THURSDAY, FEBRUARY 16, 1888.

KINEMATICS AND DYNAMICS.

An Elementary Treatise on Kinematics and Dynamics. By James Gordon MacGregor, M.A., D.Sc., &c, Munro Professor of Physics, Dalhousie College, Halifax, N.S. (London: Macmillan and Co., 1887.)

THE logical order of arrangement has been carefully attended to in this book: Part I., on "Kinematics," building up a new subject on the foundation of Euclid's axioms in conjunction with the idea of the variables, such as velocity and acceleration, due to the flow of time; while Part II., on "Dynamics," requires three new axioms —Newton's Laws of Motion—to make a fresh start and connect mechanical effects with their causes.

But it is doubtful if the strictly logical order is the best order for the student to make his first acquaintance with a new mathematical subject: the ideas must grow in his brain by accretion round simple fundamental problems. A student would master the present treatise more easily by reading Part II. first, and referring back to Part I. as occasion required, for the explanation of the details of the mathematical calculations. There is nothing to prevent this order of study here, although the author has, from logical considerations, placed the kinematical part first.

One defect of the logical system is that it places some of the most difficult parts of the subject in the way of beginners: for instance, the theory of the change of units, a theory of which the importance can only be appreciated by those who have made considerable progress in the subject.

In Part I., "Kinematics," the treatment is simple and concise, but we should like to see more examples of phenomena on a large scale, such as those of physical astronomy, or even of railway-train problems.

In questions involving the size of the earth (pp. 74 and 80) it is the circumference and not the diameter which should be given in metres, the circumference being 40,000,000 metres, a kilometre being a centesimal minute of latitude. Or, if the size of the earth is given in miles, it is the nautical mile which should be used, the circumference of the earth being $360 \times 60 = 21{,}600$ nautical miles, a nautical mile being a sexagesimal minute of latitude.

The expression "knots an hour" (p. 60) is irritating to a sailor, as emanating from the engine-room; the proper nautical expression is "knot" simply, a speed of 10 knots being 10 nautical miles an hour.

The formula $\frac{1}{2}v^2 = \frac{1}{2}v_0^2 + as$ is to be preferred to that on p. 34, $v^2 = v_0^2 + 2as$; in all cases the factor $\frac{1}{2}$ should go with the v^2 in the equation of energy, so that the objectionable expression "*vis viva*" may finally be stamped out from all dynamical treatises.

In dealing with rotation, in Chapter V., the author would do well to study Maxwell's geometrical representation of the direction by means of the screw, right-handed or left-handed; and to discard all attempts by comparison with a clock-wise or counter-clock-wise rotation, requiring as these do a specification of the aspect of the plane of motion.

Pure homogeneous strain is analyzed in Chapter VII. as far as is possible by simple geometrical methods; such a strain may be produced by the superposition of three linear strains in directions at right angles to one another. In a linear strain the increment of distance of two points in the line of the strain is properly their *elongation;* while the ratio of the elongation to the original distance is called the *extension*, not the *elongation*, as on p. 167.

In Part II., "Dynamics," we find in Chapter I. the discussion on the units of measurement of weight, mass, and force customary in mathematical treatises, and of the usual unsatisfactory nature. The author, disregarding the vernacular use of the word "weight," defines the weight of a body as the force with which it is attracted by the earth, but is at variance with his own definition in the statement of the majority of the subsequent examples, relapsing into the language of ordinary life. A collection of 500 different ways of spelling the name of the town of Birmingham has been made, and a similar collection could be made from the present treatise of different ways of expressing the simple ideas of the pound *weight* and the pound *force*, to use the ordinary language of practical men. The attraction of the earth on a pound is, in the vernacular, "the force of a pound," not the "weight of a pound," the latter implying what the mathematician likes to distinguish as the "mass of a pound." Thus a mathematical precisionist, to express the simple idea of a force of 10 pounds, to be consistent should call it "a force equal to the weight of the mass of 10 pound weights," the absurdity of which is evident.

Again, in straining after the equation $F = ma$, when using the gravitation unit of force, the mathematician in the F.P.S. (foot-pound-second) system of units is obliged to use the variable unit of mass of g pounds to measure the invariable quantity, the mass of the body; while what he calls the weight of the body, and denotes by w, measuring it in pounds, is, although variable with g, always measured by the same number.

Next we have the equation $w = mg$, the source of all the confusion in dynamical teaching, and only to avoid writing the dynamical equation with gravitation units in the form

$$F = w\frac{a}{g}.$$

This terminology culminates in the solecisms that on p. 477 we must suppose pressure to be measured in poundals on the square foot in hydrostatical problems; and that if the equation $w = mg$ is supposed to be used with absolute units, that the weight of a body is measured in poundals; as if a mathematician asked in a shop for "half a poundal of tea, or tobacco." Ordinary people measure weight in pounds, so that if mass is also measured in pounds, then $w = m$.

It is time now, as Prof. Minchin has pointed out, that "the astronomical unit of mass," defined in § 315, should disappear, and that in all problems of physical astronomy the gravitation constant k should be retained, while m, the mass, is measured in terms of the ordinary units.

Although the author does not allow himself the use of the methods and notation of the Calculus, still he has managed to discuss a number of interesting problems in the dynamics of a rigid body, usually proved by the methods of Analytical Mechanics.

Working under these restrictions, he has given elegant elementary proofs of the chief properties of the common catenary; but here, again, it is time that the equation should be presented in the form $y/a = \cosh x/a$, using the notation of the hyperbolic functions; which might also be employed with advantage in the statement of the results of the examples on p. 302. Chains of 5000 feet span, and 400 feet versed sine, are in existence, providing striking numerical examples in this part of the subject.

Most of the examples are carefully chosen, but the author by diligent search could easily add more interest to the collection, particularly to the examples on parabolic trajectories, and problems concerning the motion of railway-trains. Ex. 85, p. 499, certainly requires careful revision. The diagrams of the simple machines are of the usual academic nature; the author should consult Prof. Kennedy's "Mechanics of Machinery" for better illustrations, especially of the differential pulley, and of pulley tackle in general. If the differential screw is given (p. 435), why not also the integral screw, which is to be met with more commonly in real life—for instance, in railway couplings, and in the rigging of ships.

Except for the parts criticized above, on the units of weight, mass, and force, the present treatise shows that the author has read with profit and discrimination the most recent treatises on dynamics; he has produced a very useful work, suitable for instruction in technical colleges, and likely also to prove a necessary corrective to the very abstract treatment of the subject of mechanics too common in the character of University instruction.

A. G. GREENHILL.

ATLAS OF THE DISTRIBUTION OF PLANTS.

Atlas der Pflanzenverbreitung. (Berghaus's "Physikalischer Atlas," Abtheilung V.) Bearbeitet von Dr. Oscar Drude. (Gotha: Justus Perthes, 1887.)

THE history of the science of the distribution of plants begins with Linnæus, who was the first to cite systematically the countries and situations in which the plants he described grew. This we find carefully done in the first edition of the "Species Plantarum," published in 1753. No perceptible advance beyond this was made before the appearance of Humboldt and Bonpland's "Essai sur la Géographie des Plantes" in 1805, which work may be designated the real foundation of the science. It was followed in 1823-24 by the Dane, Schouw's "Grundtræk" and "Plantegeographisk Atlas," the latter containing twenty-two maps illustrating the vegetation of the world, and especially the distribution of plants cultivated for food. There is also a German edition of both the "Outlines" and the "Atlas." From this date onward many of the most eminent botanists investigated distribution in connection with classification of plants, notably R. Brown, A. P. De Candolle, H. C. Watson, C. Darwin, A. De Candolle, J. D. Hooker, Edward Forbes, Von Martius, and Grisebach, to say nothing of the younger botanists. But the results of their labours ars still scattered, or at least only partially elaborated; for Grisebach, in his "Vegetation der Erde," deals with the facts from a peculiarly narrow stand-point.

It is true that both Drude and Engler ("Versuch einer Entwicklungsgeschichte der Florengebiete") have attempted something beyond this, but neither, we suspect, regards his work as more than a preliminary effort. The primary geographical divisions of these two writers are essentially the same, though their nomenclature differs; but, considering the complexity of the subject, probably no two persons would agree exactly on these points; yet it is highly desirable that there should be something approaching uniformity in the names of the divisions. Grisebach designates his primary divisions "Gebiete," and Drude his "Reiche"; whilst Engler's four primary divisions are designated "Reiche," and his secondary ones "Gebiete." Let us now briefly examine the main features of Drude's Atlas. Following the most authoritative English writers on zoological and botanical geography, we will call the primary divisions regions, and the secondary divisions sub-regions.

Drude divides the world into fourteen floral regions, and each of these into a number of sub-regions, indicating by lines and dots the overlapping of the elements of contiguous sub-regions. The regions are: (1) Northern, (2) Central Asia, (3) Mediterranean, (4) East Asia, (5) Middle North America, (6) Tropical Africa, (7) East African Islands, (8) Indian, (9) Tropical America, (10) Cape, (11) Australia, (12) New Zealand, (13) Andes, (14) Antarctic.

While agreeing in the main with the foregoing divisions, we cannot but regard some of them as including too, much or too little, according to the number of primary divisions adopted. We recognize the difficulties of the task, and admit that it is practically impossible to divide the vegetation of the world into regions of equal value and importance, even leaving out of consideration the mountain flora within the tropics. Instead, however, of giving Madagascar and the neighbouring islands the rank of an independent region, we should treat it as a sub-region of the tropical African flora. On the other hand, the Indian region seems too comprehensive, as it includes the whole of tropical India, Malaya, Cochin-China, the Malayan Archipelago, New Guinea, North Australia, and Polynesia, even to the Sandwich Islands. The very extensive recent collections of Madagascar plants, made by various English and French travellers, prove that the flora is really a sub-region of the tropical African flora. With regard to the flora of Polynesia, it is true that the littoral element consists almost exclusively of species common to the Malayan Archipelago and North Australia, many having an even wider range; but the Australian and American affinities of the endemic element are certainly too pronounced, in our opinion, to treat this flora as a sub-region of the Indian; and the Sandwich Island flora is as highly specialized, to say the least, as that of New Zealand. Perhaps it would be more convenient to make it an independent region. Again, the purely Australian types surely predominate largely over the Asiatic in North Australia, especially if we eliminate the widely-dispersed coast plants. Dr. Drude's New Zealand region includes the surrounding islands, except the more southern Macquarie; yet, of the eighteen vascular plants recorded from this island, sixteen are common to the New Zealand group. The Auckland and Campbell groups should be reckoned in the Antarctic region rather than New Zealand; and St. Paul and Amsterdam Islands, as well as the Tristan d'Acunha group, do not belong to the same category. Further, the higher mountain flora of Central America and South Mexico has certainly a greater claim to be included in the Andine region than has that of the Galapagos, though Dr. Drude separates them.

We have called attention to these defects or inconsistencies in the limitation of the regions, because we believe that the latest and fullest data relating to the regions in question clearly indicate that it is not a matter of opinion.

The Atlas as a whole is a most laborious and careful compilation, and we do not doubt that it will meet with the favour it deserves. The second sheet of maps illustrates the areas of certain important natural orders and genera; the third, the horizontal zones of vegetation of the world; the fourth, the flora of Europe; the fifth, the floras of Europe and Asia; the sixth, the floras of Africa and Australia; the seventh, the floras of America; and the last represents the areas of plants cultivated for their economic products.

It would be easy enough to find fault with some of the details of the limitation of Dr. Drude's sub-regions, those of tropical Africa and Eastern Asia for example, though it would not always be so easy to suggest more satisfactory ones; but we prefer judging the work by its merits rather than by its real or assumed defects.

This Atlas, it should be added, is a cartographical development of Dr. Drude's "Florenreiche der Erde," which appeared in 1884, and formed the Ergänzungsheft 74 to *Petermann's Geographische Mittheilungen;* that is to say, it is a development so far as the maps are concerned, but the explanatory letterpress has been reduced to four pages folio. The maps, sixteen in number, are admirably executed, and exceedingly elaborate; indeed, the only fault we find in them is an excess of detail, with perhaps too little explanatory text for beginners.

As the author very truly observes, the material available for such a work is now almost inexhaustible, and the task of selecting from it for the purposes in view was no easy one. He brings into contrast the position of botanical geography in 1848, when the first edition of Berghaus's "Physical Atlas" was published, and there was nothing approaching a complete flora of any of the larger areas outside of Europe in existence. Even in 1855, when De Candolle gave to the world his now classical work, "Géographie Botanique Raisonnée," he could only deal with fragments of floras. Now, though it may safely be asserted that future discoveries can in no way affect the main theories of distribution based upon what is already known, very much remains to be done in fossil botany before we shall be able to trace in detail the early migrations of plants. Therefore the only thing that can be successfully accomplished yet is to work out more completely the present distribution of plants, which is practically all that Wallace has done for animals. But he deals specially with the quality and probable origin of the zoology of his regions; and it is just this aspect of botanical geography that awaits further development.

OUR BOOK SHELF.

Tenants of an Old Farm. By Henry C. McCook, D.D. (London; Hodder and Stoughton, 1888.)

THE object of this book is to present "a series of exact truths from natural history in a popular form." The author's original intention was to write a number of essays upon insect life, and particularly upon the life of ants and spiders, which he has especially studied. Friends, however, persuaded him to give the essays a colloquial form, so that they might appeal to as wide a circle of readers as possible. We are not sure that the change was in all respects an improvement, for, as Dr. McCook says, "the truths of Nature are attractive enough in themselves, and need not the seasoning of fiction." The book is very popular in the United States, and there can be little doubt that it will also be appreciated on this side of the Atlantic. The author is a keen and accurate observer of Nature, and his enthusiasm for his subject is so steadily maintained that it cannot but exert some influence on the minds of young students. For the present edition a brief introduction has been written by Sir John Lubbock, who bears cordial testimony to the fidelity and skill with which Dr. McCook has carried on his researches. The work is remarkably well illustrated.

Digging, Squatting, and Pioneering Life in the Northern Territory of South Australia. By Mrs. Dominic D. Daly. (London: Sampson Low, 1887).

THIS is an interesting account of a part of South Australia which is sure to become more and more important. The writer spent three years—from 1870 to 1873—in the Northern Territory, and by far the best chapters are those in which she records her own experiences. The history of the district during the last fourteen years has, however, been carefully compiled from the most trustworthy sources. She has, of course, a good deal to say about the natives, her accounts of whom are freshly and brightly written. Mrs. Daly is of opinion that, so far as the treatment of the aborigines is concerned, only one rule holds good—"firmness accompanied by kindness, fair play, and an honest payment for work done." If they make themselves disagreeable, they must be kept "in their proper place," "for," she says, "when a native shows signs of sulkiness and defiance, it is perfectly certain some mischief is brewing."

Photography Simplified. (London: Mawson and Swan, 1887.)

THIS is a third edition, considerably revised and enlarged, of an elementary and practical treatise, intended chiefly for amateurs and those about to become acquainted with the subject. The earlier chapters deal with the purchasing of apparatus, followed by the various processes of taking the negative, developing, printing, &c., and are written in a very plain and intelligent way. The book concludes with an appendix containing additional useful formulæ, together with a set of labels for the photographic laboratory.

LETTERS TO THE EDITOR.

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

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

An Explanation explained.

I AM glad to find that Dr. Guppy has at last enabled us to get to the bottom—I cannot say to the foundation—of the story which was related by the Duke of Argyll on November 17 in last year, to the discredit of Prof. Bonney and the authorities of the Geological Society. It is now admitted that the paper, said to have been "offered to," and "refused by," the Society, never came before the President and Council in any form whatever; and that in fact the paper was not only never presented, but was never even written!

Dr. Guppy's references to myself are capable of the simplest explanation. During the whole time that he was absent in the Solomon Islands, I was in the habit of receiving specimens and letters from him; and, as he has acknowledged in his book, I carefully studied these specimens and gave him all the advice and assistance in my power in carrying on his geological investigations. Upon his return we had several conversations, always of the most friendly character, concerning the best mode of embodying his observations for communication to different Societies; and until the present time I had not the smallest idea that he was in any way dissatisfied with anything I had ever said or done in connection with the subject.

With respect to a particular conversation referred to by Dr. Guppy as having occurred in the spring of 1885, I have no recollection whatever; but I unreservedly accept his statement as to the *facts* of the case, and only demur to his *interpretation* of them. If Dr. Guppy or anyone else asked my opinion as to the fitness for the Geological Society of "a paper in which Mr. Darwin's theory of coral reefs would be brought under consideration," I should undoubtedly point out to him that the Geological Society had always been averse to publishing papers dealing with such broadly theoretical questions as the origin of coral reefs, and I should advise some other means of publication as more appropriate.

That the Geological Society is not partial in its reluctance to publish papers of a theoretical character will be seen from the fact that although Mr. Darwin in 1837 read a paper to the Society, embodying the chief points of his theory of coral reefs, yet the Society never published the paper in their Transactions. At the time that this occurred Darwin was a member of the Council, and a few months later he became Secretary of the Society.

It is surely unnecessary for me to remark that in any advice which I gave to Dr. Guppy, I was acting simply on my own judgment and individual responsibility. Dr. Guppy was a Fellow of the Society at the time, and had precisely the same right to present papers to it which I had myself. Dr. Guppy chose to ask my advice; I gave it to him as to a friend, and he was perfectly free to act upon it or to reject it as he thought fit. I may add that the Secretary of the Geological Society has nothing to do with the acceptance or rejection of papers, except as a member of the Council, and then only when the question has been brought up by the President for the approval or condemnation of his own action.

