THURSDAY, DECEMBER 14, 1893.

A BOOK OF PRACTICAL EXAMPLES IN ELECTRICITY.

Problèmes et Calculs Pratiques d'Électricité. Par M. Aimé Witz. (Paris: Gauthier-Villars et Fils, 1893.)

THIS is, in the main, a book of fully worked-out exercises in electricity and magnetism, designed for the help of practical students. Its idea and arrangement are good, and the examples seem to have been chosen with much care, and, as far as possible, from actual cases which have occurred in laboratory and practical work. The aim of the author has not been to furnish a set of examples, like the collection of Walton, in theoretical mechanics, or that of Hall Turner, in heat and electricity. These works illustrate general mathematical theories by examples, the solutions of which are in many cases important or interesting particular theorems; but their interest is, to a great extent, mathematical. M. Witz has had in view the wants of students endeavouring to obtain a sound elementary knowledge of electricity, who are not afraid of a piece of calculation involving, when necessary, a little differentiation or integration, when it comes in its proper place as the simplest and most direct means of attaining the required result.

The work is divided into three parts: (1) containing definitions and formulas, (2) numerical constants, and (3) the greater portion of the book—a collection of illustrative examples. Part 1 deals first with magnetism, and gives the ordinary relation between magnetic intensity and magnetic induction, introduces the notions of magneto-motive force and magnetic resistance, and shortly states the main facts of lamellar and solenoidal magnetisation. In like manner the next chapter *states*, merely, some of the principal theorems of electrostatics; the third deals with phenomena of steady currents; and the last two with electro-magnetism and induction of currents.

In connection with electro-magnetism a paragraph is devoted to the researches of Ewing and "M. Hopkins" on the magnetization of iron. All the latter experimenter (the distinguished inventor of characteristic curves of dynamos appears to be meant!) is credited with is a demonstration that the "travail" (consumed in putting a sample of iron through a magnetic cycle) "exprimé en ergs et rapporté à l'unité de volume, est égal au produit de la force coercitive de l'échantillon par l'induction maximum, divisé par π." The measure of coercive force assigned by Hopkinson does not seem to be explained in the book, and so a really important idea, rendering definite what was before a perfectly vague expression, is passed over. As to the demonstration referred to, we must confess to never having heard of it. Dr. Hopkinson, we had supposed, simply used the rule stated in the quotation as a rough and ready method of rapidly finding the approximate dissipation of energy in a closed magnetic cycle.

Ampère's law ("formule classique, connue de tous nos lecteurs") of the action between two current elements is explained, but no hint is given that any number of other laws can be obtained which give for the only cases with which we can without ambiguity deal, those of closed circuits, precisely the same result as is given by Ampère's formula, although the latter may have certain advantages in point of simplicity.

The word "law" is a good deal misused in electrical science; we have Kirchhoff's laws, Ohm's law, Joule's law, Lenz's law, and many others; but we have here a law that we do not remember to have come across before, namely "Pouillet's law," which asserts that the quantity of electricity conveyed by a current I in time t is It! No doubt if the electro-magnetic definition and measure of a current are adopted, it is a proper subject of investigation to settle whether it is simply proportional to the current defined electrostatically as the time rate of flow of electricity; but the real proof that this is the case, is the consistency with the results of experiment of the great mass of results deduced from this proportionality.

The chapter on induction is brief, but contains a great deal of information very accurately expressed. The function of the current which multiplied into the speed gives the electromotive force of a dynamo, is referred to as the "fonction caracteristique" of M. Marcel Depres; but Hopkinson's extremely important dynamo characteristic curves are merely referred to, without any mention of their author.

The so-called law of Jacobi, namely, that "Le travail utile d'un moteur est maximum lorsque sa force contre-électromotrice est égale à la moitié de la force électromotrice de la generatrice," is no doubt correctly stated, since by "le travail utile" is meant the electrical work done on the motor in a given time, otherwise than in heating its conductors. But it would be better to say that the electrical activity as specified in the motor is a maximum when the condition stated is fulfilled. The phrase "useful work," here used, has caused this result, simple as it is, to be completely misunderstood by many practical electricians of high standing. In the present case the tendency to error is obviated by the statement immediately following, that "Le rendement electrique d'une transformation d'energie, est égal au rapport de la force contre-électromotrice e de la réceptrice à la force électromotrice E de la géneratrice. Ce rendement peut devenir egal à l'unité lorsque e devient egal à E; mais alors le travail produit tombe à zéro. C'est la *loi de Siemens*."

The late Sir William Siemens objected in 1883 to the erroneous interpretation put upon Jacobi's result by Verdet and others, and likewise stated the true principle of efficiency; but the law of maximum efficiency of a circuit containing a motor was given in Lord Kelvin's very important paper on the "Mechanical Theory of Electrolysis," published in 1851 in the *Philosophical Magazine*. As not only this result, but others, forming practically the whole of the simple but immensely important elementary theory of the electrical efficiency of a generator and motor, are there incidentally given by Lord Kelvin, and are usually stated in practical treatises and lectures as theorems of much later date, we may be allowed to give here a short abridgement of the passage.

Denoting by ω the angular velocity with which a Faraday disk magneto-electric machine is driven, by Ω the velocity with which the machine would have to be driven to give a back electromotive force equal to that of the generator (a battery in this case), Lord Kelvin points out that if ω is less than Ω, the current is opposed to the electromotive force of the disk, and that therefore in this case "the chemical action is the source of the current instead of being an effect of it; and the disk by its rotation produces mechanical effect as an electromagnetic engine" (or, as we now call it, a motor) "instead of requiring work to be spent upon it to keep it moving as a magneto-electric machine." If γ' be the current flowing, F the intensity of the field in which the disk revolves, r the radius of the disk, R the resistance of the circuit, W′ the rate at which work is done by the current on the engine, M′ the rate at which energy is spent by the battery, then the results—

$$\gamma' = \frac{r^2 F}{2R}(\Omega - \omega),$$

$$W' = \tfrac{1}{2} r^2 F \omega \gamma',$$

or

$$W' = \frac{\omega}{\Omega} M'$$

are given, and it is pointed out that the fraction of the "entire duty of the consumption which is actually performed by the engine is equal to ω/Ω." The ratio ω/Ω is the ratio of the back electromotive force of the motor to the total electromotive force of the generator, and is therefore the law of efficiency stated above in the words of M. Witz.

The examples worked out in the book are many of them highly instructive, and, so far as we can judge from the examination of a selected few, seem clearly and correctly dealt with. They are not merely numerical, but include in most cases the deduction from general theorems of formulas for particular cases, which are then illustrated by numerical problems in which results are expressed in C G.S. units. The value of these problems is enhanced by the fact that they are, as we have said, for the most part actual problems which have turned up in experimental or practical work. The subjects thus elucidated include magnetism, electrostatics, steady flow of electricity, electro-magnetism, dynamos, motors, and the distribution and transmission of electric energy. There can be no doubt that the book will prove very useful to teachers and students. Its only fault is that it leaves nothing for the student himself to do. A moderate number of unworked examples, on which he might test his grip of the subject, and power of applying principles, would have been very valuable. It is undesirable to spend very much time in solving mere arithmetical or algebraic conundrums, but enough must be done to acquire a fair amount of readiness and expertness of calculation; and of the great benefit derived from working out numerical examples of physical principles, there can be no doubt. We think, therefore, the author would do well to supply material for this in a future edition. A. GRAY.

BESANT'S DYNAMICS.

A Treatise on Dynamics. By W. H. Besant. (Cambridge: Deighton, Bell, and Co., 1893.)

THIS popular text-book has now reached a second edition, and contains several additions which have increased its size from 334 to 448 pages. A new chapter has been added on disturbed elliptic motion, which shows how the elements of an elliptic orbit are affected by small disturbances in the same plane. This chapter will serve as a useful introduction to the planetary theory, since the limitation of the problem to two-dimensional motion enables various difficulties, which arise from taking into account the longitude of the node and the inclination of the orbit, to be got rid of. The principle, upon which the method of the variation of the elements is based, is one to which students should be introduced at an early stage; but unless some simplification is made, the analysis becomes rather complicated. We are inclined to suggest that this chapter might be extended in a future edition.

The last chapter of the first edition has been amplified into two, the first of which deals with motion in three dimensions, whilst the second discusses several important problems relating to the motion of tops, discs, gyroscopes, &c.; and the book concludes with a new chapter on Lagrange's equations, together with several applications illustrating their use. To discuss any of the higher developments of this branch of the subject, including the Hamiltonian transformation, and the mixed transformation which in 1887 was for the first time given in a *complete* form by the author of this review, would probably be thought beyond the scope of an elementary work; but it would be well to bring out more pointedly the fact that the kinetic energy of a dynamical system can be expressed in several different forms, and that when employing Lagrange's equations there is only one form which it is permissible to use, viz. the Lagrangian form, in which the kinetic energy is expressed as a homogeneous quadratic function of velocities which are the time-variations of coordinates. Mistakes are frequently made upon this point; and it is most necessary to impress upon the minds of students that Lagrange's equations are double-edged tools, which are apt to cut the fingers of those who unskilfully handle them.

Dr. Besant has used the word *phoronomy* in the place of *kinematics*, and he has stated his reasons for so doing in a letter published in NATURE, March 17, 1892. The word appears to be a good one, and has the merit of being classical, and not Teutonic; but notwithstanding occasional flights into the regions of radicalism, the ingrained conservatism of the English mind is so strong that it is by no means certain whether phoronomy will supplant a word which has long held the field.

One of the most satisfactory features of the work is that Dr. Besant has drawn marked attention to the principle of momentum. This principle is in some respects a more fundamental one than the principle of the conservation of mechanical energy; for the former principle is true in the case of viscous systems in which there is a conversion of mechanical energy into heat, whilst the latter does not hold good when internal friction or viscosity exists. The principle of linear momentum can be shown to be a direct consequence of Newton's second and third Laws of Motion; but doubts have been entertained whether the principle of angular momentum can be deduced from Newton's Laws without the aid of an additional hypothesis. The question, however, is far too recondite a one to be discussed in a review.

It is possible that some of those whom a recent correspondent in NATURE has described as "the slug and the bug school" may object to the large amount of problems and examples which are contained in the text and at the end of the chapters. Persons whose curiosity is limited to finding out *what electricity does*, and adopt the unscientific attitude of considering it waste of time to try and ascertain *what electricity is*, and *why* it is capable of performing so many wondrous feats, may perhaps rebel against a system, one of whose objects is to train the mind to inquire into the *causes* of natural phenomena. It must be recollected that young men, who are just emerging from the schoolboy stage of existence, invariably find that Rigid Dynamics is a very difficult subject to master, and that a thorough knowledge of the principles of the subject, combined with analytical skill in applying them to natural phenomena, can only be acquired by working out a large number of problems and examples. It is also an excellent plan for students to work out the same problem (for example, the motion of a top) by various methods, and to study the different results obtained by each; for they will thereby not only obtain analytical skill, but will learn that their symbols, instead of representing mere mathematical quantities, are the embodiment of important scientific facts.

A. B. BASSET.

INSECT PESTS.

Our Household Insects: an Account of the Insect Pests found in Dwelling-houses. By Edward A. Butler, B.A., B.Sc.Lond., author of "Pond Life," "Silkworms," &c. (London and New York: Longmans, Green and Co., 1893.)

MR. E. A. BUTLER has done useful service to the cause of popular entomology by reprinting the present series of his contributions to *Knowledge* in book form. Not very many species are discussed, but these seem to comprise most of the principal insect pests belonging to the various orders of insects which infest our houses, and attack ourselves or our property.

As insects (exclusive of insect-parasites) attack all kinds of dead or decaying animal and vegetable matters, and play the part of general scavengers, nothing is exempt from their ravages. Books are particularly subject to their attacks; and many of their enemies are noticed by Mr. Butler. We remember once being much amused by an account of a book-worm, which was ridiculed by a writer in a bibliographical magazine, as being evolved from the describer's own consciousness, but which was really fairly recognisable as applicable to *Lepisma saccharina*, the common silver-fish. But the critic regarded the book-worm as necessarily a small grub or beetle (we forget which), and displayed his own ignorance of entomology accordingly. Prof. Westwood once named a minute beetle, which had done much mischief to the cover of a book, *Hypothenemus eruditus*; and specimens of books damaged by insects may be seen in one of the cases in the public insect-room at the Natural History Museum, South Kensington. We believe that Mr. Zaehnsdorf, the well-known book-binder, has also formed a collection of the book-pests which he has met with in the exercise of his vocation. We may add that the Arabs sometimes write the name Kabikaj, said to be that of a genius who presides over insects, on their manuscripts, in order to protect them from the ravages of his subjects.

There is no doubt that insects of various kinds get mixed with human food from time to time; but we imagine that the passage which Mr. Butler quotes from Curtis's "Farm Insects," relating to maggots in cocoa-beans, is somewhat out of place at the end of a chapter on beetles, for we have good reason to believe that the insect to which Curtis alluded was not a beetle, but a moth of the genus *Ephestia*.

The seven plates of magnified insects and their structure, and the numerous woodcuts, add much to the usefulness of the book. Clear definitions, and accurate demonstrations, are extremely useful in entomology, not merely to beginners, but even to more advanced students, who often find much difficulty in obtaining the necessary explanations of the characters and terminology, when they take up the study of a group of insects with which they were not previously familiar.

The insects which Mr. Butler discusses may be divided into three classes: those which really cause serious destruction to property, as the clothes-moths and various kinds of small beetles; those which are rather troublesome and annoying than destructive, such as the flies and wasps; and those which attack man himself. Among the latter are the lice, which the increase of cleanliness has fortunately rendered rather unfamiliar objects to the better classes in recent times. Yet they are highly interesting creatures, from many points of view, and several entomologists have not scrupled to make a special study of them; among others, Denny, who wrote an elaborate monograph on the British species, illustrated with twenty-six coloured plates; and the old Dutch naturalist, Van Leeuwenhoek, who actually reared a brood in a stocking on his own leg! We think the figure of the proboscis of a louse, which Mr. Butler copies from the Danish entomologist Schiödte, on p. 332, will be new to most of his readers. But there is a curious omission of a necessary explanation in Mr. Butler's observations, in the following passage:—

"Man is not exceptional among mammals in harbouring these vermin; he is but in the same category with the rest, for it seems to be the rule, from elephant to mouse, largest to least, that some member of this group of parasites should be attached to each species; and even aquatic animals, such as the seal and walrus, do not evade their attacks."

Surely Mr. Butler, to avoid being misunderstood, should here have stated that the presence of a true louse on seals is quite an exception as regards marine animals, and that the so-called "whale-louse," and similar parasites, are not true lice, or even insects at all, but parasitic crustacea.

Our author mentions the fact of colonies of fleas having sometimes been found on sandy sea-shores, and wonders what they can find to eat there. But although certain species of fleas are attached to different species of animals, they are perhaps not so particular about their food as is generally supposed. In all warm countries it is very common to find colonies of fleas camping-out in the open; and the late Mr. F. Smith once recorded an instance of a suburban garden, in which a particular bed was swarming with dog-fleas; no particular dog being mentioned as the probable origin of the invasion. In the Western States of America, the "wild fleas," as the late

Frank Buckland would have said, actually feed on the larvæ of a white butterfly which abounds in the pine-forests.

Some curious stories are related by Mr. Butler respecting Longicorn beetles, and *Sirex gigus* perforating sheets of lead. Many years ago, a tin canister was exhibited before the Entomological Society, through which a stag-beetle had gnawed its way, and the marks of its jaws were distinctly visible on the tin.

In his remarks on the bed-bug, which is almost invariably, if not always, apterous, Mr. Butler makes some general observations (p. 287) on the use of wings to insects. It may be mentioned that the late Mr. Wollaston has observed that most insects inhabiting the Atlantic Islands, are either strongly winged, or practically incapable of flight. The explanation which he gives is very curious and interesting. Insects living on small islands exposed to gales are very liable to be blown out to sea. Hence it is almost equally beneficial to them either to be gifted with such strong powers of flight that they can make their way back, in case of such an emergency, or else that they should never fly at all, and thus never run the risk of being blown away.

There are many interesting subjects touched upon in Mr. Butler's work, and much that would admit of further comment; but we have perhaps said enough to indicate its general scope and character. Should it reach a second edition, we think it might be made a little more comprehensive with advantage; for the subject is a very large one, and those who feel a real interest in it rarely find a book too long or too much detailed.

OUR BOOK SHELF.

Text-book of Biology. By H. G. Wells, B.Sc.Lond., F.Z.S., Lecturer in Biology at University Tutorial College. With an Introduction by G. B. Howes, F.L.S., F.Z.S., Assistant Professor of Zoology, Royal College of Science, London. Part II. Invertebrates and Plants. (London: W. B. Clive, University Correspondence College Press, 1893.)

IN dealing with a small number of Vertebrate types in Part I. of this book (see NATURE, vol. xlvii. 1893, p. 605), the author showed distinct capability and promise; but we feel that he would have done well to wait and work for a few years before publishing this second volume, which covers a larger field. As the types of plants and invertebrates treated of have already been described in so many text-books, the writer had, at any rate, the opportunity of getting his facts and deductions second hand and fairly correctly stated, even without an extensive acquaintance with biological science. There is, therefore, all the less excuse for the many errors and misstatements which occur in this volume, the preface to which would lead one to expect better things in this respect, as well as in the selection and arrangement of facts. Prof. Howes's introduction appeared in Part I.; and before inserting his name on the title-page of Part II. it would, we think, have been only just to have at least submitted the proofs to him. The book would certainly have gained by so doing.

Apart from the more serious faults, which are so numerous that it is not easy to give a short selection of them, awkward terms and misprints abound. Prof. Goebel would probably be surprised to hear that he had written a text-book on botanical "mythology" (p. 94)!

The illustrations are exceedingly crude, and are mostly rough copies of well-known figures. It is, however, only fair to state that the author has purposely made them "as simple and diagrammatic as possible." W.N.P.

The New Technical Educator. Vol. ii. (London, Paris, and Melbourne: Cassell and Co., Ltd., 1893.)

IN a notice of vol. i. of this useful work we pointed out that it filled a want in our general technical literature, and that its contents were of a high order. The present volume is quite equal to its predecessor in this respect, and forms a continuation of all the subjects treated in the previous volume.

Under the heading of the "Steam Engine" we find an admirable series of chapters, by Mr. Archibald Sharp, on the subject of valve gear generally, particularly the diagrammatical treatment of the subject illustrative of the various movements of eccentrics, piston and valve. "Electrical Engineering" is also in good hands, being clearly treated by Mr. Edward A. O'Keefe. The many explanations and descriptions given are of a high order of merit. On the subject of "Cutting Tools" much useful information is to be found from the pen of Prof. R. H. Smith, who is an authority on this particular subject. The other subjects embraced in the volume, including practical mechanics, plumbing, photography, steel and iron, drawing for engineers and carpentry, are all well written and illustrated, forming a very useful collection of articles on technical subjects. It is to be regretted, however, that the various articles on different subjects continue in this volume to be mixed together, thus causing the reading of one subject to be a matter of frequent reference to the page of contents.

