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SOME UNRECOGNISED LAWS OF NATURE.

Some Unrecognised Laws of Nature. An Inquiry into the Causes of Physical Phenomena, with Special Reference to Gravitation. By Ignatius Singer and Lewis H. Berens. With illustrations. Pp. xvi + 483. (London: John Murray, 1897.)

"AT last, after years of patient plodding in dim regions, where the footprints are few and the pitfalls many, the time has arrived when we are enabled to place before the world of science the first-fruits of our explorations."

So say the authors, Messrs. Ignatius Singer and Lewis H. Berens, at the opening of the preface to a handsome volume of nearly 500 octavo pages, published under the above title by Mr. John Murray, and printed in good style by the printer to the University of Oxford.[1] A subordinate title tells us that it is "an inquiry into the causes of physical phenomena, with special reference to gravitation," and a cursory inspection shows that it is decorated with quotations from great writers and philosophers at the head of each chapter, and that it is altogether a most ambitious work, courting the widest and closest attention.

The present reviewer is not one to regard lightly the danger of summarily rejecting a germ of new discovery because it happens to conflict with orthodox opinions, nor has he failed to have borne in upon him the conviction that there are indeed many real laws of nature at present unrecognised, some unsuspected, others scouted, by contemporary science.

It is, therefore, in no spirit of levity that he finds it his duty to state not only that the present volume contains much that is plainly and simply trash, he has to state further that, so far as he has been able to judge, there is nothing in it of the slightest value from cover to cover.

The former of these statements it is easy to justify by quotation; the latter may very likely be considered by the authors as an unfounded individual opinion typical of the arrogance of every professed physicist with regard to any novel view of the universe which may be brought to his notice. It is, however, difficult to imagine what manner of men these authors can be, nor how it happens that writers so conspicuously ignorant of the elements of physics, beyond what they have culled from a superficial acquaintance with a few popular books, should yet think fit to overflow in a wordy and pretentious volume, controverting many of the best-established scientific truths, criticising lightly the acknowledged masters of science, and setting forth a quantity of false philosophy and misapprehended facts with a flourish as if a new Principia were being given to the world. Who, again, are the readers of such a book? It has been most respectfully received in some quarters. Can it be that the state of school education in England is such that any considerable number of men of letters or of philosophy, or any other branch of study, find themselves able to accept such a book as this as containing possibly as good and sound science as, let us say, Thomson and Tait? Neither work is by them really read; perhaps both works seem to them nearly on a par. Perhaps they think that since Thomson and Tait and Helmholtz and Newton are all adversely criticised in the work before us, it is very likely true that the pretended facts of our boasted science are all uncertain fictions, and that the authorised scientific version of to-day is little better than a mass of hypothetical dogma.

If any such suspicion is in the slightest degree possible among the members of so-called educated society, it means that we are, after all, not far away from the dark ages; the death of a comparatively few individuals would perhaps loosen the grip of the human race on the truths of science, and plunge us once more into pre-Galilean ignorance. It is to be wondered if there are not more than a few who would welcome the change.

If the book is really an isolated phenomenon, it is as insignificant as the customary production of those paradoxers who were so delightfully gibbeted years ago by De Morgan; but it has not the usual air of the work of the paradoxer: the first half, at any rate, may easily impose upon the unwary as being a serious and sober attempt to solve perplexing problems, and contribute to the progress of natural knowledge. Sober and serious in intention we doubt not that it is, we do not suppose that the authors are playing off a conscious and expensive hoax; but, without the slightest desire to be abusive, we must maintain our view that a very small ingredient of common modesty would have prevented their shouting out their ignorance to the world—would have withheld them, with their little pittance of popular knowledge, from "undertaking to write the whole body of physicks," as Montaigne has it.

However, it is only fair now that we give some idea of the scope of the book, and that we make from it some illustrative extracts, so as to justify the terms of strictly measured depreciation which we have been constrained to use in our attempt to estimate it truly.

The four great fundamental and universal laws of nature, according to Mr. Singer (we are told in a note at the end of the preface that the science and philosophy of the work are exclusively due to Mr. Singer), are Persistence, Resistance, Reciprocity, and Equalisation.

The law of persistence is thus worded: "All matter tends to persevere in whatever state it may happen to be, and to resist change"; merely a simple and vague parody of part of the first law of motion, but it is intended to be much more comprehensive, and to include among other things such a statement as the following: "All organisms have a tendency to preserve their structure, organs, functions, habits, and dispositions unchanged."

As to "Resistance," we are told that it is the same thing as Persistence, and constitutes its quantitative measure; so the law of resistance, if it were to be stated definitely, would become a part of Newton's second law. The authors, however, rather avoid definite statements, and prefer nouns in capitals or italics, as if such nouns were laws of nature. By "Reciprocity" seems to be meant

[1] We do not hereby intend to imply that the University of Oxford is responsible for, or cognisant of, the production; but we do think that some care should be taken not to let its imprimatur be even apparently affixed to such a book as the one before us.

something akin to the third law of motion, though that is not in the least understood by them; and by "Equalisation" is intended something like the second law of thermodynamics, viz. that unless a difference of state exists between two bodies, there is no activity or tendency to activity.

Thus it may be admitted that the customary teachings of science have not been wholly thrown away upon Messrs. Singer and Berens; they have acquired some rudimentary notions from their perusal of popular expositions of science, but the amount of vagueness and inaccuracy with which they have contrived to shroud those notions is almost incredible except to those who take the trouble to read their book.

In the first place, they are careful to confuse their terminology, as the following explanatory notes testify:—

"We use the terms force and energy synonymously, and in the sense of power or strength, and shall use either of these indiscriminately as the one or the other may happen to be most convenient (p. 49).

"To us 'heat' means simply temperature." Work can be got from a body by either heating or cooling it, and it is the degree of difference of temperature that is important in connection with the mechanical equivalent of heat, not heat *per se* (pp. 86, 88).

"Pressure is 'work,' and so is motion" (p. 106).

"'Force' and 'mass' (or 'weight' and 'mass') are synonymous terms; two different names for one and the same thing." And yet the proportionality of weight and mass or of gravitation force and mass is put forth as a real fact of the Newtonian theory (p. 349).

In the second place, they ignore in detail the laws which they admit in general. For instance, on p. 77 occurs the following illustration:—

"Two bodies attracting each other, the one with a force of 100 and the other with a force of 10, the stronger body would be pulled one-tenth and the lesser nine-tenths out of their respective positions."

And many other instances might be given of their entire misapprehension of Newton's third law.

There are many singular instances of what appears to be malevolent criticism, statements of the beliefs or usages of orthodox science which are utterly destitute of basis in fact. The following may serve as illustrations:—

"Confining our attention to what are called the physical sciences, we still find the greatest possible differences in both language and conception. We have hydraulics and hydrostatics, mechanics, dynamics and pneumatics, electricity and magnetism, light, sound, and heat, and so forth; each separate branch of knowledge having its own distinctive theory and peculiar terminology not applicable to the others. It is like the confusion at the Tower of Babel. The workers in the different fields of knowledge cannot understand each other, therefore cannot commune with each other for the purpose of seeing whether the conclusions deduced from one group of facts agree with the conclusions derived in other fields of inquiry."

"The close relation of the different phenomena embraced under electricity, magnetism, and galvanism is now well known, yet are they still treated as separate and distinct sciences."

On p. 39 it is asserted that Newton's pendulum experiments, whereby he proved that inertia and weight were proportional, were proofs of nothing whatever, being only equivalent to using first one balance and then another. And on p. 40 it is said that when Newton found that pendulum bobs composed of two different materials swung together if their centres of oscillation agreed, he must have adjusted their centres of oscillation experimentally by means of equal times of swing.

In other words, he was either a fool or a knave; the full-blown paradoxer would not have shrunk from the appropriate epithets. But our authors are more polite; they are good enough to say, "Our quarrel is not with Sir Isaac Newton, but with human frailty"!

"The fixing of the absolute zero at -273° C. is based on the theory that at that temperature a body would practically be annihilated—a conclusion which in a previous chapter we have shown to be unwarranted by the facts"!

There is more about this absolute zero of temperature, which is evidently a bugbear, and is considered as nonsensical as absolute position or absolute truth; in fact, on the subject of thermodynamics generally the authors are naturally afflicted with wholesale ignorance, which they believe to be scepticism. "The action of the steam engine is generally attributed to heat," they say on p. 84, "and calculations are based as to the quantity of work to be got out of a heat engine on the supposed mechanical equivalent of heat," and they proceed to point out the errors in this view. It appears that "the true cause of the motion of the piston is a disturbed equilibrium, and consequent tendency to equalisation." "Heat is no more an entity than acidity or hardness or depth of colour." In fact the whole doctrine of energy is absurd.

"The whole doctrine of 'energy,' with all its astounding and contradictory corrollaries, has not been deduced from any facts at all, but is begotten of those conceptions which have come down to us in unbroken succession from our primitive and ignorant ancestors. The facts have merely been distorted in order to make them fit into the mould of the prejudiced human mind."

After this they proceed to "deal with the error which has given rise to the doctrine of the dissipation of energy." "When estimating the efficiency or power of doing work of a body we do not measure the *force which is just necessary to arrest its motion* [as we ought to do, is implied], but that which actually causes it to move a certain distance; in which case there is bound to be a discrepancy, an apparent loss."

Joule's experiments involved also a fundamental error, "owing to his assumption that a certain weight descending a certain height in a certain time is the measure of its mechanical power."

"Where he committed the error was that he assumed the whole power of the descending weight to have been expended in driving his mechanism, [but] to produce motion in the mechanism the weight had to be greater than the resistance of the latter, . . . The power of the weight was greater than the mechanical effect in his paddles, and hence *greater than the corresponding thermal effect*. But *by how much greater* Joule never thought of determining."

No, alas, he believed practically as well as theoretically that action and reaction were equal.

"Instead of estimating the 'heat' produced by friction, and then dividing the assumed mechanical power by the number of heat units—as Joule did—he might have estimated the mechanical effect produced by 'heat,' and then have calculated the heat equivalent of work by dividing the calculated thermal units (which no doubt he would have assumed as being all converted into mechanical effect) by the resultant work expressed in foot-pounds. Manifestly he would, in the latter case, have given too high a heat value to the mechanical effects; just as in his actual reasoning he has given too high a mechanical value to his heat units."

And then in a footnote :

"There can be no doubt that it was purely accidental that the latter process has not been adopted by Joule"!

If he had happened to make his experiments in this inverse way, the authors go on to say, the law deduced would be "that the heat energy of the universe is becoming every day more and more changed into mechanical energy."

"Thomson's error was due to his acceptance of Joule's conclusions. Joule, in his turn, arrived at his fallacious conclusions by the double error, . . . and both these philosophers have shared in the fundamental error that 'energy' is a *something* which is transferred from one body to another. Thus do we see how fertile an erroneous conception is in producing error. . . . Verily, as Lord Bacon expressed it"—

Then follows a quotation about the tendency of the human mind to reject contrary instances—not without great and pernicious prejudice—"in order that the authority of those previous conclusions may remain unshaken."

But not only do they thus misapprehend matters of real physics: the authors also fail to understand even such simple popular phrases as Tyndall's "Heat a mode of motion," and criticise it as if "mode" meant "result": saying that if heat were due to motion, then equal weights of different substances falling from equal heights should manifest the same increase of temperature (p. 165), and they go on to say that experiments on this point would be desirable. The generation of heat, they say, is due to states or acts of coercion, and in one of the few modest sentences to be found in the book they confess, on p. 163, that

"our own belief is that all bodies while in states of coercion [like compressed air] are constant sources of heating, but that the diffusion is equal to the rate of generation, and hence no sensible increase of temperature can take place."

Further on they recover from this unusual weakness, stoutly maintaining that what they call Tyndall's view of heat is quite wrong, "since heat is due to arrested motion, not to motion itself," and boldly asserting that the amount of heat generated by bodies falling from a height depends upon their hardness,

"a quantity of water or mercury falling from a certain height would not generate as much heat as would a like quantity of, say, steel falling from an equal height."

It would be tedious to follow the authors through their wild statements concerning electricity and magnetism in detail: the following brief extracts must suffice.

"The gold-leaf electroscope, consisting as it does of glass and metal, will be found to be analogous to an electric machine. Indeed, the principle of the instrument is not yet understood, and no attempt has been made to explain it" (p. 211).

"The resistance of thin wires is less than the resistance of thicker wires of the same material, which is the opposite of what is currently believed to be the case" (p. 222).

"The total resistance of 6 lbs. of copper, for instance, would be the same whether the 6 lbs. of copper were only a yard in length, with a corresponding diameter, or a mile long and correspondingly thinner."

"Air is a most powerful electric, and when between the poles of a powerful electro-magnet partakes of the character of a viscous fluid."

Then follows a reference to the usual well-known experiment of moving or spinning copper between the poles, with the following note appended :

"which shows that air is attracted by powerful magnets and held there with a firm grasp."

"The distinction between electric and magnetic attraction is arbitrary, and no more philosophic than is the distinction between light and heavy bodies, hot and cold bodies, voltaic electricity and galvanism."

The distinction usually drawn between conduction and induction is asserted to be spurious. Heat electricity and magnetism are said to be identical, being all excited by friction. Magnetic attraction may just as well be called gravitation, since the only difference is that it is not necessarily in a vertical direction.

Thus we get led back once more to the subject of gravitation, and Newton's law is replaced by the following foggy statement.

"Bodies attract each other in proportion to their different states of excitation, in proportion to relative mass, and inversely proportionally to the square of the distance and intervening resistance."

"The point of attraction of the earth for bodies on its surface cannot be in the centre . . . for that would involve the assumption that the opposite half of the globe . . . exerts a force equal to that of the nearer half."

"The theory that attraction is proportional to mass is not borne out by facts: attraction on earth is actually less where the diameter of the earth is greatest. If we turn to the heavens matters are even less satisfactory."

The astronomical determination of the mass of central bodies turns out on their view to be quite illusory.

With regard to "action at a distance," our philosophers find no difficulty whatever; they point out that air and water and other matter only obstruct the fall of bodies, wherefore, of course the less there is between bodies the better they can attract. They quote the usual extract from Newton's letter to Dr. Bentley, and add comments

"in order to show why such luminaries of the human intellect were unable to see what to us seems as plain as the noon-day sun; and we trust to be able to show the reader that it was again the *ignis fatuus* of suggestive words and false concepts by which these great intellects were decoyed from the path of philosophy into the quagmire of metaphysical word-quibblings."

But now, towards the end of the volume, when the Newtonian theory is once more attacked, the authors fail to keep their mantle so strictly down, and the cloven hoof of the familiar old paradoxer becomes at length conspicuous.

"This fact, that the planets do not fall into the sun notwithstanding the assertion that they mutually attract each other, but, quite contrary to this theory, actually recede from the sun at regular intervals, is as yet an unexplained problem. In works on astronomy, however, the fact is generally glossed over, and it is made to appear as if the recessions were most satisfactorily accounted for. But on closer examination the supposed explanation turns out to be either mere rhetoric or a series of equations of abstract mathematical problems. Why, if sun and planets constantly attract each other proportionally to their quantities of matter, and inversely proportionally to the square of the distance, do they not fall into each other and all into the sun? The question is a perfectly legitimate one, seeing that every writer on astronomy has deemed it necessary to offer explanations. But these explanations are far from satisfactory, as we shall now endeavour to show."

And so they proceed ultimately to arrive at the conclusion that,

"while there is no doubt that the curvilinear motion of terrestrial bodies is due to the joint action of two forces, viz. the impelling force and the attraction of the earth, there is no evidence either direct or indirect that planetary motions are due to two such independent forces."

The ridiculous ideas capable of being formed by men of so-called education concerning the meaning and function of mathematical reasoning is illustrated on page 353, where it is said first that Newton's achievement was not what is usually supposed:

"Newton, however, has done nothing of the kind; nor has he even attempted to do so. What Newton did prove was the *proposition* that 'A body [acted on] by two forces conjoined will describe the diagonal of a parallelogram in the same time as it would describe the sides, by those two forces apart.' It is the truth of this mathematical proposition that Newton proved, and not that planetary motions are actually due to two such forces."

And then it is added:

"Abstract mathematical formulæ can never prove a fact in nature." All that mathematics can do may be illustrated by the case of a man sent to market with eighteenpence, who returns with sixpence, then on the hypothesis that he spent the difference on a dozen apples, mathematics enables us to calculate the price of each apple; or, given the price, it can find the number of apples bought.

Criticism of this sort is gravely printed. It appears to be literally true that many men, including a few schoolmasters, believe mathematics to be represented by schoolboy studies, and especially by "problems leading to simple equations with one unknown quantity."

In Section A, at Toronto, recently, Lord Kelvin implied, *en passant*, that a schoolboy in the very first month of his algebraical studies came across something which when developed led into the heart of the mathematical arcana; viz. when he came across the imaginary roots of a quadratic equation. No wonder that in Lord Kelvin's perspective quadratic equations are relegated in imagination to something like the first month of a boy's mathematical study; but, alas, many of us know boys of sixteen who have been for years at what schoolmasters call mathematics, and who have hardly yet arrived at quadratic equations. In the perspective of many a British school, trigonometry forms a sort of goal, and the elements of the differential calculus loom dim and gigantic in the far future. When feebleness of the kind quoted by the authors is believed to represent anything like mathematics, no wonder that practical men and others regard it with aversion thinly veiled by contempt.

But not only is mathematical reasoning lightly regarded by our authors, they differ from most of their species in treating experiment also in a slight and unsubstantial manner, which though by no means intended for disrespect really amounts to it. A number of experiments are quoted, some of them made apparently by the authors themselves, which if they were true would constitute some of the greatest discoveries of the century; but they are obtained in the most casual manner, as if, like the scientific hero of many novels, the authors had only to retire into a back room for about twenty minutes, in order to make the most momentous and fame-bringing discovery. We quote two instances.

"By heating a body it is made to weigh less: that is, not merely is its specific gravity lowered, but its absolute weight is less, and it regains its former weight on cooling" (p. 380).

"Our explanation of this is that, though cool to touch, the molecules are still in a state of excitation, and hence their lesser weight" (p. 383).

"Another experiment was made as follows. A glass tube sealed at one end was contracted in the middle. In the lower portion were placed about 10 c.c. of water, and in the upper portion a stick of dry potassium hydrate of about an equal weight, and the tube was sealed. After cooling, the tube was weighed and then turned upside down, so that the water could flow on to the potassium hydrate. The stick of potassium hydrate partially dissolved, and the solution crystallised. On weighing it was found to be lighter by about 20 mgr. On shaking the glass the crystals partially redissolved, and the tube became heavier; but after some time the crystals re-formed, and the tube weighed again less" (p. 384).

On pp. 397 and 401 we are told that inside the crust of the earth there is a neutral sphere or zone outside which bodies press inwards, but within which they press outwards, so that gravity at a certain depth in the earth is inverted and bodies press upward. So this naturally explains volcanoes,—in fact the earth, if cooled, might explode like a Rupert's drop (p. 402).

Matter from the interior, if it ever got above the neutral zone, would be found to be light instead of heavy, and hence it is that the dust of Krakatoa took so long to settle (pp. 400 and 407)!

After this we can be surprised at nothing, and so we go on to learn:—

That the axial revolution of the earth is due to the radiation of the sun, like the motion of a radiometer or of a sunflower.

That the real diameter of the earth's orb is 506,734, and not 8000, miles.

That by reason of its axial rotation the earth rolls on its own orb, pressing against the zone of neutral attraction, and thus effecting its annual journey round the sun like a coach-wheel.

That the sunspots are circumsolar planets with satellites, their apparently irregular shape being an easily explained optical illusion.

That the terrestrial seasons are caused by unequal solar attractions; and other extraordinary nonsense.