How my unofficial act of friendly advice concerning the destination of an unwritten paper came to be represented as the refusal of a paper offered to the Society I am at a loss to conceive! Why the Duke of Argyll, having received the statement which is now before us, should proceed to formulate the very grave accusation against Prof. Bonney personally, and the authorities of the Geological Society, it is for his Grace to explain.

With respect to Dr. Guppy's complaint that his memoirs have been "studiously ignored" during the recent controversy, I cannot help thinking that he has been unduly sensitive. Writing to Prof. Huxley in October last year, and pointing out that the Duke of Argyll was mistaken in supposing there had been no discussion on Mr. Murray's theory, I said "it would be an endless task to attempt to give references to the various scientific journals which have discussed the subject," but in penning these words I had not the smallest idea of speaking slightingly of any of the memoirs which want of space prevented me from citing, and least of all concerning those which contain the facts and observations of Dr. Guppy, of the value and importance of which I had such good opportunities of judging.

JOHN W. JUDD.

Reason and Language.

PROF. MAX MÜLLER has been so kind as to favour the readers of NATURE with his views on language and reason, concisely expressed in a letter to an American friend. As one grateful reader, I much desire both to express my thanks, and also to beg for yet a little further information with respect to matters of such extreme interest.

The Professor says: "Because we reason—that is, because we reckon, because we add and subtract—therefore we say that we have reason." Now, in the first place, I should be glad to be told why "reason" is to be regarded as identical with such "reckoning"? I have been taught to distinguish two forms of intellectual activity: (1) acts of intuition, by which we directly apprehend certain truths, such as *e.g.* our own activity, or that A is A; and (2) Acts of inference, by which we indirectly apprehend others, with the aid of the idea "therefore"—evolving into explicit recognition a truth previously implicit and latent in premisses. The processes of addition and subtraction alone, seem to me to constitute a very incomplete representation of our mental processes.

The Professor also identifies language and reason, denying to either a separate existence. As to "reason" he says: "We have only to look into the workshop of language in order to see that there is nothing substantial corresponding to this substantive, and that neither the heart nor the brain, neither the breath nor the spirit, of man discloses its original whereabouts." The expression "whereabouts" would seem to attribute to those who assert the existence of "reason," the idea that it possesses the attribute of extension! In order to understand clearly the passage quoted, we should learn what Prof. Max Müller really means by the term "spirit," which here figures as one species of a genus also comprising the breath, the brain, and the heart. Reason, however, is not represented as being simply language "as we now hear it and use it," but "as it has been slowly elaborated by man through all the ages of his existence upon earth." Thus understood, the Professor "cannot doubt" "the identity of reason and language." Nevertheless, he immediately proceeds to point out a striking want of identity between them. He says, quite truly, "We have two words, and therefore it requires with us a strong effort to perceive that behind these two words there is but one essence,"—namely, that denoted by the Greek word *logos*—"the undivided essence of language and thought." Now, the intimate connection of language (whether of speech or gesture) with thought is unquestionable; but intimate connection is not "identity." If thought and language are "*identical*," how came two words not to have two meanings, or two thoughts to be expressed by one word? The plain fact that we have different words with one meaning, and different meanings with one word, seems to demonstrate that thought and language cannot be "identical."

"No reason without language—no language without reason" is a statement true in a certain sense, but a statement which cannot be affirmed absolutely. Language (meaning by that term only *intellectual* expression by voice or gesture) cannot manifestly exist without reason; but no person who thinks it even *possible* that an intelligence may exist of which ours is but a feeble copy, can venture dogmatically to affirm that there *is* no reason without language, unless he means by reason, mere "reasoning," which is evidently the makeshift of an inferior order of intellect unable to attain certain truths save by the roundabout process of inference.

But I demur to the assertion that truly intellectual processes cannot take place in us apart from language. In such matters our ultimate appeal must be to our own reflective consciousness. Mine plainly tells me that I have every now and then apprehensions which flash into my mind far too rapidly to clothe themselves even in mental words, which latter require to be sought in order to express such apprehensions. I also find myself sometimes expressing a voluminous perception by a sudden gesture far too rapid even for thought-words, and I believe that other persons do the same. A slight movement of a finger, or the incipient closure of an eyelid, may give expression to a meaning which could only be thought in words by a much slower process.

It is the more remarkable that Prof. Max Müller should deny the existence of reason, since he unequivocally affirms, in rather lofty language, the existence of truth. Yet surely the existence of truth, in and by itself, is inconceivable. What can truth be, save a conformity of thoughts and things? I affirm, indeed, the certain existence of truth, but I also affirm that of reason, as existing anteriorly to language—whether of voice or gesture. What is the teaching of experience? Do men invent new concepts to suit previously coined words, or new words to give expression to freshly thought-out concepts? The often-referred-to jabber of Hottentots is not in point. No sounds or gestures which do not express concepts would be admitted by either Prof. Max Müller or myself to be "language."

The Professor speaks of the "alarmingly small" number of primitive concepts; but who is to be thereby alarmed? Not men who occupy a similar stand-point to mine. I fully agree with Prof. Max Müller in saying, "After the genesis of the first concept, everything else becomes intelligible."

We come now to the supreme question of the origin of languages. As to this, the Professor observes: "No one who has not himself grappled with that problem can appreciate the complete change that has come over it by the recognition of the fact that roots are the phonetic expressions of the consciousness of our own acts. Nothing but this, our consciousness of our own repeated acts, could possibly have given us our first concepts. Nothing else answers the necessary requirements of a concept, that it should be the consciousness of something manifold, yet necessarily realized as one. . . The results of our acts become the first objects of our conceptual thought." The truth of these statements I venture to question. After noting the dogmatic nature of the assertion "Nothing but this *could*, &c.," I must object to the statement of fact as regards human beings now. I do not believe that the infant's first object of thought is "the results of its own acts." In the first place, no object of our early thoughts is merely the "results of our own acts," but a combined result of our own activity and of the action on us of our environment. Secondly, my observations lead me to believe that the infant's first thoughts relate to things external, and certainly not to the results of its own activity as such, which is a highly complex and developed thought. It may be that the Professor, when he says "The results of our acts *become* the first object of our conceptual thoughts," means that such acts in remote antiquity *became* the object of man's first thought. This is probably the case, since, with respect to the origin of thought and language, Prof. Max Müller has adopted Noiré's crude notion that they sprang from sounds emitted by men at work, conscious of what they were doing, in the presence of others who beheld their actions and heard the sounds; the result being the formation of a conceptual word, to attain which five stages had to be gone through, as follows:—

"(1) Consciousness of our own repeated acts.

"(2) *Clamor concomitans* of these acts.

"(3) Consciousness of our *clamor* as concomitant to the act.

"(4) Repetition of that *clamor* to recall the act.

"(5) *Clamor* (root) defined by prefixes, suffixes, &c., to recall the act as localized in its results, its instruments, its agents, &c."

But, if language and reason are identical, reason could not exist before a single conceptual word existed. Nevertheless, to attain to this first single word, we see, from the above quotation, that man must have had the notion of his own acts as such; the notion of their repetition; the notions of clamor, action, and the simultaneity of clamor and action; the will to recall the act (yet *nihil volitum quia præcognitum*); and finally the notions of consequence, instrumentality, agency, or whatever further notions the Professor may intend by his "&c."

Thus he who first developed language must be admitted to have already had a mind well stored with intellectual notions! But can it for one instant be seriously maintained, close as is the connection of language with reason, that their genesis (miracle apart, of which there is no question) was *absolutely* simultaneous? He must be a bold, not to say a rash, man who would dogmatically affirm this. But if they were not *absolutely* simultaneous, one must have existed, for however brief a space, before the other. That intellectual language could have existed without reason is absurd. Reason, then, must, for however short a period, have preceded language.

In conclusion, I desire to point out a certain misrepresentation with respect to natural selection. The Professor says: "In the evolution of the mind, as well as in that of Nature, natural selection is rational selection; or, in reality, the triumph of reason, the triumph of what is reasonable and right; or, as people now say, of what is fittest." But, we may ask in passing, if reason has no existence, how can it "triumph"? The misrepresentation of natural selection, however, lies in his use of the word "fittest." When biologists say that the "fittest" survives, they do not mean to say that that survives which is the most "reasonable and right," but that that survives which *is able to survive*. What there is less "reasonable and right" in a Rhytina than in a Dugong, or in a Dinornis than an Apteryx, would, I think, puzzle most of our zoologists to determine; nor is it easy to see a triumph of reason, in the extermination of the unique flora of St. Helena by the introduction of goats and rabbits. ST. GEORGE MIVART.

Mechanical Equivalent of Heat.

I FIND that the mode of regarding J advocated in my letter in last week's NATURE (p. 320) is not quite new, for my brother, Dr. Oliver Lodge, writes to tell me that Clerk-Maxwell, on p. 298 of his "Theory of Heat," has called J the specific heat of water. However, he has not done so throughout the book, and I do not think it is the meaning generally attached to the symbol, though it seems to me that it should be so; that is to say, J should always be considered as denoting the specific heat of water at the temperature 0° C. ALFRED LODGE.

Coopers Hill, Staines, February 6.

"Is Hail so formed?"

I CANNOT accept Dr. Rae's explanation as a "simpler solution" of the phenomenon described by me in NATURE of January 26 (p. 295), because it is based upon meteorological conditions that were at the time non-existent.

My own observation of the pine-tree convinced me that at or near the summit there was no adherent ice or rime; and had there been beads of ice upon the leaves I should still have failed to see what should have caused them while frozen to become detached and change from beads to pellets.

There was a fine mist during the whole of the day, and I observed the phenomenon at 3.30 p.m.

A letter appeared in NATURE upon the same day as mine, drawing attention to the unusual atmospheric conditions observed about that time, and containing facts which manifestly support my theory. CECIL CARUS-WILSON.

Bournemouth, February 11.

The New Army Regulations.

THE new regulations for the Woolwich entrance examination have been very unfavourably received by men of science. This hostile criticism is in some respects the consequence of the absence of clear discrimination between them and those already in force for the Sandhurst examination.

It must be remembered that candidates for Woolwich cadetships must be between the ages of 16 and 18; that 6000 marks are awarded for mathematics, with 1500 more for drawing and English composition; and that in both the last June and December competitions less than 4000 marks sufficed to place a student among the successful competitors. Since candidates can pass in these subjects alone, it appears unreasonable to complain that youths of scientific power are excluded from the Royal Military Academy. Classics are sufficiently discouraged by the fact that they have no mark value after the cadet has entered the Academy. The 5000 marks offered in the entrance examination for Latin and Greek merely serve to encourage candidates who have been educated on the classical sides, which are almost always the stronger at our public schools. They really tend to widen rather than to narrow the sources from which candidates are drawn.

After a quarter of a century of continuous experience as a student and teacher of elementary science, I find myself reluctantly forced to the conclusion that chemistry, physics, and geology are not good educational subjects for lads under 16 years of age. I believe that it is in most cases desirable that youths intended for a scientific career should not specialize too early. A sound foundation of mathematics and modern languages is almost necessary to enable them to attack their scientific subjects efficiently. With minds trained to the use of the exact and powerful processes of mathematical reasoning, and able to readily appreciate and avail themselves of the wealth of scientific literature in France and Germany, they will probably become more useful officers than if they had acquired a smattering of science.

On the other hand, your wise censure of the discouragment of science in the Sandhurst regulations must commend itself to all thoughtful men. The case is even stronger than at first sight appears in the studious moderation of your judicious article. The limits of age are higher for Sandhurst, being 20, or in some cases 24. The training of the Line cadets is less complete. As they only spend one year at Sandhurst, they are obliged to confine their attention more strictly to professional subjects. Officers of the Line have often more leisure than those in the scientific corps, and there are many reasons why even a slight acquaintance with science would be helpful to them. It also seems hard that a candidate should be handicapped by not taking up Latin. Sometimes it has been discontinued for a considerable period, and a candidate can ill afford to take up "a 2000 subject," considering the severity of the competition.

I would wish respectfully to suggest that a memorial should be presented to the War Office by all interested in the teaching of science, praying that, if a candidate for an army examination wishes to substitute for Latin one of the sciences enumerated in Group II., it should be allowed a maximum of 3000 marks.

2 Powis Square, W. HENRY PALIN GURNEY.

"British and Irish Salmonidæ."

As your reviewer allows that he "intentionally omitted" five words from a sentence of mine which he quoted in order to criticise, I may well leave comments on such a proceeding to your readers. I willingly acquit him of having purposely made me to suggest utter nonsense, as I cannot help thinking that his knowledge of fish-culture was such that he was unaware he was doing so.

As to the second point he says, "I doubted and still doubt if there is any method practised in which layers of moss are used and are separated from the eggs by muslin and similar material." As he rejects the Howietoun account which I gave, I now submit extracts from two standard works, one American, the other English, which will, I believe, be conclusive to those who are ignorant of fish-culture, for every fish-culturist is aware that this plan is commonly adopted. Livingstone-Stone ("Domesticated Trout," ed. 3, 1877) remarked:—"Theodore Lyman recommends placing each layer of eggs in a fold of mosquito netting to keep them from mixing with the moss and so facilitate the unpacking of them. *This is a great improvement. By all means use mosquito netting*" (p. 149). Mr. Andrews, of Guildford, wrote thus in the Badminton Series ("Salmon and Trout," 1885):—"The plan of packing does not vary much with trout breeders. The eggs are placed in alternate layers between moss, and protected by a covering of mosquito netting, muslin, swans' down, calico, or butter cloth, so arranged that the eggs shall not be crushed or escape" (p. 447).

As regards the third point, your reviewer now appears to be convinced that *Salmo namaycush* is a char, as I stated it to be. It must be a matter of regret that he omitted to investigate the foregoing questions prior to authoritatively writing upon them in such a well-known publication as NATURE.

Cheltenham, February 4. FRANCIS DAY.

IN his last letter Mr. Day has certainly proved the correctness of the statement in his book that salmonoid eggs are packed with layers of moss from which they are separated by muslin or other textile fabric. If I had known as much about salmon-culture as he, I certainly should not have questioned the statement; it is to be noted that I only questioned and did not deny. If I had been as completely versed in the knowledge of Salmonidæ as Mr. Day, I should have written a book on the subject instead of reviewing his. But the essential point, which Mr. Day seems incapable of appreciating, is this: that there was nothing in the notes on the subject of packing in his book which confirmed the statement in the text; and although my doubts as to the correctness of that statement are removed by his letter, they were perfectly justifiable in a reader of his book. Mr. Day does not apparently suspect that people interested in the subject, including the reviewer, read his book for the sake of gaining information, and not because they already know as much about the subject as himself. All I had to do was to give my impressions of the book as I found it: the fitness of my criticisms is only the more established by the lengthening appendix to his book which Mr. Day is now publishing in your correspondence columns. YOUR REVIEWER.

MODERN VIEWS OF ELECTRICITY.[1]

PART III. MAGNETISM—(*continued.*)

VIII.

IT will now be perceived that a fly-wheel in rotation is the mechanical analogue of magnetism, or more definitely of a section of a line (or tube) of magnetic force; and that a brake applied to such a fly-wheel, with consequent slip, dissipation of energy, and production of heat, is in some sort a mechanical analogue of an electric current.

The field is regarded as full of geared elastic vortices or whirls, some of which are cogged together, so to speak, while others are merely pressed together by smooth rims. It is among these latter that slip is possible, and in the regions occupied by them that currents exist; the energy dissipated here being transmitted through the non-slippery or dielectric regions from the source of power, just as energy is transmitted from a steam-engine through mill-work or shafting to the various places where it is dissipated by friction.

[1] Continued from p. 348.

Mechanical Force acting on a Conductor conveying a Current.

In Fig. 41 the conducting portion is shown with opposite rotations on either side of it. Now superpose a uniform rotation all in one direction upon this, so as to increase the spin on one side and diminish it on the other. Immediately the extra centrifugal force on one side will urge any movable part of the conductor from the stronger to the weaker portion of the field.

The field for a direct and return circuit may be similarly drawn by superposition of their separate whirls (see Fig. 40); and so it becomes evident why a circuit tends to expand so as to inclose the largest possible area, even if no other magnetic field than its own be acting on it.

Also if two circuits are arranged near each other in a plane, with their currents in opposite directions, they will more or less neutralize each other's effect on the space between them, causing (if equal) a region of no spin there. Their neighbouring portions will thus get urged together by the unbalanced pressure on the other side: or, currents in the same direction attract.

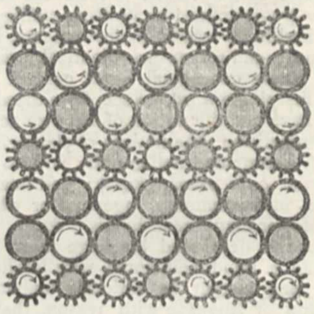

FIG. 44.—Two parallel conductors conveying equal currents in one direction and getting pushed together by the centrifugal force of the outside whirls, no whirl existing between them. The length of the arrows again suggests the distribution of magnetism in the conductors. Fig. 40 showed the correlative repulsion of opposite currents.

As for the effect of iron introduced into a circuit, it brings into the region of space it occupies some two or three hundred times as many lines of whirl as were there before, and these naturally contribute mightily to the effects, both those exhibiting mechanical force and those exhibiting inertia.

When one says, as roughly one may do, that iron brings 300 fresh lines into the field, one means that for every whirl otherwise excited, 300 more are faced round in the iron. And this process goes on while the field is increasing in strength until the total number of whirls in the iron begins to be called upon; when this point is reached the rate of addition is not maintained, and the iron is said to show signs of saturation. Ultimately, if ever all its whirls were faced round, the iron would be quite saturated; but long before this point is reached another cause is likely to make itself felt, viz. the falling off in the strength of the whirls already faced round, by the action of the strong magnetic induction, which is all the time acting so as to weaken the iron currents so far as it is able. And thus at a certain point hitherto unreached by experiment the iron may not only fail to increase the strength of the field any more, but may actually begin to diminish it.