Heat, and the Principles of Thermodynamics. By Dr. C. H. Draper. (London: Blackie and Son, 1893.)

IN these days of innumerable books, it is often a difficult task to correctly appraise the value of a new work, and this is especially the case with books intended for use in classes. The only thing a reviewer can do is to judge whether the volume under his notice differs much from previous volumes on the same subject; and if the author shows no originality of treatment, it seems to us that his book could very well have been left unwritten. Viewing Dr. Draper's work in this light, we find as follows: (1) Much more attention is paid to the principles of thermodynamics than is usual in class-books of its kind; (2) the examples and exercises distributed throughout the book, and at the end, are more numerous than in most text-books of heat, and cover a wider range of examinations; (3) the mathematical section of the subject has not been shirked. Little more can be said. The book is as good as any of its class, and to the student who desires to read up for an examination in heat it should be very helpful.

LETTERS TO THE EDITOR.

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

Systematic Nomenclature.

WITH reference to Prof. Newcomb's suggested nomenclature for radiant energy, which appeared in NATURE, November 30, p. 100, it seems advisable to be rather cautious in adopting new words, or rather terminations to words already more or less in use, for at the present time the student beginning the study of physical science is fairly bewildered with the various forms of words used under the present system, or rather want of system, in nomenclature.

If once for all some system of termination was settled upon (as in chemistry, for the increasing oxidation results, &c.), the coinage of words as fresh needs arose would proceed automatically on rational lines.

This might very well form the object of a special committee of the British Association.

Mr. Oliver Heaviside's system for electromagnetic matters has much to recommend itself for adoption, also, in general physics.

For example, after the plan (1) conduc*tion*, (2) conduc*tance*, (3) conduc*tivity*, we would have, in the case of radiant energy, (1) radia*tion*, (2) radia*tance*, (3) radia*tivity*.

The first is for reference in a general way to the phenomenon in question; the second refers to its amount in appropriate units in any individual case; while the third is suitable for expressing the peculiar action or factor in the phenomenon possessed by different *kinds* of bodies. Thus the radia*tance* from a hot kettle would be the total quantity of energy lost per second. The radia*tivity* would be the quantity of this per square centimetre.

With a view of examining the feasibility of this system, the following list is subjoined. Many of the words appear at first as if they would prove most awkward in practice, but remembering similar fears (which subsequently proved groundless) in electromagnetic matters, one is afraid to say they are due to more than unfamiliarity.

Phenomenon	Amount of	Coefficient of
Absorption	Absorbance	Absorbivity
Attrition	Attritance	Attritivity
Diffusion	Diffusance	Diffusivity
Emission	Emissance	Emissivity
Expansion	Expansance	Expansivity
Extension	Extensance	Extensivity
Friction	Frictance	Frictivity
Gravitation	Gravitance	Gravitivity
Heat	Heatance	Heativity
Inertia	Inertance	Inertivity
Polarization	Polarizance	Polarizivity
Reflection	Reflectance	Reflectivity
Refraction	Refractance	Refractivity
Rotation	Rotatance	Rotativity
Solution	Solutance	Solutivity

Special attention deserves to be called to inertance as a good name for mass, and inertivity for density, to rotatance for moment of momentum, and rotativity for moment of inertia.

GEO. FRAS. FITZGERALD.
FRED. T. TROUTON.

Physical Laboratory, Trinity College, Dublin,
December 5.

On the Nomenclature for Radiant Energy.

IN connection with this subject there are many things to be considered, and one of the most important is the question of radiation and absorption, which requires a completely new nomenclature to get over very serious ambiguities that at present embarrass the subject. It is very necessary to distinguish between what may be called, on Prevost's theory of exchanges, the total radiatance from the actually observed loss of energy by radiation which is, according to this theory, the difference between the total radiatance and the total absorbance. This difference per degree of temperature is very commonly called the radiating power, but this same word is used in quite a different sense when it is attempted to prove, from Prevost's theory of exchanges, that the radiating is equal to the absorbing powers by a consideration of thermal equilibrium. In this latter case the term radiating power means obviously the total radiatance of Prevost's theory.

It may also be worth while calling attention to the theory, given at Nottingham by Lord Rayleigh, as to the absorbivity of rough surfaces being equal to unity. The general idea underlying his investigation is that owing to diffraction the waves amongst the deep corrugations in the surface spread abroad within them, and hardly any of their energy escapes out again. At the time I called his attention to the way a similar theory would explain the radiating power of rough surfaces, as I have taught here for years back. I am mentioning this now to call attention to an experiment of Magnus' mentioned in Jamin ("Cours de Physique," vol. iii. part 3, p. 113, top line, edition 1881; *Pogg. Ann.* vol. cxxiv. p. 476), where I have an old note concerning this theory, and which I had forgotten, to the effect that the radiation from platinised platinum was much greater than that from smooth platinum, but that the increase was chiefly in the ultra-red rays, for that the difference between the two plates was almost completely annulled by a plate of alum. This is what would be expected from the above theory, because corrugations that are small enough to affect the ultra-red radiations might still be too large to be anything but a smooth surface for the visible radiations. There is evidently a good deal still to be done on radiativity. GEO. FRAS. FITZGERALD.

Physical Laboratory, Trinity College, Dublin,
December 5.

Flame.

I TRUST that, in common with other readers of NATURE, I feel duly chastened by the homily which Dr. Armstrong has addressed to you on the subject of my lecture on "Flame." It is perhaps well that we should be warned from time to time against the sin of dogmatising. The only objection I have to the process is that I should be singled out as a sinner without some good reason being given for the selection. I am charged with forgetting that certain alleged facts "are but phenomena interpreted by our own limited intelligence," and yet I actually wound up my lecture with a quotation from Carlyle, intended to emphasise that very point. If Dr. Armstrong had said that *this* was an "appeal to the gallery," I should not have complained.

I do not feel equal to the metaphysical discussion to which Dr. Armstrong opens the way. I know only of one kind of fact, namely, "phenomena interpreted by our own limited intelligence," and it seems better to spell the thing in four letters than to bury it in phrases that smack of the pulpit.

Now let us see what I have done. I found on burning a hydrocarbon with a limited supply of oxygen, that in the cooled products of combustion all the carbon was oxidised, and that some of the hydrogen was set free. I had been brought up, like Dr. Armstrong, to cherish certain chemical dogmas, one of which was that the hydrogen of a burning hydrocarbon was oxidised before the carbon. I now asked myself what were the grounds for this dogma? It seemed to me to spring from the narrowest view of things, probably from the fact—I mean the by-limited-intelligence-interpreted-phenomenon—that hydrogen gas is easier to set on fire than a lump of charcoal. This was obviously an unscientific conclusion, for the carbon of a burning hydrocarbon is part of a gas, and when it is oxidised it has not, like a lump of charcoal, to be virtually gasified in the act of burning, and so to demand a high temperature and an untold amount of heat. I then read with great profit a paper by Dr. Armstrong, which confirmed my opinion that the heat of combustion of an atom of gaseous carbon, in forming carbon monoxide, must be exceedingly high, and so on all grounds I concluded that there was no *prima facie* reason for assuming that the hydrogen of a hydrocarbon would be oxidised in preference to the carbon. Experiment showed the opposite result; the carbon was oxidised, and I adopted the straightforward explanation, and renounced the old dogma. There were alternative explanations. It was conceivable that the hydrogen burnt first and liberated the carbon, which then acted upon the steam to produce one or both of the oxides of carbon and free hydrogen. We should then have two successive chemical reactions. I pointed out that there was only one piece of indirect evidence in favour of this view, and that has since been contradicted by Prof. Dixon. But Dr. Armstrong appears to suggest the view that the two chemical reactions are simultaneous. Now we know of plenty of chemical reactions which are best understood and remembered if we represent them by two simultaneous equations. When, for instance, zinc is heated with strong sulphuric acid, and we do not get hydrogen, we may explain the apparent anomaly by saying that hydrogen *is* liberated, but that it immediately attacks some of the hot sulphuric acid, producing sulphur dioxide and water. Or we may choose another pair of "normal" reactions which, being supposed to happen simultaneously, will explain the "abnormal" result. But surely no one thinks that the two reactions do proceed simultaneously. I use this method of exposition very largely, but I always tell my students that it is analogous to the treatment of forces in dynamics. We suppose a body acted upon by two forces whose direction and intensity may be conveniently represented by the adjacent sides of a parallelogram. The body *really* moves along the diagonal; *virtually* it has a double track, one along each of the adjacent sides of the parallelogram. In like manner, in our hydrocarbon-oxygen system, we may picture two compelling forces, viz. the tendency of oxygen to combine with carbon, and of oxygen to unite with hydrogen. You may, if you please (and this seems a fascinating exercise to Dr. Armstrong), shut your eyes alternately to each force and say, "first the hydrogen gets all the oxygen, and then the carbon snatches some from it," or you may just as well put it the other way about. I simply recorded the fact that the carbon got the most of the oxygen, with an explicit reference to the fact that I was dealing with the cooled products. Before my experiments were published Dr. Armstrong thought that hydrogen got most of the oxygen. He had actually tried to persuade Sir G. G. Stokes and many others to this effect. When my paper appeared he sought to discount the *facts* it contained by flights of polysyllables worthy of a great statesman.[1] Is it not strange that he should now turn to rend the man who relieved him from what he so abhors—a dogma?

The assumption that the products which are collected from a flame may have totally altered in kind during a minute fraction of a second is perfectly gratuitous, and a similar assumption might be made about nearly every reaction in chemistry. Dr. Armstrong might just as well forbid me to say that zinc and hot sulphuric acid give sulphur dioxide, as to say that when a hydrocarbon burns with limited oxygen, the carbon has the preference.

I will not trespass on your space with a discussion of the liberation of carbon in luminous flames. Dr. Armstrong's contentions on that matter are of precisely the same character as those I have dealt with above. Dr. Frankland has promised us some new evidence in favour of his theory. I trust I have always treated this theory with respect. I am not bigoted on the subject, though I await Dr. Frankland's promised publication with the same sort of feelings as those with which a Neapolitan might look forward to the reawakening of Vesuvius.

I have now, I hope, given an adequate reply to the question of scientific fact raised by Dr. Armstrong. I will not say much about the imputations he casts upon my scientific honesty. It ought not to be a light thing for a man in Dr. Armstrong's position to accuse a scientific worker of deluding an audience into unsound opinions by means of dazzling experiments, of playing to the gallery; of doing, in short, just those things which are most repugnant to the conscience of an earnest investigator. After years of personal friendship, I know Dr. Armstrong's idiosyncrasies very well, and they are, I imagine, pretty well known to the scientific world in general. I feel compelled, none the less, to ask him either to justify or withdraw his aspersions. I make no appeal *ad misericordiam*, and seek no comforting eulogy. It is a duty Dr. Armstrong owes no less to the scientific world than to myself to state clearly and precisely how I have departed from the standards of diffidence, deliberation, and exactitude that are becoming to a man who is honestly seeking to expound the truth. This, at any rate, is a matter the settlement of which is not contingent upon the arrival of that chemical millennium when we shall recognise "chemical interchange and electrolysis as interchangeable equivalent terms"; and I have the right to ask for an immediate and definite reply to my demand.

ARTHUR SMITHELLS.

December 2.

The Second Law of Thermodynamics.

CLAUSIUS' supposed deduction of the second law from the ordinary equations of dynamics in the form

$$\frac{\partial Q}{T} = 2\partial \log (iT)$$

has been discussed at length by Messrs. Larmor and Bryan in their Report on Thermodynamics for the British Association. They accept the deduction on condition that the system be conservative, that is, that the external as well as the internal forces acting on it are to be derived from a potential.

Now it is admitted that the equation

$$\frac{\partial Q}{T} = 2\partial \log (iT)$$

can be proved for a conservative system with the meaning of i given in the report. But in order that this equation, however true, may express the second law, T and i must be *independent variables*, or (which is the same thing when there is only one controllable coordinate v) T and v must be *independent variables*.

Now the second law implies comparison of two states, in either of which a substance can exist permanently. So if we seek to prove the law, or an analogous law, for a dynamical system, it is essential that we should compare two states of the system in either of which it is in *stationary motion*. One state may have the variables T and v, and the other may have $T + \partial T$ and $v + \partial v$, but there must be *stationary motion* with either pair of values. If then K be the virial of all the forces, external as well as internal, the Clausian equation, $K = 2T$ must hold. But if the system be conservative, as Larmor and Bryan assume, K is a determinate function of v, and the virial equation constitutes a relation between T and v, so that only one of them is independent. For example, a fixed quantity of gas in equilibrium in a vertical cylinder under a piston of mass "m" acted on by gravity. Clearly if T be given, v, the volume, is determinate, or we have only one independent variable. If m be disposable, you may make T and v vary independently, but then the system is not conservative. It seems to me that Larmor and Bryan's equation does not express the second Law.

Prof. J. J. Thomson, in his "Application of Dynamics to Physics and Chemistry," pages 95–100, proves the second law on a certain assumption. And Boltzmann, "Uber die Mechanischen Analogien des zweiten Hauptsatzes," has proved it on, I think, the same assumption. In order to show clearly the nature of the assumption I will begin a proof as follows, treating only the case of T temperature, and v volume. If χ denote the potential of all the conservative forces, p the external force necessary to maintain v constant, we have T being the mean kinetic energy of one of N molecules

$$\partial Q = N\partial T + \partial\overline{\chi} + p\partial v.$$

But by the virial equation

$$p\partial v = \tfrac{2}{3}NT\frac{\partial v}{v} - \frac{\overline{d\chi}}{dv}\partial v,$$

in which $\frac{\overline{d\chi}}{dv}$ is to be distinguished from $\frac{d}{dv}\overline{\chi}$ as explained in Watson's "Kinetic Theory of Gases."

Therefore

$$\partial Q = N\partial T + \tfrac{2}{3}NT\partial \log v + \partial\overline{\chi} - \frac{\overline{d\chi}}{dv}\partial v,$$

or

$$\frac{\partial Q}{T} = N\partial \log T + \tfrac{2}{3}N\partial \log v + \frac{1}{T}\left(\partial\overline{\chi} - \frac{\overline{d\chi}}{dv}dv\right),$$

now make

$$i = \frac{v^{\frac{1}{3}}}{T^{\frac{1}{2}}}$$

(a definite time), then

$$\partial \log T + \tfrac{2}{3}\partial \log v = 2\partial \log (iT),$$

and therefore

$$\frac{\partial Q}{T} = 2N\partial \log (iT) + \frac{1}{T}\left(\partial\overline{\chi} - \frac{d\chi}{dv}\partial v\right).$$

Now J. J. Thomson assumes (p. 97) that $\overline{\chi}$, in his notation V, is to be a function of v only, whence it follows that

$$\partial\overline{\chi} - \frac{\overline{d\chi}}{dv}\partial v = 0,$$

as he says, and so

$$\frac{\partial Q}{T} = 2N\partial \log(iT),$$

which I submit as a form of his result (118). I understand Boltzmann in the treatise above cited to make the same

[1] Here is a quotation from one of his letters to Sir G. G. Stokes: "Regarding the interactions in flames as consisting in a series of simultaneous and consecutive explosions, of which we can only examine the final steady state, it seems to me that the phenomena are necessarily of an excessively complex character, and that their appreciation and successful interpretation must tax our powers of mental analysis in a high degree. It will certainly be unwise at present to infer that the oxidation of the hydrocarbons, or the separation of carbon and also of hydrogen from them, takes place entirely in any one way." This seems to me like saying of a fall downstairs, that it is "a series of simultaneous and consecutive" bumps, &c. so difficult to trace out and presenting so many possible varieties of motion that it is hardly safe to call it a fall *down* stairs at all.

assumption. J. J. Thomson subsequently considers the case of χ being a function of T as well as of v (p. 100). But he does not in this case make $\frac{\partial Q}{T}$ a complete differential.

I think that in the general case we must regard χ as a function of the unconstrainable coordinates, and as varying from one configuration to another, through which the system passes in the same stationary motion with constant v. When v becomes $v + \partial v$ we do work in two ways. Firstly, we alter the value of χ for each configuration, doing thereby on the whole an amount of work equal to $\frac{d\overline{\chi}}{dv}\partial v$. Secondly, we alter the comparative frequency of different values of χ in the stationary motion. This is essential; for without this the system would not be in stationary motion with the altered values of T and v. I think J. J. Thomson had this in his mind when he made χ a function of T as well as v (p. 100).

Let, then, $f dx_1 \ldots dx_n$ or $f d\sigma$ denote the frequency of the configuration $x_1 \ldots x_n$, so that

$$\overline{\chi} = \int f\chi d\sigma, \quad \frac{d\overline{\chi}}{dv} = \int f\frac{d\chi}{dv}d\sigma,$$

and

$$\partial\overline{\chi} = \int f\partial\chi d\sigma + \int \chi\partial(fd\sigma)$$

$\partial d\sigma$ referring to variation of the limits of integration

$$= \int \chi\partial(fd\sigma) + \frac{d\overline{\chi}}{dv}dv.$$

and so

$$\partial\overline{\chi} - \frac{d\overline{\chi}}{dv}\partial v = \int \chi\partial(fd\sigma)$$

and

$$\frac{\partial Q}{T} = 2N\partial \log (i T) + \frac{1}{T}\int \chi\partial(fd\sigma).$$

Now how to make

$$\frac{1}{T}\int \chi\partial(fd\sigma),$$

or

$$\int \frac{\chi}{T}\partial(fd\sigma),$$

a complete differential?

If

$$B = \int f(\log f - 1)d\sigma$$

(Boltzmann's minimum function),

$$\int \log f\partial (fd\sigma) = \partial B + N\partial \log v,$$

and is a complete differential.

Hence one solution, and probably the only general solution, of the problem is obtained by making $\log Cf$ proportional to $\frac{\chi}{T}$, or

$$f = C\epsilon^{-c\frac{\chi}{T}},$$

where c is numerical. That gives

$$\frac{\partial Q}{T} = 2N\partial \log (iT) - \frac{1}{c}\partial B - \frac{N}{c}\partial \log v.$$

Since $2iT$ is the Action of the system during the definite time i, we see that the second law stands in a certain relation to the principle of least Action. But I think the complete treatment of it must be based on the virial equation. And it may be regarded as the law of the variation of B when T and the controllable coordinates vary.

S. H. BURBURY.

THE LOSS OF H.M.S. "VICTORIA."[1]

III.

LAST week we discussed the opinions expressed by the Board of Admiralty, in their Minute of the 30th of October, upon certain points that relate to the construction and stability of the *Victoria*; but the remainder was left for consideration in the present concluding article.

