge of the great Cretaceous formation, and upon the probability of the intercalation of fresh-water conditions in the Lower Greensand. The formation at Punfield seemed to present an intercalation of marine between purely fresh-water conditions. He indicated how a slight change of level might have intercalated marine conditions in the Wealden. The deposition of the White Chalk and Oolite occupied enormous periods (in both cases purely marine), during which the northern hemisphere was a great northern ocean; and as the distribution of land and water was due to the operation of great cosmical laws, the duration of terrestrial and of the intermediate freshwater conditions was probably of equal length. Mr. Judd, in reply, said that he did not propose the term Punfield formation as a definitive term, but only as a matter of convenience. He believed that strata could be positively identified by he organic remains contained in them, although the method may have been grossly abused. Physical investigations alone led to nothing but confusion, as might be seen by the stratigraphical attempts of the predecessors of William Smith. The name *Vicarya* for the shell which had been referred to was only provisionally adopted, on the authority of De Verneuil and other writers. 2. "Some remarks on the Denudation of the Oolites of the Bath district, with a theory on the Denudation of Oolites generally." By Mr. W. S. Mitchell. The author briefly referred to the theory according to which oolitic deposits were supposed to have been originally spread out in continuous sheets over the country which they occupy, and to owe their division into separate hills to the action of denudation after their original deposition and consolidation. He suggested, as an equally probable hypothesis, that whilst the marls and clays of oolitic areas were probably originally deposited in continuous beds, the limestones in many cases may never have extended beyond the areas now occupied by them. He described the beds of limestone in the oolitic hills as thinning out towards the valleys on all sides, maintained that the limestones owed their origin to coral reefs, and cited several descriptions of coral islands by Prof. Jukes, to show the agreement in their structure with that which he ascribed to the oolitic hills. He assumed that in the event of a coral-area becoming one of sedimentary deposition, the sedimentary deposit would preserve intact the contour of the coral islands, and inferred that this has been the case in the Bath district, so that the Great Oolite cappings of the hills of that area may represent the original contours of coral islands, exposed by the denudation of the Bradford clay. The amount of denudation undergone by the Great Oolite limestone he considered to be very small. The Inferior Oolite, on the contrary, he believed to have suffered denudation, and he considered that the course of the valleys formed by this agent was dependent on the form of the limestones capping the hills. Prof. Morris did not consider that the author's views as to the oolitic masses round Bath being originally isolated coral banks with clay beds, although suggestive, were quite satisfactory. He pointed out that the strata on each side the valleys were similar in structure, mineral character, and fossil contents, and were once continuous; and the present intervening deep valleys were rather due to the movements which the area had undergone in producing lines of weak resistance, subsequently assisted by the erosive action of percolating and running water, both in excavating and undermining the harder rocks, so as to cause them to bend towards the hill-sides, or fall in larger or smaller masses on their slopes. Mr. Mitchell, in reply, stated that he had seen both sides of what he regarded as coral reefs. He remarked that his hypothesis was arrived at by deduction, by inferring from observations on existing coral-reefs that those of the Oolites must have been covered up as islands. He remarked that if the oolitic beds had slipped, as described, upon the underlying clays, they could hardly range on opposite sides of the valleys. He noticed that the action of water in covering the blocks of Oolite with crystallised carbonate of lime would be protective, and remarked that the surface of the reefs was virtually a sea-bottom on which mollusca lived, so that their occurrence at corresponding levels in different hills was not to be wondered at.

