THURSDAY, APRIL 27, 1893.

DYNAMICS IN NUBIBUS.

Waterdale Researches; Fresh Light on Dynamics. By "Waterdale." (London: Chapman and Hall, 1892.)

WHEN St. Paul tried to convince the Athenians that they were mistaken in their philosophy, he probably spoke to them in Greek instead of expecting them to learn Hebrew. "Waterdale" is trying to convince nineteenth century philosophers that it is possible to invent mechanism by which he can attain "the undoubted theoretical possibility of *perpetual motion*," and he does not take the trouble of learning the language of those whom he desires to convince, but insists that they must learn his language, simply because he *professes* to have invented a possible explanation of gravity. He acknowledges that his work would require at least a month's hard work to comprehend, and taunts the scientific world for not gladly spending this time in refuting what most of them have already spent weeks on—namely, refuting the very ingenious inventions of cranks, who think to cheat nature in the dark by some roundabout way of doing what simple considerations show to be impossible. A good month's work to teach him! Let him pay somebody with a reputation whose time is probably worth twelve hundred a year, say a month's time, one hundred pounds, to explain and convince him of the impossibility of his mechanical arrangement. It would take more than a month, however. If human experience is worth much it proves that there is very little use in trying to convince people with missions whether they are right or whether they are wrong. And fortunately so; for, if they are right they will ultimately prevail, and if they are wrong after all they generally do more good than harm by interesting the world in something outside and better than the selfish interests of individuals.

"Waterdale" attributes a good deal of importance to this mechanism. He says in his preface: "Let the scientific reader, I would ask, take the trouble first to go through these calculations, and he will then have some idea as to whether the rest of the book is worthy or not of careful perusal." In the body of the work he invents a very complicated hydrodynamic machine to effect his purpose. He there refers to the very much simpler arrangement described in the appendix, and says: "Unless the possibility" (of perpetual motion) "is admissible, then I must confess that the theory of equal real ponderosity to all matter can *never* be accepted." He acknowledges at the same time "that with full knowledge of the liability to error when dealing with the action of forces," all he can reasonably do is to ask "that . . . pure mathematics be once more applied to the subject." All the same, he asserts that "no disproof can be, or has up to the present been given." "There is no speculation about this, but simple fact, if calculation by figures can be accepted to be true." There are so many things touched on in the work that do not seem in any way necessarily connected with the question of "equal real ponderosity," that it is desirable to show how much interest "Waterdale" feels in this part of his theory in order to justify the paying of any serious attention to what can, on general principles, be so easily disproved. It would certainly not be worth while investigating the question in a scientific journal in order to convince the author of the paradox. He could only be convinced by very painstaking and judicious personal interviews of his error and of the unimportance of this question of equal real ponderosities. It would hardly be worth while investigating the question merely because "Waterdale" attributes importance to it, but it is worth while doing so because others may attribute importance to it, and still more so because "Waterdale's" mechanism is interesting and involves a principle that is intimately connected with the second law of thermodynamics, Boltzmann's hypothesis, and a lot of recondite questions which are puzzling the scientific world, so that it is not much wonder that even a clever and ingenious person should get involved in its meshes, especially when that person is involved in a "mission."

The general idea involved in "Waterdale's" mechanism is as follows:—Suppose a large body (he objects to the word "mass") M and a small one m, and a spring or other means by which kinetic energy can be given to the bodies. If the spring exert a constant force F through a space s_1, it would communicate a velocity V_1 to $(M + m)$, given by the equation—

$$Fs_1 = \tfrac{1}{2}(M + m)V_1^2.$$

If now it work through a distance s_2 it will increase this velocity to V_2, when

$$Fs_2 = \tfrac{1}{2}(M + m)(V_2^2 - V_1^2).$$

So far all is plain sailing. But we may proceed in another way. We may let the spring work against m alone, and then by suitable mechanism use m's kinetic energy to make the combined system $M + m$ move. In this way we might expect to give m a velocity v_1, such that $Fs_1 = \tfrac{1}{2}mv_1^2$, and when this energy was spent on the two bodies $M + m$, they would acquire a velocity V_1 the same as before, given by $\tfrac{1}{2}mv_1^2 = \tfrac{1}{2}(M + m)V_1^2$. Now comes an important assumption, that if the *relative* velocity of m and M be equal to v_1, then by proper mechanism it must always be possible to *increase* M's velocity by V_1, while m's velocity is being reduced to V_1.

Suppose now m_1, moving with velocity V_1, we act upon m by means of the force F, again through the distance s_2 we have for its final velocity v_2—

$$Fs_2 = \tfrac{1}{2}m(v_2^2 - V_1^2).$$

Hence the relative velocity of M and m is $v_2 - V_1$. By choosing $s_2 = 3s_1$, we can arrange that $V_2 = 2V_1$, as it simplifies the further argument. In this case

$$v_2^2 - V_1^2 = 3v_1^2 \text{ or } v_2^2 = 3v_1^2 + V_1^2;$$

$$\therefore v_2 = \sqrt{3v_1^2 + V_1^2},$$

and the relative velocity

$$v_2 - V_1 = \sqrt{3v_1^2 + V_1^2} - V_1,$$

which may be much greater than v_1, if v_1 be much greater than V_1, *i.e.* if m be much smaller than M. This shows that the *relative* velocity after the second blow may be much greater than after the first, even though the two blows were so chosen as that if applied directly to the combined body they would produce equal increments of velocity in that body. Assuming then that a given *relative* velocity can always produce a given *increase* of velocity in the combined system, it appears by our assumption that, as the *relative* velocity is much greater after the second blow given to m than after the first, the *increase* of velocity of the system produced by this indirect method of applying the second blow will be much greater than by the first, and consequently much greater than the velocity that could be given to the system by applying the blow directly. By reducing the system to its otherwise produced velocity V_2, we could obtain a certain amount of energy, and then repeat the process *ad infinitum*, thus obtaining a continual supply of energy.

An investigator without a mission would be led by this curious result to assume that there must be some mistake in his arguments, and "Waterdale" evidently has some lurking doubts. He sees that it is impossible in the simple case of bodies having only one direction of velocity. Impact can never reduce two bodies of a system to move with the same velocity *and* conserve energy. We cannot have momentum and energy both conserved. Unless M = o we cannot have

$$mv_1 = (M + m)V_1$$
$$\tfrac{1}{2}mv_1^2 = \tfrac{1}{2}(M + m)V_1^2.$$

In order to divide the energy $\frac{1}{2}mv_1^2$ between the two bodies and reduce them both to a common velocity, we require a *third body*, and then what becomes of the principle that seemed so plausible, that the increased velocity that m could impart to M depended on their *relative* velocity only? "Waterdale" sees the hitch all right in the simple case, and consequently, in order to cheat nature by inventing a complicated case in which he hopes that she will get as muddled as himself, he interposes bent channels, a third and fourth body to receive the blows, springy arms to absorb energy, and smooth surfaces to divert the motion. He evidently has some doubts about all this, for, notwithstanding his assertion that "Appendix II. is a mechanical demonstration to prove that by the principle of *velocity* of force, a saving in mechanical work, . . . can be effected," and that "there is no speculation about this, but simple fact," yet he gives only a series of suggestions and vague estimates as *unspeculative proofs*, that the energy spent in bending his springs, in jumping his bodies about, and so forth, is negligible, while in reality it is an important part of his system. That it is so necessarily is proved conclusively by the impossible result he obtains by neglecting it. This is the really interesting principle in the whole matter, that it is not possible to give energy to a system of bodies by giving a series of impulses to some particles of it, to be transmitted to the rest of the system by actions within the system without some part of the energy being spent on internal motions in the system. It is here that the example touches upon the second law of thermodynamics, Boltzmann's hypothesis, and so forth. In order to minimize the effects of these internal vibrations, &c., "Waterdale" argues thus: "Loss No. 2" (giving rise to internal vibrations of his system) "if it arises" (he himself shows that it would, though he overlooks a more important loss), "would be of the nature of internally asserted work." . . . "This loss of work could not be great, for we see by the diagram that the span of work already done when the ball arrives at o is small compared with what it has to do." Notwithstanding his profession of calculating everything he does not calculate here, nor does he calculate with what velocity the ball would rebound after it hit the body B, which ultimately stops it; in fact he omits this important question altogether, and goes to the "third factor, the bending of the arm of the system," which he goes on to say, *without calculation*, "can be almost neglected if we take the tension of elasticity of the arm to be small." "I should say that one-eighth internal loss of work would certainly more than cover everything." This blessed "I should say!" Is it thus that "Waterdale" gives "a mechanical demonstration to prove . . . . a saving in mechanical work"? "There is no speculation about this"! It is "simple fact, if calculation by figures can be accepted as true." Most people would agree that "if calculation by figures can be accepted as true" the velocity that could be given by *any* mechanism to the system indirectly could not be greater than what would give it kinetic energy corresponding to the work supplied. If "Waterdale" will apply a system of levers, springs, &c., acting on the fixed bodies of his system, so as to reduce all the bodies to relative rest, and thereby gives up as hopeless the task of inventing some method by which he can by internal actions alone transfer kinetic energy from one body of a system to the whole of the system without wasting any of it in internal kinetic or potential energy, then he will see how he has to give up the apparently legitimate assumption that the velocity one body of a system can give to the whole system by being itself reduced to relative rest depends *only* on the *relative* velocity of the body and the rest of the system. He will see that it depends also on the velocity of his system relative to those supposed fixed bodies he will require as fulcrums for the mechanism required to transfer the energy of the one body to the rest of the system. He sees that something is required to keep his wedge moving forward. He arranges "that the wedge is supported by a following force . . . during this part." The amount of work required he without calculation *assumes* to be small, and he is probably right here; but it is only one of several losses that he does not *calculate*, and there are others, such as the conditions of impact at the end of the flight of m, that he does not even notice, though this is the very first that should strike a person investigating the subject after he had clearly seen, as "Waterdale" appears to do, that it is here, in the laws of impacts, that the simple case of velocity in one direction and direct impacts fails. It is interesting how cases of this kind illustrate the warming of a gas by compression, the vibrations produced in a bell when struck, and other such cases where energy is given to one part of a dynamical system for this part to distribute amongst the whole, and also how it illustrates the way in which the amount of this internal energy depends on the mobility of the part originally moved. Of course it is all plain enough when the subject is attacked by means of general principles of conservation of energy and momentum, but when the interactions of the different parts of the system are individually considered and the mind distracted by the complexity of the problem, there is real danger that what is important may be overlooked as trivial, as has been done by "Waterdale." He is not to be blamed for this, but he is to be blamed for putting forward as a proof in which "there is no speculation," as a "simple fact, if calculation by figures can be accepted as true," an investigation in which he acknowledges that he estimates this, that, and the other without calculation.

And after all, what does he require all this elaborate attempt to cheat nature by complex mechanisms for? Simply because he does not understand fully the position of scientific men in respect of the word "mass," and because he has some *à priori* difficulties in his own mind as to how atoms of different masses can require equal quantities of heat to warm them through equal ranges of temperature. He *says* that scientific men say that *because* a cube of gold weighs seven times as much as a cube of aluminium, "it is therefore taken to comprise seven times the quantity of matter; *ergo* it possesses seven times the attractive force, and falls with equal acceleration; *ergo* also it requires seven times the force or work to move it." Now this is a gross libel on scientific men. That it requires seven times the force to move a cube of gold that is required in the same time to generate the same velocity in an equal cube of aluminium is a matter of experience, and is the only reason why it is said that the mass of the gold is seven times as great as the mass of the aluminium, and this is said because the statement is only using the word mass in accordance with the definition of the word. That there is *therefore* seven times the quantity of matter is really no question of *therefore*, for the statement is again merely a definition of the term "quantity of matter," which is, in its scientific use, only another name for mass. Now come questions about gravity, and as no satisfactory explanation of gravity (*pace* Le Sage, Tolver Preston, Osborne Reynolds, "Waterdale," and a host of other theorisers upon this interesting subject) has yet been propounded, no scientific person can rightly say that a body attracts seven times as much as another *because* it has seven times the mass or quantity of matter, for until we know the cause of the attraction we have no right to say that it is *because* of this or that. Hence there is no *therefore* at all put forward by scientific men between "seven times the quantity of matter," "or mass," and "seven times as heavy," or "seven times the attractive force." That a body with seven times the mass of another does as a matter of fact weigh seven times as much is a matter of experience, but that it does so *because* it has seven times the mass is a mere conjecture, and that it is so held by scientific men is proved by attempts having been made to prove by experiment that weight is proportional to mass, and even to find whether weight varies with the direction of the axis of a crystal, &c. "Waterdale" objects to supposing the elementary atoms bulk for bulk to be of equal density, because "we should have to place the atoms in a light substance too far apart," a fairly good reason for investigating the question though not for deciding it. On the other hand he objects to supposing "each atom to be more or less porous—a very incredible hypothesis"—for reasons depending on specific heats to which he evidently attaches some weight, as he harps upon it more than once. Why he should think it so incredible that the atoms may be porous does not clearly appear, for his own atoms, as described in the book, are eminently porous, and it is upon their porosity that his whole explanation of their behaviour to force, and his application of his principle of "velocity of force," and his theories of light and electricity and chemical action and adhesion all depend. He would probably reply that he does not want them *so* porous as all that, though this hardly justifies the epithet "incredible" in respect of a hypothesis he himself holds. Anyway, his serious reason for disbelieving in the unequal masses, or, as he calls it, ponderosities, of atoms is the difficulty he has in seeing how equal quantities of heat can raise unequal masses through equal ranges of temperature. His difficulty rests upon his imagination that he understands fully what the wisest men would probably say they did not understand at all fully, namely, on what property of the atoms of a body temperature depends. He discusses the matter pretty carefully. He says: "How are we to account for the apparent fact that the work of a quantity of heat which is equal to raising weight, 1 of water 1° of temperature—or, in other words, to accelerate, it is to be inferred, the vibrative motion of the whole of its parts in the degree corresponding to one more degree of heat—will be also equal to giving equal acceleration to the entire parts of 8·784 and 30·816, respectively, more matter in the cases of iron and gold?" He here assumes that equal increments of temperature correspond to equal increments of "acceleration" of the atoms. This, if it means anything, is not true, and it is not *à priori* at all likely. Take another place, where he says, ". . . . The fact that a given quantity of imparted heat raised a *really* heavy atom to the same temperature as it would a *really* lighter atom, would indicate that equal temperatures were marked by a slow motion of heavy wedges in respect of a heavy atom, and by a quick motion of light wedges in respect to light atoms."

"Although the same quantity of heat might thus be imparted to the two atoms, it is reasonable to infer that the intensity of the heat, as made apparent to our senses, would not in the two cases be identical." ". . . It is reasonable to infer," on the other hand, that there is some hitch in an argument that depends to any important degree upon such a form of expression as "it is reasonable to infer." Is it not, on the other hand, most reasonable to infer that the blow given by a light body moving quickly would be very much the same as by a heavy body moving more slowly, and that, consequently, the "intensity of heat," as he calls it, would feel the same? In any case a very cursory study of the kinetic theory of gases would point out how there is certainly no incongruity or incredibility, but quite the reverse, in the notion that equal quantities of heat do make light atoms move rapidly and heavy ones slowly; and that, notwithstanding their different atomic velocities, the temperatures of two bodies may be the same. There is no real difficulty in supposing that it requires thirty times as much heat to raise the temperature of water as is required to raise an equal mass of gold through the same range of temperature, if we bear clearly in mind the very complex structure of both water and gold, and all that has to be done by the heat in each case, and at the same time recollect how very little we know of the conditions that determine when two bodies are at the same temperature, *i.e.* that determine that on the whole no energy shall flow from one to the other when placed in contact or radiating to one another.

There are many other matters treated of in the book, but if one were to take "Waterdale" at his word and judge "whether the rest of the book is worthy or not of careful perusal" by one's experience of Appendix II. and its supposed proof, nobody would read another word, and unless one had a great deal of leisure to devote to speculative conjectures, or were well paid for it, there does not seem much inducement to wade very carefully through it. "Waterdale" professes to explain gravitation by a sort of hotch-potch of Bjerknes' sound wave attractions and Osborne Reynolds's theory founded on dilatancy. He seems to think that any attempt to explain gravitation is very remarkable. "The author would have thought that when the unusual occurrence of the publication of a work announcing the discovery of gravity and other original theories as important arises, that the scientific world would display sufficient interest in the subject as to read and examine the arguments, although the work might be by an unknown pen." "Waterdale" seems ignorant of the fact that the scientific world has been inundated with theories of gravity and other original theories. To mention only a few of the better known ones, there are Le Sage's corpuscular theory, worked out very carefully by Mr. Tolver Preston and Mr. George Forbes. Others founded on wave motion and fluid flow, such as Bjerknes has popularized, and which Mr. Karl Pearson has devoted so much ingenuity to, though he takes refuge in nondynamical suggestions, such as a fourth dimension, which might just as well be introduced as a region in which a convenient series of strings existed to hold atoms together without any action at all going on in our stupid tridimensional space. What the difference is between such a theory and the good old hypothesis of inherent qualities seems difficult to discover. Then there is the suggestion that every atom is connected to every other by means of vortex filaments, though how the poor things work when they get tangled is rather a difficulty here. Finally, there is Osborne Reynolds's interesting theory founded on dilatancy, which very possibly has a future before it, especially if we consider that the ether is probably full of vortices, and that vortices cannot cut one another. These theories almost all suffer from the apparently incurable defect to which "Waterdale's" is also liable, that they give a rate of propagation of gravity comparable with that of light. Parents are proverbially partial to their children, and "Waterdale" probably will cherish his suggestions as very valuable, notwithstanding this and other serious objections. The confident way in which, after pages of suggestions as to what might happen, he sta es that a current from right to left will produce one effect, while one from left to right will not neutralise it is quite refreshing, but is not an attractive investigation to those who are accustomed to call nothing a proof that is not founded upon something better than suggestions. That gravity is propagated with such amazing rapidity as it is seems to show that it must be an action of the medium to whose *structure* the electromagnetic properties of the ether are due. Such actions are known to exist in a perfect liquid, and it is natural to attribute gravity to such actions. The reasons for attributing great velocity of propagation to gravity are not apparently very well known. The difficulty is owing to the component of the force at right angles to the radius vector that would come in, owing to the aberration of the force, and which would cause an acceleration of areas of planets. This might be partly neutralized by a resisting medium, but hardly completely, especially in the case of comets, because the resisting force would be tangential to the path, while the aberration component would be at right angles to the radius vector. It is possible, by assuming an increase of force due to velocity of approach and a decrease due to recession, to get over this latter difficulty; but even then it is hard to explain the persistent rotation of the earth when the surface is not moving freely as a projectile, and when consequently the supposed exact balance between gravitational acceleration and resistance of medium does not hold. Even then there is the possible suggestion that cohesional and other forces, being similarly propagated in time, would prevent any possible effect being produced by the resisting medium, and so matters return to much as they were at first, and no final answer be given to the questions, "Is gravity propagated in time?" "Does the ether offer resistance to motion?" It remains much in the same position as the question of the motion of the ether at the surface of the earth.