The value of an armour-belt at the ends for resisting damage.—Their lordships say "the fact that the *Victoria* was not armour-belted to the bow had no influence upon the final result of the collision. No armour-belt could have prevented the ripping open of the bottom below water by the ram-bow of the *Camperdown*, and the flooding of the compartments to which water could find access through the breach." Mr. White argues strongly against the assertion, which he states has been made, that if a strong armour-belt had existed at the place where the blow was struck, the damage might have been greatly reduced and the ship kept afloat. He considers that all the most important compartments which were flooded in the *Victoria* must have been thrown open to the sea under the conditions of the collision, even if there had been such a belt. "The breach in the side might have been different in form and possibly less extensive, especially above water; but it must in any case have been of large extent, and have admitted very large quantities of water in a short time." Mr. White proceeds to argue that the extent to which the *Camperdown* penetrated into the interior of the *Victoria* was not altogether a disadvantage, as the *Camperdown's* bow thus became virtually locked in the protective deck of the *Victoria*, till the relative forward movement of the latter ship was destroyed and the tearing action of her spur upon the side of the *Victoria* was thereby prevented. "Under the assumed condition of a non-penetrable armour-belt, this relative forward movement and tearing action must have taken place." But the Admiralty cannot admit the assumption of impenetrability. Reference is made to cases of collision, such as those between *Vanguard* and *Iron Duke*, and between *Grosser Kurfürst* and *Konig Wilhelm*, which prove, in Mr. White's opinion, that "the existence of an armour-belt is no sufficient safeguard against injuries resulting from serious collision."

The objections that have been made to leaving so much of the ends of some of our first-class battleships unprotected by armour, have been mainly in connection with their defence against gun-fire. The gun is, and appears likely for some time to be, the weapon of attack which a battleship must be designed primarily to resist. The attack of the ram can often be evaded by speed or skilful handling; and that of the torpedo by watchfulness, tactical resource, and smart conduct on the part of the officers in command. The real defence against rams and torpedoes lies at present much more in the judgment and skill with which a ship can be safeguarded or manœuvred by her officers, than in her own intrinsic power of resistance.

At the same time, it is obviously desirable that everything possible should be done to increase the amount of resistance that can be offered by a ship's hull to attack from ram or torpedo. The Admiralty say that an armour-belt would have no influence upon the final result of ramming. This statement is based upon two assumptions: (1) that "under a blow of such energy as was delivered on the *Victoria* the strongest armoured side ever constructed must have yielded and been driven in. Its water-tightness and that of the bulkheads, &c., within it adjacent to the place where the blow was struck, must have been destroyed, and the ultimate result (as regards the admission of water) would have been practically as serious under the same conditions of open water-tight doors, &c. as that which actually occurred in the *Victoria*"; and (2) that if the *Camperdown's* bow had been prevented by an armour-belt from penetrating to so great a depth as was stated into the side of the *Victoria*, her spur would have torn away much more of the bottom plating than it actually did.

The truth of both these assumptions appears very

[1] Continued from p. 127.

questionable. With regard to the first, it is pointed out that the ram-bow of the *Iron Duke* drove the armour of the *Vanguard* bodily inwards more than a foot. The armour of the *Vanguard* was, however, only 6 to 8 inches thick, while the force of the blow with which she was struck is said to have been two-thirds of that delivered to the *Victoria*. The armour at the point where the *Victoria* was struck would have been 15 or 16 inches thick if she had been fitted with an armour-belt, while the energy of the blow delivered to her is stated to have been "about the muzzle energy of a 12-inch 45-ton B.L.R. gun, the estimated perforation of its projectile being about 22½ inches of wrought-iron armour." The armour of the *Victoria* was not, however, of wrought iron, but of iron faced with steel, on the "compound" principle, which offers much greater resistance to penetration than wrought iron.

Seeing that the depth of the armour-belt would be 7 to 8 feet, and its thickness 15 or 16 inches: and that the projectile referred to, whose energy is about equal to that of the blow delivered to the *Victoria*, only succeeds in penetrating the plate by concentrating its whole effect upon an area 12 inches in diameter, it does not appear that the armour ought to suffer much from a blow distributed over so much greater an area. The armour of the *Vanguard* was driven in because the supports in its rear were not strong enough to resist the blow. In our present ships the top of the armour-belt comes against the edge of a protective deck, which is 2½ or 3 inches thick, and could well be supported and connected to it in such a manner as to effectually resist being driven inwards; and it appears to be mainly a question of fitting a similar bearing at the bottom of the armour, in connection with the armour-shelf, to furnish sufficient resistance at the lower edge. Such an arrangement for supporting the armour would not be difficult to devise; and it does not appear impossible to thus construct an armour-belt, in a ship like the *Victoria*, that would resist being driven in by such a blow as she received; and would do so without necessarily causing the water-tightness of the bulkheads, &c. adjacent to the place where the blow was struck, to be destroyed by the shock of the collision. The fact is that armour-belts have usually been arranged exclusively for keeping out projectiles from guns, and not with the view of resisting ramming. Had the latter been regarded as an important function for armour-plating to perform, the lower edge of armour, which would receive the first force of the blow in many cases, would have been supported in the rear better than it now is, and probably somewhat in the manner indicated.

The second assumption upon which the opinion that an armour-belt would have been useless is based is that, by preventing the *Camperdown* from penetrating so far as she did into the interior of the *Victoria*, there would have been serious tearing of the bottom abaft the breach as the ships got clear of each other. In support of this it is stated that the bow of the *Konig Wilhelm* tore open the bottom of the *Grosser Kurfürst* for some distance abaft the first breach, owing to the speed with which the latter vessel tried to cross ahead of her. This tearing action would depend very much, however, upon whether the point of the ram would have penetrated far enough into the bottom below the armour-belt to keep the ships together for a sufficient time, and it is quite likely that it would. Anyhow, it is impossible to say what depth of penetration would be necessary for this purpose, especially as the ram bows of ships by which a British vessel might be attacked are very different in length and form; and it seems a doubtful process of reasoning which leads to the result that the great depth to which the side of the *Victoria* was penetrated might not have been considerably reduced with advantage.

But the objections that have been made to leaving so great a length at the ends of a battleship without armour are not, as we have said, with reference to their being rammed, but because of the damage to which they are thus exposed by gun-fire. The results of the Admiralty calculations show that the effect of gun-fire upon the unarmoured ends of such ships as the *Victoria* might be very serious. We are informed by Mr. White that the *Victoria*, as she was at the time of the collision, would change her trim 3 feet by the bow in consequence of 110 tons loss of buoyancy above the protective deck. It follows, therefore, that if the whole of the compartments above the protective deck were penetrated so as to admit water, there would be a loss of buoyancy sufficient to change her trim fully 5 feet by the head. The change of trim and extra mean immersion thus caused by the loss of buoyancy would bring the top of the armour-belt close to the water-line at its fore end; and the slightest inclination would then be sufficient to immerse the fore end of the armour-belt on its inclined side. Perforation of the thin side plating at this point above the armour would thus admit water into the ship over the top of the armour-belt, and lead to a growing loss of buoyancy and stability, both transverse and longitudinal, which would soon place the vessel in a perilous position. The destruction of such a ship does not thus appear very difficult by the large rapid-firing guns that are carried in cruisers and in the secondary batteries of battleships. These guns, firing twelve to twenty projectiles of 6 inches and 4¾ inches diameter, per minute could be aimed with great precision at the water-line of a ship, and would very soon cause the whole of the thin partitions in the unarmoured ends to be penetrated through and through, and admit water freely into the whole of the compartments. If the vessel thus attacked were steaming ahead, at the slowest speed possible, the additional water that would thus be forced in would greatly increase the change of trim, and it would only be necessary to follow up the process of aiming at the water-line along the fore end, and over the top, of the armour-belt in order to soon disable or sink her.

The foregoing considerations may suffice to show that we see no sufficient grounds for believing the Admiralty to be right in the assertion that the absence of an armour-belt at the bow had no influence upon the final result of the collision in the case of the *Victoria*; still less that an armour-belt could not be made more effective than it now is against the attack of a ram; and still less, again, that an armour-belt of sufficient length to furnish all the buoyancy and stability necessary for safety would not afford a most powerful protection to a battleship against the destructive effects of the present rapid gun-fire.

The sufficiency of the stability possessed by the ship.—The Admiralty say "the capsizing of the *Victoria*, under the special circumstances described, does not suggest any insufficiency of stability in the design of that vessel. The provision made was ample for all requirements. When fully laden and in sea-going trim the metacentric height was 5 feet, stability reached its maximum at an angle of 34½ degrees to the vertical, and the range of stability was 67½ degrees." It will be observed that it is only the stability that would be possessed when the hull of the ship is absolutely intact that is here spoken of; and it is doubtless sufficient for that condition, and would enable the vessel to take considerable quantities of water on board without danger. This is not a point, however, which has great practical importance in connection with actual fighting requirements. In order to judge of the sufficiency of the stability under ordinary fighting conditions it is necessary to know what it would be when the thinly-plated ends and compartments outside the armour-belt are penetrated by projectiles. This is a factor of such great importance to the problem, as to make the bare information with regard to the stability in the intact condition comparatively valueless. Whether the provision of stability was ample, in the *Victoria*, for all requirements, as the Admiralty assert, or is ample in existing ships of similar type, depends almost entirely, as regards the fighting requirements of a first-class battleship, upon what it is when the thinly-plated ends are penetrated. It is quite certain that damage to the ends would soon make demands upon the stability, which necessitate the provision of a large reserve in the intact condition for drawing upon, and that this reserve should be sufficient to cover not merely the heeling effect of water held over to one side by longitudinal partitions, but also the reduction of stability due to loss of buoyancy in compartments that are opened up to the sea, and the effect of speed of ship upon the quantity of water that might be admitted, and the position into which it might be forced. There is no necessity to look far in order to see that the stability could thus be seriously reduced very early in an action, and might soon become insufficient to enable the ship to be handled and fought as she should be, if not to place her in absolute peril.

The steps that should be taken "to prevent the recurrence, under similar circumstances, of the conditions which, after the collision, resulted in the loss of the ship."—The Admiralty considers that the only step requisite is to issue regulations to the fleet which will ensure "that, under special circumstances, and particularly when there is risk of collision, doors, hatches, &c. shall be kept closed as far as possible, and men stationed at any that are necessarily left open"; also, "that under certain conditions arising out of collision or under-water attack, the gun-ports and other openings in the upper structure shall be closed before water can enter and endanger the stability of the ship." Now, everyone acquainted with the Admiralty and the Navy must know perfectly well that this really leaves matters as they were. Officers in command of H.M. ships are quite aware that water-tight doors, hatches, &c. require to be worked in the manner described; but the difficulty is to do it, in any emergency that may arise, so as to be effective for the purpose. The great number of water-tight doors, the difficulty of properly securing some of them, and the necessity that exists for many to be frequently open in order to carry on the ordinary work of the ship, makes it practically impossible to ensure that safety can always be relied upon by such precautions. It is true that the *Victoria* would, in all probability, not have been lost if all the water-tight doors and hatches in the fore part of the vessel had been closed; but it does not therefore follow that a similar disaster can be prevented in future merely by an order from the Admiralty directing that all such doors, &c. are to be closed in future in sufficient time to keep water from passing out of one compartment into another.

We would recommend that the number of water-tight doors in the various compartments be reduced; that no door which is essential to efficient water-tight sub-division, and is ever required to be left open without attendance, be fastened merely by clips; that all doors which are relied upon for safety should be capable of being closed either by a satisfactory self-acting arrangement, or by appliances for working them from a deck at a safe height above water. We would also call attention to the danger of making the safety of a ship depend upon the complete closing of a large number of small compartments. The only arrangement that can be relied upon is one of subdivision into a series of main compartments, formed by bulkheads that are carried to a deck that is high above the water-line, which will be perforated as little as possible by doors, or by pipes, &c. below water. Such an arrangement as that in the *Victoria*, where the efficiency of some of the divisional bulkheads, which appear to have formed part of the system of water-tight subdivision, depended upon the closing of scuttles in a water-tight deck only 3 feet above water, at which the bulkheads stopped, is manifestly untrustworthy; and it is impossible for the Admiralty to remedy its defects by promulgating orders to make it work.

So far as other ships of the type of the *Victoria* are concerned, the Admiralty does not see that the necessity for any improvement is indicated. We consider, however, as the foregoing remarks show, that the result of the ramming of the *Victoria* points clearly to the necessity of making the armour-belts longer in such ships if the armour is to be made really effective. This would increase the power of resistance to gun-fire, while the belts might be so fitted as to reduce the injuries likely to be caused by ramming. Water-tight flats at a small height above water, and thin bulkheads above water, are of little good against rapid gun-fire. Vessels with short armour-belts, such as the *Victoria* and others of her type, might, as has been pointed out, be destroyed by rapid gun-fire without any penetration of their armour; so that their defensive power is not measured by the resistance to penetration of the armour they carry. They thus belong more to the type that are called protected ships than to that of armour-clads; and it would probably be more correct to classify them as such. Their names now figure in the list of first-class battleships, and make our Navy appear stronger in this class of ships than it really is. If they were classed according to their real fighting value, the necessity for adding to the number of battleships would appear stronger than it now does to those who cannot judge the relative merits of ships.

Another lesson taught by the *Victoria* disaster appears to be that the officers of such ships should be more fully instructed with regard to the probable effects of various kinds of injury than they now appear to be. It is quite certain that the officers of the *Victoria* never imagined that the ship could sink so rapidly as she did, even with many of the water-tight doors open, or that her safety depended, to the extent it did, upon the absolute closing of so many small compartments. They require to be advised, and to obtain some experience as to the best mode of treatment under different conditions of damage. Would the captains of the ships with short armour-belts all know, for instance, whether it would be better or not to admit water into the ends before going into action? Has it been decided that it would be better to thus admit water, and prevent changes of trim as the thin ends become perforated by projectiles, or to keep water out as long as possible, and to submit to changes of trim and of heeling to one side or the other as the various compartments were opened up? The effect of loose water in the ends might be very objectionable if the speed of the ship were changing, or if she were rolling to any extent; but it would exist as soon as the ends became damaged; and it is clear that the presence of a large body of water in the long unarmoured ends of some of these ships would be a great source of difficulty in keeping speed and in manœuvring.

The general result of the Admiralty investigation, and of the judgment based upon it, is that there is no fault to be found with any single point connected with the construction and arrangements of the *Victoria*, or other ships of her type, for which those who conducted the investigations, or pronounce the judgment, could be held responsible; but that the whole blame is due to the one cause with which no one at the Admiralty could be in any way connected, viz. the failure to close all the water-tight doors, hatches, &c. in time to prevent the disaster. In saying this we do not wish to cast any doubt upon the accuracy of the calculations which have been made, or upon the desire of the Admiralty to arrive at a fair decision upon the questions raised. It is impossible for persons who are deeply interested in these questions, and in the results of the investigation, to divest themselves of all feeling and bias, and to judge their own ideas and work from an absolutely impartial standpoint. It would probably happen in any inquiry, that if one of the parties implicated were allowed to draw up the judgment, the result would not be unfavourable to itself. Most people appear satisfied, however, that this course should be taken when the question involved is that of the efficiency of the battleships upon which the defence of the British Empire would mainly depend in the event of war.

FRANCIS ELGAR.

THE NEW LABORATORIES OF THE INSTITUTE OF CHEMISTRY.

At length the members of the Institute of Chemistry may feel entitled to cry with Proteus, "Time is the nurse and breeder of all good," for now the object, kept steadily in view through evil report and good, though there was mighty little of the latter, has been achieved, and the Institute of Chemistry finds itself in the possession of a house with offices, council chamber, examination rooms, laboratories for examination, and everything handsome about it.

The successive councils are to be congratulated on the firmness with which they have resisted the numerous and persistent attempts which have been made by a somewhat important body of members to force the Institute into becoming a publishing and paper-producing body, thus adding another to the already too numerous chemical journals.

The Institute was not founded for this purpose; but the fact was forgotten again and again by those who were apparently unable to resist the temptation to spend the gradually accumulating funds of the Institute on "doing something," no matter what, but preferably holding meetings and printing a journal. The councils, however, proved wiser than some of their constituents, and held to the true view of their function, namely, that they were an examining and qualifying body.

Upwards of twenty years ago the passage of the Food and Drugs Act led to a series of appointments of public analysts that taken in the mass were little short of scandalous. The chemical profession had no corporate existence; it had never been consulted in the matter of drafting the Act, and the Government of the day, though having eminent chemists at command, never asked any advice. County and borough, corporation and vestry, were required to appoint "analytical chemists," and, left to their own sweet will in making the selection, with results that can be more easily imagined than described. It was this that literally forced the then small number of men who were practising chemistry professionally, to organise themselves with a view, not of undoing the mischief already done, for that was irreparable, but of gradually supplying a body of men whose qualifications were vouched for by a searching examination.

These examinations, at first held in town and at a number of provincial centres, have gradually concentrated in London, and the increasing number of examinees at length warned the council that the time had come when the Institute must be able to examine under its own roof.

The presidency of Dr. Tilden has been signalised by the carrying through of this project. After a prolonged search, suitable premises were found at No. 30 Bloomsbury Square, and the lease purchased. The House Committee, consisting of the President and Treasurer, with Prof. J. M. Thomson and Mr. R. J. Friswell, immediately set about planning the laboratory, the architectural work being placed in the hands of Mr. H. V. Lanchester.

The immediately surrounding property being residential, it was of great importance to prevent any nuisance from the escape of fumes, and the committee, in view of the almost universal failure of most of the fume apparatus in existing laboratories, placed themselves in the hands of one of their members whose experience as a chemical manufacturer led him to adopt the novel expedient of treating the laboratory as an acid factory, and scrubbing and burning the fumes, the latter by means of a specially constructed furnace, which also causes the draught by which the fumes are removed. So far this arrangement appears to work well, and it will no doubt be watched with interest by future builders of laboratories.

When the premises were taken over they consisted of a house of 36 feet frontage and 45 feet depth. Behind this lay a space of 60 feet by 36 feet, partly covered by an old building, once no doubt a stable, and partly occupied by an area and a built-out basement kitchen, which had a very large chimney, built independently of the house chimneys, and about 95 feet high and 18 inches square. The old building being removed, there remained an area of 34 by 36 feet for the principal laboratory, while the basement kitchen could be easily converted into a combustion laboratory, and its tall chimney—a factory shaft on a small scale—utilised for ventilating the fume cupboards and working benches.