February 17.—Mr. Joseph Prestwich, F.R.S., president, in the chair.—The Secretary read the reports of the Council, of the Library and Museum Committee, and of the auditors. The general position of the Society, as shown by the state of its finances and the continued increase in the number of Fellows, was said to be very satisfactory. In presenting the Wollaston Gold Medal to Prof. Ramsay, F.R.S., F.G.S., the President spoke as follows:—"Prof. Ramsay,—I have great pleasure in presenting you with the Wollaston Medal, which has this year been awarded to you by the Council of the Society, in recognition of your many researches in practical and in theoretical geology. Distinguished as your services have been in connection with the Geological Survey since you entered upon it as the Assistant Geologist of Sir Henry De la Beche in 1841, and more particularly since your appointment as Local Director in 1845, during which period you have superintended and carried out the admirably minute style of mapping now general on the survey, and done so much in training its members in the field, you have not less distinguished yourself by your investigations of the higher problems involved in the study of geology. Your first work was on the Isle of Arran; and although then only a beginner, you, instead of taking the rocks to be what they looked, worked out what they were, and gave a new and independent reading of them, which has since in great part proved to be the right one. In 1846 your well-known memoir, 'On the Denudation of South Wales and the adjacent Counties of England,' showed the enormous amount of denudation that the Palæozoic rocks had undergone before the deposition of the New Red Sandstone. At subsequent periods you dwelt on the power that produced 'Plains of Marine Denudation,' a term introduced, I believe, by yourself, and showed in all cases, by a series of true and beautiful sections, how this has operated in planing across the older strata, and how valleys had been scooped out by subsequent aqueous causes in the great plains so formed. Whilst unravelling the complicated interior phenomena of the Welsh rocks, you were not unmindful of the very different order of phenomena exhibited on their exterior surfaces. Here you showed the vast extent and power of ice-action, and what a glacier land Wales once was. Reasoning from the present to the past, you also boldly pushed your ice-batteries far back into geological time, and were the first to bring them to bear on rocks of Permian age. That advanced post you long had to hold alone; but other geologists have since followed your lead, and we have even lately had evidence in the same direction from Southern Africa, where it is asserted that boulders and glaciated surfaces have been found at the base of the Karoo formation of supposed Jurassic age. You have also held a prominent place among those who, by their public teaching, have done so much during the last twenty years to advance the cause of our science. To myself, personally, whose geological career has run nearly parallel in time with your own, it is a source of much pleasure that it has fallen to my lot to hand you this, the highest testimonial the Society has to bestow."—Prof. Ramsay made the following reply:—"Mr. President,—I cannot say whether I am more pleased or surprised by the unexpected award to me of the Wollaston Medal by the Council of this Society. Pleased I well may be, not because I ever worked for this or any other honour, but because I feel a sense of satisfaction that the work on which I have been engaged for the last thirty years has been esteemed by my friends and fellows of the Council of the Society so highly, that they have deemed me a fit recipient of this honour. It is also a special satisfaction to me that this award has been bestowed by the hand of one of my oldest geological friends, who is so universally esteemed and beloved, and is himself so distinguished a contributor to physical and other branches of our science. My first endeavour in geology (the construction of a geological map and model of Arran) necessarily drew my attention to the physical part of our science; and when, consequent upon that work, I was, through the intervention of my old and constant friend, Sir Roderick Murchison, appointed by Sir Henry De la Beche to the Geological Survey of Great Britain, my whole subsequent life was thereafter necessarily involved in questions of physical geology, for no man can work on or conduct the field-work of such a survey who does not, aided by palæontology, necessarily make that his first aim. If some of my theories, induced by that work, were long in being recognised, the recognition has been all the more welcome when it came. Probably I never should have been able to do what I have done but for the wise example of my old master, Sir Henry himself, in his time the best thinker in England on the physical branch of our science, and to whose remarkable work, 'Researches in Theoretical Geology,' all geologists are to this day indebted. The papers which I have written are mere offshoots from my heavier work on the Geological Survey. Perhaps they are enough for the readers; but I wish they had been more numerous, for I certainly have had many more in my mind. Two of these, on old physical geographies of the world, I have lately given to the Society; and if they should be printed, I shall be well pleased should they soon or late be found worthy. The present physical geography of the world is but the sequel of older physical geographies; and to make out the history of these is one of the ultimate aims of geology. These are the subjects I have striven to master in part. I consider your award as a sign that I have had some success; and if, before I cease to work, I have a little more, I may be well content."—The President then presented the Balance of the Proceeds of the Wollaston Donation Fund to Mr. Robert Etheridge, F.G.S., in aid of the publication of his great stratigraphical "Catalogue of British Fossils," and addressed him as follows:—"Mr. Etheridge,—The Council of the Society has awarded to you the Proceeds of the Wollaston Fund, to aid in prosecuting your valuable work on the fossils of the British Islands, stratigraphically arranged. In this work, on which you have been engaged during the last eight years, and which occupies nine volumes of MS., representing as many geological groups, you give the natural history lists of each group, and trace the history of each species both in time and space. Of the magnitude of the work few can have any idea, nor would many have an idea of the marvellous extent of past life in our small portion of the globe without a comparison of our recent fauna with those (necessarily incomplete, because only partly accessible) which you have enumerated in your most useful lists. This comparison shows:—