We will on these topics make only two quotations, though the exposition of the doctrines occupies several chapters.

"Nobody will question that at a certain distance between sun and earth a body will be more strongly attracted by the latter than by the former, or by the former than by the latter, as the body may happen to be nearer the one or the other. In other words, nobody will question that the earth has its own field of attraction as against the sun; hence all that is within that field of attraction would form part and parcel of the mass. Now, the distance between earth and sun is at all times determined by their relative states of excitation, as before explained. Hence the earth may be regarded at any one moment as being kept at a certain definite distance from the sun, as if held there by ropes or bars. But the earth revolves, and in revolving meets with a greater resistance on the side nearest the sun than on the opposite side; hence there is greater retardation on the one side than on the other: from which follows the translation in orbit. The laws of rigid mechanics find, therefore, here application. The earth is drawn towards the sun, but cannot pass a certain line; *and this line is the rigid surface against which it presses.* To the eye nothing may be there impenetrable; but to the earth this invisible circle is like a hoop of adamant, against which it presses and along which it is rolling in space."

And this brief note:—

"Prof. Helmholtz estimated the depth of the luminous envelope at 500 (!) miles. This estimate . . . is ludicrous."

Thus at length the lofty position appropriate to superior knowledge is fully assumed; and from pitying and apologising for Newton, rebuking and correcting Thomson and Tait for their fundamental errors, the authors proceed to laugh at an estimate by von Helmholtz.

Behold, a greater than these men has arisen; another still larger volume already in manuscript is threatened; and the natural philosopher of the twentieth century is to be Ignatius Singer! O. J. L.

PHILOSOPHY OF KNOWLEDGE.

Philosophy of Knowledge. By Prof. G. T. Ladd. Pp. xv + 614. (London: Longmans and Co., 1897.)

PROF. LADD, of Yale, is well known as an industrious writer on psychology, and upon philosophy regarded mainly from a psychologist's point of view. The present volume gives his Theory of Knowledge, and with some *naïveté* he claims for it "the treatment due to a pioneer work," and avows a "quite unusual interest" in its success. His method he characterises as a constant striving "to make epistemology vital—a thing of moment, because indissolubly and most intimatel y connected with the ethical and religious life of the age."

Most modern psychologists of note have definitely broken with that abstract and detached view of their science which would argue the question of the "origin" of knowledge within the limits of a narrow subjectivism, and which would assume that thereby the questions of nature growth and validity had received adequate solution. The present tendency of psychology is rather to offer itself as that specialist training which makes general metaphysic profitable, or, at any rate, to endeavour to set itself right with metaphysic, by an alliance based, perhaps, upon a compromise.

Of such a tendency Prof. Ladd is a representative. He puts forward a strenuous plea for an ultimate view of knowledge and reality which shall neither be subjective idealism nor crude realism, neither wholly dualism in one sense of that word, nor wholly monism in one sense of that word. He insists on the implication in the act of knowledge of what he not very happily calls "extra-mental" or "trans-subjective" reality, meaning that we apprehend that which is manifestly independent on our individual consciousness, and does not emerge in it as a a matter of course as the result of its own laws. He urges the claims of our feelings and our will to be satisfied equally as well as our thinking taken abstractly. He demands as a sort of postulate of faith that we take it that things are known in some sense as they are. The self, he holds, is known as it really is, thinking, feeling, and willing. And things are known as what will not always as we will, and by an analogical *saltus* as other will.

This is interesting speculation, but it is rather eclectic than original. Prof. Ladd would seem to be a pioneer only in the sense that we are at present fortunately free from *Erkenntnisstheorie* as it is too often understood in Germany. For he is something too well read in theories of knowledge which pass with undue freedom from psychology to metaphysic, and often approach perilously near to *ignoratio elenchi*.

It is in his criticisms that this influence, which constitutes, as we venture to think, the weakn ess of his work, makes itself apparent. Interlined with positive views as to the relation of faith to knowledge, of æsthetic and ethical value to reality, where Prof. Ladd obviously owes much to Lotze, or as to the relation of thought and reality to will, where Wundt's influence is manifest, there are offered an erudite though inconclusive history of opinion and a running fire of uncomplimentary comment upon Kant, Mr. Spencer, Mr. F. H. Bradley, and others. And the sympathetic insight of the successful critic is wanting to the Yale professor. The results, as well as the methods, of certain thinkers are agnostic or sceptical, and that must not be. And so a discursive appreciation of various points of their doctrine is put forth, which, *e.g.* in the case of Kant, ranges, with the exception of a suggestion as to the implication of will in the treatment of the second analogy of experience, entirely within the circle of commonplaces on the subject, and is altogether unconvincing to believers in the results of sympathetic interpretation. Again, in dealing with post-Kantian idealism, it is not its alleged panlogism which he criticises, as he might well have done from his Wundtian standpoint, but rather its general tone or points so truistic that any theory of knowledge must, and if not explicitly yet tacitly does, meet them.

The discursive and rhetorical style of the book would of itself tend to ineffectiveness in matters of criticism. It is often picturesque, and has many happy phrases; it sometimes rises to eloquence, and is always eminently readable. But it is singularly vague and elusive. And the volume needs condensation. H. W. B.

OUR BOOK SHELF.

Bau und Leben unserer Waldbäume. Von Dr. M. Büsgen, Professor an der grossherzoglich sächsischen Forstlehranstalt in Eisenach. Pp. viii + 230, mit 100 Abbildungen. (Jena: Verlag von Gustav Fischer, 1897.)

THIS comprehensive volume on forest botany, which is essentially on the same lines as the well-known text-books of Döbmer-Nobbe, Hartig, and Schwarz, gives an interesting account of the structure and physiology of forest trees. The introductory chapters are devoted to a general external survey of the tree, note being made of the various forms of buds and shoots, while the influence of their position and development on the habit of trees is clearly indicated. An interesting subject is touched upon in the annual and periodic rate of height-growth, and reference is made to the relationship that exists between the rapidity of growth in youth and the light-requirements of trees. With hardly an exception trees that are intolerant of shading grow with great rapidity when young (larch, birch, &c.), and are thus enabled to keep their crowns well above the level of those of competing species. Slow-growing species, on the other hand, are not prejudically affected by moderate shading (silver fir, beech, &c.). Were they otherwise they could hardly have survived in the mixed primæval forest, where the struggle for existence proceeded without interference from the woodman's axe.

An important section of the book deals with the annual wood-ring, the characteristics of which are so useful in aiding in the identification and in explaining the properties of timber. Although our knowledge of the causes that lead to modifications in the annual ring of trees has been much advanced of recent years, there are still many interesting problems awaiting solution, as, for instance, in the matter of eccentric growth. The explanations that are usually offered can hardly be said to be sufficient to account for the constant eccentricity that occurs on sections of wood taken from roots and branches, as well as from stems that have grown upon a hillside. And even after all that has been written by Sachs, de Vries, Krabbe, Hartig, Strassburger, and others, who will confidently say whether pressure, nutrition, physical exhaustion of the cambium, water, or heredity is the true cause of the difference that exists in the structure of wood formed early and late in the growing season?

After discussing the formation of duramen and the properties of timber—where, by the way, one misses any reference to the latest work of Roth in America and of Schwappach in Prussia—the author proceeds to an examination of the leaf and root. As was to be expected, a good deal is said regarding the many theories that have from time to time been advanced to account for the ascent of water in trees—so ably summarised up to date by Marshall Ward in his book on timber—and while greatest prominence is given to Strassburger's experiments, the work of Dixon and Joly receives appreciative recognition. A chapter on fruits, seeds, and seedlings completes a volume which, while designed chiefly for foresters, cannot fail to be of use to a larger public, and especially to students of botany.

WILLIAM SOMERVILLE.

Physiography for Advanced Students. By A. T. Simmons, B.Sc. Pp. viii + 483. (London: Macmillan and Co., Ltd., 1897.)

THIS book is a supplement to "Physiography for Beginners" by the same author, and for those who have mastered the earlier work it will furnish an excellent continuation course. Matter, energy, the air, the sea, and kindred subjects occupy more than half the volume, while the description of the different members of "the universe" and of the various natural laws relating to them, occupies the remainder.

Both terrestrial and celestial subjects are admirably dealt with, the explanations being clear and to the point, and the selection of illustrations, which number 218, leaves nothing to be desired. The experimental method so successfully adopted in the previous book has been adhered to as far as possible, though there is naturally less scope than before for this treatment. Another notable and praiseworthy feature is the large number of references to books and *Proceedings* of societies dealing with special branches of the subject, and it is much to be desired that advanced students should acquire the habit of utilising information of this kind.

At the end of each chapter is a series of test questions, which will doubtless be greatly appreciated by teachers. There is also a summary of the chief points of each chapter, which will be valuable if not misused; but there is possibly some danger of the less serious students confining their studies to these condensed statements.

We believe that the book will admirably supply the need which must have been felt by teachers and students under the new conditions created by the revised syllabus. In conjunction with the volume to which it is a supplement, it will also provide the general reader with a comprehensive view of the earth and its relation to other bodies in space.

Chemistry for Photographers. By C. F. Townsend, F.C.S. Pp. xviii + 158. (London: Dawbarn and Ward, Ltd., 1897.)

GREAT is the number of those who practise the art of photography at the present day, but how many of these are acquainted with the chemical reasons underlying the numerous manipulations which are performed? Every photographer, it does not matter how much or how little he employs his camera, should make himself familiar with, at any rate, the chief rudiments of this science, even if he does not wish to enter more deeply into details.

The book which we have before us gives the reader a concise and clear insight into the various chemical questions which come into the sphere of photography. The author has carefully drawn attention to the fact, that by good judgment, and by paying heed to the actions of various chemicals employed, the photographic plate can be made to give results far better than when such knowledge is lacking.

Not only is the chemistry of the photographic image, developers, reversal, intensification and reduction, printing, &c., clearly explained, but useful information is collected, bearing on impurities, recovery of residues, cellulose, resins, varnishes, &c. Curiously enough, no mention is made concerning the pros and cons of mounting solutions, an important question for those who wish their prints to last more than a year or two. Perhaps this subject will receive attention in a future edition.

The book should be read by all who wish to gain an insight into the chemical side of photography.

My Fourth Tour in Western Australia. By Albert F. Calvert, F.R.G.S. Pp. xxvii + 359. (London: William Heinemann, 1897.)

MR. ALBERT CALVERT has written much upon Western Australia, and has been generous in publishing his views and convictions as to the mineral resources of that Colony. He now informs his readers that the object of previous works was to advance the interests of the Colony, whereas in the present volume the subject is treated "entirely from a personal standpoint." Open confession is proverbially good for the soul, and by declaring that the book contains a narrative of personal impressions, intended to interest and amuse the public, Mr. Calvert leaves no room for doubt as to the purpose of his new publication. In keeping with this object, a number of illustrations of purely personal matters are included in the volume.

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.*]

Astronomical Constants and the Paris Conference.

MANY objections have recently been raised—above all, in America—to the decisions of the International Conference that met in May 1896, at Paris, with the object of choosing a uniform system of fundamental stars and astronomical constants for the four great ephemerides of Berlin, London, Paris and Washington. The matter is of the greatest importance, inasmuch as it refers to the bases of precise astronomy, together with a general tendency of all science to a method of international discussion that, leaving free and autonomous all personal and local initiatives, bring workers in such agreement as is necessary for nullifying discrepancies and contradictions. Of such tendency we have the most comforting manifestations in the International Geodetical Association, in the Commission (also international) for unifying weights and measures, and in the similar meetings of scientific men of every nation for the electrical measurements, for meteorological services, for the photography of the heavens. Astronomy is, perhaps, among all physical sciences the one destined by its historical tradition, no less than by its present and future necessities, to second—nay, to promote and develop—the cosmopolitan tendency. The grand spectacle of the face of the heavens, ever before the eyes of all; the difference of phenomena according to the horizons, which carries with it the need of co-operation between the observers diversely situated with regard to the celestial sphere; in fine, the high and significant moral education that comes to astronomers from the continual contrast between the immensity of the heavens and the miserable narrowness of the limits traced out conventionally on the globe between one country and another; here are the causes through which a spirit superior to any narrow nationalism was soon breathed into our souls. Tycho Brahe, the proud Danish patrician, the founder of practical astronomy in the Renaissance, sings sternly—

> Omne solum forti patria est, coelumque
> Undique supra. . . .

And his name, with those of Copernicus, Kepler, Galileo and Newton, form a constellation that shines not more for the sky of Denmark, than for that of Germany, Italy, or England!

On the other hand, on the sentimental considerations of the solidarity of the human race, to which a great weight must always be given, other arguments of a more modest and more positive character rest and flourish; these comfort, and, if I do not deceive myself, put out of all reasonable doubt the necessity of imprinting on our science an international character. From the day in which Frederic William Bessel solidly constructed, on the admirable observations of James Bradley, the "Foundations of Astronomy," enriching with his teutonic analysis the product of the patient British ingeniousness, a new current of collaboration among astronomers of every country was opened. The works of the said Bessel, of Struve, of Argelander, and lately those of Auwers and Newcomb, set out by admitting that the modern era of precise astronomy opens with Bradley, as the era of the Renaissance began with Tycho Brahe, and the Hellenic with Hipparchus. Each of those three great observers had his precursors, whose work seems to establish a mysterious continuity with the astronomy of the civilisation immediately preceding it. Aristillus and Tymocharis, the first Alexandrian observers, are connected with the astronomy of the Oriental nations; King Alfonso of Castile, Paolo Toscanelli and Regiomontanus with that of the Arabians; Hevelius, Cassini, Flamsteed with that of the Renaissance. But as in the great catalogue of Hipparchus we have the measure of what the genius of the Greeks could give when applied to observations of the heavens, as only in the celestial places determined by Tycho Brahe we have the material for the new planetary theories of Kepler, which led to the discoveries of Newton, so to Bradley (and to Bradley alone) we must refer the rational application of these delicate means of astronomical observation that, revealing the most minute corrections to be applied to the places of stars, led to the foundation of a sidereal astronomy. The precession of the equinoxes more rigorously determined, the nutation of the terrestrial axis and the aberration of the fixed stars discovered and measured in their effects; the places of 3222 stars exactly settled in regard to the fundamental circles of the sphere, with a precision unknown till now, and comparable only to that of the modern meridian observations: these are the fruits of the long, fatiguing and sapient vigils of the Royal Astronomer in that Greenwich Observatory whence astronomy issued transformed one hundred and fifty years ago.

But if Bradley merited in the competent and severe judgment of Bessel the glorious epithet of *vir incomparabilis*, if Lindenau and Struve had been able to say that astronomy had been renovated at Greenwich and that the work of the great English observatory surpassed at the end of the eighteenth century all remaining astronomical productions, if, after the exhaustive discussion of Bessel (*Fundamenta Astronomiæ*), Bradley's work has still been good enough for a more refined elaboration in the last quarter of our century, by the merit of Auwers, who has given it all the value of which it was intrinsically susceptible, it is no longer allowable nowadays to put entirely on one side all the material that has been accumulated from 1750 till now, and which ought to concur with Bradley's observations in fixing with all possible approximation the values of the fundamental constants and the coordinates of the leading stars. As to what refers more especially to the precession of the equinoxes and to the proper motions of the stars, it is undeniable that the value of Bradley's observations is notably augmented by their antiquity: the greater the length of time that has elapsed, the more is manifested the influence of corrective terms, in which time itself is multiplied by the coefficient of the precession or by the proper motion on the one or the other coordinate. But to render such influence really efficacious in the calculation of the factors that enter into the same terms, it is necessary that the lapse of time should be so great as to carry the terms that contain it to a higher order of term (unknown) that represents the accidental and systematic errors of the places settled by Bradley. Now no one could affirm that the century and a half that has passed between Bradley and our time be sufficient for this; on the contrary, indications are not wanting that authorise an opposite opinion. Shall we, for this, resign ourselves to a perfectly passive indifference, allowing time to mature the terms still too restricted? For the accidental errors, there is nothing else to be done; but they are not the most dangerous, and, on the other hand, the abundance of Bradleyan observations would allow us in many cases to rely upon fortuitous compensations. For the systematic errors, however, the case is different. We can and we ought, with all the means of investigation with which the improved state of the science furnishes us, to seek out those systematic errors that still remain in Bradley's Catalogue, even after the acute revision of Auwers. Such a work would in no wise be irreverent to the labours of the talented Berlin astronomer, who, for the first, has given us precious materials for investigations of that sort, by his new and masterly discussion of the observations that Tobias Mayer made at Göttingen, contemporaneously with those of Bradley. It is necessary in a special manner to ascertain (as my revered master Schiaparelli suggests to me) whether and how far it is possible to render independent of Bradley the enormous amount of observations made by Piazzi at Palermo between 1792 and 1814. To such research I am now attending, while at the New York Observatory (through Dr. Davis) and at Turin the elements are being prepared in accord for a new reduction of the Palermitan Catalogue. Until such a reduction be completed, and until analogous researches have been instituted on all the observations that can lend themselves to the solution of the problem, and that were executed in the first half of the century, it will be vain to impose by international agreement the values for the precession and for proper motions that should be not provisory. The antiquity of the equinoxes observed by Bradley is not sufficient title to make us exclude *à priori* equinoxes more recent but more precise, those observed in Germany especially. For the needs of astronomy until now Bradley's work has rendered incalculable service; but what Bradley could by himself give, he has given. It is perhaps illusory to believe that a deeper and minuter discussion (as is undoubtedly Newcomb's last, compared to those of the Struves) can lead us to a more intimate knowledge of the truth.

But it is not alone in the exclusion of intermediary observations, of those in a special manner belonging to the first half of our century, that the conclusions of the Paris Conference find serious objections, for what refers to the precession and to proper motions. As Prof. Lewis Boss notes in a deep and animated paper in the *Astronomical Journal*, two great astronomical undertakings adapted to throw new light on the controversy await their fulfilment: the zones of the *Astronomische Gesellschaft* and the Catalogue of the Paris Observatory. Notwithstanding the differential character of the first, and the heterogeneous origin of the second, Prof. Boss thinks that we cannot estimate from these two copious springs of information on the proper motions, in which the abundance of data compensates (in certain limits at least) their dependence upon the errors of the fundamental systems adopted.

Allow me to add another consideration to those put forward in all competence by the American astronomer. The problem of the precession is indivisible, as we have said, from the problem of the proper motions: the problem of proper motions may depend in its turn on the displacement of the solar system in space. Now there is no doubt that a solution, fit to be considered definitive for a long series of years, is still wanting as to the last problem. And the reason of it is clear: it has been till now attacked on different sides, but always with individual method, inherent (we must say) to the state of science, that has not yet mobilised all its forces for a simultaneous attack. The classical methods based on confronting between stellar coordinates observed at many years' distance, and on subtracting in the observed displacements the parallactic part from that due to the real motion of single stars, are singularly convenient to determine the directions of the translatory movement of the solar system. To determine its velocity it is instead more convenient to use spectroscopic methods, with which the velocity of each star is measured in the direction of the visual ray. The very brief time that has elapsed since the day that Vogel, at Potsdam, with such ability rendered possible the measurement on the photographs of the slender and nearly imperceptible displacements of the spectrum lines, has not yet allowed this method to be applied in all its potentiality, the fecundity of which Secchi had already divined, as many worthy observers, such as Pickering and Maunder, had attempted to practically employ it. When the new photographic equatorial (to be mounted at Potsdam) has given the means of studying a large number of stars inferior to the second magnitude (which is the limit till now of the instrument used by Vogel), when the rigorous proceedings of the illustrious German spectroscopist become familiar to the American astronomers, who until now are perhaps the only persons to whom the question of expense presents no difficulty, when a new installation is to be made, then, and only then, the moment for approaching the solution of the problem of the solar translation in space by the medium of a rational combination of astronomical and spectroscopical methods will be arrived.