The house faces nearly due west, and this permitted the laboratory to be lighted entirely from the north. As it was not possible to erect a lofty roof, it was decided to divide it into three gables, each having one side of glass, the other, turned towards the south, slated and matchboarded inside. These unglazed sides rise at an angle of 40°, and are so prolonged that the glazed sides, rising at an angle of 60°, meet them at an angle of 80°, and the entrance of direct sunlight is thus prevented, and, except for a very short time in the middle of the day, at midsummer. The main laboratory, 35 × 32 feet, is fitted with thirty-two working benches, and a desk for the examiner; two fume chambers and one bench of muffles being arranged at each end. It is lined throughout with white glazed bricks, the floor is of 2-inch pitchpine, and the working benches are of the same wood with mahogany edges, and tops saturated with high melting paraffin wax. Each bench has the necessary reagent shelves, seven drawers, and ample space with shelves beneath for the storage of large apparatus. It is also provided with two gas-cocks, a low-pressure water-cock, another for the supply of a condenser, and one high-pressure cock for a Sprengel filter pump. The sinks are circular, of salt-glazed stoneware, and so arranged that each supplies accommodation for four benches. Under each bench, below the floor level, runs an 8-inch Doulton pipe (which gives off 3-inch branches to each bench), and connects with a 12-inch main which runs along the front wall and descends to the level of the combustion laboratory floor, where it enters a salt-glazed stoneware tower packed with coke, and provided with a water shower. Passing up this, which is 2ft. 6in. diameter and 13ft. 6in. high, the washed fume is carried by another 12-inch pipe to the ground level again, and enters the ashpit of a furnace 4ft. × 1ft., which has fire-brick doors closing air-tight against planed cast-iron rims. Separate 8-inch pipes communicating with the 12-inch main go to each fume cupboard, and when the furnace is alight a most powerful draught, amounting to about 12,000 cubic feet of air per hour, is drawn from the benches and fume cupboards.

The stone muffle benches are each provided with a small flue in the wall, and will accommodate eight full-sized gas muffles. All the gas, water, heating, and fume pipes, together with the drains, which have specially arranged intercepting tanks to prevent the loss of mercury or the carrying of solid matter into the sewers, are carried beneath the benches in an ample stone-paved recess below the floor level. There is an easy means of access to all these pipes by sliding out the bottoms of the apparatus stores, which are arranged below each bench, and protected by an iron foot-rail.

Air from outside is also admitted from the same space, and is thus slightly warmed before entering the laboratory. The glass lights in the roof can, if desired, be opened. Artificial light is provided by six powerful self-ventilating Wenham gas-lamps, but it is hoped in time to provide incandescent electric lights to each bench.

Just outside the laboratory is a balance room fitted with six Oertling balances; this room is small, but the exigencies of the site did not permit of a larger area. Opposite the balance room a spiral staircase enables the examiner in charge to at once descend to the combustion laboratory. This room, 23 × 13, is fitted with seven stone-topped combustion benches, each 4ft. 6in. × 1ft. 3in., provided with a ¾in. fullway gas cock. Behind this is a vault lighted by prism light in the laboratory floor, in which is placed a powerful high-pressure water heating apparatus. Outside in the area is a washing-up room, provided with requisite shelves, sink, &c., and supplied with gas, so that the rougher operations of a laboratory, the handling of carboys, storage of acids and bulky chemicals, &c., can there take place.

From the house the laboratory is entered by a corridor starting from the cloak-room. The latter is large and amply provided with all necessaries, and with it communicates a commodious and well-fitted lavatory, having hot and cold water and all necessary fittings.

Behind the office, a handsome oak-floored room in the house itself will serve as a suitable laboratory for gas analyses.

Besides the accommodation here described, the house contains fifteen large rooms and a fine entrance hall. On the ground floor the front room serves as the office. The first floor supplies two large council and committee rooms, while the basement furnishes the housekeeper with ample accommodation. It will thus be seen that there is plenty of room for expansion.

The proverbial delays of the law prevented the House Committee from getting to work until August had begun. Its members are to be congratulated on the work they have done, and the time, four months, in which it has been accomplished.

The opening of the laboratories took place on Friday, December 8, at one o'clock, when the President received a number of gentlemen, who subsequently inspected the new buildings. The company included Sir F. Abel, F.R.S., Dr. Bell, F.R.S., Dr. H. E. Armstrong, F.R.S., Dr. Russell, F.R.S., Prof. Ramsay, F.R.S., Prof. Hartley, F.R.S., Prof. Clowes, Mr. C. E. Groves, F.R.S., Prof. Meldola, F.R.S., Mr. R. J. Friswell, Mr. O. Hehner, Dr. T. A. Lawson, Mr. D. Howard, Mr. Ernest Hart, and many other gentlemen and representatives of the press. Letters and telegrams regretting absence were received from Sir W. Foster, M.P., Sir H. Roscoe, M.P., Mr. Fowler, M.P., Mr. Norman Lockyer, F.R.S., Prof. J. M. Thomson, the Duke of Bedford, &c.

At half-past one the President delivered a short address dealing with the history and objects of the Institute, which now consists of 731 fellows and 104 associates, and has 200 registered students on its books. On the conclusion of this brief ceremony the laboratories being declared open, the President invited the assembled company to luncheon, which was laid in the council rooms. Sir F. Abel proposed the President's health, to which Dr. Tilden briefly replied, after which the meeting broke up.

SCIENCE IN THE MAGAZINES.

DR. A. R. WALLACE contributes to the *Fortnightly* the second part of his article on "The Ice Age and its Work." He deals in detail with the erosion of lake basins, first describing the different kinds of lakes, and their distribution, and then the conditions that favour the production of lakes by ice-erosion. The objections of modern writers are afterwards considered *seriatim*, and the manner in which they are handled will give pleasure to all glacialists. The alternative theory to that of ice-erosion, for the origin of the class of lakes discussed, viz. that they were formed before the glacial epoch, by earth-movements of the same nature as those concerned in mountain formation, appears to be fairly presented, and the difficulties in the way of accepting it are pointed out. Evidence is adduced to show that the contours and outlines of the lakes in question indicate erosion rather than submergence, and, finally, the Lake of Geneva is taken as a test of the two rival theories. As the subject discussed is very complex, and the argument essentially a cumulative one, Dr. Wallace gives the following summary of the main points:—

In the first place, it has been shown that the valley lakes of highly glaciated districts form a distinct class, which are highly characteristic, if not altogether peculiar, since in none of the mountain ranges of the tropics, or of non-glaciated regions over the whole world, are any similar lakes to be found.

The special conditions favourable to the erosion of lake-basins, and the mode of action of the ice-tool, are then discussed, and it is shown that these conditions have been either overlooked or ignored by the opponents of the theory of ice-erosion.

The objections of modern writers are then considered, and they are shown to be founded either on mistaken ideas as to the mode of erosion by glaciers, or on not taking into account results of glacier-action which they themselves either admit or have not attempted to disprove.

The alternative theory—that earth-movements of various kinds led to the production of lake-basins in all mountain ranges, and that those in glaciated regions were preserved by being filled with ice—is shown to be beset with numerous difficulties, physical, geological, and geographical, which its supporters have not attempted to overcome. It is also pointed out that this theory in no way explains the occurrence of the largest and deepest lakes in the largest river valleys, or in those valleys where there was the greatest concentration of glaciers, a peculiarity of their distribution which points directly and unmistakably to ice-erosion.

A crucial test of the two theories is then suggested, and it is shown that both the sub-aqueous contours of the lake-basins, and the superficial outlines of the lakes, are exactly such as would be produced by ice-erosion, while they could not possibly have been caused by submergence due to any form of earth-movements. It is submitted that we have here a positive criterion, now adduced for the first time, which is absolutely fatal to any theory of submersion.

Lastly, the special case of the Lake of Geneva is discussed, and it is shown that the explanation put forth by the anti-glacialists is wholly unsupported by facts, and is opposed to the known laws of glacier motion.

The *Contemporary* is included among the magazines that we have received, and to it Mr. Herbert Spencer contributes a rejoinder to Prof. Weismann. "As a species of literature," he remarks, "controversy is characterised by a terrible fertility. Each proposition becomes the parent of half-a-dozen, so that a few replies and rejoinders produce an unmanageable population of issues, old and new, which end in being a nuisance to everybody." If this opinion had come from anyone but one of the debaters it would have been ungracious. The questions at issue between Weismann and Spencer and Romanes have become so involved that some discrimination is required to unravel the tangled skein of argument. Mr. Spencer therefore confines his replies to those arguments of Prof. Weismann which are contained in his first article. The following points are of interest:—

Prof. Weismann says he has disproved the conclusion that degeneration of the little toe has resulted from inheritance of acquired characters. But his reasoning fails against an interpretation he overlooks. A profound modification of the hind-limbs and their appendages must have taken place during the transition from arboreal habits to terrestrial habits; and dwindling of the little toe is an obvious consequence of disuse, at the same time that enlargement of the great toe is an obvious consequence of increased use.

The entire argument based on the unlike forms and instincts presented by castes of social insects is invalidated by an omission. Until probable conclusions are reached respecting the characters which such insects brought with them into the organised social state, no valid inferences can be drawn respecting characters developed during that state.

A further large error of interpretation is involved in the assumption that the different caste-characters are transmitted to them in the eggs laid by the mother insect. While we have evidence that the unlike structures of the sexes are determined by nutrition of the germ before egg-laying, we have evidence that the unlike structures of classes are caused by unlikenesses of nutrition of the larvæ. That these varieties of forms do not result from varieties of germ-plasms is demonstrated by the fact that where there are varieties of germ-plasms, as in varieties of the same species of mammal, no deviations in feeding prevent display of their structural results.

Mr. Spencer also shows that for such caste-modifications as those of the Amazon ants, which are unable to feed themselves, there is a feasible explanation other than that given by Prof. Weismann. With regard to panmixia, he says:—

The tacit challenge I gave to name some facts in support of the hypothesis of panmixia—or even a solitary fact—is passed by. It remains a pure speculation having no basis but Prof. Weismann's "opinion." When from the abstract statement of it we pass to a concrete test, in the case of the whale, we find that it necessitates an unproved and improbable assumption respecting *plus* and *minus* variations; that it ignores the unceasing tendency to reversion; and that it implies an effect out of all proportion to the cause.

It is curious what entirely opposite conclusions men may draw from the same evidence. Prof. Weismann thinks he has shown "that the last bulwark of the Lamarckian principle is untenable." Most readers will hold with me that he is, to use the mildest word, premature in so thinking.

A short article on "Water Bacteriology and Cholera," by Mrs. Percy Frankland, appears in *Longmans' Magazine*. It deals chiefly with the value of sand filtration as a means of purifying water. The report of the cholera epidemic in Hamburg and Altona has strikingly proved that sand-filters offer a remarkable and obstinate barrier to the passage of disease organisms, as well as the ordinary harmless water bacteria. Here is a statement of the facts:—

These two cities are both dependent upon the river Elbe for their water-supply, but whereas in the case of Hamburg the intake is situated *above* the city, the supply for Altona is abstracted below Hamburg *after it has received the sewage of a population of close upon* 800,000 *persons*. The Hamburg water was, therefore, to start with, relatively pure when compared with that destined for the use of Altona. But what was the fate of these two towns as regards cholera? Situated side by side, absolutely contiguous in fact, with nothing in their surroundings or in the nature of their population to especially distinguish them, in the one cholera swept away thousands, whilst in the other the scourge was scarcely felt; in Hamburg the deaths from cholera amounted to 1,250 per 100,000, and in Altona to but 221 per 100,000 of the population. So clearly defined, moreover, was the path pursued by the cholera, that although it pushed from the Hamburg side right up to the boundary line between the two cities, it there stopped, this being so striking that in one street, which for some distance marks the division between these cities, *the Hamburg side was stricken down with cholera, whilst that belonging to Altona remained free*. The remarkable fact was brought to light that in those houses supplied with the Hamburg water cholera was rampant, whilst in those on the Altona side, and furnished with the Altona water, not one case occurred. We have seen that the Hamburg water, to start with, was comparatively pure when compared with the foul liquid abstracted from the Elbe by Altona, but whereas in the one case the water was submitted to exhaustive and careful filtration through sand before delivery, in Hamburg the Elbe water was distributed in its raw condition as drawn from the river.

Also in *Longmans'*, Sir John Evans writes on "The Forgery of Antiquities." From his history of ingenious frauds perpetrated in every branch of archæology we select the following:—

Of prehistoric antiquities, both in stone and bronze, forgeries are numerous, but it seems needless to enter into all the details of their character, and of the means that may be employed to detect their fraudulent origin. Suffice it to say that in the gravel-pits of the valley of the Somme and of the neighbourhood of London the manufacture of palæolithic implements takes rank as one of the fine arts. The chipping of the English forgeries is superior to that of the French, but in each case the lanceolate form is the favourite. The appearance of antiquity is usually given by a thin coating of fine clay, but at Amiens a plan of whitening the flint by long boiling in the family kettle has been introduced. . . . In some of the bone-caves of the Reindeer period, both in France and Germany, ancient bones have had designs engraved upon them by modern forgers, and ancient flint tools have been inserted in sockets of ancient bone so as together to form a composite falsification. Something of the same kind has been practised with regard to relics from the Swiss lake-dwellings, many of the bronze objects from which have also been imitated by casting.

Of neolithic implements forgeries are equally abundant, and in some instances equally difficult to detect. Large perforated axe heads when made of soft sandstone which could not possibly be used for cutting purposes, of course betray themselves; but the modern flint axes and arrowheads are not so easily distinguishable from the ancient. To the experienced eye there is, however, a difference both in the workmanship and the character of the surface, the ancient arrowheads having probably been worked into shape by pressure with a tool of stag's horn, and not by blows of an iron hammer. The grinding of the edges of modern imitations has usually been effected on a revolving grindstone; in ancient times a fixed stone was always used, on which the surface and edges of axes or hatchets were ground by friction.

"A Naturalist's Notes off Mull," by "Nether Lochaber," in *Good Words*, is a chatty account well worth reading.

Blackwood's Magazine contains a paper by Prof. Andrew Seth on "Man's Place in the Cosmos," being a criticism of Prof. Huxley's Romanes lecture on "Evolution and Ethics." Mr. J. Bickerdyke writes on "Successful Fish-culture in the Highlands." He explains some of the facts and principles which should be understood and considered before Highland fish-culture is attempted, and illustrates his subject with an account of some experiments made by Mr. Stewart at Kinlochmoidart.

An article on "Anthropometry as Applied to Social and Economic Questions" is contributed by Mr. C. Roberts to the *Humanitarian*. In it we note that the mean height of Fellows of the Royal Society is given as 5 feet 9·76 inches.

We have also received the *National Review* and the *Century*, but neither contains any articles of scientific interest.

EXPERIMENTS ON FLYING.

IF we imagine the linear dimensions of a bird increased n times, its weight will be increased n^3 times. On the other hand, the work necessary to keep it flying will, as Helmholtz has shown, increase n^7 times.[1] Now, we can assume that the power, that is to say, the amount of work that can be done in the unit of time, increases in proportion to the weight, or even less. Helmholtz, therefore, concluded that large dimensions are a disadvantage, and that there is a limit beyond which the power will become inadequate to the increased weight. This limit, in his opinion, is already attained in the largest birds, whose bodies appear to be constructed with the utmost economy in weight, and whose constitution and food seem adapted to furnish the highest power. And he therefore thought it improbable that man would ever be able to fly by his own power.

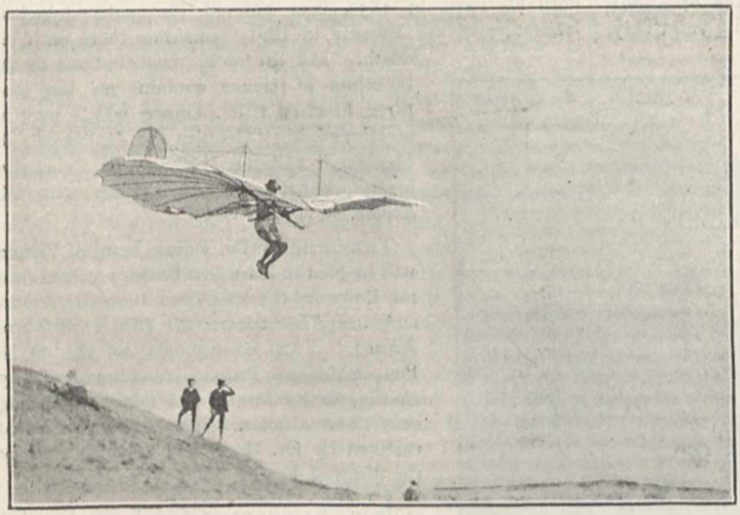

FIG. 1.

To these discouraging observations, however, some objections may be raised. First, the work necessary to keep a bird flying horizontally depends largely on its horizontal velocity. It decreases with increasing velocity up to a certain limit, when, on account of the friction, too much work must be spent on the horizontal component of the movement. The air will carry a body moving horizontally better than a stationary one, for the same reason that thin ice will sometimes carry a skater, but break under his dead weight. The moving skater is carried as if he rested on long skates that spread his pressure over a large area. The work which is expended in flying horizontally with a sufficiently high velocity may, in spite of Helmholtz's observations, be quite within the reach of human power. The difficulty, then, would only be to start and to arrive at this velocity, and this difficulty might be met by special contrivances. The size of a flyer might therefore be increased many times without losing the possibility of quick horizontal flight, though birds must be able to do without such contrivances for starting and arriving at the necessary velocity.

[1] Helmholtz, *Gesammelte Abhandlungen*, bd. 1, p. 165.

A second objection is that we see many birds—and especially the large birds—when soaring, evidently doing an extremely small amount of work, or none at all, but nevertheless moving rapidly, and even rising to great heights. It seems certain that the wind must do the work for them. The experiments of O. Lilienthal have shown how this is effected. He has made diagrams of the direction of the wind blowing over a plain, and has found it to be on the average three degrees upwards.[1] His idea is that the lower regions of the air are retarded by friction against the earth, and that it is therefore heaped up. Of course the rising air or an equal amount would have to come down again somewhere, and this might take place in calm weather. But however this may be, the wind in some way or other does the necessary work for soaring birds. With a bird of linear dimensions increased n times, this work, it is true, would only increase in proportion to the surface of the wings, that is, proportional to n^2, while the weight increases proportional to n^3. But for man there would be no difficulty in constructing the wing surface much larger in comparison than that of a bird.