	Polyzoa. Zoophytes. Echinoderms.	Crustacea.	Mollusca.	Fishes.	Reptilia.	Birds.	Mammalia.	Plants.	Total.
Number of Species in the existing fauna and flora of Gt. Britain . . .	616	278	567	263	15	354	76	1820	3,989
Number of Species found fossil in Gt. Britain . . .	2574	746	7091	815	224	12	172	819	12,453

I trust that this work will not be allowed to remain in MS., and that, presuming you will begin with the oldest, we may soon look for an instalment in the fauna of the Palæozoic rocks. I have much pleasure in presenting you with this token of the importance which the Geological Society attaches to your labours."—Mr. Etheridge made the following reply:—"I have great satisfaction in receiving from you, Sir, and the Council of the Geological Society, the award of the Wollaston Fund. It is given for work known to be nearly done, and faith in its completion. The time and labour devoted to my book upon the 'Stratigraphical Arrangement of the British Fossils' has extended over nearly nine years of incessant work, and has been an arduous, yet pleasant undertaking, now made lighter by the recognition of those who know and value the researches made for so extensive a catalogue of the British organic remains, now numbering nearly 13,000 species. It is this estimation of my labour by the Council and Society that tends to increase the desire to make my work as perfect as possible, well knowing how difficult, if not impossible, it is to do so. This acknowledgment, Sir, from your hands will stimulate me to finish my researches into the literature of the British species, and their history through space and time throughout Europe."—The President then proceeded to read his anniversary address, in which he discussed in considerable details the bearing of the recent deep-sea dredging operations upon geological reasoning. The address was prefaced by biographical notices of deceased fellows, including Sir Proby Cautley, Sir Frederick Pollock, Mr. Robert Hutton, and Prof. Gustav Bischoff.—The ballot for the Council and Officers was taken, and the following were duly elected for the ensuing year:—President: Joseph Prestwich, F.R.S.. Vice-presidents: Sir P. de M. G. Egerton, M.P., F.R.S., Prof. T. H. Huxley, F.R.S., Sir Charles Lyell, Bart., F.R.S., Prof. John Morris. Secretaries: John Evans, F.R.S., David Forbes, F.R.S. Foreign Secretary: Prof. D. T. Ansted, F.R.S. Treasurer: J. Gwyn Jeffreys, F.R.S. Council: Prof. D. T. Ansted, F.R.S., Dr. W. B. Carpenter, F.R.S., William Carruthers, W. Boyd Dawkins, F.R.S., Prof. P. Martin Duncan, F.R.S., Sir P. de M. G. Egerton, Bart., F.R.S., John Evans, F.R.S., David Forbes, F.R.S., J. Wickham Flower, Capt. Douglas Galton, C.B., F.R.S., R. A. C. Godwin-Austen, F.R.S., J. Whitaker Hulke, F.R.S., Prof. T. H. Huxley, F.R.S., J. Gwyn Jeffreys, F.R.S., Sir Charles Lyell, Bart., F.R.S., C. J. A. Meyer, Prof. John Morris, Joseph Prestwich, F.R.S., Prof. A. C. Ramsay, F.R.S., R. H. Scott, F.R.S., Prof. J. Tennant, Rev. Thomas Wiltshire, Henry Woodward.