Once the movement of our system is known with sufficient exactitude, it will be easy to eliminate its effect from the proper motions of the stars, which can then be reduced, free from all systematic part, to the merely accidental variations of the coordinates, and can therefore be treated as accidental errors, and submitted to calculation, together with the eventual correction of the constant of precession. This will only gain in precision the longer its deduction is delayed, which, for obvious reasons, may be afterwards made definitive for a long series of years. It is exactly for such a consideration that I think it right that all the equinoxes determined after Bradley's, and which are not affected (as those of Piazzi are) by incurable errors, should be used in the new determination of precession, it being possible only, from the comparison of these equinoxes, to rise again by extrapolation to Bradley's epoch, with data adapted to render clear the law of the systematic errors remaining in the revision of Auwers.

As to the nutation, it does not seem that the value 9″·21 of this constant, adopted by the Conference, has yet given rise—at all events, in public—to disputes and oppositions. Anyhow, it will not be useless to remember that fresh rigorous determinations of it were by several authorities demonstrated to be necessary in recent years, and that especially the persistent uncertainties have their being through the disagreement, not yet well explained, existing between the value of the terrestrial flattening given by the mechanical theory of the nutation and that given by geodetical measures. Harkness has sustained the need of a new series of observations, tending to furnish a new value of the constant of nutation, more precise than that of Peters. While such a series is projected and executed (and it will be necessary to prolong it at least for one of the periods of nineteen years that reconduct the node of the moon's orbit to the equinox), the discussion of the enormous geodetical material that is being accomplished will furnish us more secure facts as to the flattening and to the moments of inertia of the earth, thanks to the comparison between the values given by the arcs of meridian and by the measured lengths of the pendulum for seconds. Hence a correction (that can be but arbitrary) seems premature as to the value already assigned by Peters, whose exactitude, according to Harkness, is remarkable.

For the solar parallax, the Conference thought it opportune to fix a value of 8″·80, by an act of authority unjustified by sufficient explanations. This appears to many astronomers to be an acceptable mean among the most discordant values that laborious researches have furnished in the last twenty years. The lamented Tisserand found no other motive for being satisfied with this choice, except its approach at less than a hundredth part of a second to the value fixed by Laplace. But the example of the famous mistake of Encke, not too opportunely quoted by him, should have admonished him to use greater prudence. We have, it is true, two distinct groups of results in these last years; some approximate to the value of 8″·75, the others about 8″·85. Are we to conclude that the first defect 0″·5, the others exceeding as much? Or is it not—given the great intrinsic accordance of the single determinations on which every value reposes—more legitimate to suspect that some series are affected by constant errors relatively enormous, and such to take weight from them all in the definitive discussion? The fact is that, notwithstanding the transits of Venus and the oppositions of Mars, notwithstanding the photographs and the heliometers, or the combined forces of men of value like Gill, Auwers, and many others, the problem of the parallax remains more open than ever. It is true that, as Boss remarks, one active period for this research seems at an end; but it is true also that, on the contrary, the question of the aberration is more lively than ever; and that, in the present state of astronomy, it would be absurd to settle the value of the former just when the greatest efforts are concentrated on the latter. To avoid contradictions, I see nothing better at present than to adopt (which, for the immense majority of astronomers, would simply mean to preserve) the constant of aberration given by Nyren, and the corresponding parallax, as it is calculated with the last value of the velocity of light, determined by Michelson at Breteuil. In a few years the material for a new calculation of the aberration will not be wanting; each of the series instituted lately for the study of the variations of latitude at short periods will furnish a useful element for discussion.

We see from this rapid analysis how all fundamental problems of astronomy, even being connected with each other in a single problem, by effect of the equations of condition that unite the single constants, can, for simplicity of investigation, be gathered in diverse groups, according to the more intimate dependence of some constants upon others. We see also that the gigantic general combination, attempted by Harkness a few years ago, is really premature, but has notable importance, as the first essay of the method which sooner or later must be used. Harkness' work in every way teaches us the inutility, or at least the small efficacy, of isolated researches, that are not sought to be coordinated with the analogous researches on elements of the same group. If it be too bold to establish the solar parallax on several dozen physical constants, mechanical or astronomical, bound more or less with it, it would be too timid and behindhand not to take into account with it the velocity of light, the stellar and planetary aberration, and the variation of the geographical latitudes. The nutation joins with the constants that define the figure of the globe and the law of the distribution of matter in the internal strata of it. Finally, the precession is united to the proper motions of the stars, and to the translation of the solar system in space, elements which in their turn depend on the systematic corrections of the stellar catalogues.

A general renovation, then, of fundamental astronomy must precede a new and authoritative definition of the numbers that the twentieth century must accept as the more probable values of the constants. The compilation of a fundamental catalogue, which must be the consequence of all this work, cannot be subordinated to considerations of opportunity, and much less to what Dr. Chandler shrewdly calls the "sentimental association vulgarly attaching to round figures." If, for reasons foreign to science, the editors of the *Connaissance des Temps* and of the *Nautical Almanac* will not accept the Newcomb's or the Auwers' stars, let them keep their own; the slight residuary discrepancy between the ephemeridæ will certainly be less harmful than agreement on a new provisional system, that, not having more value than the existing ones, would acquire superior authority without justifiable motives. Dr. Auwers, who is undoubtedly the most competent person in such matters at the present time, has well delineated the limits of his fundamental catalogue, calling it not purely and simply fundamental, but "fundamental for the observations of the zones of the *Astronomische Gesellschaft*." In other words, he has not intended to do anything else than furnish an indispensable basis for the great international undertaking about the zones, and has set up a guard against extending the signification of "fundamental" that should legitimise every delicate use of his stellar positions. The researches to which he is attending now, and to which the Paris Conference justly attributes "un intérêt scientifique de premier ordre," prove how in his mind (as in those of all the astronomers assembled in Paris) the fundamental catalogue now in use may be considered alike provisional, just as the new catalogue to be compiled at the end of this century for the needs of national ephemerides. To give to-day the title and the authority of a fundamental catalogue to a collection of stars is not sufficient in fact for the single stars to answer simply to the conditions which are enumerated at page 6 (Appendix C) of the *Annuaire du Bureau des Longitudes* for 1897; it is necessary that their positions be founded on absolute observations, executed with all the precision of which perfected modern instruments are susceptible, and on an exhaustive discussion of the series obtained till now on the same stars at Greenwich, Pulkova, Leyden, Washington, and in a few other observatories. For the present time the needs of the practice will be satisfied with collections like those of Auwers, Boss, Safford, and similar astronomers.

After all, if an international accordance were proposed exclusively to cause the four great ephemerides to adopt a uniform system of constants and of fundamental stars (as it appears to have been decided at Paris), without taking care at the same time that the reasons for the preference granted to such a system should consist in the undeniable superiority of it with regard to every other pre-existing, one might say that the agreement shows the absurdity of losing time, labour and money in the compilation of four different ephemerides, whilst one alone is enough for the needs of astronomy and navigation. In fact, for what object are four separate bodies of calculators employed to draw from planetary tables the places of the sun, moon and other bodies of our system? Would not one office alone, even international, be more than enough, and one sole almanac, published in several languages? And would it not be convenient to profit by the occasion to separate more clearly than has hitherto been done what is necessary for astronomers from that which is sufficient for geodetical observers, for geographers, for sailors? The papers in the *Astronomical Journal* touch this matter with great ability, calling attention to the fact that the national ephemerides (except, perhaps, that of Berlin) show rather too much the effects of their practical destination; if this could be fused with the supreme scope of astronomy some centuries ago, when the decay of astrological tendencies obliged science, from reasons of self-preservation, to find for herself a utilitarian basis, it is not at the end of the nineteenth century that she should found on its applications the justification of her existence. Like geometry, like all positive and speculative knowledge, like fine art, even the science of the stars aims especially at the honour of the human mind, and, from this point of view, the discovery of Neptune is worth as much as the discovery of a new salutary remedy or of a new electrical engine. On the other hand, the positions of the stars and planets are now known and calculated with a precision far superior to that which suffice for the applications. An immediate accord like that of Paris seems thus superfluous for applied astronomy and premature for pure astronomy. In any way, I agree with Messrs. Boss and Chandler in the view that if an agreement is to be come to, it is not in the form in which it was given at Paris. American astronomers justly note that the bureaucratic governmental character of the four offices publishing the ephemerides is not a sufficient title for them to represent all official and private astronomical science of the different countries in the definition of a merely scientific controversy of such moment. The directors of the four ephemerides indisputably occupy an eminent position amongst their colleagues; but, whatever may be their personal merits, their opinion (in a matter that touches the foundations of science) is not such as to impress itself authoritatively and without discussion.

Moreover, I allow myself to add that such an opinion cannot bind that of the numerous astronomers that belong to countries where ephemerides are not published, and where they are compiled (as at Trieste and at San Fernando) second-hand. The resolution adopted by the organisers of the Conference of inviting Messrs. Gill and Backlund, with a deliberative vote, Messrs. Van de Sande Bakhuyzen and Trépied with a consultative vote, does not seem to me to represent anything more than a well-deserved homage rendered to those learned astronomers, and perhaps might contribute to render more significant the exclusion of other countries, such as Austria-Hungary, Italy, Sweden, Norway, Denmark, the Argentine Republic, whose astronomers till now have strongly and efficaciously contributed to the theoretical and practical study of the arguments. As to what refers specially to my own country, it may not be inopportune to recall to mind the ancient series of ephemerides published, first at Bologna, afterwards at Milan, which was interrupted twenty-five years ago in order not to lose time and money by repeating what was abundantly done at Paris, Berlin, London and Washington. The history of the Milan Observatory in this last quarter of the century, proves that the promises made by its director Schiaparelli were not vain, that thus the Observatory might "dedicate itself with greater alacrity to those researches that constitute the real progress of science" (*consacrarsicon maggiore alacrità a quelle ricerche, che costituiscono il vero progresso nelle scienze*).

Finally, I believe that the discussion on the conclusions and aims of the Paris Conference should be continued by correspondence in scientific periodicals, as well as by direct treaty between the more competent bodies. And, perhaps, it would not be without some utility were the Royal Astronomical Society and the *Astronomische Gesellschaft* to agree to examine and extend the plan devised some few years since by Dr. Gill for an international Congress of Astronomers, by which alone the deliberations of the Conference could have full and authoritative sanction. FR. PORRO.

P.S.—The present paper was already finished when I read, in No. 413 of the *Astronomical Journal*, Prof. Newcomb's reply to the criticism of Prof. Boss. Notwithstanding the reasons strenuously advanced by the learned astronomer of Washington in support of his proposal of a new value for precession, it does not seem doubtful that the question must be considered from a wider and more general point of view. No one contests the delicacy and the rigour of the procedure adopted by Newcomb in drawing out his precession: no one denies but that he has treated the difficult argument in a masterly manner, enlightening it with his original and profound views. Where it seems to me that Boss dissents from Newcomb is in the opportunity of expending such talent and labour about a material already exhausted and not susceptible of giving more sure results, in whatever way it be treated. In any way, even accepting the Newcomb's new contribution to the study of the particular question of precession with the praise due to it, the general question still remains open. —FR. P.

The Treatment of Stamp Battery Slimes from Gold Ores.

ON page 501 of your issue of September 23, there is given a brief abstract of a paper read by myself at the July meeting of the Chemical and Metallurgical Society of South Africa.

The essential features of the paper have hardly been correctly rendered in the condensation, inasmuch as at present mechanical stirrers are employed for agitation of slime-pulp, jets of air serving merely for oxidation, though their use as a means of agitation is suggested.

The primary use of aeration is described in the paper as the oxidation of FeS, FeO_2, H_2, and other reducers, so as to effect a preliminary preparation of pulp before adding cyanide; hence the KCy is not protected by the presence of FeS, which, with other ferrous compounds, has already undergone oxidation and become converted to ferric hydrate, in which state it neither consumes cyanide nor abstracts oxygen.

The CO_2 in the air blown through the pulp is neutralised by the free alkali (CaO_2H_2) present, which thus serves to protect the KCy from decomposition. W. A. CALDECOTT.

Johannesburg, November 8.

Abnormal Colours of Flowers.

WITH reference to your correspondent's communication in NATURE for December 2, on abnormal colours of flowers, I fancy the following note may be of interest. Towards the end of

August 1894, near the Bernina Hospice in Switzerland, I came across several plants of myosotis growing by a pool. In some cases the flowers were a bright blue, but in others they were distinctly pink, several being entirely pink, and others showing pink blotches and lines. The plants were in a hollow, and on the day of my visit there was an extremely cold wind blowing.

London, December 2. HECTOR COLWELL.

Fire-fly Light.

REFERRING to my reply to Prof. S. P. Thompson (NATURE, July 29), concerning fire-fly light, I can confirm what then I wrote. Mr. H. Muraoka has sent to me from Kioto in Japan a letter from which I derive the following particulars. (1) In the neighbourhood of Kioto there are about nine kinds of *Luciolæ*, which Mr. H. Muraoka continues improperly to call, in German, Johanniskäfern. (2) The insects used by him were probably *Luciola vitlicollis* and *Luciola picticollis* (Kiesenn).

Florence, December 1. CARLO DEL LUNGO.

AN ENGLISH BEAVER PARK.

SINCE the Marquis of Bute established a colony of beavers on his estate near Rothesay in 1874, no such interesting experiment has been made in acclimatising these animals as that which Sir Edmund Loder has carried out in Sussex. The beavers have now been inhabitants of his park at Leonardslee, near Horsham, for eight years, or rather they occupy an enclosure inside the park. There they have been placed on the banks of a small stream, with a rather rapid fall, a situation which exactly suits them. It is sheltered, for the valley is deep and wooded, and there was an ample supply of timber, large and small, in the enclosure when the industrious beavers, reversing the story of "Settlers in Canada," were brought from Canada and settled in Sussex. In the course of their eight years' sojourn they have ensured their comfort by constructing in great perfection, and in the most durable form, the engineering works for which beavers are so justly famed, and which gave rise to the Indian legend that the Creator, after separating land from water, employed gigantic beavers to "smooth" the earth into shape. Meantime the colony increases in number, so that some of the produce have been sold to go elsewhere. Nevertheless the beavers' industry is such that the size of their works, and consequently the area of the pool which they have formed, constantly increases.

The space in which they were originally enclosed was less than an acre. This was only one-third of the size of the Marquis of Bute's "beaver park"; but it gave quite sufficient scope for the beginnings of the colony. It was surrounded with a corrugated iron fence, which the beavers could not gnaw down, while at the same time they could not see through it, and so felt more secure and "private" in their park. Beaver engineering is directed entirely to one end. This is to form a pool deep enough and wide enough for them to be able to swim beneath the water to the entrance of their burrow, and to keep this entrance submerged in dry weather, when the streams run low, and covered with such a depth of water that even in the longest frosts, when the ice in Northern Canada is two feet thick, there shall still be water-space below it.

In the water the beaver knows it is safe; and, though it also stores branches for food below water, fastening them down with stones and mud, it is to serve as a place of refuge rather than as a storehouse, as a combined moat and temporary hiding-place, that the beaver forms his pool. All his clever engineering, his wood-cutting, building, canal-making and construction of "rolling ways," are subordinate to this end. The two last works, the beaver canal and the beaver road—the one for floating, and the other for rolling logs to the pool—are only brought into play when the supply of timber near at hand is exhausted. But they are part and parcel of beaver devices, and, though only recently brought to notice, are not less creditable than their other feats.

In the present paper we shall not use the technical phrases of hydraulic engineering, but term the reservoir made the "pool," and the containing barrier the "dam." In Canada, when the beavers were numerous, these dams were noticed to be so nicely adjusted in form to the material with which they were made, and to the force of the stream which they barred, that they could be classified in relation to these circumstances. Dams built mainly of mud and stones had a different section from dams built of sand and wood; and some made across rapid streams were curved, to resist the extra strain. But the greater number were made of battens of wood about three feet long, with the crevices stuffed with mud, stones, and the twigs and small branches; and in every case the first engineering principle necessary in the construction of a dam is observed. This is that the top shall be exactly level, so that the water of the pool, which must overflow, because the stream enters it from the top end, shall flow evenly over the whole length of the dam. As every one conversant with that most difficult form of the profession—river engineering—knows, any small gap or inequality soon ruins a dam. The water pours through these by preference, and at once cuts a gap. The beavers know this, too, and at Leonardslee, no less than in Canada, constantly examine the top of the dam, and mend the smallest gap along the line. The Leonardslee dam is of the ordinary kind, not curved but straight, and built of battens of wood, made of the boughs from trees cut down inside their enclosure, or from those which were given them as food. In all cases they ate most of the bark; then they cut the sticks into lengths of about three feet, and worked them into the structure. Plenty of mud was pushed into the crevices on the upper side, and all the small twigs and sticks were pushed in to make the whole dam tight. With great judgment they spared a small oak growing just below the dam. This now acts as a support to the structure; all the other trees in the enclosure, except those protected by metal guards, and one very large fir, were either felled, or attempted to be felled. It seems obvious that they kept this tree purposely as a buttress; for the dam is made higher and, therefore, wider each year, as the pool above increases; the tree is now almost in the centre, and its roots are already worked into the dam foundations. Even the baby beavers at Leonardslee, no bigger than rabbits, are put to "light jobs" in mending the dam, and the elders are most industrious. Each winter brings down a quantity of mud, which would make the pool shallower. But the beavers raise the dam so rapidly that the pool gains in depth, and spreads for a long distance up stream and laterally. The dam is at least five and a half feet high, and the depth of water above it five feet, yet it is so well made that, though the human-built dams of several artificial pools higher up the stream were carried away in a winter flood, the beaver-dam was undamaged. Near the point at which the stream enters the enclosure three large trees, formerly on the bank, are now submerged in three feet of water, owing to the fresh height added to the barrier below. The beavers had begun to cut these trees down—a very hard task, but one in which they would have succeeded had not the water risen so fast that they were floated off their legs when trying to go on cutting. One large beech tree, standing on a raised bank washed by the ever-increasing pool, was an object of envy to the beavers. They concluded that the quickest plan was not to cut it down, as it was very large and the wood hard, but to dig it up. So, with the aid of the increasing waters, they undermined the tree, which fell across their pool. This gave them occupation for some weeks. They ate every morsel of bark off the trunk and branches, and then cut off the boughs, gnawed them into lengths, and took them to the dam and to their "lodge." The latter began as a burrow in the bank. As they gradually enlarged this, and filled up the bottom with wood chips, they broke open the roof to get head room, and constantly added to the dome with mud and sticks. It is now a large untidy mound on the bank, which at this point is steep.

The process described above accounts partly for the evolution of the beaver lodge from the burrow. But the keeper of Lord Bute's beavers stated that the beavers at Rothesay did clean out the old shavings which they took in to make beds of, and plastered them on the outside of their lodge, or on the same embankment. The present writer inclines to believe that this is likely to be correct, not in every case, but in some, for it is very much in keeping with beaver character. In such cases he ventures to offer the following solution of the growth of the lodge, in cases where the beavers remove their bedding of chips. The water above the dam, owing to the causes mentioned above, rises higher yearly. This must also raise the level in the sub-aqueous passage leading to the beavers' chamber, and in time tend to invade the chamber itself. To remedy this the beavers would naturally raise the floor, and leave the débris of old "beds" on it, piling fresh stuff on the top, and at the same time quarry out the roof, both for head-room and to get fresh earth for their floor. When once the artificial roof was made the same process would go on, until the rising water flooded out the floor altogether, and surrounded the lodge, as one sees it in old Canadian pictures. The lodge would thus take the beehive, moated, form which it has in those conventional plates, even though it lacks the windows which the French artists added for effect. The beavers occasionally escape by burrowing under the corrugated iron fence. This is not always intentional on their part, and they are easily caught again. Whenever one gets out it travels up the stream, visiting the pools above. There a box-trap is set next night, baited with dog-biscuit, of which the beavers are fond, and the animal is certain to be caught. One beaver, out for a stroll like this, tried to cut down a large Scotch fir, and did cut down a silver birch. The first indication to the keeper next morning that a beaver had escaped was the sight of this tree, in full leaf, lying across a path.