"Waterdale" and others seem to think that fluidity necessarily implies that a medium is divisible into hard or soft particles. No ordinary mind is forced to this conclusion. Most minds look upon water, for instance, as a perfectly continuous medium, any part of which can flow past any other part with perfect freedom. Hardness, softness, and so forth may require structure, but mere fluidity does not. Again, "Waterdale" and others seem to imagine that elasticity essentially involves the *compressibility* of the elastic body: *i.e.* that it must consist of atoms that are themselves compressible. "Waterdale" himself invents a structure for an atom that resists *deformation* without its constituents being themselves compressible, and the existence of vortex rings shows how a perfect liquid can have a real elasticity to deformation given to a part of it by giving it motion without any part being composed of particles, or any part of it being at all compressible.

The rest of "Waterdale's Researches" concern suggestions as to how cohesion, chemical action, light, electricity, &c., may at some future time be explicable by the structure he proposes for the ether, which is to all intents and purposes the same as Osborne Reynolds already has suggested, a whole collection of absolutely hard bodies of different sizes, or, as "Waterdale" suggests, spheres of two different sizes. There is considerable cleverness displayed in the way he has reasoned out for himself such a well-known theorem as that a body moving in a perfect liquid will behave as if its mass were increased, but the labour bestowed upon such a well-known theorem does not entice the reader to try and follow the vague suggestions that follow, and that are much the same as have been over and over again given to show how *every* theory as to the nature of the ether explains a lot of things which can on the face of them be explained by *any* ether through which bodies can move, and upon which they exert pressures. Mixed up with these plausible suggestions are such things as hypothetical whirls of ether within the solar system that seem, to say the least of them, to require some elucidation as to how comets go through them in every sort of direction without any sensible action of the whirl on the comet.

A person who has brought forth, after enormous labour of thought, a series of theorems concerning the universe, and who is not very familiar with the equally carefully thought-out suggestions of others naturally looks with more favour upon his own children than upon those of others; but, if he is reasonable, and in a reasonable mood, he will not be surprised nor even distressed, because those who look at all these children with critical eyes see very serious defects in all of them, and feel very confident that without great changes no one of them can possibly grow into a second Newton.

VERTEBRATE BIOLOGY.

Text-book of Biology. By H. G. Wells, B.Sc. Lond., F.Z.S. With an Introduction by G. B. Howes, F.L.S., F.Z.S., Assistant Professor of Zoology, Royal College of Science, London. Part I. Vertebrata. (London: W. B. Clive and Co., University Correspondence College Press.)

MR. WELLS'S book is avowedly written mainly for the purpose of helping solitary workers to pass the Intermediate Science examination of the University of London, and it would therefore be unfair to criticise it from a wider point of view. The scope for originality in such a work is naturally somewhat limited, but it is a pleasant surprise to come across one which is far above the average as regards soundness of treatment and method. The author not only possesses a practical knowledge of the greater part of the subject he deals with, but also evidently takes pleasure in it for its own sake, and has a healthy dislike of "that chaotic and breathless cramming of terms misunderstood, tabulated statements, formulated 'tips,' and lists of names, in which so many students, in spite of advice, waste their youth." He states that "the marked proclivity of the average schoolmaster for mere book-work has put such a stamp on study that, in nine cases out of ten, a student, unless he is expressly instructed to the contrary, will go to the tortuous, and possibly inexact, description of a book for a knowledge of things that lie at his very finger-tips" (p. 31); and again, on p. 125, that "it is seeing and thinking much more than reading, which will enable" the student "to clothe the bare terms and phrases of embryology with coherent knowledge." Throughout the book the importance of actual observation is insisted upon.

The present part deals with the Rabbit, Frog, Dog-fish, and Amphioxus, and includes an account of the development of these animals and of the theory of evolution, as well as a number of questions, most of which have been set at the examinations of the London University. The morphological portions are, on the whole good and clearly written, and a fair amount of physiology is also introduced. A syllabus of practical work is given at the end: this would in many respects bear amplifying. The student is not warned that his time will be wasted if he wanders off the direct path of the examination syllabus; and on the contrary, points of general biological interest are referred to here and there, and these go far to show what a good many of our elementary text-books do not —viz. that the London University syllabus, "as at present constituted," affords "considerable scope for efficient biological study." The student, moreover, is told that this "little book is the merest beginning in zoology," and the last paragraph, on p. 131, indicates the aspect of mind with which the author regards his subject.

Twenty-four folding sheets of sketches are inserted in the text, but the figures are, on the whole, exceedingly rough; and though many of them may be found useful as guides, we feel that the student would do better to postpone drawing until his dissections are made, or even copy some of the numerous good figures to be found elsewhere, than to "copy and recopy" these sketches first, as advised by the author.

Numerous inaccuracies and awkward expressions occur, only a few of which can be here mentioned. The terms superior and inferior, as applied to the great veins, are likely to confuse a beginner after reading the definition of the regions of the body given on p. 3. "Metabolism" and "metaboly" occur even in consecutive sentences on p. 23. Peristaltic movement is said to move the food "forward" (p. 41). It is stated that the thyroid is similar in structure to the thymus and to "botryoidal tissue" in general (p. 26), and that the epithelium of the villi, with its striated border, "is usually spoken of as leading towards "ciliated" epithelium (p. 22). It is misleading to say that "a tarsus (tarsalia) *equals* the carpus," and that the vomer of the dog is paired (pp. 38 and 76). As the term "Chordata" is adopted on p. 96, it is unfortunate that the student is told on p. 60 that vertebrata occur in which cartilage is absent, and that Amphioxus possesses the "essential vertebrate features," is "*twisted*, as it were," and that its "vertebral column is devoid of vertebræ:" it is, moreover, inadvisable to use the term "hyoidean" with regard to this animal. On p. 61 "classes" and "orders" are used in a correct and an incorrect sense in the same sentence. The expression, "carotid gland" requires a better explanation on p. 67. The morphology of the cardinals, azygos, and post-caval is incompletely explained (pp. 87, 120, and 124). Several serious mistakes are made with regard to the homologies of the urinogenital apparatus (*cf.*, *e.g.* pp. 92 and 114). Misprints are also fairly abundant throughout.

Most of these faults are, however, such as can be remedied in a future edition, and the book will, we think, serve the purpose for which it was written very satisfactorily. W. N. P.

OUR BOOK SHELF.

Pflanzenleben. Von Anton Kerner von Marilaun. Band II. Geschichte der Pflanzen. (Leipzig und Wien: Bibliographisches Institut.)

THE first volume of this excellent book was reviewed in NATURE, vol. xxxix. p. 507. The present volume, which completes the work, treats of the "history of plants," by which is meant their *development*, in the widest sense, including both ontogeny and phylogeny. The former subject ("origin of descendants") occupies the first 480 pages, while the remainder is devoted to the "history of species."

It is not proposed to enter into any detailed criticism of this volume. Some idea of the scope of the work was given in the former notice; we are glad to hear that an English translation is in preparation, and when this appears a further opportunity will be given for a general account of the whole. In point of interest the second volume is fully equal to the first; there is, however, perhaps more room for adverse criticism of certain parts. Speaking quite generally it may be said that while the "biology," or natural history of the subject is admirable, the morphology is on the whole rather weak. The former, however, is the more important for the general reader, for whom the book is intended.

The account of reproduction begins with the asexual organs of propagation, including spores, buds, and gemmæ. This is succeeded by the much more extensive section on reproduction by fruits, including all sexual processes. The great value of this part lies in the extremely full, and in many respects original, treatment of the fascinating subject of the pollination of flowering plants, to which nearly 300 pages are devoted. Special stress is laid here on the phenomena of *geitonogamy*, or the crossing of different flowers on the same inflorescence, and of *autogamy*, or self-fertilisation of hermaphrodite flowers. The whole account is of the greatest possible interest, and familiar as the subject has now become, innumerable fresh points of view are opened up.

The second part of the volume is on the history of species, including the whole subject of variation. Changes produced by external agencies, such as parasitic fungi, and gall-forming insects, form the subjects of special sections.

As regards the origin of new species, the author, like Prof. Weismann, attributes the greatest importance to sexual reproduction, and especially to cross-fertilisation. He occupies a peculiar position in so far as he believes that hybridisation has played an important part in nature as a source of new forms.

This second part of vol. ii. includes classification, and a fairly full account is given of all the important groups of plants, each cohort, or "Stamm," receiving separate treatment.

Sections on the distribution of species, and on their extinction, conclude the book.

A really good index is added, which will be a great boon to all who wish to make use of the vast store of facts which the book contains. The illustrations, consisting of twenty coloured plates and 1547 figures in the text, reach the same high standard as those of the previous volume.

To the book as a whole the highest praise must be given. No such popular account of the natural history of plants has appeared before. The publication of an English version will be anticipated with great interest.

D. H. S.

Bibliografia Medica Italiana. By P. Giacosa, Prof. straord. di Materia Medica e Chimica fisiologica all' Università di Torino. (Torino-Roma: L. Roux e C., 1893.)

THIS work is a collection of abstracts of the chief papers bearing on the medical sciences published by various Italian authors during the year 1892. Prof. Giacosa has been aided in his work by several experts, whose names are a sufficient guarantee for the accuracy of the abstracts, such as Profs. Marcassi of Palermo, Maggiora of Modena, and Sperino of Torino. The medical reading public is familiar with the excellent *Jahrberichte* and *Centralblätter* published in Germany, which deals chiefly, though not exclusively, with scientific papers by German authors. There has been a great want of similar publications of Italian work, and Prof. Giacosa's "Bibliografia" is a welcome addition to medical literature. In it will be found abstracts of all the chief Italian papers published in 1892 on parasites and helminthology (zoology), physiology, biological chemistry, pharmacology, histology, human and pathological anatomy, bacteriology and hygiene. The abstracts are done by experts in the particular subject, are short but clear and intelligible, and have the advantage of not being critical.

The Evolution of Decorative Art. By Henry Balfour, M.A., F.Z.S. (London: Percival and Co., 1893.)

IT is remarkable that in these days, when the question of "origins" holds a place of commanding importance in almost every department of investigation, comparatively little should have been done to trace the evolution of art back to what Mr. Balfour calls "its very simplest beginning." Mr. Balfour does not, of course, undertake to present in this small book anything like a complete view of the subject. His aim is merely to indicate some of the main conclusions to which he has been led by his own researches. He finds in early art three distinct stages—(1) adaptive; the appreciation of curious or decorative effects occurring in nature or as accidents in manufacture, and the slight increasing of the same by artificial means in order to augment their peculiar character or enhance their value as ornaments; (2) creative; the artificial production of similar effects where these do not occur (imitation or copying); (3) variative; gradual metamorphosis of designs by unconscious and conscious variation. Mr. Balfour brings out admirably the significance of these stages, and it is scarcely necessary to say that the Pitt Rivers collection, of which he is curator, provides him with ample means for the clear and effective exposition and illustration of his ideas.

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.]*

Palæontological Discovery in Australia.

MANY readers of NATURE will learn with interest that I have this day received a telegram from Prof. Stirling, of the University of Adelaide, as follows:—

"Made discovery immense deposit fossil remains excavated several nearly complete skeletons Diprotodon besides two thousand bones also large Struthious bird giant Wombat particulars letter."

I need scarcely add that I shall await with impatience the promised particulars of this discovery, which may prove to be one of great importance.

ALFRED NEWTON.

Magdalene College, Cambridge, April 21.

An International Zoological Record.

IT is much to be regretted that the praiseworthy agitation of this subject, opened by Mr. Minchin (NATURE, vol. xlvi. p. 367), has not been continued. There cannot be the slightest doubt of the desirability of such a reform. Possibly the reason why the letters of Mr. Minchin and Mr. Bathers (*ib.* p. 416) have not aroused more interest lies in the fact that they both wrote as recorders. They showed the absurd burdens that the actual system imposes upon the recorders; but they left somewhat in the background the advantages which the great world of zoologists could receive.

However this may be, it is certain that the rank and file of investigators of the present day are supporting an utterly unnecessary burden, and one from which they ardently desire to be freed. Any one who desires to test the sentiment has only to make inquiries among those of his acquaintance. Having myself agitated in this quiet way a method of reform that had occurred to me nearly two years ago, I can hardly doubt that the concourse of opinion is strong enough to effect a radical change, if only concerted action can be taken.

Mr. Minchin and Mr. Bathers have pointed out that the recorders at present do the same work many times over, and suggest a plan by which it can be avoided. The salient feature lies in separating the duties of recorder and bibliographer, and in having the entire mechanical work *done once* for all concerned in the preparation of the record. The plan is an admirable one, but why thus restrict the blessings of a competent bibliographer? The scheme to which I have alluded in the preceding paragraph simply substitutes a *bibliographical bureau* for the bibliographer, a feature necessitated by the additional duties imposed upon it.

The business of recording a publication according to the latter plan may be referred to *three stages*. Let me suppose the bureau constituted at a centre such as the British Museum, and show its working. The *first stage* of recording is conducted wholly by the central bureau, with such aids from outside as might be found expedient. [I refer to assistants in other countries. In the case of Russia it would be at first probably necessary, although in general to be avoided as far as possible.] In the first place the bureau would make complete lists of all zoological papers as they are published. At intervals of a week, or of two weeks, these lists would be given to the press and printed successively in two forms. One would constitute a pamphlet similar to the bibliographical part of the *Zoolog. Anzeig.*, but would give all the titles *promptly*. The other form in which the list would be printed would have the titles widely spaced, would be printed on strong, stout paper, and would appear in sheets, leaving one side blank. These sheets could then be cut up at will into slips of uniform size and shape to serve further bibliographical elaboration. During the printing of the slips it would have been the duty of the bibliographer to have sorted the titles carefully, and, in the case of larger works and works with ill-characterised contents, it would further have been his duty to have ascertained the *topics* dealt with, so that at the end of the period he would be able to *sort and classify* the 150 titles, which appear at present weekly.

Thereupon the second stage of recording would be begun. Each reviewer would receive at once slips indicating the papers concerning him, together with a page-number in the case in which his topic is only incidentally dealt with. Thus the mechanical labour of the reviewer would be reduced to a minimum. Not merely, however, the reviewers could be thus informed, but *also any specialist whose field of work sufficiently* coincided with one of the divisions of the Record to induce him to subscribe to the series. Thus, for example, a worker on the development of the vertebrate nervous system would find his wants admirably met. The second stage of recording would be carried on wholly by the reviewers, who, however, in addition to writing reviews as at present, would also index the topics of the paper in a more detailed way than would be possible for the bibliographer in his first hasty survey; or this work might be left to the bibliographer, who, in what I have called the third stage, collates the reviews which have been returned to him. The reviewer should also note any incidental observations of interest to other reviewers which the bibliographer may have overlooked.

In stage 3 the bibliographical bureau becomes a bureau of publication, and it is believed that with such an organisation the Record for the year could be very promptly issued. At the same time, however, the bureau would be able, by the use of the slips at its disposal, to embody the indexes furnished by the reviewers (or, possibly better, made out by the bibliographer from their abstracts) in a permanent slip index, which would grow with the years and become a record of inestimable value. This part of the plan alone, I see, has been independently advocated by Mr. Cockerell (NATURE, vol. xlvi. p. 442); but, inasmuch as he overlooked the indefinite multiplication made possible by the use of printed slips, he failed to note the highest use which the bureau can serve. To my mind this consists in *informing the individual investigator of every work which concerns his speciality* by sending him the proper slips.

The value of such a service can hardly be exaggerated. It relieves the individual of endless labour: it gives a completeness to his knowledge of the literature that no individual endeavour could attain; and finally, it saves him the annoyance which indefinite titles occasion him in using the ordinary means of seeking for papers relating to his subject. So long as a fundamental observation on the development of the Wolffian Duct can be published under the title, "Observations on the Lymph," so long as the bulletins announce "Contributions to the Development of the Vertebrates," we have no right to expect authors to have a full mastery of their subject, unless they can receive aid from a central bureau such as I have described.

The expense of maintaining at several points a complete index, such as that in the bibliographical bureau, is not such as to make it infeasible, and I fancy it would be done in several zoological centres. The labour of the bureau would probably assume considerable proportions; but, inasmuch as it would in each case save much more of the scattered and oft-repeated labour of individuals, it would be quite self-supporting. For the perfect working of the scheme it is important that authors should send "extras" to the bibliographer. Mr. Bathers suggests that they would gladly do this if there were only one asking for them instead of a number, as is now the case. Here Mr. Bathers again writes as a recorder. I was unaware that papers were desired, and would not know even where to send a copy. With the scheme I have proposed, also those who now unintentionally withhold their papers would contribute them; for the organisation would at least be well known.

Respecting further details, there is no occasion of bringing these forward now. I may simply add that I have had opportunity of seeing paper slip catalogues in use in a very large scale in the Government service in Russia, and learned that they gave excellent satisfaction. It may be also of interest to any who may further concern themselves with this subject that the present volume of zoological publication is not far from 2000 pages weekly.

I have made inquiries among many of my friends in different countries in respect to their interest in such a plan as I here propose, and it has received such endorsement that I cannot doubt that it affords a remedy for a real evil. I am well aware that such a plan needs to be much modified; but I submit it in this form. I have already a long list of persons and institutions who have promised to subscribe to the slips, could they be obtained at a reasonable price; among others of librarians, who would use them to save copying in making out the "card catalogues" in vogue in America. This support was obtained when the scheme was but little elaborated, and when there was almost no prospect of success. I am confident that were the undertaking once begun the support would be very great. It needs organised action such as the various scientific bodies can give it. Let the British Association appoint a committee and invite others to join them in forming an International Commission, or let them respond should the call come to them. Let all considerations of national pride be set aside. Surely England, with her enormous library and museum facilities, will receive her share.

Leipzig, Germany, April 16. HERBERT H. FIELD.

Lion-tiger and Tiger-lion Hybrids.

SINCE the date of my previous communication on the above subject (see NATURE, p. 390) I have had some correspondence with Mr. John Atkins, son of Mr. Thomas Atkins, the result of which has been not only to clear up several discrepancies which I pointed out as occurring in the previously published accounts by Sir Wm. Jardine and Mr. Griffiths, but moreover it enables me to present for the first time a detailed account of what, so far as I can ascertain, are the only authenticated cases of the interbreeding of a lion and tigress. I am aware of the classical references to the reputed breeding of the leopard and lioness; but that part of the subject I do not propose to discuss now. In the first place I should state that the proprietor of the menagerie, when the first hybrids were seen, was Mr. Thomas Atkins, not "F." or "J. Atkins" as quoted previously. Mr. John Atkins came into possession later on. The parents of the hybrids were the same all through for ten years, from 1824 to 1833, during which period six litters were born. The lion was bred in Mr. Atkins's menagerie from a Barbary lion and a Senegal lioness. The tigress was born in the Marquis of Hastings's collection in Calcutta, and was purchased when about eighteen months old by Mr. T. Atkins from a captain, to whom she had been given by the Marquis. Being of the same age as the lion, she was placed together with him in the same cage, and two years afterwards she proved to be in cub.

The following statement regarding the successive litters has been revised by Mr. John Atkins, and as he has preserved notes of the facts which are recorded, they may be accepted as authentic. I need hardly add that but for his ready and full response to my queries this account could not have been written.

First Litter.—Born October 24, 1824, at Windsor, two males and one female. Reared by terrier bitch, all died within a year. They were exhibited to King George IV. at the Royal Cottage, Windsor, on November 1, 1824.