London Mathematical Society, Thursday, Feb. 9.—Mr. W. Spottiswoode, President, in the chair. Mr. C. R. Hodgson, B.A., was proposed for election, and the Rev. J. Wolstenholme and Mr. R. B. Hayward, of Harrow, were elected members. Prof. Cayley made a communication "On an Analytical Theorem from a New Point of View," and also a second communication "On a Problem in the Calculus of Variations." The problem is, $z = \frac{1}{3}(3x - y^2)y$, to find y a function of x such that $\int z\,dx =$ maximum or minimum, subject to a given condition $\int y dx = C$ (the limits of each integral being $x_1\ x_0$ where these quantities are each positive, and $x_1 > x_0$). The ordinary method of solution gives

$$y^2 = x + \lambda, \text{ for}$$

$$\text{where } (x_1 + \lambda)^{\frac{3}{2}} - (x_0 + \lambda)^{\frac{3}{2}} = \tfrac{3}{2} C.$$

So long as c is not less than $(x_1 - x_0)\frac{3}{2}$, there is a real value of λ, but for a smaller value of c there is no real value. The difficulty arising in this last case is somewhat illustrated by replacing the original problem by a like problem of ordinary maxima and minima; viz., $x_1, x_2, \ldots x^n$ being given positive values of x, in the order of increasing magnitude—and if in general

$$z_i = \tfrac{1}{3}(3x_i - y_i^2)\, y_i,$$

then the problem is to find y_i a function of x_i, such that Σz_i = max. or min., subject to the condition $\Sigma y_i = c$. We have here $y_i^2 = x_i + \lambda$, where λ is then to be determined by the condition $\Sigma y_i = c$; the remainder of the investigation turns on the question of the sign

$$y_i = +\sqrt{x_i + \lambda} \text{ or } y_i = -\sqrt{x_i + \lambda}$$

to be taken for the several values of i respectively.—Prof. Henrici exhibited a plaster model of a tubular surface of the 6th order, which may be generated in either of the two following modes. Either a sphere of constant radius moves with its centre on a parabola, or it rolls along the same parabola always touching both its branches. The two envelopes thus produced differ in position only. The second mode of generation shows that the surface has a nodal curve, which is a parabola congruent to that on which the centre of the sphere moves; but in a plane perpendicular to it. Through a part of it only do real sheets of the surface pass. There is also a cuspidal curve of the 6th order, which has two cusps. The nodal curve passes through them, and has at these cusps the same tangents. The equation to the surface is

$$(27py^2 + 9xK - x^3)^2 = (x^2 + 3K)^3$$

where

$$K = (x + 2p)^2 + y^2 + z^2 - r^2$$

r = radius of the sphere and $4p$ is the parameter of the parabola. The equations to the parabola, on which the centre of the sphere moves, are

$$y^2 = 4p\,(x + 2p),\ z = 0;$$

those of the nodal curve,

$$y = 0,\ z^2 = -4px + r^2 - 4p^2;$$

the equations to the cuspidal curve are

$$27py^2 - 4x^3 = 0,\ x^2 + 3k = 0;$$

the first is a cylinder, which cuts the plane $z = 0$ in the evolute of the parabola, the second represents an ellipsoid of revolution. The model was constructed to the scale

$$p = \tfrac{1}{20},\ r = 2 \text{ inches.}$$

It was agreed, on the suggestion of Dr. Hirst, that Prof. Henrici should order a second model to be cast for the use of the society. Mr. Merrifield, F.R.S. laid the following statement before the society. "If the equation of a surface be

$$Z = F(x\,y) \qquad (1)$$

it is very well known that the condition that it should be a ruled surface is that

$$\left(\lambda \frac{d}{dx} + \mu \frac{d}{dy}\right)^2 z \qquad (2)$$

and

$$\left(\lambda \frac{d}{dx} + \mu \frac{d}{dy}\right)^3 \qquad (3)$$

should have a common factor of the form $A\lambda + B\mu$; and also that the condition of its being developable is that (2) should have two equal factors of that form. I have found upon actual trial that for a conical surface (3) will have two equal factors, and for a cylindrical surface, three equal factors; that is to say, if we write, $\alpha = \frac{d^3z}{dx^3}$, $\beta = \frac{d^3z}{dx^2dy}$ &c., we have for a conical surface

$$(\alpha\delta - \beta\gamma)^2 = (\alpha\gamma - \beta^2)(\beta\delta - \gamma^2)$$

and for a cylindrical surface we have separately

$$(\alpha\delta - \beta\gamma) = 0,\ (\alpha\gamma - \beta^2) = 0,\ \beta\delta - \gamma^2 = 0$$