We mentioned above that the Leonardslee beavers had not made either a canal or a rolling path. There is no need for either; for there are no more trees to cut down, or logs to roll from a distance. But it is worth devoting a few lines to these, two of the less known, but not least extraordinary exhibitions of beaver intelligence. An old-established colony soon clears off all the timber near its home. In order to convey the more distant logs to the dam they carefully clear paths, and roll the battens of wood down to the water. But the beaver canal, to which proper attention was first drawn by Mr. Lewis Morgan, is, in the writer's opinion, more wonderful still. It is nothing more nor less than a waterway, or several waterways cut from points on the stream to such parts of the adjacent plantations or woods as the beavers wish to visit, or cut timber in. Sometimes, also, it is made through the centre of an island, to make short communication by water. To this canal the beavers roll their logs, and then tow them to their dam. It has been urged that these canals are accidental, merely worn out along the customary roads; some are, no doubt. But wherever the writer has seen tame beavers kept, even at the London Zoo, he has seen them at different times cut trenches to the edge of their pool, though as that at the Zoo was surrounded by a stone rim, the beavers could not fill the trench with water. Of course, there this trench was only a few feet long. But it is quite clear that Lord Bute's beavers, which had a considerable area in which to cut their timber, tried to dig canals. This is the description given by their keeper, who, though well acquainted with beaver stories, evidently had never heard of the canal. "Their burrows they make by cutting a road from the middle of the dam for several yards into the dry ground, where they scoop out a dome-shaped burrow. . . . Some of the roads to these burrows are from fifteen to twenty yards long, *and so level that the water follows them in the whole length*"! Here is, undoubtedly, an instance of the beaver canal.

Those who care to compare the methods of the Canadian beavers acclimatised at Leonardslee with the works and ways of the European beavers still surviving in Norway, will find an interesting account of the past and present history of the Norwegian beaver, by Mr. R. Collett ("Bieveren i Norge"),[1] illustrated with twelve plates of beaver lodges and dams, and supplemented by an English summary.

The beavers' present range is confined to the Stifts of Christiania and Christiansand, but a few remain on Bratsberg Amt and Stavanger Amt. The larger colony is on the River Nisser (or Nid), the westernmost colonies being on the river Mandal. In all cases they feed not on fir, but on deciduous trees, mainly the aspen. When not on the banks of large rivers they make dams, one of which, near Hellersli (Trungen), was built entirely in three weeks, and formed a lakelet more than 100 yards across. The length of the dam was 14 metres. In only one respect do the habits of the Norway beavers differ from those of the Canadian species. Those that live on the banks of the large rivers cannot make a dam over such rapid and deep waters. Yet these rivers rise and fall, and there is a danger of the lodge being either flooded or left high and dry. In these places the beavers build long lodges, at right angles to the stream, and sloping up the bank. When the river rises, the beaver can go up to the higher end of his lodge; when it falls, the entrance is still submerged and safe.

In 1880 Mr. Cocks estimated the number of beavers surviving in Norway at 60; in 1883 Mr. Collett believes there were 100. Since 1894 and 1895 the beavers have been protected by law in their two principal haunts, for a period of ten years in each district.

C. J. CORNISH.

SCIENTIFIC INVESTIGATIONS OF THE LOCAL GOVERNMENT BOARD.[2]

THE Annual Reports of the Medical Officer of the Local Government Board constitute in many respects the best treatise on practical hygiene we possess. Their diligent perusal by the embryo health officer would equip him as no ordinary text-book can do for the intelligent discharge of his duties. It would be well if candidates for Public Health degrees were examined upon the salient features of these reports. The volume for 1895–96, which has just been issued, contains certain features of interest. There is the usual admirable summary of the year's work by Sir Richard Thorne; statistics with regard to vaccination, and a compilation of returns of notified infectious diseases in urban districts and in the county of London. Valuable reports are also contributed by Dr. Copeman and Dr. Buchanan upon outbreaks of enteric fever, and by Dr. Sweeting upon an outbreak of diphtheria. The able report by the late Mr. R. W. Thomson upon the sewerage and drainage arrangements of certain valleys in the counties of Monmouth and Glamorgan, leads one to join in the regret expressed at the loss of this talented official. It is, how-

[1] Bergen: Grieg's "Bogtrykkeri."

[2] Twenty-fifth Annual Report of the Medical Officer of the Local Government Board, 1895-96.

ever, the auxiliary scientific investigations which call for more special notice in these columns.

The several cases of Asiatic cholera that occurred in this country during 1893, led the Local Government Board to keep a diligent watch in the subsequent year over any cases that presented symptoms of a choleraic nature, and Dr. Klein gives an account of the bacteriological examination made by him in twenty-nine instances. The comma bacillus was not found in any of the cases; they therefore proved to be examples of cholera nostras, or English cholera. This conclusion is a further confirmation, if that were needed, of the diagnostic value of Koch's methods. When Dr. Klein says, "if the vibrio of Koch could be demonstrated in the bodies of persons not connected in any way with the cholera localities of 1893, the bacteriological test as an important help in distinguishing between Asiatic cholera and cholera nostras would become practically worthless," he is surely suggesting a doubt that does not exist amongst the great mass of observers. In a number of the cases the bacillus coli was found to be the predominant micro-organism. Of fresher interest is Dr. Klein's description of an organism isolated from cases of diarrhœal illness due to the consumption of milk, and named by him, "Bacillus enteritidis sporogenes." There is a misleading sentence on p. 196: "ordinary milk, such as is bought in many a shop in London, contains as a rule an abundance of bacillus coli, and if put aside it will rapidly 'sour' and coagulate spontaneously owing to the multiplication of bacillus coli." This might lead most readers to assume that the ordinary "souring" of milk is due to bacillus coli, and not, as is the case, to one or other of a considerable number of lactic acid producing organisms. Dr. Klein proceeds to a further report on prophylaxis in diphtheria, in which are mainly detailed the results of injecting living diphtheria cultures into the horse with the view of obtaining antimicrobic substances in the blood. The experiments do not appear to have brought out results of practical value, either with his own "antimicrobic" serum, or with antitoxic serums obtained from various sources. Dr. Klein also contributes a "Further Report upon Protective Inoculations" which will be read by all interested in the subject of which it treats.

"Snake venom in its prophylactic relations with 'poison' of the same and of other sorts," is the subject of a lengthy communication by Dr. Kanthack. The work of Calmette and others upon the effect of cobra antitoxic serum upon the poison of snakes other than the cobra is discussed, and there follow observations upon the nature of the immunity of certain snakes to certain snake poisons. Dr. Kanthack has not been able to confirm Calmette's statement, that the injection of solutions of chlorinated lime have value as a curative or immunising agent. On the other hand, as Calmette states, an antitoxic serum can be produced in rabbits by administration of the cobra poison itself, and still better results are obtained by using the mixture of venom and chlorinated calcium. The inhibitory influence of different antitoxic serums upon cobra venom is next dealt with. The experimental data given are in many respects incomplete, and it would perhaps have been well to wait till the experiments were finished before publishing any conclusions from them.

Dr. Kanthack instances the marked retarding influence of liver extracts on the action of cobra venom. This most interesting statement is supported by three experiments, and their continuation left for a future date. There follow notes on "immunising serum." It is again disappointing to find experiments quoted with the addendum, "the number of my observations are too limited." Attention should be drawn to an experiment Dr. Kanthack performed on himself with most remarkable results. In the course of nine days Dr. Kanthack swallowed 51·4 cc. of living and dead broth cultures of the cholera vibrio, and a solution of sodic carbonate to neutralise the gastric acidity. A week later blood was obtained from the arm, and 3 cc. of the separated blood serum was injected into each of three guinea-pigs intraperitoneally. Four days later the guinea-pigs were respectively inoculated with *B. prodigiosus*, *B. pyocyaneus* and *V. choleræ*—*only* the guinea-pig inoculated with cholera remained alive. The immunisation Dr. Kanthack had effected on himself *per os*, was apparently transmitted by means of his blood serum to the guinea-pig.

The paper concludes with notes upon Pfeiffer's, Bordet's, and Durham's tests for the cholera and typhoid organisms. It would have rendered the description clearer in one or two instances if it had been explained what actually was seen, instead of expressing the results as "not quite negative" and "did not react absolutely negatively." Dr. Kanthack considers that "a general conclusion at this stage as to the specificity of antitoxic or immunising serum would be premature," though the facts so far obtained tend to support this conclusion. The paper is one more adapted for perusal by specialists intimately acquainted with the subject than by the general reader, who might be apt to carry away false notions as to the finality of a number of the results obtained. One cannot resist the impression that the material for half a dozen researches has been crowded into the space of one paper.

The difficult subject of food poisons is considered from the bacteriological aspect by Dr. Cautley. The object of the inquiry was to ascertain whether the multiplication or pathogenic properties of bacteria normally present in the digestive tract, viz. *B. coli* and *Proteus vulgaris*, are influenced by association with the non-pathogenic organisms to be commonly met with in food. Dr. Cautley was able to demonstrate that the virulence of the above bacilli is increased by simultaneous injection with certain food organisms into animals. On the other hand, feeding experiments on similar lines gave negative results. Dr. Monckton Copeman and Dr. Blaxall contribute reports on the advantages of glycerinated vaccine lymph. The volume, like its predecessors, contains much of interest and value to sanitarians and bacteriologists.

THE ARCTIC WORK OF MR. R. E. PEARY.

ON Monday evening Mr. R. E. Peary, U.S.N., presented to the Royal Geographical Society a statement of the results of his Arctic explorations and of his plans for continuing these.

Mr. Peary has been engaged in exploring Northern Greenland for the greater part of the last ten years, and in the course of his sledge journeys he has had more opportunities of studying the inland ice than has fallen to the lot of any other explorer. The most remarkable journey of the series was that of 1892, when he crossed the ice-cap from Inglefield Gulf and discovered Independence Bay on the north-east coast of Greenland. This journey showed that the inland ice terminated to the north, and that there was no road that way to the pole. Land was seen to the northward, which Mr. Peary believes to be separated by a channel from Greenland, and to extend probably some distance beyond the furthest north attained by Lockwood on Greely's expedition, 83° 24′. A second journey to Independence Bay in 1893 was frustrated by the unexampled severity of the weather; the party, which started with forty-two dogs returned with only one. In 1895 another attempt was more successful, but the resources of the expedition and the transport available were insufficient to allow any advance beyond the position of 1892.

After this journey Mr. Peary resolved to wait until the results of the Nansen and the Jackson-Harmsworth expeditions were available as a guide to further work, but he had no intention of abandoning his endeavour to attain a high latitude on the American side. This summer he visited Cape York in a whaler, and succeeded in taking on board and transporting safely to New York a vast mass of nickel-iron weighing about 100 tons. This block is reputed by the Eskimo to have fallen from heaven, and it bears all the external appearance of a meteorite, while it shows the characteristic Widmannstätten figures when the surface of the polished metal is etched with nitric acid. The transport of the block was a brilliant engineering feat, as the only means available for getting it brought on board and lowered into the hold were hydraulic jacks and timber stagings. We understand that the great meteorite is still the property of Mr. Peary, and has not yet been acquired by any museum. It is not to be confounded with the masses of telluric iron described by Baron Nordenskiöld.

Mr. Peary's description of the inland ice was extremely graphic, and illustrated by a superb series of coloured slides, prepared from photographs selected from his collection of over 4000. The movement of the surface he believes to be due as much to the drift of snow by the wind as to the discharge of glaciers, summer melting and evaporation taken together. The snow-dunes in parts convert the inland ice into a veritable Arctic Sahara. In addition to collections of the restricted but brilliant flora of the ribbon of land freed from snow in summer along the coast of Greenland, much valuable scientific work was done. Ethnology is the branch of science which has perhaps been most advanced. The Northern Eskimo, the Arctic Highlanders of Sir John Ross, have been most thoroughly studied and photographed. The entire tribe numbers some 250 individuals, every one of whom is personally known to Mr. Peary. They have lived quite untouched by civilisation, and free even from communication with the Southern Eskimo. They are of good physique and, while far from attractive in appearance or customs, they possess many good qualities.

Mr. Peary, as the result of his experience of these people and of the conditions of Arctic travel, has formulated a scheme of progressive exploration which he will put into practice next year. Equipped by funds that are already provided, he proposes to take a ship up to Sherard Osborn Fjord in latitude 82°, taking two other white men and a picked group of eight or ten Eskimo with all their belongings. They will send the ship back and form a camp, advance by the autumn moonlight along the coast, and winter as far north as possible. In the following year the northward march would be resumed until the furthest point of land was reached. This he hopes to find in 85°, if not even further north. From the camp at that point as a base a rapid journey must be attempted over the sea ice to the pole and back, leaving most of the Eskimo behind. If the first year did not afford an opportunity the next year might, or at least the third. Mr. Peary's principle is to wait for favourable conditions, and he is prepared to remain at his most northerly camp five years if necessary. In the course of that time he believes that a door to the pole will open, or can be pushed open. This principle has always been approved by the leading Arctic men, and recent experience has amply confirmed the importance of not struggling against obstacles when it is possible, by skill or patience, to take advantage of natural aids.

Mr. Peary's project has in it nothing sensational; he holds no prospect of startling discovery before the public; and the fact that his expedition is already provided for, obviates any necessity for making an appeal for financial support. The enthusiastic character of his reception by a large and deeply interested audience can only be paralleled in recent years by the greeting accorded to Dr. Nansen.

NOTES.

IT has been decided that the statue of Lavoisier, for which international contributions have been subscribed, shall be erected on the Place de la Madeleine, Paris. The Municipal Council of Paris have sanctioned this site.

M. DITTE, professor of chemistry at the Sorbonne, has been elected a member of the Section of Chemistry of the Paris Academy of Sciences, in succession to the late M. Schützenberger.

PROF. F. R. JAPP, F.R.S., will deliver the Kékulé Memorial Lecture at the Chemical Society next Wednesday evening, December 15.

AT the next meeting of the Royal Photographic Society on Tuesday, December 14, Prof. Gabriel Lippmann will read a paper on "Colour Photography." The meeting will not be held in the usual rooms at Hanover Square, arrangements having been made for it to take place in the theatre of the Society of Arts; a large gathering is anticipated. Non-members of the Royal Photographic Society will require admission tickets, which can be obtained on application to the Secretary at 12 Hanover Square.

WE regret to see the announcement that Prof. Winnecke, the distinguished astronomer, died on Friday last at Bonn.

LORD STRATHCONA AND MOUNT ROYAL, High Commissioner for Canada, will preside at Prof. Roberts-Austen's lecture on "Canada's Metals" at the Imperial Institute next Monday evening (December 13). The lecture, which will be fully illustrated by experiments, is open to the public, without tickets.

IT has been resolved to hold an international fisheries exhibition in Aberdeen in the summer of 1899.

A SKELETON of the moa was sold at Mr. J. C. Stevens' auction rooms on Tuesday, the price reached being forty-eight guineas. The bird was set up by Captain F. W. Hutton, F.R.S., from bones obtained at Enfield, New Zealand. The deposits in which these *Diornis* remains were found was described by Dr. H. O. Forbes in NATURE of March 3, 1892 (vol. xlv. p. 416).

A GENERAL meeting of the Aeronautical Society of Great Britain will be held on Thursday, December 16, at 8 p.m., at the Society of Arts, John Street, Adelphi, W.C. Major-General Sir Charles Warren, G.C.M.G., K.C.B., will occupy the chair. The following gentlemen and others have kindly arranged to exhibit apparatus:—Mr. Pilcher, a soaring machine and an oil engine; Major Moore, a model flying machine; Mr. E. S. Bruce, balloon signalling apparatus; Captain Baden-Powell, kites and a gliding machine; the Society, cinematograph of flying birds.

WHEN a large number of crickets are chirping at night in a field, they do so synchronously, keeping time as if led by the wand of a conductor. Prof. A. E. Dolbear says, in the *American Naturalist*, that the rate of chirp seems to be entirely determined by the temperature, and this to such a degree that the temperature can be estimated when the number of chirps per minute is known. At a temperature of 60° F. the rate was found to be 80 per minute, and at 70° F. it was 120 a minute; this gives a change of four chirps per minute for each change of one degree.

THE following are among the lecture arrangements at the Royal Institution before Easter:—Prof. Oliver Lodge, six Christmas lectures (specially adapted for young people) on the principles of the electric telegraph; Prof. E. Ray Lankester, eleven lectures on the simplest living things; Prof. Dewar, three lectures on the halogen group of elements; Prof. J. A. Fleming, five lectures on recent researches in magnetism and diamagnetism; Prof.

Patrick Geddes, three lectures on Cyprus. The Friday evening meetings will begin on January 21, when a discourse will be given by the Right Hon. Sir John Lubbock, Bart., M.P., on buds and stipules. Succeeding discourses will probably be given by Prof. C. Lloyd Morgan, Mr. A. A. Campbell Swinton, Dr. J. Hall Gladstone, Prof. L. C. Miall, Captain Abney, Prof. T. E. Thorpe, Mr. James Mansergh, the Dean of Canterbury, Prof. Dewar, and others. Lord Rayleigh will deliver lectures after Easter.

At the meeting of the General Medical Council last week, the Committee entrusted with the duty of preparing the new edition of the British Pharmacopœia, submitted copies of their draft of the work for the approval of the Council. The estimated amount to be expended on its production will be about 6000*l*. The Committee suggest that the price of the volume might suitably be seven shillings and sixpence, but the power of actually fixing the price rests with the Commissioners of the Treasury. In order that the Pharmacopœia might be issued early next year, the Committee recommended that it be delegated to the Executive Committee to adopt the fully corrected work as The British Pharmacopœia, 1898, to communicate with the Commissioners of the Treasury on the question of price, to publish the Pharmacopœia, and to make the usual legal announcement of publication. The report of the Committee was adopted.

The difficulties in the way of a permanent establishment of the Essex Field Club's County Museum of Natural History have been very largely removed by Mr. Passmore Edwards' munificent offer to build a museum at West Ham (adjoining the new Technical Institute) to contain the Club's collections, on condition that the building is maintained by the Corporation of West Ham as a permanent institution, and that it is opened on Sundays. The Town Council have gratefully accepted Mr. Passmore Edwards' offer, and arrangements are being entered into whereby the annual upkeep of the museum will be provided for by a joint fund subscribed by the Corporation and the Club. The task of gathering the collections, and the scientific control of the same, will remain with the Club. The museum is intended to illustrate the county of Essex generally, while the Club's smaller museum in Queen Elizabeth's Lodge, Chingford, will be confined to objects from Epping Forest—the two institutions being mutually dependent and illustrative, the one of the other. Further details will be issued when the negotiations are completed.