Second Litter.—Born April 22, 1825, at Clapham Common; there were three cubs, sexes not recorded. Reared by the mother, as also were all the subsequent litters. They only lived a short time.

Third Litter.—Born December 31, 1826 or '27, at Edinburgh, one male and two females. As stated in the previous paper, the year is given as 1827 in the handbill of the menagerie from which I quoted, and the other references seem to support that date; but Mr. John Atkins says it is given as 1826 in a printed catalogue in his possession.

Fourth Litter.—Born October 2, 1828, at Windsor, one male and two females.

Fifth Litter.—Born May, 1831, at Kensington, three cubs, sexes not recorded. They were shown to the Queen, then Princess Victoria, and to the Duchess of Kent. The whole group performed in a specially constructed cage at Astley's Amphitheatre, and in 1832 were taken by Mr. Atkins for a tour in Ireland. To a separate account of this tour reference has been made in my previous paper.

Sixth Litter.—Born July 19, 1833, at the Zoological Gardens, Liverpool, one male and two females. One, the male, lived for ten years in the gardens. The young male lion-tigers when about three years old had a short mane something like that of an Asiatic lion; the stripes became very indistinct at that age.

Mr. Atkins informs me that there is a badly stuffed specimen of one cub which was about a year old in the Museum at Salisbury, and from Mr. Harmer's letter (see NATURE, p. 413) there is one also in Cambridge.

From the account quoted by him it would seem improbable that that particular specimen, had it survived, could have bred. As a matter of fact I learn from Mr. Atkins that none of them ever did breed, though he does not know of any reason why they should not have done so.

Mr. Atkins thinks that the cubs of the earlier litters died from over-feeding; when he adopted a different treatment he had no difficulty in rearing them.

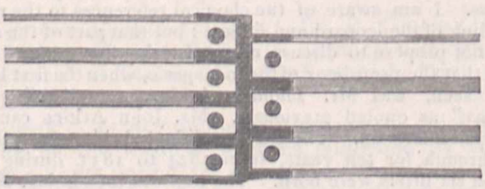
In my previous paper, in the quotation from Griffiths, the word "superfineness" should read "superficies."

This record, it may be noted, while correcting some errors in the previously published accounts, also extends over a period subsequent to all of them. V. BALL.

Science and Art Museum, Dublin, April 15.

Soot-figures on Ceilings.

As the subject of dust-images was recently considered in some interesting letters in NATURE, I wish to record an example of a soot-image which was far more detailed and remarkable than any I have yet seen. The example is to be found on the ceiling of the billiard-room in the Golf Club House at Felixstowe. Abundant soot has been deposited above the lamps by which the table is lighted, and this is distributed so as to map out on the ceiling not only the outline of the joists, but that of the laths and even of the nails by which the ends of the latter are secured. The mark corresponding to the nail-head is certainly much larger



than the latter. I made rom memory a rough sketch of the appearance, which is reproduced in the accompanying woodcut. I may be mistaken in the position of some of the light and dark shades. If the example is as new to others as it was to me it would be interesting to have a photograph of the ceiling before it is again whitewashed. E. B. POULTON.

Oxford, April 17.

THIS phenomenon is often observed, though not often so clearly as in the case noticed by Mr. Poulton. It is due to the same cause as produces the dust-free space seen rising from hot bodies in illuminated smoky air, viz. a peculiar Crookesian (or rather Osborne Reynoldsian) bombardment of sufficiently small dust-particles, in the direction of decreasing temperature, by the extra energy of the gas-molecules on one side See papers by myself and the late Mr. Clark in NATURE (especially July 26, 1883, April 24, 1884, vol. xxix. p. 417, and January 22, 1885), and in Phil. Mag., 1884, Proc. R.I., &c.; also by Mr. Aitken, Trans. R.S. Edin., 1884. And see the remarkable theoretical paper by Prof. Osborne Reynolds on "Dimensional Properties of Gases," Phil. Trans., 1879.

Dust gets bombarded out of hot air on to all colder surfaces. The details of this effect are specially given by Mr. Aitken in NATURE, vol. xxix. p. 322. The badly-conducting plaster of a ceiling is no doubt fully heated by contact with the air below except in places where the conducting power of wood or iron keeps it comparatively cool; hence the picking-out of the pattern. Solid deposit from warm air on to cool surfaces can occur without any actual smoke; *e.g.* it can be noticed above incandescent lamps. OLIVER LODGE.

The Use of Ants to Aphides and Coccidæ.

I HAVE just had an opportunity of seeing Dr. Romanes' interesting work, "Darwin, and after Darwin," and find therein (p. 292) the production of honey-dew by Aphides adduced as a difficulty in the way of the Darwinian theory. I have not paid any particular attention to Aphides, but have lately been much interested in the allied Coccidæ, which, since they produce a similar fluid attracting ants, may be considered to offer a parallel instance. Both Coccidæ and Aphides suffer from many predaceous and parasitic enemies, and there seems to be no doubt that the presence of numerous ants serves to ward these off, and is consequently beneficial. There is an interesting Coccid, *Icerya rosæ*, which I find on Prosopis here, and on more than one occasion I have been unable to collect specimens without being stung by the ants. At the present moment some of these Iceryæ are enjoying life, which would certainly have perished at my hands, but for the inconvenience presented by the numbers of stinging ants.

Belt and Forel have also written on the protection of Coccidæ by ants ("Naturalist in Nicaragua;" and *Bull. Soc. Vaud.*, 1876). Maskell has given an account of the honey-dew organ of Coccidæ, from which it appears that it is something more than a mere organ for the excretion of waste products. This author also figures some of the fungi which grow on honey-dew, and it may well be that these also serve to prevent the attacks of enemies. When, as we sometimes see in Jamaica, the leaves appear to be coated with soot (***Antennaria robbinsii*** is the fungus), it cannot be so convenient for coccinellid larvæ, Chrysopa larvæ, &c., to crawl about on them in search of Coccidæ.

Jamaica, April 3. T. D. A. COCKERELL.

Blind Animals in Caves.

IN his last letter (p. 537) Mr. J. T. Cunningham states that the "early stages" of the European Proteus have not yet been obtained. This assertion is incorrect. In 1888 and 1889 the oviposition and development have been described by E. Zeller (*Zool. Anz.*, 1888, No. 290, and *Jahresh. Ver. Naturk. Württ.*, xlv., 1889, p. 131, plate iii.), who gives a coloured figure of the larva, and particularly refers to the development of the eyes. As early as 1831 (Oken's "Isis," 1831, p. 501) Michahelles remarked that the eyes in young specimens are more distinct and somewhat larger than in the adult. G. A. BOULENGER.

OBSERVATIONS IN THE WEST INDIES.[1]

HERE we are back at Nassau for the third time, and thinking you might be interested to hear of my cruises, I send you a short sketch of our trip. The first time we left Nassau we entered the Bahama Bank at Douglass Channel and crossed the bank to North Eleuthera, where we examined the "Glass Window" and the northern extremity of Eleuthera, we then sailed along the west shore of the island close enough to get a good view of its characteristics as far as Rock Harbour at the

[1] A letter from Alexander Agassiz to J. D. Dana; dated Steam Yacht *Wild Duck*, Nassau, March, 1893. Printed in the *American Journal of Science* for April, and communicated to NATURE by the author.

southern end. We steamed out into Exuma Sound through the Powell Channel and round the southern end of Eleuthera to little San Salvador, and the north-west end of Cat Island, where are the highest hills of the Bahamas. We then skirted Cat Island along its western face, rounded the southern extremity and made for Riding Rocks on the Western side of Watling's Island. We circumnavigated Watling, passed over to Rum Cay, then to northern part of Long Island, visiting Clarence Harbour; next we crossed to Fortune Island, and passed to the east side near the northern end of the island on the Crooked Island Bank. From there we crossed to Caicos Bank, crossing that bank from French Cay to Long Island, passed by Cockburn Harbour and ended our eastern route at Turks Island; from there we shaped our course to Santiago de Cuba to coal and provision the yacht. We were fortunate enough to strike Cape Maysi a short time after daylight, and I thus had a capital chance to observe the magnificent elevated terraces (coral reefs) which skirt the whole of the southern shore of Cuba from Cape Maysi to Cape Cruz and make so prominent a part of the landscape as seen from the sea. We were never more than three miles from shore and had ample opportunity to trace the course of some of the terraces as far as Santiago, and to note the great changes in the aspect of the shores as we passed westward due to the greater denudation and erosion of the limestone hills and terraces to the west of Cape Maysi, which seems to be the only point where five terraces are distinctly to be seen. The height of the hills east of Pt. Caleta, where the terraces are most clearly defined, I should estimate at 900 to 1000 feet; though the hills behind the terraces, which judging from their faces are also limestone, reach a somewhat greater height, perhaps 1100 to 1200 feet.

After coaling at Santiago de Cuba we visited Inagua, and next steamed to Hogstey Reef, a regular horseshoe-shaped atoll with two small cays at the western entrance. There we passed three days studying the atoll. This to me was an entirely novel experience; to be at anchor in 3 fathoms of water 45 miles from any land with water 900 fathoms within three miles outside, surrounded by a wall of heavy breakers pounding upon the narrow annular reef which sheltered us. I made some soundings in the lagoon and on the slope of reef outside. From there we returned to Crooked Island Bank to the westward of which I also made some soundings to determine the slope of the Bank. We next again visited Long Island, taking in the southern and northern ends which I had not examined. From there we passed to Great Exuma, stopping at Great Exuma Harbour and sounding into deep water on our way out to Conch Cut when we sailed west crossing the Bank to Green Cay. From there we made the southward face of New Providence, and before going into Nassau Harbour made some trials in deep water in the Tongue of the Ocean with the *Tanner* deep-sea townet in 100 and 300 fathoms, depth being 700 fathoms—after which we returned to Nassau. I had on board a Tanner sounding machine kindly loaned me for this trip by Colonel McDonald of the Fish Commission, and some deep-sea thermometers were also kindly supplied by him and by Prof. Mendenhall, the superintendent of the U.S. Coast Survey. I supplied myself with a number of Tanner deep-sea townets and with a supply of dredge and of townets and carried on board a Yale and Towne patent winch for winding the wire rope which I used in my dredging and towing in deep water. The yacht was provided with a steam capstan and by increasing its diameter with lagging we found no difficulty in hauling in our wire rope at the rate of 8 min. to 100 fathoms. I carried 600 fathoms of steel wire dredging rope with me of the same dimensions which I had used on the *Blake* and which has also been adopted on the *Albatross*. During our second cruise we steamed from Nassau for Harvey Cay crossing the Bank from north to south to Flamingo Cay, and then to Great Ragged Cay, from which we took our departure for Baracoa. At Baracoa I hoped to be able to ascend the Yunque; unfortunately I had to give up my trip owing to the desperate condition of the roads. From Baracoa we steamed close to the shores to the westward, touching at Port Banes, Port Padre, Cay Confites, Sagua, Cape Frances, Cardenas, Matanzas, and finally ending at Havana. This trip was a continuation of the observations we made on the south coast of Cuba and enabled me to trace the gradual disappearance of the terraces from Baracoa to Nuevitas, and their reappearance from Matanzas to Havana, from the same causes which evidently influenced their state of preservation from Cape Maysi west. I also got a pretty clear idea of the mode of formation of the fine harbours found on the northern coast of Cuba to the eastward of Nuevitas, and of the mode of formation of the extensive systems of cays reaching from Nuevitas to Cardenas and which find their parallel on the south coast of Cuba from Cape Cruz to Cape Corrientes. After refitting at Havana we left for Nassau. Both on going into Havana and on leaving we spent the greater part of a day in towing with the Tanner net. I thought I could not select a better spot for finally settling the vertical distribution of pelagic life than off Havana which is in deep water—900 fathoms—close to land, on the track of a great oceanic current, the Gulf Stream, noted for the mass of pelagic life it carries along its course. We towed in 100, 150, 250, and 300 fathoms and on the surface at or near the same locality, and I have found nothing to cause me to change the views which I expressed in my preliminary reports of the *Albatross* expedition of 1891. Nowhere did I find anything which was not at some time found also at the surface. At 100 fathoms the amount of animal life was much less than in the belt from 100 fathoms to the surface. At 150 fathoms there was still less and at 250 fathoms and 300 fathoms the closed part of the Tanner contained *nothing*. At each one of these depths we towed fully as long as was required to bring the net to the surface again. Thus we insured before the messenger was sent to close the lower part of the bar as long a pull through water as the open part of the net would have to travel till it reached the surface, giving the fauna of a horizontal column of water at 100, 150, 250, and 300 fathoms of the same or greater length than the vertical column to the surface for comparison of their respective richness. From Havana we steamed to Cay Sal Bank, visited Cay Sal, Double-headed Shot Cays, Anguila Islands, and then crossed over to the Great Bank to the west of Andros Island. The bottom of this bank is of a most uniform level, 3 and 3½ fathoms for miles and then very gradually sloping to the west shore of Andros, so that we had to anchor nearly six miles from the "Wide Opening" of the central part of Andros which we visited. The bottom consists of a white marl, resembling when brought up in the dredge newly mixed plaster of Paris, and having about its consistency just as it begins to set. This same bottom extends to the shore; and the land itself, which is low where we went on shore not more than 10 to 15 inches above high-water mark, is made up of the same material, which feels under foot as if one were treading upon a sheet of soft india rubber; of course on shore the marl is drier and has the consistency of very thick dough. It appears to be made up of the same material as the æolian rocks of the rest of the Bahamas, only that it has become thoroughly saturated with salt water, and in that condition it crumbles readily and is then triturated into a fine impalpable powder almost like deep sea ooze which covers the bottom of the immense bank to the west of Andros. After leaving Andros we crossed the bank again to Orange Cay and followed the eastern edge of the Gulf Stream to see Riding Rocks, Gun Cay, and the Beminis. We then passed to Great Isaac, where we saw some huge masses of æolian rocks which had been thrown up along the slope of the cay about 80 feet from high-water mark to a height of 20 feet. One of these masses was 15′ 6″ × 11″ × 6′. We then kept on to Great Stirrup Cay coasting along the Berry Cays, crossed over to Morgan's Bluff, on eastward of Andros down to Mastic Point on the same Sound, and then returned to Nassau.

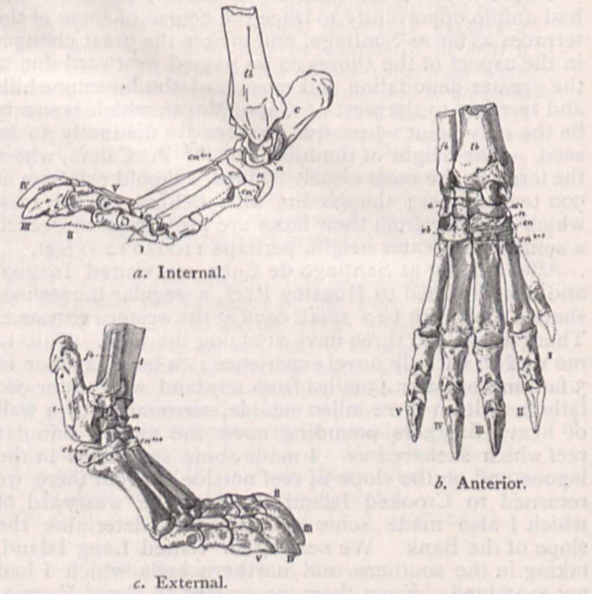
The islands of the Bahamas (as far as Turks Island) are all of æolian origin. They were formed at a time when the Banks up to the 10-fathom line must have been practically one huge irregularly shaped mass of low land, from the beaches of which successive ranges of low hills, such as we still find in New Providence, must have originated. After the islands were thus raised there was an extensive gradual subsidence which can be estimated at about 300 feet, and during this subsidence the sea has little by little eaten away the æolian lands, leaving only here and there narrow strips of land in the shape of the present islands. Inagua and Little Inagua are still in the original condition in which I imagine such banks as the Crooked Island Banks, Caicos Banks, and other parts of the Bahamas to have been; while the process of disintegration going on at the western side of Andros shows still a broad island which will in time leave only the narrow eastern strip of higher land (æolian hills) on the western edge of the tongue of the ocean. Such is the structure also of Salt Cay Bank which owes its present shape to the same conditions as those which have given the Bahamas their present configuration. My reason for assigning a subsidence of 300 feet is the depth of some of the deep holes which have been surveyed on the bank and which I take to be submarine blow-holes or caverns formed in the æolian limestone of the Bahama hills when they were at a greater elevation than now. This subsidence explains satisfactorily the cause of the present configuration of the Bahamas, but teaches us nothing in regard to the substratum upon which the Bahamas were built. The present reefs form indeed but an insignificant part of the topography of the islands and have taken only a secondary part in filling here and there a bight or a cove with more modern reef rock, thrown up against the shores so as to form a coral reef beach such as we find in the Florida Reef. I have steamed now nearly 3300 miles among the Bahamas, visiting all the more important points and have made an extensive collection of the rocks of the group.

I hoped to have made also a larger number of deep soundings than I have been able to take; unfortunately the trades were unusually heavy during the greater part of my visit to the Bahamas, greatly interfering with such work on a vessel no larger than the *Wild Duck*—127 feet on the water line. For the same reason the number of deep-water pelagic hauls was also much smaller than I hoped to make, as in a heavy sea the apparatus would have been greatly endangered. It is a very different thing to work at sea in a small yacht like the *Wild Duck* or in such vessels as the *Blake* and the *Albatross* of large size and fitted up with every possible requirement for deep sea work. The *Wild Duck*, on the other hand, was admirably adapted for cruising on the Bahama Banks, her light draught enabling her to go to every point of interest and to cross and recross the banks where a larger vessel could not follow. I am under the greatest obligations to my friend Mr. John M. Forbes for having so kindly placed his yacht at my disposal for this exploration, and I hope soon after my return to Cambridge to publish more in detail the results of this examination of the structure of the Bahamas.

ARTIONYX—A CLAWED ARTIODACTYLE.

If any further evidence were needed to disprove Cuvier's famous generalisation, it is found in the recently discovered hind foot of Artionyx. In this foot each of the digits with all the phalanges are modified very much as in the primitive bears, and combined with metatarsals and an ankle joint almost identical with those of the pigs. The termination of the limb in claws would have led Cuvier to predict that the whole skeleton and the dentition was of a clawed or carnivorous type, whereas in this animal we find the foot alone belongs to two types as widely separated as can be, and the probabilities are that the skeleton and teeth are also mixed in character.

The foot of Artionyx was found last summer by the American Museum party under Dr. Wortman, in the same beds with the remarkable Protoceras recently described in Nature. It belonged to an animal about the size of a peccary. The terminal claws were first exposed, and although found uncleft, they at once suggested a reference to Chalicotherium, for which the party was keeping a sharp look-out; but a further removal of the matrix showed a pes of an entirely distinct character. In the foot of *Chalicotherium magnum* of the Upper Miocene of France we find three toes, thus odd in number, but not strictly perissodactyle, for the largest is


a. Internal. *b.* Anterior. *c.* External.