The easiest way to picture the effect of iron is to think of its wheels as some two or three hundred times as massive as those of air, so that their energy and momentum are very great.

That which is commonly called magnetic permeability may in fact be thought of as a kind of inertia, an inertia per unit volume; though how it comes to pass that the ether inside iron is endowed with so great inertia one cannot say. Perhaps it is that the iron atoms themselves revolve with the electricity, perhaps it is something quite different. Whatever the peculiar behaviour of iron, nickel, &c., be due to, it must be something profoundly interesting and important as soon as our knowledge of their molecular structure enables us to perceive its nature.

Induction in Conductors not originally carrying Currents but moving in a Magnetic Field.

To explain the currents induced in a conductor moving through a uniform magnetic field is not quite easy, because none of the diagrams lend themselves naturally and simply to the idea of circuits changing in form or size.

If we take a rigid circuit in a magnetic field, like Fig. 45, and revolve it out of its plane 180°, it is obvious that a current will be excited in it, for the process is essentially the same as if the conductor were kept still and the field reversed.

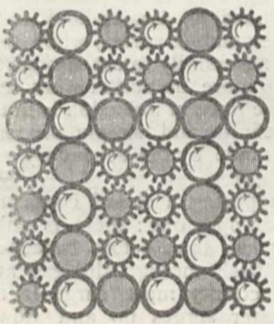

FIG. 45.—Section of a uniform magnetic field with two rails and a slider in it. If the slider be moved to or fro, the wheels inside get initially, compressed or extended, and thereby gain or lose energy respectively thus exciting the state of slip known as induced current.

But to understand the current excited in a closed circuit when a portion of it moves across the lines so as to embrace a greater number of them, one has to take into account the fact that the inside whirls are expanding and doing work in forcing the conductor away, while the outer whirls are resisting the motion, and being thereby compressed and rendered more energetic. Thus the wheels inside revolve slightly slower as the circuit expands, and those outside revolve slightly quicker. Both these processes cause a slipping of the gearing, first all round the inside and then all through the substance of the wire, whereby positive electricity moves forward in one direction round the circuit, the negative moving oppositely; and so a current is accounted for. It is not to be supposed, however, that any finite expansion of the wheels really occurs: the motion is rapidly equalized by diffusion through the wire, and fresh wheels come in round it from outside; hence directly after the conductor has stopped moving the field is again steady, but with many more wheels inside the contour than it possessed at first.

Representation of an Electrostatic Field again, and superposition of it on a perpendicular Magnetic Field.

An electrostatic strain is, we know, caused by a displacement of positive electricity one way along the lines of force, and by an equal displacement of negative the other way. Half the process was indicated crudely in Fig. 6; we may now represent it rather more fully with the help of our elastic cells by Fig. 46.

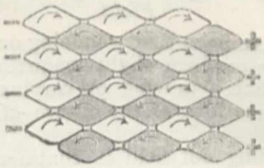

FIG. 46.—A portion of an electrostatic field between two oppositely charged bodies, with its lines of force going from right to left, and showing a tension along and a pressure at right angles to them, due to the elasticity of the cells (which elasticity may be due to their containing fluid in a state of whirl). Magnetic lines of force perpendicular to the paper are also shown in section. While this magnetic field was being excited and propagated from below upwards, a slight strain would be produced in the elastic cells, like but immensely less than that shown; as contrasted with its normal condition (Fig. 37). Conversely, while this electrostatic strain was being produced, the positive whirls would be infinitesimally quickened and the negative ones retarded during the displacement, thus producing a minute magnetic effect. If the medium is not magnetized, the whirls are not necessarily absent, only faced all ways.

Here the positive cells have been pulled one way, the negative the other way; and when the distorting force is removed, the medium tends to spring back to its normal condition, exerting an obvious tension on bodies attached to it in the direction of its lines of force, its elongated direction, and an obvious pressure in all perpendicular directions, its compressed directions.

Now, if all the cells are full of parallel whirls, as in the preceding magnetic diagrams, it is not improbable that this electrostatic distortion or "shear" of the medium may affect its magnetic properties slightly, and that, if the direction of electrostatic strain were rapidly reversed, a small magnetic oscillation would also ensue; but the exact details of these mutual actions are difficult to specify at present.

Disruptive Discharge.

Disruptive discharge may be thought of as a pulling of the shaded cells violently along past the others; the process being accompanied by a true disruption—a sort of electrolysis—of the medium, and a passage of the two electricities in opposite directions along the line of discharge.

Consider the locomotion of any one horizontal row of shaded cells in Fig. 46 during the occurrence of such a disruption of the medium. The cells slide on towards the right, and, as they slide, the spin of the negative cells above them is retarded while that of those below them is accelerated; consequently a true magnetic effect is produced, just like that accompanying a current, and a disruptive discharge has therefore all the magnetic properties of a current.

Effects of a Moving Charge.

This locomotion of a set of positive cells, or of negative cells the other way, as just considered, is very near akin to the motion of a charge through a dielectric medium.

A charge can only exist at the boundary between a dielectric and a conductor, or at least between one dielectric and another of greater density. So, when a charged body moves along with extreme rapidity, it can be thought of as exciting a rotation in the cells most closely in contact with it greater than that which it excites in the opposite kind of cells, and thus produces the whirl proper to a magnetic field. Thus does a moving charge behave just like a current of a certain strength.

It may be, indeed, that this is the customary way of exciting a voltaic current; for the chemical forces in a cell cause a locomotion of charged atoms, and thus set up a field, which, spreading out in the way Prof. Poynting has sketched, reaches every part of the metallic circuit and excites the current there.

Electrostatic Effects of a Moving or Varying Magnetic Field.

Just as we have seen that a moving or varying electrostatic field may produce slight magnetic effects, so one can perceive that a moving or varying magnetic field brings about something of the nature of an electrostatic strain.

For a spreading out field is continually propagating the rotation on from one layer of wheels to the next. If there is any slip, we thus get induced currents, and the rate of propagation is comparatively slow, being a kind of diffusion; but even if there is not any slip, yet, unless the wheel-work is absolutely rigid, the rate of propagation will not be infinite. The actual rate of propagation is very great, which shows that the rigidity of the wheels is very high in proportion to their inertia, but it is not infinite; and accordingly the propagation of rotation is accompanied by a temporary strain. One part of the field is in full spin, another more distant part is as yet unreached by the spin; between the two we have the region of strain, the wheel-work being distorted a little while taking up the motion. Thus does a spreading out magnetic field cause a slight and temporary electrostatic strain, at right angles both to the direction of the lines of force and to the direction of their advance.

Generation of a Magnetic Field. Induction in Closed Circuits.

Picture to oneself an unmagnetized piece of iron: its whirls are all existent, but they are shut up into little closed circuits, and so produce no external effect. Magnetize it slightly, and some of the closed circuits open out and expand, with one portion of them in the air. Magnetize it strongly, and we have a whole set of them opened out into vortex cores, still with the whirl round them, and constituting the common magnetic lines of force. There is no need to think of iron and steel in this connection. In air or any substance the whirls are still present, though much fewer or feebler, and their axes ordinarily form little closed circuits—it may be inside the atoms themselves. But wrap a current-conveying wire round them, and at once they open out into the lines of force proper to a circular current.

Again, think of an iron ring, or a hank of wire as bought at an ironmonger's: wrap a copper wire several times

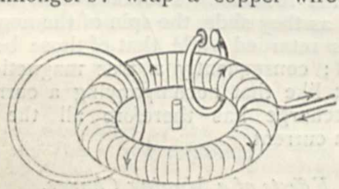

FIG. 47.—Closed magnetic circuit like Fig. 42, with a single-ring secondary circuit, and another open secondary loop; also with a short conducting-rod standing up in it.

round it, as a segment of a Gramme ring is wound (Fig. 47), and pass a current. The closed vortices in the iron at once expand: a portion of each flashes out and across the air-space inclosed by the ring (not by any means confining itself to a plane, of course), and enters the ring on the opposite side; so that directly the current is steady the lines all lie inside the iron again, but now inclosing an area—the area of the ring—instead of being shut up into infinitesimal links. In a sense the iron is still unmagnetized, for its lines of force still form closed contours within it, and none protrude any part of themselves into the air, except for irregularities. But in another sense it is highly and permanently magnetized round and round in itself, the magnetism being not easy to get out of it again, except by judiciously arranged reverse currents.

It is now like one great electric vortex ring instead of like a confused jumble of microscopic ones. Its section was shown in Fig. 42.

During the variable period, while the current is increasing in strength, or while it is being reversed, the region inclosed by the ring and all around it is full of myriads of expanding lines of force flashing across, broadside on, from one side of the iron to the other, and there stopping. It is the presence of these moving lines, changing rapidly from a "simply-connected" into a "multiply-connected" state, or *vice versâ*, which causes the powerful induced currents of "secondary generators."

In every case of varying magnetic field, in fact, we have lines moving broadside on, propagating their whirl, and more or less disturbing the medium through which they move.

Next consider a moving or spinning magnet. Its lines travel with it, and, being closed curves, they also must move broadside through the field, so that in this case we may expect just the same effect as can be obtained from a varying magnetic field.

If a broadside-moving line of force cut across a conductor, its motion is delayed, for its wheels slip and only gradually get up a whirl inside the ill-geared substance; thus, as we know, causing an induced current (see Fig. 43).

If a conducting ring is looped with the iron ring previously mentioned, as a snap-hook is looped with an eye, then every expanding vortex, while the ring is being magnetized, has necessarily to cut through the conducting ring once and no more, no matter what its shape or size. The electromotive force of induction is in this case therefore perfectly definite, and simply proportional to the number of turns made by the secondary round the core of the ring (Fig. 47).

Instead of supposing a closed conducting secondary circuit, imagine an open one: there is still an E.M.F. in it, though rather less than before because a few of the expanding lines flash through the gap and produce no effect, so the electricity must surge to and fro in the conductor as water surges up and down in a tilted trough, and a small condenser attached to the free ends will be alternately charged and discharged. The gap might become so large that nothing is left but a short rod (Fig. 47): in this also similar oscillations would occur.

But now suppose no secondary conductor at all; nothing but dielectric inclosed by the ring. In it there must be an electric displacement excited every time the magnetism of the ring is reversed. It may be an oscillatory displacement, but still on the whole in one direction during rise of magnetism, and in an opposite direction during reversal of magnetism. A charged body delicately suspended within the ring may feel the effect of the minute electrostatic strain so magnetically produced.

To see the mode in which an electrostatic displacement arises in the space embraced by the ring we have only to turn to Fig. 42, and look at the set of wheels along the line A B separating one half the section from the other. They cannot steadily rotate either way, for they are urged in opposite directions by the two halves; in other words, there is no magnetic field near such a ring, as is well known; but, nevertheless, during a change of magnetism, while the whirls inside are changing in speed, the rub on the dielectric necessary for checking the outer wheels of the conductor is either increased or diminished; and if the wheels have any elastic "give" in them, as we know they have, the electrostatic strain in the field is thereby altered during the varying stage of the magnetism.

OLIVER J. LODGE.

END OF PART III.

(*To be continued.*)

THE MECHANISM OF THE FLIGHT OF BIRDS.

THE following is a translation of an article in *La Nature* (December 3, 1887), on the mechanism of the flight of birds, by Prof. E. H. J. Marey. Through the courtesy of the editor of our French contemporary we are able to reproduce the figures illustrating M. Marey's interesting paper.

In a preceding article [see NATURE, vol. xxvi. p. 84], I showed that photography could represent the successive positions of a bird's wing, at different moments in its

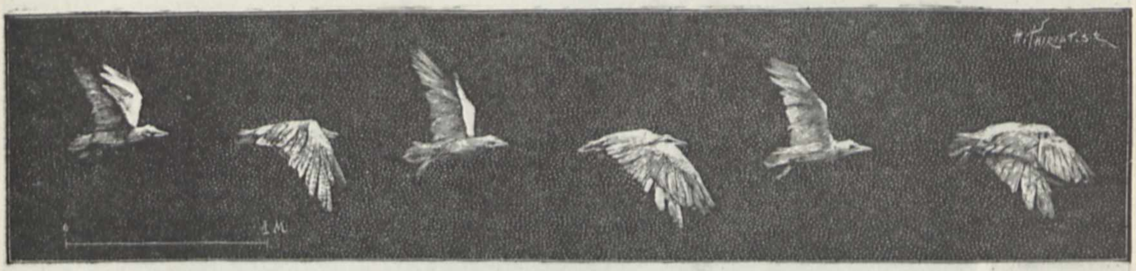

FIG. 1.—Sea-gull. Transverse flight. Ten images per second.

flight; that there might be obtained at the same time the positions of the bird in space at equal and known intervals of time; and I expressed the hope of solving by this method the obscure problem of the mechanism of flight.

Since that time, the photographic method has been perfected, and the number of species of birds to which my researches have extended has been multiplied.

From the comparison of the several species which I have had at my disposal, the results show that, except in certain differences in details, they all execute movements

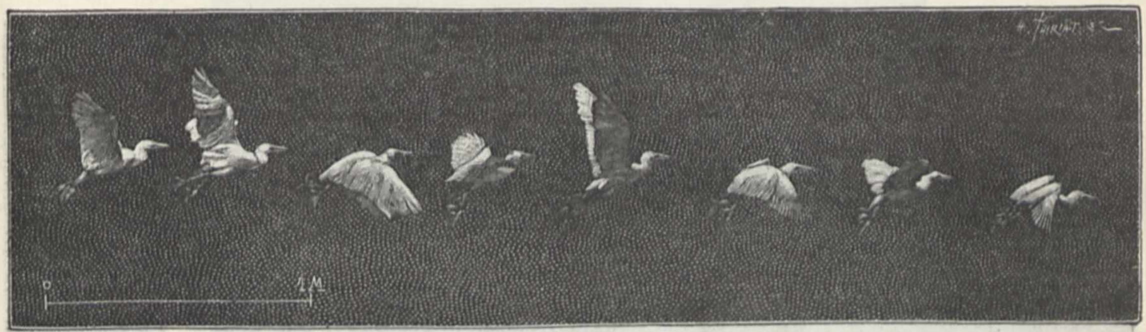

FIG. 2.—Small heron. Transverse flight. Ten images per second.

of the same nature; in all, the wings bend up at the moment of ascension, spread out quickly when at the wished for height, are then lowered, carried in front, and approached to the body; at the close of the descent, the ioints anew bend up, and the ascent recommences.

The illustrations 1, 2, 3, 4, and 5 represent the flight of the sea-gull, the heron, the pigeon, and the pelican.

These illustrations reveal curious attitudes which the eye has not time to seize, and with which we are not familiarized in the artistic interpretations of birds. According

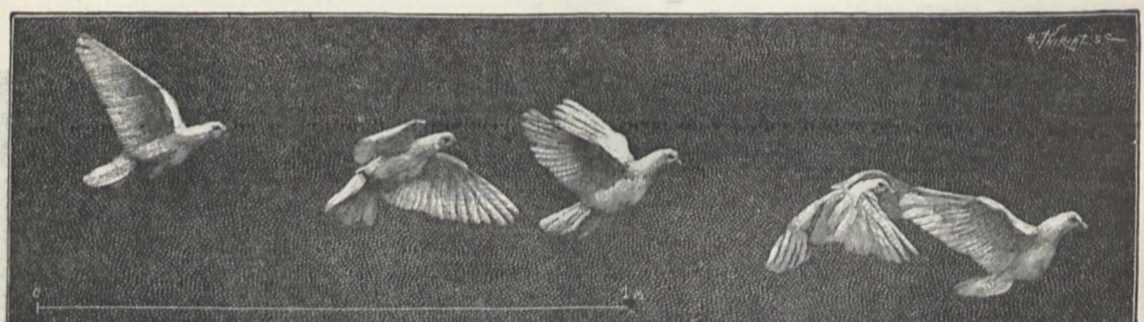

FIG. 3.—Pigeon. Transverse flight. Ten images per second. (Fac-simile of instantaneous photographs taken by the author.)

to a just remark of Mr. Muybridge, the European painters almost always represent birds flying with their wings elevated; the Chinese and Japanese, on the contrary, represent them indifferently with wings both raised and lowered. That does not, however, mean that the artists of the extreme East have faithfully reproduced the different attitudes of birds: the comparison of their representations with those of instantaneous photography shows clearly that no more in China than here does the eye perceive actions which last only for a very brief moment.