If, following Monge, we regard the surface as traced out by a right line moving on three director curves, the condition of two or three equal roots is evidently the same as that, out of the three characteristics passing through a point, two or three should become coincident. I have not yet had time to look into the question whether the converse of the proposition is true, viz., whether the introduction of the condition of developability ($rt = s^2$) necessarily reduces the surface, in which two or three of the characteristics coincide, to a cone or cylinder." The president and members present expressed their wish that Mr. Merrifield would be able to find time for the consideration of this converse proposition. Dr. Hirst then made some remarks on the connection between the correlation of two planes, as described in his last communication to the Society, and Sturm's solution of the problem of projectivity, as given by him in his memoir on the subject, published in the *Mathematische Annalen*, Vol. i., p. 533.

Linnean Society, February 16.—Mr. G. Busk, Vice-president, in the chair. Dr. J. D. Hooker presented to the Society on behalf of a committee appointed for the purpose, a half-length portrait of the President, Mr. G. Bentham, the expense of which had been defrayed by a subscription raised among the fellows of the Society. The following papers were then read, the interest of which was purely technical:—On Tremellineous Fungi and their Analogues, by L. and C. Tulasne; Bryological Remarks by Dr. S. O. Lindberg.

Entomological Society, February 20.—Mr. A. R. Wallace, president, in the chair. Mr. Bond exhibited a hybrid between *Bombyx Pernyi* and *B. yama-mai*, two of the larger silk-worm moths; this individual was of the colour of the one parent with the form of the other. He also exhibited an example of *Bombyx mori*, bred by Dr. Wallace, still retaining the larval head. Mr. McLachlan called attention to the first-recorded instance of a similar arrest of development, being a paper by O. F. Müller in "Der Naturforscher" for 1871. Mr. Smith mentioned that a common Egyptian wasp, *Rhynchium brunneum*, obliterated, by its nest, the inscriptions on the ancient monuments in that country; and he exhibited an example of the same wasp which had been found in the folds of the covering of a mummy, showing that the same species had inhabited Egypt for many ages. Mr. Smith further alluded to a passage in Pepys's Diary, dated May 1665, in which the writer narrated how he had seen a glass-hive where the bees could be seen at work, proving that observatory hives were not a modern invention. Mr. Müller read a paper "On the Dispersion of Non-migratory insects by Atmospheric Agencies," in which he had collected together a number of records of showers of insects after violent storms, and at sea at long distances from land; and he was of opinion that these agencies played a considerable part in the geographical distribution of insect life, though, no doubt, in many cases, the species thus involuntarily dispersed died out from inability to cope with the pre-existent denizens of the localities to which they were driven. Mr. H. Jenner-Fust communicated a supplement to his treatise on the geographical distribution in these islands of the indigenous Lepidoptera.

DUBLIN

Royal Irish Academy, Feb. 13.—Rev. J. H. Jellett, B.D., president, in the chair.—Dr. Ferguson read a paper "On the Difficulties attendant on the Transcription of Ogham Legends, and the Means of Avoiding them." Leave was given to Mr. Charles E. Burton to read notes "On the Results obtained by the Agosta Sicily Expedition to Observe the recent Solar Eclipse."—A paper was read by Profs. W. King and T. H. Rowney, "On the Geological and Microscopical Structure of the Serpentine Marble or Ophite of Skye."—Papers "On Eozöon Canadense," by Principal Dawson, and on Messrs. King and Rowney's paper "On Eozöon Canadense," by Dr. T. S. Hunt, were deferred to the meeting of the 27th inst. when the discussion of all the papers on this subject will be taken.—Rev. President Henry, D.D., Belfast, H. Dix Hutton, LL.B., and T. W. Ellison Macartney were elected members of the Academy, and Prof. Traquair was admitted a member.—Sir W. Wilde presented on behalf of the Earl of Mayo, a collection of ancient Indian Coins, for which the marked thanks of the meeting were voted.