Right hind foot of *Artionyx gaudryi*.

not the median but the outer toe. Above the toes is an ankle joint of a modified perissodactyle type, that is, the astragalus is grooved upon its tibial side, and flattened where it rests upon the navicular. The navicular and cuneiforms are also flattened, so that the foot must have been placed somewhat at an angle with the leg, as it is in the Sloths. In Artionyx, on the other hand, there are five digits; the first, or thumb, was a dew-claw, very much shorter than the rest; the remaining four, as shown in *b* of the figure, are nearly symmetrically placed in pairs on either side of the median line, precisely as in the Artiodactyla. This has suggested the name of the animal, its even-numbered toes terminating in claws. Above these elements we have a coalescence of the outer and middle cuneiforms as in many Artiodactyla. The cuboid, navicular, astragalus, and calcaneum, are also modified precisely as in the artiodactyles. The fibula comes down upon the heel bone, and there is the characteristic double hinge. The tibia is strongly interlocked on the outer side of the astragalus. The three accompanying cuts exhibit the peculiar features of this foot; the side views showing that the animal was digitigrade like the cats, and not plantigrade like the bears, although the claws were more of the bear than the cat type.

The discovery of this foot is one of those complete surprises which render palæontological research so fascinating. The existence of such a type was not even suspected, for nothing at all similar has ever been found before. We were daily expecting to find remains of Chalicotherium in the Lower Miocene of America, but no one could have anticipated the complete counterpart in foot structure which this animal exhibits. Of course it will remain an open question whether Artionyx is actually related to the other type until we procure more of its skeleton, and especially of its teeth. This discovery seems to support Cope's opinion that Chalicotherium represents a distinct order—the Ancylopoda, including animals of an ungulate type of skeleton, with unguiculate phalanges. The writer has recently suggested that this order may have been given off from the most primitive hoofed mammals, the Condylarthra, at a period when they still exhibited many of the characters of their clawed ancestors. If this supposition is correct, and Artionyx proves to be a member of the Ancylopoda, it will very possibly present a unique double parallelism with the subdivisions of the Ungulata, Chalicotherium representing an odd-clawed division—the Perissonychia, and Artionyx an even-clawed division—the Artionychia—these divisions being parallel with the perissodactyle and artiodactyle ungulates. This is advanced as a provisional hypothesis, pending the discovery of additional remains. HENRY F. OSBORN.

THE HODGKINS FUND PRIZES.

IN October, 1891, Thomas George Hodgkins, Esq., of Setauket, New York, made a donation to the Smithsonian Institution, the income from a part of which was to be devoted "to the increase and diffusion of more exact knowledge in regard to the nature and properties of atmospheric air in connection with the welfare of man."

With the intent of furthering the donor's wishes, the Smithsonian Institution now announces the following prizes to be awarded on or after July 1, 1894, should satisfactory papers be offered in competition:—

1. A prize of 10,000 dollars for a treatise embodying some new and important discovery in regard to the nature or properties of atmospheric air. These properties may be considered in their bearing upon any or all of the sciences—*e.g.* not only in regard to meteorology, but in connection with hygiene, or with any department whatever of biological or physical knowledge.

2. A prize of 2000 dollars for the most satisfactory essay upon (A) the known properties of atmospheric air considered in their relationships to research in every department of natural science, and the importance of a study of the atmosphere considered in view of these relationships; (B) the proper direction of future research in connection with the imperfections of our knowledge of atmospheric air, and of the connections of that knowledge with other sciences. The essay, as a whole, should tend to indicate the path best calculated to lead to worthy results in connection with the future administration of the Hodgkins foundation.

3. A prize of 1000 dollars for the best popular treatise upon atmospheric air, its properties and relationships (including those to hygiene, physical and mental). This essay need not exceed 20,000 words in length; it should be written in simple language, and be suitable for publication for popular instruction.

4. A medal will be established, under the name of "The Hodgkins Medal of the Smithsonian Institution," which will be awarded annually or biennially, for important contributions to our knowledge of the nature and properties of atmospheric air, or for practical applications of our existing knowledge of them to the welfare of mankind. This medal will be of gold, and will be accompanied by a duplicate impression in silver or bronze.

The treatises may be written in English, French, German, or Italian, and should be sent to the secretary of the Smithsonian Institution, Washington, before July 1, 1894, except those in competition for the first prize, the sending of which may be delayed until December 31, 1894.

The papers will be examined and prizes awarded by a committee to be appointed as follows:—One member by the secretary of the Smithsonian Institution, one member by the President of the National Academy of Sciences, one by the President *pro tempore* of the American Association for the Advancement of Science, and the committee will act together with the Secretary of the Smithsonian Institution as member *ex officio*. The right is reserved to award no prize if, in the judgment of the committee, no contribution is offered of sufficient merit to warrant an award. An advisory committee of not more than three European men of science may be added at the discretion of the Committee of Award.

If no disposition be made of the first prize at the time now announced, the Institution may continue it until a later date, should it be made evident that important investigations relative to its object are in progress, the results of which it is intended to offer in competition for the prize. The Smithsonian Institution reserves the right to limit or modify the conditions for this prize after December 1, 1894, should it be found necessary. Should any of the minor prizes not be awarded to papers sent in before July 1, 1894, the said prizes will be withdrawn from competition.

A principal motive for offering these prizes is to call attention to the Hodgkins Fund and the purposes for which it exists, and accordingly this circular is sent to the principal universities and to all learned societies known to the Institution, as well as to representative men of science in every nation. Suggestions and recommendations in regard to the most effective application of this fund are invited.

It is probable that special grants of money may be made to specialists engaged in original investigation upon atmospheric air and its properties. Applications for grants of this nature should have the indorsement of some recognised academy of sciences or other institution of learning, and should be accompanied by evidences of the capacity of the applicant in the form of at least one memoir already published by him based upon original investigation.

To prevent misapprehension of the founder's wishes it is repeated that the discoveries or applications proper to be brought to the consideration of the Committee of Award may be in the field of any science or any art without restriction, provided only that they have to do with "the nature and properties of atmospheric air in connection with the welfare of man."

Information of any kind desired by persons intending to become competitors will be furnished on application.

All communications in regard to the Hodgkins Fund, the Hodgkins Prizes, the Hodgkins Medals, and the Hodgkins Fund Publications, or applications for grants of money, should be addressed to S. P. Langley, Secretary of the Smithsonian Institution, Washington, U.S.A.

S. P. LANGLEY,
Secretary of the Smithsonian Institution.

Washington, March 31, 1893.

THE SOLAR ECLIPSE.

THE telegrams relating to the total solar eclipse of April 16 indicate that the observations at the various centres were carried on under very favourable conditions. The Senegal party—which will be home next week—was evidently remarkably successful. Prof. Thorpe, who was in charge of this expedition, sent to Lord Kelvin the following telegram:—"April 19, 1893. Thorpe to President Royal Society, Burlington House, London. Eclipse successfully observed at Fundium. Position good, weather fine, very slight haze. Slit spectroscope good, but mainly prominence lines; calcium and hydrogen seen projected on moon. Thirty prismatic camera photographs, eighteen excellent; mainly prominence lines; corona lines doubtful. Ten coronograph pictures, six very good. Photometric work successful; twenty comparisons with equatorial, eleven with integrating apparatus. Deslandres and Colculesco also observed at Fundium, with good results. No word from Bigourdan at Joal. Health of expedition good. Blonde leaves for Teneriffe to-morrow.—Thorpe."

With regard to the work of the same expedition, a correspondent of the *Times* telegraphed from Bathurst on April 19:—"The solar eclipse was successfully observed at Fundium, Senegal. The weather was fine, with only a very slight haze. The results of the slit spectroscope were good. Thirty prismatic camera photographs were taken, eighteen of which are excellent, while of ten coronograph pictures six are very good. The photometric work was successful, and twenty comparisons were taken with the equatorial and eleven with the integrating apparatus. The French astronomers, MM. Deslandres and Colculesco also made observations at Fundium with good results. The health of the expedition is excellent."

Last week we gave the substance of a telegram regarding Prof. Pickering's observations at Minasaris. The *New York Herald* has published a telegram from Valparaiso, containing the following supplementary information as to Prof. Pickering's work:—"The sunlight changed during the period of totality and presented a pale yellow hue. A faint chill was perceptible in the air. The photographic results with the differential spectroscope give twenty lines in the solar atmosphere at a time of 34 seconds previous to totality. Two rays of light were seen issuing from the cusps, their terminal points corresponding to the horns of the new moon. The cusps were in violent motion. The corona showed a conical structure with a network of fine filaments visible to the naked eye. Four light streamers from the corona were noticeable, and seven prominences were observed, which latter were estimated to attain a height of 80,000 miles. The integrating spectroscope showed one red, one yellow, and one blue line and two green lines in the corona. The prominences were well photographed."

The following is a Reuter's telegram from San Francisco, relating to the work of the American expedition to Chili:—"Prof. Holden, the director of the Lick Observatory, has received a telegram from Prof. Schaebele, the leader of the American expedition to Chili, stating that his observation of the sun's total eclipse was successful. The drawings of the corona made a year ago by Prof. Schaebele were found to be a true representation of the corona actually visible in the present eclipse. Fifty photographs were secured by means of the three telescopes used by the observers. One of these gave an image of the sun 4 in. in diameter, and the corona covered a plate 18 by 22 in."

NOTES.

All the most essential arrangements have now been made for the Nottingham meeting of the British Association. The first general meeting will be held on Wednesday, September 13, at 8 p.m., when Sir Archibald Geikie will resign the chair, and Dr. J. S. Burdon Sanderson will assume the presidency and deliver an address. On Thursday evening, September 14, there will be a soirée; on Friday evening a discourse will be delivered by Prof. Arthur Smithells on "flame"; on Monday evening Prof. Victor Horsley will deliver a discourse "on the discovery of the physiology of the nervous system"; on Tuesday evening there will be another soirée; and on Wednesday afternoon, September 20, the concluding general meeting will be held. Excursions to places of interest in the neighbourhood of Nottingham will be made on the afternoon of Saturday, September 16, and on Thursday, September 21. The following will be the presidents of sections:—A (Mathematical and Physical Science), Prof. R. B. Clifton, F.R.S.; B (Chemistry and Mineralogy), Prof. J. E. Reynolds, F.R.S.; C (Geology), Mr. J. J. H. Teall, F.R.S.; D (Biology), Rev. H. B. Tristram, F.R.S.; E (Geography), Mr. H. Seebohm; F (Economic Science and Statistics), Prof. J. S. Nicholson; G (Mechanical Science), Mr. Jeremiah Head; H (Anthropology), Dr. Robert Munro.

The Chemical Society will hold on Friday, May 5, a Hofmann Memorial Meeting. Addresses will be delivered by Lord Playfair, Sir F. A. Abel, and Dr. W. H. Perkin.

The annual dinner of the Royal Geographical Society will take place on Saturday, May 13, at the Whitehall Rooms, Hôtel Métropole, Sir Mountstuart E. Grant Duff in the chair.

At the recent Graduation Ceremony of the University of St. Andrews the honorary degree of LL.D. was conferred on Prof. Henry E. Armstrong, Ph.D., F.R.S., in recognition of his eminent services to organic chemistry.

On Thursday, May 4, the forty-first anniversary of the election of the Secretary of the Institution of Civil Engineers as an Associate, the first "James Forrest" lecture will be delivered by Dr. W. Anderson, F.R.S., the subject being "The Interdependence of Abstract Science and Engineering."

The City and Guilds of London Institute has forwarded to county councils throughout the kingdom, and to the secretaries of technical schools in connection with the Institute, a circular letter indicating various ways in which it has improved and enlarged the scope of its technological examinations. Among the alterations may be mentioned the addition of practical tests in photography, boot and shoe manufacture, goldsmiths' work, mechanical engineering, and other subjects; the subdivision of many subjects into sections to suit the requirements of different branches of the same trade; and the addition of examinations in such subjects as manual training and dressmaking. After careful consideration of the difficult questions involved in the organisation, for the first time, of a system of inspection of technical classes, the Committee of the Institute have adopted a scheme, and are prepared to receive applications from county councils or school committees for the inspection of classes in technical (other than agricultural) subjects, and also for special reports on the results of the examination of the students of separate classes registered under the Institute.

It has been resolved by the Council of the Zoological Society of London to award the Society's Silver Medal to Donald Cameron, of Lochiel, and John Peter Grant, of Rothiemurchus, in recognition of the efforts they have made to protect the Osprey (*Pandion haliätus*) in Scotland. The osprey, formerly common in many parts of the British Islands, has become so rare of late years that it is stated that only three pairs of this bird have been known to breed in this country for some years past.

The hon. secretaries of the Australasian Association for the Advancement of Science are sending out invitations to the leading scientific societies in Europe drawing attention to the meeting of the Association, which will be held in Adelaide, commencing on September 25 next. Sydney, Melbourne, Christchurch, and Hobart are the places in which the previous meetings of the Association have been held. The meeting in

Adelaide will be presided over by Prof. Ralph Tate, of the University in that city.

THE late Admiral Marquis Ricci of Genoa, formerly Minister of Marine of the Kingdom of Italy, has left a large sum, estimated to amount to about three million lire (£120,000) to the authorities of his native city, for the purpose of founding a Scientific Institution. It is believed that this is likely to be devoted to a new site and building for the Museo Civico of Genoa, an Institution which, under the directorship of the Marquis G. Doria, has, as is well known to all naturalists, carried on splendid work in zoology for many years. We are sure that no better object could be selected for the appropriation of this munificent donation.

MR. G. W. LICHTENTHALER, who died lately at San Francisco, bequeathed to the Illinois Wesleyan University at Bloomington—where he had lived during most of his life—his very valuable natural history collection. It includes from 6000 to 8000 species of shells, 1000 species of marine algæ, and 500 species of ferns, besides thousands of duplicates. Mr. Lichtenthaler also bequeathed 500 dollars to put the collection in suitable shape for preservation.

IN connexion with the International Congress of Medicine and Hygiene, to be held in Rome next September, there will be an exhibition opened (from September 15 to October 15) for apparatus, plans, materials, models, &c., relating to the following: Research in biology, therapeutics and hygiene, medical practice, improvement of the soil, sanitation and hygienic service of towns, hygiene of the interior of public and private buildings, individual hygiene, the health of workpeople, hydrology and balneo-therapeutics, &c. Diplomas and medals will be awarded. For information on the subject application is to be made to the President, Prof. L. Pagliani, Minister of the Interior, Rome.

A NEW scientific society has been organised in Washington, called "The Geological Society of Washington." There are already more than a hundred members. The object of the society is the presentation of short notes on work in progress rather than the reading of elaborate papers. At the first meeting Major J. W. Powell, director of the U.S. Geological Survey, presided, and papers were read by Mr. H. W. Turner, on the structure of the gold belt of the Sierra Nevada, and by Mr. S. F. Emmons on the geological distribution of ore deposits in the United States.

THE disturbed weather conditions referred to in our last issue resulted in a few thunder showers only, more particularly in the southern districts, accompanied by slight rain at some stations. With these exceptions and some local fogs, brilliant weather has been experienced throughout the whole of the United Kingdom. The temperature in the southern and midland districts has been much above the average; a considerable increase set in on the 17th inst., the maximum in London reaching 70°, and since that time some remarkably high readings have been recorded. On the 20th the thermometer over the inland counties ranged from 80° to 84°, while at Yarmouth it read 30° lower, and for several days the difference between these neighbouring districts has been very considerable. In the night of the 22nd a sharp thunderstorm occurred over South Devonshire, accompanied by a local rainfall amounting to nearly three-quarters of an inch, and another storm, with slight rain, occurred at Holyhead in the night of Monday, 24th; but in the early part of the present week the conditions were anti-cyclonic over a great part of the country, and the weather was very dry. The *Weekly Weather Report* of the 22nd instant states that rainfall was upon the whole less than the mean in all the wheat-producing districts and in the south-west of England, while in Ireland and the west of Scotland there was a slight excess. Bright sunshine was less prevalent than for some weeks past; the percentage of possible duration ranged from 24 in the east of Scotland to 58 in the south of England.

THE meteorological authorities in the United States are doing their utmost to utilise weather forecasts by adopting various means for their wide and rapid dissemination. The *American Meteorological Journal* for April contains accounts of two methods recently inaugurated in New England. From September 12 to October 1, 1892, an electric search light placed on Mount Washington was used for flashing forecast signals over the surrounding country. Reports received from persons in the vicinity show that the plan was quite successful, and the flashes were reported to have been seen at a distance of eighty miles. It is intended to resume this novel method next summer. The local forecast official at Boston sends out three hundred printed copies of forecasts daily by rail. The bulletins are distributed from the trains, and posted immediately on receipt in the various railway stations in neat frames provided for the purpose by the Weather Bureau. In this way the forecasts are brought before the public in as short a time as possible.

PROF. J. MARK BALDWIN, of the University of Toronto, has accepted the Stuart Professorship of Psychology in the Princeton University, and will begin work there in September. *Science* says that a suite of rooms has been set apart in North College for experimental psychology, and that a liberal appropriation has been made for its equipment.

MR. W. DE MORGAN, in accordance with the request of the Egyptian Ministry of Public Instruction, has been making experiments at Cairo with Egyptian clays with a view to determine whether it would be possible to use them for the production of glazed earthenware. A correspondent of the *Times* at Cairo says that after about eight weeks' work Mr. de Morgan considers that, whilst the production of porcelain and white earthenware is quite out of the question, there exist abundant materials for other descriptions of pottery, especially white majolica, similar to delft or della Robbia ware. But the cost of fuel is a stumbling-block. Mr. de Morgan, says the correspondent, considers that nothing can exceed the skill of the native throwers, who, with the most simple contrivances, produce far better results than the European workmen with elaborate apparatus.

IN his report for 1892 Dr. Trimen, the director of the Royal Botanic Gardens, Ceylon refers to the fact that of every 100 lbs. of tea consumed in England during the year 84 lbs. were of British growth, viz. 53 in India and 31 in Ceylon, only 16 lbs. being the produce of China. There was an increase of nearly 2,000,000 lbs. in the direct export of Ceylon tea to Australia, viz. 5,166,154 lbs. against 3,210,598 lbs. in 1891; and Dr. Trimen thinks that the costly advertisement at the forthcoming Exhibition in Chicago may reasonably be expected to lead to a large sale in the future in America. Ceylon, he says, urgently needs this; for while there is no reason to fear any drawback to continued success as far as cultivation and manufacture are concerned, there is a real danger of over-production; and its consideration as a possibility, by no means remote, induces him earnestly to recommend those concerned to devote some portions of their land to other cultivations. In the low-country especially much caution should be exercised in opening further land in tea estates. One result of the enormous development of the tea industry in the island is unfortunate. The industry so overshadows all other cultivations that there is now little room for trial or experiment with smaller products on estates, and not much stimulus to investigate them in the Botanic Gardens.

A COMMITTEE called the School Gradation Committee is at present being formed, the object of which, according to the

Times, is to promote the systematic and consecutive gradation of schools and universities, and to supplement the valuable work of recent years in respect of technical instruction by an effort to bring all effective schools and colleges, whether specialised or not, within a comprehensive national scheme. It is thought that this "may be most economically done, with the *minimum* of interference, centralisation, and narrowing uniformity, by the recognition and encouragement of existing effective schools, and by using available resources under local control mainly to facilitate the ascent of pupils from lower to higher grades." Among the members of the committee are Sir H. Roscoe, Sir P. Magnus, Prof. R. C. Jebb, Prof. Max Müller, and Prof. H. Sidgwick.