Seen only under one aspect, representations of a bird on the wing do not give us correct ideas or the movements of the wings; we must photograph the bird under several aspects in order thoroughly to comprehend this mechanism. We have made several arrangements in order to procure this effect. One of these, placed at a height of 12 metres (nearly $13\frac{1}{2}$ yards), gave representa-

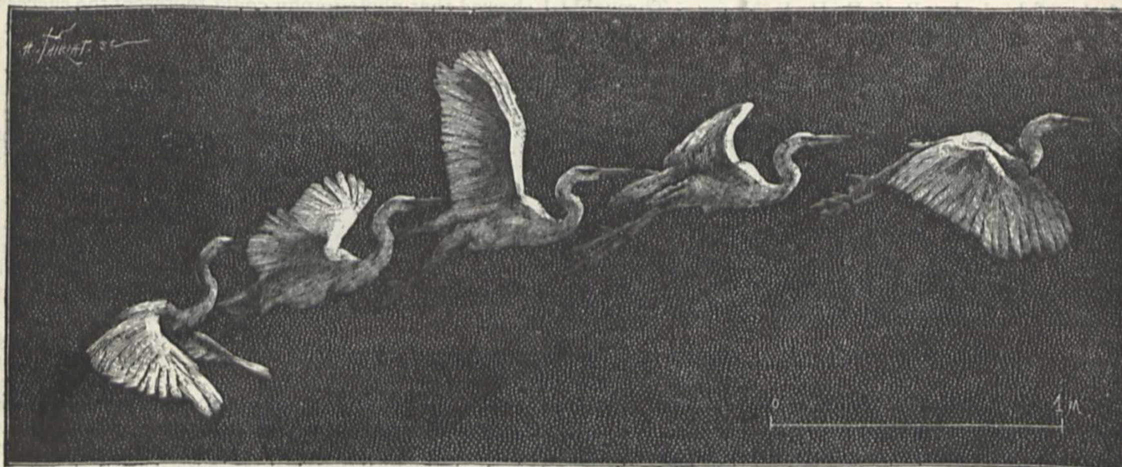

FIG. 4.—Crested heron. Transverse flight. Ten images per second.

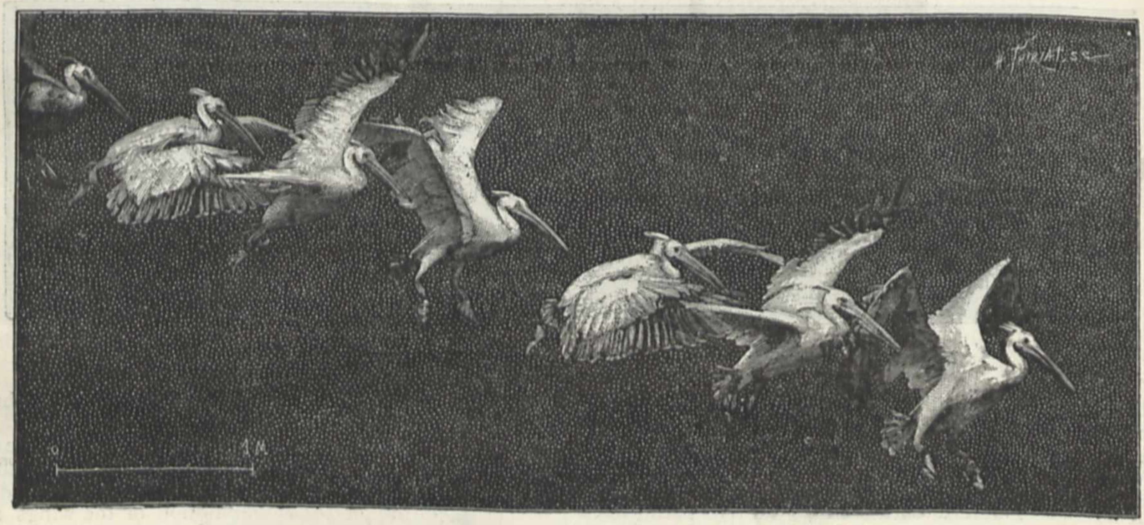

FIG. 5.—Pelican. Transverse descending flight. Ten images per second.

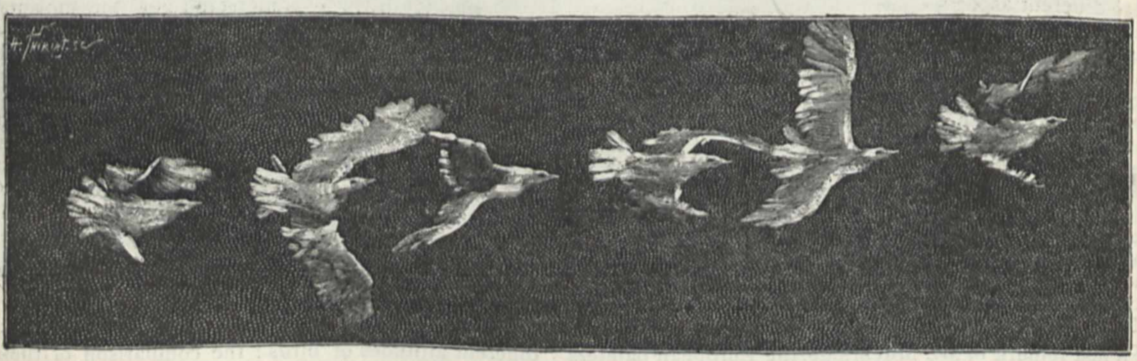

FIG. 6.—Sea-gull seen from above. Ten images per second. (Fac-simile of instantaneous photographs taken by the author.)

tions of the bird as seen from above (Fig. 6); others, variously placed, showed it from the side, or flying in the direction of the photographic apparatus (Fig. 7). These representations, taken under different conditions, complement each other. Thus, the birds seen from above show a singular curvature in the flat surface of the wing, the existence of which one would not suspect from the profile representations. This curvature appears at the end of the depression of the wing, at the moment in which the joints begin to bend upwards in order to prepare for an ascent. Hence results a spiral aspect of the wing, recalling the form which Mr. Pettigrew considers the essential element in a bird's propulsion. But we must observe that this form is only produced at the very close of the act of descent, at the "*point mort*" of the wing's action, as we say in mechanics, and at a moment in which it, having become passive, is about to remount by the resistance of the air. These figures also show a fact wholly unforeseen—namely, that the movements in flying are not symmetrical. It had been previously supposed that the bird, when desirous of turning laterally the direction of its flight, executes movements more extended from the side which is to progress most rapidly; that is to say, that it gives more amplitude to the movements of the right wing if it wishes to turn to the left, and reciprocally. It is scarcely needful to say that photochronography condemns entirely the hypothesis in which it was supposed that one of the wings of the bird could bend more frequently than the other; the movements of the two wings are perfectly synchronous, if not equal, in extent. It is seen, in short, from these representations, that the body of the bird inclines and moves in different ways, so as to carry its centre of gravity to one side or the other, according to the necessities of the equilibrium. The bird whose attitudes are portrayed in Fig. 6 seemed careful to bear the weight of its body to the left on account of the smaller surface of its right wing, from which some feathers were missing.

The representations taken in front and a little obliquely, as in Fig. 7, give also useful information. They show that the extremity of the wing—a part of the organism in full activity, since it strikes the air with greater speed—presents, at the time of lowering, changes of surface which the secondary *rémiges* extending from the carpus to the shoulder do not offer. There exists in the wing feathers of the different orders a species of separation, showing that the carpal articulations are the seat of a light twisting movement favourable to the bending of the surface of the carpal *rémiges*. In these representations may also be readily seen the bending and convergence of the wings at the close of their lowering, the depression which the anterior side of the wing presents at this moment from the effect of a flexion beginning at the elbow. In order to follow in all their details the changes of movement in the wings, it has been necessary to make many experiments, so as to obtain, during a single stroke of the wing, ten or twelve successive views of the bird seen under each of these different aspects.

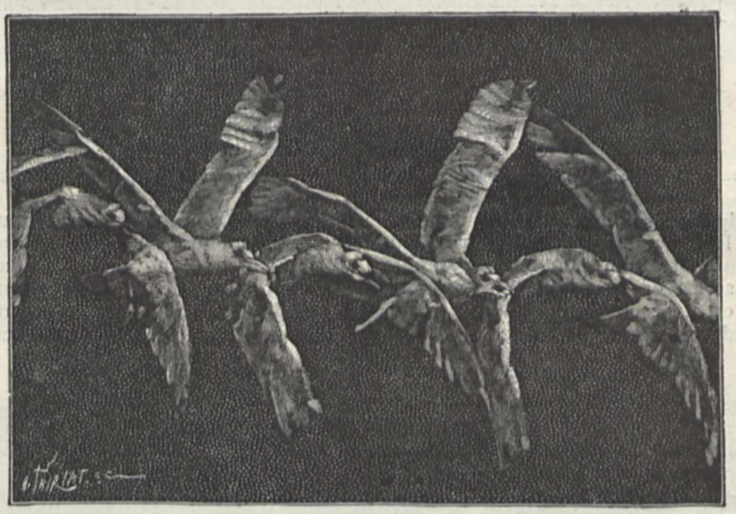

Fig. 7.—Sea-gull flying obliquely in the direction of the photochronographic apparatus. (Fac-simile of instantaneous photograph taken by the author.)

These representations having once been obtained, I was in possession of all the elements necessary to understand completely the motions of the wings according to the three dimensions of space. But in order to represent them, figures in relief were necessary; and circumstances were favourable to this. At Naples, where I then was, the almost lost industry of casting bronze in wax has been preserved from the most remote antiquity. I modelled in wax a series of figures representing the successive attitudes in a single revolution of the wing, ten for the sea-gull, eleven for the pigeon: these models, when given to a skilful moulder, were reproduced in bronze with perfect fidelity.

Fig. 8 represents, disposed in a series, and following each other in their order of succession, at intervals of 1/88 of a second, the phases of one stroke of a pigeon's wing.

These bronze figures were made white, in order to render more apparent the effects of light and shade. Thanks to the multiplicity of the attitudes represented in this series, all the phases of the motion of the wings are easily followed: it is seen how they fold, rise, expand, and sink.

In order the better to understand how the movements of the bird's wing follow each other, of which photochronography gives an analysis, I have had recourse to the use of the zootrope, which recomposes them, and gives to the sight the impression of a bird flying.

The zootrope, represented in Fig. 9, offers this speciality, that it is formed by figures in relief. This is a great advantage from the point of view of the impression which it gives; in fact, these small figures of birds, arranged in a circle in the apparatus, present themselves to the observer under various aspects.

At the beginning of the movement the bird's backs are seen; then, in their circular course, they present their sides, pass across in full view, and at last return to the observer. Besides, the movements of the wings, which in nature are extremely rapid, and consequently imperfectly seen, are here much slower, so that the phases may be easily followed, and in an instant, more may be perceived than the most attentive observer of the flight of birds could discover by the most careful observation.

Fig. 9 shows the arrangement of the zootrope; it cannot unfortunately give an idea of the effect produced by the apparatus in motion.

But it may be said that this rotatory method interprets the movements of the bird without indicating the forces which produces them. While it would be well to know that force, it is better still to measure the mechanical labour expended in order to sustain and transport itself in the air.

Let us see whether our photographic images reveal to us anything in regard to this.

When one knows the mass of a body, and the speed with which it moves, one can calculate the force which has set this body in motion, and the labour expended by this force. If we take a projectile of a certain weight, and throw it before the photochronographic apparatus, and take a series of images of this projectile at intervals of 1/100 of a second, Fig. 10 shows the trajectory curve followed, and the space which separates the images from each other shows the space traversed by the projectile in each of the hundredth parts of a second during which its movement has lasted. From ten to ten a more brilliant image has been produced by an aperture in the diaphragm larger than the others: these marks are useful in order to facilitate the numbering of the images, a fixed metrical scale, photographed at the same time as the object in motion, serves to measure the spaces traversed at each moment; then it is a problem in dynamics, whose solution may be readily obtained by the usual methods of calculation.

The successive images of the flying bird lend themselves to the same dynamical analysis. The balance indicates to us the weight of the bird; we know its size; and in order that photochronography may give us to perfection the trajectory of this mass, it only requires manifold multiplication of the images obtained (a hundred may be taken in a second if need be). But those images

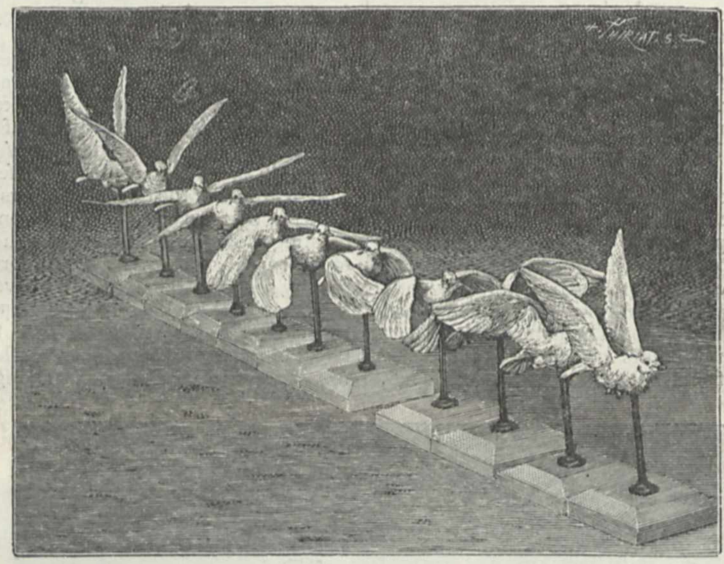

FIG. 8.—Bronze figures representing eleven successive positions at successive moments in the stroke of a pigeon's wing.

FIG. 9.—Zootrope, in which are placed ten figures, in relief, of a sea-gull in the successive positions of flight.

will be partially confused, because the bird, in the hundredth part of a second, only traverses a space equal to the length of its body: the image of the second will therefore partly cover that of the first, the third that of the second, and so on. In this confusion one can scarcely distinguish the moment in which the wing lowers itself, or that in which it is raised. But this is of no importance: we fix on the head of the bird a small but very brilliant metallic point, and the image of this point, clearly seen in the series of figures, reveals the trajectory of the bird, together with its speed, and the accelerations and slackening of speed produced by the movements of the wings. One may then face the dynamic problem of flight. It is granted first that the bird does not oscillate sensibly in the vertical sense, whence one must conclude that the resistance of the air under its wings is precisely equal to its weight. On the other hand, it is to be observed that the motion of the animal presents alternations of speed and slowness, showing that the propelling force and the resistance of the air predominate

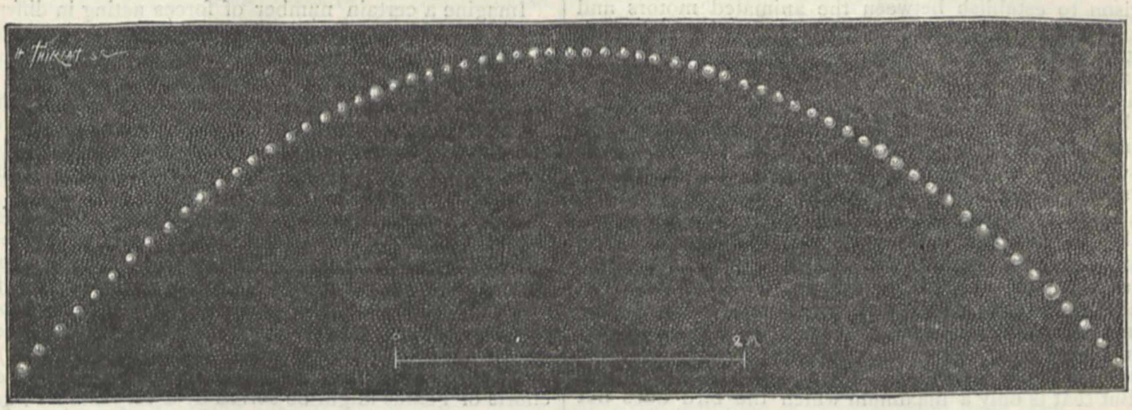

FIG. 10.—Trajectory of a white ball thrown in front of a black screen. The interval between two successive images is measured on the metrical scale. The time taken to travel over this interval is 1/100 of a second.

by turns. From the value of these accelerations there must be deducted the value of the horizontal component of the bird's motion, and that of the resistance of the air.

The calculations based on these experiments have given the following results for the forces which act during the flight of the sea-gull:—

Vertical component	... ...	0·623 kilogramme
Horizontal component	... ...	0·898 ,,
Total	...	1·521 ,,

These forces develop themselves during the act of lowering the wings; the ascent is passive, and is due to the pressure of the air upon the lower surface of the wings, which act then for the support of the bird, as in a paper kite.

As the resistance of the air under the wings acts at a point a considerable distance from the articulation of the shoulder, and as the pectoral muscles, by which the wings are lowered, act very near the articulation—that is to say, on the arm of a very unfavourable lever—it results

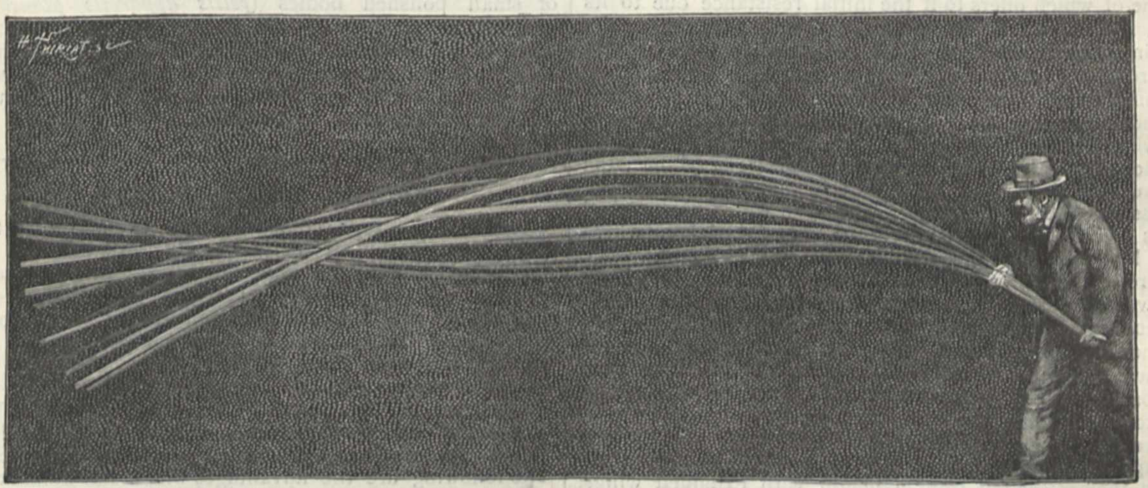

FIG. 11.—Curves and nodes produced by a vibrating stalk, one end of which is fixed. (Fac-simile of instantaneous photographs taken by the author.)

that the effort of the muscles is much greater than the resistance of the air which they surmount. For the pectorals of the sea-gull, the effort developed would be 19 kilogrammes.