The principal difficulty would lie in the management of the apparatus, in keeping the surface in the right position according to the variations of the wind, and according to the direction that one intends to follow. Perhaps it is not greater than the difficulty a skater meets with in keeping his balance while moving in the direction he pleases; but the consequences of a wrong movement are worse. O. Lilienthal seems to me to have taken a step in the right direction by trying to learn

FIG. 2.

soaring.[2] The accompanying illustrations, which are reproductions of instantaneous photographs taken in Steglitz, near Berlin, show the way he slides down a

[1] O. Lilienthal, "Der Vogelflug," p. 115; see also No. 55 of *Prometheus*, p. 37.
[2] See his article in Nos. 204 and 205 of *Prometheus*, from which the illustrations are taken.

slight decline of 10° or 15°. The shape of the wings is not flat but slightly curved. The experiments recorded in his book, "Der Vogelflug," show that the curved form has decided advantages both as regards the amount and the direction of the resistance. The wing surface is 15 square metres. It is not safe to take a larger surface before having learnt to manage a smaller one. He takes a sharp run of four or five steps against the wind, jumps into the air, and slides down over a distance of about 250 metres. By shifting his centre of gravity relatively

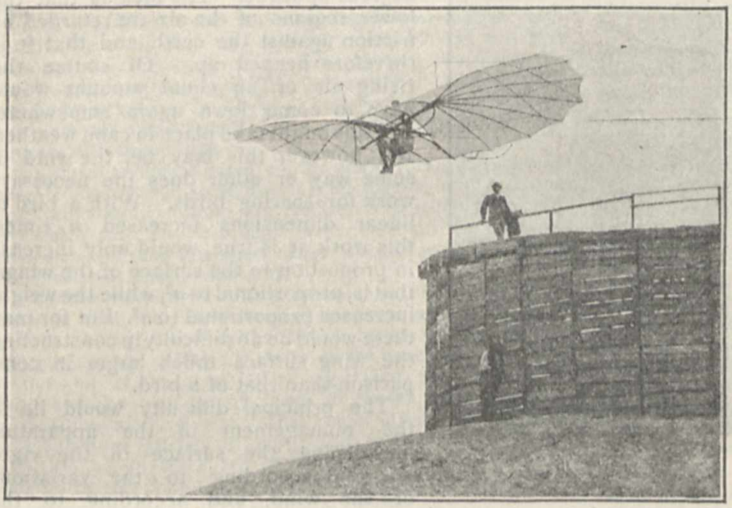

FIG. 3.

to the centre of resistance he can give the wing surface any inclination, and thereby can, to a certain extent, either slide down quicker, or slacken the movement, or alter the direction. If the wind is not too strong, and the surface of the apparatus not too large, I think there is very little danger in this kind of practice. If it is taken up by a great many people, improvements of the apparatus are sure to follow, and the art of keeping one's balance in the air will be developed. Perhaps this is the road to flying. At any rate it must be fine sport.

C. RUNGE.

NOTES.

THE funeral of the late Prof. Tyndall took place on Saturday, in the parish churchyard at Haslemere. It was the desire of Mrs. Tyndall that the assemblage upon that sad occasion should not be large, so the mourners were chiefly Tyndall's close friends. Among them were the following men of science:—Prof. Huxley, Sir Joseph Hooker, Sir James Crichton Browne, Lord Rayleigh (representing the Royal Institution), Sir John Lubbock, Prof. Michael Foster (representing the Royal Society), Prof. Rücker (representing the Royal College of Science), Prof. Williamson, the Hon. Rollo Russell, Mr. Alex Siemens (representing Sir William Siemens), Dr. Buzzard, and Dr. Atkinson. These mourners are eminent in many different branches of science; and it is hardly too much to say that their presence not only marked the regard in which Tyndall is held in our best scientific institutions, but also testified to the grief of all students of natural knowledge at the loss of one of the pioneers of the scientific movement in England.

A SPECIAL general meeting of the members of the Royal Institution will be held on Friday, December 15, to pass a vote of sympathy and condolence with Mrs. Tyndall on the occasion of the death of Dr. Tyndall, who was Honorary Professor of Philosophy of that Institution.

THE death is announced at Paris of the biologist Dr. Chabry, known for his work in experimental teratology.

THE Muséum d'Histoire Naturelle lost its able conchologist, M. Paul Fischer, on the 29th ult. He was born at Paris in 1835, and entered the palæontological laboratory of the Museum in 1861, remaining there until his death. The list of his contributions to the literature of science contains no less than three hundred titles, among which may be mentioned his "Histoire des Mollusques du Mexique," and the "Manuel di Conchyliologie," written in collaboration with M. Crosse.

THE friends of Dr. Julius Hann, of Vienna, will be glad to learn that he has received from the Emperor the rare decoration for science and art (*Ehrenzeichen für Wissenschaft und Kunst*). This corresponds to the Order Pour le Mérite in Prussia, but is bestowed very charily, the total number of holders of it being only about a dozen. The actual decoration received by Dr. Hann had been set free by the death of Prof. J. Stefan, the physicist.

PROF. RIGGENBACH has been elected a Correspondent of the Paris Academy, in the place of the late Dr. Colladon.

DR. J. RUSSELL REYNOLDS, F.R.S., has been elected President of the Royal College of Physicians, in the place of the late Sir Andrew Clark.

THE eleventh International Medical Congress will be held in Rome, from March 29 to April 5, 1894.

A REUTER's telegram from Berne announces that the Federal Council has decided to introduce the time of Central Europe into the Swiss postal telegraph, railway, and steamship services on June 1, 1894.

A PRIZE of 3000 liras is offered by the R. Istituto Veneto di scienze lettere ed arti, for the most important innovation in Venetian pisciculture. The research for which the prize will be awarded may relate to the artificial hatching of the eggs of any important species of marine fish, the introduction of new species, improvements in methods of ostriculture, or the production of better kinds of fish.

FOR some time negotiations have been in progress for the purchase of the Little Barrier Island, with a view to setting it apart as a home for New Zealand fauna. We are glad to learn that the island has now been obtained from its owner, and that there is nothing to prevent the scheme being carried into effect.

THE *Kew Bulletin* (Appendix i. 1894) contains a list of seeds of hardy herbaceous plants and of trees and shrubs available for exchange with colonial, Indian, and foreign Botanic Gardens, as well as with regular correspondents of Kew. No application for seeds can be entertained after the end of next March, except from remote colonial possessions.

THE Director of the Botanic Garden of Rio de Janeiro has prepared and issued a list of plants cultivated there, and offered in exchange. A descriptive catalogue will shortly be published containing a description of each separate species in the Garden.

THE weather during the past week has been very unsettled over the whole of the British Isles, owing to the approach of several large depressions from the Atlantic. On the 6th a large disturbance passed eastwards to the north of Scotland, causing south-westerly gales in the north and west, and during the night of Thursday, the 7th, another deep depression advanced from the south-westward, attended by serious gales in all parts, but of great severity in Scotland and in Scandinavia. The barometer at Stornoway fell to the exceptionally low reading of 27·97 inches during the afternoon of the 8th, giving a difference in the pressure of nearly an inch and a half between the extreme north and south of our islands. Further disturbances arrived from the westward both on Sunday and Tuesday, again causing gales from the south-east and south. The storm on the latter day was chiefly restricted to the southern parts of England and the northern parts of France, and has not been exceeded in violence by any that has visited our southern counties this season. Several places reported force 11 of the Beaufort wind scale. Much rainfall and some sleet accompanied these various disturbances; in the north of Scotland 1·2 inch of rain was measured on the morning of the 9th, and the Meteorological Office Reports for the week ended the 9th inst. show that in that district the rainfall greatly exceeded the average, the total amount being 2·6 inches, while in most of the English districts the fall was less than the average.

IT is now known that the earthquake which affected Tashkend on November 5, was also felt in other parts of Russian Turkestan. At Samarkand it was felt one minute later than at Tashkend—that is, at 8h. 23½m. a.m., and pretty strong oscillations of the soil lasted for about 1½ minutes. Crockery was shattered, and the water in the ponds and irrigation canals was set in motion. At Marghelan the strongest shock took place at 9h. 35m., and lasted for about five seconds; it was followed by a feebler one about three minutes later.

ON November 5, the magnetic instruments at Potsdam were disturbed in a manner which showed that a distinct earthquake had reached the observatory. The supposition that such a cause produced the movements of the needles was afterwards confirmed by the record of the seismometer of the geological laboratory of the Faculty of Sciences at Grenoble. From the magnetic curves at Potsdam it appears that the wave reached the observatory at 5h. 4m. 50s. a.m. (Potsdam mean time), and produced the greatest effect at 5h. 8m. 55s., a vibration also being recorded at 5h. 7m. 15s.. According to *Comptes Rendus* of November 6, the shock was first felt at Grenoble at 4h. 56m. 35s. (Potsdam time), hence the time taken to travel a distance of about 956 kilometres was 8m. 15s. The rate at which the wave was propagated was therefore about 1·94 kilometre per second. It is estimated that the time can be read off from the magnetograph curves with an accuracy of ten seconds.

A FEW months ago the President of the Alpine Club invited the co-operation of the Government of India in obtaining a record of observations on the movements of glaciers in the Himalayan Range, to supplement a similar record maintained of the movements of glaciers in Europe. Believing that such a record would prove of importance to geological and meteorological science, the Government have communicated with officials and others who are stationed in or near the snows, or who may visit from time to time the glacial regions of the Himalayas. Copies of the Alpine Club's memorandum of instruction in glacier observation have been forwarded to the Foreign and Military Departments of the Government of India, the Governments of the Punjab, North-Western Provinces, and Oudh and Bengal, the Meteorological Reporter, and the Director of the Geological Survey, for distribution to such officers as may be in a position to supply the requisite information. The energetic action of the Indian Government in the matter deserves high praise, and it will doubtless result in some interesting data being obtained.

A COPY of the splendid volume published in honour of M. Pasteur's jubilee has been sent to us. It opens with a brief account of the formation of the memorial committee; this is followed by a reprint of the address delivered by M. C. Dupuy at the jubilee celebration, and of the numerous addresses and telegrams received from all parts of the world. The volume also includes five beautiful plates, three of which represent medals struck in M. Pasteur's honour, one the investigator himself in his laboratory, and one is a fac-simile of the address presented by the Stockholm School of Medicine. This testimony of the esteem in which Pasteur is held brings to our mind the words, "Wisdom raineth down skill and knowledge of understanding, and exalteth them to honour that hold her fast."

AT the Adelaide meeting of the Australasian Association for the Advancement of Science, a lecture was given by Mr. C. W. de Vis, on the "Diprotodon and its Times." Popular interest has lately been aroused in this subject owing to an important discovery of fossil marsupial bones at Lake Mulligan. Mr. de Vis pointed out the mistake of the current idea that the Diprotodon was a gigantic kangaroo, any great resemblance between the two being confined to the teeth. In general build, Diprotodon was more like a wombat, but the bones of the thigh were even longer in proportion to those of the lower leg than is the case in the wombat, hence it might be concluded that the Diprotodon was less capable of rapid motion than the wombat. The spongy texture of the bones of the skeleton indicates that it frequented lakes and marshes. There were two species of Diprotodon found in Central Australia—*D. australis*, Owen (circa 6 feet high and 10 feet long), and *D. minor*, Huxley (circa 5 feet high and 8 feet long). The arid central plains of the present had been occupied in Diprotodon times by vast extents of luxuriant forest and richly vegetated districts, well-watered by wide rivers. The marsupials were even then the dominant type of life in Australia; lizards were also numerous, and some were of unusually large proportions, *e.g.* Megalania, an extinct "guana," 18 to 20 feet in length. Extinct forms of alligators and turtles infested the waters, and amongst the fishes was the still existing Ceratodus. The remains of a varied bird fauna have been well preserved in the same deposits. This fauna included some ancestral forms connecting, on the one hand, the wingless birds of New Zealand with the Australian emus, and, on the other hand, the Australian birds with the New Zealand Apteryx. Mr. de Vis was inclined to attribute the disappearance of so many of these ancient forms of life from Australia quite as much to senile decay as to altered climatic influences.

THE slow ascensional movement of Scandinavia, evidenced by the displacement of tide marks, the peculiarities of Scandinavian lake fauna, and other geographical and geological phenomena, is subjected to mathematical investigation by M. A. Badonrean, who, in the *Comptes Rendus* of last week, treats the subject from the point of view of thermal expansion. At the time of the last glacial epoch, the Scandinavian ice-sheet covered the greater portion of the peninsula, as well as Finland and the Baltic, the area of this sheet being about 1500 km. in diameter. Where the soil touched and partly liquefied the mass of ice, its temperature must have been 0° C. At the present time, the mean temperature of the soil over the area of the ancient ice-sheet is 3° C. Taking the coefficient of expansion of the rocks as eight-millionths, the elevation of the centre of the ice-cap is calculated at 229 m., and the isoanabatics, or lines of equal elevation, should be parallel to the contour. De Geer's map of these isoanabatics, traced in 1890, satisfies these conditions, allowance being made for the want of homogeneity in the rocky mass, and the want of fixity of its borders.

AN interesting account of a fine series of glacial potholes on Cooper's Island, Little Harbour, Cohasset, U.S., is given by Mr. William O. Crosby, in a paper on the "Geology of the Boston Basin" (Occasional Papers of the Boston Society of Natural History, IV.). It is shown that the potholes were formed by *moulins*, or glacier mills, and Mr. Crosby discusses a question raised in these columns a short time ago, viz. why, as the ice-sheet moves continuously forward, carrying the crevasses and *moulins* with it, the potholes escape elongation in the direction of the movement? The true explanation of many glacial potholes is found in the fact, that a crevasse closes as it is carried forward by the general movement of the ice, a new one subsequently being formed just where in relation to the land at the margin of the glacier the former one existed. This explanation, however, is not applicable to the Cohasset potholes, and in place of it Mr. Crosby makes the suggestion that a *moulin* may remain approximately stationary, while the ice moves on, through the backward erosion and melting of its up-stream side; and that when a pothole is formed at the bottom of a *moulin*, it is not the direct impact of the water upon the face of the ledge that does the work, nor do the stones carried down by the water wear the ledges appreciably by their direct fall, but the pothole is due to their subsequent movement, and especially their rotation, by the water. This rotation implies an antecedent depression or hollow to hold the stones, and thus the conditions are seen to be essentially the same as for ordinary river potholes. Since the rotation of stones in a pre-existing hollow appears to be an essential condition of glacial as of other potholes, and the *moulin* simply supplies the power, it would seem to make little or no difference whether the water plunges into the up-stream side, the middle, or the down-stream side of the hollow. The pothole is made where the hollow exists, and during the progress of a *moulin* across the hollow, there would not, apparently, be any marked tendency to elongate it. In the case of a linear group of potholes on the ice-slope of a ledge, concludes Mr. Crosby, it is reasonable to suppose that the upper one, which on Cooper's Island is always the smallest and most indefinite, marks the shifting position of the *moulin*, and that the others were formed by the subglacial flow of water from the bottom of the *moulin*.

IT has been supposed, says Mr. A. J. Jukes-Browne, in the *Geological Magazine* for December, that the total amounts of silica existing in the chalk with flints and the chalk without flints respectively, are very nearly equal; and this supposition favours the theory that flints have been formed by some process of segregation after the consolidation of the chalk containing them. It is generally conceded that the silica from which such flints were made was a soluble form like that of sponge spicules, diatoms, or radiolaria; hence by chemical analyses, aided by microscopical discrimination between crystalline and colloid siliceous particles, it is possible to determine whether flintless chalk always contains soluble silica, and whether chalk with flints contains little or none. Mr. Jukes-Browne has made this investigation, and he finds that there is no definite relation between the occurrence of flints and the absence or presence of soluble silica in the surrounding chalk. He thinks that chalk which is now destitute of any remains of siliceous spicules, has, since it became chalk, always been destitute of such spicules. These conclusions have a very important bearing upon the question of the formation of flints.

IN a recent number of the *Comptes Rendus*, M. A. Delebecque gives the results of some observations made last summer on water from various depths in inland lakes, which show that the amount of solid matter in solution increases with the depth. Thus in the lakes quoted below the amount of dissolved solid matter in grammes per litre was:—Annecy, surface 0·138, bottom (65 metres) 0·157; Aiguebelette, surface 0·114, bottom (71 metres) 0·1605; Nantua, surface 0·154, bottom (43 metres) 0·190; Saint-Point, surface 0·152, bottom (40 metres) 0·182; Remoray, surface 0·1605, bottom (27 metres) 0·205; Crozet, surface 0·0275, bottom (37 metres) 0·0368. The water samples were collected about 3 metres above the bottom, by means of Dr. H. R. Mill's water-bottle. M. Delebecque agrees with Dr. Duparc, of Geneva, that the small amount of dissolved matter in the surface water is due to its removal by the calcareous organisms which swarm in the upper layers.

THE *Philosophical Magazine* for December contains a paper, by Sidney J. Lochner, on the elongation produced in soft iron by magnetisation. The author undertook the investigation of this subject in order, if possible, to settle whether the experiments of Bidwell or Berget represented what really happens. In order to measure the elongation, what was essentially a Michelson's interferential refractometer was made use of, which was capable of measuring an elongation of a millionth of an inch. The bar of iron, whose elongation was to be measured, was placed inside a long magnetising coil, and carried at one end one of the mirrors of the refractometer. The expansion due to the heating effect of the coil being slow, while that due to magnetisation was rapid, the two could be distinguished. The author finds that, for a given magnetising field, different elongations are produced according to the manner in which the magnetising current is applied. Thus different elongations were produced in the cases where the current had been turned on suddenly, or had been applied gradually; and in the latter case it made a difference whether the current had reached its final value by increasing slowly, or by decreasing slowly from a higher value. Another peculiarity observed was that if the current be gradually increased from zero, at a certain point a maximum expansion is reached; after this a further increase of the current will produce a decrease in the elongation; if, however, instead of increasing the current when the maximum is reached, it is gradually decreased, it is possible to obtain a still greater elongation. The observations show that the expansion is a function of the ratio between the diameter and length of the bar, and that the elongation varies approximately directly as the square root of this ratio; also, the expansion varies directly as the permeability. The amount of current required to produce the maximum expansion also depends on the ratio between the diameter and length.

THE bacterial efficiency of porous cylinders in the filtration of water for domestic purposes is the subject of considerable discussion just now. Kirchner (*Zeitschrift f. Hygiene*, vol. xiv. p. 307) found in his experiments with water purposely infected with typhoid bacilli, that such filters were incapable of arresting these organisms. Large quantities of typhoid infected broth were added to the water before filtration, and the filtrate after 48 hours was found to contain very large numbers of typhoid bacilli. Dr. Schöfer, in a recent number of the *Centralblatt f. Bakteriologie*, vol. xiv. p. 685, gives the results of his investigations of porous cylinders as regards their retention of typhoid bacilli. In these experiments as small a quantity as possible of nutritive material was added with the typhoid organisms to the water (previously sterilised), and even after 24 days the filtrate was found to be perfectly sterile, although the unfiltered water was freshly infected with typhoid bacilli no less than twelve times during the investigation. Very different results were, however, obtained when broth was purposely added to the unfiltered water, an addition of as little as 5 c.c. to 600 c.c. of water so stimulating the growth of the typhoid organisms, that two days later they appeared in the filtrate; the numbers present, however, gradually decreased, but on again adding 5 c.c. of broth they rose on the following day from 9 to 6,139 per c.c. This large increase was due to the rapid multiplication of the few isolated bacilli still remaining in the pores of the filter in consequence of the supply of food material to the water in the shape of broth, for no fresh infection with typhoid organisms had taken place. Dr. Schöfer is of opinion that typhoid bacilli as present in water, under ordinary circumstances, are not supplied with the requisite conditions for their growth and multiplication, and are, therefore, incapable of growing through these porous filters, and so reaching the filtrate; but these conditions are, however, undoubtedly furnished when a sufficient supply of food material is contained in or added to the water, under which circumstances the cylinders are unable to retain them. These experiments not only explain the unsatisfactory results obtained by Kirchner, but indicate what precautions should be taken in the bacteriological investigation of such filters.