To determine the light refraction of liquid oxygen, Herren Olszewski, and Witkowski (Cracow Academy) have lately made use of total reflection. The liquid was held in a metallic case having windows, and a number of protective envelopes. Into it dipped a double plate formed of two plane-glass plates, with an air layer of 0·006 mm. between, which could be turned from without through a given angle. Monochromatic light was introduced, and the refraction of the liquid determined by means of the bright interference-fringes observed with the netting of the telescope at the borders of the field of total reflection. The relative index of refraction was found to be 1·2232, and the absolute coefficient 1·2235 (Dewar and Liveing, with the prism method, obtained 1·2236). The same authors sought also to determine light-absorption, using for the liquid a protected tube closed below with a glass plate, while another tube with terminal glass plates, dipped in the liquid above, and could be screwed up or down. A ray of light was sent through from below, and passing through various thicknesses of liquid (according to the position of the inner tube) was reflected in a spectral photometer, and compared with a direct ray. For the spectral region of most intense absorption of the green yellow (between $\lambda = 577$ and $\lambda = 570$), values between 84 and 89 per cent. were obtained for the light passing through 1 mm. thickness of oxygen; for the red absorption band 88.

The question of the purity of ice consumed for alimentary purposes in Paris has been lately before the Conseil de Salubrité de la Seine (*Rev. Sci.*). This ice is of two sorts, manufactured and collected. The production of artificial ice is about 27,000 tons a year, and of the "crop" of natural ice, the lac Daumesnil at Vincennes yields about one-half (12,000 to 15,000 tons a year). The price of the manufactured ice is eighteen to twenty francs a ton; that of the collected ice three to four francs. The demand in Paris is not wholly met from those two sources; and there is some ice imported from Sweden and Norway, which is, naturally, dearer than the ice from lakes, &c., in France. Now the lake Daumesnil just referred to is polluted on the one hand by the entrance of a sewer, and on the other by an artificial stream from the plateau of Gravelle; this stream traverses the Bois de Vincennes, and in the fine season receives all sorts of impurities from its banks. It is a question, therefore, of interdicting the collection of ice from this lake. The sewer it appears, might be suppressed, but the Administration cannot touch the stream. It is proposed to limit the use of ice from sources like this lake to applications in which the ice is not brought into direct contact with the liquids or solids to be cooled, and that when such contact takes place (as in cooling drinks) artificial ice alone should be used, got exclusively from spring water, or river water sterilised by heat.

The Agricultural Department of New South Wales has been making a series of interesting and useful inquiries as to the plants most visited by bees in the various districts of the colony. Some of the results are set forth in the February number of the Department's *Gazette*. It has been clearly proved that the flora of Australia includes honey-producing trees, shrubs, and plants of a high standard of excellence; the honey produced by bees in the near neighbourhood of the forest being of the finest quality, and having few (if any) faults. While a gum-tree is in bloom the bee will pass over the most tempting plant in a garden and wing its way to the borders of the bush; but, on the other hand, a field of maize in tassel is a source of the greatest pleasure to the busy little workers, who swarm in countless numbers, collecting the pollen so necessary for their wants. The plants which next seem to have the greatest attraction are the fruit-trees, familiarly called summer fruits. Clover (both white and red) yields a large quantity of first-rate honey, and bees kept at places where clover grows never fail to visit the modest flowers of the plant; dandelion, also, is a valuable honey-yielding flower, and is noted in all districts from Albury to Tenterfield. As to the size and colour of the flowers most affected by the bees, much diversity of opinion exists among apiarists, and in the face of the very conflicting replies, the *Gazette* thinks it would be vain to try to determine what coloured flowers are most attractive.

It is not, perhaps, generally known that the largest wine-growing district in Germany is Alsace-Lorraine. According to a report forwarded to his Government by the French consular agent at Frankfort, while the Wiesbaden regency has only 7,300 acres planted with vines which in 1890 yielded 1,644,040 gallons, the Coblenz regency 18,950 acres, giving 3,755,220 gallons, that of Trèves 8,980 acres, giving 1,832,400 gallons, Alsace-Lorraine alone contains 75,640 acres, the production of which in 1890 was 16,999,000 gallons (6,429,740 gallons in 1891), a production which is chiefly consumed in the country itself. According to the same authority (whose report is summarised in the current number of the Board of Trade Journal) the average annual production of wine in the whole world during the five years from 1886 to 1890 is estimated at 2,811,600,000 gallons. In this production Italy figures for 690,008,000 gallons, Spain for 657,250,000 gallons, and France for 606,562,000 gallons; that is to say these three countries supply two-thirds of the total quantity produced. Germany, with an average annual production of 51,705,610 gallons, only occupies the tenth place among wine-growing countries. The value of some of her wines partly compensates her, however, for the relatively small quantity of her annual crop.

The Imperia Forest School at Dehra Dun seems to be exercising a remarkably wholesome influence on the native students who attend its classes. Addressing the students at the recent annual distribution of certificates and prizes, Sir E. C. Buck, secretary to the Government of India in the Revenue and Agricultural Department, said that the school had been a signal success in the widest sense. The student who passed through a technical school was usually fitted only for the technical profession which he was taught at the technical school. But the Dehra School teaching was of such a broad and useful character that he believed its students, that is, the students who passed out of it successfully, would be more fit for any kind of work requiring originality and practical treatment than the students of any school or college in India. It was the only important educational institution in India in which the student was taught more in the field and in the museum than in the lecture room; in fact in which he was taught how to observe, and how to draw conclusions from observation. The consequence had been that the only signal instances which had, to his knowledge, occurred of original research leading to position and useful results being accomplished by natives of India, had been those in which such results had been produced by ex-students of the Dehra School. Only recently the Government of India had been obliged to close apprenticeships attached to the Geological Department, because natives of India could not be found qualified for original research. It was not that natives of India had not in them the necessary qualifications; it was that the power lay undeveloped in them, and had not been brought out by a training in habits of observation.

MESSRS. SWAN SONNENSCHEIN and Co. have in the press and will shortly publish a work by Dr. Edward Berdoe, entitled "The Healing Art," a popular history of the origin and growth of medicine in all ages and countries.

AT a recent meeting of the Société Française de Physique M. Janet gave an account of his experiments on electric oscillations of medium frequency (about 10,000 per sec.). The arrangement he employs is as follows:—A battery of accumulators furnishes a current which passes through a high resistance CD and a low resistance AB placed in series. The ends of AB are joined to the plates of a condenser and also to the extremities of a circuit AHB. The latter consists of a coil of self-induction L and resistance $\frac{R}{2}$ joined in series with an equal non-inductive resistance. The quantities RL and the capacity of the condenser are so chosen that when the circuit is broken at AB oscillations are set up in the condenser circuit. By means of an interruptor, closely resembling that used by Mouton in his work on the discharge of a condenser, the differences of potential e_1 and e_2 at the extremities of the inductive and non-inductive resistances are determined at different times after the breaking of the primary circuit, and the values plotted on a curve as functions of the interval after the break. If i is the current in the circuit AHB at any instant then the first curve gives the value of $\frac{R}{2}i + L\frac{di}{dt}$, and the second that of $\frac{R}{2}i$; so that $L\frac{di}{dt}$ is equal to the difference of the ordinates. The value of $\frac{R}{2}\frac{di}{dt}$ can be obtained from the second curve, so that if the self-induction is constant during an oscillation the ratio of these two quantities will be constant. This is found to be so, the value of L deduced from the curves being constant and also independent of the capacity of the condenser and of the electromotive force employed in the primary circuit. The author has also obtained from his observations the value at each instant of the difference of potential V between the plates of the condenser (a mica one) and of the charge Q, and he finds that the quotient $\frac{Q}{V}$, representing the capacity of the condenser, is greater for decreasing than for increasing values of V. The shape of the curves obtained recall those Ewing has found in the case of the magnetisation of iron, and indicate a kind of hysteresis or viscosity in the dielectric.

THE problem of obtaining a well-defined and trustworthy standard for the intensity of a source of light can hardly be said to be completely solved. Violle proposed as a unit the amount of light radiated by one square centimetre of molten platinum at the moment of solidification. But in order to keep the platinum absolutely pure, and its surface clean and smooth, it would be necessary to melt large quantities of the metal in an electric furnace. Siemens proposed platinum foil at the instant of melting, but a series of 500 meltings gave deviations of 10 per cent. in spite of the greatest precautions, mainly on account of the tearing of the foil on melting. According to a report recently presented to the Reichstag, the physicists of the Imperial Physico-Technical Institute at Berlin have been endeavouring to make Siemens' unit available for practical purposes by fixing the temperature of the platinum in some manner independent of its melting point. It was found that at any given temperature the ratio of the total radiation to that transmitted by a layer of water of a certain thickness was constant within 2 per cent. for plates of platinum of different thicknesses and from different sources. To measure the amounts of radiation a very delicate bolometer was constructed. A piece of platinum foil was welded on to a piece of silver foil of ten times its thickness, after which the combination was rolled between copper rollers down to a thickness of $\frac{1}{100}$ mm. It was then cut in a dividing machine so as to form a long continuous strip of 1 mm. breadth within a small area. Four such strips were mounted in a frame, and freed from silver by etching with acid, thus leaving strips $\frac{1}{1000}$ mm. thick. When tested, the bolometer was found to be extremely satisfactory. The Institute is at present engaged on determining the absorptive action of water and of the quartz vessel containing it. Further important questions are those regarding the effect of impurities in the platinum and the kind and duration of incandescence, questions which must be answered before the method can be regarded as thoroughly practical.

AMIDOPHOSPHORIC ACID, $PO.NH_2.(OH)_2$, the primary amine of orthophosphoric acid, has been isolated by Mr. H. N. Stokes, and its properties and those of several well-crystallising salts are described by him in the March number of the *American Chemical Journal.* It has hitherto been found impossible to obtain this substance owing to the extreme difficulty of regulating the decomposition by water or acids of the products obtained by the action of ammonia on pentachloride or oxychloride of phosphorus. It has now been obtained, however, by employing, instead of the two latter compounds, the ethers of phosphoric acid. The most advantageous method is to dissolve the chloride of diphenylphosphoric acid, $PO.Cl(OC_6H_5)_2$, in alcohol and to react upon it with alcoholic ammonia, when a beautifully crystalline substance, diphenylamidophosphate, $PO.NH_2.(OC_6H_5)_2$, is at once formed. This diphenyl ether of amidophosphoric acid yields an alkaline salt of amidophosphoric acid upon saponification with caustic potash or soda; upon converting this into the lead salt, decomposing the latter with sulphuretted hydrogen, and precipitating with alcohol, the free acid is obtained in the form of fine microscopic crystals. In the actual preparation it is not necessary to first isolate the chloride of diphenylphosphoric acid. It is only necessary to boil one molecular equivalent of phosphorus oxychloride with two molecular equivalents of phenol in a flask attached to a reflux condenser until no further evolution of hydrochloric acid occurs: the product contains, along with other derivatives, the chloro-diphenyl ether required. The liquid only requires to be diluted with alcohol, when the alcoholic ammonia may be directly added and the crystals of the amido diphenyl ether precipitated. Diphenylamidophosphate is a comparatively stable substance melting at 148° and resolidifying to a mass of crystals. The crystals are readily converted into acid potassium or sodium amido phosphate by means of a concentrated solution of caustic potash or soda; the reaction is very energetic and is complete in ten minutes. Upon acidification with ice-cold acetic acid and addition of alcohol, the acid salt is precipitated. Acid potassium amidophosphate, $PO\begin{cases}NH_2\\OK\\OH\end{cases}$, crystallises in six-rayed stars or rhombohedra; the neutral salt is extremely soluble in water and is very difficult to obtain crystallised. The acid sodium salt usually forms small hexagonal crystals, and the neutral sodium salt also crystallises well and, unlike the potassium salt, is not deliquescent. The lead salt is obtained in the form of a precipitate, consisting of groups of radiating plates, upon adding a solution of lead acetate to a solution of the acid potassium salt. In order to obtain the free acid from it, the crystals are suspended in iced water and a current of sulphuretted hydrogen allowed to bubble through. The filtrate from the sulphide of lead may then be allowed to fall directly into alcohol when the crystals of amidophosphoric acid are at once deposited.

AMIDOPHOSPHORIC ACID crystallises in tabular or short prismatic crystals which are insoluble in alcohol, but readily soluble in water, to which they impart a sweetish taste. The solution is readily distinguished from phosphoric acid inasmuch as it yields no precipitate with silver nitrate. It evolves no ammonia upon treatment with caustic alkalies, but merely forms the salt of the alkali metal. The solution slowly decomposes into ammonium phosphate. The solutions of the acid and neutral salts of the alkaline metals yield many corresponding acid and neutral amidophosphates of other metals by double decomposition with soluble salts of those metals.

NOTES from the Marine Biological Station, Plymouth:—Last week's captures include *Phoronis hippocrepia*, the Actinian *Corynactis viridis*, and the Foraminiferan *Haliphysema*. In the floating fauna the Cœlenterate element remains unchanged; the larvæ of the Nemertine *Cephalothrix* have made their first appearance for the year; the number of Polychæte larvæ and of Cirrhipede *Nauplii* has become considerably smaller; the later stages of various Decapod Crustacea (*Megalopa*, *Mysis*-stages) have appreciably increased in numbers; and minute young *Oikopleuræ* now occur in considerable quantity. The "gelatinous alga" and *Rhizoselenia* are extremely abundant. The Hydroid *Tubularia bellis*, the Gastropod *Nassa reticulata*, and the Decapod Crustacea *Pagurus lævis*, *Galathea squamifera*, *Porcellana platycheles* and *Pilumnus hirtellus* are now breeding.

THE additions to the Zoological Society's Gardens during the past week include an Orang-Outang (*Simia satyrus*, ♂) from Borneo, presented by Mr. Thomas Workman; a Spotted Ichneumon (*Herpestes nepalensis*) from India, presented by Lady Blake; a Raven (*Corvus corax*) British, presented by Lady Rose; a Peregrine Falcon (*Falco peregrinus*) British, presented by the Old Hawking Club; a Greek Tortoise (*Testudo græca*) European, presented by Mrs. Alcock; a Martineta Tinamou (*Calodromas elegans*) from Argentina, three Spotted-sided Finches (*Amadina lathami*) from Australia, purchased; a Panama Amazon (*Chrysotis panamensis*) from Panama, received in exchange; six Indian Wild Swine (*Sus cristatus*), four Barbary Mice (*Mus barbarus*) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

LARGE TELESCOPES.—Much has been written during the last few months with reference to the usefulness or non-usefulness of large telescopes. That the verdict is given in favour of the former is not at all surprising, for are we not far away from the limit, if there be one, beyond which larger instruments will be available? Dr. Common has many times pointed out the practicability of constructing large reflectors (his five-foot being a good example of the type of instrument he could enlarge), while the Lick instrument, the work of the Clarks, is really only a beginning of what will be done in large telescope building. For refractors it has many times been urged that the increase in size of lenses involves such a thickness that much light is thereby lost by absorption. M. Alvan G. Clark, with reference to this particular point, says a few words in *Astronomy and Astrophysics* for April, in which he points out that such is not the case. Greater aperture means greater light-grasping power, and as it is quite unnecessary to considerably increase the thickness of the lens, the former predominates over the latter. With the forty-inch discs, he says, only a combined thickness of four inches is required, and with lenses of an object-glass of even six feet aperture a combined thickness of only six inches would be necessary. It is pleasing to hear through him that a steady improvement is being made in manufacture of glass, and that the present discs are *infinitely* superior to the early ones, and "who knows," as Mr. Clark says, "how soon still more transparent glass may be at hand."

SPECTRUM OF β LYRÆ.—The extreme interest that lies in this variable, especially for spectroscopists, makes it a subject of keen research, and the important observations made by Prof. Keeler with the great Lick refractor, and contributed to the number of *Astronomy and Astrophysics* for April, will be the more eagerly read. The observations were undertaken with the intention of connecting possible changes in the spectrum of the star with its period of light variability. After plotting a number of observations on the light curve of the star, the recorded appearances of the spectrum "were in some degree contradictory," although certain results were obtained, but they were left incomplete, owing to Prof. Keeler's withdrawal from the observatory. The results may be briefly stated as follows:—

(1) Bright hydrogen lines C and F, bright D_3 line and dark D lines are always visible with the Lick refractor. Certain fainter bright lines are absent only at principal minimum.

(2) Light variations due to changes in brightness of continuous spectrum.

(3) Bright lines brightest when continuous spectrum brightest.

(4) Bright lines broad and diffuse, particularly when star at maximum. D lines very hazy, so that components are hardly distinguishable.

(5) No really remarkable changes in the appearance of the spectrum took place during greater part of period. Observations show no relation between spectral changes and secondary minimum of the star.

(6) Most remarkable changes at principal minimum. "The bright lines became dimmer and perhaps sharper. The fainter bright lines disappear. The D lines become darker. Strong absorption lines appear on the more refrangible side of certain bright lines in the green, the separation of the dark and bright lines being at least five-tenth-metres. Other bright lines are perhaps similarly affected. A narrow dark line appears above the D_3 line at the same time. Shortly before the first maximum is reached the dark lines disappear."

Prof. Keeler adds that the method of observation he adopted was only capable of allowing him to observe a "part of a much more complex series of changes" which no doubt takes place.

SOCIÉTÉ ASTRONOMIQUE DE FRANCE.—In the *Bulletin* of this society for 1892, the sixth year since its foundation, several articles of importance will be found to be scattered throughout its numerous pages. Of these we may refer our readers to some selenographic studies by MM. Gaudiberi, Flammarion, and Antoniadi, examination of recent studies of Jupiter by M. Flammarion, and M. Edouard Foulséré's graphical method of determining the co-ordinates of solar spots. The valuable observations made by M. E.-L. Trouvelot on the planets Venus and Mercury, a full account of which has been given in these columns (NATURE, vol. xlvi. p. 468), will also be found here, together with a discourse by M. F. Tisserand on the movement of the moon, with reference to ancient eclipses. Among other communications M. Schmoll gives a summary, with tables, of the solar spots observed during the year 1891, M. Guillaume describes his observations on the surface and rings of Saturn, and MM. Quenisset and Trouvelot contribute their observations on planets and remarkable solar protuberances respectively. A simple method of determining the positions of solar spots, and of measuring their displacements, is treated by Dr. Huette, while M. Bruguière gives a most interesting account of M. Lippmann's work on photography in colours, and M. Pluvinel on the coming (now past) eclipse of the sun.

WOLSINGHAM CIRCULAR, NO. 34.—With reference to the contents of the Wolsingham Circular, which we recently gave, Dr. Kreutz, in *Astronomische Nachrichten*, says:—"The first star, Esp.-Birm 180, is certainly given by Chandler as (2258). Aurigæ in his list of probable variable stars A. J. 216; see also *Astronomische Nachrichten*, 2764, p. 63, No. 2. The second star is evidently identical with B.D. + 57°·727 = A. G. Hels. 3032. Position for 1900: 3h. 23m. 23s., + 58° 9′·0. The original magnitudes of the Hels. zones are: 9·1m (February 15, 1872), and 9·0m. (February 15, 1873).