At the last meeting of the Council of the Australasian Association for the Advancement of Science, the following gentlemen were proposed as vice-presidents of Sections:—Section A (astronomy, mathematics, and physics), Prof. A. M'Aulay and Prof. T. R. Lyle; Section B (chemistry), Mr. R. T. Bellemey, Prof. E. H. Rennie, and Prof. Orme Masson; Section C (geology and mineralogy), Mr. W. Howchin and Mr. R. L. Jack; Section D (biology), Prof. J. T. Wilson, Prof. C. J. Martin, and Mr. J. J. Fletcher; Section E (geography), Mr. P. G. King and Mr. A. C. Macdonald; Section F (ethnology and anthropology), Prof. W. Baldwin Spencer, Mr. Thos. Worsnop, and the Rev. Lorimer Fison; Section G (economic science and agriculture), Dr. H. N. MacLaurin, Mr. R. Teece, Mr. W. M'Millan, Mr. Sydney Smith, Mr. E. M. Shelton, and Mr. W. Farrer; Section H (engineering and architecture), Mr. H. Deane and Prof. W. H. Warren; Section I (sanitary science and hygiene), Dr. D. Hardie, Dr. J. W. Springthorpe, and Dr. J. Ashburton Thompson; Section J (mental science and education), Dr. A. Garran, Dr. R. N. Morris, and Mr. R. H. Roe. A large number of additional papers were announced as having been promised to the various Sections.

The Meteorological Council have published a very useful volume of Rainfall Tables for the years 1866–90, based upon observations at 492 stations, selected so as to show as nearly as possible the general distribution of rain over the United Kingdom. The present volume has been prepared in connection with the previous set of tables for 1866–80 published in the year 1883, and while it includes in the general averages the monthly and annual means of the earlier series, it contains the separate monthly and yearly values for each of the years 1881–90, and of any years prior to 1881 which were not previously published. To facilitate the use of the tables, references to them are arranged both under counties and stations. There is no discussion of the observations, but the averages for the years 1881–90 are shown upon three maps, together with the main watersheds and the catchment basins of the principal rivers, and these exhibit the general distribution of the rainfall very clearly. The whole work has undergone careful supervision, and will be very valuable as a standard of reference in this branch of meteorological statistics.

The meteorological observations made at the Rousdon Observatory, Lyme Regis, during last year, have been brought together by Mr. Cuthbert E. Peek, and published in the form of a Report. The observatory is a second order station of the Royal Meteorological Society. Mr. Peek not only describes the meteorological conditions and statistics of the different months of the year, but also records the results of a number of experiments to compare rain-gauges and anemometers of different kinds. A daily comparison of the actual weather experienced at the observatory with the forecasts of the Meteorological Office showed that, taking wind and weather together, eighty-eight per cent. of the forecasts were correct. Of wind alone, ninety-one per cent. of the forecasts were fulfilled; and of weather, ninety-two per cent. The most trustworthy forecasts were in August, there being only one day doubtful during that month. The lowest percentage of trustworthy forecasts was in January, as might be expected from the remarkable barometric movements of that month. A comparison of forecasts and actual wind and weather for the thirteen years 1884–96, shows that the percentage of successes was only fifty-nine in 1884, but in the following years it reached seventy per cent., and has been increasing year by year ever since. The best forecasts seem to have been made in 1894 and 1895, in which years eighty-nine per cent. of the predictions were fulfilled.

Dr. Van der Stok, the Director of the meteorological and magnetical observatory of Batavia, has published a very comprehensive atlas, containing a large amount of information on the winds, weather, currents, &c., of the East Indian Archipelago. Some idea of the magnitude of the work may be formed from the fact that it extends to more than two hundred large folio pages of tables and charts, and that it embraces the whole area between the western coast of Sumatra and the northern coast of New Guinea. For forty different points of this area the meteorological conditions have been deduced for each month and for seasons, while monthly wind-roses have been drawn for the day and night separately. The work is divided into three parts: (1) observations made on ships; (2) rainfall and wind observations made on land; and (3) data relating to tides. The materials made use of in the first part have been extracted from many hundreds of log books kept on Dutch men-of-war during the years 1814–1890, and were obtained from the Ministry of Marine at the Hague for the purpose of this discussion. The work is a valuable contribution to both the physical geography and meteorology of that portion of the globe, and will be of great practical use to navigators.

THE first occasion upon which Röntgen rays were applied to surgical diagnosis was referred to by General Maurice in the course of a few remarks made at the close of a lecture delivered at the Royal Artillery Institution, Woolwich, by Mr. J. W. Webster. The credit of having first used the rays to determine whether a patient (who had severely injured his elbow) was suffering from a fracture or a dislocation, has hitherto been given to Mr. Howard Marsh. It appears, however, that Mr. Marsh really only recommended the application of the rays to the case, and that the actual photograph was taken by Mr. Webster, who had been working for some time with Mr. T. Moore. The photograph showed distinctly that the injury was due to a dislocation, and was not a fracture at all. The diagnosis having been established, the operation of putting the arm in its place was performed by Captain Salvage, an army surgeon. Mr. Howard Marsh described the case in the *British Medical Journal*; but, says General Maurice, "as he was simply dealing with it as a scientific case, the names of Mr. Webster and Mr. Moore were, according to professional etiquette, necessarily omitted." The lecture from which we gather these facts appears in the *Proceedings* of the Royal Artillery Institution (No. 5, vol. xxiv., 1897).

THE Annual Report for 1896 of the State Board of Health of Massachusetts has just been issued. The work of the now historic series of experimental filters has been recorded as usual, and there is but little to add to the deductions already published in previous years regarding their respective efficiency in the purification of water. The presence of iron in public water supplies has been engaging the attention of the Board, and experiments have been started which, as far as they have gone, indicate that this evil may be remedied by filtering water through a fine filter of coke breeze, which is stated to practically remove all the iron. Another important matter which has been investigated is the purification of manufacturing refuse; such factory-sewage may contain several times as much organic matter as an equal volume of domestic sewage, and is extremely difficult to deal with; in many cases the chemicals used in manufacturing processes being such as would destroy nitrification if the sewage were applied to an ordinary sewage filter. The dissemination of typhoid fever through the use of polluted water in ponds in the vicinity of so-called "picnic groves"—places of summer resort brought within easy reach of large cities by the extension of electric railways—has also been carefully inquired into, and the sanitary conditions of a large number of such resorts and their water-supplies have been investigated. Other matters dealt with in the Annual Report are the production and use of diphtheria antitoxin; an experimental inquiry as to the diagnosis, genesis and diffusion of malaria; examinations of sputum and other material suspected of containing tubercle bacilli, &c.

AT the meeting of the Chemical and Metallurgical Society of South Africa, held on October 16 last, some interesting data on dry crushing and direct cyaniding of Rand ore were given by Mr. Franklin White. Great things were expected of this method of treatment by some metallurgists a year or two ago, but its progress has been far from rapid, largely owing, no doubt, to the comparatively low percentage of gold extracted by its use. According to Mr. White, however, 917 tons of oxidised ore from Botha's Reef, treated recently, yielded 65·77 per cent. of its gold, the tailings containing 2·708 dwts. per ton. The only preparation of the ore was to pass it through a Gates crusher, after which nearly 20 per cent. remained on $\frac{1}{2}$-mesh screens. Experiments seem to show that if the material had been passed through $\frac{1}{8}$-mesh screens, about 76 per cent. of the gold would have been extracted in one operation. About 1000 tons of pyritic ore from the Village Main Reef were also experimentally treated with excellent results, one charge giving a theoretical extraction of 89·25 per cent., as deduced from the difference in the assays before and after treatment. The cheapness and simplicity of the process are much in its favour. In another paper read at the same meeting, Mr. Wilkinson estimated the present total costs of twenty-nine Rand outcrop mining companies as averaging only about 26*s.* per ton for mining, milling, development, &c., or about 4*s.* per ton less than in 1896, when the native wages were higher. A further reduction in cost of 4*s.* or 5*s.* per ton would be accompanied by an enormous increase in the profits. The life of the gold field is estimated at fifty years, and its average yield during that period at about 18,000,000*l.* per annum.

IT is stated in *Engineering* that a company of considerable importance and with an influential board has just been formed in Sweden for the purpose, principally, of exploiting, through electric transmission, the vast unused power of the famous Trollhättan waterfalls, situated at some distance from Gothenburg, Sweden. The new company is to take over Dr. de Laval's waterfalls and property at Trollhättan, a carbide manufactory at Okan, with turbines and other installations, the Edenäs waterfall in Upper Lulea, and certain of De Laval's patents and inventions. There are in both places considerable areas of land, while the water-power at Trollhättan is estimated at 220,000 horse-power, and that at Edenäs at 100,000 horse-power. There is consequently both power and space enough for industrial installations on a large scale. It is intended to build a tunnel at Trollhättan, which is to receive the water above the falls in question (for there are others above it, already partly utilised) just above the "King Oscar Bridge," through which the tunnel will lead to a power station below the "Hell Fall." It is proposed to take some 20,000 to 30,000 electric horse-power through this tunnel. Of this power the company proposes as soon as possible to make available 10,000 horse-power, of which half has already been let to a well-known electrical engineer for a period of fifteen years. Of the 3000 horse-power (effective) at Okan, a portion is already being used for the manufacture of carbides.

A SURVEY of the conditions of artificial flight, and the experiments which have been made in connection with it, is contributed to the current number of *Science Progress* by Prof. G. H. Bryan, F.R.S. It is pointed out that every one of the conditions for successful flight has been fairly satisfactorily dealt with by various experimenters; and it only remains to embody them all in a single machine. The directions in which the solution of the problem should be sought are thus summarised by Prof. Bryan:—"If any experimenter can so thoroughly master the control of a machine sailing down-hill under gravity as to increase the size of the machine and make it large enough to carry a light motor, and if, further, this motor can be made of sufficient horse-power, combined with lightness, to convert a downward into a horizontal or upward motion, the problem of flight will be solved. The first flights need not be long—a hundred yards, rising, say, twenty or thirty feet above the ground, will be sufficient; all else will be simply a matter of improving on the original model, and once success is assured workers will not be wanting. Another promising direction for success lies in an elaborate and exhaustive investigation of balance and stability, such as would allow of the safe use of motor-driven machines too large to be controlled by mere athletic agility. This might partially be acquired by experiments with models, gradually increasing in size till they were capable of carrying a man and a motor. But if the future development of artificial flight is not to continue a repetition of the chapter of accidents by which naval architects gained their theoretical knowledge, there is abundant work for mathematicians in reducing the conditions of stability of aerial machines to a matter of pure calculation. One thing is certain, till this is done designs of large air-ships for the wholesale transport of passengers, officers and cargo are not of the slightest value; their designers would do better to study mathematics, and help in the heavy work of calculation still requiring to be done."

FOR the purpose of testing whether change of chemical structure ever results in change of weight, Landolt carried out a number of experiments in the years 1890-92. Using a variety of reactions, he made a careful series of weighings, both before and after chemical combination, of substances sealed in glass tubes, but the results were inconclusive. In three reactions with silver sulphate and ferrous sulphate, producing silver and ferric sulphate, the apparent loss of weight was, on the average, more than nine times as great as the probable error of weighing, and a similar apparent loss was indicated as the result of six reactions between iodic acid and hydriodic acid. In some other reactions, however, the observed change of weight was sometimes positive and sometimes negative. A similar set of weighings for another silver reaction closely related to the one used by Landolt have now been made by Fernando Sanford and Lilian E. Ray, and are described in the *Physical Review* (October). The reaction used was the reduction of silver from an ammonia solution of the oxide, by means of grape sugar. The weighings show greater irregularities than those of Landolt, but they are still accurate enough to justify the authors' conclusion that the reaction used by them was unaccompanied by any such change of weight as was observed in the similar reaction studied by Landolt.

THE September number of *Terrestrial Magnetism*, the publication of which has been unavoidably delayed, contains, among other contributions, a paper "Ueber die Fehler bei Erdmagnetischen Messungen," by H. Wild, and a translation of Prof. Max Eschenhagen's paper "On Minute, Rapid, Periodic Changes of the Earth's Magnetism," already noted in NATURE.

A COPY of an inaugural address on "Advances in Biological Science during the Victorian Era," recently delivered by Mr. Isaac C. Thompson as president of the Liverpool Biological Society, has been sent to us. The address is an instructive review of the most prominent biological work of the past sixty years, and the nature of it shows that the Liverpool Biological Society ranks high among the scientific bodies of the city.

MR. ALFRED HARKER'S excellent little guide to the study of rocks in thin slices under the microscope, published under the title "Petrology for Students" by the Cambridge University Press, has reached a second edition. The book has been revised throughout, and more attention has been given to American examples, at least among the igneous rocks. No better introduction to the study of petrology could be desired than is afforded by Mr. Harker's volume.

THE Essex Field Club will publish about Christmas, as one of their "Special Memoirs," a volume on the "Mammals, Reptiles and Fishes of Essex," by Dr. Henry Laver, one of the Vice-Presidents of the Society. Taken in conjunction with Christy's "Birds of Essex," issued by the Club in 1891, it will afford a complete guide to the vertebrate fauna of the county. The work will be illustrated by original drawings by Major Bale, of Colchester, and Mr. H. A. Cole, of Buckhurst Hill, depicting some of the more interesting species and their haunts in the county.

THE magnetic observations made at 509 places in Asia and Europe during the period 1867-1894, by Dr. H. Fritsche, Emeritus Director of the Russian Observatory at Pekin, have been brought together and published as a pamphlet in which the whole of the MS. is reproduced in facsimile. The first part of the pamphlet deals with the formulæ used in the calculation of horizontal intensity, and following it are tables showing the magnetic elements at 509 places in Europe, Siberia, and China, the longitudes and latitudes of the places being given, and also the dates of observation. Magnetic anomalies are discussed, and two local anomalies—one near the island of Ioussar-oe in Finland, and another near Moscow, are described in detail, and illustrated by three maps.

WITH reference to the note on p. 59 of our issue of November 18, as to the storm signals used by various maritime countries, which was based upon information contained in the U.S. Pilot Chart of the Pacific Ocean for November, we have received a note from Admiral Capello pointing out that, in addition to the use of flags of the Commercial Code and occasional semaphoric signals for the purpose of giving notice of the state of the weather between the Bay of Biscay, Madeira and Gibraltar, to passing vessels that may require it, the drum and cone signals adopted by the late Admiral FitzRoy are regularly hoisted at the semaphores to notify the probable approach of stormy weather.

THE additions to the Zoological Society's Gardens during the past week include a Black-handed Spider Monkey (*Ateles geoffroyi*, ♀) from Central America, presented by Mr. F. Colsell; two White-naped Weasels (*Pœcilogale albinucha*) from South Africa, presented by Mr. W. Champion; two Flat-backed Terrapins (*Cyclemmys platynota*) from Johore, Malay Peninsula, presented by Mr. S. S. Flower; two Chameleons (*Chamæleon vulgaris*) from North Africa, presented by Mr. Horace Dibley; two Ring-tailed Coatis (*Nasua rufa*), a Kinkajou (*Cercoleptes caudivolvulus*), a Punctated Agouti (*Dasyprocta punctata*), a Globose Curassow (*Crax globicera*) from British Honduras; two Vervet Monkeys (*Cercopithecus lalandii*, ♂ ♀) from South Africa, deposited; two Scaup Ducks (*Fuligula marila*, ♂ ♀), European, purchased.

DR. C. BÖRGEN asks us to state that in his letter on "The Law of Divisibility," which appeared in NATURE of November 18 (p. 54), he should have written $\Sigma(a_{v+a-1}$ instead of $\Sigma(a_{v-a+1}$.

OUR ASTRONOMICAL COLUMN.

METEORS (GEMINIDS).—The interest awakened by the recent expectation of a brilliant return of the Leonid meteors may induce many to make observations of other showers. It may be well, therefore, to remember that from the 9th to the 12th inst. the fairly good shower of the Geminids takes place. The radiant point is situated near Castor in R.A. 7h. and north declination 32°. In the years 1885 and 1892 there were remarkably good displays.

o CETI (MIRA).—In this column last week reference was made to the brightness of "Mira" near its maximum. Further observations made at the Solar Physics Observatory, South Kensington, show that, as at previous times, the light curve of this star is subject to fluctuations near maximum, but the star is still about the third magnitude, being considerably brighter than γ Ceti (3·38 N.O.U.). Only visual observations of the spectrum have been possible in consequence of the troubled state of the weather, which has not allowed of the long exposures necessary to secure the violet portion of the spectrum; the visual observations show the same bands as recorded by Sir Norman Lockyer at Westgate in 1888 (NATURE, vol. xxxviii. p. 61). This year the star will be well situated for observation for a considerable time after its brightest period, before being lost in the twilight, as it is now on the meridian about 9.30 p.m. We hope later to be able to give some additional facts as to the change which takes place in the invisible portion of the spectrum in passing from maximum to minimum.

THE COMPANION OF SIRIUS.—Prof. Burnham, using the 36-inch Lick refractor, was the last to observe the companion of Sirius before its perihelion passage. The observation was by no means an easy one, the distance being measured as 4″·19 (1890 April 22). Since that date, especially during the last two years, several observers have attempted to catch a glimpse of this body with varying results. Dr. See, in a communication on this subject (*Astr. Journal*, No. 418), points out that although several objects have been measured which were thought at the time to be the companion in question, there seems to be evidence to show that in some cases these were spurious. There is, however, no doubt that the companion can now be seen, and several measures show that it is following with great accuracy the orbit which Dr. See computed for it in his work on "Researches on the Evolution of the Stellar Systems" (vol. i. p. 84). The satisfactory agreement of the observed and computed positions shows that this orbit is of a high degree of accuracy, and will be serviceable for many years to come. The following ephemeris, taken from Dr. See's paper, gives the position angles and distances for the next three years:—

	Pos. angle.	Distance.
	°	″
1897·70	174·5	4·59
1898·20	169·0	4·72
1899·20	158·9	4·97
1900·20	149·5	5·25

A LIBERAL GIFT TO ASTRONOMY.—It was briefly announced in last week's NATURE that Miss Alice Bache Gould, daughter of the late Dr. Gould, had entrusted to the U.S. National Academy of Sciences a fund of 20,000 dollars; we are now able to state the precise conditions under which the gift was made. The money given by Miss Gould is to be known as the Benjamin Apthorp Gould Fund, and the net income of the fund is to be expended under the direction of Prof. Lewis Boss of Albany, Dr. Seth C. Chandler of Cambridge, and Prof. Asaph Hall of Washington, who are constituted by Miss Gould a board of directors for that purpose. The income is to be devoted to the prosecution of researches in astronomy by assisting observers and investigators in such manner and sums as may be agreed upon by all three of the directors.

In a letter to the directors, Miss Gould writes:—"My object in creating the fund is two-fold; on the one hand to advance the cause of astronomy, and on the other to honour my father's memory and to ensure that his power to accomplish scientific work shall not end with his own life. . . . Throughout my father's life his patriotic feeling and scientific ambition were closely associated, and I wish therefore that a fund bearing his name should be used primarily for the benefit of investigators in his own country or of his own nationality. I recognise, however, that sometimes the best possible service to American science is in the maintenance of close communion between the scientific man of Europe and of America, and that therefore, even while acting in the spirit of the above restriction, it may occasionally be best to apply this money to the aid of a foreign investigator working abroad."

One idea in the creation of the fund is that it may relieve the directors of the Bache fund of the Academy of some of their astronomical expenses, so that they may be able to devote more of their money to other departments. Miss Gould expresses a strong wish that astronomy of precision shall be distinctly preferred to work in astrophysics, first, because of Dr. Gould's personal preference, and, second, because of the existence of endowments for astrophysics. The fund is distinctly intended for the advancement and not for the diffusion of scientific knowledge; actual expenses of investigation are to be considered rather than the personal support of investigators, and the directors are advised not to cover with their grants the field provided for by existing endowments.

CORAL BORING AT FUNAFUTI.[1]

THE boring at Funafuti, according to the latest advices, had reached a depth of 643 feet. Prof. David's report is transcribed from notes made during the progress of the work, and gives his first impressions of the materials brought up, down to a depth of 557 feet, which had been reached when he quitted the island to return to his duties at Sydney, leaving the work in charge of his assistant. The latest advices informed him that the boring was arrested at 643 feet, but as it was hoped this was only for a time, we are daily expecting to hear yet more gratifying news. His last letters, received during the present week, give a few particulars of the materials pierced between 557 and 643 feet. The work, states Prof. David, often presented most serious difficulties, which would probably have frustrated their efforts, but for the experience gained on the former occasion.