It is frequently asked whether the muscles of birds have not a specific strength greater than those of other animals—that is to say, whether two bundles of the same thickness of muscles belonging, one to a bird, the other to a mammal, would have different powers. In the sea-gull which served for my experiments, one transverse section of the pectoral muscles arranged perpendicularly to the direction of their fibres had about 11 centimetres square of surface, or about 1·600 kilogramme per square centimetre. Other birds had formerly given me nearly similar returns for their specific strength; thus, the buzzard developed 1200 grammes per square centimetre, the pigeon 1400 grammes.

Aëronauts hope that they will one day invent a machine capable of transporting man through the air, but many of them are troubled by a doubt; for they ask themselves whether the force of the bird does not exceed that of the known motors. The experiments on that subject may reassure them, for, if we compare the muscular force of the bird with that of steam, we see that one muscle would be comparable to an engine at very low pressure. In fact, the steam which would develop 1·600 kilogramme per square centimetre would scarcely have more than an atmosphere and a half of pressure. But the true comparison to establish between the animated motors and the engines consists in measuring the work which each of these motors can furnish, with equal weight, in the unity of time.

The measure of the work of a motor is obtained by multiplying the effort put forth, by the path which the point of application of that effort traverses. Photochronography expresses at each moment the spaces traversed by the mass of the bird and the displacement of the centre of pressure of its wings, giving thus the factor path in the measure of the work. In this way it is found that for the five strokes of its wing which the sea-gull gives every second, at the moment when it flies away, the labour done would be 3·668 kilogrammes. This calculation is very high; it corresponds to that which an engine would make in raising its own weight to a height of more than 5 metres in a second.

But that is only a maximum which the bird does not attain to except at the moment of flight, when it has not attained much speed. In fact, according as the passage of the bird is accelerated, the air under its wings presents a more resisting fulcrum. I have previously experimentally demonstrated this fact, announced by the brothers Planavergue, of Marseilles, and of which the following is the theory.

When the bird is not yet in motion, the air which is struck by its wings presents, in the first instance, a resistance due to its inertia, then enters into motion, and flies below the wing without furnishing to it any support. When the bird is at full speed, on the contrary, its wing is supported each moment upon new columns of air, each one of which offers to it the initial resistance due to its inertia. The sum of these resistances presents to the wing a much firmer basis. One might compare a flying bird to a pedestrian who makes great efforts to walk on shifting sand, and who, in proportion as he advances, finds a soil by degrees firmer, so that he progresses more swiftly and with less fatigue. The increase of the resistance of the air diminishes the expenditure of labour; the strokes of the bird's wing become, in fact, less frequent and less extended. In calm air, a sea-gull which has reached its swiftest, expends scarcely the fifth of the labour which it had to put forth at the beginning of its flight. The bird which flies against the wind finds itself in still more favourable conditions, since the masses of air, continually renewing themselves, bring under his wings their resistance of inertia. It is, then, the start which forms the most laborious phase of the flight. It has long been observed that birds employ all kinds of artifices in order to acquire speed prior to flapping their wings: some run on the ground before darting into the air, or dart rapidly in the direction they wish to take in flying; others let themselves fall from a height with extended wings, and glide in the air with accelerated speed before flapping their wings; all turn their bill to the wind at the moment of starting.

My experiments have, up to the present, only been able to apply to the flight of departure. In order to study the full flight there are conditions difficult to realize. With a courtesy for which I thank him, M. Eiffel has offered to me on the gigantic tower which he is erecting (at Paris) a post of observation which will leave nothing to be desired. From that enormous height, birds photographed during a long flight will give photochronographic images much more instructive than those which I have hitherto been able to obtain.

Without entering into the dry details of experiments and calculations made,[1] I have aimed at showing that the movements of birds, if they escape the sight, may be faithfully recorded by a new method which is applicable to the most varied problems of rotation and of mechanics.

Photochronography, in fact, gives experimentally the solution of problems often very difficult to solve by calculation.

Imagine a certain number of forces acting in different ways upon a known mass; the complicated way in which those forces are arranged sometimes renders long calculations needful in order to determine the positions which the moving object will occupy at successive moments; whilst if the body itself, submitted to those different forces, can be placed before the photochronographic apparatus, the path which it will follow expresses itself upon the sensitive plate.

Distinguished physicists disputed lately as to the form the free extremity of a vibrating stalk ought to present in which are produced curves and nodes: the greater number of them supposed that between the last node and its free extremity the stalk would present a bent form. Experiment has shown that it is not so, and that the last elements of the vibrating stalk are perfectly rectilinear (Fig. 11).

How many problems whose solution has formerly cost efforts of genius might be solved by a very simple experiment! Galileo in our day would not have needed to lessen the speed of the falling body in order to observe its motion. He would let fall a brilliant ball before a dark field, and would receive from it photographically successive images. Upon the sensitive plate he would have read, in the simplest way possible, the laws of space, of the speed and the accelerations which he has had the glory to discover.

To return to our subject, the laws of the resistance of the air to the living creatures of different forms which move in it ought to be searched into by photochronography. Already interesting results have been acquired: we have been able to determine the path of motion and the speed of small polished bodies (*petits appareils planeurs*) which move freely in the air, and which the eye has not time to follow in their rapid motions. Studies like these, undertaken and methodically carried out, will certainly lead to a comprehension of the still obscure mechanism of the hovering of birds.

[1] See the *Comptes rendus* of the Académie des Sciences 1886–87.

TECHNICAL EDUCATION.

WHEN the time comes for the discussion of the new Technical Instruction Bill, attention will no doubt be given to an important series of resolutions (printed on the next page) which have just been passed by the Executive Committee of the North of England Branch of the National Association for the Promotion of Technical Education. The first six of these resolutions were unanimously adopted by the Committee, and the seventh was, on the motion of Mr. T. Burt, M.P., seconded by Mr. J. H. Girling (President of the Trades Council), adopted with one dissentient. The following are the advantages which the Committee desire to secure:—(1) For primary and secondary education a greater freedom of instruction under the existing code preparatory to technical education in the higher schools. (2) A direct or indirect pecuniary aid for superior education in science and art schools and in Colleges which afford technical education. (3) For all apprenticeship schools or trade classes a supervision by members of the trade, but no Government grant, thus to avoid any objections which might be raised by Trades Unions, or any jealousy arising from an apparent protection of one or more particular trades. (4) For University Colleges a grant similar to that made to training Colleges for education afforded to persons intending to become teachers.

The resolutions are as follow:—

1. That public funds (rates and taxes) should not be employed to meet the current expenses of teaching specific trades.

2. That it is undesirable that instruction in the use of tools should be introduced into primary schools as a grant-earning subject.

3. That with a view to preparing pupils for technical education later on—

(*a*) The grant to day-schools should depend, to a much less extent than at present, on the results of the examination of individual pupils in reading, writing, and arithmetic, and should be largely dependent on the inspector's report of the general character of the teaching and the equipment of the school.

(*b*) There should be greater liberty in the choice of subjects in primary schools, and the same class subject should not necessarily be taken throughout the school.

(*c*) The grant to evening continuation schools should be regulated by the report of the inspector on the character of the teaching, and on the attendance list, and not upon the result of the examination of individual pupils.

4. That when a technical school is combined with a science and art school, the contribution to the building fund, through the Science and Art Department, should exceed £1000, if, in the opinion of the Department, the requirements of the locality demand it.

5. That it is desirable that, when specific trades are taught in technical schools, the practical teaching of each trade should be under the general direction of a committee, consisting mainly of members of that trade; that the teaching should be given in the evening, and be restricted to pupils actually engaged in the respective trades, and that, when specific trades are taught, any deficiency in current expenses should be guaranteed by the trade of the district.

6. That a certain percentage of persons preparing for appointments as teachers in elementary schools should be allowed to attend lectures and laboratory work at Universities and University Colleges, where a curriculum satisfactory to the Education Department is provided, and that the same grant should be made on account of such students as in the case of ordinary training Colleges.

7. That it is desirable that University Colleges in which higher scientific and technological training are combined should be assisted by a Government grant, provided that evening instruction is given in all the subjects taught, at fees which shall bring the advantages of the College within the reach of all classes. The due administration of the grant should be secured by the appointment of certain nominees of the Government on the Executive Council of the College.

THREATENED SCARCITY OF WATER.

THE appendices to the Weekly Weather Reports for the year 1887, recently published by the Meteorological Office, contain some interesting details relative to the rainfall. It is shown that the mean rainfall for the whole of the British Islands during 1887 was only 25·8 inches, whereas the mean for the twenty-two years 1866 to 1887 was 35·3 inches, so that there is a deficiency of nearly 10 inches over the whole area of the British Islands, or 27 per cent. less than usual. In the wheat-producing districts, which comprise the east of England and Scotland, the south of England, and the Midland Counties, the fall during 1887 was 21 inches, and the average value for twenty-two years is 28·5 inches, showing a deficiency in these parts of the Kingdom of 7·5 inches, or 26 per cent. less than usual. In the principal grazing districts, which comprise the west of England and Scotland, as well as Ireland, the fall in 1887 was 30·5 inches, and the value for the twenty-two years is 42·0 inches, showing a deficiency of 11·5 inches, or 27 per cent. less than the average. In the north-west of England the rainfall for 1887 was only 24·9 inches, which is 15·7 inches or 39 per cent. less than the average, and in the south-west of England the fall was 28·3 inches, which is 16·6 inches or 37 per cent. less than usual. Last year was the driest of any year since 1866, and this feature was common to all parts of the United Kingdom; the amount of rain measured was only about one-half of that recorded in 1872, which was the wettest year of the period. If the comparison is confined to the last ten years, the deficiency is nearly as marked, and 1887 is still found to be about 25 per cent. below the average, but the greatest deficiency in this case occurs in the Midland Counties, where it amounts to 36 per cent. of the average. The reports issued by the Meteorological Office for the first five or six weeks of the present year show the deficiency of rainfall still to be augmenting, and even more quickly than in any period last year. In the Midland Counties the rainfall to February 6 was only 0·6 inch instead of 2·9 inches, so that the deficiency from January 3 is as much as 79 per cent. of the average fall; and at Hereford, where the total fall is only 0·29 inch, the deficiency is 90 per cent. of the average. In the east of England the deficiency is 64 per cent., in the south-west of England 61 per cent., and in the north-west of England 58 per cent. There has been a deficiency of rain in all districts of England each week for seven consecutive weeks since December 19, with the exception of a single district (England N.E.) in one week, and since the beginning of October there have been but four weeks in which the excess of rain was at all general. Out of fifty-seven weeks since the commencement of 1887 there have been but ten in the south-west and east of England with an excess of rainfall, and only eleven in the north-west of England. With these facts to hand, there seems reasonable ground for alarm being felt in some localities at the threatened scarcity of water.

CHARLES HARDING.

PROFESSOR ASA GRAY.

WHEN the history of the progress of botany during the nineteenth century shall be written, two names will hold high positions: those of Prof. Augustin Pyrame De Candolle and of Prof. Asa Gray. In many respects the careers of these men were very similar, though they were neither fellow-countrymen nor were they contemporaries, for the one sank to his rest in the Old World as the other rose to eminence in the New. They were great teachers in great schools, prolific writers, and authors of the best elementary works on botany of their day. Each devoted half a century of unremitting labour to the investigation and description of the plants of continental areas, and they founded herbaria and libraries, each in his own country, which have become permanent and quasi-national institutions. Nor were they unlike in personal qualities, for they were social and genial men, as active in aiding others as they were indefatigable in their own researches; and both were admirable correspondents. Lastly, there is much in their lives and works that recalls the career of Linnæus, of whom they were worthy disciples, in the comprehensiveness of their labour, the excellence of their methods, their judicious conception of the limits of genera and species, the terseness and accuracy of their descriptions, and the clearness of their scientific language.

Asa Gray was born in Paris, Massachusetts, on November 18, 1810, and took his M.D. degree when twenty, at the Medical College of Fairfield, Oneida County. His proclivities were all scientific from a very early age, and he is said to have, whilst still a student, delivered lectures on chemistry, geology, and botany, in private establishments of that county. The two former subjects were at first his favourites—indeed, his earliest contribution to science is a paper, by G. B. Crawe and himself, on the mineralogy of Jefferson and St. Lawrence Counties (N.Y.), in *Silliman's Journal* (1834, 346)—but they soon gave place to botany, owing to his having attracted the attention of Dr. John Torrey, State Botanist for New York, and Professor of Chemistry and Botany, but practically of botany only, in the New York Medical College. In 1833, Dr. Torrey made Gray his laboratory assistant, a post he held for some months, during which he presented what was his first botanical paper to the Annals of the New York Lyceum. This, which was on a very intricate and much misunderstood group of American sedges (*Rhynchospora*) showed Gray's acuteness as an observer, and skill in systematizing, as clearly as anything he has since written. In the following year he was appointed Curator of the New York Lyceum, where he extended his studies to the North American grasses and Cyperaceæ, and prepared his first botanical text-book, which was published in 1836, under the title of "Elements of Botany." The "Elements" is a noteworthy book; it was at once accepted as the best that had appeared in the States, and as second to none in the English language; its only rival was Lindley's "Introduction to the Natural System of Botany," the first edition of which had (in 1831) been reprinted under Dr. Torrey's supervision for the use of the American schools.

Whilst still attached to the New York Lyceum, Gray accepted the appointment of naturalist to Capt. Wilke's South Pacific Exploring Expedition, which was then being fitted out; but after two years' delay, and the curtailment of the opportunities for research that were to have been afforded him on the voyage, he threw up the appointment. This result is much to be deplored, for no young naturalist was ever better fitted by education, and by training as an observer and collector, to have taken advantage of the splendid opportunities which that expedition afforded.

Having relinquished the prospect of Pacific exploration, Dr. Gray was invited by his friend Dr. Torrey to co-operate with him in the preparation of a flora of the North American Continent; and into this work, which became the leading object of his scientific life, he eagerly entered. At the same time he accepted the Chair of Botany in the University of Michigan, subject to the condition of being allowed a year's leave to be passed in Europe for the purpose of verifying the nomenclature of the American flora by a study of the species of which the types existed only in European herbaria. This was in 1838, and his first visit was to Glasgow, where the then Professor of Botany (Sir W. Hooker) was engaged on a flora of British North America, under the auspices of the Secretary of State for the Colonies. After a long sojourn in Glasgow, Dr. Gray visited the principal herbaria in London, France, Switzerland, Italy, Austria, and Prussia, making life-long friendships with scientific men of all pursuits wherever he went; his letters of introduction, coupled with his bright intelligence, genial disposition, and charming personality, giving him the *entrée* to *salons* as well as to the museums of every capital. This was the first of seven visits that Dr. Gray paid to Europe, and of which the last was concluded a very few weeks before his fatal illness.

On his return to America in 1839, Dr. Gray resided at New York, when the first volume of the flora of North America was completed, in conjunction with Dr. Torrey, and the second, elaborated wholly by himself, was begun, but not completed till 1843. In the meantime (in 1842) he had been appointed by the Fellows of Harvard College, Cambridge, Fisher Professor of Natural History, the duties of which Chair were restricted to an annual course of lectures on botany, and the charge of the College Botanical Gardens, to which an official residence is attached. This was his home for the rest of his life, and here, with funds partly derived from the College, and partly from private sources, largely supplemented by interchanges of specimens and books, he founded the Harvard Herbarium and Library.

The great desideratum for the conduct of Dr. Gray's new duties was a much fuller class-book of botany than the "Elements" of 1836, and in the same year he completed the first edition of his "Botanical Text-book." In this the subject-matter is divided into two parts, one devoted to structural and physiological botany, the other to the principles of systematic botany, including chapters upon plants useful to man. This was the first of a series of editions of a work that has been for nearly half a century the text-book of schools and colleges throughout the United States, and the latter issues of which have been generally recommended by the botanical professors of the United Kingdom as the best of its class. In 1880 the first volume of the sixth edition appeared, but the advances in botanical science made since the fifth was published, quite a quarter of a century before, had been so many and great that the amount of matter which this sixth will contain is quadruple that of the fifth. It will be when complete a co-operative work in four volumes. The first is by Gray himself, and is devoted to morphology, taxonomy, and phytography; the second, by Prof. Goodale, Gray's able successor in the Fisher Professorship, includes vegetable histology and physiology; the third, by Prof. Farlow, will treat on Cryptogams; and the fourth, which Dr. Gray reserved for himself, was intended to be occupied by the classification of Phænogams, their special morphology, distribution, and products. Gray's other educational works are: "Lessons in Botany and Vegetable Physiology," somewhat on the plan of Lindley's "School Botany," but much fuller; also two smaller works, that for charm of matter and style have no equal in botanical literature—"How Plants Grow," and "How Plants Behave"—they rival chapters in Kirby and Spence's introduction to entomology in instruction and interest; and lastly, "Field, Forest, and Garden Botany."