ASTRONOMICAL JOURNAL PRIZES.—The judges appointed by the editor of the *Astronomical Journal* say a few words in the current number (No. 293) with reference to some general considerations connected with the presenting or withholding of the awards. For comet observations, allowance for optical qualities of telescopes will to a certain extent be made; relative freedom from systematic peculiarities of observation will be regarded "as a mark of excellence. Even more important than the nominal or apparent precision in other respects." For individual precision of observations, freedom from large or abnormal errors would be of the first importance. In orbit computation the judges will regard with special attention care exercised in revision of published observation, ingenuity displayed in searching out and evaluating systematic errors, completeness and soundness of discussion, ability shown in indicating probable limits of uncertainty in adopted elements, &c. With regard to variable stars, enough has already been said, but the judges remark that definite reductions cannot of course be expected, as from the nature of the case many years must elapse.

GEOGRAPHICAL NOTES.

THE Hon. G. N. Curzon, M.P., read a paper on his recent journey in Indo-China at the meeting of the Royal Geographical Society on Monday. The whole region, he pointed out, is dominated by its great rivers, and may be divided into the mountain district of the north cleft by vast gorges, and the low plains of the south mainly composed of alluvial deposits, where the coast lands are steadily encroaching on the sea. In the seventh century Tongking, now 60 miles inland, was on the coast. A very remarkable feature which gives to parts of the coast a beauty comparable with that of the Inland Sea of Japan is a broken belt of limestone cut into curious flat-topped sections of all sizes, and perforated by the sea or rivers with many fantastic caverns and tunnels. The masses of caverned rock rise to a height of from 50 to 500 feet, and are best seen in the Bay of Along in Tongking. In Annam Mr. Curzon travelled to Hué by the "Mandarin's Road," a track which is carried over several cols by some skilful engineering in the form of rock staircases. Throughout Annam the traveller is much confused by the number of names applied indiscriminately to each village, and the maps hitherto constructed by the French officials are far from satisfactory. The people of Annam have the submissiveness without the nerveless apathy of the Hindu, and as craftsmen they are industrious and artistic. Coal is abundant, some seams being more than 180 feet thick at Haton, on the Bay of Along. Hué is a city of great interest, being beautifully situated and near a number of magnificent ancient tombs. Cambodia or Cambogia, as Mr. Curzon prefers to spell the name, is of interest, mainly on account of its ruins, the number and character of which make a long stay desirable, if the traveller would do justice to his opportunities.

THE newly published report of the Bengal census reveals the interesting fact that there is a steady transference of population from the most densely to the more thinly peopled parts of the province, the former prejudice against leaving the native village having apparently vanished. Mohammedanism is increasing rapidly in Bengal, and the custom of widow marriage amongst Hindus has become common. These facts are significant of progress.

THE supremacy of the great ports of Europe as entrepôts for the trade of the world is rapidly becoming a thing of the past. Two recent instances of independent action on the part of the colonies are of more than local importance. One is the establishment of a line of steamers trading direct from New York to Cape Town, another the commencement of a regular service of fast steamers from Vancouver to Sydney, N.S.W.

A COMMUNICATION was lately made to the Paris Geographical Society on the strength of a statement in a Russian newspaper, describing a curious mountain group in Podolia. This is said to rise abruptly from the plain with a grandly rugged crest composed of a broken circular rim surrounding a crater-like depression. The whole mass is composed of limestone, in which fossil corals abound, and the inference drawn is that this is, in fact, a full-sized fossil tertiary atoll. The name of the mountain is given as Miodoborski, but it is called Toltra by the natives.

AT a general meeting of the Royal Geographical Society called by the requisition of a few Fellows who objected to the action of the Council in the manner of admitting women to the Fellowship of the Society, it was proposed to frame a bye-law restricting the privileges of lady Fellows, and rendering them incapable of serving on the Council or in any office in the Society. The question whether ladies should be admitted at all was voted upon after a somewhat heated discussion, and it was decided by 147 to 105 that women should not be admitted as Fellows of the Society. This decision was entirely unforeseen; it is a retrograde step which, we feel sure, will be disapproved and regretted by the majority of the Society.

THE Royal Medals of the Royal Geographical Society have been awarded to Mr. F. C. Selous for his travels in Africa, and to Mr. W. W. Rockhill for his journeys in Tibet. The Gill Memorial was awarded to Mr. H. C. Forbes, and the Cuthbert Peek Grant to Mr. Charles Hose for his travels in Sarawak. Major Powell, Washington, Prof. Ratzel, Leipzig, and M. Vivien de St. Martin, Paris, were elected honorary corresponding members of the society.

INSTITUTION OF MECHANICAL ENGINEERS.

ON the evenings of Thursday and Friday last week, April 20 and 21, an ordinary general meeting of the Institution of Mechanical Engineers was held in the theatre of the Institution of Civil Engineers, by permission of the Council of the latter body. There were three papers on the Agenda, but only two were read, namely, Mr. Dean's paper on copper plates for locomotives, and the second report of the Alloys Research Committee, the author of which was Prof. W. C. Roberts-Austen, C.B., F.R.S. Our readers will remember that the first report of the Alloys Research Committee was read, and discussed at the October meeting of 1891, and an abstract of it appeared on page 22 of our 45th volume. A large part of the first report was taken up by the consideration of the effect of various alloys on gold, and it will be remembered that the author was somewhat sharply criticised for the course he had taken in framing his report, gold being a metal not used by engineers, at least for constructive purposes. This second report carries the matter further, and it is possible now to appreciate Prof. Roberts-Austen's reasons for taking the course he did. In opening the subject he referred again to the "periodic law" of Newland and Mendeleeff, and upon it he based a large part of his reasoning in the first report. The researches of Raoult Van't Hoff, and Arrhenius, led to the view that the molecules of small quantities of elements, distributed through a mass of a solvent, retain their individuality. The work of Heycock and Neville (and also the experiments described in the author's previous report) point to the conclusion that the added elements may retain their freedom when they are present in much larger quantities than 0·2 per cent., which is the amount of added matter the Committee usually dealt with in their researches. The point raised was whether the added element does, or does not, remain free in the mass of the solvent, and as the author pointed out, it is a vital one in limiting the scope of the inquiry.

If the added element enters into combination with the solvent its individuality will be changed, and it might be that the mechanical properties of the metallic mass would mainly depend on the degree of fusibility of the compound formed. If the concentration of the solution is such that a fraction of the dissolved body alone remains isolated, the influence of the volume of the added elements, will evidently be disturbed, as this influence is supposed to be exerted only by a single constituent of the mixture, whilst the mechanical properties of a solidified mixture are functions of both constituents, in the favourable circumstances where the solvent is not started by the added element, and where the law of atomic volumes is applicable. A metal is seldom homogeneous and is more often formed of rounded polyhedral grains, and the cohesion in the interior of a grain differs from the adherence between the neighbouring grains. The law of atomic volumes cannot apply, the report pointed out, to the adherence of the grains, that being regulated by other causes, such as the rate of cooling and pressure, and whether a compound be formed, which solders the grains together. Arguing from these facts, the author pointed out that an attempt to prove the nature of the influence of atomic volumes by mechanical tests only led to anomalies, and more or less grave irregularities being encountered. The investigation was not, however, limited to mechanical tests, independently of which it had been shown that the influence of impurity on the molecular transformation in iron, studied by Osmond, may be shown in several ways. Transformation may be assisted by the presence of impurity, the temperature at which they occur may be altered, or the molecular changes may even be entirely prevented by the presence of elements which behave in strict accordance with the law of atomic volumes. The author referred to the remarkable series of experiments recently made by E. Warburg and F. Tegetmeier, which would seem to demonstrate the possibility of producing eventually a degree of porosity in vitreous bodies, which will admit the passage of elements having comparatively small atomic volumes, while other elements, having larger atomic volumes, are strained off, thus occasioning a mechanical sifting of the elements. In making these experiments, a cup-like receptacle was used, which had a vertical partition of sheet glass placed in it, so that the cup was divided into two parts by the glass. Sodium amalgam was placed on one side and pure mercury on the other; the whole was then heated to a temperature of 200° C., at which the glass became slightly conducting. By the aid of a battery, the sodium atoms of the sodium silicate were set in motion, and after 30 hours it was found that a considerable quantity of sodium, amounting to 0·05 gramme, had passed into the mercury which was originally pure. A corresponding amount of sodium had been lost by the amalgam, but the glass had exactly preserved its original weight and clearness. The glass was partly composed of neutral molecules of sodium silicate, together with free molecules both of sodium (base) and of the acid, and of the free sodium capable of being transported under the influence of the electric current. When Tegetmeier replaced the sodium amalgam by lithium amalgam and repeated the experiment, the sodium of the glass passed as before into the originally pure mercury, and the glass became opaque on the side touching the lithium amalgam; but after a time the opacity extended right through the thickness of the glass, and the metallic lithium began to accumulate in the previously pure mercury. It is not possible thus to chase out all the sodium present in the glass; but the free sodium atoms are replaced by those of lithium. Analysis showed that the glass originally contained 2·4 per cent. of potassium and 13·1 per cent. of sodium; but after the experiment, while retaining the same percentage of potassium, it had only 4·3 per cent. of lithium, and only 5·3 per cent. of sodium. The glass in which lithium had thus replaced part of the sodium was very tender, opaque, and friable. The conclusion to be drawn is that the atoms of lithium, having an atomic weight of 7, and an atomic volume of 15·98, can pass along the tracks, or molecular galleries left in the glass by the sodium atoms, the atomic weight and volume of which are 23 and 16·04 respectively. When a metal of superior atomic weight and volume to sodium was substituted for the lithium—such as potassium, with atomic weight 39 and atomic volume 24—it was found not possible to chase out the sodium, the new atoms being too big to pass along through the spaces where the sodium had been. We are thus confronted with a molecular porosity which can in a sense be gauged; and the mechanical influence of the volume of the atom is made evident. Proceeding to the details of the experiments made by the committee, the influence of impurities on copper was next referred to. The question was raised whether normal copper can be made to assume an allotropic state, analogous to that in which there is reason to believe iron can exist, and if so are the properties of normal and of allotropic copper as widely different from each other as those of the distinct varieties of certain well-known non-metallic elements. The point is one of considerable interest, and Prof. Roberts-Austen seems to have little doubt that copper can be prepared by electrolytic deposition, in an allotropic state, in which the density of the metal is from 80 to 82 as compared with 8·92, which is that of normal copper. The effect of mechanical and thermal treatment upon copper was then referred to, and some interesting figures were given, showing how different may be the properties of a metal chemically pure; for instance, rods of very pure electrolytic copper, all the same sample, but variously treated, broke under stresses varying between 8·219 tons and 18,750 tons to the square inch; the former being the tensile strength of cast rods, and the latter of cast rods worked and not annealed; whilst cast rods carefully worked, and annealed gave a tensile strength of 18·259 per square inch. The experiments show a difficulty in determining a standard tenacity for copper.

The effects of arsenic, bismuth, and nickel upon copper afforded one of the most interesting parts of the investigation, and from the engineer's point of view an extremely important section of the series of experiments. It has been too often accepted as a matter of fact that pure copper is the best that can be used for engineering purposes, and specifications are generally framed to this effect. The Research Committee, however, show that the metal may be, and frequently is, as a matter of practical fact, too pure for the purpose; thus, it has been found that a very fair percentage of arsenic improves the copper used in fire boxes of locomotives. It is well known that of old these parts of the boiler lasted for a much longer period of time than they do in the present day. In fact, as Mr. Tomlinson, an old railway engineer, said in the discussion, they used to expect to get half a million miles of running out of a copper fire box, whereas about half that distance is all that is obtained in the present day. This he attributes to the effect of electrical matters upon engineering practice. The electricians insist on their copper being absolutely pure, and that has raised the standard, so that now the copper smelters get all the impurities out of the metal, whereas in old times a considerable percentage of alloy, especially arsenic, was present. Antimony appears to behave like arsenic, and when present in proper proportion greatly strengthens the copper. Bismuth, on the other hand, renders copper singularly weak. With 0·1 per cent. of bismuth a sample of copper was too brittle to work, and had at the ordinary temperature a tenacity of 18,000 tons to the square inch; but at a higher temperature the fall in tenacity was very rapid, and there was practically no elongation. The prejudicial effects of bismuth did not seem to disappear, even though but a trace were present. In one test of a singularly pure copper, containing only 0·002 per cent. of bismuth, although the metal was strong and worked well, the elongation was very small. The variation in the effect of arsenic and antimony on the one hand, and of bismuth on the other, is of considerable interest, for according to the classification of Mendeleeff, arsenic, antimony, and bismuth all belong to the same family, of which nitrogen is a type. The atomic volume of bismuth—20·9—is, however, higher than that of arsenic—13·2, or of antimony—17·9, and therefore, according to the principle laid down by Prof. Roberts-Austen, bismuth ought to diminish the tenacity of copper, of which the atomic volume is only 7·1. But in accordance with this reasoning the influence of arsenic and antimony should be exerted in the same direction, even though in a less degree. The author has turned his attention to this matter, and has already been conducting a series of experiments which have extended over nearly twelve months. The investigations are, we believe, not yet complete, but the results will be given subsequently. A diagram was, however, exhibited at the meeting, in which curves were shown, illustrating the behaviour of various alloys of copper and bismuth during cooling, and the wholly unexpected fact was revealed that the copper passed below the freezing point before it actually became solid. On each curve there was a second or lower point of solidification, which occurred at a constant temperature in all the alloys, and was very close to the melting point of bismuth itself. The existence of this second point was very evident, even when the copper contained only one per cent. of bismuth, and this fact goes far to explain the peculiar action of bismuth on copper. It would appear that whether very poor or very rich in bismuth, the alloy of copper may be a portion of bismuth, containing perhaps a little copper, always remains fluid until the temperature of the mass has fallen to 260° C., which is the point at which bismuth tself solidifies. The presence, Prof. Roberts-Austen stated, of a fluid constituent in an alloy long after the mass itself had become solid, is doubtless the determining cause which enables the metal to assume a highly crystalline, and consequently an intensely brittle structure. So far as he was aware the cause of the peculiar behaviour of bismuth could not have been revealed by any other method of investigation than the one adopted. In connection with this point, a fact brought forward during the discussion by Mr. Gowland, is of interest. In the course of his metallurgical work at the Japan mint, he had brought before him a large number of bars of silver for the purpose of coining, but they were so brittle that it was impossible to work them at all. On investigation he found that there was an appreciable quantity of bismuth in the silver. The structure was coarsely crystalline, and though the whole mass was so hard and brittle, the crystals themselves were very ductile. The conclusion he came to at the time was that the crystals of silver had become separated, as it were, by a film of bismuth. The fact bears out the correctness of Prof. Roberts-Austen's mode of reasoning. Judging from their polished surfaces, the alloys of copper rich in bismuth are to all appearances as coherent as the alloys of copper and tin, which have great strength. The report gives some interesting particulars of the effect of pressure. The passage of iron from one allotropic modification to another is accompanied not only by a change of heat capacity, but also by a change of volume. This matter was referred to in the previous report, but the author gave some further interesting particulars of experiments carried out by compressing a piece of steel in a hydraulic press, in order to obtain recalescence at a lower temperature than would be the case if the pressure were not applied. In one case a cylindrical piece of steel, 1" long and ¾" in diameter, was bored through two-thirds of its length by a hole $\frac{1}{10}$" in diameter, in which a thermal junction was placed. The mass was heated to 1000° C., and it was found that without the application of pressure recalescence occurred at 650° C., but when a load of 9 tons per square inch was applied, recalescence occurred at 620° C., and was comparatively feeble. The experiment, it need hardly be said, is one very difficult to make, and could only be done by those having command of special apparatus. Other experiments were carried out, the result showing that the recalescence point is lowered by pressure, but it was found that the lowering was not affected, unless the load was applied at a temperature well above that at which recalescence takes place. Experiments were made with Newton's alloy of bismuth, lead, and tin, the full results of which will be published at some future time. In considering the whole scope of the report, the author said that it might be asked what evidence had been gathered as to the mode of action of added elements, and whether it appeared that the atomic volume of the added element had a dominating influence on the mechanical properties of the mass in which it is hidden? The true action of an added element, the author pointed out, may readily be masked by its action as a deoxidiser. Notwithstanding these difficulties, it is undoubtedly proved that bismuth, potassium, and tellurium, all of which have atomic volumes, greatly lower the tenacity of copper. Arsenic, which has a larger atomic value (13·2) than copper (7·1) confers strength on copper, but it is very certain that the limit of elasticity, and the ductility of a metal are greatly influenced by the presence of an element with large atomic volumes. This fact may be of more molecular significance than the diminution of tenacity, to which, for the sake of simplicity, attention was mainly directed, when the early experiments on gold were made.

In the discussion which followed the reading of the paper a number of speakers took part. The most important contribution was that of Dr. Watson, of the Broughton Copper Company, who brought forward some practical experience to reinforce the deductions of the author. Mr. Arnold, of the Technical Schools, Cambridge, read a very long manuscript, which it would be rash on our part to attempt to abstract, and which we cannot afford the space to give in full. Mr. Hadfield, of Sheffield, questioned the accuracy of the beta form of iron theory promulgated by Osmond and adopted by the author. The point is one of considerable importance, but requires a wide field for its discussion.

On the whole it cannot be doubted that the report is a most valuable contribution to the scientific knowledge at the command of the engineer, and were the attention called to the action of bismuth on copper its sole result, the labours of the committee would not be without warrant.

The summer meeting of the institution will be held this year at Middlesborough on August 1 and three following days.

CONIFERS.[1]

THIS is a bulky volume of nearly 600 pages, and contains a vast amount of information. If the Royal Horticultural Society had published nothing but this since 1891 they would have amply satisfied those who are interested in conifers, and have keenly felt the want of such a book of reference as the one now under notice. Some of the papers published in the report could have been omitted without loss, but on the whole the editors have done their work well. In the preface they say, in sending out this memorial of the Conifer Conference, 1891, "we would draw attention to the fact that it contains far more than a mere verbal report of the conference, Dr. Maxwell T. Masters, F.R.S., and Prof. Carl Hansen, of Copenhagen, having promised at the time to recast their notes more fully. This they have done most kindly, and with infinite labour and research, but not without some little expenditure of time, the final sheets of MS. having only come into our hands in July, and the corrections extending up to September 29.

"The names adopted by Dr. Masters and Prof. Hansen may, of course, be relied upon as representing the latest decisions of botanical science in England and on the continent of Europe respectively, though future research may necessitate some still further slight alterations. However, the hitherto inextricably confused nomenclature of conifers may safely be described as settling down upon the lines adopted in this volume by these two eminent authorities, who, although not yet in absolute agreement, will be found to approach very nearly."

The list of conifers and texads, by Dr. Masters, is by far the most important contribution to the nomenclature and synonymy of conifers which has appeared since the publication of Parlatore's monograph in De Candolle's "Prodromus" in 1868; it is much more complete than Beissner's "Handbuch der Coniferen-Benennung," and the more recent "Handbuch der Nadelholzkunde," of the same author. There seems no reason to doubt that Dr. Masters's list will be used and followed by English systematists generally. Dr. Masters, in drawing up the list of genera, follows Bentham and Hooker's "Genera Plantarum" as the standard authority. A few deviations from it have, however, been made in accordance with more recently obtained knowledge. Pseudolarix is accorded generic rank (and not united with Larix, as in the "Genera Plantarum," whose authors had not seen male flowers); Keteleeria too, after a careful study of living material, has been separated from Abies and reinstated as a genus—Dr. Masters's studies having on these points proved the justice and accuracy of Carrière's views. The Chilian Prumnopitys is restored to generic rank, and separated from Podocarpus, with which it was united by Bentham and Hooker.