The bore-hole is situated about half a mile north-east of the Mission Church, and its height above sea level is about 1 foot above high water mark at spring tides. The diameter is 5 inches down to 68 feet; it is lined with 5-inch tubing down to 118 feet, and 4-inch from surface to 520 feet, so that on September 6 a 4-inch core was being obtained.

The following is a general description of the materials pierced:—For about a yard at the top there was a hard coral breccia. This was followed down to a depth of 40 feet by "coral reef rock," into the composition of which *Heliopora cerulea*, with spines of echinids and nullipores entered largely, the last predominating over the coral at from 15 to 20 feet. From 40 to 200 feet came more or less sandy material, but with a variable quantity of corals. These were scattered through the sand (calcareous and of organic origin; foraminifera, at about 40 feet, making from one-half to two-thirds of the whole) sometimes as fragments (forming occasionally a kind of rubble), but sometimes in the position of growth. Between 120 and 130 feet, and from about 190 to 200 feet, the material is described as fairly compact coral rock, so that very probably reefs *in situ*, though of no great thickness, were pierced at these depths. The sand appears to be largely derived from coral, but foraminifera occur, sometimes in abundance; so too do nullipores, and here and there spines of echinids. Towards 150 feet signs of change begin to appear in the corals, and these become more conspicuous as the boring approaches its greatest depth. In such case, if I understand rightly, some of the branching corals crumble away and are represented only by casts, while others remain, the surrounding matrix becoming solid, cemented apparently by calcite. Below 202 feet a decided change takes place in the character of the deposit. All above this seems to be largely composed of material derived from corals, with occasional rather brief interludes of true reef; and this mass, measuring, as said above, rather over 200 feet in thickness, may be termed the first or uppermost formation. Below this, down to about 373 feet, sandy material distinctly dominates, which sometimes is almost a calcareous mud. Still even there coral fragments and rubble ocoasionally appear, and now and then a few isolated corals. Other organisms may be detected, including nullipores, foraminifera, and mollusca; but until this material has been examined microscopically, it would be premature to attempt any precise statement. This mass, in thickness about 170 feet, may be termed the second or middle formation. It is not reef, though obviously produced in the vicinity of a reef. Below 370 feet is the third or lowest zone; in this beds composed of broken coral become frequent, which are intercalated with masses of dead coral, though sandy bands also occur. The character of the material suggests that it has been formed in the immediate vicinity of a reef, which has occasionally grown out laterally, though only for a time, and has built up a layer of true reef, from 2 to 3 feet in thickness, upon a mass of detrital coral. In one place the rock is specially noted as "hard," and hereabouts even the shells of gastropods have perished, only their casts remaining. From 526 to 555 feet the bore passed through fairly compact and (in places) very dense and hard "coral limestone" and "cavernous coral rock," in which dendroid forms were numerous. As regards the part between 557 feet and 643 feet only brief information is to hand, but Prof. David states that it is reported to be chiefly coral limestone, hard and dense, with occasional soft bands of coral sand or coral rubble. Thus the third, or lowest zone, about 270 feet in thickness, corresponds apparently with the first, but it seems to contain larger and more numerous masses of true reef.

Prof. David has also forwarded with his latest letters a section of the boring and of the exterior form of the island, down to about 730 fathoms: the one drawn from his notebook, the other from Captain Field's record of soundings. From this I gather the following particulars:—The bore-hole is, roughly speaking, rather over 100 yards from the margin of the ocean, and about 165 yards from that of the lagoon; it is about 240 yards from the spot where a sounding of 10 fathoms was obtained,

[1] "Summary of Prof. Edgeworth David's Preliminary Report on the Results of the Boring in the Atoll of Funafuti." Communicated by Prof. T. G. Bonney, F.R.S., Vice-Chairman of the Coral Reef Boring Committee. Read at the Royal Society, November 25.

nearly 400 yards from a 36-fathom sounding, and rather more than a quarter of a mile from one of 130 fathoms. After this the submarine slope, for a considerable depth, is not quite so steep. He also states that, at Funafuti, the vigorous growing portion of the reef appeared to be limited to within about 40 feet of the surface.

It would be premature, as Prof. David remarks, to express an opinion as to the theoretical bearing of these results until the core has been thoroughly studied. But two things seem clear, (1) that true reef has been pierced at depths down to more than 600 feet, and (2) that throughout the whole of the time represented by the mass which has been tested, coral must have grown in great abundance in some part or other of the locality now represented by Funafuti; for the atoll, it must be remembered, is surrounded by water about 2000 fathoms deep, which would completely isolate it from any other coraliferous locality.

THE VITALITY OF REFRIGERATED SEEDS.[1]

A CONSIDERABLE difference of opinion still exists amongst biologists as to the condition of the protoplasts of resting seeds, spores, &c., in which all ordinary signs of life may be unrecognisable for a considerable period.

According to one view, the essential elements of the cell, during the period of inertness, are still undergoing feeble but imperceptible alteration, accompanied by gaseous exchange with the surrounding atmosphere; and, even when ordinary respiration is in abeyance, it is assumed there are small internal changes going on, due to the interaction of certain constituents of the protoplasm, reactions which may be independent of the outside gaseous medium, and which are often referred to under the somewhat vague term of "intramolecular respiration."

On the other hand, it is sometimes maintained that all metabolism is completely arrested in protoplasm when in the dormant state, and that it then loses, for the time being, all power of internal adjustment to external conditions.

According to one view, therefore, the machinery of the dormant protoplasts is merely "slowed down" to an indefinite extent, whilst according to the other it is completely brought to rest for a time, to be once more set going when external conditions are favourable.

It appears to us that the advocates of the "slowing-down" hypothesis have scarcely given sufficient attention to the experimental evidence available, and that they have been somewhat biassed by a supposed analogy between the dormant state of seeds and the hibernating state in animals, and have also, perhaps, been unconsciously influenced by Mr. Herbert Spencer's well-known definition of life, which implies a constant internal adjustment in the living protoplasm.

The experiments of the late G. J. Romanes, which were described in a short paper laid before the Society in 1893 (*Roy. Soc. Proc.*, vol. lvii. p. 335), are full of interest in their bearing on this question. Seeds of various plants were submitted in glass tubes to high vacua of 1/1000000 of an atmosphere for a period of fifteen months. In some cases, after the seeds had been *in vacuo* for three months, they were transferred to other tubes charged respectively with oxygen, hydrogen, nitrogen, carbon monoxide, carbon dioxide, hydrogen sulphide, aqueous vapour, and the vapour of ether and chloroform. The results proved that neither a high vacuum, nor subsequent exposure for twelve months to any of the above gases or vapours, exercised much, if any, effect on the subsequent germinative power of the seeds employed.

These experiments of Romanes are certainly of the highest importance, since the seeds were submitted for a long period to conditions which must certainly have excluded anything like respiration by ordinary gaseous exchange, but the conditions did not preclude with the same certainty the possibility of chemical interactions of some kind or other within the protoplasm, those ill-understood changes, in fact, which have been referred to "intermolecular respiration." It is true that in some of the experiments, notably those in which the vapours of chloroform and ether were employed, the probability of any such internal reactions is rendered somewhat remote; but still, in most cases, the experiments admit of the possibility of feeble metabolic activity continuing in the cytoplasm.

[1] "Note on the Influence of very Low Temperatures on the Germinative Power of Seeds." By Horace T. Brown, F.R.S., and F. Escombe. (Read before the Royal Society, November 18.)

It occurred to us, some months ago, that more evidence would probably be forthcoming on these points if we could submit seeds to a temperature below that at which ordinary chemical reactions take place, thus eliminating any possibility of interactions between the constituents of the protoplasm.

Owing to the kindness of Prof. Dewar, who has been good enough to place the resources of his laboratory at our disposal, and to undertake this part of the work for us, we have been able to ascertain how far the subsequent germinative power of a considerable variety of seeds is affected by prolonged exposure to the very low temperatures produced by the slow evaporation of *liquid air*.

The seeds, enclosed in thin glass tubes, were slowly cooled, and immersed in a vacuum-jacketed flask containing about two litres of the liquid air, which was kept replenished so as to submit the seeds for 110 consecutive hours to a temperature of from -183° C. to -192° C. About ten litres of liquid air were required for the experiment.

The seeds had been previously air-dried only, so contained from about 10 to 12 per cent. of natural moisture. After the above treatment they were slowly and carefully thawed, a process which occupied about fifty hours, and their germinative power was then compared with control experiments made on other portions of the seed which had not been treated in any way.

The seeds experimented on were as follows:—

Hordeum distichon.	*Trigonella fœnum-græcum.*
Avena sativa.	*Impatiens balsamina.*
Cucurbita Pepo.	*Helianthus annuus.*
Cyclanthera explodens.	*Heracleum villosum.*
Lotus Tetragonolobus.	*Convolvulus tricolor.*
Pisum elatius.	*Funkia sieboldiana.*

These include representatives of the following natural orders:—Gramineæ, Cucurbitaceæ, Leguminoseæ, Geraniaceæ, Compositæ, Umbelliferæ, Convolvulaceæ, and Liliaceæ.

Some of the seeds are endospermous, others non-endospermous, and the reserve material consists in some cases of starch, and in others of oil or of mucilage.

Their germinative power, after being submitted to the low temperature, showed no appreciable difference from that of the controls, and the resulting plants, which were in most cases grown to full maturity, were equally healthy in the two cases.

In 1892 Prof. Dewar and Prof. McKendrick found that a temperature of -182° C. continued for one hour is insufficient to sterilise putrescent substances such as blood, milk, flesh, &c., and that seeds would germinate after the action of a similar temperature for the same period of time (*Roy. Inst. Proc.*, 1892, vol. xiii. p. 699).

When we commenced our experiments we were unaware that any other observations of a similar nature had been made, but whilst they were in progress our attention was drawn to an important memoir by C. de Candolle,[1] in which the latent life of seeds is discussed in the light of a number of low temperature experiments made principally by himself and R. Pictet, and described at intervals in the Geneva "Archives."[2] In the earlier experiments of C. de Candolle and Pictet, made in 1879, temperatures of -39° C. to -80° C. were employed, and these only from two to six hours, whilst Wartmann in 1881 exposed seeds for two hours to -110° C. without effect. In 1884 Pictet found that an exposure of various kinds of Bacteriaceæ for three days to -70° C., and afterwards for a further period of thirty-six hours to -120°, did not destroy their vitality, and in the same year Pictet and C. de Candolle exposed seeds to -100° C. for four days with the same result. Pictet, in 1893, further extended his observations to various microbes, and also to a large number of seeds, and claims to have cooled them down without effect to nearly -200° C., but he gives no details of the experiments, nor any indication of the length of time during which the cooling lasted. His conclusions, however, are that, since all chemical action is annihilated at -100° C., life must be a manifestation of natural laws of the same type as gravitation and weight.

In his memoir of 1895 (*loc. cit.*) C. de Candolle discusses very fully whether we must regard the life of the resting seed as com-

[1] *Archives des Sci. Phys. et Nat.*, Geneva, 1895, vol. xxxiii. p. 497.

[2] E. Wartmann, 1860, *Archives des Sci. Phys. et Nat.*, 1860, p. 277; C. de Candolle and Pictet, 1879, *ibid.*, vol. ii. p. 354: *ibid.*, vol. ii. p. 629; E. Wartmann, 1881, *ibid.*, vol. v. p. 340; R. Pictet, 1884, *ibid.*, vol. xi. p. 320; R. Pictet and C. de Candolle, *ibid.*, p. 325; R. Pictet, 1893, *ibid.*, vol. xxx. p. 293; C. de Candolle, 1895, *ibid.*, vol. xxxiii. p. 497.

pletely arrested for a time or merely temporarily slackened (ralentie), and he gives the results of some new experiments on seeds maintained at from – 37° C. to – 53° C. in the "snow-box" of a refrigerating machine for a period of 118 days. Most of the seeds resisted this treatment successfully, and the author concludes that after a sufficient interval of time has elapsed the protoplasm of the ripe seed passes into a state of complete inertness in which it is incapable either of respiration or assimilation, and that whilst in this condition it can support, without detriment to its subsequent revival, rapid and considerable lowering of temperature.

De Candolle then points out that if it really be a fact that the suspended life of a resting seed is in any way dependent on respiration we might expect this to be shown by submitting seeds to a barometric vacuum for some time. He does not appear to have followed out this suggestion, and is apparently unaware of the direct experiments on this point carried out by Romanes two years previously; he argues, however, that if ordinary respiration is a factor of any importance, this may be determined by immersing the seeds in mercury for such a length of time as to ensure the complete consumption of the small amount of oxygen contained within their tissues. It was found that when seeds of *wheat*, and of *Lepidium sativum* were thus treated, for periods varying from one to three months, their power of germination was not sensibly affected.

Although these last described experiments of C. de Candolle go far to show that any considerable amount of respiration is unnecessary for the maintenance of potential life in the protoplasm of resting seeds, they are not inconsistent with the view that some minute amount of gaseous exchange may be going on during the whole course of the experiment at the expense of the oxygen contained in the seeds at the time of immersion in the mercury. The results would have been far more conclusive on this point if it had been shown that the gaseous oxygen originally contained in the seed tissue had been completely used up in an early stage of the experiment. The experiments of Romanes, however, conducted with high vacua and atmospheres of various gases, leave no room for doubt on this question, and we must consequently abandon all idea of the dormant state of resting seeds having any necessary dependence whatever on ordinary respiratory processes. Neither set of experiments, however, excludes the possibility of molecular interchanges in the protoplasm itself, such molecular transpositions in fact as those which can often be induced in living cells placed under anaërobic conditions, and which are all exothermic in character, and generally, but not necessarily, attended with the liberation of more or less CO_2. The great value of the low temperature experiments we have described lies in the fact that such processes of autoxidation, and in fact any conceivable internal chemical change in the protoplasts, are rendered impossible at temperatures of – 180° C. to – 190° C., and that we must consequently regard the protoplasm in resting seeds as existing in an absolutely inert state, devoid of any trace of metabolic activity, and yet conserving the potentiality of life. Such a state has been admirably compared by C. de Candolle with that of an explosive mixture, whose components can only react under determinate conditions of temperature; as long as these conditions remain unfulfilled the substances can remain in contact with each other for an indefinite period without combining.

It appears to us that the occurrence of a state of complete chemical inertness in protoplasm, without a necessary destruction of its potential activity, must necessitate some modification in the current ideas of the nature of life, for this inert state can scarcely be included in Mr. Herbert Spencer's well-known definition, which implies a continuous adjustment of internal to external relations.[1] The definition doubtless holds good for the ordinary *kinetic* state of protoplasm, but it is not sufficiently comprehensive to include protoplasm in the *static* condition in which it undoubtedly exists in resting seeds and spores. The definition becomes in fact one of "vital activity" rather than of life.

[1] The following passage from the "First Principles" (Section 25) clearly shows that the author in constructing his definition had not anticipated the possibility of a *living* organism attaining a state of absolutely stable equilibrium. "All vital actions, considered not separately but in their *ensemble*, have for their final purpose the balancing of certain outer processes by certain inner processes. There are unceasing external forces tending to bring the matter of which organic bodies consist into that state of stable equilibrium displayed by inorganic bodies; there are internal forces by which this tendency is constantly antagonised, and the perpetual changes which constitute life may be regarded as incidental to the maintenance of the antagonism."

As it is inconceivable that the maintenance of "potential vitality" in seeds during the exposure of more than 100 hours to a temperature of – 180° to – 190° C. can be in any way conditioned by, or correlated with, even the feeblest continuance of metabolic activity, it becomes difficult to see why there should be any time-limit to the perfect stability of protoplasm when once it has attained the resting state, provided the low temperature is maintained; in other words an immortality of the individual protoplasts is conceivable, of quite a different kind from that potentiality for unending life which is manifested by the fission of unicellular organisms, and with which Weismann has rendered us familiar.

In what manner and to what extent "resting" protoplasm differs from ordinary protoplasm we do not at present know, but there are indications, notably those afforded by the resting state of desiccated Rotifera, and also by some recent experiments of Van Eyck on discontinuous germination ("Ann. Agron," vol. xxi. (1895), p. 236), that ordinary protoplasts may, by suitable treatment, be brought to the "resting" condition.

In 1871, Lord Kelvin, in his Presidential Address to the British Association, threw out the suggestion that the origin of life as we know it may have been extra-terrestrial, and due to the "moss-grown fragments from the ruins of another world," which reached the earth as meteorites.[1] That such fragments might circulate in the intense cold of space for a perfectly indefinite period without prejudice to their freight of seeds or spores, is almost certain from the facts we know about the maintenance of life by "resting" protoplasm; the difficulties in the way of accepting such a hypothesis certainly do not lie in this direction.

We must express our thanks to Mr. Thiselton-Dyer and to Dr. D. H. Scott, for the facilities they have given us in carrying out these experiments in the Jodrell Laboratory.

Addendum.

After the completion of the above Note, our attention was called to a recent investigation by M. R. Chodat, on the influence of low temperatures on *Mucor mucedo* ("Bulletin de l'Herbier Boisier," vol. iv. (1896), p. 894). He found that a lowering of temperature for several hours to – 70° to – 110° C. failed to kill young spores of the mucor, and he adduces certain evidence, which is not, however, wholly convincing, that even the mycelium itself, when cultivated on Agar Agar, and whilst in active growth, is able to resist the action of these low temperatures. The author sums up his opinion as to the bearing of his experiments on the nature of life in the following words:—"La respiration elle-même est évidemment complètement arrêtée à cette température où les corps chimiques ne réagissent plus les uns sur les autres. Si l'on considère que la vie consiste principalement en un échange continuel de substance, soit par la nutrition intracellulaire, soit par la respiration, alors il faut convenir qu'à ces temperatures basses la vie n'existe plus. C'est une fatale erreur qu'on rencontre dans presque tous les traités que la respiration est une condition nécessaire de la vie, alors qu'elle n'est qu'une des conditions de sa manifestation. La vie est conditionée par certaines structures. Les forces qui les mettent en jeu peuvent être des forces toutes physiques. Elles sont simplement les sources d'énergie qui pourront mettre la machine en mouvement."

THE LAW OF CONDENSATION OF STEAM.

AT the meeting of the Institution of Civil Engineers on Tuesday, November 30, an important paper was read on "The Law of Condensation of Steam," by Prof. Hugh L. Callendar, F.R.S., and John T. Nicolson.

In the discussion of steam-engine trials it had generally been assumed that the rate of condensation of steam on a surface was practically infinite, so that any surface in direct contact with the steam was immediately heated to the saturation temperature corresponding with the pressure of the steam. It had also been supposed that the amount of condensation under any given conditions was limited, either by the resistance of the film of condensed water to the passage of heat, or by the capacity of the

[1] We find that Prof. Dewar called attention in one of his Royal Institution lectures in 1892 to the bearing of his low temperature experiments with spores, &c., on this suggestion of Lord Kelvin's (see *Roy. Inst. Proc.*, 1892, vol. xiii. p. 699.)

metal or of the circulating water to carry off the heat. In many cases condensation was diminished by films of oil or grease, or by accumulations of hair, or by other incrustations or deposits, but these were not considered in the paper.

The authors found, on the contrary, as the result of their experiments on a steam-engine running under normal conditions, that a practically clean and dry metal surface was not immediately heated to the temperature of the saturated steam in contact with it, that the rate of condensation of steam was not infinite, but finite and measurable, and that the amount of condensation in any given case was limited chiefly by this finite rate of condensation, and could be calculated in terms of it.