The great outcomes of Gray's labours in systematic botany are his works on the flora of North America, from the Arctic islands to Mexico, and from the Atlantic to the Pacific Ocean. In one form or another these embrace a great proportion of the 10,000 or 12,000 species which that continent is supposed to contain. More than half are included in the two volumes published in conjunction with Torrey, and in his "Synoptical Flora," of which two parts are published, and in his "Manual of the Botany of the Northern States." The remainder are described or mentioned in more or less detail in multitudes of detached papers, and especially in memoirs upon collections made by naturalists attached to the many Expeditions organized by the Government for the exploration of railway routes across the continent, and by collectors and private individuals in previously unexplored regions. It was the hope of their author that the publication of these collections would have accelerated the completion of the general flora, and such would have been the case had their author lived; but as it is, the stars have in great measure obscured the planet, for one of the greatest obstacles to the study of North American plants is the multitude of these detached memoirs, with complicated titles, in which so many genera and species are to this day buried. If the great work cannot be continued, as it is to be hoped it may be, by Dr. Gray's most competent herbarium keeper, Sereno Watson, it is most desirable that the contents of these memoirs should be reduced to a systematic form with the least possible delay.

Next to the synoptical flora, Dr. Gray's most original work is his "Genera Floræ Boreali-Americana Orientalis," which was intended to contain descriptions, with figures, of every genus of the Eastern States, with discussions upon their affinities, morphology, and distribution. Only two volumes had appeared when want of funds decreed its end. As a fragment it is unique, and had it but been completed it would have been of great morphological value. To have done this would, however, have required more than the ten volumes that were regarded when the work was commenced as sufficient to complete it, and this independently of the Cryptogams.

Nor was Dr. Gray's all closet work: he diligently collected and observed over a considerable area of his native continent; along the Altantic coast from Canada to Florida; in the prairie and Rocky Mountain regions from Wyoming to the borders of New Mexico; in the great basin of Utah and Nevada; and along the Pacific coast from Oregon to St. Barbara.

With two notable exceptions, Dr. Gray confined his descriptive work to North American botany. These exceptions were: one, the fragmentary botany of Wilkes's South Pacific Exploring Expedition, with the execution of which he was intrusted, but which came to an end before it was half finished, for want of funds it is believed, after the publication of one quarto volume with a superb atlas of plates; the other is a memoir on the flora of Japan, founded chiefly on the collections made in that country by the United States North Pacific Exploring Expedition, which in point of originality and far-reaching results was its author's *opus magnum*. By a comparison of the floras of Japan with those of Eastern and Western America, and of these with one another, and all with the Tertiary floras of the Northern States, he drew in outline the history of the vegetation of the north temperate hemisphere in relation to its geography, from the Cretaceous period to the present time. It is a brilliant generalization, bearing the unmistakable stamp of genius.

It remains to allude to Gray's admirable defence of the doctrine of "the origin of species by natural selection," of which he, as one of a favoured few, had been fully informed by Darwin himself in 1857 ("Life and Letters," ii. 120), before it appeared in the Linnean Journal. His opinion, which was, from the first, cautiously favourable, but with reserve, soon ripened into a conviction of the truth of the principles involved. He alludes to it first in the concluding remarks to his essay on the flora of Japan, cited above, published in 1859, wherein he says that he is "disposed to admit that closely related species may, in many cases, be lineal descendants from a pristine stock." Again, in a letter to Mr. Darwin, dated early in July 1860, speaking in terms of highest praise of the "Origin," the following passages occur:—"The moment I understood your premisses, I felt sure that you had a real foundation to hold on. Well, if one admits your premisses, I do not see how he is to stop short of your conclusions, as a probable hypothesis at least." And, referring to his own review of it in *Silliman's Journal* (March 1860), he says:—"It naturally happens that my review of your book does not exhibit anything like the full force of the impression the book has made upon me. Under the circumstances, I suppose I do your theory more good here by bespeaking for it a fair and favourable consideration, and by standing non-committed as to its full conclusions, than I should if I announced myself a convert; nor could I say the latter with truth." It may be remarked here that just at this time a battle over species was raging in America, of which but faint echoes reached our shores. This was over the question of the single or multiple origin of species by creation. Gray was the champion of single creations, and, believing himself strongly supported by theological considerations, may well have felt that the further leap to evolution was one into the dark. Be this as it may, for the five years following the publication of the "Origin," Gray devoted himself to impressing upon the American public his opinion of its extraordinary merits by reviews in weekly and monthly periodicals, by lectures, and by discourses at scientific Academies. Latterly (in 1876) he collected many of these into a single volume which he entitled "Darwiniana." In it he defines his own position "as one who is scientifically, and in his own fashion, a Darwinian, philosophically a convinced theist, and religiously an acceptor of the 'creed commonly called the Nicene,' as the exponent of the Christian faith." From this position he never moved, and he subsequently delivered two lectures in further exposition of these views, at the Divinity School of Yale College. These were published in 1880, under the title of "Science and Religion." Finally, Mr. Darwin, whilst fully recognizing the different stand-points from which he and Gray took their departures, and their divergence of opinion in some important matters, regarded him as the naturalist who has most thoroughly gauged his work, and as a tower of strength to himself and his cause.

As a reviewer and bibliographer, Gray's labours must have been Herculean, and they were uninterrupted for nearly half a century. Even when on his travels in Europe, he was in the habit of contributing scientific notices to periodicals in the States. In 1836 he commenced writing reviews of botanical works, and notices of botanists, travellers, and collectors for *Silliman's Journal of Science and Arts*; and this function he continued to perform without intermission (latterly as a co-editor of that important periodical) till within a few days of his last illness. The number of these articles is truly astonishing, as is the knowledge they display of all branches of botany, Phænogamic and Cryptogamic. They are without exception just, sober, and discriminating, critical rather than laudatory, and eminently considerate in tone where censure is necessary. A selection from these, many being discussions full of original matter and suggestive observations, would be an instructive and acceptable contribution to the botanical literature of the century, and a meet tribute to their author's merits and memory.

Dr. Gray's figure and features were familiar in the scientific circles of this country; but for the information of others it may be stated that he was of spare, wiry figure, rather below the average height, his expression was keen and vivacious, and his movements, like his intellect, alert. He was a Foreign Fellow of the Royal and Linnean Societies, a correspondent of the Institute of France, and of the other Continental Academies, a Doctor of Laws of Oxford, Cambridge, and Edinburgh, and had served as President of the American Academy of Arts and Science, of the American Association for the Advancement of Science, and as a Regent of the Smithsonian Institution. Accompanied by Mrs. Gray he spent the summer of 1887 in Europe, chiefly in England, returning to Cambridge in September. In October he went to Washington on the affairs of the Smithsonian Institute. Soon after his return, on the 28th of November, he was struck with paralysis, from which he never rallied, and he died at the end of the following January. It is characteristic of him that his last letter, written in pencil immediately before his seizure to the contributor of these lines, was on the subject of a review for *Silliman's Journal* of Planchon's "Review of the Vines." Dr. Gray married in 1848, Jane, daughter of Judge Charles G. Loring, of Boston, who survives him. He left no family. An excellent medallion likeness of him in bronze was, on his seventy-fifth birthday, presented by his friends to Harvard College, Cambridge, U.S.

J. D. H.

NOTES.

ON Tuesday evening a question was asked in the House of Commons, by Mr. Howorth, about the new regulations for the entrance examination at Woolwich. Mr. Howorth inquired whether these regulations were final and permanent, or only temporary. Mr. Stanhope, we regret to say, replied that the regulations are intended to be of permanent application. If that be so, it is the more necessary that a vigorous protest against the scheme should be made by those who have any true appreciation of the place which properly belongs to the study of science in education.

ON Tuesday the Committee of the Athenæum Club elected three new members in accordance with the rule which empowers the election of nine persons annually "of distinguished eminence in science, literature, or the arts, or for public services." The names of two of the new members are familiar to students of science—Major-General Donnelly, R.E., C.B., Secretary to the Department of Science and Art; and Prof. G. Carey Foster, F.R.S.

ONE of the leading native residents of Bombay, Sir Dinshaw Petit, has just given the Bombay Government a property valued at 300,000 rupees for the establishment of the proposed Technical Institute of Bombay.

WE regret to announce the death of Dr. Maximilian Schmidt, an eminent geologist and Director of the Zoological Gardens at Berlin. He was born at Frankfurt in 1834, and died at Berlin.

EMILE ROUSSEAU, the well-known French chemist, died on the 4th inst., at the age of seventy-three.

THE annual general meeting of the Fellows of the Royal Horticultural Society was held on Tuesday, Sir Trevor Lawrence, President of the Society, in the chair. The special Committee appointed to inquire into the working of the Society recommended that premises in No. 111 Victoria Street should be secured, and that for shows and meetings the Society should hire the drill-hall of the London Scottish Rifle Volunteers. The Committee also reported that they had under consideration "the construction of new by-laws intended to facilitate the carrying out of as complete a horticultural policy as possible—one in which all aspects and departments should be considered to the undue preponderance of none; but to the general advantage of all." After some discussion the Committee's recommendations were adopted. Several members of the Council having placed their resignation in the hands of the Fellows, it was resolved on the motion of Mr. Wilks, seconded by Mr. Veitch, to decline to accept their resignation, and they were then all formally re-elected, including Sir Trevor Lawrence, who was re-appointed President.

THE Calendar and General Directory of the Science and Art Department for 1888 has been issued.

THE curious fact was some time ago brought to light by Nahrwold that solid particles are ejected from a platinum wire glowing under the influence of an electric current, and form a metallic incrustation upon the walls of a glass tube by which the wire is surrounded. The cause of the emission of these solid particles of platinum has, however, until recently, remained a complete mystery. In the number of the *Annalen der Physik und Chemie* just received will be found an interesting paper by Dr. Alfred Berliner, who, in the course of a series of experiments upon the occlusion of gases by platinum and palladium, has discovered the source of this singular phenomenon. Thin strips of platinum, before being charged with the gas under experiment, were inclosed in a narrow glass tube, and freed from all occluded gas by being heated to redness, *in vacuo*, by the passage of a constant electric current for several hours. At the expiration of this time the metallic incrustation was invariably found when occluded gas had been evolved. On charging the strips with various quantities of any particular gas, the amount of incrustation formed after the complete expulsion of the gas in each experiment was found to vary in the same proportion. Hence it appears pretty clear that the evolution of gas is necessary for the emission of solid particles. This result is strongly confirmed by the fact that palladium, which has such a remarkable power of occluding gases, produces a similar incrustation much more readily and at a lower temperature. It appears probable that the action is merely mechanical; that we have, in fact, an immense number of microscopic volcanoes or solfataras, evolving the occluded gases with such energy that portions of the crater walls are detached and carried away by main force, like their brethren on the large scale, the scoriæ and lapilli, to distances very considerable in comparison with the size of the vents.

THE next meeting of the French Association for the Advancement of Science will be held in Oran from March 29 to April 3. Interesting excursions will be made to Biskra and other places.

AT the meeting of the Meteorological Society of France, on December 20, M. E. Lemoine was elected President for the ensuing year. M. Renou made a communication on the relations which exist between the annual number of solar spots and thunder-storms in various places, and concluded that the works hitherto published were far from sufficing to show any direct dependence between the two phenomena. At the meeting of January 11, M. Janssen, the retiring President, expressed his opinion of the necessity of organizing meteorological stations on a uniform plan at a certain number of stations, under the superintendence of professional and paid observers on whom a definite programme could be imposed, instead of having volunteer observers; this view was also supported by M. Renou. The latter gentleman also spoke of the importance of adopting a uniform cloud nomenclature, and announced that he would shortly present to the Society a work upon this subject. The general secretary presented a note from M. Garrigou-Lagrange on a new electrical anemometer giving the direction of the wind and the horizontal and vertical components of its force (see NATURE, November 3, 1887, p. 18), and recommended its adoption at some coast stations.

THE Council of the Royal Meteorological Society, 30 Great George Street, Westminster, S.W., have appointed a Committee to collect observations on British hail- and thunder-storms from volunteer observers. The objects which they hope to attain thereby are:—(1) A knowledge of the nature and causes of the different kinds of thunder-storms, their attention having been specially called to the subject by the great loss of life and property during the past summer. (2) A discovery of the localities where hail and thunder are most frequent and destructive. (3) If possible, to obtain an increased power of forecasting hail and thunder, whereby they hope that eventually damage to persons, stock, and property might be lessened. Forms and instructions will be sent to intending observers.

WE have received a "Brief Sketch of the Meteorology of the Bombay Presidency in 1886-87." It is by Mr. F. Chambers, Meteorological Reporter for Western India. Mr. Chambers points out that the meteorology of the year 1886-87 in the Bombay Presidency presents several features of special interest. There was a decided reappearance, for some time, of almost exactly the same unfavourable meteorological conditions which characterized the year 1877, when the rainfall in many parts of the Presidency was disastrously deficient. Fortunately, these unfavourable conditions did not last long enough to produce distress, for although a prolonged break in the rains caused considerable anxiety for a time, an excessive fall of rain late in the season brought relief, and on the whole the year was a favourable one.

A SEVERE earthquake occurred in Grenada on January 10. A rumbling noise was immediately followed by a slight shock and gentle lateral oscillations. Then came a very violent shock and vertical undulating oscillations. These were succeeded by gentle oscillations. The shock is supposed to have lasted from twenty to thirty seconds. Several houses in the town of

St. George's were so much damaged that they cannot be safely occupied. The walls of the St. David's Roman Catholic Church are so seriously injured that they will have to be taken down in some places and rebuilt. The sacristy was all but demolished; the basement wall of the presbytery was thrown down, and the building itself had to be propped up with posts. The Court House in the same parish sustained considerable injury. The tower of the Anglican Church in Grenville was cracked. During the succeeding week several mild shocks of earthquake were experienced in the island, the strongest of which occurred between 7 and 8 o'clock on the night of Sunday, January 15.

DR. FORBES WATSON'S collection of commercial products, which was lately offered to the University of Aberdeen at the comparatively small price of £250, has been bought by Dr. Carnelley and his father, and presented to University College, Dundee. Prof. D'Arcy W. Thompson, at whose suggestion the purchase was made, writes to the *Dundee Advertiser*: "This excellent gift puts us at once in possession of a museum which is first-class of its kind, and of which town and College should be proud for ever. Dr. Forbes Watson is well known to many of the older men in Dundee for his knowledge of jute and all other commercial fibres. His works are standard on the subject. His collection was amassed with unrivalled opportunities and the highest technical skill. Great part of it was brought together as an official duty for the India Museum, and was presented by the Department to Dr. Forbes Watson when that Museum was broken up. It contains nearly 7500 samples. Between 700 and 800 of these are fibres, including textiles and paper-making materials. There are over 500 dyes and dye-stuffs, 500 oils and oil-seeds, 600 or 700 gums, resins, and guttas, nearly 2000 medicinal substances (may they be useful to us in the future), and more than as many samples of food-stuffs. The bulk of the collection is stored in bottles, filling fourteen cabinets, and there are also stands and cases for the display of specimens. Altogether the cases and bottles in which this great collection is stored represent a cost greater than the price which Dr. Forbes Watson now asks and receives."

THE Batavia *Niewsblad* announces that the Government has decided upon establishing a bacteriological laboratory in that town. An institution for the pursuit of that special branch of study will be built immediately the funds for the purpose become available. The existing laboratory arrangements will be improved and extended, so as to admit of pathological and bacteriological investigations.

WE are glad to learn from the *American Naturalist* that the project of a Marine Biological Laboratory on the New England coast is not languishing. Several thousand dollars have already been subscribed towards the erection of the necessary building and its equipment and maintenance. The Committee on the Laboratory have arranged a course of eight lectures, the proceeds of which are to be added to the fund.

LIEUT. NIBLACK, U.S.N., has returned to Washington from a three-years' voyage to Southern Alaska, where he has been engaged on coast-survey duty. He has brought with him many photographs and objects which will be of interest to students of ethnology and anthropology. He devoted special attention to the totem posts of Southern Alaska. He says that in that country winter is the best season for ethnological studies. The natives are then at home, whereas in the summer they are often far inland.

ASSISTANT CHARLES A. SCHOTT, assistant in charge of the Computing Division of the United States Coast and Geodetic Survey, has addressed a letter to the Superintendent of his Bureau about the recent discovery, by Assistant G. Davidson, of records of the magnetic declination, A.D. 1714. He says that these records greatly increase our knowledge of the secular variation of the declination. *Science* gives the following account of the substance of M. Schott's letter:—"By means of these observations we are able to improve materially the expressions for San Blas and Magdalena Bay, to add the new station Cape San Lucas, and to make their influence felt as far north as San Diego and Santa Barbara. It is the range which is greatly improved; besides, the epoch of maximum declination is shifted in the right direction. Apart from the fact that a region of west declination is here for the first time observationally indicated on the Pacific coast, the power of the newly recovered declinations is due to the circumstance, that, as far as known, they cover a time when the needle was in or near a phase the opposite of the present one. For want of early observations, those previously collected for San Diego and Santa Barbara, Cal., were extremely difficult to handle; and, while it was not an easy matter to establish new expressions for these stations, their correctness, or rather applicability over the whole period of time the observations cover, is quite reassuring. He points out the desirability of new observations (either using funds yet available before July next, or providing funds to be used after that date) at San Diego, Santa Barbara, and Monterey, and states that these stations have received no attention for seven years. These observations are demanded to give greater precision to the computed variations on our charts."

A NUMBER of American geographers, all belonging to Washington, have incorporated the American National Geographical Society for a term of 100 years. The principal objects of the Society are to increase and diffuse geographical knowledge, to publish the Transactions of the Society, to publish a periodical magazine and other works relating to the science of geography, to dispose of such publications by sale or otherwise, and to acquire a library under the restrictions and regulations to be established by its by-laws.