THE last two parts of the well-known "Notes from the Leyden Museum," forming parts 3 and 4 of vol. xv., were published in July and October. They contain numerous papers describing new or rare species of mammals, birds, reptiles, &c., added to the museum. Among the articles we notice one which is by F. E. Blaauw, the Secretary of the Zoological Society of Amsterdam, on a comparative list of the birds of Holland and England. Holland, although so much smaller than the United Kingdom, is the regular abode, at different seasons, of 221 species of birds, whilst the British Islands can only boast of 211. Dr. R. Horst continues his descriptions of earth-worms, giving a list of species found, for the most part by Dr. H. ten Kate, during his journey in the Malay Archipelago in 1891. A large number of the species belong to the genus Perichæta, of which no less than seven species are described as new, bringing the number of the species of this genus already found in the Malay Archipelago to thirty-three. The following note, by Dr. Jentink, will be interesting to others besides book collectors. In the Proceedings of the Zoological Society of London for 1880 (p. 489), Mr. F. H. Waterhouse gives the dates of the publication of the parts of Sir Andrew Smith's "Illustrations of the Zoology of South Africa," and states that as the copy he examined "did not contain plates 18 and 38 (Mammalia), he had examined three or four other copies, and as neither of these plates are to be found in any of these, he presumed they do not exist." Now, in the copy in the Leyden Museum's library, plate 38 is present, but plates 18 and 37 are wanting, and at the bottom of the page containing an index of the Mammalia, there is the following: "Plates 18 and 37 *not* published." Librarians will call to mind how often the collating of this fine work has perplexed them.

THE Royal Meteorological Institute of the Netherlands has recently issued its *Jaarboek* and *Onweders in Nederland* for the year 1892. The first work has been regularly published for forty-four years, and now contains hourly observations taken at four stations, in addition to those taken at specified hours at a number of other places. It also contains observations taken in Surinam (South America) and French and Upper Congo. The second work is the thirteenth of the series, and contains a discussion of each of the thunderstorms which have occurred during the year, with reference to the general weather conditions over Europe.

WE have received from Mr. John Elliot, the Meteorological Reporter to the Government of India, the daily weather charts of January, 1893, for the Indian sea and land areas.

MM. J. B. BAILLIÈRE ET FILS, Paris, have issued an ornithological bibliography containing announcements of five or six hundred works on ancient and modern birds.

HERR MORITZ, Berlin, has published Nos. 1–4 of his "Antiquariats-Katalog." The catalogues are of special interest to geographers and anthropologists, and they contain many rare works.

MESSRS. FRIEDLÄNDER AND SON, Berlin, have sent us Nos. 16–21 of their "Naturæ Novitates." These bibliographical lists contain works in every branch of science, and are invaluable to the scientific book-hunter.

ANOTHER catalogue, recently issued, is one containing the titles of works on geology offered for sale by Messrs. Dulau and Co.

THE first number of the *Psychological Review* will be published early in January, by Messrs. Macmillan and Co., London and New York. It will be edited by Profs. J. Mark Baldwin (Princeton) and J. McKeen Cattell (Columbia). The *Review* is intended to contribute to the advancement of psychology by publishing the results of original research, constructive and critical articles, &c., in connection with the subject.

THE *American Naturalist* for November contains several interesting articles. Mr. Howard Ayers writes on the genera of the Dipnoi Dipneumones, and Dr. J. Weir gives a number of examples of animal intelligence. A collection of molluscs from North-Western Louisiana is described by Mr. T. Wayland Vaughan, and Mr. H. C. Mercer compares the Trenton and Somme gravel specimens with ancient quarry refuse in America and Europe.

MESSRS. NEWTON and Co. have issued a new catalogue of optical lanterns, microscopes, and polariscopes for demonstrations in science. There are very few class experiments that do not admit of being projected upon a screen by means of the many good lanterns in the market, and certainly there is no better method of demonstrating scientific facts to a large audience. One of the finest lanterns made by Messrs. Newton is the triple rotating electric lantern designed by Sir David Salomons. We learn that the Royal Society has just ordered an instrument of this kind.

A GENERAL method of artificially preparing crystallised anhydrous silicates similar to the naturally occurring pyroxenes, is described by Dr. Hermann Traube in the current *Berichte*. It consists in precipitating the particular metallic silicate, which it is desired to obtain in anhydrous crystals, by the addition of a solution of sodium silicate to a solution of a salt of the metal. The amorphous hydrated silicate thus precipitated is heated to a high temperature with boric acid for some hours. When most of the boric acid has volatilised, the anhydrous metallic silicate is usually left in the form of good crystals. Ebelmen has already succeeded in artificially preparing the magnesium pyroxene $MgSiO_3$ by this method; and Dr. Traube now extends its application. When precipitated silicate of zinc, for instance, obtained by the addition of a solution of sodium silicate to one of zinc sulphate, is dried, and then heated with eight times its weight of fused boric acid, in a platinum crucible, for a few days, to the highest temperature of a porcelain manufacturer's furnace, a large proportion of the boric acid disappears by volatilisation, and upon extraction of the remaining portion from the cooled residue with water, beautiful little insoluble crystals of anhydrous silicate of zinc, $ZnSiO_3$, remain. When examined under the microscope these crystals are observed to be perfectly transparent prisms with domal terminations. Their optical characters indicate that they belong to the rhombic system of symmetry. This artificial silicate of zinc would thus appear to be isomorphous with the naturally occurring magnesium silicate, enstatite, $MgSiO_3$. The method is also applicable to the synthesis of complex mixed silicates, and it is possible by means of it to reproduce almost any of the naturally occurring silicates of this class.

AT the last meeting of the Southern District Association of Gas Engineers and Managers, Dr. L. T. Thorne gave an account of further experiments with the new process for enriching coal gas by means of oxy-oil gas. Dr. Thorne has been enabled to carry out an exhaustive series of tests at Huddersfield, where the process is now in actual operation. His conclusions are summarised as follows: (1) The addition of oxygen to oil gas, preferably while the latter is still hot, not only increases the illuminating value of the oil gas when employed directly as illuminant, but also when it is used for purposes of enrichment. (2) Oxy-oil gas is a highly permanent gas, and when used as an enricher of coal gas actually increases the stability of that gas. (3) Enrichment of coal gas by oxy-oil gas would cost about one-third of a penny per candle per thousand cubic feet. Dr. Thorne concludes by expressing the opinion that the experimental results place oxy-oil gas at the head of the enriching processes yet known, and fully justify the favourable view of the process which was expressed in an earlier communication. With regard to the actual working of the Huddersfield plant, we learn from *London*, the organ of the London County Council, of November 30, that the Huddersfield Corporation have now used the new gas continuously for over two months, and have obtained a steady white flame, affording a better light, while enabling a saving to be effected at the rate of £10,700 per annum. They are now using 36,000 cubic feet of the new gas per day for enriching the ordinary product. They have been in the habit of enriching their ordinary gas, which is of about sixteen candle power, to the extent of four additional candles, by means of cannel coal. The cost per candle at Huddersfield, using Yorkshire cannel, has been about three-halfpence per cubic foot. With the new plant of the oxy-oil process the actual working cost is at present less than a halfpenny per candle per thousand cubic feet, and will eventually be still less by thirty per cent. or more, as crude petroleum is rapidly becoming cheaper. Moreover, the coke produced from cannel coal is so useless that the Huddersfield Corporation have been unable to dispose of it, even to give it away. Under the new process they find no difficulty in selling all the coke they can produce, for seven shillings and sixpence per ton. The saving due to enrichment amounts to £7,700 per annum, and the gain from sale of coke to £3,000, results which will have the practical effect of reducing the price of gas to the consumers at Huddersfield by at least threepence per thousand cubic feet, while supplying them with a more cheerful light which is stable even in winter.

NOTES from the Marine Biological Station, Plymouth.—There has been little that is novel to record lately, owing to the inability of our small boats to face the stormy seas. Last week several specimens of the Teleostean *Sciæna umbra* were brought in, and the Nemertine *Eupolia curta* (second capture) and the Crustacean *Gebia stellata* were taken in the Sound. The floating fauna is poor as a rule, but there is an increasing number of Annelid trochospheres, *Scyphonautes* and Opisthobranch veligers. There is a noteworthy scarcity of Medusæ. The Anthozoa *Alcyonium digitatum* and *Cereus pedunculatus* (= *Sagartia bellis*), and the Crustacea *Pandalus annulicornis*, *Crangon vulgaris*, and one-year-old *Carcinus mænas* have begun to breed.

THE additions to the Zoological Society's Gardens during the past week include a Pale-headed Parrakeet (*Platycercus pallidiceps*) from North-East Australia, presented by Mr. C. B. Lewis; two Common Crossbills (*Loxia curvirostra*), a Song Thrush (*Turdus musicus*) British, presented by Mr. H. C. Martin; two Alligators (*Alligator mississippiensis*) from the Mississippi, presented by Mr. Austin E. Harris; a Chacma Baboon (*Cynocephalus porcarius*, ♀) from South Africa, presented by Mrs. Rowland Tomson; two Leopards (*Felis pardus*) from India, deposited; thirteen Rufous Tinamous (*Rhynchotus rufescens*) from Brazil, purchased; a Japanese Deer (*Cervus sika*, ♀) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

NEW NOTATION FOR LINES IN SPECTRUM OF HYDROGEN.—The application of the photographic plate to that important instrument of physical astronomy, the spectroscope, has brought to our view, in addition to the four well-known lines of hydrogen in the visible part of the spectrum, another set of similar lines, the first of which, having a wave-length less than that of H_1, coincides with one component of H_1 of the broad double line in the solar spectrum which Fraunhofer termed H. The second component, written H_2 or K, is wanting in many stars of Vogel's class Iα; yet its coincidences with the line H_2 or K, where in this class another line in the region of H_1 makes its appearance, became established, so that no opportunity offered itself to make a special nomenclature for the two first lines above H*d* outside of the star's spectrum situated in the violet region. The other lines Huggins named with the Greek characters α, β, γ, &c. A new system of nomenclature, suggested by Prof. Vogel, in the *Astronomischen Nachrichten* (No. 3198), has many points in its favour. The four lines in the visible region, C, F, G, and *h*, retain their old signs of Hα, Hβ, Hγ, Hδ, but H or H_1 is here changed to Hϵ, and the α, β, γ lines of Huggins to Hζ, Hη, &c., thus making the nomenclature thoroughly consecutive. Prof. Vogel says that in future he shall adopt this new notation, and that Dr. Huggins has also agreed to the arrangement, viz. that the hydrogen lines should always have the element sign H coupled with a Greek letter as index, as shown in the following table, in which are given the new and old notations with the wave-lengths:—

Wave-lengths.	Notation.	
	New.	Old.
656·3$\mu\mu$	Hα	Hα or C
486·1	Hβ	Hβ or F
434·1	Hγ	Hγ (written often wrongly with G)
410·2	Hδ	Hδ or *h*
396·9	Hϵ	H or H_1
388·9	Hζ	α
383·6	Hη	β
379·8	Hθ	γ
377·1	Hι	δ
375·0	Hκ	ϵ
373·4	Hλ	ζ
372·2	Hμ	η
371·2	Hν	θ
370·4	Hξ	ι

THE SPECTRUM OF NOVA NORMÆ.—Prof. Pickering, in *Astronomischen Nachrichten*, No. 3198, gives some details about the discovery of the new star in Norma. The star was found by Mrs. Fleming on October 26 when, examining a photograph of the *spectra* of the stars in this constellation, the negative having been taken by Prof. S. J. Bailey at the Arequipa station on July 10, 1893. Comparing the spectrum with that obtained in the case of Nova Aurigæ, nearly the same dispersion having been employed, it seems that they are nearly identical—"about a dozen lines are visible in each, and are identical in wave-length." The line F, although bright in both stars, is more intense in Nova Normæ, and, further, is more intense than any other line, while G was generally strongest in Nova Aurigæ. With regard to the time of the outburst of this new star, photographs indicate that it must have occurred within the first ten days of July 1. A photograph taken June 21, 1893, shows no trace of it upon the plate exposed to that region, while charts of the same region taken on June 6, June 10, July 21, 1889; May 16, May 16, June 10, June 23, June 23, 1891; May 7, and May 27, 1893, also give no indication of a star in that position. The similarity of the spectra of these two new stars is of interest, as Prof. Pickering points out, in that it has proved a means of discovering one of these objects, and that, if confirmed by other new stars, it will indicate that they belong to a "distinct class resembling each other in composition and physical condition." The nearest catalogue stars to which the Nova lies are Cord. G.C. 20,940 and Cord. G.C. 20,926 of the 8 and 8·75 magnitude respectively, the Nova being nearly midway between them. We may add that the above communication seems to throw some doubt on the accuracy of the note we wrote three weeks ago (November 23), with reference to Prof. Kapteyn's search through his Durchmusterung. Until the exact position of Nova Normæ is obtained, one cannot of course make any statement, but it seems probable that Prof. Kapteyn's and Mrs. Fleming's stars are not the same.

PROF. RUDOLF WOLF, OF ZURICH.—We are very sorry to have to record this week the death of Prof. Rudolf Wolf, the well-known director of the Zurich Observatory. He died at midday on November 6, after a short illness, at the age of seventy-eight years. By his death astronomical science has lost one of her most devoted servants. It was through his work, coupled with that of Schwabe, that the existence of the periodicity of the sunspots was without doubt first accepted, and its length determined to be eleven and one-ninth years. The deceased was, among other things, the author of the work on the "Geschichte der Astronomie," and also of a "Tachen-buch für Mathematik, Physik, Geodäsie und Astronomie," both of which ran through several editions.

THE COMPANION TO THE *Observatory*.—The Companion for the year 1894 follows the same lines as it has done in former years. No additional matter has here been added, unless we mention the ephemeris for the elongations of the satellites of Mars, which planet comes into opposition during next year. We notice that in Mr. Denning's list of meteor showers, instead of November 27, he has this year thought fit to alter it to November 23–27, an alteration justifiable by facts. With regard to eclipses, on March 20–21 a partial eclipse of the moon will take place, but will be invisible at Greenwich. An annular eclipse of the sun, just visible as a partial one in Norway, Sweden, Eastern Europe, and Asia, occurs on April 5, while on September 14 a partial eclipse of the moon will be partly visible at Greenwich. The total eclipse of the sun, on September 28, lasts only for eleven seconds (maximum duration), and as the path of the centre of the shadow lies entirely across the Southern Indian Ocean, the occurrence is of little scientific interest. On November 10 a transit of Mercury across the sun's disc will be partly visible at Greenwich, the first contact taking place before sunset. The times are—

	Ingress. h.	m.	s.	Egress. h.	m.	s.
External contact ...	3	55	40	9	13	9
Internal ,, ...	3	57	23	9	11	26

For the sun in the zenith at the time of egress, the place of observation lies 63° W. and 17° S. or in Bolivia, South America, that for egress lying 142° W. and 17° S.

SOLAR OBSERVATIONS AT ROME.—In the September number of the *Memorie della Societa degli Spettroscopisti Italiani*, Prof. Tacchini contributes the results of the solar observations made at the Royal Observatory during the second and third trimestre of 1893. The same number also contains two large diagrams of the limb of the sun, the first showing the observations made at Catania, Palermo, and Rome, during the second three months of the year 1892, and the second indicating observations made at the last-mentioned place during June and July.

GEOGRAPHICAL NOTES.

MR. R. D. OLDHAM, Superintendent of the Geological Survey of India, read a paper at the last meeting of the Royal Geographical Society, on the evolution of the geography of India. He pointed out that the three main divisions of India were natural regions the individuality of which had been marked throughout a long range of geological time. The peninsula consists of very ancient land which has not been submerged since the early Palæozoic period, while the continental division has been frequently under water until Tertiary times, and the great plain is relatively recent alluvium. There is evidence from the close resemblance of fossil forms of a continuous land connection between India and Africa in the Cretaceous period. This former continent has been named Gondwana Land, and must not be confused with the hypothetical continent of Lemuria. It had disappeared by the end of the secondary period. At the close of the Cretaceous period there was an unparalleled outburst of volcanic activity contemporary with a series of great earth-movements which went far to give its present outline to peninsular India, and led to the first appearance of the extra-peninsular mountains. This activity continued during the Tertiary period. The depression at the base of the Himalaya, now filled up by alluvium, was simultaneously formed. The Indus was the original outlet of drainage from the Himalayan district, the river system splitting up later, and the diversion of the Jumna to the Ganges may even have occurred in historical times. The latter part of the paper gave an able summary of Indian types of scenery.

THE crossing of the eastern horn of Africa is fast becoming one of the commonplaces of travel, having been again accomplished this year by Prince E. Ruspoli, who, starting from Berbera in December last year, reached the Jub in March. The last number of the *Bulletin* of the Italian Geographical Society contains a letter giving an account of the journey and a sketch-map showing his route. Another Italian expedition, under Captains Bottego and Grixoni, made the journey by a somewhat different route about the same time.

THE *Verhandlungen* of the Berlin Geographical Society states that the Swedish traveller in Persia, Mr. Sven Hedin, has undertaken a serious attempt to reach Lhasa in the disguise of a Persian merchant. He will start from Leh, and follow the route of the Pundit Nain Singh to Tengri-Nor.

THE death is reported of Dr. D. Scott Moncrieff, of Harvard University, who had been making a journey of exploration, mainly with a view to ethnological observations, in Eastern Siberia. He left a Gilyak village near the mouth of the Amur for a sail in an open boat, on August 11, and nothing further was heard of him until a fortnight later his body was found on the coast of Sakhalin.

M. E. PONCINS, a French traveller, writes from Gilgit to the Paris Geographical Society under date August 26, that he has crossed the Pamirs, from north to south, and paid special attention to the source-region of the Oxus. He proposed to proceed to Simla, and there complete a full account of his journey.

THE source region of the Irawadi is still one of the most unknown parts of Asia, and it is satisfactory to learn that Captain Bower, whose recent journey in Tibet is well known, intends making explorations in that region during the present cold season.

THE meeting of the Paris Geographical Society on November 17 was devoted to the memory of the navigator Entrecasteaux, whose somewhat unfortunate voyage of discovery round the coast of Australia and amongst the islands of the Western Pacific was interrupted by his death in 1793.

UNVEILING OF THE JOULE MEMORIAL STATUE.