The cyclical variations of temperature in the metallic walls of the cylinder, with each stroke of the engine, were measured by means of thermo-couples inserted at various distances from the inner surface. It was possible thus to deduce the amount of heat absorbed and given out by the metal, and to infer the quantity of steam condensed and re-evaporated at different points of the stroke. The temperature-cycles of the steam were simultaneously measured by a very sensitive platinum thermometer. The observations showed that the temperature of the steam in different parts of the cylinder differed in a systematic way from the saturation temperature as deduced from indicator diagrams.

In order to deduce the condensation from the observed temperature-cycles, it was necessary to determine the conductivity and specific heat of cast iron. A series of experiments were made upon a four-inch bar of cast iron, and the result found for the conductivity was nearly 30 per cent. smaller than that generally assumed.

At the lowest speed of the experiments, namely, forty-five revolutions per minute, the temperature of the surface of the metal at the end of the admission period was found to be never raised higher than within 20° F. of the temperature of the steam, and the rate of condensation at any moment was simply proportional to the difference between the temperature of the steam and the surface. The numerical value found for the rate of condensation was 0·74 B.T.U. per second per square foot of surface per degree Fahrenheit of difference between the temperature of the steam and the surface. This was equivalent to the condensation of 27 pounds of steam per square foot per hour at 300° F., for a difference of temperature of 10° F. Assuming this law, the total amount of condensation at any point of the stroke could be inferred by measuring the "Condensation Areas" on the temperature-cycle diagram, *i.e.* the areas included between the curves representing the temperatures of the steam and of the metal surface.

To compare the results thus found with the missing steam deduced from the indicator diagrams and the feed measurements, the leakage of the valve and piston was determined as nearly as possible under the conditions of running. It was found to be proportional to the difference of pressure and nearly independent of the speed through a considerable range. The usual test for leakage with the valve stationary was found to be of little or no value. From a comparison of leakage tests, it was inferred that a valve in motion, however well fitted, was subject to leakage of a definite type. The leakage took place chiefly in the form of water, by condensation and re-evaporation on the moving surfaces, and was directly proportional to the perimeter of the ports and inversely to the width of the bearing surfaces. The amount of condensation observed during the admission period in a single-acting non-condensing cylinder 10·5 inches in diameter with a stroke of 12 inches, was only 20 per cent. of the feed at a speed of 100 revolutions per minute. The smallness of this result was probably due to the early compression and the dryness of the steam supply. It was found that re-evaporation was completed very quickly, and that the walls were dry for the greater part of the cycle. It was inferred from the form of the temperature curves and from other evidence that the rate of re-evaporation was the same as that of condensation.

From the form of the law of condensation it was possible to make an important theoretical deduction with regard to cases in which re-evaporation was incomplete, and the walls remained wet throughout the whole cycle. Under these conditions the mean temperature of the walls should be the same as the time average of the temperature of the steam to which they were exposed, and the cyclical condensation was the maximum possible for the given steam cycle. If the extent of the clearance surfaces was known, this limiting value of the condensation in any case might be easily deduced from the indicator diagram. If the surfaces were dry during part of the stroke, the condensation was less than the limit, and it was necessary to know the mean temperature of the clearance surfaces in addition. Upon these views of the nature of condensation and leakage, the missing quantity of steam W in pounds per hour might be expressed by an equation of the general type, $W = S(t' - t^{\circ}) + L(p' - p'')$,—where the first term represented condensation and the second term leakage, S being the equivalent clearance surface in square feet, and $t' - t^{\circ}$ the mean difference of temperature, in degrees Fahrenheit, between the walls and the steam during admission reduced to one-half cut-off. L, the rate of leakage per pound difference of pressure $p' - p''$, might be taken to vary approximately as the product of the diameter and the square root of the normal piston-speed, for engines of different sizes. It would appear from this formula that the effect of leakage on the performance was relatively more important in small engines and at high pressures, and that the loss due to condensation was most effectively reduced by increase of piston-speed.

As an indirect verification of this law of condensation, the temperature of the clearance surface in cases in which water was present in the cylinder was measured, and was found to agree with that of the mean of the steam cycle. The amount of condensation was also correctly calculated in several cases of published tests in which sufficient data were available. The rate of condensation deduced was also directly verified by an entirely different method. The experiments gave approximately the same rate of condensation, and appeared to show that the water-drops condensed on the metallic surface, owing probably to their rapid action, did not appreciably diminish the rate. Assuming it possible to estimate the condensation occurring in any given case by the method indicated, from a knowledge of the indicator diagram and of the temperature and area of the clearance surfaces, it then became possible to determine the amount of leakage under the actual conditions of running.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The Institution of Civil Engineers has resolved to exempt Bachelors of Arts who obtain honours in the Mechanical Sciences Tripos from the examination prescribed for the Associate Membership of the Institution.

The Managers of the John Lucas Walker Studentship have elected Mr. J. W. W. Stephens, of Caius College, to the studentship in Pathology, and Dr. Hamilton K. Wright, of McGill University, Montreal, to an Exhibition of the value of 50*l.*

The proposal to authorise the Examiners for the Natural Sciences Tripos Part II. to inspect the laboratory note-books of candidates for honours was carried in the Senate by forty-seven votes to eighteen.

Dr. H. A. Giles has been elected to the Professorship of Chinese. Dr. W. H. R. Rivers, of St. John's College, has been appointed University Lecturer in Physiological and Experimental Psychology. Mr. W. L. H. Duckworth, Fellow of Jesus College, has been recognised as a Lecturer in Anthropology.

The Walsingham Medals for research in Biology, including Physiology, have been awarded to Mr. V. H. Blackman, of St. John's College, and Mr. W. M. Fletcher, of Trinity College. The electors report that the essays of four other candidates were of a high order of merit.

The election to the Isaac Newton Studentship in Astronomical Physics will be held in the Lent Term 1898. Candidates must be Bachelors of Arts of less than twenty-five years of age on January 1. The studentship is of the value of 200*l.* a year for three years. Applications are to be sent to the Vice-Chancellor between January 14 and 24.

The University delegates to the International Congress of Hygiene at Madrid, next April, are Dr. Kanthack, Dr. D. MacAlister, and Dr. Anningson.

THE death is announced of Dr. Brownless, Chancellor of Melbourne University.

DR. H. H. HOFFERT has been promoted to a senior inspectorship under the Science and Art Department, and Mr. S. J. Cartlidge—at present head-master of the Hanley School of Art—has been appointed to the vacant inspectorship.

THE London Technical Education Board will proceed in July next to award three junior scholarships in practical gardening, which will be tenable at the new School of Practical Gardening which has recently been opened at the gardens of the Royal Botanic Society in Regent's Park. This school has been established with the view of providing a complete course of instruction for lads who desire to become gardeners. The scheme of work, which has been drawn up by the Royal Botanic Society, combines thorough practical instruction in all the operations of gardening with theoretical instruction in botany and the nature of soils and manures. The course is arranged so as to extend over three years.

THE list of entrance scholarships and exhibitions awarded at Pembroke, Gonville and Caius, King's, Jesus, Christ's, and Emmanuel Colleges, Cambridge, affords an indication of the comparative encouragement given to classics and science at the University. It appears from the list that the scholarships for classics have the value of 1360*l*. Mathematics comes second with scholarships amounting to the value of 640*l*., and the natural sciences take the last place with scholarships having a total value of 390*l*. Considering that the Science Tripos is the largest, or nearly the largest, this seems a discouraging division of the scholarship fund. It is only fair to add that at Trinity and St. John's Colleges the authorities are far more liberal to science, the value of scholarships awarded for natural sciences at the former College being 330*l*., and at the latter, 205*l*.

THE *Times* of Monday contains a detailed report of a conference held on Saturday last at the rooms of the Society of Arts, Adelphi, to consider the expediency of further development in the constitution of the Royal Holloway College in the light of the founder's expressed desire that powers should ultimately be sought enabling the college to confer degrees on its students. Mr. Bryce, M.P., one of the governors of the college, presided, and there was a large and influential gathering of educationists. Papers were read by Mr. R. D. Roberts in favour of an application from Holloway College for a separate charter to enable it to confer degrees upon its students; by Mr. Strachan Davidson, of Balliol College, Oxford, in support of the establishment of a women's university, of which Holloway College should form a part; and by Mrs. Bryant in advocacy of the proposal that Holloway College should become an integral member of the new teaching University of London. A number of letters from prominent educationists were read, expressing their views on the subject. A discussion followed, in which Mrs. Henry Fawcett, Miss Emily Davies, Mrs. Sidgwick, of Newnham, Miss M. Gurney, Sir Joshua Fitch, Mr. H. Sidgwick, and others took part. There was a practical consensus of opinion against the proposal that Holloway College should give degrees; a great majority of the speakers were opposed to the creation of a separate University for women, and many of them were in favour of connection with the new London University.

THE new Academic Hall of Edinburgh University, opened on Saturday last, is another testimony to the close and friendly connection which has always existed between the University and the city. In 1888, Mr. William M'Ewan, M.P. for the Central Division of Edinburgh, offered a sum of 40,000*l*. with which to build the Academic Hall, which had formed a part of the original plans of the new University buildings, which had already cost 250,000*l*. to complete. This offer Mr. M'Ewan afterwards, on its being made clear that the amount was inadequate if the hall was to be proportionate to the buildings already erected, agreed to increase to 62,000*l*., which was the original estimate of the cost of the hall. As the scheme grew under the hands of the architect, artificers, decorators, and organ builders, the liberality of Mr. M'Ewan kept pace, and the hall, as it now stands completed, has cost him 115,000*l*. The gift is a noble one, and it provides a noble example of the interest which the citizens of Edinburgh take in the welfare of the University. In accepting the deed of conveyance, on behalf of the University, Mr. Balfour said: "I confess that I have seen with feelings of regret, sometimes almost amounting to shame, the extreme difficulty which there has been not merely in connection with Edinburgh, but in connection with other great seats of learning, to obtain from the liberality of a not illiberal public sufficient means to make our great British Universities all that British Universities should be. I fear that in this respect we can but ill stand comparison with our cousins of the United States. There, if my information is not incorrect, they have never failed to find men with the means and with the will to keep the institutions of higher education in their country abreast with the ever-growing necessities of such institutions; and the number of generous benefactors which America has been able to show may well cause some feeling of shame, I think, in us on this side of the Atlantic, speaking the same language, possessing the same culture, aiming at the same objects, but who have not always shown in pursuit of those objects the same uninterested generosity. The relations between Edinburgh and the University, always close, almost always friendly, have not been diminished by changes in the status of the University. In connection with this very hall, or rather with the surroundings and accessories to the hall, the city of Edinburgh has shown itself possessed of the same generous public spirit, the same desire to do everything in its power to promote the interests of this great seat of learning which it has shown throughout all the centuries since this University was first founded." Mr. M'Ewan's liberality and Mr. Balfour's remarks upon the relations between the city of Edinburgh and the University should furnish food for reflection to the citizens of London.

THE *Technical Education Gazette* publishes a few particulars with regard to the entries in the various classes at the nine polytechnics which are in receipt of aid from the Technical Education Board of the London County Council. The most significant fact in connection with the polytechnics is that, notwithstanding the opening of four new large polytechnics during the four years that the Board has been at work, no diminution has been caused in the number of students attending the older institutions, but on the contrary, every one of the nine institutions shows an increase in the number of class entries for the present session. Thus the remarkable result has been brought about that, although these four new institutions show this session a total of over 8000 class entries, representing a total number of nearly 5000 individual students, yet the enrolment of these new students has not only not decreased the membership of the other older institutions but has actually stimulated their growth. It may be estimated that there are now in attendance at the evening classes of the nine polytechnics about 18,000 individual students, the great majority of whom are engaged in systematic courses of evening instruction under the direction of the principal of the institution where they are studying. If we take the class entries in detail according to the various branches of study, we find that the classes in the building trades show in almost every case an increase in the number of students. In the engineering trades there is likewise a very general increase in the attendance at the classes. The great demand that exists for evening instruction in electrical engineering is shown by the fact that in both the two polytechnics in the south-west district of London, the Battersea Polytechnic and the South-west London Polytechnic at Chelsea, the number of students has increased during the past year with remarkable rapidity. Another satisfactory instance of increase is shown in the classes in typography and letterpress printing. Perhaps the most remarkable fact of all is that the original polytechnic at Regent Street, which draws its students from all parts of the metropolis, and which might therefore have been expected to suffer from the growth of new institutions, continues not only to maintain its numbers but even shows a further increase on last year. Last year the number of individual students attending on November 1 was 5583; this year there were in attendance on November 1 as many as 5848, representing an increase of about 5 per cent. A very rapid development has taken place in the classes at the South-west London Polytechnic. So numerous are the entries this session for both the day and the evening departments, that the resources of the building are taxed to the utmost, and very serious inconvenience is being caused in some of the departments by the want of sufficient accommodation.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, November 18.—"Account of a Comparison of Magnetic Instruments at Kew Observatory." By Dr. C. Chree, F.R.S., Superintendent.

Last July, M. T. Moureaux, of the Parc Saint-Maur Observatory, near Paris, brought over to England the travelling instruments employed in his magnetic survey of France, and a comparison was made between these and the standard magnetic instruments at Kew Observatory.

The comparison serves to connect the standard instruments at Kew Observatory with the standard French instruments at Parc Saint-Maur, the latter, as M. Moureaux has had the goodness to inform the author, being in excellent agreement with his travelling instruments. Parc Saint-Maur may be regarded as the base station for M. Moureaux's great survey of France and Algeria. M. Moureaux's observations occupied the afternoon of July 26, and the forenoons of July 27, 28 and 29. On the afternoons of the last three days, observations were made with the Kew standard instruments by Mr. T. W. Baker, chief assistant at the Observatory. The observations, being made at different hours of the day, had to be connected through the intermediary of the curves from the self-recording magnetic instruments.

The means of the declination and inclination readings from the Kew instruments exceeded the means from M. Moureaux's instruments by 0'·5 and 2'·0 respectively, but the mean horizontal force reading from the Kew instrument was lower than M. Moureaux's by ·00012 C.G.S. unit. In calculating the last-mentioned element, M. Moureaux made use of new values of the constants of the French instruments, to be put into general use after January 1, 1898.

The comparison is utilised to extend a table ("Brit. Assoc. Report for 1896," p. 97) in which Prof. Rücker and Mr. W. Watson embodied the results of their comparison of the standard instruments at various English and Irish observatories, made by means of travelling instruments in 1895.

The results may be summarised as follows :—

Observatory	Departures from imaginary mean instrument of the five Observatories		
	Declination	(Hor. force) × 10^5 C.G.S. units	Inclination
Kew	+ 0'·2	− 1	+ 0'·2
Parc Saint-Maur.	− 0'·3	+ 11	− 1'·8
Falmouth	+ 1'·0	+ 17	+ 1'·8
Stonyhurst ...	− 0'·9	+ 5	− 2'·0
Valencia	+ 0'·2	− 30	+ 2'·0

"On *Spencerites*, a new Genus of Lycopodiaceous Cones from the Coal-measures, founded on the *Lepidodendron Spenceri* of Williamson." By D. H. Scott, F.R.S.

The fossils which form the subject of the present paper are Cryptogamic strobili, showing evident Lycopodiaceous affinities, but differing in important points from other fructifications of that family, so that it appears necessary to establish a new genus for their reception.

Two species are described, one of which (*Spencerites insignis*) is already known to us from the investigations of Williamson, who named it first *Lepidostrobus insignis*, and afterwards *Lepidodendron Spenceri*, while the other (*Spencerites majusculus*) is new.

In one of his latest publications, Williamson pointed out that it might ultimately be necessary to make his *Lepidodendron Spenceri* the type of a new genus. The separation thus suggested is now carried out, on the basis of a renewed investigation of the structure of this fossil.

Spencerites insignis is a pedunculate strobilus ; the vegetative organs are not as yet identified. The specimens are calcified, and their structure admirably preserved.

The anatomy of the axis is of a simple Lycopodiaceous type, but differs in details (such as the course of the leaf-trace bundles) from that of the axis of *Lepidostrobus*. The peduncle bears sterile bracts, similar to the sporophylls of the cone itself; the latter are arranged spirally, or in some cases in alternating verticils.

The individual sporophylls are of peltate form, consisting of a short cylindrical pedicel, expanding into a relatively large lamina. The sporangia are approximately spherical bodies ; unlike those of *Lepidostrobus*, they are quite free from the pedicel, and are attached by a narrow base to the upper surface of the lamina, where it begins to expand.

The details of the sporangial wall are quite different from those of *Lepidostrobus*, and the spores are also characteristic. In size they are intermediate between the microspores and macrospores of *Lepidostrobus*. They are of tetrahedral form, becoming spheroidal when mature, and each spore has a hollow, annular wing running round its equator. The wing is no doubt formed by a dilation of the cuticle, and not, as Williamson supposed, from the abortive sister-cells.

Spencerites majusculus, the new species, is much larger than the former, the axis of the cone being twice as thick. The anatomy is similar, but the sporophylls, and consequently the leaf-traces, are more numerous. The sporophylls, which are arranged in alternating verticils, are relatively short, and of peculiar form ; the lamina is very thick, and of great tangential width. The sporangia are like those of the former species, and similarly inserted, but the spores are quite different. They are smaller than those of *S. insignis*, and have the form of quadrants of a sphere, with narrow wings along their three angles.

The genus is separated from *Lepidostrobus*, mainly on account of the very different mode of insertion of the sporangia, a character which is accompanied by differences in the form of the sporophylls and sporangia, the structure of the sporangial wall and of the spores, and the whole habit of the strobilus.

Spencerites, and especially *S. insignis*, bears a considerable resemblance to the *Sigillariostrobus Crepini*, of Zeiller, but cannot be united with the genus *Sigillariostrobus*, for the insertion of the sporangia in the latter, as shown in the *Sigillariostrobus ciliatus*, of Kidston, is totally different. The author is much indebted both to M. Zeiller and Mr. Kidston, for the loan of their specimens for examination.

Zoological Society, November 30.—Mr. E. T. Newton, F.R.S., in the chair.—Mr. Oldfield Thomas exhibited specimens of a remarkable partially white Antelope of the genus *Cervicapra*, which had been obtained by Mr. F. V. Kirby in the mountains of the Lydenburg district of the Transvaal, and read an account of them contributed by Mr. Kirby himself. Mr. Oldfield Thomas also exhibited a skin of a new Skunk of the genus *Spilogale* from Sinaloa, Mexico, proposed to be termed *Spilogale pygmæa*. It was interesting as being of barely half the size of any previously known species, and also differed from all its congeners in the median dorsal stripes being uninterrupted posteriorly, and in having white hands and feet.—Mr. Thomas likewise exhibited a Badger from Lower California, proposed to be termed *Taxidea taxus infusca*, which differed from the described forms of *T. taxus* in its dark coloration and broad nuchal stripe.—Mr. Sclater exhibited the head of a *Capra* from Arabia, which had been recently described as *Capra mengesi*. Mr. Sclater was inclined to believe that the specimen was referable to *Capra sinaitica*, in which opinion Mr. O. Thomas agreed with him.—Mr. R. E. Holding exhibited a pair of curiously deformed horns of the Fallow Deer, and made remarks on the associations between organic disease and defective horn-growth.—On behalf of Mr. R. Lydekker was exhibited a skin and antlers of a small form of the Mule Deer from Lower California, for which he suggested the name *Mazama hemionus peninsulæ*. It differed from *M. h. californicus* in its small size, black dorsal line, and in the reduction of white on the tail.—Mr. G. A. Boulenger, F.R.S., exhibited some specimens of a South-American Siluroid Fish (*Vandellia cirrhosa*), and made remarks upon its curious habits.—A communication from Mr. H. H. Brindley, on regeneration of the legs in *Blattidæ*, was read. It consisted of an account of the statistical and experimental evidence of the reproduction of lost or injured legs in the *Blattidæ*, obtained since the publication of Mr. W. Bateson's book, "Materials for the Study of Variation," in 1894, and of some points in the post-embryonic development of the Cockroach (*Periplaneta orientalis*).—Mr. G. A. Boulenger, F.R.S., read a paper on a gigantic Sea-perch, *Stereolepis gigas*. This fish was described both externally and internally, and the author pointed out that *Megaperca ischinagi*, Hilgendorf, was specifically identical with it. Mr. G. A. Boulenger also described a new Tortoise of the African genus *Sternothærus*, a specimen of which had lately been received at, and was still living in, the Society's Gardens. It was proposed to name it *Sternothærus oxyrhinus*.—A communication from Mr. W. E. Collinge, on the structure and affinities of some further new species of slugs from Borneo, was read. Three new species, namely, *Parmarion fultoni*, *P. flavescens*, and *Microparmarion constrictus*, were described, and the author intimated that Simroth's genus *Microparmarion* would, on examination of more material, probably be found to be of only sectional value.