HOBART TOWN

Royal Society of Tasmania, October 11, 1870.—His Excellency, C. Du Cane, Esq., President, in the chair. The Secretary read some "Notes on an experiment with the fumes of sulphur, and of other methods for the destruction of rabbits in their burrows," by W. Archer, Esq., F.L.S. The fumes were forced into a burrow by means of bellows, attached to a receptacle in which the sulphur was burned; and that this was effectually done was proved by the escape of sulphurous vapour from the bolt-holes. When the burrow was afterwards opened, however, no trace of the fumes was left, nor were the animals destroyed. The experiment was recorded as a "guide or warning to others who may be induced to try further experiments with the fumes of sulphur, or with any other vapour." (Carbonic acid gas would not become condensed, and it would be fatal to animal life, but its use would probably be much too expensive). Mr. Abbott read a paper "On the Sun and its Office in the Universe." Some discussion of a conversational character having taken place, Mr. M. Allport begged to call the attention of the meeting to the fish presented by Mr. Wise (presentation No. 8), on account of its high scientific importance, as furnishing a complete answer to the theory raised by Dr. Günther in reference to the salmon first sent to England. The Doctor then assumed that the fish sent was hatched from one of the eggs imported to England in 1866. This assumption was met by the statement that the fry unnaturally detained in fresh water had reached a higher state of development than the smolt sent to England, and as the fish now presented was but just assuming the smolt stage, all the arguments used in reference to the smolts first caught apply with tenfold force to this specimen. It was, moreover, fortunate that they had in the Museum one of the fry hatched from the English eggs received per *Lincolnshire* in 1866, and which died in the spring of 1867. Upon comparing this with the fish now caught, it would be found that they accorded with one another so closely, as to leave little doubt of their identity in species. No report had yet been received from England as to the smolt last sent, though they had heard of its safe arrival. Mr. Allport further observed that Mr. Youl, in writing to Sir Robert Officer, had expressed a wish that the Salmon Commissioners should make it publicly known that after careful examination he entirely concurred with Dr. Günther in the opinion that the specimen first sent to England was a Salmon trout (*Salmo trutta.*)

BERLIN

Royal Prussian Academy of Sciences, June 2, 1870.—Prof. G. Rose communicated a long and elaborate memoir on the connection between the hemihedric crystalline form and thermo-electrical properties in iron-pyrites and cobalt-glance, with some remarks on the theory of hemihedric forms in general; and Prof. Dove read a paper on the reference of the annual curve of temperature to the conditions upon which it depends.

June 16.—M. Kummer read a paper on the simplest representation of the complex numbers formed from unitary roots, which can be effected by multiplication with unities. Prof. W. Peters read a description of *Propithecus Deckenii*, a new species of Lemuroidea from Madagascar; it is the species which had previously been identified by him with *P. diadema* Bennett.

June 23.—A paper was read on the Morphology of *Chondriopsis cærulescens*, Crouan, and the optical phenomena presented by that Alga, by Dr. Leopold Kny. The author described in some detail the peculiar cell development and mode of ramification of the plant, and noticed more briefly the structure of the reproductive organs. The peculiar colour presented by the plant is produced by the contents of the outermost cortical layer of cells, and is due to the presence in them of certain corpuscles which have the faculty of reflecting blue light. Prof. du Bois Reymond read a supplement to his memoir on the aperiodic movement of muffled magnets.

June 27.—Prof. C. Rammelsberg read some contributions to the knowledge of meteorites. He first communicated some remarks on the analysis of meteorites, relating to a more recent process for the separation of nickel from iron, to the separation and determination of meteoric iron in stony meteorites, and to the analysis of the silicates, and then furnished analyses of meteoric irons, of the pallasite of Brahin, and of the chondrites of Pultusk, Richmond, and Iowa. His analyses of these chondrites and of that of Klein Wenden, lead him to the conclusion that they all contain only two silicates, olivine and broncite, a result which he finds to be confirmed by other analyses, and he affirms, that mesosiderite and chondrite do not differ petrographically but only in structure.

BOOKS RECEIVED

ENGLISH.—The Descent of Man, 2 vols.: C. Darwin (Murray).—The General Structure of the Animal Kingdom, 4th edition (Van Voorst).—A Treatise on Smoky Chimneys: F. Edwards (Longmans)—Mathematical Papers of the late George Green: N. M. Ferrers (Macmillan).—A Synopsis of the Family Unionidæ: Isaac Lee (H. C. Lea, Philadelphia).—Thesaurus Syriacus, fasc. ii.