The Pinetum Danicum of Prof. Carl Hansen is unsatisfactory, and its omission from the report would have been desirable. It is a somewhat ambitious performance, but in bulk is very largely made up of extracts from books and periodicals. Many of the records are certainly useless; for instance, under *Pinus longifolia*, it is stated: "one plant, however, exposed out of doors does not appear to have suffered"; this Indian species is tropical in its requirements, and as it will not grow out of doors even in the south of England, it is in the highest degree improbable that it would, even under the most favourable conditions, exist in the open air in Denmark. A curious mistake occurs on p. 372, where the Viennese botanist, Prof. Günther Beck, Ritter von Mannagetta, figures as Prof. Günther, Knight of Beek von Managetta. On p. 330 Prof. Hansen remarks under Prumnopitys that its wood is much valued by "ebonists." He probably means cabinetmakers (ébénistes). *Tsuga hookeriana* and *T. pattoniana* are kept up as distinct species by Hansen; but Prof. C. S. Sargent, who is familiar with the two forms in their native habitats, has no hesitation in regarding them as specifically identical. Hansen accords generic rank to Biota, Thuyopsis, and Chamæcyparis, the first and second being merged into Thuya, and the third into Cupressus by Dr. Masters. It is rather annoying to find the obsolete geographical expression "New Holland" constantly used by Hansen. New Holland and South-east Victoria are given as the native countries of one species.

The coniferæ of Japan, by H. J. Veitch, is a valuable paper. From it we learn the somewhat startling fact that, in proportion to the area of the country, the flora of Japan contains more coniferous species than that of any other country in the world. Japan boasts of forty-one species and thirteen genera, whereas in the whole of Europe there are but eighteen species and seven genera.

A. D. Webster, "Conifers for Economic Planting." Mr. Webster is a practical forester of wide experience, and he considers that out of all the conifers cultivated in Britain only sixteen can be utilised in an economic sense, or for truly profitable planting. These are the larch, silver fir, Corsican pine, Douglas fir, *Pinus Strobus*, Scotch fir, *Thuya gigantea*, Spruce fir, Austrian pine, *Pinus Pinaster*, *Abies nordmanniana*, *Sequoia sempervirens*, *Cupressus macrocarpa* (or, as Mr. Webster calls it, *C. lambertiana*), *Cedrus atlantica*, *Pinus rigida*, and *Cupressus lawsoniana*. The order in which these names are given represent the relative value of the trees as timber producers. Under each heading Mr. Webster gives valuable data as to rates of growth under different conditions as regards soil, elevation, &c.

In a compact paper of thirteen pages Mr. W. Somerville gives a very good *résumé* of the present state of our knowledge of the quality of coniferous timber as affected by sylvicultural treatment. Mr. Somerville's remarks are sure to be perused with profit by landowners and foresters.

Mr. D. F. Mackenzie, on the timber of exotic conifers: uses and comparative value, contributes much valuable information. Taking the value of Scotch fir timber at 100, the author calculates that of *Cupressus macrocarpa* at 190 and that of *C. lambertiana* at 283; as these two names represent one and

[1] Report of the Conifer Conference, 1891 (issued November, 1892).

the same species, the widely different results are probably due to the trees furnishing the timber having been grown under different conditions. Mr. Mackenzie mentions a curious fact "observed in the working of the various pine timbers I have named. It was found that the wood of pines having three leaves in a sheath was, as a rule, much harder than those having only two, whilst all those having five leaves in a sheath were uniformly soft, and when dressed had a silky appearance. So general is this characteristic that one could almost at once tell to what class a certain plank of pine timber belonged." These observations we do not remember to have seen previously recorded.

"The Diseases of Conifers." Although in German there is a literature of considerable extent on this subject, the publications in English are few. Prof. Marshall Ward is a very careful and competent observer, and his contribution to the report is of great value both to the man of science and to the practical forester.

Mr. W. F. H. Blandford's insects injurious to conifers is an excellent *résumé* of all that is known up to date of the life-history of the various insect pests, which have been noted as injurious to conifers. How important this subject is may be judged by the destruction wrought by the larvæ of *Liparus monacha* between 1853 and 1868 in East Prussia, Poland, and Russia, where the spruce was killed over an area of 7000 square German miles. A similar instance is that afforded in 1890 in the Bavarian forests by the same destructive insect, the loss caused by this to the revenue being estimated at £40,000. Those, however, who, like the writer of these notes, travelled over the districts affected during the ravages of the larvæ, would realise much more vividly the gravity of the attack than others could from a mere perusal of statistics.

Not the least valuable portions of the report are the statistics of conifers in the British Islands, and the value in the British Islands of introduced conifers, by Mr. Malcolm Dunn. These statistics represent an enormous amount of energy and perseverance on the part of the compiler. The tabulated forms give particulars from a large number of places in the British Islands, and deal with the soil, altitude, age of trees, their height, girth, &c. The list of conifers and largest specimens, also by Mr. Dunn, gives the dimensions of the largest specimens taken from the above-mentioned tables and also the number of returns respecting each species. G. N.

THE EARTHQUAKES IN ZANTE.

LAST week we noted the fact that another disastrous earthquake had occurred in Zante on Monday, April 17, and that it had been followed by various slighter shocks. According to a special correspondent of the *Times* at the town of Zante, the centre of the disturbance seems to have been under the sea about two miles from land. Before the great shock the inhabitants of the district of Vasilikos, near this centre, heard submarine rumblings, which increased in loudness till the earthquake occurred. Two huge boulders were detached from the neighbouring mountain and rolled into the valley beneath. The same correspondent records that on the afternoon of April 21 there were several violent shocks.

The conditions under which this series of earthquakes has occurred will no doubt be carefully studied. Meanwhile we may call attention to a good article contributed to the *Mediterranean Naturalist* for April by Mr. W. G. Forster, seismologist, manager, and electrician, Eastern Telegraph, Zante, on the earthquakes which did so much damage in January. From this paper we reprint the following historic statement:—

"From the traditions of the place it has always been considered pretty certain that Zante must invariably expect a more or less severe earthquake about every thirty years. I find, however, that this cycle of seismic disturbances is common to all earthquake districts in south-eastern Europe and Asia Minor, and that there exists also a fairly proven and established law which governs these periods of visitation, for instance, whenever any long time has elapsed without the slight shocks—which average one or more a week in earthquake districts of non-volcanic regions—and when to these periods of comparative quiescence succeeds one of constant earth tremors, then a disastrous shock is nearly certain to take place. This is a very important point, and cannot be neglected when the question as to the origin of the shocks is under consideration.

"The last strong local earthquake previous to the present series of shocks occurred on October 26, 1873, and although it was far less severe, it originated within a mile or so of the present one's centrum, as proven by a knot of submarine cable having been then lost, buried under the immense mass which fell into it, at the bottom of the sea; and by the measurements taken at the time.

"This earthquake had precisely the same characteristics as the present one, both previously and subsequently to its occurrence, and although very many severe and slight shocks have been felt since 1873, in no case were they of so pronounced a local nature as those just recently experienced. When the great earthquake of August 27, 1886, occurred, which destroyed Filiatra on the mainland to the south-east of Zante, this island was fortunately outside the direct vibrative waves of seismic forces radiating from the centrum of that shock; which covered up six knots of submarine cable in latitude 37°25′, longitude 21°11′ east of Greenwich; but still it did considerable damage, and its force was severe enough to cause the greatest alarm even in so distant a place as Malta.

"From that year until the spring of 1890 there were numbers of small shocks, but after then and up to August, 1892, only a very few tremors were recorded. On August 16 last year about twelve small shocks suddenly occurred during the day, purely local, and all from east to west. After three days of absolute tranquillity they began again, and although merely pulsations they were of a very pronounced character.

"At midnight on August 27 the shock was strong, and from then until the still smarter shocks of September 3 and 5 the earth seemed always shaking. Another few days of quiescence were followed by a renewal of shocks. This state of things continued until the middle of January last—and was again succeeded by a fortnight of perfect tranquillity. At 9 p.m. on January 30 a very distinct rumbling occurred, which was followed by a short, sharp shock, as if from some falling mass, and then all was still again. I noticed after the shock a series of small ripples on the sea, which was previously and subsequently quite calm. The night passed very quietly until 5.34 a.m., local time, when the whole island began to sway terrifically from east to west, with a purely undulating motion, finishing up by a movement which I can only describe as being similar to that of some mighty force wrenching out the bowels of the earth. This shock lasted twelve seconds, and its centre was undoubtedly in the sea very close to the town, and due east of the same. From its apex of origin its range of destruction, on the frontage of the town, was not wider than two miles, spreading out to about fifteen when it reached the villages at the base of the range of hills, six miles off.

"The destructive force had a tendency to incline from due east to the north-west of the island, which is about 27 miles in length by an average breadth of eight, a subsequent shock taking a much lower range. During the whole day shocks were alarmingly frequent and numbered some hundreds between the first and nightfall when everybody went to the open ground in a most panicstricken condition. At 1.56 a.m. on February 1 another terrific shock took place—not so severe as the first, but with a range towards the south-west and of increasing destructive force. This shock lasted 20 seconds and was also succeeded by numberless others. After 23 hours a third severe shock occurred and periodically during the whole week others of decreasing intensity took place. Since the first shock until the present date, at least one thousand (including pulsations and tremors) have been felt.

"Of course the direct and indirect damage has been very great owing to the extensive zone of destruction, the scattered nature of the villages and to the bad construction of the houses in general and to their dilapidated condition owing to extreme poverty of the island. At least half a million sterling is required to rebuild the place, and as this amount can never be realised many of the ruins are likely to remain untouched and most of the population will have to emigrate."

SCIENTIFIC SERIALS.

American Journal of Mathematics, vol. xv. 1 (Baltimore: Johns Hopkins Press, January, 1893).—The pièce de résistance of this number is a memoir by Prof. Cayley on symmetric functions and seminvariants (pp. 1–74), in which the author further develops the theory of seminvariants, and in connection therewith is led to some investigations on symmetric functions. The subject is treated with characteristic ability and affords ample evidence of the writer's recovery from his recent serious illness. Prof. Cayley also contributes some tables of pure reciprocants to the weight S (pp. 75–77). Two short notes follow on the differential equation, $\Delta u + k^2u = 0$ by Maxime Bôcher (pp. 78–83), and geometrical illustrations of some theorems in number by Ellery W. Davis (pp. 84–90, with a diagram). M. Halphen is the mathematician whose portrait is given with this opening number.

Bulletin de l'Académie Royale de Belgique, No. 3 (1893).—Among the scientific papers communicated to the Academy are the following: On the common cause of surface tension and evaporation of liquids, by G. Van der Mensbrugghe. The author deduces from his theory an explanation of the fact that evaporation is more rapid from a convex, and less rapid from a concave, than from a plane surface.—Survival after the successive section of the two vagi, by M. C. Vanlair. Survival after successive section of both the branches of the vagus nerve can be obtained in full-grown animals as well as in young ones. The time necessary for the regeneration of its inferior laryngean branch is generally much longer than that hitherto accepted. In the full-grown dog the period exceeds at least ten months. The regeneration of one branch is quite independent of the section of the other. The question whether the pneumogastric, like the sciatic nerve, possesses the power of regenerating itself twice in succession remains as yet unanswered. It is, however, certain that an interval of six months and a half does not suffice for its second regeneration.—On the digestion of the cœlenterata, by Marcelin Chapeaux. The action of the ferments secreted by the actinia upon starch, cellulose, chlorophyll, and fat, was investigated. Starch submitted to the action of an aqueous solution of these ferments, or injected into the gastrovascular cavity, was transformed into glucose. The action was slow in the case of non-hydrated starch. The transformation took place equally well in acid and in alkaline solutions. Cellulose and chlorophyll were not digested. The fats were emulsioned by the ferments contained in the endodermic cellules. These ferments were without effect upon the algæ. Among the Siphonophora digestion is certainly exclusively intracellular. No dissociation of fibrine is, on the other hand, ever observed in the gastrovascular cavity, and no difference could be established between the alkalinity of the liquid contained in this cavity and the surrounding sea-water.—Contribution to the nitrogen question, by A. Petermann. This is an experimental confirmation of the results of MM. Schlœsing fils and Laurent, showing that free nitrogen is absorbed from the air by the micro-organisms of the soil.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 2.—"Harmonic Analysis of Hourly Observations of Air Temperature and Pressure at British Observatories," by Lieut.-General R. Strachey, R.E., F.R.S.

This paper is a discussion of the results of the computations contained in a volume recently published by the Meteorological Office, of the harmonic components of the first four orders, for each month for twenty years, of the daily curves of temperature and pressure at Greenwich; and for the first three orders, for the temperature and pressure, for each month for twelve years, at the seven observatories maintained by the Meteorological Office.

This system of analysis supplies the means of establishing an exact comparison between various unsymmetrical curves, such as those representing hourly values of temperature, by resolving them into symmetrical components, having periods of twenty-four hours, twelve hours, eight hours, and six hours, and so forth, and its application to the records dealt with in the tables contained in the volume above referred to gives satisfactory proof of the important light it can bring to bear on the periodical changes of diurnal temperature.

In the usual expression the coefficients of the cosines of the arcs are designated by the letter p, and those of the sines by q. The total amplitude of the component is designated by P.

A modification of the usual notation is made by the introduction of the value of the epoch of the first maximum that occurs after midnight, which is designated by the letter μ, and establishes the connexion of the component with the hour of the day and the sun's place more conveniently than the method usually adopted.

1. *Greenwich Temperature.*

The examination of the tables shows that, with very considerable variations of absolute magnitude, there is on the whole very marked consistency in the main characteristics of the components.

Taking as a test the position of the epoch of maximum, which is more directly dependent on the sun's action and on his position than the amplitude, it will be seen that the values of μ indicate very clearly the closeness of this connexion.

In all the components a truly periodical variation of the value of μ is apparent, and the period of maximum always travels backwards, that is, it becomes earlier as the year passes from winter to summer, while it returns in the opposite direction in the change back to winter.

For the first component the variation of the five years' mean of μ from the twenty years is in no month more than $2\frac{1}{2}°$, or ten minutes of time, and the average for all months is less than half that amount.

In the second component the variation of the five-year mean from the twenty-year mean is in no month more than 6°, and the average is only 2°·3, or nine minutes of time.

In the third component the variation of the five-year from the twenty-year mean in no month exceeds 5°, and the average in all months is only 2°·1, or $8\frac{1}{2}$ minutes of time.

The largest variation of the five-year mean of the fourth component for any month from the twenty-year mean is 10°, and the average for all months is 4°·3, or seventeen minutes. Considering how small are the absolute values of the coefficients p_4 and q_4, on which the value of μ_4 depends, the average being a little less than $\frac{1}{10}$th of a degree Fahrenheit, it is rather a matter of surprise that the variations should be so small than that they should reach their actual amounts.

The component of the first order, which in the winter is more than double the magnitude of any of the others, and in summer more than ten times as great, gives the dominant character to the daily curves of temperature. In the series of twenty years variations in different years of as much as 100 per cent. are to be found for almost every month, but for the most part even these irregularities disappear in the mean of a series of five years, and the monthly means for the twenty years are remarkably consistent.

The progression of the value of P, in the course of the year, follows approximately the sine of the sun's meridional altitude and the empirical formula

$$P = 10 \cos z - 0{\cdot}91$$

gives a close approximation to the values shown in the tables, if a "lagging" of eight or ten days is allowed in reckoning the sun's place.

The second component has two clearly marked *maxima* about the time of the equinoxes, and a principal *minimum* at midsummer.

The component of the third order varies in a converse manner, having two well-marked *minima* at the equinoxes, with a principal *maximum* at midsummer.

The component of the fourth order appears to combine the characters of the two previous ones, having two *maxima* about the time of the equinoxes, and a principal *minimum* in the winter.

The mean value of μ for the first component is 214°, corresponding to 2h. 26m. p.m., the variation due to season being 12° or 48m. of time, by which the maximum is earlier in summer than in winter.

In the second order the first maximum in June is 24°, or 1h. 20m. earlier than in January.

In the third order the difference in the same direction is 63°, or 4h. 12m. of time.

In the fourth order there is some doubt as to the manner in which the change of epoch of the summer and winter maxima is brought about. But remembering that the fourth component includes four series of undulations, the most probable explanation of these changes is to be found in a change of the position of these undulations, during which, between January and February, when the first maximum is about 10° after midnight, or 0h. 40m. a.m., the first recedes, and its place is taken by the second, which leads to sudden appearance of a maximum about 60°, or 4 a.m. A similar change between October and November in an opposite direction would reproduce the maximum at 10° after midnight.

In the summer months (May, June, and July) the temperature curve during the day hours, from 8 a.m. to 8 p.m., hardly differs from a curve of sines, the first component being more than ten times as large as any of the others, which therefore influence the temperature, relatively, very little.

The relation of the epoch of the first maximum of the component of the third order to the time of sunrise is decidedly marked, the former occurring, on the average, about 12°, or 48m. after sunrise; the mean deviation of the interval from that amount being only 7°, or 28m.

The periodical variation in the position of the maximum leads, during the winter months, to a *positive* maximum of this component about 1 p.m., which is combined with *negative* maxima four hours earlier and later, which correspond to the *reduced* temperature in the mornings and afternoons of the *shorter* days. In like manner, in the summer months, when this component has a negative *maximum* about 1 p.m., instead of a negative *minimum*, as in winter, there will be two *positive* maxima, one four hours earlier, the other four hours later, corresponding to the *higher* temperature in the mornings and afternoons of the *longer* days.

It will be seen that these positions of the midsummer and midwinter maximum phases correspond respectively to days of 16 hours with nights of 8 hours, or days of 8 hours and nights of 16 hours, and that at these seasons, when the variations of temperature, due to these differences, are greatest, the amplitudes of this component are also the greatest. At the equinoxes, with 12 hour days and nights, the component becomes a minimum; and at this season the change in the position of the maximum takes place as already noticed.

It might be supposed that an analogous relation between the fourth component and the occurrence of days of 18 hours, combined with nights of 6 hours, and *vice versâ*, is likely to arise. But the data are not forthcoming to test this.

In the summer months the time of mean temperature is nearly where the first component becomes zero, the second and third components then balancing one another.

In the winter the time of morning mean temperature is later than in summer, and occurs when a positive value of the first component is equal to a negative value of the second.

The time of afternoon mean temperature throughout the year is somewhat either before or after 7 p.m., and almost exactly coincides with the time when the first and second components are equal, with opposite signs.

In the summer the time of absolute minimum is between the hours of 3 a.m. and 6 a.m., during which the whole of the components are negative.

Sunrise in December is about an hour and a half before the time of mean temperature; while in June it is more than four hours earlier.

Sunset in December is rather more than three hours *before* the time of mean temperature; in June it is about half an hour *after* that time.