EDINBURGH.

Mathematical Society, November 12.—Mr. J. B. Clark, Vice-President, in the chair.—Prof. George A. Gibson contributed a paper on the "Treatment of Arithmetical Progressions by Archimedes," and communicated a paper by M. Lémeray, entitled "Quatrième Algorithme Naturel." The following were elected office-bearers for the current session:—President, Mr. J. B. Clark; Vice-President, Dr. Alexander Morgan; Hon. Secretary, Mr. J. W. Butters; Hon. Treasurer, Mr. D. Tweedie; Editors of *Proceedings*, Mr. W. J. Macdonald, Dr. Knott, Mr. Charles Tweedie; Committee, Messrs. G. Duthie, R. F. Muirhead, and J. D. H. Dickson.

MANCHESTER.

Literary and Philosophical Society, November 30.—Mr. J. Cosmo Melvill, President, in the chair.—Mr. H. W. Freston (Prestwich) and Mr. C. E. Stromeyer (Manchester) were elected ordinary members of the Society.—The President announced that the Council had awarded the Wilde Gold Medal of the Society for 1898 to Sir Joseph Dalton Hooker, C.B., K.C.S.I., F.R.S., in recognition of his eminent services to all branches of botanical science; and had awarded the Dalton Medal of the Society (struck in 1864) for 1898 to Dr. Edward Schunck, F.R.S., for his remarkable series of researches on the natural colouring matters; also, that the premium for 1898 of fifteen guineas had been awarded to Mr. John Butterworth, of Shaw, for his paper, printed in the *Manchester Memoirs*, on some further investigations of fossil seeds of the genus *Lagenostoma* (Williamson).—Prof. H. Lamb read a paper entitled "On waves in a medium having a periodic discontinuity of structure." The main object of the paper is to examine the selective total reflection which takes place at the boundary of a medium of this character. In the examples chosen for discussion the medium is represented by a string supposed to be capable of longitudinal vibrations, and the periodic interruption of properties may consist in a series of attached masses, or of attached particles, which are, moreover, urged towards fixed positions by springs, or to particles connected with the string by loose springs. The same analysis applies to media constituted in many other ways, and it is further shown how the methods may be adapted to cases where dynamical systems of a much more general character are interpolated at regular intervals. There are some instructive contrasts between the results obtained in the special cases above enumerated; in particular, in the last-mentioned case (that of particles attached by loose springs) it appears that relatively short waves may be transmitted freely unless the wave-length happens to fall within certain narrowly defined intervals. We have here, perhaps, an illustration of the theory of refraction sketched by Sir George Stokes in the Wilde Lecture; but some caution is, of course, necessary in drawing inferences as to theories of radiation and absorption from the study of a one-dimensional model.

PARIS.

Academy of Sciences, November 29.—M. A. Chatin in the chair.—New method of preparing carbides by the action of calcium carbide upon oxides, by M. Henri Moissan. Calcium carbide reacts with many metallic oxides at the temperature of the electric furnace, giving the carbide of the metal and lime, the latter being again partially converted into carbide by the carbon of the crucible. The carbides of aluminium, manganese, chromium, molybdenum, tungsten, titanium, and silicon were obtained by this method. The oxides of lead, bismuth' and tin gave the pure metals on similar treatment.—Experimental typhoid infection, produced by the introduction of a virulent culture into a Thiry cavity, by MM. R. Lépine and B. Lyonnet.—M. Ditte was elected a Member in the Section of Chemistry, in the place of the late M. Schützenberger.—Report on a memoir of M. Le Roy, entitled "On the integration of the equations of heat."—Observations of the new planet Villiger (1897, November 19) made at the Observatory of Algiers, by MM. Rambaud and Sy.—On two occultations of the Pleiads by the moon, by M. Lagrula. The observations are utilised for the determination of the semi-diameter and parallax of the moon at its mean distance.—Employment of the method of least squares to reveal the presence of systematic errors, by M. Jean Mascart. The determination of the vertical by means of the meridian circle is subject to errors much larger than would follow from the possible error of each microscope reading. With a view to see how far these fluctuations were due to alterations of temperature, a series of fifty determinations was made, giving fifty equations of condition for each microscope for three unknowns, the deviations being assumed as a quadratic function of the temperature. The results showed, however, that the fluctuations due to temperature changes are extremely small, and that the comparatively large errors observed must be due to other causes.—Observations of the sun made at the Observatory of Lyons, with the Brunner equatorial, during the third quarter of 1897, by M. J. Guillaume.—Influence of altitude and of heat upon the decomposition of oxalic acid by sunlight, by M. J. Vallot and Mme. Gabrielle Vallot. It has been found that, although oxalic acid is practically undecomposed by heat alone, the velocity of the reaction with actinic light is very greatly accelerated by a rise of temperature. Thus two solutions of oxalic acid exposed to sunlight under similar conditions, except that one was maintained 12° higher than the other, gave decompositions of 10 and 50 per cent. respectively. The rate of decomposltion also increased rapidly with the altitude.—On the fundamental theorem of projective geometry, by M. H. G. Zeuthen.—On the equation to periods, by M. X. Stouff.—On the Bessel functions $S^n(x)$ and $O^n(x)$, by M. L. Crelier.—On the statical and dynamical explosive potentials, by M. R. Swyngedauw. A reply to some criticisms of M. Jaumann.—A simple method of proving the change of period of sodium light in a magnetic field, by M. A. Cotton. A flame feebly tinged with sodium is observed through another flame also containing sodium. The edges of the second flame appear to be black, owing to its gaseous envelope being absorbent. Any small change in the period of vibration of the more distant flame, produced by the action of the magnetic field, suppresses the absorption, and causes the black border of the interposed flame to disappear. In this way it is easily shown that a change of period of light emitted parallel to the lines of force, completely extinguishes the dark border, whilst with observations made perpendicularly to the lines of force, the border grows lighter on completing the circuit round the magnet, but does not completely disappear.—Osmotic researches on very dilute solutions of cane sugar, by M. Ponsot. The author was successful in making membranes impermeable to sugar. With these measurements of osmotic pressure were made of sugar solutions containing only 1·235 and 0·6175 grams per litre. The mean of the observed pressures was exactly equal to that calculated by the Van t' Hoff formula, on the assumption that no dissociation of the sugar took place, or the coefficient $i = 1$.—On the alcoholic isocyanurates and the constitutional formula of cyanuric acid, by M. Paul Lemoult. The heats of combustion of methyl and ethyl isocyanurates were determined, and the conclusion drawn that isocyanuric acid has a ring-shaped constitution, a symmetrical tricarbimide.—Quinones and hydroquinones, by M. Amand Valeur. Determinations of the heats of combustion of toluquinone, thymoquinone, hydroquinone, hydrotoluquinone, and hydrothymoquinone.—On the transformation of sorbite into sorbose by the *Mycoderma vini*, by M. A. Matrot. The best experimental conditions for the production of sorbose from sorbite by means of the yeast *Mycoderma vini* are worked out in detail.—On the germinative plates of the Coleoptera, by M. A. Lécaillon. For certain Coleoptera it is shown that the blastula stage does not appear in development, the gastrula stage following immediately upon segmentation, and showing no typical invagination.—On the *Rouget*, a human parasite, by M. Brucker. This parasite, which appears as a parasite to man in August and September, is shown to be probably the larva of *Trombidium gymnopterorum*.—On the culture of the nostoch in presence of glucose, by M. Raoul Bouilhac.—On the characteristics of nerve and muscle stimulation, by M. G. Weiss. A reply to the criticisms of M. Dubois. The author claims that the experiments cited by M. Dubois are in reality confirmatory of his views.—Analysis of vocal sounds by the phonograph, by MM. Marichelle and Hémardinquer. In spite of the various influences which act upon the form of the period, such as musical pitch, intonation, intensity, and individual conformation of the sounding organ, each vowel is distinguished by certain invariable characters, constituting it a real individual.—On the absorption of organic materials by roots, by M. Jules Laurent. Experiments were made with maize upon solutions of glucose and invert sugar, and in every case a certain quantity of the sugar was absorbed by the roots, the amount of which appeared to be proportional to the dry weight of the plant. The sugars are utilised by the plant, and in great part excreted as carbon dioxide.—The favourable times in the treatment of black rot, by M. A. Prunet. A treatment with copper salts applied immediately after one invasion of the disease, acts beneficially against a future invasion, the maximum effect being produced by treating the vine from five to eight days after the first appearance of the spots.—On the rational construction of mills with metallic rollers, by M. J. Schweitzer.—On the analysis of silicates, by M. A. Leclère. The conditions are prescribed under which silicates may be safely opened up in a platinum crucible by means of lead oxide, the chief points being the purity of the oxide, and the complete exclusion of reducing gases by the use of a muffle.—On some peculiar circumstances which appear to have accompanied the fall of a meteorite on April 9, 1891, at Indarck in Transcaucasia, by M. Stanislas Meunier.—On the contamination of the springs of Sauve (Gard), by M. E. A. Martel. The contamination of the water supply of Sauve by sewage was proved directly by means of fluorescin. The frequent epidemics which have decimated this town are thus explained.—On two radiographs of the thorax, by M. F. Garrigou.

DIARY OF SOCIETIES.

THURSDAY, DECEMBER 9.

ROYAL SOCIETY, at 4.30.—On the Densities of Carbonic Oxide, Carbonic Anhydride, and Nitrous Oxide: Lord Rayleigh, F.R.S.—On the Application of Harmonic Analysis to the Dynamical Theory of the Tides. Part II. On the General Integration of Laplace's Dynamical Equations: S. S. Hough.—A Note on some Further Determinations of the Dielectric Constants of Organic Bodies and Electrolytes at Very Low Temperatures: Prof. Dewar, F.R.S., and Prof. Fleming, F.R.S.—On Methods of making Magnets independent of Changes of Temperature, and some Experiments upon Negative Temperature Co-efficients in Magnets: J. R. Ashworth.—The Electric Conductivity of Nitric Acid: V. H. Veley, F.R.S., and J. J. Manley.—On the Calculation of the Co-efficient of Mutual Induction of a Circle and a Coaxial Helix, and of the Electromagnetic Force between a Helix and a Coaxial Circular Cylindrical Sheet: Prof. J. V. Jones, F.R.S.—On the Refractivities of Air, Nitrogen, Argon, Hydrogen, and Helium: Prof. W. Ramsay, F.R.S., and M. W. Travers.

MATHEMATICAL SOCIETY, at 8.—The Construction of the Straight Line joining Two Given Points: Prof. W. Burnside, F.R.S.—A Theorem concerning the Special Systems of Point Groups on a Particular Type of Base Curve: Miss F. Hardcastle.—A General Type of Vortex Motion; R. Hargreaves.—Note on a Property of Pfaffians: H. F. Baker.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—Annual General Meeting

FRIDAY, DECEMBER 10.

PHYSICAL SOCIETY, at 5.—An Exhibition of an Apparatus for Self-acting Temperature Compensation of a Standard Cell: Albert Campbell.—On Lord Kelvin's Absolute Method of Graduating Thermometers: Rose Innes.

ROYAL ASTRONOMICAL SOCIETY, at 8.—Occultation of Ceres by the Moon on 1897 November 13, observed at the Hamburg Observatory: G. Rümker.—A Determination of the Latitude Variation and of the Constant of Aberration from Observations made at the Royal Observatory, Cape of Good Hope, 1892–94: W. H. Finlay.—The Binary Star *h* 5014: R. T. A. Innes.—Mean Areas and Heliographic Latitudes of Sun-spots in the Year 1895 deduced from Photographs taken at the Royal Observatory, Greenwich, at Dehra Dûn (India) and in Mauritius.

MALACOLOGICAL SOCIETY, at 8.—A Description of a Supposed New Species of *Monodonta* (*Austrocochlea*) from Tablas Island: G. B. Sowerby.—On a New Species of *Amphidromus* from the Malay Archipelago (Alor Island): Hugh Fulton.—On a New Species and Probable New Sub-Genus of *Endodonta* from Ceylon, collected by O. Collett: Lieut.-Colonel H. H. Godwin-Austen, F.R.S.—Notes on a Second Collection of Marine Shells from the Andaman Islands, with Descriptions of New Forms: J. Cosmo Melvill and E. R. Sykes.—On a Small Collection of Marine Shells from New Zealand and Macquarie Island, with Descriptions of New Species: E. A. Smith.

SATURDAY, DECEMBER 11.

ESSEX FIELD CLUB (at Chingford), at 6.30.—Notes on the Conference of Delegates of Corresponding Societies of the British Association at Toronto: Prof. Meldola, F.R.S.—Two Essex Minerals: T. S. Dymond and F. W. Maryon.

SUNDAY, DECEMBER 12.

SUNDAY LECTURE SOCIETY, at 4.—Colour: Dr. C. W. Kimmins.

MONDAY, DECEMBER 13.

SOCIETY OF ARTS, at 8.—Gutta-Percha: Dr. Eugene F. A. Obach.

IMPERIAL INSTITUTE, at 8.30.—Canada's Metals: Prof. W. C. Roberts-Austen, C.B., F.R.S.

TUESDAY, DECEMBER 14.

ZOOLOGICAL SOCIETY, at 8.30.—On the *Lepidosiren paradoxa* from the Amazon: Dr. E. A. Goeldi.—On a Small Collection of Lepidoptera made by Mr. F. Gillett in Somaliland: Dr. A. G. Butler.—On the Mammals obtained by Mr. A. Whyte in N. Nyasaland, and presented to the British Museum by Sir H. H. Johnston, K.C.B.; being a Fifth Contribution to the Mammalogy of Nyasaland: Oldfield Thomas.—On a New Genus and Species of *Acaridea*: Rev. O. Pickard Cambridge, F.R.S.

INSTITUTION OF CIVIL ENGINEERS, at 8.—The Great Land-Slides on the Canadian Pacific Railway in British Columbia: Robt. B. Stanton.

ROYAL STATISTICAL SOCIETY, at 5.30

ROYAL PHOTOGRAPHIC SOCIETY (Society of Arts, John Street, Adelphi), at 8.—Colour Photography: Prof. Gabriel Lippmann.

WEDNESDAY, DECEMBER 15.

SOCIETY OF ARTS, at 8.—The Purification of Sewage by Bacteria: Dr. Samuel Rideal.

GEOLOGICAL SOCIETY, at 8.—On the Pyromerides of Boulay Bay, Jersey: John Parkinson.—The Exploration of the Ty Newydd Cave, Ffynnon Beuno, North Wales: Rev. G. C. H. Pollen.

ROYAL METEOROLOGICAL SOCIETY, at 7.30.—Daily Values of Non-Instrumental Meteorological Phenomena in London, 1763–1896: R. C. Mossman.—The Rainfall of Seathwaite, Borrowdale, Cumberland: William Marriott.

CHEMICAL SOCIETY (Extra Meeting), at 8.30.—Kekulé Memorial Lecture: Prof. F. R. Japp, F.R.S.

ROYAL MICROSCOPICAL SOCIETY, at 8.—A New Form of Photomicrographic Camera and Condensing System: E. B. Stringer.

THURSDAY, DECEMBER 16.

ROYAL SOCIETY, at 4.30.

LINNEAN SOCIETY, at 8.—On the Affinities of the Madreporarian Genus Alveopora: H. M. Bernard.—On West Indian Characeæ collected by T. B. Blow: H. and J. Groves.

CHEMICAL SOCIETY, at 8.—Stereo-Chemistry of Unsaturated Compounds. Part I. Esterification of Substituted Acrylic Acids: Dr. J. J. Sudborough and Lorenzo Lloyd.—Formation and Hydrolysis of Esters: Dr. J. J. Sudborough and M. E. Feilmann.—A New Method of Determining Freezing Points of very Dilute Solutions: Dr. M. Wilderman.

FRIDAY, DECEMBER 17.

INSTITUTION OF ELECTRICAL ENGINEERS (Chemical Society's Rooms), at 8.—Accumulator Traction on Rails and Ordinary Roads: L. Epstein.

INSTITUTION OF CIVIL ENGINEERS, at 8.—The Elastic Properties of Steel Wire: Archer D. Keigwin.—The Elasticity of Portland Cement: W. L. Brown.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—By Roadside and River: H. M. Briggs (Stock).—Catalogue of the Books in the Library of the Indian Museum, Supplement 2 (Calcutta).—Memoirs of the Geological Survey, Scotland: The Geology of Cowal: W. Gunn, C. T. Clough, and J. B. Hill (Edinburgh, Neill).—Stirpiculture: Dr. M. L. Holbrook (Fowler).—Studies in Psychical Research: F. Podmore (Paul).—Student's Guide to Submarine Cable Testing: H. K. C. Fisher and J. C. H. Darby (*Electrician* Company).—The Book of the Dead: Dr. E. A. W. Budge, 3 Vols. (Paul).—The Rod in India: H. S. Thomas, 3rd edition (Thacker).—Wild Life in Southern Seas: L. Becke (Unwin).—Famous Problems of Elementary Geometry: Profs. Beman and Smith (Boston, Ginn).—A Text-Book of Special Pathological Anatomy; Prof. E. Ziegler, translated and edited by Drs. D. MacAlister and H. W. Cattell, Sections ix.–xv. (Macmillan).—The Lepidoptera of the British Islands: C. G. Barrett, Vol. 4 (Reeve).—Philip's Revolving Planisphere and Perpetual Calendar (Philip).—Le Végétaux et les Milieux Cosmiques. J. Costantin (Paris, Alcan).

PAMPHLETS.—Radiography in Marine Zoology: Dr. R. N. Wolfenden (Rebman).—Untersuchung über die Bahn des Cometen 1822 IV.: Dr. A. Stichtenoth (Leipzig, Engelmann).

SERIALS.—Contemporary Review, December (Isbister).—Bulletin of the American Mathematical Society, November (New York, Macmillan).—Knowledge, December (Holborn).—National Review, December (Arnold).—Proceedings of the Liverpool Geological Society, Session Thirty-eight (Liverpool).—Geological Magazine, December (Dulau).—Fortnightly Review, December (Chapman).—An Illustrated Manual of British Birds: H. Saunders, 2nd edition, Part 2 (Gurney).—Physical Review, October (Macmillan).—Archives of the Roentgen Ray, November (Rebman).—Zeitschrift für Physikalische Chemie, xxiv. Band, 3 Heft (Leipzig, Engelmann). L'Anthropologie, Tome viii. No. 5 (Paris, Masson).—Gazzetta Chimica Italiana, 1897, fasc. v. (Roma).—Revue de l'Université de Bruxelles December (Bruxelles).

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