DIARY

THURSDAY, MARCH 2.

ROYAL SOCIETY, at 8.30.—Further Experiments on the Effect of Diet and Exercise on the Elimination of Nitrogen: Dr. Parkes, F.R.S.—Magnetic Observations made during a Voyage from St. Petersburg to the Coasts of the Arctic Sea, in the Summer of 1870: Capt. Belavenetz, I.R.N.
SOCIETY OF ANTIQUARIES, at 8.30.—On Roman Antiquities at Lydney Park: Rev. W. H. Bathurst.
CHEMICAL SOCIETY, at 8.
LINNEAN SOCIETY, at 8.—On the Tamil names of Plants: Rev. S. Mateer. —Contributions towards a knowledge of the *Curculionidæ*: H. P. Pascoe.
ROYAL INSTITUTION, at 3.—Davy's Discoveries: Dr. Odling.
LONDON INSTITUTION, 7.30.—On the Colonial Question: Prof. J. E. Thorold Rogers.

FRIDAY, MARCH 3.

ROYAL INSTITUTION, at 9.—Pressure of Fired Gunpowder: Capt Noble.
GEOLOGISTS' ASSOCIATION, at 8.—On the Range in Time of the Foraminifera: Prof T. Rupert Jones, F.G.S.—On the English Crags, considered in reference to the Stratigraphical Divisions indicated by their Invertebrate Fauna: Alfred Bell.
ROYAL COLLEGE OF SURGEONS, at 4.—On the Teeth of Mammalia: Prof. Flower.

SATURDAY, MARCH 4.

ROYAL INSTITUTION, at 3.—Socrates: Prof. Jowett.

SUNDAY, MARCH 5.

SUNDAY LECTURE SOCIETY, at 3.30.—Iceland: its Physical Features, Volcanoes, Hot Springs, &c.: Jon A. Hjaltalin.

MONDAY, MARCH 6.

ROYAL INSTITUTION, at 2.—General Monthly Meeting.
ENTOMOLOGICAL SOCIETY, at 8.
ROYAL COLLEGE OF SURGEONS, at 4.—On the Teeth of Mammalia: Prof. Flower.
LONDON INSTITUTION, at 4.—On Astronomy: R. A. Proctor, F.R.A.S.
ANTHROPOLOGICAL INSTITUTE, at 8.—On the Racial Aspects of the Franco-Prussian War: J. W. Jackson.

TUESDAY, MARCH 7.

ROYAL INSTITUTION, at 3.—Nutrition of Animals: Dr. Foster.
ZOOLOGICAL SOCIETY, at 9.—Notes on rare or little-known Animals now or lately living in the Society's Gardens: P. L. Sclater.—List of the Lizards belonging to the family *Sepidæ*, with Notes on some of the species: Dr. A. Günther.—On new Insects collected by Dr. John Anderson during the Expedition to Yunan: F. Moore.—Observations on the Record of Accessions to the Gardens of the Zoological Society: Dr. J. E. Gray.

WEDNESDAY, MARCH 8.

SOCIETY OF ARTS, at 8.—The Cultivation and Uses of Sugar-beet in England: Dr. A. Voelcker.
GEOLOGICAL SOCIETY, at 8.
ROYAL COLLEGE OF SURGEONS, at 4.—On the Teeth of Mammalia: Prof. Flower.
ROYAL MICROSCOPICAL SOCIETY, at 8.
PRESS LITERARY FUND, at 3.—Anniversary Meeting.

THURSDAY, MARCH 9.

ROYAL SOCIETY, at 8.30.
SOCIETY OF ANTIQUARIES, at 8.30.
ROYAL INSTITUTION, at 3.—Davy's Discoveries: Dr. Odling.
LONDON MATHEMATICAL SOCIETY, at 8.—Remarks on the Mathematical Classification of Physical Quantities: Dr. Clark Maxwell, F.R.S.—On Skew Cubics: Prof. H. J. S. Smith, F.R.S.
LONDON INSTITUTION, at 7.30.—On the Colonial Question: Prof. J. E. Thorold Rogers, M.A.

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ERRATUM.—Page 322, second column, line 20, for "western or right hand" read "eastern or left hand."