The *rationale* of some of the empirical rules for obtaining the mean daily temperature from a limited number of observations is supplied by reference to the harmonic expressions for the hourly deviations of temperature from the mean value.

In the first place, it will be seen that by adding together the harmonic expressions for any two hours twelve hours apart, the whole of the *odd* components disappear, and that the sum is twice the mean value, added to twice the sum of the *even* components of the selected hours, which are equal.

By taking the mean of observations at any four hours, at intervals of six hours, both the odd components and those of the second order will disappear, and the result will only differ from the true mean by the amount of the fourth component for the selected hours.

So, if the mean of any three hours at equal intervals of eight hours be taken, the sums of the first, second, and fourth components will disappear, and the result will only differ from the true mean by the amount of the third component for the selected hours, which in no case can be so much as $\frac{1}{4}$°.

2. *Temperature at the Seven Observatories.*

The examination of the tables will show that in their main characteristics the results closely resemble those for Greenwich, and it will not be necessary to discuss them in any detail.

The amplitude of the component of the first order is, however, in all cases less than that observed at Greenwich, the lowest values being those for Valencia and Falmouth, no doubt due to their position on the sea coast, for which stations the means for the years are 2°·28 and 2°·35 compared with 5°·10 at Greenwich.

The Kew values most resemble those at Greenwich, but the mean maximum at Kew is more than 1° less, and the mean for the year $\frac{1}{2}$° less.

The mean values of μ_1 for the seven observatories lie between 205° and 220°, that for Greenwich being 214°. The means of the summer values are about 3° or 4° less than the mean of the year, and of the winter values as much above it, as in the case of Greenwich.

The amplitude of the first component conforms approximately, but not so closely as at Greenwich, with the sine of the sun's meridian altitude, but with a flattening of the curve in the summer months, and a tendency at some of the stations to a maximum value in May.

The components of the second and third orders, beyond which the analyis is not carried for these observatories, conform in all important respects to those for Greenwich, the numerical values of the latter being, however, in all cases somewhat higher. The epochs of maximum follow the same laws, with an increased divergence of the summer epoch from that of the winter at the more northern stations.

In order to test, and in some degree throw light, on the character and significance of the harmonic components of temperature that have been under discussion, and bearing in mind that they cannot be considered to represent separate effects of physical forces operating at the assumed periods of the components, I have, at the suggestion of Prof. G. Darwin, calculated the harmonic components from a curve representing an intermittent heating action such as that of the sun, continued only during a portion of the day, and commencing and ending abruptly at sunrise and sunset.

All cooling effects have been disregarded, and the sun's direct heating action is assumed to be proportional to the sine of his altitude, the power of a vertical sun being taken to be 10. Having calculated the sun's altitude for each hour of the day, for midwinter, the equinox, and midsummer, for certain selected latitudes, the corresponding heating effects have been computed to which the usual method of analysis has been applied.

The comparison of the results thus obtained with the corresponding components derived from actual observation at places having nearly the same latitudes as those selected, establishes their close similarity, and the conclusion is unavoidable, that, although both in the actual and hypothetical cases the harmonic components when combined are truly representative of the peculiar forms of the curves from which they were derived, this affords no evidence of the existence of recurring cycles of action corresponding to the different components, but that the results are, to a great extent, due to the form of the analysis.

The diurnal curve of temperature is *not* symmetrical in relation to the mean value, the maximum day temperature being much more in excess than the minimum night temperature is in defect. To adjust the first component, which *is* symmetrical about its mean value, to the actual unsymmetrical curve, it must be modified by the other components. That of the second order, which has one of its maxima not far removed from the minimum of the first order, supplies the chief portion of the compensation due to this cause.

Further, from the character of the analysis, when the diurnal curve is symmetrical on either side of the hour half way between noon and midnight—that is, when the day and night are equal in length—the third component becomes zero. Any departure from this symmetry introduces a component of the third order, with the result that with a day shorter than 12 hours one maximum will fall in the day between 6 a.m. and 6 p.m., and the other two in the night between 6 p.m. and 6 a.m.; while with a day longer than 12 hours, two maxima will occur in the day and only one in the night. In the former case the negative portions of the component correspond with the reduced morning and afternoon temperatures of the short day, and in the latter the two positive phases correspond with the higher temperature of the mornings and afternoons of the longer day.

These conclusions are in conformity with those previously indicated.

The available data are insufficient to enable us to say whether the corresponding results connected with the fourth component are as fully supported by observation as in the case of the third, but the facts so far as they go confirm this view.

Anthropological Institute, April 11.—Prof. A. Macalister, President, in the chair.—Mr. G. M. Atkinson exhibited a cranium and several metal ornaments found by Mr. A. Michell Whitley and Dr. Talfourd Jones in a grave at Birling, near Eastbourne, Sussex. The peculiar coffin-like shape of the skull seemed to point to its belonging to the early Saxon period, while the metal ornaments were assigned to the late Roman or immediately post-Roman age.—Mr. R. Duckworth read a paper on two skulls from Nagyr, recently added to the Cambridge University collection. One of them is a female skull, and is remarkably dolichocephalic, the cephalic index being 69·94. The other skull is that of an adult male.—Prof. Macalister read a paper on Egyptian mummies. He described the manner in which they were prepared, the unguents used by the Egyptians and the various cloths in which the mummies were rolled. He explained the difference between the Egyptian cloths and those manufactured in England at the present day, and said that the object of using so few threads in the weaving was for the purpose of saving time and trouble. The material at the same time was brought to a high state of perfection as a manufacture, and indeed might even compare with some of the finest linen productions at the present day. Specimens of cloth were exhibited and the author stated, on the authority of a linen manufacturer, that there was only one specimen of linen manufacture in the United Kingdom which could be recognised as of similar structure to the Egyptian productions.—A paper on Damma Island and its natives by P. W. Bassett Smith, R.N., was also read.

Geological Society, April 12.—W. H. Hudleston, F.R.S., President, in the chair.—The following communications were read:—On some Palæozoic Ostracoda from Westmoreland, by Prof. T. Rupert Jones, F.R.S. In 1865 the author determined for Prof. Harkness some fossil Ostracoda which he had obtained from the Lower Silurian rocks of South East Cumberland and North-East Westmoreland, and subsequently other specimens mentioned by Harkness and Nicholson in 1872. In 1891 Prof. Nicholson and Mr. Marr submitted a series of similar microzoa from the same district; and the author now endeavours to determine their specific alliances, and revises the list of those previously collected. He has to notice about eleven forms of *Primitia*, *Beyrichia*, *Ulrichia*, *Æchmina*, and *Cytherella*—several of them being closely allied as varieties, but all worthy of study as biological groups, such as have been illustrated from other regions by writers on the Ostracoda, with the view of the exact determination, if possible, of species and genera, of their local and more distant or regional distribution, and of their range in time.—On some Palæozoic Ostracoda from the Girvan district in Ayrshire, by Prof. T. Rupert Jones, F.R.S. This paper aims at the completion of the palæontological account of the Girvan district, so far as the Ostracoda are concerned; and follows up the researches indicated in the "Monograph of the Silurian Fossils of the Girvan District in Ayrshire," by Nicholson and Etheridge, vol. i., 1880. In about a dozen pieces of the fossiliferous shales, submitted for examination some few years ago, the writer finds nearly thirty specimens of *Primitia*, *Beyrichia*, *Ulrichia*, *Sulcuna*, and *Cypridina* which show interesting gradations of form, not always easy to be defined as specific or even varietal, but valuable as illustrating modifications during the life-history of individuals, thus often leading to permanent characteristics of species and genera. Like those formerly described in Nicholson and Etheridge's "Monograph," the specimens have all been collected by Mrs. Elizabeth Gray, of Edinburgh.—The reading of these papers was followed by a discussion, in which the President, Mr. Marr, and the author took part.—On the dwindling and disappearance of limestones, by Frank Rutley. The existence of chert between two sheets of eruptive rocks at Mullion Island seemed to the author to require some explanation. Cherts are usually associated with limestones, and the absence of limestones in many cases where cherts are found points to their removal by underground waters. The older the limestone the greater the probability of its thickness having dwindled. The thicknesses of the Ordovician, Silurian, Devonian, and Carboniferous Limestones seem to be in the ratio of 1 : 15 : 15 : 100. Many limestones once existing in Archæan rocks may have disappeared, as also limestones in later rocks. The author comments on the difficulty of distinguishing some cherty rocks from felstones. Two appendices are added to the paper, the first on the transference of lime from older to newer deposits, and the second on the formation of nodular limestone-bands.—This paper gave rise to a discussion in which the President, Prof. Hull, Mr. Walford, Prof. Judd, General McMahon, Prof. T. R. Jones, Prof. Hughes, Mr. H. W. Monckton, Dr. G. H. Hinde, and the author took part.—On some Bryozoa from the Inferior Oolite of Shipton Gorge, Dorset, Part II., by Edwin A. Walford.

Royal Meteorological Society, April 19.—Dr. C. Theodore Williams, President, in the chair.—The following papers were read:—The direction of the wind over the British Isles, 1876–80, by Mr. F. C. Bayard. This is a reduction on an uniform plan of the observations made twice a day, mostly at 9 a.m. and 9 p.m., at seventy stations during the lustrum 1876–80; and the results are given in tables of monthly and yearly percentages.—Notes on two photographs of lightning taken at Sydney Observatory, December 7, 1892, by Mr. H. C. Russell, F.R.S. These photographs were taken with a half-plate view lens, mounted in a whole plate camera, and, as a matter of course, there is some distortion at the edges. Both photographs show the gaslights in the streets as white specks, the specks being circular in the centre and crescent-shaped in other parts of the plate owing to distortion. The lightning flashes are also distorted. Mr. Russell believes that this distortion may account for the so-called "ribbon" flashes, which are seen in many photographs of lightning. He has also made some measurements of the length and distance of the flashes, and of the intensity of the light.—Notes on lightning discharges in the neighbourhood of Bristol, 1892, by Dr. E. H. Cook. The author gives some particulars concerning two trees in Tyntesfield Park, which were struck by lightning, one on June 1 and the other on July 18, and also some notes concerning a flagstaff on the summit of Brandon Hill, which was struck on October 6.—Constructive errors in some hygrometers, by Mr. W. W. Midgley. The author, in making an investigation into the hygrometrical condition of a number of cotton mills in the Bolton district, found that the mounting of the thermometers and the position of the water receptacle did not by any means conform to the regulations of the Royal Meteorological Society, and were so arranged that they gave the humidity results much too high. The Cotton Factories Act of 1889 prescribes the maximum weight of vapour per cubic foot of air at certain temperatures; and the author points out that if the instruments for determining the amount present in the mills have an error of 20 per cent. against the interests of the manufacturer, it is necessary that the makers of the mill hygrometers should adopt the Royal Meteorological Society's pattern for the purpose.

Paris.

Academy of Sciences, April 17.—M. Lœwy in the chair.—Note on the observation of the partial eclipse of the sun of April 16, 1893, by M. F. Tisserand.—On the observation of the total eclipse of the 16th inst., by M. J. Janssen.—Effects of the drought upon this year's crops; reply to M. Demontzey's note on the planting of the highlands, by M. Chambrelent.—Expansion of water at constant pressure and at constant volume, by M. E. H. Amagat. At pressures higher than 200 atmospheres water has no maximum density above zero. At the lower temperatures, contrary to what takes place in the case of other liquids, the coefficient of expansion increases with the pressure. This increase is gradually effaced as the temperature rises, is sensibly zero at 50° or 60°, and changes sign for higher temperatures. If water is kept at a constant volume the pressure increases rapidly with the temperature. Thus, for unit volume the coefficient of pressure increases fourfold between 10° and 100°, and the variation is proportionately even more rapid between 0° and 10°.—On the structure of simple finite and continued groups, by M. Cartan.—On a simple group with fourteen parameters, by M. F. Engel.—Demonstration of the transcendental nature of the number *e*, by M. Adolf Hurwitz.—Comparison of the international meter with the wave-length of cadmium light, by M. Albert A. Michelson.—Photography of gratings engraved upon metal, by M. Izarn. It is possible to reproduce opaque gratings engraved upon metal in a manner analogous to the reproduction of transparent ones already described. On covering such a grating with a layer of bichromated gelatine, and exposing to the sun through this layer, a grating effect is produced which, although rather feeble, is due to successive differences of structure corresponding to the rulings. These differences of structure are probably due to stationary reflected waves, and need not necessarily be alternations of transparency and opacity in order to produce the desired effect. Very close contact between the film and the grating is essential.—On atmospheric polarisation, by M. A. Hurion.—Researches on the higher alcohols and other impurities in vinic alcohol, by M. Émile Gossart.—On the general relations which exist between the coefficients in the fundamental laws of electricity and magnetism, by M. E. Mercadier.—On the reflection of electric waves at the end of a linear conductor, by M. Birkeland.—Multiplication of the number of periods of sinusoidal currents, by M. Désiré Korda.—On the hygroscopic properties of several textile fabrics, by M. Th. Schlœsing fils.—Contribution to the study of the Leclanché cell, by M. A. Ditte.—Attempt at a general method of chemical synthesis; formation of nitrogen compounds, by M. Raoul Pictet.—On the stereochemistry of the malic compounds, and the variation of the rotatory power of liquids, by M. Albert Colson.—On a chlorobromide of iron, by M. Lenormand.—On the saccharates of lime, by M. Petit.—On a new soluble ferment doubling trehalose into glucose, by M. Ém. Bourquelot.—On the circulatory apparatus of *Mygale Cæmentaria*, Walck, by M. Marcel Causard.—Influence of the pressure of gases upon the development of vegetables, by M. Paul Jaccard.—On the ammonite layers of the inferior Malm in the county of Montejunta, Portugal; little known phases in the development of the mollusca, by M. Paul Choffat.—On the mode of reproduction of the parasites of cancer, by MM. Armand Ruffer and H. G. Plimmer.—M. Lippmann presented to the Academy, in the names of MM. Auguste and Louis Lumière, coloured photographs obtained by the interference method.

Berlin.

Physical Society, March 10.—Prof. Kundt, President, in the chair.—The President gave an account of some researches undertaken as an introduction to the study of Hall's phenomenon. As is well known, this is directly proportional to the intensity of the primary current, but inversely proportional to the pressure of the plates; on the other hand, it is not strictly proportional to the magnetising current in the case of the several metals so far examined, and it appeared probable that it might more possibly be proportional rather to the magnetisation of the plate. Prof. Kundt wished to test this possibility in the case of iron, nickel, and cobalt, employing transparent metallic films of these metals magnetised to 28,000 units, whose magnetisation could be tested directly by means of their rotatory power. It was found that the Hall effect increased hand in hand with the increase of rotatory power, and therefore proportionally to the magnetisation of the plates. The effect was, as Hall had already shown, positive in the case of iron and cobalt, negative in that of nickel. Bismuth deposited electrolytically in a transparent film gave very feeble or no results, whereas, when drawn out into a thin plate the effect was considerable.—Dr. Wren spoke on Maxwell's proposition that waves of light exert pressure in the direction of their transmission, as proved in a certain case by Boltzmann. He deduced, under certain assumptions, a formula for the calculation of temperature based upon a determination of maximal energy.

Amsterdam.

Royal Academy of Sciences, March 25.—Prof. van de Sande Bakhuysen in the chair.—Mr. Pekelharing spoke of the peptone of Kühne. Some years ago he argued there was not a real difference between the substances called peptone, and the substance called propeptone or hemialbumose. The researches of Kühne and his disciples afterwards proved that what was called peptone by Schmidt-Mülheim and by Salkowski, contained albumose. But it was not proved by Kühne that the substance called by himself peptone was really free from albumose. Out of a solution of Kühne's peptone, saturated with ammoniumsulphate, there can be precipitated by metaphosphoric acid, and more fully by trichloracetic acid, a substance which has the properties of albumose. It gives the biuretreaction, it is precipitated, the reaction may be acid, neutral, or alkaline, by ammoniumsulphate, it is precipitated by picrinic acid, and, in acid solution, by saturation with natriumchlorid. So it is clear that there is no ground for believing with Kühne that the substance called by him peptone is a substance *sui generis*, and not an impure albumose.—Mr. Bakhuis Roozeboom dealt with the cryohydrates in systems of two salts. Three cases are to be considered. The first is that the two salts may exist without combination. Then there is a cryohydratic point in which the two salts A and B exist with ice next the solution. This point is a minimum temperature. Besides, there are two cryohydratic lines, representing the series of solutions which may exist with ice and A or ice and B as solids. In the other cases when A and B form a double salt D, there are two cryohydratic points, one for the solution in equilibrium with ice + D + A, the other for ice + D + B; and three cryohydratic lines for the solutions in equilibrium with ice + D, ice + A, ice + B. When the double salt is soluble without decomposition, the two cryohydratic points are both minimum temperatures, and therefore there must exist a maximum temperature on the line for ice + D; this maximum relates to the solution which presents the same relation A/B as in the double salt. All these conclusions may be deduced from thermoedynamical rules; they were confirmed in experimental research by Mr. Schreinemakers.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Books.—Carlsbad, a Medico-Practical Guide: Dr. E. Kleen (Putnam).—Louis Agassiz, his Life and Work: Dr. Holder (Putnam).—Die Natürliche Auslese beim Menschen: O. Ammon (Jena, Fischer).—Public Health Laboratory Work: H. R. Kenwood (Lewis).—Annual Statement of Works carried out by Public Works Department (Sydney, Potter).—The Principles of Agriculture: G. Fletcher (Derby, Central Educational Company).—Science et Religion: T. H. Huxley (Paris, Baillière).—Au Bord de la Mer: Dr. E. L. Trouessart (Paris, Baillière).—Conférences Scientifiques et Allocutions—Constitution de la Matière: Lord Kelvin. Traduites et Annotées sur la Deuxième Edition: P. Lugol and M. Brillouin (Paris, Gauthier-Villars).—Premiers Principes d'Électricité Industrielle: P. Janet (Paris, Gauthier-Villars).—The Great Barrier Reef of Australia: W. Saville-Kent (W. H. Allen).

Pamphlets.—Meteorological Results deduced from Observations taken at the Liverpool Observatory during the Years 1889, 1890, 1891 (Liverpool).—On the Effects of Urban Fog upon Cultivated Plants: Prof. F. W. Oliver (Spottiswoode).—The Fundamental Hypotheses of Abstract Dynamics: Prof. J. G. MacGregor.—Il Clima di Torino: G. B. Rizzo (Torino, Clausen).—On the Application of Interference Methods to Spectroscopic Measurements: A. A. Michelson (Washington, Smithsonian Institution).—Recreation: W. Odell (Torquay, Iredale).

Serials.—Journal of the Chemical Society, April (Gurney and Jackson).—Annalen des k. k. Naturhistorischen Hofmuseums, Band viii No. 1 (Wien, Holder).—Timehri, No. xxii. (Stanford)—Notes from the Leyden Museum, vol. xv. No. 2 (Leyden, Brill).—L'Anthropologie, tome iv. No. 1 (Paris, Masson).—Journal of the Royal Microscopical Society, April (Williams and Norgate).—The Asclepiad, No. 37, vol. x. (Longmans).—Records of the Geological Survey of India, vol. xxvi. Part 1 (Calcutta).

CONTENTS.