MANCHESTER claims the distinction of having been the home of two of our greatest men of science—Dalton and Joule—and it has shown itself worthy of the honour. A beautiful statue of Dalton has adorned the vestibule of the Town Hall for some years, and on Friday last, one of Joule, by Mr. A. Gilbert, was unveiled in the same place, the two philosophers standing face to face.

It was in 1889 that the Manchester Literary and Philosophical Society proposed to raise a memorial to Joule, and, to the credit of Manchester be it said, the suggestion was taken up with enthusiasm. On November 25 of that year, a meeting was convened by the Mayor of Manchester at that time (Mr. Alderman Mark), and was attended by a large and influential company. The following resolution was then adopted :—"That this meeting desires to mark its deep sense of the benefits conferred on mankind for all time, as well as of the great honour which has accrued to this district, by the scientific work of the late James Prescott Joule, by the erection of a durable memorial of him in Manchester, in the form of a white marble statue." A representative committee was appointed to raise subscriptions to carry the resolution into effect, and the sum of £2611 was eventually obtained.

Almost the first act of the committee was to pass a resolution to the effect that the movement should be directed to secure not only a marble statue of Dr. Joule as a companion to that of Dr Dalton, but also a replica in bronze to occupy some public place in the city. This object was kept in view for some time, but eventually it was thought advisable to relinquish the idea. After abandoning the scheme of raising a bronze replica of the statue, it was decided that the surplus should be handed over to the Literary and Philosophical Society as a nucleus for the institution of a permanent Joule Memorial Fund, the income of which is to be employed, as the council of the Society may direct, for the encouragement and promotion of science.

The unveiling of the Joule statue was performed by Lord Kelvin, in the presence of a large company. The *Manchester Guardian* gives a full report of the proceedings, and from it we extract the following remarks made by Lord Kelvin in the course of his address.

The Literary and Philosophical Society had the distinguished honour of being really the cradle of Joule's work—first as Dalton's home, and afterwards as Joule's life-long scientific harbour. From very early days he kept constantly in touch with that Society. Many of his most important papers were first given to the world there, and during the last years of his life he was an exceedingly regular—it might almost be said a constant—attendant at the meetings of the Society. The citizens of Manchester did not require to be told what great things this Society in its rather more than a century's existence had done. Their presence in such numbers on that occasion showed how much they appreciated the results of that very effective scientific institution. Now he ought to say something of the electrical, mechanical, and chemical character of Joule's work, although to examine it properly would require the space not of one address, but of a whole course of lectures illustrated by experiments. A great surprise that came out very early in Joule's work was burning without heat—an absolutely novel idea which Joule developed most wonderfully and most magnificently by his experiments in the generation of heat in the voltaic battery, which in those days was the only source of electricity on a large scale. Joule was the first to develop the idea, and it came to him not as a bright flash of genius, but as the demonstrated result of years of hard, measuring, calculating work. This was the fundamental idea that pervaded all Joule's work. A few years later he expanded it in a wonderful way. About 1846, in a joint paper by himself and Scoresby, he brought out the wonderful, the truly philosophical, and at the same time startling idea that when a man or any other animal walked uphill, only a part of the heat or combustion of his food was developed, and that it was when the body was quiescent that the chemical attraction between the food and the oxygen dissolved in the blood developed its whole energy in actual animal heat. He showed, further, that the animal body was more economical for fuel than was any steam engine hitherto realised. This was a very far-reaching idea, and seemed to hold out prospects of greatly advancing the efficiency of the steam engine. That promise had not been lost. It was due to Joule, more than to any other individual, that the great improvement of surface condensation was now universal, although very rarely practised, indeed, before 1860 or 1862. Joule, about the year 1860, in working upon a little steam engine, applied a surface condenser on an entirely new principle, and in doing so he was led to think out a mode of getting heat out of the steam to be condensed without sending a jet of water into it, as on the old plan. But he (Lord Kelvin) had not yet touched upon Joule's great fundamental discovery, the discovery which was first in everyone's mouth—that of the mechanical equivalent of heat. They would understand that it was not merely by a chance piece of measurement that he stumbled on this result, which was afterwards found to be of great value. It was measurement, rigorous experiment and observation, and philosophic thought all round the field of physical science that made this discovery possible. Very early indeed in his working time Joule brought out the mechanical equivalent of heat, and in a paper read at the British Association at Cork in 1843, and afterwards in the *Philosophical Magazine*, he gave the number "772." Six years later a second determination gave him the same result, and twenty-five years later he made a third determination, which gave him the final and corrected result "772·56." In the year 1824 a great theory was originated by a very young man, who died only a few years later—Said Carnot, son of the Republican Minister, and uncle of the present President of the French Republic. It was he who made "Carnot's theory" a household word throughout the world of science; and great as was the French President, much as he had done for his country and the world, in after times his uncle would be always remembered as one of the most distinguished characteristics attached to that great name. Carnot's theory gave an important fundamental principle regarding the development of motive power from heat. Joule's work, on the other hand, so far as the mechanical equivalent was concerned, was the generation of heat by mechanical work. It was quite the middle of the century before Carnot's work began to attract attention; but Joule was early made acquainted with it, and after fighting a little against it as differing from his own theory, he of all others took it up in the most hearty manner. Lord Kelvin went on to say that he could never forget the British Association at Oxford in the year 1847, when in one of the sections he heard a paper read by a very unassuming young man who betrayed no consciousness in his manner that he had a great idea to unfold. He (Lord Kelvin) was greatly struck with the paper. He at first thought it could not be true because it was different from Carnot's theory, and after the meeting he and the reader of the paper, James Joule, had a long and thoroughly discursive talk on the subject, and he obtained ideas he had never had before, although he thought he too suggested something worthy of Joule's consideration when he told him of Carnot's theory. He had the great pleasure and satisfaction for many years, beginning just forty years ago, of making experiments along with Joule, which led to some important results in respect to the theory of thermodynamics. This was one of the most valuable recollections of his life, and was indeed as valuable a recollection as he could conceive in the possession of any man interested in science. Joule's initial work was the very foundation of our knowledge of the steam engine and steam power. Taken along with Carnot's work it had given the scientific foundation on which all the great improvements since the year 1850 have been worked out, not in a haphazard way, but on a careful philosophical basis. James Watt had anticipated to some degree in his compound engine and his expansive system the benefits now realised, but he was before his time in that respect, and had the complete foundation which Joule's mechanical equivalent and Carnot's theory had since given for the improvement of the steam engine. Might he be allowed, Lord Kelvin added, to congratulate the city of Manchester on its proceedings that day? When the cover was lifted from the statue of Joule, he felt deeply touched at the sight of the face of his old friend. To his mind it was a most admirable likeness, and the ideality of the accessory of the little model held in the hand seemed to him most interesting and most striking—he thought he might say poetical. That little model was not one of Joule's first or second, but of his third and greatest apparatus for the determination of the mechanical equivalent of heat—that by which he corrected the British Association's standard ohm, which he proved to be 1·7 per cent. wrong. Regarding that standard a diplomatic correspondence was now going on between our Foreign Office and other European Governments with a view to arranging the precise terms of the definition of the ohm, which was really first worked out by Joule. Lord Kelvin further asked to be allowed to congratulate the sculptor on the great beauty and the great success of his work, and added that Manchester now possessed statues both of the man who laid the foundation of the atomic theory in chemistry, and of the man who was the originator of the whole subject of thermodynamics. If the prosperity of Manchester did not depend on chemistry and on the steam engine and thermodynamics, he did not know upon what it did depend. The energy and industry of its inhabitants were no doubt essential to its success, but they must ever remember that the material prosperity of the city was as much dependent on philosophic thought as it was upon any material appliance whatever.

Sir H. E. Roscoe, M.P., in moving a vote of thanks to Lord

Kelvin for his address, mentioned that for thirty years he himself sat at the feet of Joule, whom he might therefore claim in some sense as his scientific father. He remained in constant communication with Joule up to the day of the philosopher's death. It was a great thing that in a city like Manchester, devoted as it was to industry and commerce, the citizens should be entitled to place in their Town Hall the statues of two such fellow-citizens as Dalton and Joule. Few cities in the world could boast of two greater men. In London, also, they had been doing something to show the appreciation in which not only this country but the world held Joule. A sum of money had been raised and placed in the hands of the Royal Society for the purpose of founding a Joule studentship, and on Thursday the Council of the Society resolved that the money should be spent in founding such a scholarship, to be awarded alternately in England and in other countries, for the purpose of encouraging young scientific men to walk in the steps of Manchester's great citizen. The first of those scholarships, of the value of £100, would be shortly awarded, and he thought he was not going too far when he said it would come to the city in which Joule lived and worked. He might mention that since the foundation of Owens College that institution could claim nine medallists of the Royal Society, and had they lived Prof. Jevons and Prof. Schorlemmer would have been added to the number. It would be seen, therefore, that Manchester had taken up the thread spun by Dalton and Joule, and that there was no reason to fear that their work would not be continued.

Prof. Osborne Reynolds seconded the resolution, which was adopted.

On the motion of Mr. Alderman Mark, seconded by Principal Ward, a vote of thanks was given to the Lord Mayor for presiding. At the conclusion of the proceedings the Lord Mayor and Lady Mayoress held a reception in the state apartments.

THE ETHNOLOGICAL MUSEUM AT LEYDEN.

IT is from twenty-five to thirty years ago that the interest in ethnology as a science was awakened. Ethnological objects are no longer considered by scientific men as mere curiosities; collections of them have ceased to be shops of foreign bric-a-brac. In America and in Europe, museums have arisen, variable in size and importance it is true, but all with the same object in view, viz. the study of man from his handiwork as illustrative of his mental development in various directions, in time as well as in space.

The realisation of the importance of collecting and studying ethnological material in distant lands has not come too soon. What has been done in this respect within the last twenty-five years surpasses all that has been performed in the centuries before, from the beginning of the circumnavigation of the globe to the early seventies of our century. The result of this has not only been the publication of valuable ethnological monographs and studies, but also the foundation of new, and the further development of existing, museums in Europe and America. Amongst the ethnological collections which have grown considerably are those of the National Ethnological Museum at Leyden. A pamphlet by the director, Dr. L. Serrurier, recently published under the very suggestive title of "Museum or Storehouse?" induces me to write the present notice.

The origin of this museum dates back as far as 1837, the year in which the Dutch Government purchased von Siebold's Japanese collections. This formed the nucleus of the little museum, which gradually increased by means of collections made in the Indian Archipelago by Macklot, Salomon Müller, von Rosenberg, and other naturalists. In the meanwhile von Siebold had returned to Japan, and the late Dr. Leemans was appointed acting director of the collection in 1859, as a part of the National Museum of Antiquities. A long period of inactivity then ensued until 1880, when it was decided that the ethnological department should become a separate museum, under the direction of Dr. Serrurier, until then curator of the Japanese department.

From that moment the museum sprang into new life. The period of curiosity shop had ceased for ever; and the institution was developed in the right direction. Many objects were properly identified, check lists and manuscript catalogues corrected and newly made,[1] relations with sister institutions established, exchanges made, and valuable objects and collections bought. Many residents in the Dutch colonies who hitherto had hardly heard of the existence of an ethnological museum at Leyden, now gladly presented or loaned their private collections to it. Nothing shows more clearly the extraordinary growth of these collections than the maps accompanying the pamphlet of Dr. Serrurier. On these maps—two of the world, and two of the Indian Archipelago—is indicated the number of groups of objects in the Leyden Museums representing special ethnological regions or areas. These groups refer to a very rational division or classification established by Dr. Serrurier and used in the Leyden Museum. One set of maps shows the condition of the collections in 1881, the other in 1893. A single glance at the maps is sufficient to illustrate the vast difference wrought in those twelve years. Southern Asia, the Orient, Africa, and North America were hardly or not at all represented in 1881, and so were many parts of the Indian Archipelago and South America, but in 1893 the ethnology of these countries can be studied in the museum by means of valuable and more or less representative collections. In short the collection has decupled in these twelve years, not to speak of the rich and varied anthropological section, entirely the work of the present director.

It would be only natural to suppose that collections of this importance were exhibited in a special building where they were not only safely stored but also made of interest to the public, as well as for professional men. Nothing is, however, farther from the reality than this supposition. To the amazement of foreign ethnologists, and every friend of science in fact, the Leyden collections are scattered, as Dr. Serrurier summarises, over not less than five different places—ugly, dark, and damp private houses, or other localities, all equally unfit for the conservation of the ethnological material. Thousands of precious objects are stowed away as in a storehouse, where moths and moisture are hard to fight against, and where the danger of fire is so much greater than elsewhere. Should ever a fire destroy these collections, the loss would, for the greater part of them, be irreparable; they could not be again collected. In many distant countries, all over the world, the inhabitants have given up their native industries, and are losing rapidly their originality in every respect, the result of their contact with Western civilisation.

An appendix to Dr. Serrurier's pamphlet, being a number of testimonies from different travellers and authors as to the disappearance of primitive conditions among foreign races, tends to prove the value of present ethnological collections and the necessity of collecting objects and data without delay, before it will be too late.

Irrespective, however, of the present buildings the museum collections are not quite what they ought to be, which perhaps is partly due to the fact that they are situated in a small old-fashioned city with a public—University students included—taking very little or no interest in ethnological exhibitions. If the museum were situated in a great city, say Amsterdam or The Hague, things might probably take a turn for the better, but still, as long as there is no special building, a thorough improvement will be impossible.

There are many things which the Ethnological Museum of Leyden ought to be, and should be, if proper attention was paid to it by the Dutch government. For a nation like the Dutch, which ranks third as a colonial power, a museum like this could be a sort of a bureau of ethnology, more or less similar to that of Washington, and a place where both University students and the general public could be taught sounder ideas about races of mankind which they have been used to consider as "savages."

For years past the director has called the attention of the Dutch government to this state of things. In each of his annual reports Dr. Serrurier has pleaded for the sake of a new and proper building with the ardour and conviction of a man who pleads for the thing to which he devotes his life and talents, but all in vain; *vox clamantis in deserto.*

The present pamphlet of Dr. Serrurier's is a supreme effort to improve this sad state of affairs, a last appeal to the national

[1] A system of cataloguing introduced not long ago by Dr. Serrurier in the Leyden Museum, as far as I know not followed in any other ethnological museum, is what might be called the "Note Catalogue." Each object has an inventory number referring to a separate note on a slip of stiff paper, which contains, besides a small photograph of the object in question, a full description of it, and bibliographic indications relating to its origin, occurrence, use, &c. This system facilitates greatly the study of the objects.

representatives, the members of the First and Second Chambers. The granting of a considerable sum of money for the building of a new National Museum of Natural History at Leyden, a necessity long felt and perseveringly advised by its director —Dr. Jentink—furnished an occasion to bring the question once more before the public. What the result will be—museum or storehouse?—we cannot tell. If a man is not convinced after reading Dr. Serrurier's pamphlet, he will never become convinced.

"But whatever may be done"—Dr. Serrurier concludes his interesting paper—"every change, in this case, will be an improvement, for now the life of the museum is ebbing away. The time is near that it will sink into a lethargic sleep, the end of which will be death." H. TEN KATE.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—Mr. T. I. Pocock, of Corpus, has recently been elected to the Burdett-Coutts Geological Scholarship. It is his intention, we believe, to devote himself ultimately to the science of astronomy, which he studied at Oxford under the late Prof. Pritchard.

Mr. E. A. Minchin has been elected to a Biological Fellowship at Merton College, and Mr. H. M. Vernon, of that college, has been elected to the Oxford Biological Scholarship at Naples.

CAMBRIDGE.—The council of the Royal Geographical Society offer in the present academical year a studentship of £100, to be used in the geographical investigation (physical or historical) of some district approved by the council. Candidates must be members of the University of not more than eight years' standing from matriculation, who shall have attended the courses given in Cambridge by the late or present University lecturer in geography.

The following awards in Natural Science were made at St. John's College on December 11:—K. C. Browning (Dulwich College), Foundation Scholarship of £80; E. R. Clarke (Tonbridge School), Foundation Scholarship of £50; O. F. Diver (Winchester College) and K. B. Williamson (St. Paul's School), Minor Scholarships of £50; A. A. Robb (Queen's College, Belfort), R. F. C. Ward (Epsom College), J. A. Glover (St. Paul's School), and G. D. M'Cormick (Exeter Grammar School), to various Exhibitions of £50 and under.

SOCIETIES AND ACADEMIES.

LONDON.

Linnean Society, November 16.—Prof. Stewart, President, in the chair.—Mr. J. H. Veitch exhibited a large and interesting collection of economic and other vegetable products of Japan, recently brought by him from that country, and described the various uses to which different kinds of wood, fibre, grass, &c., were applied for domestic purposes, as also the way in which various seaweeds were collected and prepared for food. —Mr. A. G. Renshaw exhibited a remarkably large specimen of the giant puffball, *Lycoperdon giganteum*, which he had gathered at Catford Bridge.—On behalf of the Rev. Prebendary Gordon, the secretary exhibited a plant of *Veronica salicifolia* of New Zealand, found growing in Langland's Bay, Mumbles, Swansea, having been introduced by some chance.—A paper was then read by the Rev. G. Henslow, on the origin of plant structures by self-adaptation to the environment, exemplified by desert and xerophilous plants. The purport of this paper was to prove by a direct appeal to facts the probably universal application of Mr. Darwin's assertions, viz.: (1) that natural selection has no relation whatever to the primary cause of any modification of structure ("Animals and Plants, &c." vol. ii. p. 272); (2) that modifications of structure are due to the direct action of the environment (*fide* Darwin, Weismann, Spencer, &c.). This always results in "definite variations," by which Mr. Darwin signifies (3) that all, or nearly all, the individuals became modified in the same way ("Origin of Species," 6th ed., p. 106), and consequently (4) that "a new variety would be produced without the aid of natural selection" ("Animals and Plants," ii. 271, "Origin of Species," pp. 72, 175). Mr. Henslow showed (1) that all the species constituting the peculiar *facies* of a desert flora are the direct result of their climatic conditions; (2) that these peculiarities are in nearly all cases of the utmost benefit to the plants, such as the hardening of the tissues, the reduction of parenchyma, the minute size of the leaves, the dense clothing of hair, a thick cuticle, the presence of wax, storage of water tissues, &c. But (3) these features are just those which systematists utilise as descriptive characters of varieties and species. Mr. Henslow observed that by Darwin's assuming that "indefinite variations" which are characteristic of *cultivation* were equally so in *nature*, he reasonably required natural selection to correspond with artificial selection; but that assumption he believed to be erroneous. For experiments proved that by sowing seeds in a very different medium, *all* the seedlings vary in the same direction, viz. that of adaptation to the new environment, verifying Mr. Herbert Spencer's statement that "under new conditions the organism immediately begins to undergo certain changes in structure, fitting it for its new conditions." The conclusion is thus arrived at which is expressed in the title of this paper. The functions of natural selection therefore become limited, as follows: (1) The survival of the constitutionally strongest amongst seedlings; (2) delimitation of species by the non-reproduction of intermediate forms; (3) the geographical distribution of plants by self-adaptation. An interesting discussion followed, in which Prof. Reynolds Green, the Rev. Dr. Klein, Mr. Perry Coste, and others took part.