THE Commissioners of Public Schools of Baltimore, Md., deserve much credit for the efforts they are making to secure for the schools under their care important reforms which have always been advocated by students of sanitary laws. They lately resolved that the Mayor and City Council of Baltimore should be requested to authorize them to appoint an officer, to be known as the Sanitary Superintendent of Public Schools, whose duty would be: (1) to carefully examine all plans submitted for the construction of new school-houses, and suggest such modifications as may be necessary from a sanitary point of view; (2) to advise with the Commissioners with reference to necessary alterations in school-buildings to improve their hygienic condition; (3) to examine all text-books before adoption, in order that type, printing, or paper injurious to the eyesight of pupils may be avoided in selecting such books; (4) to satisfy himself, by personal examination if necessary, that all pupils admitted to the schools have been properly vaccinated, or are otherwise protected against small-pox; (5) to take such other measures, in conjunction with the Health Commissioner of the city, as may be necessary to prevent the spread of contagious diseases in, or through the medium of, the public schools; (6) to examine annually the eyesight of all children attending the public schools, and keep an accurate record of such examinations; (7) to report annually, or as often as may be required by the Commissioners, upon the sanitary condition of the schools, and of the pupils attending them, and to advise the Commissioners upon sanitary questions connected with schools whenever required; (8) to give instruction, by lectures or otherwise, to the teachers in the schools upon the elementary principles of school hygiene.

WE have received the second volume of the Transactions of the Meriden (Conn., U.S.A.) Scientific Association. It contains, among other things, a valuable list, drawn up by Mr. Franklin Platt, of the birds of Meriden.

A THIRD edition of Mr. John Venn's "Logic of Chance" (Macmillan) has just been issued. The work has been re-written, but the author explains that the alterations he has made do not imply any appreciable change of view on his part as to the foundations and province of probability. In the preface Mr. Venn mentions that he is engaged in preparing a work on inductive logic.

THE scientific writings of Jean Méry (1645–1722) have been brought together in a work edited by M. L. H. Petit, Assistant Librarian to the Medical Faculty of Paris. The work contains many contributions to biology which have not hitherto been generally known.

TOME III., Cahier I, of the "Mémoires de la Société des Sciences Physiques et Naturelles de Bordeaux," is devoted to a full bibliography of the Γ function.

WE have received the *Annuaire de l'Observatoire Royal de Bruxelles* for 1888, this being the fifty-fifth year of its publication. It contains the usual astronomical tables and data for the current year, and a mass of meteorological, geographical, financial, and other statistics. There is a complete list of the asteroids and comets discovered in the past year, with the elements of their orbits, and there is also an account of the state of solar activity in 1886. The tables of units and physical constants have been considerably extended, and a detailed account of electrical and magnetic measurements has been added. In addition there are, as usual, several scientific papers by the officials of the Observatory. M. Folie gives an account of his further investigations into the movements of the earth's axis, and in a subsequent article M. Niesten applies the correction for diurnal nutation to the various and widely differing determinations of the annual parallax of γ Draconis; the value obtained is $+0''\cdot086$. The important series of barometric observations, extending over a period of fifty years, are discussed at great length, and illustrated by diagrams.

MR. JOHN HEYWOOD, of Manchester and London, has issued a little book called "Flower-Land," by the Rev. R. Fisher. It is written in a simple style, and will no doubt be useful as an introduction to botany for children.

THE Perthshire Society of Natural Science has begun to issue Transactions and Proceedings. We have received the first part of the first volume. It contains notes on some rare birds lately placed in the Society's Museum, by Colonel H. M. Drummond Hay; mnium riparium, by R. H. Meldrum; some localities for Perthshire plants, by R. H. Meldrum; origin of the interbedded and intrusive volcanic rocks of Kinnoull Hill, by H. Coates; and the flora of the Woody Island, by W. Barclay.

MESSRS. KEGAN PAUL, TRENCH, AND CO. will publish immediately, in the "International Scientific Series," Sir J. William Dawson's new book entitled "The Geological History of Plants."

"A HISTORY OF PHOTOGRAPHY," by Mr. W. J. Harrison, will shortly be published. It is intended to serve as a practical guide to photography, and as an introduction to its latest developments.

A WORK containing a full account of the volcanic eruptions for the last sixty-four years on the island of Hawaii has been printed and will shortly be published in the city of Honolulu. The author is Mr. William Lowthian Green, at present Prime Minister of the kingdom of Hawaii, whose work on "The Vestiges of the Molten Globe" appeared in England some years ago, and has since attracted the attention of M. Lapparent and other Continental geologists. Mr. Green's new work will contain a complete tabular statement of the eruptions, and a map of the island of Hawaii.

MR. EDGAR THURSTON, Superintendent of the Government Central Museum, Madras, has printed a list of fishes obtained during a residence of three weeks at Rámeswaram Island, which is separated on the one hand from the Indian continent, and on the other from the Island of Manaar, by an interrupted ridge of rocks known as Adam's Bridge. The fish-fauna of the coral reefs of this island stands out in striking contrast with that of other places on the Madras coast. "Coral Fishes," *i.e.* brightly coloured fishes—*Chætodon*, *Platyglossus*, *Heniochus*, *Pseudoscarus*, &c.—abound round the reefs, and feed either on the small delicate marine Invertebrata which swarm on the living corals, or, if their teeth are adapted for the purpose, on the hard calcareous substance of the corals. The bright colouring of the fishes is explained by Mr. Thurston on the well-known principle that "the less the predominant colouring of any creature varies from that of its surroundings, the less will it be seen by its foes, the more easily can it steal upon its prey, and the more it is fitted for the struggle for existence." Conspicuous by their abundance are several species belonging to the family Sclerodermi, including *Balistes* (file-fish), whose jaws are armed with teeth well suited for breaking off pieces of hard coral, or boring holes into the shells of the Mollusca, on the soft parts of which they feed. The file-fishes are said to destroy an immense number of mollusks, thus becoming most injurious to the pearl-fisheries. Present, too, in great numbers are several species of the family Gymnodontes: *Tetrodons* (globe fishes), including the beautifully marked little *Tetrodon margaritiferus*, and *Diodons*, which have a very bad reputation among the natives as being very poisonous.

IT is generally supposed that the Ainos of Yezo are amongst the disappearing races of the earth, and that they are "fast dying out," as the phrase usually runs. This appears to be an error, for according to a communication on the subject in the *Japan Weekly Mail*, from Mr. Bachelor, during the past fifteen years there has been little, if any, diminution in their number, which he puts down, so far as the Island of Yezo is concerned, at from 1300 to 1600 souls. Actual detailed statistics respecting the numbers of the Ainos do not appear to be given in the Japanese censuses, but official statistics do exist for certain Aino settlements since 1871, which may be taken as an index. These show an increase of 129 persons in sixteen years, although, by a careful examination of the data, it appears that one village not included in the earlier was given in the later years. In 1871, there were 665 males, 639 females, and 260 huts; in 1886, the numbers were 691, 742, and 318 respectively. These figures, making every reasonable allowance, show at least that there is good ground for doubting whether the Ainos are dying out, in Yezo at least, as rapidly as it is the fashion to assume that they are.

THE seventh annual meeting of the members of the Sanitary Assurance Association was held at their offices, 5 Argyll Place, Regent Street, on Monday. Prof. Roger Smith presided, and expressed his satisfaction at the continued prosperity of the Association.

IN the footnote in NATURE, December 15, 1887, p. 152, second column, line 5 from foot, *for* Careton *read* Cureton.

THE additions to the Zoological Society's Gardens during the past week include a Common Boa (*Boa constrictor*) from South America, a Royal Python (*Python regius*), two West African Pythons (*Python sebæ*) from West Africa, an Indian Python (*Python molurus*) from India, presented by Mr. Leopold Field; a Griffith's Fox (*Canis griffithi*) from Persia, two Cockateels (*Calopsitta novæ-hollandiæ*) from Australia, deposited; a Pluto Monkey (*Cercopithecus pluto*) from West Africa, an Antarctic Skua (*Stercorarius antarcticus*) from the Antarctic Seas, received in exchange.

OUR ASTRONOMICAL COLUMN.

MELBOURNE OBSERVATORY.—The Annual Report of this Observatory, dated August 14, 1887, states that the buildings and equipment of the Observatory were in good condition with the exception of the mirrors of the great Cassegrain reflector, which had become so dull as materially to interfere with the observation of the fainter nebulæ. It was proposed to substitute mirror A, the less tarnished of the two, for mirror B, now in the telescope, and either to have B repolished on the spot or to send it to Dublin to be re-polished under the care of Sir H. Grubb. The new transit circle was in excellent order, and 2487 right ascensions and 1301 polar distances had been observed during the year. Eighty-seven southern nebulæ had been examined with the great reflector, and four searched for, but not found. The use of the photo-heliograph, which had been altered in July 1886, so as to take pictures on a scale of 8 inches to the solar diameter, had been much interfered with by bad weather, and only 121 photographs had been secured. The principal fresh work proposed for the Observatory was the co-operation in the photographic survey of the heavens; the Victorian Government having consented to the Observatory joining in that undertaking, and having placed £1000 on the estimates of the current year towards the necessary expenditure.

THE AMERICAN NAUTICAL ALMANAC OFFICE.—The Report of Prof. Newcomb, Superintendent of the Office, for the year ending 1887 June 30 has recently appeared. From this we learn that the printing of the several Nautical Almanacs published by the Office fell a little into arrear in 1887, the printing of the American Ephemeris for 1890, which should, according to custom, have appeared in June, not being quite ready in October. The computations for the following years were in their usual state of forwardness. The principal part of the Report deals with the new tables of the planets on which Prof. Newcomb and his assistants are engaged. The work is divided into four sections—viz.: (I.) The computation of the general perturbations of the planets, the work now in hand relating to those of the four inner planets; on twelve of the fourteen pairs of planets which come into play in this part of the undertaking, the work has already been completed. The incomplete perturbations are those of Venus and Mars by Jupiter. (II.) The re-reduction of the older observations, and discussion of the later ones, with a view of reducing them all to a uniform system. In this section Maskelyne's Greenwich observations from 1765 to 1811, and Bradley's, 1750 to 1762, have been already reduced, the latter by Dr. Auwers. Airy's Greenwich observations, the Paris observations from 1800 as reduced by Leverrier, and Bessel's Königsberg observations, will need no discussion except that necessary to reduce them to the adopted standard system. The re-reduction of Piazzi's Palermo observations, 1791–1813, is in hand, but it is not yet decided as to whether Taylor's Madras observations should be included. (III.) The computation of tabular places of the planets from Leverrier's tables up to the year 1864—the most laborious and difficult part of the work—is in a fairly advanced state. (IV.) The final discussion of the results. Prof. Newcomb estimates that the equations of condition for correcting the elements of the four inner planets will be ready for solution towards the end of 1889, but they will involve extended discussion and comparison in order to get the results in the final form for publication. Of the work on the four outer planets, Uranus and Neptune are yet untouched; but Mr. Hill's new theory of Jupiter and Saturn is in the hands of the printer, and Mr. Hill is now engaged in the construction of the tables and ephemerides for finally correcting their elements. In connection with the lunar theory, the principal work on hand is the comparison of Hansen's tables with observed occultations since 1750. Another branch of the planetary work is the determination of the mass of Jupiter from the motions of Polyhymnia: the perturbations of the planet have been computed from its discovery in 1850 to October 1888, and the work awaits the observations during the approaching opposition to be brought to a final discussion.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *FEBRUARY* 19–25.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on February 19

Sun rises, 7h. 9m.; souths, 12h. 14m. 6·2s.; sets, 17h. 19m.: right asc. on meridian, 22h. 9·7m.; decl. 11° 22′ S. Sidereal Time at Sunset, 3h. 15m.

Moon (at First Quarter February 20, 2h.) rises, 10h. 25m.; souths, 17h. 46m.; sets, 1h. 18m.*: right asc. on meridian, 3h. 42·8m.; decl. 14° 34′ N.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.	
	h. m.	h. m.	h. m.	h. m.	° ′
Mercury..	7 29	13 17	19 5	23 13·3	3 7 S.
Venus.....	5 39	9 49	13 59	19 44·5	20 46 S.
Mars......	22 38*	3 57	9 16	13 51·7	8 43 S.
Jupiter....	2 3	6 17	10 31	16 12·1	20 12 S.
Saturn....	14 20	22 16	6 12*	8 13·4	20 29 N.
Uranus...	21 36*	3 9	8 42	13 3·3	6 0 S.
Neptune..	10 5	17 45	1 25*	3 41·7	17 56 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

Feb.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.	
			h. m.	h. m.	°	°
20	Aldebaran	1	15 56	near approach	346	—
21	119 Tauri...	5½	19 10	near approach	7	—
24	*d′* Cancri...	6	21 30	22 28	112	226

Feb.	h.	
23	1	Mercury stationary.
24	20	Saturn in conjunction with and 1° 22′ north of the Moon.

Variable Stars.

Star.	R.A.	Decl.			
	h. m.	° ′			h. m.
U Cephei	0 52·4	81 16 N.	Feb.	19,	19 38 *m*
			,,	24,	19 17 *m*
R Arietis	2 9·8	24 32 N.	,	22,	*M*
R Tauri	4 22·2	9 55 N.	,,	21,	*M*
R Leporis	4 54·5	14 59 S.	,,	23,	*M*
R Canis Majoris...	7 14·5	16 12 S.	,,	21,	20 26 *m*
			,,	22,	23 42 *m*
δ Libræ	14 55·0	8 4 S.	,,	22,	1 58 *m*
U Coronæ	15 13·6	32 3 N.	,,	21,	21 51 *m*
U Ophiuchi	17 10·9	1 20 N.	,,	19,	3 2 *m*
		and at intervals of			20 8
X Sagittarii... ...	17 40·5	27 47 S.	Feb.	19,	2 0 *M*
W Sagittarii ...	17 57·9	29 35 S.	,,	19,	4 0 *M*
Z Sagittarii... ...	18 14·8	18 55 S.	,,	25,	1 0 *M*
β Lyræ...	18 46·0	33 14 N.	,,	24,	0 0 *M*
S Sagittarii	19 12·9	19 14 S.	,,	24,	*M*
δ Cephei	22 25·0	57 51 N.	,,	20,	2 0 *M*
			,,	23,	20 0 *m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
	°	°	
Near β Trianguli... ...	30	35 N.	February 24.
From Canes Venatici ...	181	34 N.	February 20.
Near δ Serpentis	234	11 N.	Swift; streaks.
,, π Herculis	262	36 N.	Feb. 20. Swift.

GEOGRAPHICAL NOTES.

AT Monday's meeting of the Royal Geographical Society, the paper read was by Mr. Randle F. Holme, on Labrador, which he visited in July–October of last year. Mr. Holme succeeded in penetrating into the heart of Southern Labrador, as far as Lake Waminikapou, and not far from the Grand Falls, which Mr. Holme believes will turn out to be the greatest falls in the world; but, as General Strachey pointed out in the discussion, Mr. Holme's conception of the height is probably exaggerated. Mr. Holme went from Newfoundland to Bonne Espérance on the south-east coast of Labrador, and sailing northwards touched at several points, proceeding up Hamilton Inlet and the Grand River, to the point mentioned above. Mr. Holme found many difficulties in the way, and much of the country he visited was virtually unexplored. With regard to the height of the Grand Falls, Mr. Holme states that the centre of Labrador, as is generally known, is a vast tableland, the limits of which are clearly defined, though of course the country intervening between this limit and the coast always consists, more or less, of a slope. Roughly speaking, it may be said that in the south and north there is a more or less gradual slope from the height of land to the coast, while in the south-east the descent is sudden, and almost immediately after leaving the tableland there is reached a level which is but little above that of the sea. In the north-east portion the edge of the tableland approaches nearest to the coast, while it trends considerably to the west in the rear of Hamilton Inlet. The most fertile part of the country is that which lies between the tableland and the sterile belt on the coast, though the height of land itself is by no means a desert. On the height of land there is found a succession of great lakes joined together by broad placid streams. When the streams of water reach the edge of the tableland, they of course commence a wild career down towards the sea. In the case of the Grand River this rapid descent commences with the Grand Falls, and almost the whole of the great drop to the sea-level is effected in the one waterfall. The elevation of the Labrador tableland is given by Prof. Hind as 2240 feet. From this height the Moisie and Cold Water Rivers descend to the sea by means of a considerable number of falls. But in the Grand River below Lake Waminikapou there is only one fall, viz. that which occurs 25 miles from the river-mouth. This fall is 70 feet. It is true that the whole of the river from Lake Waminikapou to the First Falls is rapid, but there is no place where there is any considerable drop, and indeed no place where it is necessary to take the boat out of the water. Now the lake first above the Grand Falls is on the height of land. In the channels joining the various lakes above the falls there are no rapids and there is scarcely any stream. It therefore follows, assuming the elevation of the tableland on the east to be approximate to that on the south, that in the 30 miles beginning with the Grand Falls and ending with Lake Waminikapou, there is a drop of about 2000 feet. Some of this drop is probably effected by the rapids immediately below the falls, but the greater part is no doubt made by the fall itself. The river is said by Maclean to be 500 yards broad above the falls, contracting to 50 yards at the falls themselves. The interior of the country Mr. Holme found was richly wooded, and the climate mild, though the plague of flies and mosquitoes was almost intolerable. The few Indians who inhabit Labrador belong mostly to the Cree nation, and according to Mr. Holme are probably perfectly unmixed with either whites or Eskimo. As an agricultural or pastoral country Mr. Holme thinks Labrador has no future, though something may be made of its iron, of the existence of which strong indications exist. Mr. Holme's observations have enabled us greatly to improve our maps of Labrador, and the photographs he brought home give an excellent idea of the general character of the country.

OUR ELECTRICAL COLUMN.