Zoological Society, November 21.—Sir W. H. Flower, K.C.B., F.R.S., President, in the chair.—The secretary read a report on the additions that had been made to the Society's menagerie during the month of October, 1893, and called special attention to an example of Goliath beetle (*Goliathus druryi*), the largest of known Coleoptera, obtained near Accra, and presented October 5, by Mr. F. W. Marshal, and to an adult female and a young of the Manatee (*Manatus americanus*), captured in Manatee Bay, Jamaica, and most kindly sent home for the Society's collection by Sir Henry A. Blake. Unfortunately the Manatees had reached the gardens in a very exhausted condition, and died soon after their arrival. —The secretary read an extract from a letter addressed to him by Mr. J. S. Mackay, of the Kangra District, Punjaub, relating to a young snow-leopard which he had in captivity, and exhibited some photographs of this animal.—Mr. Sclater exhibited and made remarks on a mounted specimen of an African monkey (*Cercopithecus albogularis*) belonging to the Leyden Museum.—Mr. W. B. Tegetmeier exhibited and made remarks on two hybrid pheasants, believed to be crosses between the common pheasant and the gold and silver pheasants.—A communication was read from Messrs. G. W. and E. C. Peckham, on the spiders of the family *Attidæ* of the island of St. Vincent, based on specimens collected in that island by the agency of the joint committee of the Royal Society and the British Association for the exploration of the Lesser Antilles. The series had been collected by Mr. Herbert H. Smith and Mrs. Smith, who had been specially sent to the island as skilled collectors by Mr. F. D. Godman, F.R.S.—A communication was read from Mr. P. R. Uhler, containing a list of the Hemiptera Heteroptera collected in the island of St. Vincent by Mr. and Mrs. Herbert H. Smith, with descriptions of new genera and species.—Dr. G. Lindsay Johnson made some observations on the refraction and vision of the eye of the common seal (*Phoca vitulina*).—Mr. Sclater read a paper on some specimens of mammals from Lake Mweru, British Central Africa, transmitted by Vice-Consul Alfred Sharpe, through Mr. H. H. Johnston, C.B. The specimens were referred to seventeen species, amongst which was a new monkey of the genus *Cercopithecus*, proposed to be called *C. opisthostictus*, and a new antelope allied to the waterbuck, which was named *Cobus crawshayi*, after Mr. R. Crawshay, who had first discovered the species.

CAMBRIDGE.

Philosophical Society, November 27.—Prof. T. McK. Hughes, President, in the chair.—The following communications were made.—The action of light on bacteria, by Dr. H. Marshall Ward. By throwing the spectrum on various bacteria suspended in films of agar, it is possible to obtain photographic records of the action of the various rays; because, after incubation, those spores or bacilli, &c. which are killed by certain rays remain invisible, whereas those still left capable of development render the agar opaque. The experiments show that those germs which are struck by the infra-red, red, orange and yellow, develop as rapidly as those not exposed to light at all. The action begins as we leave the green and rises to a maximum in the blue-violet and violet, falling off as we pass into the ultra-violet. For the solar spectrum a heliostat and glass lenses and prism were used; for the electric spectrum a quartz train, and quartz covering the film. Even a thin plate of clear glass blocks out much of the effective region of the spectrum, and especially ultra-violet rays. The author recorded his thanks to Prof. O. Lodge, F.R.S., for kindly exposing the plates to the electric spectrum for him, with most successful results. The author also found that the water of the Thames, examined in August and October respectively, shows the following interesting results. (1) The number of bacteria per c.c. was distinctly smaller in the bright August weather than in the duller days of October, and differences were observable in the aspect of the colonies on plate-cultures. (2) Suspecting that this was concerned with light action, experiments showed that insolation not only kills off large numbers of the bacteria in the water, but in some cases shows its effects in diminishing the liquefying (*i.e.* enzyme action) power of certain forms, and even in altering their mode of growth, so that the aspect of the colonies is affected. These changes in aspect are not a mere matter of preventing or increasing the production of pigment, &c., but are due to effects on the *growth* of the colonies. As regards pigments, the author has examined a pigment of one of these river species, which, though so resistent as to bear solution in alcohol, evaporation and drying at 100° C., and re-solution, without apparent alteration, is destroyed in an hour or two on exposure to light.—On gynodioecism (third paper), with a preliminary note upon the origin of this and similar phenomena, by Mr. J. C. Willis. The experiments described in the two former papers have been continued, and have still further shown the very variable nature of the phenomena of gynodioecism. From the results, taken together with those of experiments upon dichogamy and cleistogamy, and the observations of other writers upon these subjects, the author is inclined to the conclusion that all these phenomena, together probably with androdioecism, andromonoecism, polygamy, dioecism, &c., are closely allied to one another, depending largely upon varying conditions of nutrition, these again depending on numerous factors, such as soil, climate, temperature, light, season of the year, moisture, and internal conditions in the plant itself. These factors acting together or separately, may call forth, in a marked degree, any of the above phenomena. In the most marked cases natural selection appears to have come into play, and the phenomena have become hereditary, but in many cases they seem to be only sporadic. A full review of the literature relating to these subjects is in preparation.

PARIS.

Academy of Sciences, December 4.—M. de Lacaze-Duthiers in the chair.—Significance of the localisation of organs in the measurement of the gradation of plants, by M. Ad. Chatin.—Estimation of manganese oxides by oxygenated water, by Harry C. Jones.—On the profound deformations of the spheroid of Mars, by Dom Lamey.—On the observations made by M. J. Vallot on the summit of Mont Blanc in 1887, by M. Alfred Angot. From the barometric pressures and temperatures observed by M. Vallot on the top of Mont Blanc, and simultaneous observations made on the Säntis, Obir, and Puy-de-Dôme, and at Berne, Geneva, and Lyon, the height of the former may calculated. The values thus obtained range from 4810·4 m. to 4824 m. The true height is 4810 m., so that an interesting confirmation of Laplace's law is obtained. The temperature at the limit of the atmosphere has been calculated by Voeïkoff according to Mendelejeff's formula, which makes the temperature of the air a linear function of the pressure. The result obtained from numerous mountain observations is −42°. A combination of the Mont Blanc observations with those of the three mountain stations mentioned gives −47°, and with the three town stations −45°. Since these values are obtained from summer observations only, they are probably a little too low.—On the complex acids which are formed by molybdic acid with titanic acid and zirconia, by M. E. Péchard.—Researches on the constitution of the albuminoid materials extracted from the vegetable organism, by M. E. Fleurent.—On the stability and the conservation of dilute solutions of corrosive sublimate, by M. Léo Vignon. If a solution of sublimate of 1 grain to 1 litre of distilled water is left to itself for a few days at the ordinary temperature, it gives rise, in a period varying from one to three days, to a white precipitate whose quantity gradually increases. Quantitative measurements of the amounts of mercury thus precipitated under varying conditions gave the following results. When the solution was left in an open vessel, the percentage of mercury left in solution after seven days was 0·57, in a closed vessel 0·97, and 0·67 after 220 days. When colouring matters were added to the solutions, the corresponding numbers were, for fuchsine, 0·67, 0·97, and 0·77; and for indigo carmine, 0·76, 0·98, and 0·80, the latter therefore giving the greatest stability.—Discovery of abrastol in wines, by M. Sangle-Ferrière. This gives a method of finding whether abrastol, a new antiseptic for the preservation of wine, has been used in a given sample. It is the sulphuric ether of β-naphthol combined with calcium. The method utilises the decomposition ensuing when abrastol is heated with dilute H Cl, calcium carbonate, sulphuric acid, and β-naphthol being formed.—On the sterilisation of bread and biscuit on coming from the oven, by MM. Balland and Masson. This gives an answer to the question whether all dangerous germs which may have been contained in the water used for breadmaking, are destroyed during the process of baking. Experiments show that microbes in general are incapable of resisting the acidity of the dough and the high temperature of baking. Certain spores notorious for their stability are indeed capable of regaining their activity under favourable circumstances, but all pathogenic bacilli, especially those of typhoid and of cholera, are certainly destroyed.—Some chronometric data relating to the regeneration of nerves, by M. C. Vanlair.—On the termination of the motor nerves of striated muscles in the Batrachians, by M. Charles Rouget.—On some points relating to circulation and excretion among the cirrhipeds, by M. Gruvel.—On phosphaturic albuminuria, by M. Albert Robin. The constitution of the group of phosphaturic albuminurias, shows that the morbid entity known as Bright's disease, is often nothing but the anatomical complication of an anterior purely functional malady, and that, like a number of similar cases, the cure should begin at this anterior functional disorder.—Parasites in cancer, by M. G. Nepveu.—The shell cavity of the Philinidæ, by M. P. Pelseneer.—On a new gregarine of the Algerian Acridians, by M. L. Léger.—On the exchanges of carbonic acid, and oxygen between plants and the atmosphere, by M. Th. Schlœsing fils.—Observations on the constitution of the membrane in mushrooms, by M. L. Mangin.—On the primary strata of Saint-Pons (Hérault), by MM. P. de Rouville, Aug. Delage, and J. Miquel.—On the Triassic and Jurassic formations of the Balearian Isles, by M. H. Nolan.

BERLIN.

Physical Society, November 3.—Prof. du Bois Reymond, President, in the chair.—Dr. Rubens discussed the experiments of Righi, who had succeeded in obtaining Hertz's oscillations of much smaller wave-length than had hitherto been found possible. Whereas the shortest waves obtained by Hertz were 55 c.m. long, and those by Döppler 20 c.m., Righi had produced waves only 7·5 c.m. in length, and had repeated all Hertz's experiments in a much more convenient form. Rubens had somewhat modified Righi's experimental arrangements, and produced waves 10 c.m. long, which he intended to submit to further investigation.

November 17.—Dr. O. Fröhlich explained a generalised form of Wheatstone bridge, and a series of applications of the same for theoretical and technical purposes. Dr. Blüme demonstrated a form of apparatus for showing refraction suitable for use in schools, giving accurate results to the third place of decimals with very little practice.

Physiological Society, November 10.—Prof. du Bois Reymond, President, in the chair.—Dr. Gumlich gave an account of feeding experiments made on dogs with nucleic acid, which showed an absorption of this substance by the animal organism. The phosphates of the urine were increased, as also the nitrogen of its extractives. There was no increase of uric acid.—Dr. Goldscheider made further communications on leucocytosis. His experiments, carried on in conjunction with Dr. Jacob, had shown that after the injection of hemialbumose, or extract of spleen, and other substances with similar action, there is a diminution (hypocytosis) in the number of leucocytes, followed by a rapid rise in their number up to the normal and then to a permanent increase above the normal (hypercytosis). When the active substance was injected into the jugular, it was found that during the brief period of hypocytosis the capillaries of the lungs were abnormally filled with leucocytes. Later on, there was a still further increase in this region at the time of increase of leucocytes in the blood generally. By using smaller doses of the active substance the stage of hypocytosis could be lessened or even entirely suppressed, leaving only the stage of hypercytosis.

November 24.—Dr. Katzenstein gave an account of experiments on the median pharyngeal nerve. In the rabbit this nerve gives off branches to the cricothyroid muscle, whereas in the monkey, dog and cat no such connection can be made out, either anatomically or physiologically. Prof. Munk made some remarks, in connection with these experiments, on Prof. Exner's belief in the existence of a median pharyngeal nerve, which could at most only be admitted in the case of the rabbit. Prof. Zuntz described a new method of measuring the amount of the circulating blood and the work done by the heart. It depends on the fact that as long as the peripheral resistance is constant, blood-pressure is dependent on the volume of blood driven into the aorta by the left ventricle. When the heart is inhibited by stimulation of the vagus the blood-pressure falls, and if now a volume of blood is injected into the aorta sufficient to raise the pressure again to the normal, then this volume must be equal to that which the heart ordinarily drives into the arterial system. The method had shown itself to be reliable in experiments made on dogs, and had already yielded some interesting results relating to the circulation, which will be further investigated.

NEW SOUTH WALES.

Linnean Society, October 25.—Prof. David, President, in the chair.—The following papers were read:—On Polycercus: a proliferating Cysticercoid parasitic in certain earthworms, by Prof. Haswell and J. P. Hill.—Some points in the anatomy of the monotreme scapula, by Prof. Wilson and W. J. Stewart McKay.—Notes on the family Brachyscelidæ, with descriptions of new species, Part III., by W. W. Froggatt.—On some new genera of nematode worms found in Port Jackson, by Dr. N. A. Cobb.—On recently observed evidences of extensive glacier action at Mount Kosciusko Plateau, by R. Helms.—Contributions to a revision of the Tasmanian land mollusca, by H. Suter.—Notes on the occurrence of a species of Plecotrema and other species of mollusca in Port Jackson, by Dr. J. C. Cox.—On the distribution of little-known mollusca from Polynesia and Australia, with their synonyms, by John Brazier.—Dr. Cox exhibited a fine specimen of the herring *Elops saurus*, Linn., purchased in a Sydney fishmonger's shop, and believed to have been captured off Broken Bay; the species is occasionally taken in Port Jackson, though it is more properly an inhabitant of tropical seas. He also showed a piece of timber in an excellent state of preservation supposed to be red gum, a portion of a tree encountered in sinking a shaft in the bed of the river during the building of the bridge at Echuca; the specimen was forwarded to him by Mr. A. P. Stewart, of Hay, N.S.W. Dr. Cox also showed specimens of the shells referred to in his paper, and a very fine example of *Voluta manilla* from Tasmania.—Mr. Froggatt exhibited a fine series of mounted galls and coccids in illustration of his paper, including a new Brachyscelid collected by Mr. A. Roxburgh at Cobar, and representatives of several new species of Opisthoscelis.—Mr. North exhibited a set of eggs consisting of three eggs of *Collyriocincla harmonica* and an egg of *Cacomantis pallida* collected on the Woolli Creek on the 19th inst. The cuckoo's egg was deposited on the 17th inst., when the nest contained but two eggs of the Collyriocincla. This is the only occasion he had known the egg of any cuckoo to be found in the nest of the Harmonious Thrush. Mr. North also communicated a note in which he pointed out that the blue wren (*Malurus cyaneus*) is developing a protective habit against the cuckoos which intrude their eggs upon it, as he had found in several instances that the intruder's eggs were covered with a layer of nest material; a parallel instance has been recorded by Messrs. Sclater and Hudson in their "Argentine Ornithology."—Mr. Mitchell, of Narellan, contributed a note on the occurrence of a fossil at Stockyard Mountain, Jamberoo, bearing a strong resemblance to Lepidostrobus and Halonia; and of certain scales at Glenlee, referable, in his opinion, to one or other of the genera Lepidostrobus or Sigillariostrobus; also of a species of Pterophyllum, at Kenny Hill, near Campbelltown.—Mr. A. M. Lea showed a small collection of insects which inhabit ant and termite nests, including a dipterous insect (*Microdon variegata*), one of the Micro-lepidoptera at present undetermined, both from Sydney; and of coleoptera, two species of Pselaphidæ from Tamworth and Inverell, Anthrenus sp., from Sydney, Lagria n.sp., from Cootamundra and Queanbeyan, and a fifth species (g. et sp. indet.).—Mr. Brazier exhibited for Mr. T. Steel three aboriginal stone axes, one with a groove for hafting, from the Herbert River, said to have been found at a depth of thirty feet in sinking a well; a second from the Tweed River, being a simple adaptation of a flat water-worn stone by grinding the thinner end; the third from Harrow, Victoria.—Mr. Fletcher exhibited for Mr. G. L. Pilcher, of Rockhampton, an undescribed longicorn, and two of the mud nests of one of the solitary wasps (*Eumenes Latreillei*, Sauss.), together with specimens of the wasp and of a species of *Chrysis* which, like members of the same family elsewhere, plays the part of cuckoo; and he communicated a note giving particulars of the mode of construction of the nests exhibited, and of the habits of the maker and of the attendant intruder.

BOOKS and SERIALS RECEIVED.

BOOKS.—Elementary Trigonometry: H. S. Hall and S. R. Knight (Macmillan).—A Theory of Development and Heredity: H. B. Orr (Macmillan).—Natural Value: F. von Wieser, translated (Macmillan).—The Vault of Heaven: R. A. Gregory (Methuen).—A Journey through the Yemen: W. B. Harris (Blackwood).—Chinese Central Asia, 2 Vols.: Dr. H. Lansdell (Low).—The Dispersal of Shells: H. W. Kew (K. Paul).—A Text-book of Physiological Chemistry: Prof. O. J. Hammarsten, translated by J. A. Mandel (K. Paul).—Die Hawaiischen Inseln: Dr. A. Marcuse (Berlin, Friedländer).—Fra I Batacchi Indipendenti: E. Modigliani (Roma, Soc. Geog. Italiani).—A Text-book on Electro-Magnetism and the Construction of Dynamos, Vol. 1: Prof. D. C. Jackson (Macmillan)—Mining: A. Lupton (Longmans).—Anwendung der Quaternionen auf die Geometrie: Dr. P. Molenbroek (Leiden, Brill).—Studies from the Physical and Chemical Laboratories of the Owens College, Vol. 1, Physics and Physical Chemistry (Manchester).—Schneekrystalle: Dr. G. Hellmann (Berlin, P. Mückenberger).—Darwinianism: Dr. J. H. Stirling (Edinburgh, T. and T. Clark).—A Catalogue of the Egyptian Collection in the Fitzwilliam Museum, Cambridge: Dr. E. A. W. Budge (Cambridge University Press).

SERIALS.—American Meteorological Journal, November (Boston, Ginn).—Bulletin de l'Académie Royale des Sciences de Belgique, 3rd Series, Tome 26, Nos. 9 and 10 (Bruxelles).—Observatory, December (Taylor and Francis).—Companion to ditto, 1894 (Taylor and Francis).—Mémoires de la Société d'Anthropologie de Paris, Tome 1, 3rd série, 1 Fasc (Paris, Masson).—L'Anthropologie, Tome 4, No. 4 (Paris, Masson).—Himmel und Erde, December (Berlin, Paetel).—Engineering Magazine, December (New York). L'Elettricista, December (Roma).—Medical Magazine December (Southwood).—Illustrated Archæologist, December (C. J. Clark).—Insect Life, Vol. vi. No. 1 (Washington).—Zoe, Vol. iv. No. 3 (San Francisco).—Bulletin de la Société Impériale des Naturalistes de Moscou, 1893, Nos. 2 and 3 (Moscou).—Bulletin du Comité International Permanent pour l'execution photographique de la Carte du Ciel, Tome 2, Deux Fasc (Paris, Gauthier-Villars)

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