SOME very interesting and remarkable trials of the transmission of energy were recently made between Kriegstetten and Solothurn in Switzerland, by Prof. H. F. Weber and others, when it was found that 30 horse-power put in at the first place delivered 23 horse-power at the other, 8 kilometres away—showing an efficiency of 75 per cent. The current, 11 amperes, driven under an E.M.F. of 2000 volts, showed absolutely no loss whatever, owing to the use of Johnson and Phillips' "oil" insulators. This mode of insulation proved absolutely perfect.

THE distribution of electricity for lighting purposes by means of secondary generators, is now being discussed at the Society of Electrical Engineers. This mode of working seems to have solved the question of the economical erection of conductors. Alternate currents of high tension in the main conductors allow wires of small diameter to be used, and a special form of induction coil transforms these currents of high tension, 2000 volts, to currents of low tension, 100 volts and under, for use in private houses. The system, due to Messrs. Gaulard and Gibbs, is in use at the Grosvenor Gallery installation, as well as at Eastbourne and Brighton, and is probably going to be largely used. Mr. Kapp's paper "On Alternate Current Transformers, with Special Reference to the Best Proportion between Iron and Copper," will lead to an interesting discussion. All induction coils, when used as transformers, are simply a magnetic circuit or closed iron core interlaced with an electric circuit or a closed copper core, and constructed so that the electric circuit shall embrace as many as possible of the lines of force of the magnetic circuit. Mr. Kapp divides transformers into two classes—one in which the copper coils are spread over the surface of the iron core as in a Gramme armature, and the other in which the iron core is spread over the surface of the copper coil. The former he calls "core transformers," and the latter "shell transformers." He advocates working transformers at low inductions—that is, far below the point of saturation of iron—because it increases the plant-efficiency, reduces the heat or energy lost in the iron core through hysteresis, and prevents the production of sound. The plant-efficiency of transformers sometimes reaches as high as 99 per cent., and they are perfectly self-regulating. There is very little choice between core- and shell-transformers, but the former have the advantage. Economy in construction and facility in manufacture and repair seem to be principal points of advantage to reach. It is amusing to find how, now that the system has proved to be practical, every man is devising his own transformer, and labouring to show that Gaulard and Gibbs were not the inventors of the system, and that their transformers are not the best.

PROF. EWING'S discovery of hysteresis in iron has been shown, both by Kapp and Ferrari, to play a very significant figure in the efficiency of transformers.

GUGLIELMO, of Turin, has shown that no loss of electricity takes place through moist air surrounding an aërial wire unless the E.M.F. exceeds 600 volts, after which the leakage increases with the E.M.F. and the saturation of the air.

IN Boston an electric lamp has recently been used to search for a body drowned in the harbour. The U.S. steamship *Albatross* is furnished with a full complement of lamps for fishing. The glow-lamp is encased in a wire netting, which acts as a trap. The fish, being attracted by the light, swarm into the net, which is then closed and pulled in.

THE new number just issued (No. 201) of the Proceedings of the Royal Society contains the following electrical papers: "On the Photometry of the Glow-lamp," by Captain Abney and General Festing; "On the Development of Feeble Currents," by Dr. Alder Wright and Mr. C. Thompson; and "On the Heating-Effects of Electric Currents," by Mr. Preece.

MAKING GLASS SPECULA BY HAND.[1]

THE author of this paper gives a very interesting account of the construction of glass specula, discusses the actual difference in form between a spherical and a parabolic mirror, and gives an account of some experiments to determine the thickness of the silver film. In making the specula Mr. Madsen used glass for the grinding tool in place of metal, as he considered that the coefficient of expansion of iron and glass being different, greater truth would be obtained by the use of the same material for the tool, thus following the practice of Foucault and of the French opticians of the present day. When a true spherical surface was thus obtained the polish was given by rouge on pitch with a tool the same size as the mirror, and the correction of the spherical curve was obtained by a very ingenious plan of graduating the polisher in such a way that the greatest action would be on the required part of the mirror, the arrangement of the squares of pitch being such as to prevent the occurrence of rings of unequal polish. In this Mr. Madsen seems to have been most successful.

In working, the mirror was uppermost, and this is a very important point in many respects. There is no doubt that in working this way the mirror is in the condition of least strain, and if it were possible this plan should always be followed, but it is absolutely impossible to do this with a mirror much larger than the size he worked, which might almost be taken as the limiting size of mirror possible with this method of working. Short, Mudge, Herschel, and all the early workers used this plan in making their comparatively small mirrors; but since, with larger sizes, the mirrors have been worked face upwards as the only possible way, and it is to be regretted that this plan was not followed.

In discussing the actual amount of the glass to be abraded to obtain the correction, the author finds that for telescopes where the focal length exceeds twentytimes their diameter this amount is

[1] "Notes on the Process of polishing and figuring 18-inch Glass Specula by Hand, and Experiments with Flat Surfaces," by H. F. Madsen. (Journal and Proceedings of the Royal Society of New South Wales, vol. xx., 1886, pp. 79-91).

so small that it can be neglected, and that a spherical form is as good if not better than any other; and there is no doubt, for telescopes of about this ratio, Sir John Herschel is quite right when he makes the statement in "The Telescope," p. 81, "that is a good form that gives a good image; and that the geometrical distinctions between the parabola, sphere, and hyperbola, become mere theoretical abstractions in the figuring and polishing of specula." But in the case where the aperture of the mirror is about one-sixth of the focal length the distinction betwen the sphere and the parabola does exist and becomes a large quantity, which only the Foucault method of working allows to be dealt with properly. In enumerating the different plans used by opticians in getting the parabolic curve, the author is in error in stating that Lassell adopted the method of local polishing, as he always used a large tool, and got the figure by alterations of the stroke. Foucault was the inventor of the system of local polishing, and this was afterwards used by Draper, who finally rested on that as the best method of working.

The author considers that when the focal length exceeds 40 feet even with a theoretically perfect mirror the slightest touch or variation in temperature will be sufficient to destroy good definition with high powers, irrespective of the disturbing effects of the atmosphere, and he comes to the remarkable conclusion that "by decreasing the focal length the rays cross at a less acute angle, and small variations in the reflecting surface have not so detrimental an effect"—a statement that is entirely unsupported.

No actual tests of the work that the 18-inch mirrors will do are given. The experiments on the thickness of silver-on-glass films are interesting, as are also those on the effect of pressure or heat in altering the colours or colour-bands seen between two plane surfaces almost in contact. Dr. Draper, by actually weighing the amount of silver deposited on a large surface, came to the conclusion that it was about 1/200,000 of an inch thick; and the author, by comparing its thickness with the length of a wave of light, comes to about the same conclusion, and considers that by ordinary care in polishing no optical change will be produced in the reflecting surface by the film of silver deposited upon it.

The roads to success in making the mirrors of a reflecting telescope are many and various. Almost every maker in this fascinating pursuit had his own that gave to him best results. This was more particularly the case before Foucault published his most admirable memoir on the construction of silver-on-glass telescopes. In this memoir Foucault describes his method of local polishing, and the tests that can be applied to the concave surface, and a method of obtaining the true parabolic surface with absolute certainty, bringing the art of specula-making at once to a system of working by measurements in place of the old empirical process that had up to that time been in use; and everyone now uses Foucault's method of *testing* concave surfaces, and nearly everyone his plan of figuring by local polishing. Mr. Madsen gives a very interesting account of the road he took, an account that would have been much more valuable if the details of the processes used in making both the concave and the flat mirrors had been fully given, as it is now more in the improvements in these details that gain is to be looked for than in any of the main lines already known.

A. AINSLIE COMMON.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, February 2.—"On the Spectrum of the Oxyhydrogen Flame." By Profs. G. D. Liveing and J. Dewar.

In a former communication the authors described simultaneously with Dr. Huggins the strongest portion of the spectrum of water; subsequently they described a second less strong but more refrangible section of the same spectrum. M. Deslandres has noticed a third still more refrangible section. The authors now find that the spectrum extends, with diminishing intensity, into the visible region on the one hand, and far into the ultra-violet on the other. These faint parts of the spectrum they have photographed, using the dispersion of a single calcite prism and a lengthened exposure; and in the present communication they give a map of the whole extent observed, and a list of wave-lengths of upwards of 780 lines.

The spectrum exhibits the appearance of a series of rhythmical groups more or less overlapping one another, and the arrangement of the lines in these groups is shown to follow, in many cases, the law that the distances between the lines, as measured in wave-lengths, are in an arithmetic progression. M. Deslandres had previously announced that the succession of lines in several spectra, as well as in the telluric groups A, B and α of the solar spectrum, follow this law when their distances are measured in reciprocals of wave-lengths, and he has stated that the groups A, B and α have counterparts in the spectrum of water. The authors find a striking resemblance between those groups and certain parts of the water spectrum, but no exact correspondence.

Dr. Grünwald, of Prague, predicted on theoretical grounds that certain lines would appear in the spectrum of water, and the authors have found a considerable number of lines which tally closely with Dr. Grünwald's predictions, some of them, in the extremities of the spectrum, being the strongest lines observed in those regions.

February 9.—"True Teeth in the Young *Ornithorhynchus paradoxus*." By Edward B. Poulton, M.A., F.L.S., of Keble and Jesus Colleges, Oxford. Communicated by W. K. Parker, F.R.S.

This paper was a preliminary account of typical mammalian teeth developing beneath the site of the horny plates, which subserve mastication in the adult animal. In the upper jaw there are three teeth on each side: in the lower jaw two teeth, corresponding to the two posterior teeth of the upper jaw, were proved to exist, but the anterior one may be also present, for the jaws examined were not complete. The animal in which the teeth were found was about 8·3 decimetres long in the curled up attitude in which it had been received, and the larger hairs had alone appeared above the skin.

The anterior tooth of the upper jaw was long, narrow, and simple, as compared with the others; it was very fully developed, containing completely formed dentine and enamel, and its apex was nearly in contact with the lower surface of the oral epithelium. All the other teeth were broad and large, those of the upper jaw possessing two chief cusps on the inner side of the crown, and three or four small cusps on the outer side, while this arrangement was reversed in the lower jaw. Dentine was only formed upon the large cusps, and was not present upon all of these. The histological details and the manner of development appear to be precisely as in the higher Mammalia, a fact which strongly supports the identification of teeth with the placoid scales of Elasmobranchs. If teeth are so extremely ancient, then we should expect them to be unmodified in the ancestral Mammalia, although the other more recently specialized characters in the higher mammals are found in a more primitive condition in the former.

The teeth were found in some sections of the skull prepared for Dr. Parker by his son, Prof. W. Newton Parker. These sections, which had not been examined by Dr. Parker, were lent to the author, and Dr. Parker most generously encouraged the publication of the discovery, and assisted the investigation with other material.

Mathematical Society, February 9.—Sir J. Cockle, F.R.S., President, in the chair.—Messrs. A. E. H. Love and G. G. Morrice were admitted into the Society.—The following communications were made:—Further remarks on the theory of distributions, by Capt. Macmahon, R.A.—The free and forced vibrations of an elastic spherical shell containing a given mass of liquid, by A. E. H. Love.—On the volume generated by a congruency of lines, by R. A. Roberts.—Isoscelians, by R. Tucker.

EDINBURGH.

Royal Society, January 16.—Prof. Chrystal, Vice-President, in the chair.—Obituary notices of some former Vice-Presidents of the Society were read.—Prof. Tait communicated a paper by Prof. A. Macfarlane, on a problem in relationship.—Mr. W. Peddie read a paper on transition-resistance and polarization at platinum surfaces. He showed that transition-resistance increases greatly while polarization is proceeding. The ratio of the final to the initial resistance is in some cases as 2 to 1, when the electromotive force of polarization is equal to that of a Daniell cell. From his results regarding the time-rate of increase of polarization he deduced (10^{-9}) cm. as the value of the distance between the platinum and the layer of gas condensed upon it.—Mr. Peddie also read a note showing that the phenomenon of "electric-absorption" must be exhibited if a dielectric has a film of gas condensed on its surface.—Prof. Tait communicated a paper by Mr. Albert Campbell on the change in the thermo-electric properties of tin at its melting-point. While the tin is solid its line on the thermo-electric diagram is inclined upwards. Liquefaction occurs before the line reaches that of iron. At this point the direction of the line changes and becomes nearly identical with that of iron. Thus the "specific heat of electricity" in tin changes sign at the melting-point. This shows that the loosening of molecular attraction, which occurs at the melting-point, produces the same effect in tin as is produced in iron, while still solid, at the higher of the two temperatures at which its magnetic and other properties suddenly alter.—Prof. Tait read a paper on the thermo-electric properties of Signor Battelli's iron; and showed from Mr. Omond's Ben Nevis observations that ice-crystals may, in the greater number of cases, have at least a share in the production of the observed phenomena.

PARIS.

Academy of Sciences, February 6.—M. Janssen in the chair.—Second note on the law of probabilities as applied to target-firing, by M. J. Bertrand. The paper deals specially with the objections urged by General Putz in the *Revue d'Artillerie* against the principle admitted by Poisson, and against the law of probability now generally adopted in schools of gunnery. Reference was also made by General Menabrea to the important researches of M. Siacci in this field of inquiry.—Remarks in reply to an objection raised by M. Khandrikoff to the theory of solar spots and protuberances, by M. H. Faye. During his observation of the recent lunar eclipse Prof. Khandrikoff noted some protuberances, the presence of which in the absence of spots for some days before the eclipse seemed to militate against M. Faye's well-known theory. To this objection M. Faye replied at some length, pointing out that it is partly based on a misunderstanding of the true character and bearing of his views.—On perfect numbers, by Prof. Sylvester. Recently M. Servais stated that a perfect number (if such exist) containing only three distinct prime factors is necessarily divisible by 3 and 5. It is here shown that no such number exists, the line of argument employed at the same time demonstrating the theorem that there exists no perfect number containing less than six distinct prime factors.—Observations made at the Observatory of Algiers during the total lunar eclipse of January 28, by M. Ch. Trépied. These observations comprise, among other matters, a study of the colours assumed by the lunar disk; a spectroscopic examination of the eclipsed portion of the disk; and the occultations of the stars contained in the list prepared by the Observatory of Pulkowa for the purpose of obtaining an exact determination of the apparent diameter of the moon. Communications were also received from the Observatories of Bordeaux and Nice on various phases of the same occurrence.—Ephemeris of the planet 252 for the opposition of the year 1888, by M. Charlois. The true positions, right ascension and declination, are given for the period from March 5 to March 19. At opposition (March 12) the magnitude will be 13·4.—Note on permanent deformations and thermodynamics, by M. Marcel Brillouin. Two propositions are established: (1) that for most elastics solids there exists no finite relation between the temperature t, the mechanic variable X, and the geometric variable x; (2) that for most solid bodies there exists a linear equation with total differentials between t, X, and x: or, more correctly, there exist as many equations of this class as there are independent geometric variables. In a future communication the theoretic results of this study will be announced.—Influence of diet in determining the fixation and elimination of carbon in man, by MM. Hanriot and Ch. Richet. The results are tabulated of mixed nitrogenous, fat, and feculent diets, including beef, bread, potatoes, butter, cheese, sugar, wine, and coffee, continued for a period of fifteen days.—On the presence of striated muscles in mollusks, by M. Raphael Blanchard. M. Hermann Fol's recent statement that true transversal striation of the muscular fibre is found in no mollusk, is shown to be erroneous and based on defective observation of these organisms, in some of which true transversal striation certainly occurs.—On the endomorphic modifications of the granulitic systems in Morbihan, Brittany, by M. Charles Barrois. This paper is devoted to a careful study of the remarkable endomorphic modifications and mechanical transformations of the Guémené, Saint-Jean Brevelay, and Grandchamp granulitic formations, which traverse the Department of Morbihan in its entire length, and the typical constituents of which are: (1) zircon, apatite, black mica, oligoclase, orthose, and quartz; (2) orthose, microcline, quartz, tourmaline, and white mica.—On the Senonian and Danian systems of South-East Spain, by M. René Nicklès. Without attempting accurately to determine the respective limits of these formations, the author indicates the presence of extensive marine deposits in the Devonian containing fossiliferous limestones with several species of Hemipneustes associated with large banks of Hippurites and Pironea.—General Menabrea presented to the Academy the prospectus of a new edition of the works of Galileo, in about twenty-five volumes, which is about to be issued at the expense of the Italian Government, and copies presented to all the more important public libraries.—The Administrative Commission of the Academy announces that it has decided to supply Corresponding Members with the *Comptes rendus* free of charge from January 1, 1888. Correspondents are requested to acknowledge receipt of the first number, and notify their change of address to Messrs. Gauthier-Villars et Fils, publishers, Paris.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Treatise on Photography, 5th edition: Capt. Abney (Longmans).—The Story of Creation: E. Clodd (Longmans).—British Dogs, parts 15 and 16: H. Dalziel (U. Gill).—Beobachtungen der Russischen Polarstation an der Lenamündung, ii. Thiel, Meteorologische Beobachtungen: A. Eigner; II. Liefg. Beobachtungen vom Jahre 1883-84: R. Lenz.—Meteorological Observations at Stations of the Second Order for the Year 1883 (Eyre and Spottiswoode).—The Geographical Distribution of the Family Charadriidæ, H. Seebohm (Sotheran).—Annuario publicado pelo Imperial Observatorio do Rio de Janeiro, 1885-86-87 (Rio de Janeiro).—Memoirs and Proceedings of the Manchester Literary and Philosophical Society, 4th Series, vol. i. No. 2.—(Manchester).—Proceedings of the Manchester Literary and Philosophical Society, vol. xxvi. (Manchester).—Zeitschrift der Gesellschaft für Erdkunde zu Berlin, Nos. 133 und 134 (Reimer, Berlin).

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